NOT FOR PUBLICATION

Supplementary Online Appendices

Wartime Ties and the Social Logic of Crime

Appendices

A Formal Model

We develop a simple model to clarify why, when, and how wartime ties—namely vertical ties to former commanders and horizontal ties to ex-combatant peers—influence ex-combatant criminality. The model considers an agency relationship between a "commander," who seeks to organize criminal activity, and a "recruit." One may interpret the commander and recruit as referring to authority-agency relationships at any level in the ranks. The roles are local to any pair of actors for which one (commander) is in an authority position relative to the other (recruit) in organizing the criminal activity.

The recruit's network connections are summarized in terms of their effects on risks that the commander and recruit face as well as the recruit's outside options.[1] We interpret these effect as capturing how recruits' horizontal network ties function to affect *capacities* for either crime or legal sector activity and how these ties function as vectors of *norm* transmission.

Suppose that a commander has a budget of $W \in \mathbb{R}_{++}$ to use for paying a wage $w \geq 0$ for recruits to undertake criminal activity. A recruit undertakes criminal activity at the expense of having less time to devote to an outside option (e.g., legal labor) that pays $l(x_r, n_r)$, where $l : \mathbb{R}_+ \to \mathbb{R}_{++}$, with $\partial l / \partial x_r \geq 0$, $\partial l / \partial n_r \geq 0$, and $x_r \geq 0$ measures the recruit's human capital and other non-network related determinants of economic opportunity, $n_r \geq 0$ measures the the strength of the association between the recruit and other combatants. By $l(\cdot)$, the stronger the network connections a recruit has, the higher are the recruit's potential returns from the outside option. This is one of the ways that horizontal network ties work to enhance the recruit's *capacities*, albeit in the legal sector. A recruit has a fixed budget of effort $E \in \mathbb{R}_{++}$ to allocate between criminality, e, versus legal labor $E - e$. The recruit receives income on the basis of this effort allocation but also faces potential risks to criminal activity. We assume these risks are convex in the amount of effort put into the criminal activity,

$$U_r = ew + (E - e)l(x_r, n_r) - c_r(n_r)\frac{e^2}{2}, \tag{1}$$

where the last term specifies the risks of criminal as squared loss, scaled by $c_r(\cdot)/2$, with

$$c_r : \mathbb{R}_+ \to \mathbb{R}_{++} \text{ and } c'_r < 0. \tag{2}$$

The function $c_r(\cdot)$ is a reduced form characterization of factors determining the likelihood of punishment and the severity of such punishment. This function, then, captures both the idea of network ties as affecting *capacities* and as vectors of *norm* transmission that induce the recruit to prefer criminality. In terms of capacities, it could be that network ties allow a recruit to receive more information about potential law enforcement activity or to have access to people who can help with protection or to hide. In terms of vectors of transmission for social norms, the idea is that exposure to other criminals reduces one's sensitivity to being perceived as a criminal, thus eliminating that as a potential cost to criminal effort. To keep the analysis simple and to focus on the key (partial)

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[1]To keep things simple and to focus on key dynamics, we assume no commitment problems or hold-up problems.

relationships, we assume that n_r is fixed with respect to e.

The commander earns a rate of return, $v > 0$, from the recruit's criminal effort, but also faces risks from inducing criminal activity,

$$U_c = (v - w)e - c_c(n_r, p_{cr})\frac{e^2}{2}, \tag{3}$$

where we have

$$c_c : \mathbb{R}_+ \times \mathbb{R}_+ \to \mathbb{R}_{++}, \partial c_c/\partial n_r < 0, \text{ and } \partial c_c/\partial p_{cr} < 0, \partial^2 c_c/\partial c_c \partial p_{cr} < 0, \tag{4}$$

and then $p_{cr} \geq 0$ measures the strength of association between the commander and recruit. The effect of n_r on $c_c(\cdot)$ again captures the idea of a recruit's network connections as a resource that enhances the *capacity* to guard against capture by law enforcement. The effect of p_{cr} on $c_c(\cdot)$ captures the idea that close vertical ties between the recruit and commander are the basis of a commander's perception of the recruit's trustworthiness. Again, we assume that n_r and p_{cr} are fixed with respect to w.

There are two ways that the model allows us to distinguish the function of the recruit's horizontal ties as *capacities* versus vectors of *norm* transmission. The first is through the way that networks, as resources, affect the recruit's outside options, captured by $l(x_r, n_r)$. The logic here is that a recruit's exposure to norms of criminality have no effect on the value of a recruit's options outside of criminality, but that the resource value of being a person with lots of horizontal network ties may be transferable across domains. The second is through the way that network resources have the potential to soften the risks that the *commander* faces in engaging the recruit to commit criminal effort. In internalizing norms that promote criminality, the recruit reduces his own personal *sensitivity* to risks, but this does not affect the material risks of capture that are then transferable to the commander. We work with these distinctions below to develop observable implications that allow us to distinguish between the effects of recruits' network ties as being mediated by capacities versus norms.

A.1 Equilibrium

We suppose the sequence of moves is that the commander chooses w, then the recruit chooses e, and then payoffs are determined as per U_r and U_c. We suppose that W is large enough such that interior solutions exist.[2] We solve the game via backward induction. Given a criminal wage offer, w, the recruit will exert criminal effort

$$e^*(w) = \max\left\{0, \frac{w - l(x_r, n_r)}{c_r(n_r)}\right\}, \tag{5}$$

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[2]If we relax this condition, then we would have the mechanical implication that for sufficiently low values of W we would tend to have lower levels of criminal effort.

where positive effort depends on whether $w > l(x_r, n_r)$. That being the case, the optimal criminal wage offer by the commander is

$$w^*(e^*) = \frac{\left(1 + \frac{c_c(n_r, p_{cr})}{c_r(n_r)}\right) l(x_r, n_r) + v}{2 + \frac{c_c(n_r, p_{cr})}{c_r(n_r)}}.$$ (6)

so long as $v > l(x_r, n_r)$. When this inequality does not hold, then the commander would not be able to induce any effort for any wage offer between 0 and $w^*(e^*)$ (and is therefore agnostic about any of them).

In equilibrium, the amount of criminal effort by the recruit is given by,

$$e^*(w^*) = \begin{cases} \frac{v - l(x_r, n_r)}{2c_r(n_r) + c_c(n_r, p_{cr})} & \text{if } v > l(x_r, n_r) \\ 0 & \text{if } l(x_r, n_r) \geq v \end{cases}.$$ (7)

Insofar as human capital and other non-network related economic endowments have a substantial effect on a recruit's legal sector opportunities (that is, $\partial l / \partial x_r >> 0$), then increases in such endowments should have an unambiguously negative effect on a recruit's criminality. This is due to the first order effect of reducing the appeal of criminality from the vantage point of the recruit, and a possible second-order effect of making recruits too expensive for them to be worthwhile from the vantage point of a commander.

