Identifying the Impact of Exposure to Armed Conflict on Individual Preferences and Field Behavior: Evidence from Turkish Draft Veterans[*]

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Abstract

This research identifies the causal impact of exposure to armed conflict on risk, ambiguity and time preferences and related field behaviors for the average male randomly picked from the population. Our study builds on a natural experiment, engendered by the mandatory conscription system and the long-running civil conflict in Turkey, with a survey design that measures preferences through lab-in-the-field experiments. The setting we explore allows us to analyze the change in preferences without confoundment by community-level effects of conflict. Results show that conflict exposure increases risk tolerance, ambiguity neutrality, patience and time consistency. Tracing the effects on real life behaviors, we find that while conflict exposure leads to an increase in entrepreneurial activity, it has no significant impacts on risky health behaviors such as being overweight, smoking, or daily drinking. Evidence highlights post-traumatic growth in the form of elevated agency as a novel explanation for the observed changes in preferences.

JEL Class: C90, C93, D01, D74, D81 I01; O17; Z13
Keywords: Political Violence, Artefactual Field Experiment, Risk Preferences, Ambiguity Preferences, Time Preferences.

I. Introduction

How does exposure to armed conflict impact behaviors in subsequent real-life situations involving risks? While a handful of recent studies explored this question by conducting economic experiments (e.g., Voors et al., 2012; Jakiela and Ozier, 2019; Callen et al., 2014; Moya, 2018; Rockmore et al., 2020) in post-conflict settings, there exist notable gaps in this literature, including conflicting findings and the speculative nature of the evidence on the explanatory mechanisms. These gaps mainly stem from the challenges in establishing causality and generalizability because of the unavailability of suitable natural experiments with access to representative data, as well as from the obstacles to identifying and isolating the complex mechanisms linking war exposure to subsequent risk attitudes and behaviors because of the difficulty of separating the impact of exposure to armed violence from that of the macroenvironmental effects of conflict (Voors et al., 2012; Callen et al., 2014; Bauer et al., 2016; Jakiela and Ozier, 2019; Cederman and Vogt, 2017; Couttenier et al., 2019).

A further challenge to the findings of this line of inquiry has been raised by the recent evidence questioning the external validity of elicited risk preferences, which are employed as the primary outcome of interest by the existing work (Cardenas and Carpenter, 2013; Verschoor et al. 2016; Galizzi et al. 2016; Charness et al., 2020). Given that most risk-bearing choice situations in real life involve unknown probabilities and time tradeoffs, the use of ambiguity attitudes (Tymula et al., 2012; Sutter et al., 2013; Anantanasuwong et al., 2024) and time preferences (Sutter et al., 2013) have been offered as complementary instruments of economic preferences to bridge the gap between the experimented measures of risk preferences and real-life behaviors.

In this study, we aim to address these voids in the literature by exploiting a population-level natural experiment with a representative and rich survey to comprehensively investigate the causal influence of exposure to armed conflict on risk, ambiguity, and time preferences, as well as on real-life economic and health behaviors that are likely to be influenced by these preferences. Accordingly, we first examine the impact of conflict exposure on elicited risk, ambiguity, and time preferences using lab-in-the-field experiments. Next, we trace the effects on economic field behaviors including entrepreneurship, savings, and insurance demand, and on health outcomes, including being overweight, smoking, and daily drinking. And finally, we investigate the mechanisms that can explain the effects we observe.

The quasi-experiment on which our study builds is created by Turkey's strict universal conscription system, requiring every healthy male citizen to serve in the Armed Forces and assigning them to their service locations through a deployment lottery after basic training. In the 1984-2011 period, 97 percent of all eligible males (roughly 14 million men) were drafted through

this system, with 93 percent of them serving for 15 to 18 months. Our data indicate that, via the deployment lottery, around twenty-five percent of these individuals were sent to bases in the conflict zone in southeastern Turkey, where an ethnic civil conflict has been going on since 1984 between the Turkish state and the separatist Kurdistan Workers' Party (PKK).

We build on this vast natural experiment with a field survey designed to identify the unbiased causal effects of armed conflict exposure on economic preferences and to isolate the individual level explanatory mechanisms without confoundment by macro-level ones. Our data come from the Exposure to Violence and Individual Behavior-Conscript Veterans (EXPOVIBE-CV) survey conducted in western Turkey in late 2019 with 5,024 randomly selected adult male respondents who completed their mandatory military service sometime between 1984 and 2011 (Kibris, 2019). The EXPOVIBE-CV collected information on a wide range of socioeconomic and demographic characteristics, including the respondents' military service history and experiences, and included lab-in-the-field experiments to elicit risk, ambiguity, and time preferences using well-established and standard measures from behavioral and experimental economics (Harrison and Rutstrom, 2008; Dohmen et al., 2010; Sutter et al., 2013). Moreover, the survey explored related real-life outcomes through self-assessment questions, as well.

Theoretically speaking, how exposure to armed conflict in a combatant role may affect risk behavior is a priori uncertain, as the effects can operate via multiple and potentially opposing mediating channels (Elder and Clipp, 1989). On the one hand, soldiers are often encouraged to take risks in armed combat to achieve the operational goals of the military (Killgore et al., 2008; Momen et al., 2010). Moreover, combat exposure-related mental health ailments, such as depression, post-traumatic stress disorder (PTSD), and suicidal ideation (Tanielian and Jaycox, 2008; Cesur et al., 2013), can lead to cognitive biases and foster impatience and inconsistency, thereby creating a tendency to engage in risky behaviors in real life (Janoff-Bulman 1992; Tull et al. 2016; Wilk et al. 2010).

On the other hand, combat zone service is also expected to enhance abilities to conduct accurate and detailed situational analyses, and therefore, to favor the development of risk and time management skills (Börjesson, 2020; Momen et al., 2010; Turner and Tennant, 2009). Moreover, soldiers may encounter a variety of adaptive challenges on duty, including deadly armed combat, where they must carry out physically and mentally demanding tasks in hazardous, complex, and geographically challenging locations.[1] In such situations, inaccurate risk assessments, impatience, lack of situational awareness, or cognitive biases due to stress might lead to poor decisions with

[1] For example, see the Department of the Army's Training Circular 3-22.69 Advanced Situational Awareness for a description of the challenges and complexity of the fighting zone as well as the range of capabilities needed for soldiers to survive.

severe consequences like injury or death. Exposure to such an atmosphere, therefore, may train soldiers to better analyze the associated risks and tradeoffs.[2] Furthermore, traumatic experiences can subsequently lead to positive psychological changes, i.e., post-traumatic growth (PTG) in different life domains, including forming more meaningful relationships with others, increased appreciation of life, improved sense of personal strength, exhibiting changes in priorities, and experiencing an enhanced spiritual life (Tedeschi and Calhoun, 2004). Research shows that such personal growth can enable a person to mitigate the adverse effects of life hardship by improving their patience as well as their ability to control risks and make calculated decisions (Dar and Kimhi, 2001; Shahrabani and Garyn-Tal, 2019; Gneezy et al., 2020; Damen, 2019; Dohmen et al., 2010).[3]

We start our empirical explorations by providing evidence on the exogeneity of the service location assignment. Our investigations show that deployment to the conflict zone is indeed orthogonal to the pre-deployment characteristics of draftees, such as birth quarter, ethnic background, landownership, age at enlistment, military rank, and service duration.

After providing evidence on the validity of our identification strategy, we also demonstrate the internal validity of our experimentally elicited preference measures by presenting the strong correlations they exhibit with measures we derive from related self-assessment questions on risk attitudes and patience.

Next, we perform our main analysis. We start with the incentivized lab-in-the-field experiment designed to elicit risk and ambiguity preferences using the multiple price list (MPL) techniques. We find that service in the conflict zone decreases (increases) the likelihood of being risk averse (lover). Results also show that conflict exposure increases ambiguity neutrality, indicating improved consistency in behavior across risky and ambiguous situations.

We continue our analysis with the estimates of time preferences elicited via an unincentivized experiment designed to measure how much (if any) participants discount future monetary rewards. We find that conflict exposure has a small but statistically significant negative impact on the discount rate. In other words, we observe exposed individuals to be slightly more patient. Interestingly, we observe another consistency-effect of conflict exposure as results indicate that service in the conflict area elevates the likelihood of being time-consistent.

We undertake a battery of robustness tests to examine the sensitivity of our estimates, including the use of nonlinear estimation methods (logistics model) for binary outcomes;

[2] Though in a different context, see Molesworth et al. (2006) and Drinkwater and Molesworth (2010) for discussions on how experience and engaging with potentially hazardous events improves pilots' risk assessment skills.
[3] The emergence of PTG does not rule out the occurrence of PTSD, given that PTG results from a person's coping efforts with life struggles (Tedeschi and Calhoun, 2004).

replicating our analysis in arguably "cleaner" subsamples; excluding those with corner choices in the experiments; controlling for birthplace by draft year cohort effects; undertaking Oster's (2019) omitted variables bias test; multiple hypothesis testing; and clustering standard errors at arguably relevant different geographic and temporal levels. Our findings remain robust.

We next analyze the impact of conflict exposure on real-life behaviors. While risk loving tendencies may lead to riskier and harmful actions in practice, increased patience and stronger consistency in preferences could offset these effects. As a result, the overall impact on real-life behavior remains uncertain, making it a compelling subject for examination. We find that conflict zone service has a positive effect on entrepreneurship, but no impact on savings or insurance demand. Exploring risky health behaviors, we observe no effect on obesity, smoking, or daily drinking, either.

Next, we discuss the potential explanations for what causes the changes in preferences. First, we show that conflict zone service does not have a statistically significant impact on depressive symptomatology, which leads us to discredit explanations based on psychological distress. On the contrary, we present evidence that conflict zone service leads to psychological effects that are in line with the PTG arguments. In particular, we show that those who served in the conflict zone exhibit a higher sense of agency, reflecting social confidence, and a stronger belief of having control over their own lives. Given the findings of recent works that positively associate risk tolerance (Kesavayuth et al., 2018; Salamanca et al., 2016; Breuer et al, 2012; Illiashenko, 2019; Dohmen et al., 2010), patience (Gneezy et al., 2020), and time consistency (Benabou and Tirole 2002), these results provide potential explanations for the increased risk taking and patience, and higher consistency in preferences in the domain of ambiguity and time. They also elucidate why the positive effect of exposure on risk taking does not transmit into risky health behaviors as the improvements we observe on the sense of agency are likely to have mitigated the potential detrimental effects of conflict exposure on life choices by preventing excessive risk taking, impatience, and inconsistencies across time and risk domains (Gneezy et al., 2020).

The improved sense of agency we observe in combat veterans can also be an explanation for their higher likelihood of entrepreneurial activity. However, we also find that conflict exposure slightly decreases income and increases unemployment. As unemployment might drive one to try self-employment (Fairlie, 2013), these findings can provide an alternative explanation for our finding on entrepreneurship. They might also explain the null effects we observe on savings and insurance demand.

Our study provides novel contributions to the literature studying the impact of conflict exposure on risk preferences and behaviors (e.g. Voors et al., 2012; Callen et al., 2014; Jakiela and

Ozier, 2019). First, we identify the causal effects of exposure to armed conflict in a combatant role for the typical adult male randomly selected from the population. Second, because they study individuals in (post)conflict environments either through observing them before and after conflict events hit their communities (Jakiela and Ozier, 2019) or comparing those living in more versus less conflict-exposed communities in the aftermath of a conflict (Voors et al.,2024, Callen et al., 2014), findings in existing works are likely to be shaped at various extents by the community-level effects of conflict.[4] Our research framework, on the other hand, purges any role that could be played by the incentives and constraints brought out by the conflict ecology, such as community-level paradigm shifts, resource shortages, postwar economic growth, and security concerns, and identifies the effects of armed violence exposure net of such potential conflict-induced macroenvironmental changes. To this end, we take advantage of the geographical concentration of the conflict in the southeast of the country and sample from the peaceful provinces in the west. In other words, we compare the exposed and unexposed members of communities which themselves are untouched by the conflict. Relatedly, our analysis provides compelling evidence on the potential explanatory pathways. Finally, we offer a comprehensive picture of the individual-level economic effects of armed conflict exposure by analyzing and presenting results on a rich array of elicited preferences and real-life economic outcomes. We are the first paper to bring together such a rich combination of preferences and behaviors, including examining the effect of exposure to armed conflict on ambiguity preferences for the first time in the literature. In addition, we are the first paper to provide a comprehensive analysis of time preferences that includes experimentally elicited measures of time discounting, consistency, present- and future-biases. Our findings evidence the importance of looking beyond risk preferences to understand the impact of conflict exposure on real-life behaviors. Note that had we focused solely on risk preferences, we would have predicted a surge in risky behaviors. However, our findings reveal that this is not the case and based on the overall profile of effects on preferences, we nominate an important and novel explanation.

