

Wartime Ties and the Social Logic of Crime

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Abstract

While ex-combatant reintegration is vital to successful transitions from war to peace, some former fighters turn to crime following demobilization. Such criminality undermines the consolidation of political order. Leading theories of crime participation emphasize the role of both individual economic opportunities and factors related to social ties. Yet, we still know little about the social logic of ex-combatant criminality and how social and economic factors relate as drivers of crime participation. This article presents a theory of how wartime social ties—namely vertical ties to former commanders and horizontal ties to ex-combatant peers—influence ex-combatant crime on their own and via their relationship to economic opportunity costs. We use the theory to derive predictions in the context of Colombia, and then test them with a combination of administrative data and high-quality original survey data. We find that both vertical and horizontal wartime ties are powerful drivers of ex-combatant criminality. Our evidence indicates that wartime ties mitigate the risks of criminal behavior by facilitating the transmission of criminal capabilities and pro-crime social norms. We do not find that economic conditions moderate the effect of wartime times nor do we find any indication that economic opportunity costs, on their own, predict criminality. These findings underscore the importance of wartime ties—both vertical and horizontal—to understanding post-conflict transitions and designing reintegration interventions.

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Introduction

Reintegrating former combatants into civilian life is of central importance to transitions from war to peace (Muggah, 2009; Soderstrom, 2015; UN, 2000). Consequently, disarmament, demobilization, and reintegration (DDR) programs in numerous countries seek to facilitate such transitions by providing training and employment opportunities, community reconciliation programs, and psychological support for former fighters, among other interventions (Gilligan, Mvukiyehe & Samii, 2013; Humphreys & Weinstein, 2007). With such assistance, many former combatants integrate into civilian life in good standing with the state.

Yet, other ex-combatants do not complete such a transition, and instead use their experience and skills as wartime specialists in violence to illicit ends. While reintegration ‘failure’ is often conceived of as remilitarization (Daly, 2016; Themner, 2015), there is growing concern about another possible manifestation: crime. Crime is distinct from remilitarization in that it encompasses any actions deemed illegal by the state, not just a return to armed conflict. Investigating the factors that drive ex-combatants into crime is critical to understanding successful transitions to legality for an important population that has been the focus of extensive academic and policymaker attention. Moreover, while ex-combatants are not necessarily significant contributors to high overall crime rates (Nussio, 2018), such knowledge can still help to contribute to order and stability in post-conflict societies (Call, 2007; Collier, 1994).

Political science has only recently started to focus on crime alongside its traditional interest in conflict (Kalyvas, 2015), and can draw upon research in economics, sociology, and criminology for insights. A prominent explanation for crime centers on the role of an individual’s economic opportunity costs. A vast body of research on the economics of crime, following Becker (1968), posits that individuals engage in criminal behavior when the expected material benefits exceed the costs, including the material opportunity costs of legal sector employment. In contrast, research in sociology roots the decision to engage in crime in social factors. This approach suggests that individuals enter into crime not solely (or principally) for individualistic material reasons but rather because their social environment enables it, for instance by transmitting norms that condone criminal behavior (Akers, 1998; Sutherland, 1947; Warr, 2002) or by serving as a resource that provides criminal knowledge and skills (Ballester, Calvo-Armengol & Zenou, 2006; Sutherland, 1947). Such

social considerations seem particularly applicable to former combatants, who often retain enduring ties to former commanders and combatant peers following demobilization.

While both social and economic factors are plausibly critical drivers of crime, there are important unanswered questions as to how they explain ex-combatant criminality. For one, these two logics of crime are often studied in isolation with respect to reintegration. A more systematic investigation of the extent to which these are competing or, in fact, complementary explanations for ex-combatant crime is needed. Moreover, while the economic incentives for crime have received extensive attention from academics and policy-makers, there remains room for greater clarity on the social logic of criminality as it applies to ex-combatants. This includes a better understanding of the role played by vertical ties to former commanders versus horizontal ties to ex-combatant peers; when wartime ties will pull ex-combatants towards crime versus away from it; and the mechanisms by which wartime ties influence crime participation.

This article provides a theory and test of the social logic of ex-combatant criminality that can also help to shed light on the relationship between social and economic explanations for crime. To structure our empirical investigation we first develop a simple theoretical framework in which an ex-combatant deciding whether to engage in crime weighs the costs and benefits, where vertical ties to former commanders and horizontal ties to ex-combatant peers modify the benefits of legal employment and the risks associated with criminal activity. This framework is quite general and may help guide empirical inquiry in any number of post-conflict countries.

We then use this framework to investigate the determinants of ex-combatant criminality in Colombia, a country that has experienced one of the longest and bloodiest civil wars in modern history and where crime has been an endemic part of the transition to peace. Colombia is also a relevant case because both economic and social factors plausibly play an important role in post-war criminality. Moreover, the 2016 peace deal between the Colombian government and the Revolutionary Armed Forces of Colombia (FARC) underscores the exigency of investigating the determinants of post-demobilization criminality among former combatants.

An obstacle to studying illegality in any country arises from data challenges. While administrative data on arrests and convictions exist, they are often imperfect ([Blattman et al., 2016](#)). Moreover, many of those who resort to crime remain beyond the detection of the criminal justice system. We therefore opt to supplement confidential administrative data from the Colombian At-

torney General’s office with data from an original survey conducted with a random sample of 1,158 demobilized combatants in their communities as well as in prison. Our survey methods employed strategies for minimizing sampling and response bias, yielding what we believe is the highest quality survey data collected to date for studying criminality in an ex-combatant population. Indeed, the data suggests that our survey provides a more complete picture of criminality than the administrative data alone; whereas the administrative data indicates that 11% of our ex-combatant population has engaged in crime, the combined data shows participation levels of 24%. We use the data to undertake an observational analysis, where causal identification relies on a covariate control strategy. The credibility of our approach derives from having a large number of available controls and from using sensitivity analysis to check the robustness of our results.