Our empirical analysis considers ex-combatants' horizontal connections to other ex-combatants, captured here by n_r, as well as vertical connections to commanders, captured here by p_{cr}. We can use these equilibrium results to study the effects of changes in n_r and p_{cr}.

A.2 Effects of increasing vertical ties

Effects of vertical ties are relatively simple so we consider them first. Consider first the effects of changes in p_{cr}. All else equal, an increase in p_{cr} weakly induces increased criminal effort, as

$$\frac{\partial e^*(w^*)}{\partial p_{cr}} = -\frac{\partial c_c}{\partial p_{cr}} \frac{v - l(x_r, n_r)}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} > 0$$ (8)

when $v > l(n_r)$ and zero otherwise. Similarly, the effects on the equilibrium wage are given by,

$$\frac{\partial w^*(e^*)}{\partial p_{cr}} = -\frac{\partial c_c}{\partial p_{cr}} \frac{c_r(n_r)[v - l(x_r, n_r)]}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} > 0$$ (9)

when $v > l(x_r, n_r)$ and zero otherwise. The intuition is that the recruit's increasing responsiveness to closer network connections induces a higher wage and, consequently, more criminal effort. It could also be the case that the recruit's costs ($c_r(\cdot)$) also decreased in strength of ties to the commander (e.g., through the commander offering protection to those close to him). This would amplify the effect of an increase in p_{cr} on the recruit's criminal effort, although the sign of the effect on the wage would depend on the relative strength of the mediating effects on the commander's versus the

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recruit's costs to criminal effort. The intuition is that the mediating effect on the recruit's costs makes it such that the recruit is willing to exert more effort at a lower wage, which moderates the effect and may even induce a negative effect on the wage offer.

A.3 Effects of increasing recruit's horizontal ties

Consider now the effects of changes in horizontal ties, n_r, holding all else equal. Effects on levels of criminal effort are given by,

$$\frac{\partial e^*(w^*)}{\partial n_r} = -\frac{[v - l(x_r, n_r)]\left[\frac{\partial c_c}{\partial n_r} + 2\frac{dc_r}{dn_r}\right]}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} - \frac{\frac{\partial l}{\partial n_r}}{2c_r(n_r) + c_c(n_r, p_{cr})} \tag{10}$$

when $v > l(x_r, n_r)$ and zero otherwise. The sign of (10) depends on whether the combined effect of changes in n_r on the recruit's and commander's risks is larger (in absolute value terms) than the effect on the value of the outside option. If this is the case, then (10) will be positive, otherwise, it will be negative. Effects on the equilibrium wage are given by the rather complicated expression

$$\frac{\partial w^*(e^*)}{\partial n_r} = \frac{[v - l(x_r, n_r)]\left[\frac{dc_r}{dn_r}c_c(n_r, p_{cr}) - \frac{\partial c_c}{\partial n_r}c_r(n_r)\right] + \frac{\partial l}{\partial n_r}\left\{c_c(n_r, p_{cr})^2 + [3c_c(n_r, p_{cr}) + 2c_r(n_r)]c_r(n_r)\right\}}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} \tag{11}$$

when $v > l(x_r, n_r)$ and zero otherwise. Note now that the sign of (11) depends on whether the combined effect of horizontal ties on the outside option and commander's risks (that is, $\frac{\partial c_c}{\partial n_r}$ and $\frac{\partial l}{\partial n_r}$) are larger (in absolute value terms) than the effect on the recruit's risk ($\frac{dc_r}{dn_r}$). If so, then the sign is positive, otherwise negative. Consider how we would interpret a negative effect of horizontal ties on criminal wages. Again, this would imply that the effect on the recruit's risks dominate over the effect on the outside option and the commander's risk. We argue that this would lend support to the *norms* mechanism that relates horizontal ties to crime. The reason is that the norms effect only applies to the recruit's assessment of risks: being a member of a group that valorizes criminality does not reduce the commander's risk, only the recruit's assessment of the costs of engaging in crime. Alternatively, if horizontal ties *increase* the wages offered, this must be because it reduces commander's perceived risks, indicative of a capacity effect, or increases the recruit's legal sector opportunities, which is likely also indicative of a capacity effect, albeit for pursuing legal sector opportunities.

Other ways to tease out the norms versus capacity effects would be in looking at moderator effects. If the norms effect is operative, factors that enhance the power of norms should simultaneously moderate the effect of network ties on criminal effort *positively* but on the wage offer *negatively*. If the capacity effect is operative, factors that enhance the role of resources should moderate the effect of network ties on the wage offer *positively*, although the effect on effort is ambiguous and likely to be smaller in magnitude than the effect on the wage offer.

A.4 Interaction effects between horizontal and vertical ties

We can also consider how change in vertical and horizontal ties interact. The interaction effect for equilibrium criminal effort is given by,

$$\frac{\partial^2 e^*(w^*)}{\partial p_{cr} \partial n_r} = \frac{v - l(x_r, n_r)}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} \left[\frac{\partial c_c}{\partial p_{cr}} \left(\frac{\frac{\partial l}{\partial n_r}}{v - l(x_r, n_r)} + \frac{2\left(\frac{\partial c_c}{\partial n_r} + 2\frac{dc_r}{dn_r} \right)}{2c_r(n_r) + c_c(n_r, p_{cr})} \right) - \frac{\partial^2 c_c}{\partial p_{cr} \partial n_r} \right], \quad (12)$$

The sign of the interaction effect depends on the value of the outside option and the relative strength of the effect of horizontal ties on the value of the outside option (that is, $\partial l / \partial n_r$). If this effect is strong or if the value of the outside option is high enough to render $v - l(n_r)$ very small, then the sign could be negative. Otherwise, the sign will be positive, indicative of complementarity between the two types of network ties in affecting levels of criminality. and wages. The same results hold for wages (which we omit to save space). As such negative interaction effects would be consistent with horizontal ties have a capacity effect on outside options that is much stronger than the commander's or recruit's risks. Positive interaction effects are not particularly telling in distinguishing capacity effects on criminality from the norms effect, however.