Our findings offer important contributions to other literatures as well. A related body of work explores the impact of community-level hardship, such as natural disasters (Eckel et al., 2009; Cassar et al., 2017; Hanaoka et al., 2018; Beine et al., 2020), economic downturns (Malmendier and Nagel, 2011; Fisman et al., 2015), violent crime[5] (Nasir et al., 2017; Brown et al., 2019), and pandemics (Drichoutis and Nayga Jr., 2021; Shachat et al., 2021).[6] As our empirical framework

[4] See Jakiela and Ozier (2019) for a more detailed discussion on the community-level effects of conflict.
[5] Important differences exist between violent crime and armed political conflict including intensity, frequency, and type of exposure (e.g., Nasir et al. 2017). Therefore, it is unclear whether the effects of violent crime on economic preferences are identical to those of armed conflicts. To be cautious, we treat these literatures separately from each other.
[6] See Chuang and Schechter (2015) and Schildberg-Hörisch (2018) for literature reviews of the literature on stability of experimental and survey measures of economic preferences over time.

identifies the impact of exposure to armed violence in the absence of conflict-induced environmental incentives and constraints, our findings then have important implications as to whether and how individual level exposure to hardship and trauma may differ from community-level exposure in terms of its effects.

We also add to the literature examining whether preferences would change during adulthood (Carmil and Breznitz, 1991; Punamäki et al., 1997; Tedeschi and Calhoun, 2004). Finally, our findings are relevant to the literature examining the validity of alternative methods of experimental risk elicitation in addressing the bridge between elicited measures and real-life behaviors (Galizzi et al. 2016; Epper and Fehr-Duda, 2024).

In the next section, we discuss our case and identification strategy. Section 3 presents the data and variables. Section 4 introduces the econometric equations. We conduct our balance tests in Section 5 and present the results from experimental analysis in Section 6. Section 7 presents the results on real-life behaviors. Section 8 considers potential explanations and Section 9 concludes.

II. Identifying the Impact of Conflict Zone Service

We identify the causal impact of armed conflict exposure by employing a natural experiment engendered by the strict mandatory conscription system in Turkey with its lottery-based deployment system and the Kurdistan Workers' Party (PKK) uprising going on in the southeast of the country since 1984.[7] By 2020, the ongoing armed struggle between the Turkish state and the PKK resulted in over 25,000 combatant deaths, with Turkish military forces accounting for around 7,500 and PKK recruits making up almost 17,500 of the total (Kibris, 2021). In response to escalated violence, the Turkish state announced a *state of emergency* (OHAL) at the heart of the conflict (Official Gazette, 1987). The OHAL region encompasses the 13 southeastern provinces, namely Adıyaman, Batman, Bingöl, Bitlis, Diyarbakır, Elazığ, Hakkari, Mardin, Muş, Siirt, Şırnak, Tunceli, and Van, as can be seen in Figure I. The declaration of the state of emergency enhanced the military's authority in the area, granting it the ability to implement extensive measures that would otherwise be impossible. The OHAL area, therefore, constitutes the officially recognized conflict zone (Official Gazette, 1983, 1987; Agamben, 2005; Öztan and Bezci, 2015).

Under Turkey's strictly enforced universal conscription system, every healthy male citizen is required to serve in the Armed Forces. Men become eligible for conscription when they turn 20 and are usually drafted before their 22nd birthday, depending on the current induction period in

[7] Although initially proclaiming the establishment of an autonomous Kurdish state in southeastern Turkey, the PKK shifted its attention towards attaining a federal structure in the 1990s (Stanton, 2016).

their registered province (Official Gazette, 1927; 2019). During the analyzed period from 1984 to 2011, service duration varied between 15 and 18 months.

The process commences with the issuance of a draft call, summoning individuals who meet the eligibility criteria within the designated induction period to report to the Armed Forces. Prior to induction, after the confirmation of enrollment, the military allocates conscripts to different branches, military vocation categories, and training locations. Comprehensive information on this classification, mostly centered around educational credentials and carried out on anonymized records, is available in the official instructional booklets for potential conscripts. We include a flowchart illustrating these procedures in the Online Data Appendix II.

Following their induction, conscripts undergo up to three months of basic training. Note that training and service durations are determined by the regulations in place at the time of the draft and do not vary within cohorts. Hence, draftees know how long they will be under duty at the time of induction. After the boot camp, conscripts are sent to military installations nationwide, except in their respective home provinces, to fulfill their service obligations. The foundation of our natural experiment is the military's service location assignment system, which eliminates draftees' autonomy in determining their deployment bases. Instead, the General Staff of the Turkish Armed Forces first determines the personnel needs across bases around the country (Official Gazette, 2019). Then, the deployment assignment is determined by a 'base lottery' which considers the branch of service, military occupation, and home provinces of conscripts (Turkish Ministry of Defence, 2015; Mater, 1999 pp.13,42,114,131,136). Recordings of base lotteries, as they were held in public ceremonies to ensure openness in the assignment process, can still be accessed on social media platforms.[8] Through this system, a significant portion of the conscripts find themselves assigned to bases in the conflict zone, and they get actively involved in the armed conflict against the PKK as combatants. Thus, the Turkish military's service location assignment system creates a natural experiment enabling us to identify the causal effects of conflict zone deployment, holding the branch of service, military occupation, and home provinces of service members constant.

From 1984 until 2011, under this strict military service regime, the Turkish Armed Forces drafted almost all men (roughly 97 percent or 14 million individuals), reaching the conscription age. We limit attention to this time frame due to the structural changes the Turkish military and the conscription system have gone through afterward.[9] With new legislation in 2011, the Turkish military initiated the recruitment of professional troops on fixed-term contracts to replace conscripts, particularly in areas of conflict, as a step towards establishing a professional army

[8] An example can be found at https://www.youtube.com/watch?v=D3w4i07_Wj4.
[9] Estimates based on authors' own calculations using the EXPOVIBE survey are shown in Appendix Table 1.

(Official Gazette, 2011). Once professional troops were established, citizens were allowed to pay to limit their service to basic training only. Since our focus is on determining the impact of Armed Conflict Exposure (ACE) for a randomly selected individual from the population (PATE), we restrict our research to the timeframe spanning from 1984 to 2011. Based on our data, we infer that 93 percent of the men drafted within that period served at least 15 months. Additionally, about one-fourth of them, which roughly corresponds to 3.5 million individuals, were sent to the conflict zone through the deployment lottery. To the best of our knowledge, this renders the current study the first to reveal the causal impact of an average male's exposure to armed violence on subsequent economic preferences, with results applicable to nearly the entire adult male population.

Although the Armed Forces' regulations state that conditional on the branch of service, military occupation, and residential origin, conscripts' deployment assignments are unrelated to their pre-enlistment characteristics, one may question the accuracy of these official statements. Below, we conduct formal balancing tests and validate our identifying assumptions to address such potential concerns.

Additional information supports the regulatory narrative of the military. Due to the life-threatening risks associated with service in conflict zones, the fairness of the assignment system has always been a concern for the general public and the media, especially during periods of heightened conflict, as a substantial number of conscripts have lost their lives or suffered injuries in clashes (Kıbrıs, 2011). Therefore, both the Turkish Ministry of Defense and the Armed Forces consistently underline in their public communications that the system does not allow any kind of discrimination (Yıldırımkaya, 2010; Turkish Ministry of Defence, 2015).

Furthermore, anecdotal evidence corroborates the system's lack of discrimination, as in the 2007 incident in which the first cousin of the Secretary of State of the time was killed on duty in a PKK attack on the Çeltikli outpost in Bitlis, a province in the southeast. It is also not unusual to have immediate family members of high-ranking military officials among the conscript casualties (Kibris, 2021; Mater, 1999). Moreover, the Turkish population's enduring faith in the Armed Forces provides evidence of public trust in military practices (Esmer, 1999; Adaman et al., 2005).

An important question regarding the validity of our identification strategy is whether individuals can escape deployment to conflict zones by evading conscription, manipulating their service location, or influencing the timing of induction. This possibility could undermine the credibility of our natural experiment in identifying the causal impact of ACE. Unlike citizens in other nations that have universal conscription systems, e.g., Israel and South Korea, where a considerable fraction of draft-eligible men can escape mandatory service, avoiding induction is not a viable choice for young Turkish men as there are very few options to bypass the strict

conscription system.[10] Individuals who evade conscription are subject to legal prosecution and are ostracized by society through social exclusion (Altınay and Bora, 2002; Altınay, 2012). Therefore, for the vast majority of the population, avoiding the draft is not a desirable option. Draft evaders and those who help them face legal charges, including arrest and imprisonment of up to three years if found guilty by the military court.[11] The legal ramifications also include losing the right to work in paid employment because it is against the law for companies to hire individuals who have evaded the draft, with employers facing imprisonment for violations.[12] Finally, the probability of acquiring a fake health-ailment exemption is low due to the rigorous medical proof such claims require and the several stages of medical examinations and approvals required from different entities.[13] The conscription system in Turkey is, therefore, an uncommon case where nearly all Turkish men, with the exception of a small fraction who were pardoned due to severe health conditions and those who unlawfully evade military service, are enlisted and serve their duty (Akyürek, 2010). In line with these arguments, within the 1984-2011 period we focus on, almost all males born between 1962 and 1991 were conscripted.

An important aspect of the conscription system in Turkey is its inclusion of differential treatment based on educational attainment. Although all individuals get a draft call upon reaching the age of enlistment, i.e., age 20, men who continue their formal schooling are granted the option to delay their enlistment until they complete their studies or until they reach the age of 29, whichever occurs earlier. According to this regulation, all drafted individuals who do not have a college degree must serve full duration as rank-and-file soldiers. Individuals with a four-year university degree, on the other hand, are assigned to either full-term sub-lieutenant positions or half-term rank-and-file positions, depending on the needs of the military at the time of their induction. Individuals who serve half-term, however, make up a small percentage of the overall population, as reflected by our estimates in the EXPOVIBE-CV, indicating that 93 percent of men conscripted between 1984 and 2011 served full term for at least 15 months.

[10] In the Israeli context, individuals can be exempted from certain obligations based on religious, health, psychological, or legal reasons. Additionally, have the option to decline serving based on pacifism, antimilitarism, religious philosophy, or political opposition to Israeli policy. In 2002, the Israeli High Court of Justice declared that the act of refusing to serve was legally permissible. (https://military-history.fandom.com/wiki/Refusal_to_serve_in_the_IDF). Although there is less room for evasion in the South Korean example, the definition of obligatory duty is nonetheless extensive, encompassing social work, research, full-time reserve enlistment, and industrial technical service.

[11] The Military Penal Code, as stipulated in statute number 1632, specifies that avoiding military duty is subject to a maximum jail sentence of three years, while harboring a fugitive is subject to a maximum prison sentence of two years. https://www.mevzuat.gov.tr/mevzuatmetin/1.3.1632.pdf.

[12] https://turkishlaborlaw.com/news/business-in-turkey/is-there-a-penalty-for-hiring-a-deserter/

[13] Individuals with severe health conditions are granted an exemption provided their diagnosis is endorsed by a committee of military physicians. The definition of a serious health problem' is outlined in the Turkish Armed Forces Health Capability Regulation, as stated in the Official Gazette 29530 on 12 November 2015.

However, a notable aspect of this system is that irrespective of education level, military rank, or service duration, everyone is bound by the lottery-based deployment assignment. Moreover, given that individuals become eligible for military conscription at the age of 20 and are given the option to delay their recruitment if they continue their education beyond high school, the majority of inductions occur after the end of formal schooling (Akyürek, 2010; Yıldırım and Erdinç, 2007). Since the branch of service and military occupation classifications are determined by the Armed Forces based on the technical specializations, service length, and conscription age, individuals with at least a four-year university degree have slightly lower probabilities of deployment to the conflict zone. Therefore, it is necessary to incorporate formal schooling among the conditional random assignment covariates to maintain the validity of our identifying assumptions.