We find that wartime ties are a powerful driver of criminal behavior among demobilized combatants in Colombia. In particular, we show that vertical ties to former commanders are strongly associated with ex-combatant criminality. Moreover, whereas horizontal wartime ties could help to keep ex-combatants in legality by facilitating legal employment or transmitting anti-crime social norms, we find instead that such peer ties also pull ex-combatants into crime. The evidence is consistent with a logic by which strong horizontal ties to ex-combatant peers mitigate the risks associated with criminal behavior. Further investigation into *how* horizontal ties affect criminal risk indicates that they both enhance criminal capacities and transmit pro-crime social norms that increase risk acceptance, although the former channel appears to predominate. Finally, while we theorize how economic opportunity costs could moderate or mediate the effect of wartime ties on crime, we find little indication that economic factors influence crime participation. While this is surprising in light of both theory and the Colombia case, this result only serves to reinforce the importance of wartime ties to ex-combatant criminality, independent of economic opportunity costs.

This article makes several contributions to research on crime and ex-combatant reintegration. Above all, it provides insights into the social logic of ex-combatant criminality. There is already substantial evidence that social ties and group cohesion are critical factors in recruitment into conflict and the conduct of civil wars (Parkinson, 2013; Staniland, 2012; Wood, 2008), but few studies to date have examined how wartime ties evolve following demobilization (for exceptions see e.g. Daly, 2016; Themner, 2015). Other recent research demonstrates how civil wars affect post-conflict crime and violence (Deglow, 2016; Rivera, 2016), but with little attention to the role

of wartime ties. Our study helps to bridge these literatures.

We also highlight the importance not only of vertical but also of horizontal wartime ties. Much of the research on wartime ties and ex-combatant reintegration has focused on the risks posed by the maintenance of strong ties between former commanders and their foot soldiers (Spear, 2002). Yet, the results presented here shed new light on how and why horizontal ties play a powerful and independent role in either influencing criminal behavior or keeping ex-combatants in legality. These findings have important implications for thinking about how to design reintegration interventions, which we discuss in the conclusion.

Theoretical framework

It is widely appreciated that both economic and social factors can play a role in criminal behavior. Yet, important questions remain as to how vertical ties to former commanders *and* horizontal ties to ex-combatant peers influence crime participation on their own and in relation to economic opportunity costs. We address these questions through a model of social ties and ex-combatant criminality. We focus on social and economic factors because of their prominence as explanations for criminal behavior and their relevance to the design of reintegration programs.¹

A leading explanation for criminality centers on the role of individual economic opportunity costs. This logic follows Becker (1968), who posits that individuals opt for crime if the material benefits exceed what can be obtained in the legal sector, taking into account risks associated with capture. Unemployment, low legal sector wages, and low levels of human capital can all increase the relative attractiveness of crime on this basis. Numerous studies provide evidence consistent with this story (for a review, see Draca & Machin, 2015).² These opportunity cost arguments also

¹Research on crime among war veterans highlights other possible determinants, such as psychological trauma (for a review see Friel, White & Hull, 2008). To keep our framework tractable we do not theorize these but control for them in our analysis (see below and Appendix H).

²Social bond theory views employment and education as commitments to ‘conventional’ activities that could constrain criminal behavior (Hirschi, 1969; Rivera, 2016; Thyne & Schroeder, 2012). Regardless of whether one believes these factors capture economic opportunity costs or social constraints, they lead to similar basic predictions in our theoretical framework.

apply to conflict participation. For instance, [Dube & Vargas \(2013\)](#) show that increases in the price of labor intensive agricultural goods reduced conflict in Colombia, presumably due to labor market stimulation. Consequently, post-conflict interventions often focus on improving economic opportunities ([Gilligan, Mvukiyehe & Samii, 2013](#); [Muggah, 2009](#)). [Blattman & Annan \(2016\)](#) show that agricultural training and capital inputs reduced illicit logging, mining, and mercenary activity by high-risk men (including ex-combatants) in Liberia.

Alternatively, criminality research in sociology and criminology has long focused on social ties. This literature emphasizes the importance of ties to ‘instigators’ who hold influential positions within their social networks and who possess the will and ability to engage in crime and to recruit others into crime ([Warr, 2002](#)). Such theories also stress the role of *peer* ties—for instance among individuals in the same neighborhood or school—to criminal behavior ([Akers, 1998](#); [Haynie, 2001](#); [Warr, 2002](#)). Some theories consider how horizontal ties strengthen the *capacity* to engage in crime by transmitting knowledge of how to commit crime or evade capture ([Ballester, Calvo-Armengol & Zenou, 2006](#); [Sutherland, 1947](#)). Other theories emphasize how horizontal ties transmit *norms* that define criminal behavior as acceptable or even desirable, awarding status to group members based on their criminality, or generating social costs for non-participation ([Akers, 1998](#); [Sutherland, 1947](#); [Warr, 2002](#)). Peer ties could thus affect both the ability and the motivation to engage in crime.³

There are good reasons to believe that wartime ties affect ex-combatant criminality. Ex-combatants are connected by powerful bonds reinforced during war ([Parkinson, 2013](#); [Staniland, 2012](#); [Wood, 2008](#)). Ex-combatants often maintain their factional relations and identities long after demobilization, as [Daly \(2016\)](#) shows for locally-recruited armed groups in Colombia.⁴ At the same time, ex-combatant identity may be stigmatized, leading to discrimination in the legal sector.

³Social ties to friends, family, or other community members could either counteract the pull of wartime ties and act as constraints on criminal behavior ([Hirschi, 1969](#)), or could draw ex-combatants into crime through other channels. We control for the strength of non-wartime ties in the analysis.

⁴Whether wartime ties remain strong likely depends on factors such as unit cohesiveness, individual versus collective demobilization, or geographic clustering following demobilization. We include such factors as controls to minimize potential confounding (see [Appendix H](#)).

Moreover, ex-combatants are specialists in violence and clandestine activity, meaning they possess and share skills especially suited to crime (Muggah, 2009).

In what follows, we propose a model that integrates explanations for crime centered on economic opportunity costs and wartime ties. The model can be found in its entirety in Appendix A. We then use the model to structure our empirical analysis of ex-combatant criminality in Colombia.