A.5 Interaction effects between horizontal ties and economic endowments

Finally, we consider how variation in economic endowments, x_r, may moderate the effects of horizontal ties. Here the interaction effects are straightforward, as the economic endowments feature only in the legal sector opportunity costs term, $l(x_r, n_r)$:

$$\frac{\partial^2 e^*(w^*)}{\partial n_r \partial x_r} = \frac{\frac{\partial l}{\partial x_r} \left[\frac{\partial c_c}{\partial n_r} + 2\frac{dc_r}{dn_r} \right]}{[2c_r(n_r) + c_c(n_r, p_{cr})]^2} - \frac{\frac{\partial^2 l}{\partial n_r \partial x_r}}{2c_r(n_r) + c_c(n_r, p_{cr})}. \quad (13)$$

In this case, we presume that the scale of the first order effect ($\frac{\partial l}{\partial x_r}$) is larger than the second-order interaction effect ($\frac{\partial^2 l}{\partial n_r \partial x_r}$), in which case the interaction should produce a negative effect (given that $\frac{\partial c_c}{\partial n_r} + 2\frac{dc_r}{dn_r}$ is negative). This reflects the idea that as legal sector opportunities become more rewarding, any draw that horizontal ties produces toward criminality would diminish.

B Sampling

The study relies on a representative survey of 1158 ex-combatants. They survey was conducted between November 2012 and May 2013 by enumerators in the respondents local language, Spanish. The sample consists of two parts:

1. A sample of 268 ex-combatants who were in prison at the time of the study.

2. A sample of 890 ex-combatants who were not in prison.

These sample sizes were determined on the basis of achieving adequate statistical precision given the amount of funds available to do the project. We base the sample size calculations on a simple difference in exposure means for a case-control analysis, where an exposure is the independent variable of interest and the outcome of interest defined case or control status. To construct the sample, we gained access to a database of the entire population of 54,750 ex-combatants who surrendered their weapons and demobilized. From this database, we constructed a list of all municipalities in Colombia that had at least 50 ex-combatants and that were accessible to the OAS Peace Mission (MAPP-OEA).[3] Excluding municipalities with fewer than 50 ex-combatants implied a coverage loss of only 15 percent of the ex-combatant population while allowing us to maximize our resources and concentrate our effort in places where issues related to ex-combatants were most salient. Of the 136 municipalities with 50-or-more ex-combatants, 83 were covered by the MAPP-OEA.

The municipalities were then split into two main strata. Stratum 1 included municipalities with more than 50 and fewer than 700 ex-combatants, while stratum 2 included municipalities with 700 ex-combatants or more. A Neyman allocation suggested that we optimally divide the sample 50-50 over these two strata. Thus, we targeted about 468 individuals in each of these main strata. Stratum 2 was a certainty stratum: all 11 municipalities in stratum 2 (Barranquilla, Bogot, Cali, Cucuta, Florencia, Medellin, Monteria, Santa Marta, Tierralta, Valledupar, and Villavicencio) were included as sites for the survey while we randomly sampled municipalities from stratum 1. To boost the power of the study, we further stratified the municipalities in stratum 1 into homogeneity strata using variables that were important for our study: geographical region, total number of ex-combatants, proportion of ex-combatants charged with a crime, proportion of ex-guerrillas relative to ex-paramilitaries, and levels of violence, measured in terms of homicide rates. The homogeneity strata were created using minimum-Mahalanobis-distance clustering on the ex-combatant attribute and violence variables within geographical regions.[4] The clustering algorithm produced 18 homogeneity strata that provided reasonable gains in terms of intra-cluster correlation values of the stratification variables. We randomly drew two units from each of the homogeneity strata to ensure representativeness with respect to the stratification variables for a total of 36

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[3]We obtained the size of the number abstaining due to death by merging our dataset of all ex-combatants with data from Medicina Legal (the Colombian Forensics Department). It is small enough (<5 percent) such that we do not correct for this potential source of bias.

[4]We used the block() function in the blockTools package as well as the matchit() function in the MatchIt package for R to conduct the minimum Mahalanobis-distance clustering.

municipalities.[5] The enumeration areas were constructed within the $36 + 11 = 47$ municipalities selected from strata 1 and 2. Each municipality included in the sample was assigned a sampling rate. The rate for municipalities in stratum 2 (the certainty stratum) was 4.38% at the municipality level. The rate for municipalities in stratum 1 was 15.6%. The design of the sample ensures that the study is representative of all of the regions in the country where ex-combatants are living and MAPP-OEA is present.[6]

The next step for the non-prisoner sample was to select the enumeration areas from the 47 municipalities. For municipalities with fewer than 150 ex-combatants, the enumeration area was defined as the entire municipality. This applied to 17 of the municipalities. We sought GIS information on the location and number of comunas and *barrios* (counties and neighborhoods) within each of the municipalities with 150 or more ex-combatants to generate more manageable enumeration areas in these places. Unfortunately, out of the 30 municipalities with more than 150 ex-combatants, GIS information was available for only 14 of them. Thus, we had no choice but to define enumeration areas as the entire municipality for the 16 municipalities with more than 150 ex-combatants for which no GIS information was available. In the 14 municipalities for which we had GIS information, we selected clusters in a manner that tried to equalize as much as possible the sampling probabilities for individual ex-combatants in the municipality.

The procedure for each municipality was as follows: 1) We first defined primary sampling units (PSUs) to be comunas in municipalities where comuna information was available, and the municipality itself in places where comuna information was not available; 2) PSUs were then broken down into equally sized secondary sampling unit (SSU) clusters. We set a provisional number of SSU clusters per PSU by dividing the size of the PSU by 50 and rounding to the nearest whole number. We then demarcated the SSU clusters on maps by grouping equally sized clusters of *barrios* together. (Figure B.1 provides an example of these clusters.) The provisional number of SSU clusters was then modified to account for complications that appeared in the maps namely, if there were too few *barrios* in a comuna to generate the provisional number of SSU clusters, then we created the maximal number of SSU clusters possible given the actual number of *barrios* available. Once all of these SSU clusters were created, they were put into a list of SSU clusters for the municipality. 3) We defined the number of SSU clusters to be sampled from a municipality by dividing the municipality-level sample size by 10 and rounding to the nearest whole number. We then randomly sampled the desired number of SSU clusters from the list for that municipality. As described above, in some cases (e.g., municipalities with less than 150 ex-combatants), we work with the entire municipality. In larger municipalities for which barrio level information was available, we work with the clusters of *barrios*.

Given that ex-combatants tend to relocate often, we then had local ACR psychologists, who

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[5]That implied that each municipality contribute on average 1.33 enumeration areas with enumeration areas always having 125 or fewer ex-combatants in them.

[6]MAPP-OEA worked in areas with the highest density of ex-combatants, which included areas with a lot of crime. The areas in our sample therefore tend to be the most relevant locations for studying criminality but they also tend to be more urban and population-dense municipalities.