III. Data and Measures

The data used in our study is derived from the EXPOVIBE-Conscript Veterans (EXPOVIBE-CV) survey, which was conducted in 2019 through face-to-face interviews with 5,024 adult males in Turkey.[14] The interviews took place at residential addresses, randomly selected by the Turkish Institute of Statistics (TurkStat) in 29 provinces in western Turkey, situated outside of and with negligible in-migration from the conflict zone. Hence, the EXPOVIBE-CV deliberately distinguishes exposure during military service from exposure as civilians.[15]

The survey was designed to collect data on various personal and family attributes, attitudes, behaviors, and conscription history. Questions on military experiences include details such as the specific branch of service, military vocation, rank, length and location of training, as well as the location and duration of service. At each randomly chosen residence, the eligible participant was the male head of the household who fulfilled military service obligations between 1984 and 2011.[16] Our armed conflict exposure measure, *Conflict Zone*, is a dichotomous variable capturing whether the respondent was deployed to a base in the conflict zone.

[14] Interviews were conducted in Turkish by trained interviewers. Informed consent was obtained from all participants. A pilot study was conducted to test the questionnaire and field organization before embarking on the main field study.
[15] TurkStat maintains the national address-based electronic census registry system in Turkey. From this registry, which serves as the sampling frame, residential addresses were randomly drawn from the 29 provinces in proportion to population distribution across these provinces.
[16] The EXPOVIBE-CV study did not include those who were exempt or had a significantly shorter service tenure due to exceptional circumstances, such as health issues.

III.I. Risk and Ambiguity Preferences

We elicit risk and ambiguity preferences via a lab-in-the-field experiment administered to a randomly selected subsample of 2,502 respondents within the EXPOVIBE-CV survey.[17] The experiment was designed using the multiple price list (MPL) technique for both the risk and ambiguity preferences elicitation.[18]

As the experiment was conducted in the field with a representative sample, special attention was paid to simplicity. In each decision problem, rather than comparing different lotteries, participants were asked to compare a simple lottery with a certain amount. The risky lottery used easily understandable 50-50 probabilities without employing technical language. Also, the words "lottery" or "gamble" were never mentioned since those concepts might carry religious connotations for Muslim participants (Falk et al., 2016; Falk et al., 2018). In each question, respondents were asked to make a simple choice between drawing a marble from a bag for a chance to win a fixed amount of money and receiving a smaller amount for sure.[19] The exact instructions that participants received are provided in the Online Data Appendix III.

Participants were shown two black bags, each containing 10 marbles. The marbles were exclusively either red or blue. While Bag 1 (which we refer to as the "Risky Bag") contained exactly 5 marbles of each color, the exact distribution of marbles in Bag 2 (which we call the "Ambiguous Bag") was unknown to the participants. Subjects could win 2,500 TL by betting on the color of their choice to be drawn blindly from a bag by themselves.[20]Accordingly, they were first instructed to pick a color, red or blue. Then, they were asked a series of ten questions using one of the bags, followed by the same ten questions again, but this time using the other bag, with the order of the risky and ambiguous bags being randomly determined to control for possible order effects. In each round, participants were instructed to choose between playing the lottery by drawing a marble from the bag and accepting a sure amount of money. The lottery was always the same: If the randomly drawn marble from the corresponding bag is the same color as the (previously elicited) color of their choice, the participant wins 2,500 TL, and nothing otherwise. The sure amount changed in

[17] Because the EXPOVIBE-CV survey included multiple incentivized experiments, to prevent them from priming each other, each experiment was conducted with a randomly selected subsample.
[18] MPLs have been used widely to elicit risk (e.g., Holt and Laury, 2002; Harrison and Rutstrom, 2008; Dohmen et al., 2010, Dohmen et al., 2011) and ambiguity preferences (e.g., Sutter et al., 2013; Dean and Ortoleva, 2019).
[19] As gambling is a sin in Islam, we paid special attention to the choice of language and execution of the game. The fact that it does not require the participants to bet any monetary amount of their own and that there is no possible negative payoff, we were confident that Turkish people would not consider it as gambling. As expected, most respondents agreed to participate in the experiment.
[20] At the time of the experiment, 2,500 TL corresponded to about $450 and was slightly higher than the *monthly* legal minimum wage in Turkey.

each question. It started at 600 TL in the first offer and ended at 1,500 TL in the 10th question with increments of 100 TL (see Tables III.1 and III.2 in the Online Data Appendix III).[21]

To ensure incentive compatibility, only one question was randomly selected to be paid out (Azrieli et al., 2018). At the end of the experiment, individuals picked a card randomly from a deck of cards numbered from 1 to 20. Their (potential) earnings were determined by their choice in that randomly selected question—they earned the sure amount if they picked the safe option in that question. Otherwise, they played the lottery by blindly drawing a marble from the corresponding bag.

Due to the prohibitively high cost of paying every single individual, two randomly selected individuals were paid in this experiment. Note that the risk and ambiguity elicitation techniques used in the experiment were adopted from well-documented experimental designs that produce reliable answers even in the absence of monetary incentives (Falk et al., 2016; Falk et al., 2018).[22]

The measures of risk and ambiguity preferences were developed without relying on specific utility forms or parametric assumptions.[23] Instead, they depend directly on participants' own choices. We construct our risk preference measures based on decisions involving the Risky Bag. Note that while a risk neutral agent is by definition neutral to risk and maximizes the expected payoff, a risk averse (loving) agent would prefer to take less (more) risk than a risk neutral agent even if it implies a lower expected payoff. Because the expected payoff for the lottery is 1,250 TL, we code *Risk Averse* equal to 1 for individuals who picked the lottery less than seven times before switching to a sure amount (i.e., for amounts less than the expected payoff) and 0 otherwise. We define *Risk Neutral* as a dummy variable for individuals who picked the lottery exactly seven times before switching to the safe option of 1,300TL. The binary *Risk Lover* indicator represents those who picked the lottery more than seven times before switching to the safe option.

Our measures for ambiguity preferences come from decisions in the Ambiguous Bag compared to the Risky Bag. Specifically, we define *Ambiguity Averse* as an indicator dummy variable that takes the value of 1 if the respondents switch to a sure amount earlier in the Ambiguous Bag than the Risky Bag. Likewise, we define *Ambiguity Lover* as an indicator dummy variable that takes

[21] While we could have selected a range for sure amount from 0 TL to 2,500 TL, we purposefully avoided asking too many questions in order not to overwhelm our respondents. In addition, we did not want to take a short cut by imposing and eliciting a unique switching point which would have made it impossible to identify confused participants.
[22] In a recent study on global variation in economic preferences, Falk et al. (2018) use a similar experiment with no monetary incentives to elicit risk preferences and find that the elicited risk preferences are meaningful predictors of economic outcomes. In addition, their findings on the determinants of risk preferences concur with previous literature.
[23] The only assumption we make is monotonicity. Specifically, if an individual prefers a safe amount over a lottery in a given question, we assume they should also prefer a larger safe amount over the same lottery. Consequently, we classify a small percentage of participants who switch back and forth between the lottery and the safe option as confused and exclude them from our analysis. See Section VI.II for a discussion on robustness of our experimental findings.

the value of 1 if the respondents switch to a sure amount later in the Ambiguous Bag than the Risky Bag. Finally, *Ambiguity Neutral* is set to 1 for respondents whose switching points in the Ambiguous and Risky Bags are the same and coded as 0 otherwise.[24] Similar to Charness et al. (2013) and Stahl (2014), we find that ambiguity neutrality (71%) is more prevalent than ambiguity aversion (18%) and ambiguity seeking (11%).

In the context of conflict exposure, we are the first paper to elicit ambiguity preferences and study the causal impact of exposure on these preferences. Even though many situations in the real world, including but not limited to investment, job, and insurance choices, involve decisions where ambiguities are present, that is, where probabilities of potential outcomes are unknown (e.g., Ellsberg, 1961; Halevy, 2007; Abdellaoui et al., 2011; Ahn et al. 2014), the literature is still scant about the impacts of conflict exposure on attitudes towards ambiguity (Cavatorta and Groom, 2020). Hence, our measures of ambiguity preferences complement our measures of risk preferences and allow us to provide a better understanding of how exposure to conflict shapes decisions involving risk and uncertainty in the field.

III.II. Time Preferences

To elicit time preferences, we follow the experimental design of Ashraf et al. (2006). Specifically, participants were first offered to choose between 200 TL now and 220 TL next week, and then, for those who chose not to wait, the offer was increased to 240 TL. Eventually, those who had not reached their crossover point were asked to state how much they should be offered to wait for one week. Using information on crossover points, we calculated the discount rates of participants and called it *Discount Rate I,* gauging the rate at which the offer obtained in the future is converted to an equivalent value received at present.

Next, the offer timing was shifted to the future by one week. Accordingly, the participants were offered to choose between 200 TL next week and 220 TL in two weeks, and then for those who chose not to wait another week, the offer was increased to 240 TL. Finally, they were asked to state the delayed reward that would make the later payment as appealing as the sooner one. Using this information, *Discount Rate II* was constructed using the same method in the previous example. In the analysis, we use the average of *Discount Rate I* and *Discount Rate II*, which we refer to as *Discount Rate*, as our elicited measure for impatience level.[25]

[24] Participants who always or never switch to the sure amount mass at the bottom or top of the response distribution. This may pose a fuzziness in the definition of ambiguity preferences in the sense that responders at the bottom are either ambiguity averse or ambiguity neutral while responders at the top are either ambiguity seeker or ambiguity neutral. We test the robustness of our findings to the exclusion of such cases in the analysis and show that our findings are resilient to this exercise.

[25] Using *Discount Rate I* and *Discount Rate II* separately produces qualitatively similar estimates.

Measuring discount rates at two different time points enables us to evaluate whether respondents' discount rates vary over time and allows us to provide a more comprehensive account of time preferences. The literature highlights that time inconsistency—systematic deviations from long-term plans—is a pervasive phenomenon in decision-making (Frederick et. al, 2002). These departures are often myopic, leading individuals to value more rewards to the present than originally planned, a phenomenon known as present bias or hyperbolic discounting (Thaler, 1981; Laibson, 1997; O'Donoghue and Rabin, 1999). However, departures can also be farsighted, resulting in greater allocation to the future than initially intended (Loewenstein, 1987; Takeuchi, 2011). To capture these variations, we define the dichotomous indicator *Time Consistent*, which takes the value of one if the discount rate remains unchanged over time and zero otherwise. The variable *Present Bias* (i.e., hyperbolic discounting) takes the value of one if the participant's discount rate decreases over time; otherwise, it is set to zero. *Future Bias* is equated to one if the respondent's discount rate increases over time; otherwise, it is coded as zero.

Similar to Ashraf et al. (2006), we opted for an unincentivized approach to elicit time preferences due to the important advantages it offers beyond cost effectiveness. First, the hypothetical nature of the questions avoids confounding factors, such as the uncertainty of receiving future payments and the differing transaction costs between present and future payouts (Cohen et al. 2020). Second, it eliminates challenges related to maintaining confidentiality and anonymity that arise with tracking participants to distribute future payments. While concerns may arise regarding whether hypothetical questions accurately reflect true preferences, evidence from similar unincentivized experimental designs suggests that participants provide truthful responses (Branas-Garza et al. 2023).

As the questions were short, simple, and straightforward, they were asked to all (5,024) survey participants.

III.III. Internal Validity of the Experimentally Elicited Preference Measures

The EXPOVIBE-CV includes self-assessment questions on risk attitudes, patience, and impulsivity allowing us to derive attitudinal indicators to test the internal validity of our experimentally elicited preference measures.

Our first indicator captures attitudes towards financial risks via a question that requires participants to indicate on a 5-point Likert scale how accurately the statement "*If necessary, I will take risks to get a good profit*," represents them with answers ranging from "*not at all*" (1) to "*exactly*" (5). We expect the self-assessments on this question to positively associate with the elicited risk-

tolerance indicators as it is related to financial risk taking (Callen et al. 2014). To achieve consistency with the dichotomous *Risk Lover*, we coded the binary *Financial Risk Taker* equal to 1 for those who indicated some (4) or full (5) representativeness and zero otherwise.

Our second indicator taps into a more general risk attitude measured via a question that requires participants to indicate on the same 5-point Likert scale how accurately the statement "*I don't like taking risks in life. If possible, I avoid it*," represents them. Note that, this question does not specify anything about the nature or the domain of the risk. The binary *General Risk Taker* is then set equal to 1 for those who indicated little (2) or no (1) representativeness.

Next, we employ indicators of patience and impulsivity. *The Patience Index* is based on two survey items asking on a 5-point Likert scale whether the respondent is able to wait out tough times and how patient he is deemed by his friends, respectively. We also construct an *Impulsivity Index* based on four survey items that inquire on the same scale whether the respondent tends to spend his money as soon as he gets it; shops for unnecessary products; likes to drive fast; and favors savoring each moment as if there was no tomorrow, respectively. Both reflect the total scores normalized to mean 0 and standard deviation of 1 for ease of comparison in the empirical analysis.