A model of ex-combatant criminality

We model an agency relationship between a former commander who seeks to organize criminal activity and a potential ex-combatant recruit. In deciding how much effort to allocate to crime, the recruit weighs the potential material benefits of crime against foregone legal sector opportunities and risks of punishment. Foregone legal sector opportunities are income, legal employment, and education. Risks include the probability of getting caught—which is determined by legal sector capacity and policing levels—and of getting harmed, for instance in the course of violent interactions with competing criminal gangs.

Wartime ties enter into the cost-benefit calculation of crime in the following ways in our setup. First, horizontal ties to ex-combatant peers can pull ex-combatants towards crime by mitigating the costs or risks associated with criminal behavior. This is consistent with the notion that horizontal ties could strengthen criminal *capabilities* by providing skills and knowledge helpful to evading capture, or make ex-combatants more willing to accept the risks of criminal activity by fostering pro-crime social *norms*. Yet, the model also allows horizontal ties, through the same mechanisms, to pull ex-combatants away from crime by enhancing their capabilities to participate in the legal sector or by conveying anti-crime social norms. This setup thus captures the fact that strong peer ties can combine either to orient an ex-combatant towards criminality or legality. Ultimately, whether horizontal ties pull towards criminality depends on which effects dominate and is a matter for empirical investigation.

The strength of vertical ties to former commanders also matter to an ex-combatant's decision. The commander offers the ex-combatant a wage, which depends on the recruit's expected return to crime (relative to legal sector alternatives) and the risk associated with recruiting a particular ex-combatant. Stronger vertical ties between a commander and ex-combatant lower the former's own risk because they imply greater knowledge of a recruit's abilities and trustworthiness. Commanders

also prefer horizontally well-connected ex-combatants insofar as those connections help to mitigate crime risks to both the recruit and the commander. Thus, commanders prefer vertically and horizontally connected ex-combatants, especially for riskier types of crime.

The model yields several predictions for the social logic of ex-combatant criminality. First, insofar as there are criminal commanders, stronger *vertical* ties between criminal commanders and former combatants should increase criminality. Second, the model yields predictions on when horizontal ties will have the net effect of pulling ex-combatants towards or away from crime. Specifically, horizontal ties to ex-combatant peers will result in *less* criminal effort if they primarily facilitate legal economic opportunities or transmit anti-crime social norms. This possibility is consistent with evidence that wartime ties can play a positive role in enabling ex-combatant reintegration (Annan & Patel, 2009; de Vries & Wiegink, 2011). Conversely, horizontal ties will result in *more* criminal effort if such relations mitigate criminal risk, either by enhancing criminal capacities or transmitting pro-crime social norms. Third, the model provides insights into the potentially complex relationship between vertical and horizontal wartime ties. We predict a negative interaction between vertical and horizontal ties if the latter generate legal sector opportunities that far outweigh their role in reducing criminal risk for both ex-combatants and commanders. Alternatively, we predict a positive interaction insofar as both types of ties primarily mitigate the costs associated with criminal activity, especially for riskier types of crime like violent crime.

Insofar as horizontal ties facilitate crime by mitigating criminal risk, our framework further sheds light on mechanisms, specifically whether this is due to enhancing criminal capabilities or transmitting pro-crime social norms. If wartime ties primarily have a capacity-enhancing effect, then a criminal commander would have to offer a well-connected recruit a higher wage to pull him into crime. In contrast, if horizontal ties operate through a norms mechanism—strengthening the motivation to participate in crime by transmitting norms and conferring social rewards for engaging in crime—the stronger an ex-combatant’s horizontal ties, the *lower* the commander’s criminal wage offer could be. The model thus identifies a way—by examining the commander’s wage offer—to disentangle whether horizontal ties facilitate criminality by enhancing criminal capabilities or transmitting pro-crime social norms.

The model also generates predictions clarifying whether wartime ties and economic opportunity costs offer competing or complementary explanations for ex-combatant crime. The model returns

the conventional prediction that the higher the opportunity costs of crime—meaning the better an ex-combatant’s employment, economic, or educational situation following demobilization—the less likely they are to engage in criminal behavior. This prediction enables us to investigate the explanatory power of economic opportunity costs relative to wartime ties. Yet, the theoretical approach also provides insights into how these logics can be complementary. For one, the discussion above demonstrates how economic factors could be mediating channels in a social theory of crime. A final implication of the model, however, is that there is also a conditional relationship between economic opportunity costs and horizontal ties. In contexts where ex-combatants enjoy better economic well-being for any reason—e.g. because of reintegration program successes in providing employment or training opportunities—the higher economic opportunity costs should attenuate the adverse effects of horizontal wartime ties. Specifically, we predict a negative interaction between economic opportunity costs and horizontal ties reflecting the fact that, as legal sector opportunities increase, any draw that horizontal ties produce towards criminality should diminish.

We note that the model assumes that former commanders play the role of “instigators.” Such a role follows from the nature of commanders’ wartime experience and their status relative to lower-ranking ex-combatants (Daly, 2014; Themner, 2013). Former commanders may turn to crime if their ex-combatant identities are stigmatized in the legal sector, as discussed above. Commanders may also cherish opportunities to recover status and power that was lost due to demobilization (Levitt & Venkatesh, 2000; Themner, 2015).⁵ Some have argued that the successful reintegration of ex-combatants requires breaking up command-and-control structures (Spear, 2002). That said, not all former commanders turn to crime—indeed, some reintegrate successfully and even facilitate the reintegration of their former soldiers (Themner, 2015). Our empirical analysis below assesses commanders’ roles as instigators by examining the effects of strong vertical ties on lower-ranking ex-combatants’ criminality.

Our model proposes that horizontal ties to ex-combatant peers increase criminality when the effect on crime risks outweighs any positive effects on legal employment opportunities. Whether horizontal ties contribute to a pro-crime balance of effects will depend on contextual factors, such as

⁵While the small number of commanders in our sample makes it difficult to investigate the factors that drive their crime participation, we do so to the best of our abilities in Appendix M.2.

the degree to which ex-combatants face stigmatization and discrimination. We discuss evidence for ex-combatant stigmatization in Colombia in the context section below and return to a discussion of other relevant contextual factors below.