Figure B.1: Example of SSU clusters
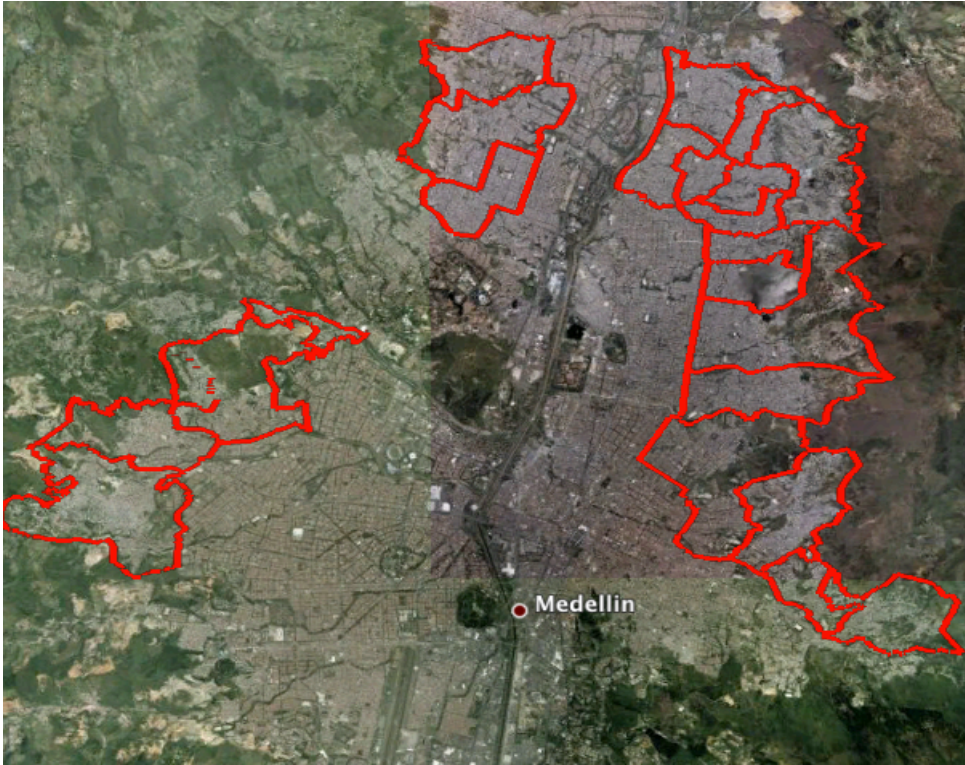
work with the ex-combatants weekly, update our database in order to generate a complete and up to date list of ex-combatants in our selected localities just prior to sampling. We then draw a random sample of these participants, with proportional stratification on former armed group (ex-guerrilla or ex-paramilitary), year of demobilization, whether charged with a crime, department of residence, and Law 1424 status (see below).

To construct the prisoner sample, we generated a list of all medium and high security prisons associated with the selected municipalities. Merging our database of all ex-combatants with information from the Colombian Penitentiary System, INPEC, we were able to determine the population of imprisoned ex-combatants. We selected prisons in our selected municipalities that contained at least 25 ex-combatant prisoners. Merging our data with those of the Colombian Attorney General (Fiscalía), we checked to verify that individuals in these prisons tended to have committed the crimes in the selected municipalities. There were 18 prisons that satisfied these criteria. We drew our sample of 268 prisoners from INPEC lists of ex-combatant inmates who were residing in these prisons. We crossed these lists with Fiscalía data on ex-combatants imprisoned under the Justicia y Paz Law to exclude these individuals who had committed acts of violence before demobilizing. The prisoners were selected in a manner that balanced, as much as possible, ex-guerrilla versus ex-paramilitary status. Based on Attorney General data, we focused our sampling on prisoners who were already convicted and sought to choose ex-combatants who were residents of a non-prisoner sample municipality prior to being captured and imprisoned. We also sampled prisoners

at a higher rate than we sampled non-prisoners. We used weighting to ensure that this does not bias our analysis.

It is important to note that our sampling frame was likely the best that could obtain in Colombia for studying ex-combatants. Our study was developed concurrently to Law 1424 (2010), which mandated, among other rulings, that all ex-combatants participate in the ACR reintegration program, contribute to reparation and constructing the historic truth, and engage in community service in their neighborhoods. In exchange, they received a suspension of their judicial sentences. In addition to the robust incentives to become 'locatable provided by the laws structure, a large propaganda campaign conducted by the ACR and MAPP-OEA further guaranteed that the maximum number showed up, entering our sample frame. While those that did not come forth likely differ from those that did, this provides us some insights into the 'non-locatable population. Comparing reintegration program administrative data from 2010 (pre-1424) with administrative data from 2012 (post-1424), we were able to identify who 'returned to the program. We stratified on this proxy for Law 1424.[7]

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[7]We did not gain access to data on Ley 1424 until after the data collection was complete.

C Confidentiality Procedure

To collect sensitive survey data on criminal behavior, we used a self-administered survey accompanied by an extensive confidentiality procedure. As described below, the confidentiality procedure was designed to ensure that no one in Colombia (other than the respondent) would be able to link the enumerated and self-administered portions of the survey to each other *or* to the identity of the respondent.

Steps to ensure confidentiality started with the creation of survey identification codes. Upon receiving a de-identified list of sampled respondents, the authors created two different identification codes (one for the enumerated survey and one for the self-administered survey). These codes were sealed in tamper resistant envelopes and sent to the survey enumeration teams in Colombia. When a respondent arrived at the survey site, a team leader first administered consent to participate in the study. This included informing the respondent of the purpose of the interview (including the focus on criminality), the risks and benefits of participation (as approved by our IRB), and the steps that were being taken to ensure the confidentiality of their responses. Upon giving consent, the respondent was given an envelope with the two codes sealed inside and asked to inspect it for signs of tampering. If there was any suggestion of tampering, the respondent was instructed to ask for a different envelope. Once a respondent was satisfied with the envelope's security, s/he proceeded to meet with the interviewer and complete the enumerated portion of the survey.

At the end of the enumerated portion of the survey, the interviewer erected a privacy screen between himself and the respondent. The respondent was then asked to open the envelope and hand one label sticker to the interviewer to afix to the main instrument. The respondent then afixed the second label in the appropriate spot on the self-administered survey. In this way, the respondent was the only individual in Colombia to ever see these two labels together. The enumerator then proceeded to read the questions aloud (to assist with focus and literacy) while the respondent privately recorded his responses. Upon finishing, the respondent sealed the self-administered survey in an envelope and deposited it with a large number of other envelopes. The envelopes were transported by MAPP-OEA diplomatic pouch to Bogotá for data entry.

D Survey Measures

This Appendix contains the question wordings for the main dependent, independent, and mechanism variables obtained from the survey and used in the analysis, as described in Section 4 of the main text. Specifically, we provide question wordings for:

- The three measures used to determine 'proven' criminality in the survey data.

- The measures of employment and education used to test economic opportunity costs.

- The 12 measures used to construct the index of objective economic welfare.

- The three measures used to construct the index of subjective economic welfare to test.

- The six measures used to construct the index of strength of ties to former commanders.