In Online Appendix Table 2, we formally test via univariate regressions whether our experimental measures correlate with the self-reported attitudes in the expected ways. Column (1) is devoted to financial risk attitude. Consistent with the findings of the associated literature, we observe that financial risk-taking tendency correlates positively with experimental risk tolerance indicators (Callen et al., 2014; Charness et al., 2020; Dohmen et al., 2011). Then in column (2), in line with its broader definition and the findings of different studies examining the correlates of ambiguity attitudes (Stein and Segal, 2006; Hoy et al., 2014), we find the self-reported general risk-taking propensity to positively associate with the likelihood of risk seeking and ambiguity neutrality and negatively associate with ambiguity aversion.

As shown in column (3), our experimentally elicited discount rate negatively correlates with self-assessed levels of patience as expected. Results also indicate ambiguity neutral and time consistent individuals to exhibit more patient and less impulsive attitudes. These results are similar to the general patterns documented in the literature (Ashraf et al., 2006; Tanaka et al., 2010; Sutter et al., 2013; Falk et al. 2018).

IV. Econometric Equations

Using the following econometric equation, we first test whether our quasi-experimental treatment assignment, conflict zone deployment, is orthogonal to pre-deployment characteristics:

(1) $(Conflict\ Zone)_c = \beta_0 + \beta_1 \mathbf{E}_c + \beta_2 \mathbf{R}_c + \varepsilon_c$

where deployment to a base in the state of emergency (OHAL) area as a conscript is represented by *Conflict Zone*.

The vector **R** consists of covariates upon which the foundations of our natural experiments rest. These variables are height in centimeters, the branch of service, military occupation, and birth province identifiers, half-term service indicator, and years of schooling by the year of induction. As mentioned before and in other sources, induction occurs after the completion of formal schooling (Kibris and Cesur, 2022; Akyürek, 2010), which is the most critical input the Armed Forces employs in determining the branch of service and military vocation of conscripts prior to induction (Yıldırım and Erdinç, 2007; Yıldırımkaya, 2010). For this reason, we include educational attainment differentiated by the timing of induction to account for both the level of years of schooling and the distribution of formal education by cohort. Moreover, as certain military occupations have minimum or maximum height requirements, we also specify height among the conditional random assignment variables (Official Gazette, 2015).

E is a vector of external factors that are not influenced by the deployment itself. These factors include birth quarter, Kurdish ethnicity, indicators for non-Muslim minority, landownership, conscription age, military rank, training length, and service duration for conscript c. Military rank, bootcamp training length and service duration are among pre-deployment characteristics because they are determined prior to deployment lottery (by military regulations in place at the time of the draft) and are unrelated to the deployment decisions, as stated by the Armed Forces. If the Turkish Military assigns conscripts to various military bases in line with its outline method, covariates determined prior to deployment should be unrelated to the likelihood of being deployed to the conflict zone, holding conditional random assignment variables constant.

ε_c is the idiosyncratic error term. Standard errors are corrected for clustering at the service province level.

We estimate the impact of conflict zone service on our outcome measures using the following econometric specification:

(2) $P_c = \mu_0 + \mu_1(Conflict\ Zone)_c + \mu_2 \mathbf{E}_c + \mu_2 \mathbf{R}_c + ¥_c$

where P denotes the outcome of interest for conscript *c*. *Conflict Zone* represents whether the respondent served in the state of emergency area and the remaining variables are similar to those presented in equation (1).

V. Evidence on the Exogeneity of Deployment to the Conflict Zone

The summary statistics for pre-enlistment variables are shown in Online Appendix Table 3. Column (1) contains the whole sample. In columns (2) and (3), we display the mean values for those who were deployed outside of the conflict zone and the state of emergency area, respectively. These summary statistics are in line with the argument that the pre-deployment characteristics do not significantly differ based on deployment location.

Formal exogeneity tests are performed in Table 1 by estimating equation (1). In particular, we contrast the pre-deployment characteristics of individuals who were deployed to the conflict zone with those who served outside of it as draftees, with columns (1) and (2) being devoted to the full and the risk/ambiguity game samples. Balance tests indicate that combat zone assignment is independent of pre-deployment variables in both samples. Therefore, these results suggest that our quasi-experiment identifies the causal effect of conflict zone assignment of draftees on their subsequent outcomes.

There is a small but statistically significant increase in the length of bootcamp training. As discussed above, this result is in line with the military's practice of providing internal safety training to individuals who are randomly chosen to be sent to war zones (Mater, 1999, p. 42). The Armed Forces conduct this safety training to reduce the probability of conscripts being targeted by the PKK while traveling to their duty locations.

Upon showing evidence on the exogeneity of our identifying variation, we examine the impact of conflict zone deployment on direct armed combat involvement in Online Appendix Table 4. In columns (1) to (4) of Panel A, we find that conflict zone deployment increases the likelihood of any direct armed combat engagement, enemy firefighting, witnessing casualties, and being injured by 40, 33.7, 30.2, and 4.7 percentage points, respectively, in the full sample. Panel B documents a similar pattern of findings in the risk sample. These results show that service in the conflict zone substantially elevates the chances of direct armed combat exposure and as such are informative about the experiences of those who served in the conflict zone.

VI. Results

VI.I. The Impact of Conflict Exposure on Preferences

Table 2 displays our results on elicited economic preferences. In Panel A, we perform the unadjusted estimates. Then, in Panels B and C, we sequentially add CRA variables and the full set of control variables, respectively. In Panel B, we find that controlling for CRA covariates produces

statistically similar coefficients to those presented in Panel A. Then, in Panel C, the inclusion of the full set of control variables leads to statistically indistinguishable but more precisely estimated coefficients. The observed pattern of results between Panels B and C is in line with the identifying assumptions of our natural experiment, implying that holding CRA variables constant, pre-deployment characteristics should have no influence on the impact of *Conflict Zone* on our outcomes of interest.

The first three columns present the risk estimates. Columns (4) to (6) display the ambiguity results. Finally, columns (7) to (10) show the time-preference findings.

Our fully specified estimates in Panel C show that conflict zone deployment decreases the probability of being a risk-averse individual by 4.6 percentage points ($p < 0.05$) (column 1) and increases the likelihood of being a risk-loving person by 5.5 percentage points ($p < 0.05$) (column 3), respectively, with no significant effect on risk neutrality (column 2).

Column (5) finds that *Conflict* increases ambiguity neutrality by 8.5 percentage points ($p < 0.01$). Then, column (6) displays that conflict zone deployment lowers the probability of being an ambiguity lover by 5.1 percentage points ($p < 0.001$). These estimates show that conflict zone deployment increases ambiguity neutrality (largely) at the expense of ambiguity seeking.

Next, in column (7) we find that conflict zone service causes a 0.092 standard deviation ($p < 0.01$) decline in the discount rate. In column (8), we document that *Conflict* increases the chances of being a time-consistent individual, i.e., being an exponential discounter, by 4.5 percentage points ($p < 0.05$). In column (9), we document that service in the state of emergency area lowers the likelihood of being future biased by 4.6 percentage points ($p < 0.05$) indicating that the improvement in exponential discounting is fully explained by the decrease in the probability of being future biased. Finally, column (10) shows that conflict zone service does not have any impact on being a hyperbolic discounter, i.e., being present-biased.

VI.II. Robustness

We perform several exercises to test the resilience of estimates. We start by restricting the sample to more uniform subsamples, allowing us to create cleaner subsets that facilitate conditional random assignment. If our estimates remain consistent within these more homogeneous groups, our confidence in the estimations will increase. In Panel A of Online Appendix Table 6, we restrict our sample to individuals with, at most, a high school education who served a full term and were not eligible to delay induction. In Panel B, we restrict the analysis sample to individuals who were conscripted at the normal age of conscription, i.e., before their 22nd birthday. In Panel C, we restrict

the sample to those with Turkish ethnicity. In Panel D, we reproduce our results using those who reside in their birth provinces. In Panel E, we limit our sample to individuals who were likely to be inducted after completing formal schooling. These exercises produced a similar pattern of results to our main findings, boosting our confidence in our findings and the credibility of our natural experiment in identifying the impact of conflict zone assignment.

Next, we employ a non-linear estimation technique, specifically the logit model, to assess the impact of conflict zone assignment on binary outcome variables. The marginal effects of conflict zone assignment, presented in Online Appendix Table 7, are similar to our baseline estimates.

In Online Appendix Table 8, we explore the resilience of our results to adjusting standard errors for clustering at alternative levels, including training province, service district, two-way adjustment at service province and induction year, birth province, branch by draft year, branch by occupation, branch by occupation by draft year. Results show that our inference is robust to employing these arguably relevant clustering units.

In Online Appendix Table 9, we further scrutinize the resilience of our findings by comparing individuals born in the same induction year. In doing so, we control for the birth province by military induction year fixed effects. Despite a significant reduction in the degrees of freedom, these estimates produced results qualitatively similar to our main estimates.

Finally, in Online Appendix Table 10, we undertake Oster's (2019) selection on observable and unobservable characteristics test. This method evaluates the extent to which unobservable factors are linked to conflict exposure, compared to observable factors, to explain the coefficient of interest by tracking variations in the coefficient of interest and the R-squared statistic in response to the inclusion of control variables. Results recommend that our findings cannot be accounted for by unobservable determinants of risk, ambiguity, and time preferences.

We further test the robustness of our experimental findings by investigating their sensitivity to nonstandard responses, order of the games, and to alternatively constructed measures. In multiple price list experiments, it is common to observe some subjects to demonstrate multiple switch points.[26] We interpret multiple switching as confusion or loss of attention and, therefore, our main analyses focus on subjects with at most one switching point. Similarly, in measuring time preferences, we exclude respondents who did not indicate an amount that would render them indifferent between imminent and future payments. If, however, conflict zone assignment

[26] Multiple switching includes those cases where a subject "switches" from the lottery to the safe option in the first decision and then "switches back" to the lottery in a later decision. This kind of behavior violates monotonicity and cannot be explained by standard utility functions (except if one assumes idiosyncratic errors).

influences the likelihood of such non-standard responses, our estimates could be biased. In light of this, we investigate whether providing non-standard responses in our field games is affected by conflict zone assignment in Online Appendix Table 11. In column 1, the dependent variable is an indicator that takes the value of 1 if respondents display inconsistency in games involving risk and ambiguity by switching to the sure amount multiple times. Columns (2) to (4) show the impact of conflict zone exposure on non-standard responses for time preference questions. Column (2) presents the likelihood of answering "No amount is enough," while column (3) shows the probability of answering "Do not know." Finally, column (4) explores all non-standard responses from either the risk or time preference experiment. The statistically insignificant estimates suggest that, overall, the conflict zone does not influence the probability of a non-standard response.

In the game involving risk and ambiguity, some respondents were randomly given the risk game first, followed by the ambiguity game, while others played the ambiguity game first and then the risk game. To explore the sensitivity of our findings to the order of the risk versus ambiguity game, we estimate our risk and ambiguity specifications for risk-first and ambiguity-first samples in Panels A and B of Online Appendix Table 12. This exercise produces qualitatively similar estimates, recommending that the order in which the risk and ambiguity games were played does not dictate our findings.

Another concern about our design might be that respondents who, regardless of the bag, always chose the sure amount or never switched to the sure amount might have done so due to the limitations imposed by the available action space, and that our risk and ambiguity results are driven mainly by the responses at the corners. In Online Appendix Table 13, we re-estimate our risk and ambiguity specification without including those who switched in the first round, who never switched, and both groups, respectively. This exercise led to qualitatively similar results to our baseline findings.

VII. The Impact of Conflict on Real-Life Outcomes

In this section, we explore whether we can trace the implications of our experimental findings in real-life by examining the impact of conflict zone assignment on economic and health behaviors that, based on previous theoretical, empirical, and experimental literatures, are expected to be shaped by the individual preferences we analyze. Specifically, on the economic behaviors side we investigate entrepreneurship (Stein and Segal, 2006; Hoy et al., 2014; Holm et al., 2013; Dimmock et al., 2016; Bonilla and Cubillos, 2021), savings (Chatterjee. et al., 2017; Finke and Huston, 2003), and insurance demand (Hakansson, 1969; Szpiro, 1985), and on the health

behaviors side we analyze being overweight, smoking, and daily drinking (Anderson and Mellor, 2008; Cutler and Glaeser, 2005; Dohmen et al., 2011; Falk et al. 2018).

Our first economic outcome measure *Entrepreneur* is a binary indicator of whether the respondent had ever attempted to set up his own business. *Savings* is the logarithmic transformation of monthly savings. *Private Pension* and *Private Health Insurance* are binary indicators of investing in such retirement and health insurance policies, respectively.