Context

We examine criminality with data on paramilitaries and guerrillas who demobilized as of 2012 in Colombia. The contemporary Colombian conflict has its roots in La Violencia, the 1948-1958 civil war between the Liberal and Conservative Parties. In the 1960s, left-wing guerrilla organizations like the FARC and the National Liberation Army (ELN) emerged. With the introduction of the drug economy to Colombia in the late 1970s and the adoption of kidnapping and extortionary financing tactics, the guerrillas began to pose a serious threat to the military, landowning elite, drug barons and political class. Accordingly, these diverse sectors of society formed regional paramilitary forces. Over the subsequent decades, both the rebels and militias extended their power over nearly the entire country. The conflict has left over 220,000 dead in its wake and displaced 4.7 million ([Grupo de Memoria Histórica, 2013](#)).

In 2002, Alvaro Uribe won the Colombian presidency and began negotiations with paramilitary leaders, resulting in peace accords between the government and each of the 37 paramilitary groups and the disarmament of their 31,870 combatants between 2003 and 2006. Concurrently, Uribe continued an individual demobilization process whereby paramilitary and guerrilla combatants could desert their armed groups and receive amnesty and reintegration benefits. Between 2003 and the present, 29,238 paramilitaries and guerrillas disarmed under this process, bringing the total number of registered demobilized combatants in Colombia to over 58,000.⁶ Following the historic peace accords signed between the Santos administration and the FARC in November 2016, an additional 7,300 FARC guerrillas have demobilized.⁷ The peace agreements have been accompanied by a comprehensive DDR program to help transition ex-combatants into civilian life ([Daly, 2016](#)).

Ex-combatant reintegration has had some limitations on its success, however. Estimates from our survey (described below) suggest high levels of perceived stigmatization among ex-combatants

⁶The ex-guerrillas in our study are those who demobilized voluntarily before the 2016 accord.

⁷This figure is current as of March 2017.

that could reflect limited legal sector opportunities. For example, when asked whether they thought they would face discrimination in applying for jobs, 84% indicated “definitely” or “probably”. This has important implications for the function of wartime ties: the wartime ties that bind ex-combatants may convey an identity that undermines, rather than increases, legal sector opportunities.

Critically, some ex-combatants have turned to crime either as individuals or with gangs following demobilization. Since 2005, Colombia’s security landscape has become populated with criminal gangs (*bandas criminales emergentes* or BACRIM) comprising re-militarized paramilitaries, narco-trafficking entities, and guerrillas. The BACRIM are widely viewed as the most serious threat to contemporary Colombian security and they are blamed for the sharp uptick in violence in recent years ([Human Rights Watch, 2010](#)).

Addressing questions about how to prevent criminality is of particular relevance in Colombia at present as FARC combatants reintegrate, and the government engages in sporadic peace talks with the ELN rebel armies. Due to the timeliness of this research for Colombia, we were afforded the rare opportunity to collect data on criminality with significant collaboration from the Colombian Government and international organizations.

Empirical methods

We test our hypotheses using administrative and original survey data from 1,158 demobilized combatants representing both right- and left-wing armed groups in Colombia. The administrative data comes from a confidential dataset obtained from the office of the Fiscalía (attorney general) that contained current information on former combatants who had been charged or convicted of criminal activities. The survey data serves as an important complement to the administrative data and provides not only our measures of the correlates of criminality and a rich set of controls but also self-reported data on criminal behavior.⁸

In combining the administrative and survey data, we mitigate biases that could arise from relying on either type of data alone. Administrative data on crime can be subject to error, especially when

⁸This study was conducted in collaboration with *Fundación Ideas para la Paz* (FIP) and implemented in Spanish by trained enumerators working for a reputable survey firm.

arrests or convictions under-report true criminal behavior (Blattman et al., 2016). Survey data, however, can suffer from sampling and misreporting biases, especially when trying to interview a hard-to-reach population on a sensitive topic. We aimed to minimize sampling bias through careful recruitment and selection procedures and reporting bias by using a self-administered survey to elicit more truthful responses. Our main results are based on weighted least squares regressions with municipality fixed effects, following the guidance of Beck (2019) for robust inference with fixed effects (see Appendix Section I). The subsections below explain the sample and variables used in the analyses.

The sample

A central challenge in studying criminality is collecting data on a hard-to-reach population, insofar as those demobilized combatants who are most likely to be criminal also may have disassociated with the reintegration agency or may be in prison. We took several steps to obtain a representative sample of the demobilized population (for more detail, see Appendix B). We gained access to a database of the entire population of ex-combatants who had surrendered their weapons and demobilized. Also, our study was conducted in 2013, shortly after the passage of Law 1424 in 2010, which mandated that all ex-combatants participate in the reintegration program in exchange for a suspension of their judicial sentences. This law created robust positive incentives for the previously hidden population of demobilized combatants to become “locatable” to the reintegration agency and thus enter our sampling frame.⁹ Our sampling frame was likely the best that one could obtain in Colombia for studying ex-combatants.¹⁰

We used this database of all demobilized combatants to construct a list of municipalities in Colombia that had at least 50 ex-combatants and that were accessible to the OAS Peace Mission (MAPP-OEA).¹¹ We collaborated with the MAPP-OEA because it was an international organi-

⁹We estimate that 4110 ex-combatants came forward following the passage of Law 1424, while 9,922 ex-combatants (that is, 19% of the total that had demobilized) could not be located by the reintegration agency.

¹⁰We opted not to use data from a 2008 survey conducted by FIP and used, for instance, in Kaplan & Nussio (2016), because our inquiries revealed this to be a convenience sample.