- The seven measures used to construct the index of strength of ties to ex-combatant peers.

- The measures used to test the capacities and norms mechanisms, including the measure of wage offers and the six measures used in the 'criminal norms' index.

For each question we provide the Spanish version (which is what was used in the actual enumeration) and the English translation. The complete survey instruments are available upon request.

DEPENDENT VARIABLES (Survey, three measures)	
P_II_ 20 ¿Desde que se desmovilizó, en cuántos incidentes ilegales ha participado usted por iniciativa propia (sin recibir ningún tipo de instrucción por parte de nadie más)	P_II 10 Since you demobilized, in how many illegal incidents have you participated in by your own initiative (without receiving any kind of instruction from someone else)
P_II_ 22 ¿Desde que se desmovilizó, en cuántos incidentes ilegales ha participado usted como parte de un grupo o banda?	P_II 22 Since you demobilized, in how many illegal incidents have you participated in as part of a group or criminal gang?
P_II_ 25 ¿En qué tipo de actividades ilegales ha participado usted? No estamos interesados en conocer incidentes específicos, sólo queremos tener una idea general. A continuación voy a leer una lista de diferentes actividades, si usted ha realizado alguna de estas actividades, marque la casilla que está al lado de la opción.	P_II 25 In what type of illegal activities have you participated in? We are not interested in knowing specific incidents; we just want to have a general idea. I will now read a list of different illegal activities, if you have done one of these, mark the box next to the option.
VERTICAL TIES TO FORMER COMMANDERS (Six measures)	
P66 Un año después de desmovilizarse, ¿qué tan seguido se comunicaba con […]? Su superior inmediato 1 Nunca 2 Rara vez 3 Algunas veces 4 A menudo	P66 One year after you demobilized, how often did you communicate with […]? Your immediate superior 1 Never 2 Rarely 3 Sometimes 4 Often
P68 Un año después de desmovilizarse, si lo hubiera necesitado ¿cuánto tiempo cree usted que le hubiera tomando enviarle un mensaje a […]? Su superior inmediato 1 Un día o menos 2 Algunos días 3 Menos de una semana 4 Más de una semana 5 Nunca	P68 One year after you demobilized, if you had needed to, how long do you think it would take for you to get a message to […]? Your immediate superior 1 One day o less 2 A few days 3 Less than a week 4 More than a week 5 Never
P69 Un año después de desmovilizarse, si su superior inmediato le hubiera pedido que le prestara dinero, ¿usted se la hubiera prestado? 1 Definitivamente si 2 De pronto 3 Probablemente no	P69 One year after you demobilized, if your immediate superior asked you to loan him money, would you do it? 1 Definitely yes 2 Maybe 3 Probably no

4 Definitivamente no	4 Definitely no
P66 One year after you demobilized, how often did you communicate with […]? El superior de su superior 1 Nunca 2 Rara vez 3 Algunas veces 4 A menudo	P66 One year after you demobilized, how often did you communicate with […]? Your superior's superior 1 Never 2 Rarely 3 Sometimes 4 Often
P68 Un año después de desmovilizarse, si lo hubiera necesitado ¿cuánto tiempo cree usted que le hubiera tomando enviarle un mensaje a […]? El superior de su superior 1 Un día o menos 2 Algunos días 3 Menos de una semana 4 Más de una semana 5 Nunca	P68 One year after you demobilized, if you had needed to, how long do you think it would take for you to get a message to […]? Your superior's superior 1 One day or less 2 A few days 3 Less than a week 4 More than a week 5 Never
P69 Un año después de desmovilizarse, si […] le hubiera pedido que le prestara dinero, ¿usted se la hubiera prestado? El superior de su superior 1 Definitivamente si 2 De pronto 3 Probablemente no 4 Definitivamente no	P69 One year after you demobilized, if […] asked you to loan him money, would you do it? Your superior's superior 1 Definitely yes 2 Maybe 3 Probably no 4 Definitely no
HORIZONTAL TIES TO EX-COMBATANT PEERS (Seven measures)	
P150 [UN AÑO DESPUÉS DE DESMOVILIZARSE] De la gente que usted conocía y lo conocía a usted, ¿qué proporción era excombatiente? 1 Casi todos 2 Más de la mitad 3 Menos de la mitad 4 Casi ninguno	P150 [ONE YEAR AFTER DEMOBILIZING] Of the people you knew and that knew you, what proportion was ex-combatant? 1 Almost all 2 More than half 3 Less than half 4 Almost none
[UN AÑO DESPUÉS DE DESMOVILIZARSE], ¿usted tuvo contacto con […]?	[ONE YEAR AFTER DEMOBILIZING], were you in contact with […]? P151 Ex-combatants from other groups

P151 Excombatientes de grupos armados diferentes al suyo 0 No 1 Si	0 No 1 Yes
P153 ¿Con qué frecuencia interactuaba usted con excombatientes de su mismo grupo armado? 1 Nunca 2 Algunas veces al mes 3 Algunas veces a la semana 4 Casi todos los días 5 Varias veces al día	P153 How often did you interact with ex-combatants from your former group? 1 Never 2 A few times a month 3 A few times a week 4 Almost everyday Several times a day
P154 Ahora quisiera preguntarle acerca de sus amigos [UN AÑO DESPUÉS DE DESMOVILIZARSE]. ¿Con qué frecuencia pasaba el tiempo con […]? Amigos que también son excombatientes 1 Nunca 2 Rara vez 3 Algunas veces 5 Seguido	P154 Now I would like to ask you some questions about your friends [ONE YEAR AFTER DEMOBILIZING]. How often did you spend time with […]? Friends who were also former combatants 1 Never 2 Rarely 3 Sometimes 4 Often
P154 Ahora quisiera preguntarle acerca de sus amigos [UN AÑO DESPUÉS DE DESMOVILIZARSE]. ¿Con qué frecuencia pasaba el tiempo con […]? Amigos que son combatientes actualmente 1 Nunca 2 Rara vez 3 Algunas veces 4 Seguido	P154 Now I would like to ask you some questions about your friends [ONE YEAR AFTER DEMOBILIZING]. How often did you spend time with […]? Friends who were in an armed group 1 Never 2 Rarely 3 Sometimes 4 Often
P155 [UN AÑO DESPUÉS DE DESMOVILIZARSE], ¿quiénes eran sus amigos más cercanos; las personas con las que más pasaba tiempo (en el día a día)?	P155 [ONE YEAR AFTER DEMOBILIZING]. Who were your closest friends; the people you spent time with on a day-to-day basis?