Finally, we analyze risky health behaviors through binary indicators of obesity, smoking, and daily drinking. *Overweight or Obese* is a binary indicator of Body Mass Index (BMI) of 25 or above. *Smoker* is a binary indicator of whether the respondent smokes. *Daily Drinker* is a binary indicator of almost daily alcohol consumption.

Online Appendix Table 5 presents the descriptive statistics for outcome variables as well as the potential mediators we consider. Column (1) displays the mean values for the full sample. Columns (2) and (3) show the summary statistics for those who served outside and in the conflict zone, respectively.

In Table 3, we examine the impact of conflict area deployment on measures of economic behaviors. Consistent with our experimental findings, we find that conflict exposure causes a 3.6 percentage point increase in entrepreneurship which once again suggests elevated risk taking, but the elevation do not extend into risky health behaviors. Our estimates do not indicate any statistically significant impact of conflict zone service on monthly savings (column 2), the probability of investing in a private pension plan (column 3) or having a private health insurance policy (column 4).

Finally, results in Table 4 do not indicate any statistically significant impact of conflict zone experience on being overweight or obese (column 1), smoking (column 2), and daily drinking (column 3).

VIII. Potential Mechanisms

To sum up, our experimental findings reveal elevated risk tolerance alongside patience and consistency in exposed individuals. Consistent with these findings, our behavioral observations indicate some financial risk taking in real life, but we do not observe any outcomes that would indicate excessive risk taking. In this subsection, we explore the potential mechanisms that may explain changes in preferences as well as the relative stability of real-life behavior. As discussed earlier, our research design eliminates potential macroenvironmental explanations, such as

community-level paradigm shifts, resource shortages, postwar economic growth, and security concerns. We, therefore, turn our attention to potential mechanisms at the individual level.

We start our exploration with the potential mental health consequences of exposure that might then reflect on economic preferences and outcomes. Depression and anxiety have been shown to affect cognition and probability assessment under risk and uncertainty, suggesting an influence on risk attitude as well (Sharot et al., 2007; Cobb-Clark et. al., 2021). Psychological distress is also known to be a driver of risky health behaviors (Tanielian and Jaycox, 2008; Cesur et al., 2013). Our first measure of psychological health is an index of depression derived from 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. We normalized the total score to construct an index with mean 0 and standard deviation 1.

We also explore the effects of exposure on anxiety by examining personal safety perceptions. Our measure, the *Personal Security Index*, is the total score (normalized to have a distribution with mean 0 and standard deviation 1) on an 8-item security scale (Vélez et al., 2016) that requires participants to indicate their perceptions of personal safety in daily life.[27]

In Online Appendix Table 14, we formally test via univariate regressions whether our experimental measures correlate with these psychological outcomes in the expected ways. In column (1) we find risk taking, patience, and time consistency to negatively associate with the *Depression Index*, and consistent with these findings, we observe them to positively associate with perceptions of safety in Column (2).

The estimated coefficient in column (1) of Table 5, however, shows no statistically significant impact of conflict exposure on depressive symptomology, nor do we observe a statistically significant impact of exposure on perceptions of personal security.

The null effects of conflict exposure we observe on these potential psychological channels provide compelling evidence pertaining to why conflict zone deployment did not lead to an increase in risky health behaviors.

[27] The questions respectively ask the respondents to indicate on a 5-point Likert scale how much they feel the following statements apply to them: "I feel safe when out at night"; "I might encounter life threatening situations where I live"; "I am fearful for my life; I think there are security risks to attending political meetings"; "I am fearful of being robbed during the day"; "I am fearful of being robbed in the night"; "I am fearful of encountering violence on the street in the day"; "I am fearful of encountering violence on the street in the night." We recoded the answers to create a scale that is increasing in feeling safe.

With no evidence to suggest any negative mental health effects, we next consider the other side of the coin and following up on the post-traumatic growth arguments, explore whether exposed individuals display any psychological empowerment and personal growth which may then explain our results.

Our measure of empowerment and growth is based on recent works that positively associate risk tolerance (Kesavayuth et al., 2018; Salamanca et al., 2016; Breuer et al, 2012; Illiashenko, 2019; Dohmen et al., 2010), patience (Gneezy et al., 2020), and time consistency (Benabou and Tirole 2002) with agency defined as having a sense of control over one's choices and life. Given that military service, and combat service in particular, has been found to foster independence, and self-control, efficacy, and awareness (Dar and Kimhi, 2001; Shahrabani and Garyn-Tal, 2019), an improved sense of agency then can be one potential explanation for our findings.

Accordingly, our index is based on survey items capturing social confidence, empowerment, and internal locus of control in life. Social confidence is measured via a survey item that required the participants to indicate on a 5-point Likert scale how accurately the statement "*I sometimes feel that people are laughing at me behind my back*," depicts them with answers ranging from "*not at all*" (1) to "*exactly*" (5). We construct an indicator variable, *Social Confidence,* that takes a value of 1 if the response is equal to 1 and 0 otherwise. Similarly, *Owning One's Path* is measured through a survey item that requires the participant to indicate on the same 5-point Likert scale how much they agree with the statement "*There is no single right way to live, everyone should make their own path*." We binarize the indicator to 1 if the answer is above 3, and 0 otherwise. Finally, the binary *Internal Locus of Control* variable is based on a survey item that required respondents to indicate on a 7-point Likert scale how much control they feel they have over their lives with possible answers ranging from 1 (*Fate has complete control*) to 7 (*I have complete control*). *Internal Locus of Control* equals 1 if the response is 6 or higher, and 0 otherwise. The Agency Index is the summation of these three measures normalized to mean zero and standard deviation of one.

We show in the third column of Appendix Table 14 that, in line with the associated literature, the Agency Index is positively correlated with risk tolerance and consistency. Then in Table 5 (column 3), we find that conflict exposure increases the *Agency Index* by 0.10 standard deviations. Note that this positive impact on the sense of agency may be responsible for the attenuation of the transmission of the positive effects of conflict exposure on risk tolerance to risky health behavior as the sense of agency, considered as a signal of PTG (Tedeschi and Calhoun, 2004), can counterbalance the adverse impacts by preventing inconsistency and excessive risk-taking (Gneezy et al., 2020).

While these results nominate exposure-induced changes in personality as a mechanism, another pathway might be through socioeconomic characteristics. We therefore continue our investigations by examining the labor market performance of veterans. We find in Table 5 that service in the conflict zone has a modest positive effect on unemployment and a negative effect on family income. However, the estimated coefficients in Appendix Table 14 do not indicate any meaningful correlations between unemployment and our preference measures. And the correlations we observe for family income indicate that if anything the reduction in income due to conflict exposure is counteracting the effects we observe on preferences. We should nonetheless point out that, as unemployment is positively associated with entrepreneurship (Fairlie, 2013), conflict exposure-driven unemployment could explain our positive finding on entrepreneurship. Higher unemployment and lower family income might also explain why we do not observe higher savings and investment in retirement and health insurance. Having noted this, we cannot rule out the possibility that the adverse impacts on unemployment and income could also be driven by increased risk-tolerance (i.e., taking on sub-optimally large economic risks) in the first place.

In summary, individuals exposed to conflict may develop a heightened sense of agency, leading to increased risk tolerance and patience while also becoming more ambiguity-neutral and time-consistent. Since these preference changes can have opposing effects on real-life behavior, it is not surprising that conflict-exposed individuals do not necessarily engage in risky health behaviors. This highlights the importance of examining how conflict exposure influences a broad range of preferences rather than focusing solely on risk preferences.

IX. Conclusion

In this study, we examine the causal impact of armed conflict exposure on the risk preferences and behavior of the average adult male randomly picked from a population inhabiting a peaceful environment where conflict-induced macroenvironmental transformations are absent. We exploit a novel natural experimental setting with an innovative survey design that incorporates lab-in-the-field experiments to identify clean and isolated individual-level exposure to armed conflict while nullifying the potential effects of war that may operate through the social and economic ecology. Therefore, we identify the effect on preferences and behavior, and the individual level explanatory channels these effects work through without the confounding role of the macroenvironmental effects of war such as community-level paradigm shifts, resource shortages, postwar economic growth, and security concerns.

By combining the natural and lab-in-the-field experiments, we demonstrate that exposure to armed conflict causes elevated risk tolerance alongside patience and consistency in preferences. Consistent with these findings, our behavioral observations indicate some financial risk taking in real life, but we observe no outcomes that would indicate excessive risk taking. We then present evidence that nominates exposure-induced changes in personality, specifically an enhanced sense of agency, as a potential explanation for the overall profile of effects we observe.

Our paper has several important strengths. First, we offer a causal and generalizable account of the individual-level economic effects of armed conflict exposure on a rich array of elicited preferences and real-life economic outcomes that are expected to be shaped by these preferences. To our knowledge, we are the first to investigate the effects on ambiguity preferences; and the first to go beyond assessing the impact on patience to explore how exposure shapes time consistency, present bias, and future bias.

Second, our findings are immune to the potential environmental confoundment pervasive in the literature as we compare the exposed and unexposed members of communities which themselves are untouched by the conflict. Consequently, our analysis provides compelling evidence on the potential individual-level explanatory pathways.

Building on these strengths, we identify a novel mechanism—agency—that aligns with the changes we observe in individual preferences. We also note that these preference shifts are consistent with the stability of behaviors observed in the field. While increased risk-taking might lead to riskier health behaviors, improved patience, greater time consistency and reduced ambiguity-loving tendencies could counteract these effects, ultimately resulting in stable overall behavior.

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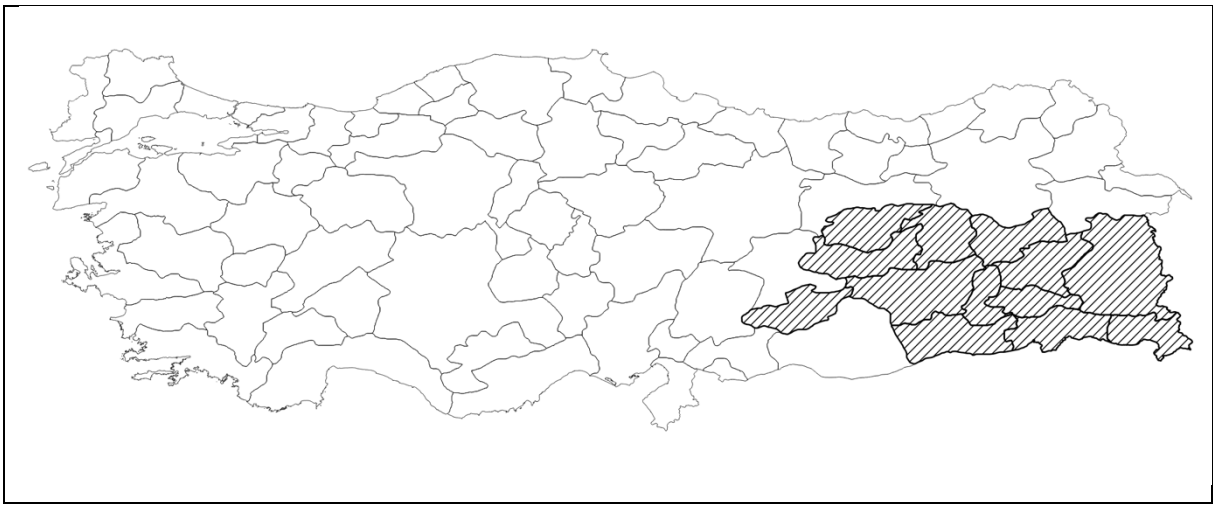
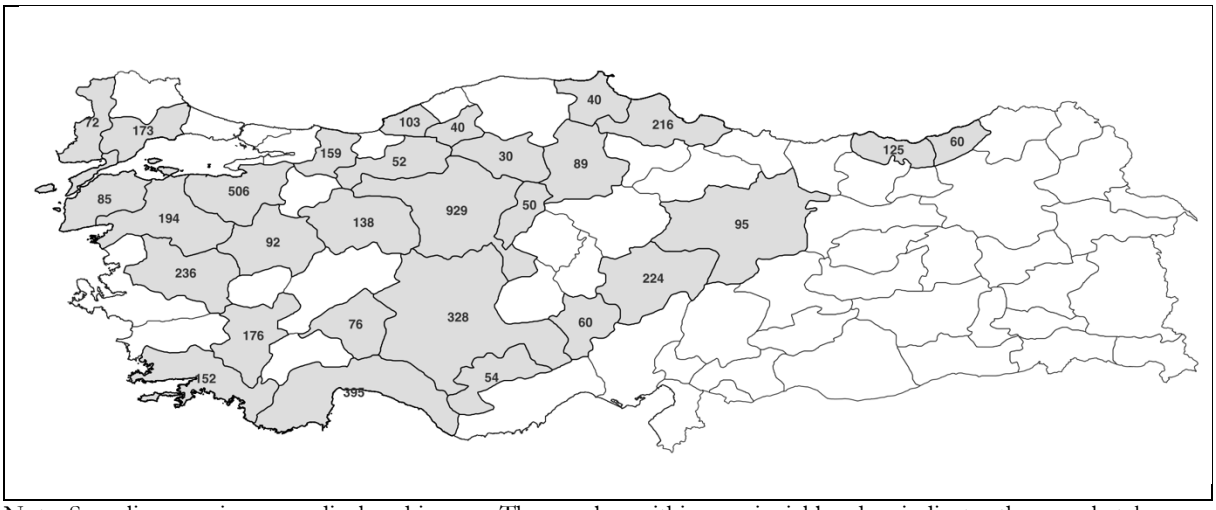
Figure I. Map of the Conflict Zone



Note: The dark highlighted Conflict Zone refers to the state of emergency (OHAL) region declared by the Turkish State. The OHAL region includes provinces Adıyaman, Batman, Bingöl, Bitlis, Diyarbakır, Elazığ, Hakkari, Mardin, Muş, Siirt, Şırnak, Tunceli, and Van.