¹¹While this choice makes it more difficult to assess the generalizability of our results to all municipal-

zation charged with verifying and monitoring the 2003-2006 peace agreements and had credibility among ex-combatants. Of the 136 municipalities with 50-or-more ex-combatants, 83 were covered by the MAPP-OEA and from these we sampled 47.¹² We then drew a random sample of these participants, stratifying on former armed group, demobilization year, criminal charges, department of residence, and registration after Law 1424.

We also randomly sampled 268 individuals in prison.¹³ While the prison sample increased our likelihood of including criminal ex-combatants in this study, we emphasize that not all individuals in prison are criminals as some who are arrested and charged are later exonerated. Likewise, those involved in crime do not come exclusively from our prison sample insofar as some criminal ex-combatants were already released while others were never captured in the first place.

Measuring criminality

We define as criminal any activity that is deemed illegal by the Colombian penal code.¹⁴ To obtain reliable survey data on criminal activity, we had to elicit honest responses to highly sensitive questions. We thus obtained our measures using a self-administered survey accompanied by an elaborate confidentiality procedure to protect respondent identity (see Appendix C). There is substantial evidence that self-administered surveys elicit more truthful reporting on sensitive behaviors than enumerated surveys (Tourangeau & Yan, 2007). Using a self-administered survey also enabled us to measure more aspects of illegality more precisely than could easily be accommodated by list

ities, it was done for practical purposes and implies a coverage loss of only 15% of the ex-combatant population.

¹²MAPP-OEA operated in areas with the highest density of ex-combatants, which included municipalities that experienced the most crime following demobilization.

¹³In the analysis that follows we pool data from the prison and non-prison samples and use appropriate sampling weights.

¹⁴The penal code can be found at https://www.oas.org/dil/esp/Codigo_Penal_Colombia.pdf (last accessed March 20, 2017). Our sample most commonly self-reports crimes such as conspiracy, carrying arms, robbery, assault, and drug trafficking (see Appendix F).

experiments and other indirect questioning techniques.¹⁵

We employ two versions of the criminality outcome variable, with summary statistics presented in Table I. First, we construct a binary measure of “proven” illegality by combining the administrative and survey measures, coding as criminal any respondent who is classified as such by at least one of the two data sources. A respondent is coded as criminal according to the administrative data if they have been *convicted*, which corresponds to about 11% of our population. We believe this is the most defensible way to operationalize illicit behavior given that a number of those arrested or charged are later exonerated. We determine that a respondent is criminal according to the survey if they self-report having engaged in criminal activity on their own or with a gang or stated the nature of at least one crime committed (see Appendix D for question wordings). As shown in Table I, according to the survey data, about 20% of our population is criminal. We interpret the fact that the survey yielded more reported criminal behavior than the administrative data as evidence that we succeeded, at least in part, in obtaining data on ex-combatants that have eluded the criminal justice system.¹⁶ Combining the administrative and survey data yields 353 ex-combatants in our sample who are criminal, corresponding to about 24% of our population. Disaggregating this, about 36% of former commanders are engaged in crime compared to about 22% of rank-and-file ex-combatants.

Table I here

Second, we create a composite measure of engagement in violent crime from two survey questions that ask those who have engaged in crime how often their criminal activities involved violence. We code those who have not engaged in any crime following demobilization as zero, those who have engaged in non-violent individual and/or gang crime that as one; and those who engaged in individual and/or gang crime that involved at least some violence as two. The mean of our violent

¹⁵List experiments, for instance, can be noisy and generally accommodate only one sensitive item per experiment. Our self-administered survey contained nearly 30 sensitive items. For a review article on different sensitive question techniques, see [Tourangeau & Yan, 2007](#).

¹⁶See Appendix G for more on the extent to which our survey identified a “hidden” population of ex-combatant criminals.

crime scale is .31, indicating that about 12% of our ex-combatant population—and 47% of our criminal ex-combatant population—has participated in violent crime.

Explanatory variables

The main measures for our explanatory variables come from the enumerated survey, with question wordings and summary statistics available in Appendices D and E, respectively. To ensure that our explanatory variables are measured temporally prior to any criminal activity, we continually prompted all respondents to answer the relevant survey questions as they pertained to their lives *one year following demobilization*. We selected this time point because piloting suggested that ex-combatants were readily able to recall their living conditions on the one year anniversary of their demobilization and because official data indicated that former combatants rarely committed crimes within the first year.¹⁷ To minimize concerns about recall bias, where possible we limited ourselves to direct questions about objective conditions or highly salient circumstances that piloting suggested were easy for respondents to remember. Asking about this first year also enhances the policy relevance of our analysis, as reintegration programs typically offer the most extensive assistance in the year or two immediately following demobilization. We note that our analysis cannot account for variable shocks over time (e.g. changes to economic conditions following demobilization); doing so would require panel data.

We study wartime social ties using measures of the extent to which a former combatant maintained active relations with other combatants and commanders one year following demobilization. We create an index of strong vertical ties to former commanders using six survey measures that inquire into regularity and speed of communication between a former combatant and his superiors, and whether a respondent would lend money to his former commander(s) if asked, which captures trust or a sense of obligation (see Appendix D).¹⁸ Our index of horizontal ties to other combatants comprises seven measures that capture the proportion of a respondent’s friends who are combatants; the amount of time spent with other combatants; and the likelihood that a respondent would turn to a combatant for help in an emergency.

¹⁷Forty-four respondents in our sample admit to having committed crimes within the first year of demobilization. All results are robust to excluding these respondents (see Appendix J).

¹⁸We use inverse covariance weighting to create all indices in this article (see Anderson, 2008).

We measure economic well-being one year following demobilization using measures of employment, education, and inverse covariance-weighted averages of economic welfare indicators. The index of objective economic well-being combines 12 measures of income, household conditions, and asset ownership, whereas the index of subjective well-being employs three measures about satisfaction with one’s economic situation one year following demobilization.

Control variables

Our strategy is to control for all factors that could be responsible for a spurious association between our social and economic variables of interest and post-war criminality. To that end, all analysis (unless otherwise specified) incorporates municipality fixed effects such that we are estimating how economic opportunity costs and social ties influence ex-combatant criminality *within* municipalities. Thus, insofar as there are municipality-level characteristics that could explain ex-combatant criminality—such as crime levels, coca suitability, or law enforcement—we are controlling for them.