P157 Amigos más cercanos: (Nota: Haga una lista de los sobrenombres teniendo en cuenta la lista de la P155 antes de completar las descripciones) Su amigo era […] 1 Amigo/familia de antes de ingresar al grupo armado (no combatiente) 2 Amigo que conoció después de desmovilizarme (no combatiente) 3 Combatiente desmovilizado 4 Combatiente actualmente	P157 Closest friends: (Note: List short names/nicknames taking into account the list P155 before completing the descriptions) Your friend was […] 1 Friend/family from before joining the armed group (not combatant) 2 New friend since demobilization (not combatant) 3 Demobilized combatant 4 Combatant right now
P156 ¿Quiénes eran tres personas a las que usted les hubiera podido pedir prestado dinero en el caso de una emergencia? La lista puede incluir personas que ha mencionado anteriormente o personas que no ha mencionado hasta el momento. P158 Ayudaría en una emergencia: (*Nota: Haga una lista de los sobrenombres teniendo en cuenta la lista de la* P156 *antes de completar las descripciones)* Su amigo era […] 1 Amigo/familia de antes de ingresar al grupo armado (no combatiente) 2 Amigo que conoció después de desmovilizarme (no combatiente) 3 Combatiente desmovilizado 4 Combatiente actualmente	P156 Who were three people you could have borrowed money from in an emergency? The list can include people you have already mentioned or other people. P158 Would help in an emergency: (Note: List short names/nicknames taking into account the list P156 before completing the descriptions Your friend was […] 1 Friend/family from before joining the armed group (not combatant) 2 New friend since demobilization (not combatant) 3 Demobilized combatant 4 Combatant right now

EMPLOYMENT	
P136 ¿Cuál de las siguientes opciones describiría mejor su situación laboral? [UN AÑO DESPUÉS DE DESMOVILIZARSE] 1 Trabajando sin buscar trabajo adicional 2 Trabajando, buscando trabajo adicional 3 Desempleado, buscando trabajo 4 Desempleado sin buscar trabajo 5 Estudiando 6 Hogar 50 Otro	**P136** Which of the following options best describe you working situation [ONE YEAR AFTER DEMOBILIZING] 1 Working and not seeking additional work 2 Working and seeking additional work 3 No job and seeking work 4 No job and not seeking work 5 Student 6 Household 50 Other

ECONOMIC WELFARE INDEX (OBJECTIVE) (Index 12 items)

P137 Ahora quisiera preguntarle sobre lo que hacía <u>DURANTE</u> el primer año después de desmovilizarse. Quiero saber todo lo que hacía para conseguir dinero, incluyendo por ejemplo: su negocio o trabajo regular, trabajos de medio tiempo, trabajos que haya realizado solo una vez, subsidios y rentas. **Nota:** *No incluye ayuda humanitaria de los programas de reintegración- PRVC o ACR.*

B. [Actividad] [Utilizar Código E.]
C. Los pagos por esta actividad eran […]?
    1 Anuales
    2 Mensuales
    3 Quincenales
    4 Semanales
    5 Diarios
    6 Por cosecha
D. En un [PERIODO DE TIEMPO] normal, ¿Normalmente, cuánto dinero recibía usted por esta actividad? Por dinero me refiero a todo el dinero que se llevó a su hogar después del trabajo.
E. Si [PERIODO DE TIEMPO] es '6-Por cosecha': ¿Cuántas cosechas tenía al año?

P137 Now I would like to ask you about what you did for a living <u>DURING</u> your first year after you demobilized. I want to know everything you did to get money including, for example: your business or regular job, part-time jobs, one-time jobs, subsidies and rents. **Note:** Does not include humanitarian aid provided by reintegration programs - *PRVC or ACR.*

B. [Activity] [Use Code E]
C. Payments for this activity were […]?
    1 Annual
    2 Monthly
    3 Two-weekly
    4 Weekly
    5 Daily
    6 Per harvest
D. In a typical [TIME PERIOD], how much money did you get paid for this activity? By money I mean all the money you took home after work.
E. If [TIME PERIOD] is '6-Per Harvest': How many times did you harvest in a typical year?

P132 [UN AÑO DESPUÉS DE DESMOVILIZARSE] ¿Cuál era el material predominante de las paredes exteriores de esa vivienda […]?

1 Bloque, ladrillo, piedra, madera pulida
2 Tapia pisada, adobe
3 Bahareque sin revocado
4 Bahareque sin revocar
5 Madera burda, tabla, tablón
6 Material prefabricado
7 Guadua, caña, esterilla, otro vegetal
8 Zinc, tela, lona, cartón, latas, desechos, plástico
9 Sin paredes
50 Otro

P132 [ONE YEAR AFTER DEMOBILIZING] What was the predominant material of the exterior walls of this dwelling?

1 Bloque, ladrillo, piedra, madera pulida
2 Tapia pisada, adobe
3 Bahareque sin revocado
4 Bahareque sin revocar
5 Madera burda, tabla, tablón
6 Material prefabricado
7 Guadua, caña, esterilla, otro vegetal
8 Zinc, tela, lona, cartón, latas, desechos, plástico
9 Sin paredes
50 Other

P131_A [UN AÑO DESPUÉS DE DESMOVILIZARSE] ¿Usted o su hogar poseía alguno de los siguientes bienes? Nevera 0 No 1 Sí	P131_A [ONE YEAR AFTER DEMOBILIZING] Did you or your home own any of the following goods? Refrigerator 0 No 1 Yes
P131_B Lavadora 0 No 1 Sí	P131_B [Dishwasher 0 No 1 Yes
P131_C TV Color 0 No 1 Sí	P131_C Color TV 0 No 1 Yes
P131_D Motocicleta 0 No 1 Sí	P131_D Motorcycle 0 No 1 Yes
P131_E Carro 0 No 1 Sí	P131_E Car 0 No 1 Yes
P131_F Ahorros en el Banco 0 No 1 Sí	P131_F Savings at a bank 0 No 1 Yes
P133_A [UN AÑO DESPUÉS DE DESMOVILIZARSE] ¿Su vivienda contaba con los siguientes servicios públicos? Acueducto 0 No 1 Yes	P133_A [ONE YEAR AFTER DEMOBILIZING] Did your dwelling have the following public services? Aqueduct 0 No 1 Yes
P133_B Alcantarillado 0 No 1 Yes	P133_B Connected to a sewer 0 No 1 Yes
P133_C Luz Eléctrica 0 No 1 Yes	P133_C Electricity 0 No 1 Yes
P13_D Gas domiciliario por tubería 0 No 1 Yes	P133_D Gas connected to the public network 0 No 1 Yes
ECONOMIC WELFARE INDEX (SUBJECTIVE) (Index 12 items)	
P134 [UN AÑO DESPUÉS DE DESMOVILIZARSE] ¿Qué tan satisfecho(a) estaba con su situación económica […]? 1 Muy satisfecho (a) 2 Satisfecho (a) 3 Insatisfecho (a) 4 Muy insatisfecho (a)	P134 [ONE YEAR AFTER DEMOBILIZING] How satisfied were you with your economic situation […]? 1 Very satisfied 2 Satisfied 3 Dissatisfied 4 Very dissatisfied