Figure II. Sampling Distribution



Note: Sampling provinces are displayed in gray. The number within provincial borders indicates the sample taken from that province. Provinces not sampled are shown in white.

Table 1. Evidence on the Exogeneity of Armed Conflict Zone Deployment

VARIABLES	(1) Full Sample Conflict vs. Non-conflict	(2) Risk Sample Conflict vs. Non-conflict
Birth-Quarter: Second	-0.0058	0.0076
	(0.0139)	(0.0266)
Birth-Quarter: Third	-0.0029	0.0204
	(0.0182)	(0.0231)
Birth-Quarter: Fourth	-0.0238	-0.0198
	(0.0212)	(0.0231)
Landowner	-0.0066	0.0120
	(0.0180)	(0.0266)
Kurdish	0.0405	0.0514
	(0.0346)	(0.0345)
Induction Age	0.0029	-0.0032
	(0.0051)	(0.0080)
Rank: Corporal	-0.0360	-0.0495
	(0.0372)	(0.0304)
Rank: Sergeant	-0.0050	-0.0011
	(0.0226)	(0.0454)
Sub-Lieutenant	-0.0844	-0.0582
	(0.0823)	(0.2038)
Training Duration	0.0525***	0.0459**
	(0.0152)	(0.0226)
Service Duration	-0.0144*	-0.0144
	(0.0074)	(0.0100)
Observations	4,068	2,096
R-squared	0.147	0.216
F-test	1.322	1.058
Prob > F	0.238	0.403

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control for conditional random assignment covariates including height, and fixed effects for military occupation, the branch of service, half-term service, the draft year by educational attainment, birth province.

Table 2. The Impact of Conflict Exposure on Risk, Ambiguity, and Time Preferences

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Discount Rate	(8) Time Consistent	(9) Future Biased	(10) Present Biased
Panel A. No controls										
Conflict	-0.030	-0.011	0.041*	-0.037*	0.079***	-0.041***	-0.053*	0.044***	-0.048**	0.004
	(0.024)	(0.008)	(0.024)	(0.019)	(0.020)	(0.012)	(0.028)	(0.016)	(0.019)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
R-squared	0.007	0.004	0.005	0.028	0.006	0.029	0.001	0.002	0.003	0.000
Panel B. CRA Variables										
Conflict	-0.038*	-0.013	0.050**	-0.037	0.089***	-0.052***	-0.084***	0.043***	-0.041**	0.002
	(0.021)	(0.009)	(0.023)	(0.023)	(0.025)	(0.014)	(0.030)	(0.016)	(0.016)	(0.008)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
R-squared	0.207	0.166	0.202	0.191	0.205	0.215	0.132	0.206	0.195	0.110
Panel C. Full Set of Exogenous Covariates										
Conflict	-0.046**	-0.012	0.055**	-0.034	0.085***	-0.051***	-0.092***	0.045**	-0.046**	0.001
	(0.022)	(0.009)	(0.023)	(0.025)	(0.026)	(0.015)	(0.034)	(0.017)	(0.018)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
R-squared	0.256	0.170	0.246	0.228	0.248	0.277	0.181	0.232	0.227	0.131

Notes: Standard errors, clustered on the military service province, are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. CRA variables include height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. The full set of exogenous covariates includes birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration in addition to CRA covariates.

Table 3. The Impact of Conflict Exposure on Real-Life Economic Behaviors

VARIABLES	(1) Entrepreneur	(2) Savings	(3) Private Pension	(4) Private Health Insurance
Conflict zone	0.036*	0.000	-0.001	0.008
	(0.022)	(0.024)	(0.017)	(0.009)
Observations	4,057	3,763	4,068	4,060
R-squared	0.180	0.210	0.146	0.133

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables, including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA variables, all columns account for the full set of exogenous covariates, including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training length, and service duration.

Table 4. The Impact of Conflict Exposure on Real-Life Health Behaviors

VARIABLES	(1) Overweight or Obese	(2) Smoker	(3) Daily Drinker
Conflict zone	0.012	0.011	0.009
	(0.020)	(0.024)	(0.006)
Observations	4,063	4,068	4,063
R-squared	0.145	0.139	0.095

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables, including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA variables, all columns account for the full set of exogenous covariates, including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training length, and service duration.

Table 5. The Impact of Conflict Exposure on Potential Pathways

VARIABLES	(1) Depression Index	(2) Personal Security Index	(3) Agency Index	(4) Unemployed	(5) Family Income
Conflict zone	0.036	-0.029	0.102**	0.016*	-0.040*
	(0.038)	(0.037)	(0.046)	(0.009)	(0.022)
Observations	4,068	4,068	3,972	4,066	3,530
R-squared	0.189	0.191	0.184	0.124	0.239

Notes: Standard errors height, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables, including, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA variables, all columns account for the full set of exogenous covariates, including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training length, and service duration.

Online Appendix (not for publication)

Appendix Table 1. Estimates of Armed Conflict Exposure Among Turkish Men Conscripted between 1984 and 2011

Direct Combat Experiences	Direct Combat Exposure Risk	Total Conscripted Men With Exposure Risk	Total Number of Exposed
Armed Combat	16.2%	14,072,760	2,279,787
Injured	2.1%	14,072,760	295,528
Witnessed Casualties	15.1%	14,072,760	2,124,987
Any Direct Combat Experience	21.4%	14,072,760	3,011,571

Notes: These calculations are based on the following numbers. About 16,120,000 (roughly 620,000 per year) male births occurred between 1965 and 1990. During this period, about 97% of Turkish men performed military service as conscripts, with close to 90% being eligible to serve in conflict zones. Therefore, about 14,072,760 men were conscripted and had the risk of exposure to direct armed combat between 1984 and 2011. The total number of exposed is calculated by multiplying the total number of conscripted men with the associated exposure risk.

Appendix Table 2. Correlations of Experimentally Elicited Preferences and Self-Assessed Attitudes

VARIABLES	(1) Financial Risk Attitude	(2) General Risk Attitude	(3) Patience Index	(4) Impulsivity Index
Risk Averse	-0.070***	-0.035	-0.040	0.108
	(0.022)	(0.022)	(0.043)	(0.159)
Observations	2,091	2,077	2,096	2,096
R-squared	0.005	0.001	0.000	0.000
Risk Neutral	-0.034	-0.241***	-0.103	0.379***
	(0.063)	(0.064)	(0.127)	(0.126)
Observations	2,091	2,077	2,096	2,096
R-squared	0.000	0.007	0.000	0.004
Risk Lover	0.074***	0.064***	0.052	-0.274*
	(0.022)	(0.022)	(0.044)	(0.160)
Observations	2,091	2,077	2,096	2,096
R-squared	0.006	0.004	0.001	0.001
Ambiguity Averse	-0.010	-0.084***	-0.131**	0.711***
	(0.029)	(0.029)	(0.057)	(0.209)
Observations	2,041	2,027	2,046	2,046
R-squared	0.000	0.004	0.003	0.006
Ambiguity Lover	-0.042	-0.050	-0.010	0.934***
	(0.035)	(0.035)	(0.070)	(0.255)
Observations	2,041	2,027	2,046	2,046
R-squared	0.000	0.004	0.003	0.007
Ambiguity Neutral	0.028	0.084***	0.099**	-0.955***
	(0.024)	(0.024)	(0.048)	(0.048)
Observations	2,041	2,027	2,046	2,046
R-squared	0.001	0.006	0.002	0.014
Discount Rate	0.010	0.006	-0.049***	0.374***
	(0.008)	(0.008)	(0.016)	(0.057)
Observations	4,052	4,035	4,068	4,068
R-squared	0.000	0.000	0.002	0.011
Time consistent	0.038**	0.036**	0.108***	-0.718***
	(0.018)	(0.018)	(0.035)	(0.129)
Observations	4,052	4,035	4,068	4,068
R-squared	0.001	0.001	0.002	0.008
Future biased	-0.045**	-0.046**	-0.140***	0.677***
	(0.019)	(0.019)	(0.038)	(0.140)
Observations	4,052	4,035	4,068	4,068
R-squared	0.001	0.001	0.003	0.006
Present biased	0.006	0.012	0.047	0.536**
	(0.035)	(0.035)	(0.070)	(0.257)
Observations	4,052	4,035	4,068	4,068
R-squared	0.000	0.000	0.000	0.001

Notes: Standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 3. Descriptive Statistics for Balance Variable

Variable	(1) All	(2) Non-Conflict Zone	(3) Conflict Zone
Panel A. Full Sample			
Birth-Quarter: Second	0.2515	0.2535	0.2455
	(0.434)	(0.4351)	(0.4306)
Birth-Quarter: Third	0.238	0.2355	0.2455
	(0.4259)	(0.4244)	(0.4306)
Birth-Quarter: Fourth	0.1641	0.1674	0.1541
	(0.3704)	(0.3734)	(0.3612)
Landowner	0.2123	0.2173	0.1972
	(0.409)	(0.4125)	(0.3981)
Kurdish	0.0659	0.0664	0.0645
	(0.2481)	(0.249)	(0.2457)
Induction Age	20.7047	20.7362	20.609
	(1.7777)	(1.8325)	(1.597)
Rank: Corporal	0.0597	0.0624	0.0516
	(0.237)	(0.242)	(0.2213)
Rank: Sergeant	0.13	0.1314	0.126
	(0.3364)	(0.3379)	(0.332)
Sub-Lieutenant	0.0086	0.0088	0.0079
	(0.0924)	(0.0935)	(0.0888)
Training Duration	2.6322	2.5922	2.7537
	(0.7856)	(0.8132)	(0.6813)
Service Duration	16.426	16.3984	16.5099
	(2.7374)	(2.8359)	(2.4131)
Observations	4068	3060	1008
Panel B. Risk Game Sample			
Birth-Quarter: Second	0.257	0.257	0.255
	(0.437)	(0.437)	(0.436)
Birth-Quarter: Third	0.240	0.233	0.257
	(0.427)	(0.423)	(0.437)
Birth-Quarter: Fourth	0.172	0.175	0.164
	(0.377)	(0.380)	(0.371)
Landowner	0.211	0.215	0.200
	(0.408)	(0.411)	(0.401)
Kurdish	0.070	0.071	0.067
	(0.255)	(0.256)	(0.250)
Induction Age	20.630	20.684	20.475
	(1.715)	(1.792)	(1.461)
Rank: Corporal	0.066	0.072	0.050
	(0.249)	(0.258)	(0.219)
Rank: Sergeant	0.131	0.133	0.126
	(0.338)	(0.340)	(0.333)
Sub-Lieutenant	0.010	0.010	0.011
	(0.100)	(0.098)	(0.105)
Training Duration	2.630	2.585	2.762
	(0.771)	(0.805)	(0.646)
Service Duration	16.489	16.432	16.654
	(2.601)	(2.724)	(2.202)
Observations	2096	1558	538

Standard deviations are in parentheses.