Our analysis also incorporates a large number of individual-level controls, including initial motivations for joining an armed group; conflict experience and fighting group characteristics (e.g. duration and intensity of fighting experience and unit hierarchy and cohesion); the nature of demobilization (individual or collective); reintegration program participation; strength of family and community ties following demobilization (as a possible counterpart to wartime ties); and demographic factors such as age and parenthood, which are often viewed as proxies for social constraints on crime. As our controls are too numerous to elaborate upon here, we refer readers to Appendices [E](#) and [H](#) for an extended discussion. To reduce the number of covariates we use in the regressions, we again use inverse covariance weighting to create indices, leaving us with a final set of 25 control indices and 20 individual covariates.

Main results

Wartime ties and crime

We first examine the impact of wartime ties on criminal behavior, with results presented in Panel A of Table [II](#). A foundational prediction of the model is that, insofar as former commanders recruit into crime, ex-combatants who maintain strong vertical ties to those commanders should be more

likely to engage in crime following demobilization. We obtain strong evidence that this is indeed the case, finding that a one standard deviation increase in the vertical ties index translates into a six percentage point greater likelihood of criminal behavior (column 1). This result remains significant at the 95% confidence level when including all controls. Similarly, a one standard deviation change in the vertical ties index is associated with a .11 unit change in the violent crime scale (column 3), again with a similar result when all controls are included.

Table II here

With respect to horizontal wartime ties, one of the main predictions is that strong connections to ex-combatant peers could pull ex-combatants away from crime if they facilitate legal sector opportunities or transmit anti-crime social norms. Conversely, intact horizontal networks could pull ex-combatants towards crime if they mitigate criminal risk by enhancing criminal capacities or pro-crime social norms. While this underscores that horizontal ties could either have positive or negative effects on reintegration, our results in Table II provide clear evidence that, in our context, horizontal ties are associated overall with a greater likelihood of criminal behavior, including violent crime.

Consistent with this, there is little indication that strong horizontal ties improve legal sector opportunities, which is one of the key theorized channels by which horizontal ties could help to pull ex-combatants away from crime. Table III presents results from a regression of our economic opportunity cost measures on the horizontal ties index. The coefficient on the horizontal ties index is near zero in almost all cases and statistically insignificant. Since this was before ex-combatants in our population entered into crime, we interpret this as evidence that horizontal ties did not improve economic well-being through better legal opportunities.

Table III here

The model suggests that, insofar as horizontal ties pull ex-combatants into crime, it does so by mitigating the risks associated with criminal activity. To further investigate this, we take advantage of the fact that the model provides a way to disentangle the possible mechanisms behind criminal risk mitigation. Our theoretical framework suggests that horizontal ties could enhance

criminal *capabilities*, reducing the chances of capture or conviction, or convey pro-crime social *norms*, increasing tolerance for the potential costs of criminal activity. Horizontal wartime ties could increase criminal activity through one or both of these mechanisms.

If horizontal ties provide knowledge that makes ex-combatants more capable of evading capture, we should observe a stronger link between horizontal ties and criminal behavior among those ex-combatants who remain “hidden” to the criminal justice system. To examine this, we evaluate how horizontal ties correlate with criminal behavior in the administrative data alone (reflecting results for ex-combatants who did not evade the criminal justice system) versus in the survey data (which includes ex-combatants who did). Indeed, Table IV shows no association between horizontal ties and criminality in the administrative data (columns 1-2) but a clear positive association in the survey data (columns 3-4). This indicates that ex-combatants who engage in crime and have strong horizontal ties are less likely to get caught.

Table IV here

We can also check evidence for the capabilities versus norms mechanisms by looking at commander wage offers. Our model predicts that better criminal capabilities should result in higher wage offers to ex-combatant recruits whereas stronger pro-crime social norms should have the opposite effect. To test this proposition, we use data from the self-administered survey to calculate the mean criminal wage offer an ex-combatant received. The results presented in Table V show that horizontal ties are correlated with higher wage offers in the regression without controls (column 1). While the result is no longer significant when controls are added (column 2), the coefficient is still large and positive, indicating that the capabilities mechanism is dominating the norms mechanism with respect to wage offers.

While the capabilities mechanism appears to be dominant, we can also check whether horizontal ties have any effect on transmitting pro-crime social norms. We construct a “criminal norms” index using six measures from the self-administered survey that capture support for criminal activity under different conditions.¹⁹ Table V shows a positive association between horizontal ties and acceptance of criminal norms (column 3) and this association remains significant at the 90%

¹⁹See Appendices D and E for more details.

confidence level in the regression with controls (column 4). The results indicate that horizontal wartime ties are pulling ex-combatants into crime through both capacities and norms mechanism but that, at least in Colombia, the former channel is more powerful.

Table V here

Both vertical and horizontal ties are associated with more crime in our data, and our model would suggest that in this case the two should complement each other, especially for riskier types of crime like violent crime. The results in Panel B of Table II support this prediction. The coefficient on the interaction of vertical and horizontal is positive and significant but only for our violent crime scale, consistent with the notion that violent crime entails harsher punishments if caught or greater chances of personal harm to the perpetrator.

Wartime ties and economic opportunity costs

The results presented above provide striking evidence for the importance of both horizontal and vertical ties to criminal behavior. To what extent are economic opportunity costs competing or complementary in explaining criminality? We first test the prediction that higher economic opportunity costs will be associated with lower levels of crime. Table VI shows that there is no evidence that any of our four measures of economic factors—employment status, objective economic well-being, subjective economic welfare, and education—predict criminal behavior in general or violent crime in particular. The coefficients are close to zero and none of the measures are statistically significant at the 95% confidence level; neither are they jointly significant according to the F-test.