P140¿De las siguientes afirmaciones, cuál expresa mejor como se sentía usted con respecto a su ingreso? [UN AÑO DESPUÉS DE DESMOVILIZARSE] 1 Viviendo comodamente con ese ingreso 2 Arreglándoselas con ese ingreso 3 Con dificultades 4 Con muchas dificultades	P140 Which of these statements comes closest to how you felt about your income [ONE YEAR AFTER DEMOBILIZING] 1 Living comfortably at that income 2 Coping at that income 3 Finding it difficult at that income 4 Finding it very difficult at that income
P142 [UN AÑO DESPUÉS DE DESMOVILIZARSE] ¿Usted pensaba que el salario/ingreso que usted recibía(en dinero o en especie) como combatiente eran […] comparando con su ingreso total (incluyendo ayuda humanitaria)? 1 Mucho mayores 2 Mayores 3 Iguales 4 Menos 5 Mucho menos	P142 [ONE YEAR AFTER DEMOBILIZING] Did you think the salary/income you received (in cash or kind) as a combatant was […] compared to your total income (including humanitarian aid)? 1 Much higher 2 Higher 3 Equal 4 Lower 5 Much Lower
EDUCATION	
P135 [UN AÑO DESPUÉS DE DESMOVILIZARSE]¿Cuál era el nivel de educación más alto que usted **había alcanzado** para ese momento? 1 Primaria incompleta (1-4) 2 Primaria completa (1-5) 3 Bachillerato incompleto (6-10) 4 Bachillerato completo (6-11) 5 Formación técnica/tecnológica incompleta 6 Formación técnica/tecnológica completa 7 Formación universitaria incompleta 8 Formación universitaria completa 9 Nunca estudió	**P135** [ONE YEAR AFTER DEMOBILIZING] What was the highest level of education **completed** by you? 1 Primary incomplete (1-4) 2 Primary complete (1-5) 3 High School incomplete (6-10) 4 High complete (6-11) 5 Technical training/*tecnológica* incomplete 6 Technical training/*tecnológica* complete 7 Undergraduate education incomplete 8 Undergraduate education complete 9 Never studied
MECHANISM VARIABLES	
Wage offer	

P_II_15: Desde que se desmovilizó ¿en promedio, y por oferta, ¿cuánto dinero le han ofrecido para ingresar a un grupo armado ilegal? P_II_17: Desde que se desmovilizó, en promedio, y por cada oferta, ¿cuánto dinero le han ofrecido por hacer algo ilegal para un grupo armado ilegal pero NO como parte del grupo?	P_II_15: Since you demobilized, on average how much money have you been offered to join an illegal gang? P_II_17: Since you demobilized, on average, how much money have you been offered to do something illegal for an illegal armed group but not as part of that group?

Criminal norms (index, six measures)

P_II_6 ¿Usted piensa que estaría bien hacer algo ilegal si estuviera desempleado, sin suficiente dinero o estuviera en condiciones de pobreza? 1 No 2 Si 3 Tal vez	P_II_6 Do you think that it would be acceptable to do something illegal if you were unemployed, without enough money or in poverty? 1 No 2 Si 3 Tal vez
P_II_7 ¿Usted piensa que estaría bien hacer algo ilegal si necesitara dinero de manera urgente?	P_II_7 Do you think it would be acceptable to do something illegal if you needed money urgently?
P_II_9 ¿Usted piensa que estaría bien hacer algo ilegal si recibiera amenazas de muerte contra usted o su familia si no hace algo ilegal?	P_II_9 Do you think it would be acceptable to do something illegal if you or your family received death threats if you did not do something illegal?
P_II_11 ¿Usted piensa que estaría bien hacer algo ilegal si quisiera tener una vida llena de aventura y propósito?	P_II_11 Do you think it would be acceptable to do something illegal if you were to want a life full of adventure and purpose?
P_II_12 ¿Usted piensa que estaría bien hacer algo ilegal si fuera rechazado por su familia o su comunidad?	P_II_12 Do you think it would be acceptable to do something illegal if you were to be rejected by your family or community?
P_II_13 ¿Usted piensa que estaría bien hacer algo ilegal si el gobierno no cumpliera con sus promesas?	P_II_13 Do you think it would be acceptable to do something illegal if the government didn't keep its promises?

E Summary Statistics

This appendix presents summary statistics for the sample and the population for all variables used in the analysis (dependent variables, independent variables, mechanism variables, and controls). To be as transparent as possible with our data, we present descriptive statistics for the sample alongside summary statistics for the population. The population statistics incorporate both our sampling weights and ten rounds of predictive-mean-matching imputation to address the small amount of item-level missingness that would nonetheless result in a large number of observations being dropped from the analysis. Measures that capture a single latent concept are aggregated into indices using inverse covariance weighting (Anderson, 2008), as described in the main text. Indices are denoted in the tables with summary statistics for their components beneath. This appendix contains the following tables:

- Table E.1 presents descriptive statistics for the sample for the main dependent variables (corresponding to Table I in the main text) as well as a map showing the spatial distribution of the dependent variable.

- Tables E.2 shows the sample and population summary statistics for the independent variables.

- Tables E.3 shows the sample and population summary statistics for the mechanism variables.

- Tables E.4 shows the sample and population summary statistics for the control variables referred to in Section 4. In Appendix H we discuss our choice of controls and perform additional analysis on how these controls correlate to criminality.

Table E.1: Summary Statistics: Dependent Variables

	Mean	SD	Min	Max	Count	% Missing
Panel A: 'Proven' criminality						
Criminal (admin.)	0.15	0.36	0	1	1158	0.00
Criminal (surv.)	0.28	0.45	0	1	1076	0.07
Ind. crime	0.15	0.35	0	1	1075	0.07
Gang crime	0.06	0.24	0	1	1075	0.07
Reported type of crime(s)	0.26	0.44	0	1	1122	0.03
Criminal	0.34	0.47	0	1	1088	0.06
Panel B: Committed violent crime						
Violent	0.36	0.67	0	2	1107	0.04
Violent crime as an individual	0.32	0.64	0	2	1124	0.03
Violent crime with a gang	0.22	0.54	0	2	1121	0.03