Appendix Table 4. The Impact of Conflict Zone Deployment on Direct Armed Combat Involvement

	(1)	(2)	(3)	(4)
VARIABLES	Any Direct Armed Combat	Enemy Firefight	Witnessing Casualties	Injury
Panel A. Full Sample				
Conflict zone	0.395***	0.340***	0.288***	0.051***
	(0.046)	(0.043)	(0.037)	(0.012)
Observations	4,065	4,065	4,053	4,063
R-squared	0.334	0.326	0.279	0.126
Panel B. Risk Sample				
Conflict zone	0.416***	0.368***	0.308***	0.058***
	(0.055)	(0.049)	(0.040)	(0.013)
Observations	2,094	2,094	2,090	2,094
R-squared	0.408	0.400	0.345	0.223

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables, including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA, all columns account for the full set of exogenous covariates, including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 5. Descriptive Statistics for Outcome Variables

	(1)	(2)	(3)
Variable	All	Non-Conflict Zone	Conflict Zone
Panel A. Risk Sample			
Risk Averse	0.529	0.537	0.506
	(0.499)	(0.499)	(0.500)
Risk Neutral	0.030	0.033	0.022
	(0.171)	(0.178)	(0.148)
Risk Lover	0.441	0.430	0.472
	(0.497)	(0.495)	(0.500)
Ambiguity Averse	0.180	0.189	0.155
	(0.384)	(0.391)	(0.362)
Ambiguity Neutral	0.709	0.689	0.767
	(0.454)	(0.463)	(0.423)
Ambiguity Lover	0.111	0.122	0.078
	(0.314)	(0.328)	(0.269)
Observations	2096	1558	538
Panel B. Whole sample			
Discount Rate I	0.637	0.6496	0.5987
	(1.7974)	(1.907)	(1.4138)
Discount Rate II	0.8098	0.8474	0.6957
	(2.2951)	(2.409)	(1.9047)
Discount Rate	0.7234	0.7485	0.6472
	(1.9025)	(2.0151)	(1.5086)
Time Consistent	0.7367	0.7258	0.7698
	(0.4405)	(0.4462)	(0.4211)
Future Bias	0.2109	0.2229	0.1746
	(0.408)	(0.4162)	(0.3798)
Present Bias	0.0524	0.0513	0.0556
	(0.2228)	(0.2207)	(0.2292)
Observations	4068	3060	1008
Panel C. Other Variables			
Agency Index	1.573	1.5658	1.5947
	(.7826)	(.7838)	(.7787)
Depression Scale	4.2586	4.1683	4.5327
	(5.5547)	(5.4465)	(5.8652)
Patience Index	7.1264	7.1526	7.0466
	(2.0963)	(2.0716)	(2.1688)
Impulsivity Index	10.3233	10.3141	10.3512
	(3.6457)	(3.6596)	(3.6051)
Overweight or Obese	.5774	.5759	.5819
	(.494)	(.4943)	(.4935)
Smoker	.66	.6578	.6667
	(.4738)	(.4745)	(.4716)
Husband Daily Drinker	.03	.0285	.0348
	(.1707)	(.1663)	(.1833)
General Risk Attitude	.4506	.4503	.4515
	(.4976)	(.4976)	(.4979)
Financial Risk Attitude	.537	.533	.5494
	(.4987)	(.499)	(.4978)
Entrepreneur	.4853	.4831	.492
	(.4998)	(.4998)	(.5002)
Saving	1.5578	1.55	1.5815
	(1.1503)	(1.1429)	(1.1729)
Family Income	3775.921	3810.169	3672
	(2035.235)	(2034.399)	(2035.403)

	(1)	(2)	(3)
Variable	All	Non-Conflict Zone	Conflict Zone
Private Health Insurance	.0759	.073	.0846
	(.2648)	(.2602)	(.2784)
Private Pension	.1207	.1193	.125
	(.3258)	(.3242)	(.3309)
Years of Schooling	9.2858	9.3727	9.0218
	(3.3025)	(3.3052)	(3.2822)
Observations	4068	3060	1008

Standard deviations are in parentheses.

Appendix Table 6. The Impact of Conflict Exposure on Risk, Ambiguity, and Time Preferences, Robustness to 'Cleaner' Subsamples

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Discount Rate	(8) Time Consistent	(9) Future Biased	(10) Present Biased
Panel A. High School Sample										
Conflict zone	-0.036	-0.019*	0.055**	-0.043*	0.087***	-0.043***	-0.060**	0.038**	-0.041**	0.003
	(0.024)	(0.010)	(0.026)	(0.025)	(0.027)	(0.015)	(0.030)	(0.016)	(0.017)	(0.010)
Observations	1,796	1,796	1,796	1,751	1,751	1,751	3,432	3,432	3,432	3,432
R-squared	0.206	0.111	0.203	0.179	0.203	0.212	0.129	0.224	0.214	0.105
Panel B. Inducted < Age 22 Sample										
Conflict zone	-0.037	-0.011	0.048*	-0.038	0.084***	-0.046***	-0.073**	0.038**	-0.040**	0.002
	(0.024)	(0.010)	(0.025)	(0.024)	(0.026)	(0.016)	(0.031)	(0.018)	(0.019)	(0.009)
Observations	1,848	1,848	1,848	1,803	1,803	1,803	3,522	3,522	3,522	3,522
R-squared	0.245	0.167	0.242	0.212	0.234	0.239	0.150	0.239	0.229	0.125
Panel C. Only Ethnically Turkish Sample										
Conflict zone	-0.034	-0.014	0.048**	-0.042*	0.085***	-0.043***	-0.067**	0.040**	-0.047**	0.006
	(0.022)	(0.010)	(0.022)	(0.025)	(0.026)	(0.015)	(0.029)	(0.018)	(0.019)	(0.009)
Observations	1,896	1,896	1,896	1,849	1,849	1,849	3,718	3,718	3,718	3,718
R-squared	0.235	0.182	0.229	0.219	0.236	0.244	0.147	0.229	0.223	0.131
Panel D. Non-migrant Sample										
Conflict zone	-0.030	-0.016	0.046*	-0.024	0.085***	-0.061***	-0.060**	0.043**	-0.037*	-0.006
	(0.024)	(0.012)	(0.024)	(0.027)	(0.031)	(0.019)	(0.026)	(0.019)	(0.019)	(0.008)
Observations	1,529	1,529	1,529	1,490	1,490	1,490	3,037	3,037	3,037	3,037
R-squared	0.239	0.178	0.238	0.230	0.250	0.244	0.127	0.263	0.252	0.131
Panel E. Likely Inducted After Completing Formal Schooling Sample										
Conflict zone	-0.034	-0.018*	0.052**	-0.040*	0.087***	-0.047***	-0.064**	0.040**	-0.042**	0.003
	(0.022)	(0.009)	(0.024)	(0.023)	(0.027)	(0.015)	(0.029)	(0.017)	(0.017)	(0.009)
Observations	1,943	1,943	1,943	1,895	1,895	1,895	3,764	3,764	3,764	3,764
R-squared	0.217	0.158	0.215	0.200	0.218	0.222	0.122	0.219	0.209	0.112

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 7. The Impact of Conflict Exposure on Risk and Ambiguity Preferences, Marginal Effects from Logistic Estimates

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Time Consistent	(8) Future Biased	(9) Present Biased
Conflict zone	-0.046**	-0.045	0.057***	-0.042	0.099***	-0.066***	0.046***	-0.047***	-0.003
	(0.020)	(0.033)	(0.021)	(0.026)	(0.027)	(0.018)	(0.017)	(0.018)	(0.011)
Observations	1,960	565	1,952	1,678	1,835	1,406	3,871	3,789	2,685

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables, including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA variables, all columns account for the full set of exogenous covariates, including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training length, and service duration.

Appendix Table 8. The Impact of Conflict Exposure on Risk, Ambiguity, and Time Preferences, Robustness to Adjusting Standard Errors at Alternative Levels

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Discount Rate	(8) Time Consistent	(9) Future Biased	(10) Present Biased
Panel A. Training Province										
Conflict zone	-0.040	-0.012	0.053**	-0.040**	0.090***	-0.050***	-0.084***	0.046***	-0.047***	0.001
	(0.025)	(0.010)	(0.024)	(0.018)	(0.022)	(0.016)	(0.026)	(0.013)	(0.013)	(0.006)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel B. Service District										
Conflict zone	-0.040	-0.012	0.053**	-0.040**	0.090***	-0.050***	-0.084**	0.046***	-0.047***	0.001
	(0.025)	(0.009)	(0.025)	(0.020)	(0.024)	(0.018)	(0.034)	(0.016)	(0.018)	(0.010)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel C. Two-way Cluster at Service Province and Induction Year										
Conflict zone	-0.040	-0.012	0.053*	-0.040*	0.090***	-0.050**	-0.084**	0.046**	-0.047***	0.001
	(0.026)	(0.010)	(0.027)	(0.022)	(0.029)	(0.018)	(0.034)	(0.020)	(0.017)	(0.010)
R-squared	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel D. Birth Province										
Conflict zone	-0.040	-0.012*	0.053**	-0.040**	0.090***	-0.050*	-0.084**	0.046**	-0.047*	0.001
	(0.025)	(0.007)	(0.025)	(0.019)	(0.032)	(0.025)	(0.035)	(0.022)	(0.024)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel E. Branch by Draft Year										
Conflict zone	-0.040*	-0.012	0.053**	-0.040*	0.090***	-0.050***	-0.084**	0.046**	-0.047***	0.001
	(0.024)	(0.011)	(0.025)	(0.022)	(0.026)	(0.017)	(0.039)	(0.018)	(0.015)	(0.010)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel F. Branch by Occupation										
Conflict zone	-0.040**	-0.012**	0.053***	-0.040	0.090***	-0.050***	-0.084**	0.046***	-0.047***	0.001
	(0.019)	(0.006)	(0.020)	(0.031)	(0.029)	(0.015)	(0.032)	(0.010)	(0.011)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
Panel G. Branch by Occupation by Draft Year										
Conflict zone	-0.040	-0.012	0.053*	-0.040*	0.090***	-0.050***	-0.084**	0.046***	-0.047***	0.001
	(0.026)	(0.009)	(0.028)	(0.023)	(0.028)	(0.016)	(0.035)	(0.017)	(0.017)	(0.008)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 9. The Impact of Conflict Exposure on Risk, Ambiguity, and Time Preferences, Robustness to Controlling for Birth Province by Year of Induction Fixed Effects

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Discount Rate	(8) Time Consistent	(9) Future Biased	(10) Present Biased
Conflict zone	-0.048	-0.022	0.070*	-0.048	0.092**	-0.044	-0.049	0.048**	-0.040*	-0.008
	(0.034)	(0.013)	(0.038)	(0.041)	(0.044)	(0.026)	(0.031)	(0.020)	(0.020)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
R-squared	0.555	0.478	0.558	0.545	0.568	0.555	0.395	0.449	0.443	0.359

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration, and birth province by year of induction fixed effects.

Appendix Table 10. The Impact of Conflict Exposure on Risk, Ambiguity, and Time Preferences, Robustness to Selection on Unobservable

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover	(7) Discount Rate	(8) Time Consistent	(9) Future Biased	(10) Present Biased
Conflict Zone	-0.044	-0.012	0.053*	-0.040	0.091***	-0.051***	-0.070**	0.054***	-0.052**	-0.002
	(0.029)	(0.009)	(0.028)	(0.025)	(0.027)	(0.015)	(0.033)	(0.019)	(0.021)	(0.009)
Observations	2,096	2,096	2,096	2,046	2,046	2,046	4,068	4,068	4,068	4,068
R-squared	0.180	0.170	0.174	0.183	0.186	0.221	0.131	0.127	0.134	0.096
Oster beta	-0.0496	-0.0129	0.0573	-0.0432	0.0968	-0.0535	-0.0770	0.0577	-0.0539	-0.00374

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 11. The Impact of Conflict Exposure on Non-Standard Responses

VARIABLES	(1) Inconsistent Responses in Risk Game	(2) No Amount Enough in Time Game	(3) Do Not Know in Time Game	(4) All Non-Standard Responses (Risk and Time)
Conflict zone	-0.011	0.010	0.013	0.015
	(0.013)	(0.010)	(0.009)	(0.013)
Observations	2,465	4,856	4,856	4,864
R-squared	0.222	0.147	0.141	0.135

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status.