Table VI here

We also check the extent to which social and economic factors complement each other. Indeed, above we present evidence that suggests that economic opportunity costs did not *mediate* the relationship between horizontal ties and crime—there is no indication that horizontal ties reduced criminal behavior by increasing legal employment. We can also see in Table VI that none of our four measures of economic opportunity costs *moderate* the effect of wartime ties. Whereas the model predicted that higher opportunity costs would diminish any draw that horizontal ties produce towards criminality, none of the interaction terms are negative and significant.

These results are surprising in light of the extensive literature on economic opportunity costs, the predictions from our own model, and the Colombia case. One possibility is that the role of social and economic factors vary for members of Colombia’s two distinct fighting groups: left-wing guerrillas and right-wing paramilitaries. Paramilitaries are widely viewed as more mercenary, which could imply a more pronounced sensitivity to economic drivers of crime. They might also exhibit a stronger link between wartime ties and criminality because of collective demobilization, whereas guerrillas—up until the 2016 peace deal—primarily demobilized individually.

Appendix M.1 examines how wartime ties differ for paramilitaries versus guerrillas. The results show that higher levels of economic welfare are not associated with less criminal activity. The results, however, provide further support for the social logic of crime. Ex-combatants with stronger ties to former commanders were more likely to participate in crime, regardless of fighting group. Yet, interestingly, strong horizontal ties were significantly more likely to pull paramilitaries into crime than guerrillas, potentially reflecting paramilitary networks’ greater criminal capacities or pro-crime norms. The fact that horizontal ties served different functions for former paramilitaries and guerrillas points to the importance of understanding how the kind of contextual factors discussed above can shape whether horizontal networks pull ex-combatants towards or away from crime, and should be further explored in the context of the collective demobilization of the FARC in 2017.

Robustness checks

We perform a number of robustness tests, reported in Appendix J. One potential concern is that fighting units that were more engaged in wartime crime could be more likely to have stronger ties following demobilization, resulting in a spurious association between wartime tie strength and criminality. We check this by including fighting unit (*bloque*) fixed effects and the presence of coca production in the area in which a fighting group was operating. We obtain almost identical results when including these as controls. Appendix J provides further evidence that our main results are robust to alternative ways of coding the dependent variable and to excluding from the analysis ex-combatants who committed crimes within one year of demobilization.

We also implemented a general analysis of sensitivity to unmeasured confounders (results reported in Appendix K) following the logic of Imbens (2003). The sensitivity analysis shows that the confounding would have to be severe—similar in magnitude to the predictive power of gender

or conflict exposure—to overturn our results on wartime ties. Additionally, we use a sensitivity analysis to assess the extent to which our inability to sample ex-combatants that remained “hidden” from the reintegration agency might bias our analysis (see Appendix L). We show that to overturn our results on wartime ties, the effect in the hidden population would have to be of the opposite sign and about an order of magnitude larger.

Discussion and external validity

It is worth considering scope conditions for our analysis. First, our results point to the critical role of commanders in organizing crime in Colombia. We would not expect to obtain similar predictions or results in contexts where former commanders have successfully reintegrated and have little incentive to recruit their former soldiers into crime. Second, the effect of horizontal ties depends on their role in enhancing legal opportunities versus mitigating the risks of criminality. We found that in Colombia, ex-combatants overwhelmingly sensed that the identity associated with their wartime network was stigmatized. This undermines the function of wartime ties for legal economic opportunities. More generally, whether horizontal wartime ties contribute to or mitigate ex-combatant criminality could depend on additional contextual factors, such as the operative norms within an ex-combatant social group or the strength of wartime ties relative to other family and community ties. The effect of horizontal ties on criminal behavior could also vary depending on overall crime rates in any given society. Colombia is a relatively high-crime context (De la hoz Bohórquez, 2013), and there is good reason to believe that the mechanisms by which horizontal ties pull ex-combatants into crime in our model are accentuated in high crime societies.

One unexpected result from our empirical investigation is the lack of any evidence for economic opportunity costs. In Appendix N, we provide an extended discussion of possible explanations for the null results on economic opportunity costs observed here, including the possibility that Colombia’s reintegration program successfully severed the link between economic insecurity and crime. Economic opportunity costs might play a bigger role in deterring crime in contexts where reintegration programs are less effective at providing economic assistance to ex-combatants.

Conclusion

This article demonstrates that both vertical ties to former commanders and horizontal ties to wartime peers can be important drivers of ex-combatant criminality. Horizontal ties will pull ex-combatants towards crime when their effect on mitigating the risks of criminal activity outweigh their ability to enhance legal opportunities. Horizontal ties can enhance both criminal capabilities and pro-crime social norms. Finally, our approach shows how economic factors could both mediate and moderate the effect of wartime ties, helping to integrate distinct literatures on reintegration, although in practice such interconnections did not appear to be strong in Colombia.

The findings call for shifting the paradigm of reintegration programming to consider not only individual material conditions but also social ones, highlighting the importance of developing and testing policy interventions that redirect the function of wartime ties, if not reconfiguring them. While recent randomized evaluations have examined the role of economic interventions ([Blattman & Annan, 2016](#)) and cognitive behavioral therapy ([Blattman, Jamison & Sheridan, 2017](#)) on criminal behavior in post-conflict countries, to date there are no equivalent evaluations of interventions focused on wartime ties. Evaluating interventions specifically designed to target social considerations is a critical avenue for future research.

One policy approach could be to focus on the design of interventions that aim to keep former commanders out of illegal or illicit activity. It is widely appreciated that reintegration programs have not devoted sufficient attention or resources to issues of commander reintegration ([Daly, 2016](#)). While our study shows that not all commanders go into crime, our evidence indicates that some clearly do and with broader detrimental effects for others in their networks.

Strong horizontal ties will keep ex-combatants out of crime insofar as their ability to transmit legal opportunities or anti-crime social norms outweighs their transmission of criminal capabilities and pro-crime social norms. Thus, it could be beneficial to keep horizontal wartime networks intact as long as they are directed towards positive ends, for instance by working to shape anti-crime norms within ex-combatant groups. Finally, while we do not examine directly the role of alternative social ties, our theory suggests that there could also be value in strengthening civilian (non-criminal) social ties insofar as such connections could counteract any pull of wartime networks towards criminality. Ultimately, paying greater attention to the social logic of ex-combatant crime

could be critical to preventing transitions from combatant to criminal and, ultimately, to facilitating transitions from war to peace.