Appendix Table 12. Risk and Ambiguity Estimates by Risky Bag First vs. Ambiguity Bag First

VARIABLES	(1) Risk Averse	(2) Risk Neutral	(3) Risk Lover	(4) Ambiguity Averse	(5) Ambiguity Neutral	(6) Ambiguity Lover
Risk First						
Conflict zone	-0.033	-0.017	0.050*	-0.064**	0.087**	-0.023
	(0.027)	(0.015)	(0.027)	(0.032)	(0.036)	(0.020)
Observations	1,055	1,055	1,055	1,029	1,029	1,029
R-squared	0.329	0.260	0.325	0.304	0.308	0.327
Ambiguity First						
Conflict zone	-0.033	-0.004	0.037	-0.037	0.125***	-0.088***
	(0.048)	(0.014)	(0.045)	(0.030)	(0.046)	(0.033)
Observations	1,041	1,041	1,041	1,017	1,017	1,017
R-squared	0.312	0.220	0.320	0.311	0.330	0.325

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 13. Robustness to Dropping Extreme Answers to Risk and Ambiguity Questions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drop bottom			Drop top			Drop both		
VARIABLES	Ambiguity Averse	Ambiguity Neutral	Ambiguity Lover	Ambiguity Averse	Ambiguity Neutral	Ambiguity Lover	Ambiguity Averse	Ambiguity Neutral	Ambiguity Lover
Conflict zone	-0.032	0.084***	-0.053*	-0.049	0.103***	-0.054**	-0.053	0.113**	-0.060
	(0.029)	(0.032)	(0.027)	(0.030)	(0.031)	(0.021)	(0.073)	(0.054)	(0.071)
Observations	1,188	1,188	1,188	1,319	1,319	1,319	461	461	461
R-squared	0.261	0.300	0.322	0.294	0.308	0.292	0.517	0.533	0.573

Notes: Standard errors, clustered on the province of military service, in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All columns control for conditional random assignment (CRA) variables including height, and binary indicators for educational attainment by draft year, the branch of service, military occupation, birth province, and half-term service status. In addition to CRA all columns account for set of exogenous covariates including birth quarter dummies, land ownership status, Kurdish ethnicity indicator, draft age, military rank dummies, training and service duration.

Appendix Table 14. Correlations of Experimentally Elicited Preferences with Potential Pathways

VARIABLES	(1) Depression Index	(2) Personal Security Index	(3) Agency Index	(4) Unemployed	(5) Family Income
Risk Averse	0.079*	-0.071	-0.109**	0.010	-0.064**
	(0.044)	(0.044)	(0.044)	(0.012)	(0.026)
Observations	2,096	2,096	2,054	2,093	1,835
R-squared	0.002	0.001	0.003	0.000	0.003
Risk Neutral	0.033	-0.079	-0.243*	0.015	0.235***
	(0.127)	(0.130)	(0.129)	(0.034)	(0.084)
Observations	2,096	2,096	2,054	2,093	1,835
R-squared	(0.000)	(0.000)	(0.002)	(0.000)	(0.004)
Risk Lover	-0.084*	0.081*	0.139***	-0.008	0.041
	(0.044)	(0.045)	(0.044)	(0.012)	(0.026)
Observations	2,096	2,096	2054	2,093	1,835
R-squared	0.002	0.002	0.005	0.000	0.001
Ambiguity Averse	0.008	-0.077	-0.314***	-0.017	0.069**
	(0.057)	(0.058)	(0.057)	(0.016)	(0.035)
Observations	2,046	2,046	2004	2,043	1,793
R-squared	0.000	0.001	0.015	0.001	0.002
Ambiguity Lover	-0.070	0.020	-0.370***	-0.015	-0.036
	(0.069)	(0.071)	(0.070)	(0.019)	(0.043)
Observations	2,046	2,046	2004	2,043	1,793
R-squared	0.001	0.000	0.014	0.000	0.000
Ambiguity Neutral	0.028	0.046	0.402***	0.019	-0.033
	(0.048)	(0.049)	(0.048)	(0.013)	(0.029)
Observations	2,046	2,046	2004	2,043	1,793
R-squared	0.000	0.010	0.034	0.001	0.001
Discount Rate	0.026*	-0.050***	0.005	0.004	-0.028***
	(0.016)	(0.015)	(0.016)	(0.004)	(0.009)
Observations	4,068	4,068	3972	4,066	3,530
R-squared	0.001	0.003	0.000	0.000	0.003
Time consistent	-0.067*	0.234***	0.137***	0.016*	-0.036*
	(0.035)	(0.035)	(0.036)	(0.009)	(0.020)
Observations	4,068	4,068	3972	4,066	3,530
R-squared	0.001	0.011	0.004	0.001	0.001
Future biased	-0.032	-0.208***	-0.128***	-0.011	0.055**
	(0.038)	(0.038)	(0.039)	(0.010)	(0.022)
Observations	4,068	4,068	3972	4,066	3,530
R-squared	0.000	0.007	0.003	0.000	0.002
Present biased	0.366***	-0.215***	-0.108	-0.026	-0.049
	(0.069)	(0.069)	(0.071)	(0.019)	(0.041)
Observations	4,068	4,068	3972	4,066	3,530
R-squared	0.007	0.002	0.001	0.000	0.000

Online Data Appendix I: Scientific Ethics Protocols

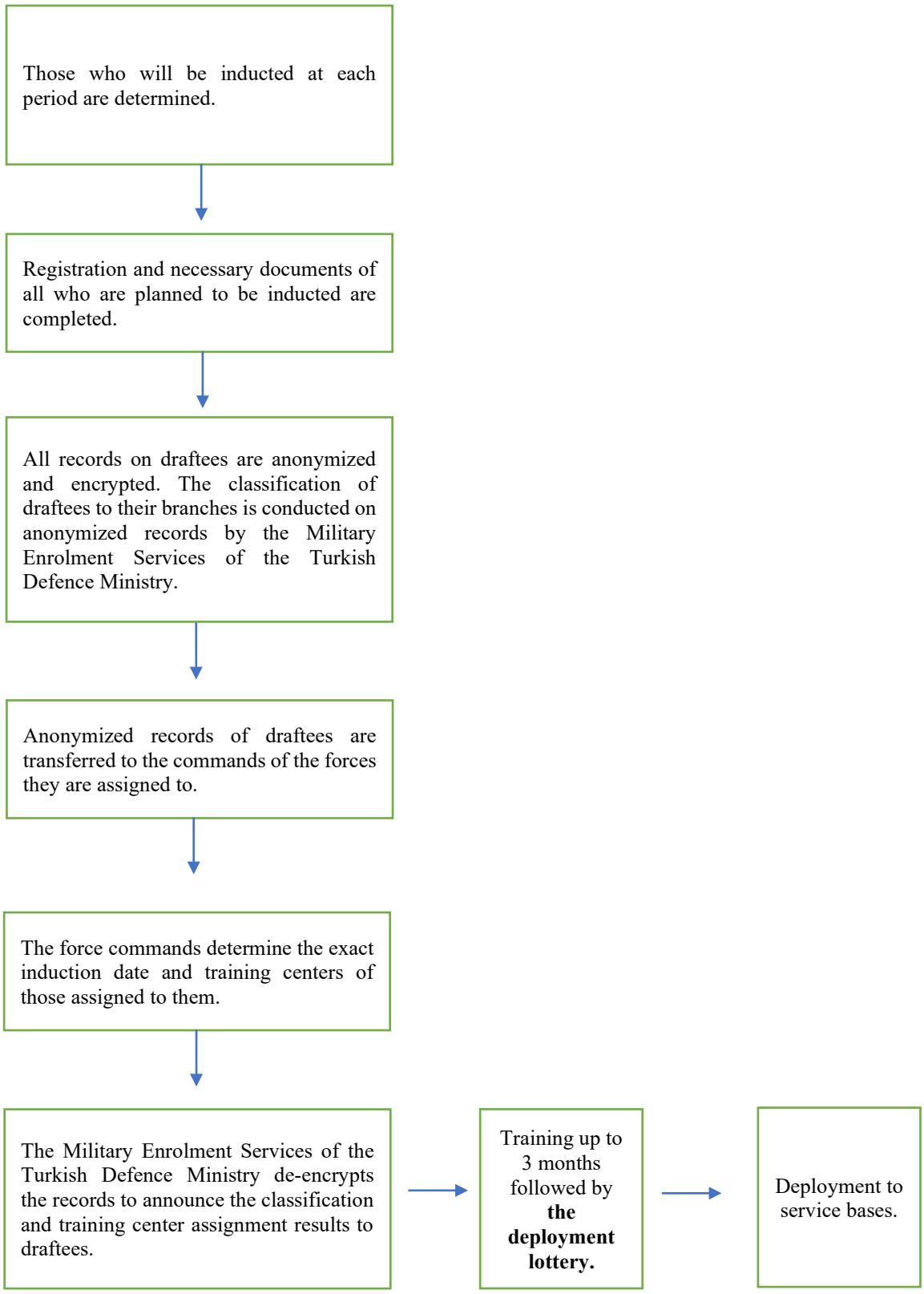
Ethical approvals for the EXPOVIBE project were received from the European Research Council, the University of Warwick, and Sabancı University. The scientific ethics boards of these institutions examined and approved all survey materials, including the questionnaires, informed consent sheets, information pamphlets, interviewer training materials, as well as data protection measures before the fieldwork. The project also had an independent ethics advisory committee composed of five expert scholars overseeing the study design and implementation at every step.

Interviews for the EXPOVIBE survey were conducted in Turkish in private settings by interviewers specially trained on interviewing techniques, survey documents, and scientific ethics protocols related to fieldwork. In addition, the PI accompanied each interviewer on his/her first day on the field to make sure that all implementation rules and procedures were followed correctly.

Informed consent was obtained from all respondents. The consent forms informed the participants about the content, purpose, and length of the study, how the data was going to be maintained and used, and participant rights, and included contact information of the PI as well as those of the scientific ethics officers of the host and partner universities.

Online Data Appendix II: Conscription Classification Procedure by the Turkish Ministry of Defense

https://www.msb.gov.tr/Askeralma/icerik/siniflandirma-islemleri



Online Data Appendix III: Field Experiment Instructions

Version (1): Field experiment on risk and ambiguity attitudes.
Instructions: Now we will play a simple and fun game. You can earn money playing this game.

In this game we have two bags (the participant is shown the bags). In Bag 1, there are 10 marbles—5 red and 5 blue. If you like, I can show you. In Bag 2, there are also 10 red and blue marbles, but we do not know how many of them are red and how many of them are blue. All of them can be red or all of them can be blue, or some of them can be red and some of them are blue. We will use these two bags in an offer game.

Here is the offer game: First, you determine your lucky color.

Now if you draw a marble in your chosen color, then you may win 2,500 TL. You will not win anything if you draw a marble with the other color.

I will make 20 offers to you. Each offer will ask you whether you prefer to draw a marble from a bag or whether you prefer to take a sure amount. I will record your choices here. Later I will ask you to randomly pick one card from this deck of cards numbered from 1 to 20 to determine which offer will be implemented. The amount you earn is determined by your own choice that you made in that randomly determined numbered offer.

We play this donation game with 1,250 participants just like you. When these 1,250 participants' survey forms are being entered to our system, our computer will randomly select one person. This randomly selected participant will receive the amount that they win in our offer game in person as a gift card. This process is under the guarantee of Sabancı University.

Would you like your lucky color to be blue or red?

Stage 1: Sure amount or draw a marble from bag 1? In Bag 1, there are 5 red, 5 blue marbles.

Table III.1

	Draw a marble (Code as 1)	**Sure amount (Code as 2)**	**Would you like to draw a marble from Bag 1 or take the sure amount?**
1	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 600 TL	**[.....]**
2	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 700 TL	**[.....]**
3	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 800 TL	**[.....]**
4	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 900 TL	**[.....]**
5	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1000 TL	**[.....]**
6	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1100 TL	**[.....]**
7	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1200 TL	**[.....]**
8	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1300 TL	**[.....]**
9	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1400 TL	**[.....]**
10	You receive 2,500 TL if the marble you randomly draw from Bag 1 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1500 TL	**[.....]**

Stage 2: Sure amount or draw a marble from Bag 2? In Bag 2, there are 10 marbles, but we do not know how many are blue and how many are red.

Table III.2

	Draw a marble (Code as 1)	**Sure amount (Code as 2)**	**Would you like to draw a marble from Bag 2 or take the sure amount?**
1	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 600 TL	**[.....]**
2	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 700 TL	**[.....]**
3	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 800 TL	**[.....]**
4	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 900 TL	**[.....]**
5	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1000 TL	**[.....]**
6	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1100 TL	**[.....]**
7	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1200 TL	**[.....]**
8	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1300 TL	**[.....]**
9	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1400 TL	**[.....]**
10	You receive 2,500 TL if the marble you randomly draw from Bag 2 is the same color as the color you picked, otherwise you get 0 TL.	Sure amount of 1500 TL	**[.....]**

Version (2): Field experiment on risk and ambiguity attitudes.
Same as Version 1 with the exception of the order of the bags. The first 10 questions refer to Bag 2 (Ambiguous bag) and the second 10 questions refer to Bag 1 (Risky bag).