Data replication

The dataset, codebook, and do-files for the empirical analysis in this article can be found at <http://www.prio.org/jpr/datasets>.

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Tables

Table I. Summary Statistics for DVs

	Mean	SD	Min	Max	Count
Panel A: ‘Proven’ criminality					
Convicted (admin. data)	0.11	0.31	0	1	1158
Criminal (surv. data)	0.20	0.40	0	1	1158
As an individual	0.10	0.30	0	1	1158
With a gang	0.05	0.21	0	1	1158
Reported type of crime(s)	0.20	0.40	0	1	1158
Proven criminality (survey + admin)	0.24	0.43	0	1	1158
Panel B: Committed violent crime					
Violent crime (combines two measures below)	0.31	0.63	0	2	1158
Violent crime as an individual	0.25	0.56	0	2	1158
Violent crime with a gang	0.20	0.52	0	2	1158

Summary statistics are weighted to the population.

Table II. Wartime ties and crime

	'Proven' criminal		Violent crime	
	(1)	(2)	(3)	(4)
Panel A: Main Results				
Vert. ties	0.06*** (0.02)	0.05* (0.02)	0.11*** (0.03)	0.08** (0.03)
Horiz. ties	0.07*** (0.02)	0.05* (0.02)	0.16*** (0.04)	0.15*** (0.04)
Panel B: Interaction Regressions				
Vert. ties	0.06** (0.02)	0.04* (0.02)	0.09** (0.03)	0.06* (0.03)
Horiz. ties	0.07*** (0.02)	0.05* (0.02)	0.16*** (0.04)	0.14*** (0.04)
Horiz. × vert. ties	0.01 (0.01)	0.01 (0.01)	0.05** (0.02)	0.04* (0.02)
Social F-test (pvalue)	0.00	0.00	0.00	0.00
Observations	1158	1158	1158	1158
Clusters	570	570	570	570
Econ controls	Yes	Yes	Yes	Yes
Other controls	No	Yes	No	Yes

Standard errors in parentheses

Weighted least squares with municipality fixed effects and individual controls.

Standard errors account for clustering by survey sampling blocks.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table III. Correlation between economic opportunity costs and horizontal ties

	Employed		Welfare obj.		Welfare subj.		Education	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Horiz. ties	0.00 (0.02)	-0.00 (0.02)	0.06 (0.05)	0.00 (0.04)	-0.00 (0.05)	-0.04 (0.05)	0.03 (0.06)	-0.04 (0.03)
Observations	1158	1158	1158	1158	1158	1158	1158	1158
Clusters	570	570	570	570	570	570	570	570
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Standard errors in parentheses

Weighted least squares with municipality fixed effects and individual controls.

Standard errors account for clustering by survey sampling blocks.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table IV. Results for administrative versus survey data

	Criminal in Admin Data		Criminal in Survey Data	
	(1)	(2)	(3)	(4)
Vert. ties	0.04* (0.02)	0.04* (0.02)	0.06*** (0.02)	0.04* (0.02)
Horiz. ties	0.02 (0.01)	0.02 (0.01)	0.07*** (0.02)	0.06* (0.02)
Observations	1158	1158	1158	1158
Clusters	570	570	570	570
Covariates	No	Yes	No	Yes

Standard errors in parentheses

Weighted least squares with municipality fixed effects and individual controls.

Standard errors account for clustering by survey sampling blocks.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table V. Capacity versus norms mechanisms

	Log wages		Criminal norms	
	(1)	(2)	(3)	(4)
Vert. ties	0.52* (0.20)	0.41* (0.18)	0.14** (0.05)	0.07 (0.04)
Horiz. ties	0.73** (0.15)	0.19 (0.15)	0.20** (0.06)	0.10 [†] (0.06)
Observations	1158	1158	1158	1158
Clusters	570	570	570	570
Covariates	No	Yes	No	Yes

Standard errors in parentheses

Weighted least squares with municipality fixed effects and ind. controls.

Standard errors account for clustering by survey sampling blocks.

[†] $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table VI. Interaction of economic and social factors

	'Proven' criminal				Violent crime			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed	-0.01 (0.04)	-0.00 (0.04)	-0.01 (0.04)	-0.00 (0.04)	-0.06 (0.06)	-0.06 (0.06)	-0.06 (0.06)	-0.06 (0.06)
Econ welfare obj. (index)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.04 (0.03)	0.04 (0.03)	0.04 (0.03)	0.04 (0.03)
Econ welfare subj. (index)	-0.00 (0.02)	-0.00 (0.02)	-0.00 (0.02)	-0.00 (0.02)	0.01 (0.03)	0.01 (0.03)	0.01 (0.03)	0.01 (0.03)
Education	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)	0.00 (0.03)
Vert. ties	0.05* (0.02)	0.05* (0.02)	0.05* (0.02)	0.05* (0.02)	0.08** (0.03)	0.08** (0.03)	0.08* (0.03)	0.08** (0.03)
Horiz. ties	0.09* (0.04)	0.05* (0.02)	0.05* (0.02)	0.03 (0.04)	0.18** (0.07)	0.14*** (0.04)	0.15*** (0.04)	0.08 (0.07)
Emp X Horiz. ties	-0.04 (0.04)				-0.04 (0.08)			
Econ obj. X Horiz. ties		0.01 (0.02)				0.04 (0.03)		
Econ subj X Horiz. ties			0.01 (0.02)				0.02 (0.03)	
Educ X Horiz. ties				0.01 (0.01)				0.03 (0.02)
Econ F-test (p-value)	0.51	0.71	0.64	0.64	0.63	0.39	0.54	0.43
Observations	1158	1158	1158	1158	1158	1158	1158	1158
Clusters	570	570	570	570	570	570	570	570
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Weighted least squares with municipality fixed effects and individual controls.

Standard errors account for clustering by survey sampling blocks.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$