Figure E.1: District map showing the spatial distribution of the dependent variable (binary measure of 'proven' criminality). Estimates are survey weighted averages.
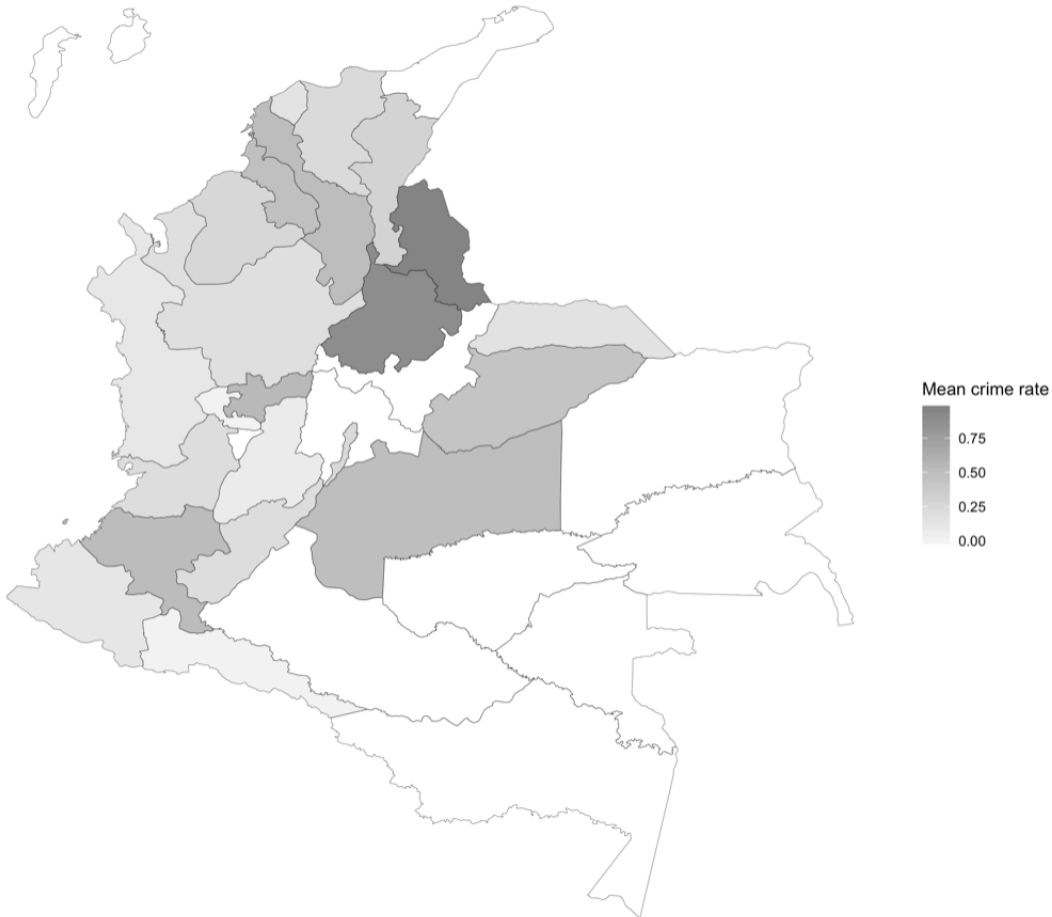
Table E.2: Summary Statistics: Independent Variables

	Min	Max	Mean	SD	Count	% Missing	Mean	SD	Count	% Missing
			Sample				**Population (missingness imputed)**			
Economic factors										
Employed	0	1	0.81	0.39	1115	0.04	0.82	0.39	1158	0.00
Econ welfare obj. (index)							-0.01	0.95	1158	0.00
Total income (000'000)	0	156	7.59	8.59	913	0.21	7.27	6.98	1158	0.00
Exterior wall material	0	1	0.83	0.37	1156	0.00	0.84	0.37	1158	0.00
Fridge	0	1	0.54	0.50	1080	0.07	0.54	0.50	1158	0.00
Dishwasher	0	1	0.26	0.44	1080	0.07	0.26	0.44	1158	0.00
TV	0	1	0.81	0.39	1080	0.07	0.82	0.39	1158	0.00
Motorcycle	0	1	0.16	0.36	1080	0.07	0.16	0.36	1158	0.00
Car	0	1	0.05	0.21	1080	0.07	0.05	0.21	1158	0.00
Savings account	0	1	0.15	0.35	1075	0.07	0.15	0.36	1158	0.00
Running water	0	1	0.90	0.30	1157	0.00	0.91	0.29	1158	0.00
Sewage system	0	1	0.82	0.39	1157	0.00	0.83	0.38	1158	0.00
Electricity	0	1	0.98	0.15	1157	0.00	0.97	0.16	1158	0.00
Gas	0	1	0.49	0.50	1149	0.01	0.50	0.50	1158	0.00
Econ welfare subj. (index)							-0.04	1.00	1158	0.00
Economic satisfaction	1	4	2.32	0.76	1151	0.01	2.33	0.75	1158	0.00
Income satisfaction	1	4	2.06	0.91	1111	0.04	2.03	0.90	1158	0.00
Comparison to combat income	1	5	2.82	1.43	1012	0.13	2.79	1.42	1158	0.00
Education	0	8	2.17	1.31	1158	0.00	2.16	1.27	1158	0.00
Conflict ties										
Vert. ties (index)							-0.08	0.95	1158	0.00
Freq. talk to immed. command.	1	4	1.31	0.77	1149	0.01	1.27	0.72	1158	0.00
Can get a message to immed. command.	1	5	1.88	1.44	1111	0.04	1.80	1.38	1158	0.00
Would lend money to immed. command.	1	4	1.60	1.05	1133	0.02	1.54	0.98	1158	0.00
Freq. talk to high command.	1	4	1.11	0.48	1138	0.02	1.10	0.46	1158	0.00
Can get a message to high command.	1	5	1.52	1.13	1094	0.06	1.48	1.08	1158	0.00
Would lend money to high command.	1	4	1.44	0.93	1124	0.03	1.39	0.86	1158	0.00
Horiz. Ties (index)							-0.08	0.95	1158	0.00
Knows excoms	1	4	1.77	1.05	1150	0.01	1.72	1.03	1158	0.00
Communicates with excom from another group	0	1	0.51	0.50	1154	0.00	0.50	0.50	1158	0.00
Freq of excom interaction	0	4	1.26	1.14	1155	0.00	1.17	1.07	1158	0.00
Friends are excoms	1	4	2.32	0.99	1154	0.00	2.25	0.98	1158	0.00
Friends are combatants	1	4	1.18	0.55	1151	0.01	1.17	0.54	1158	0.00
Portion of friends that are excoms	0	1	0.29	0.41	1128	0.03	0.25	0.39	1158	0.00
Portion of excoms who would help in emergency	0	1	0.19	0.35	1082	0.07	0.17	0.33	1158	0.00

Table E.3: Summary Statistics: Mechanism Variables

			Sample				Population (missingness imputed)			
	Min	*Max*	*Mean*	*SD*	*Count*	*% Missing*	*Mean*	*SD*	*Count*	*% Missing*
Wage offer										
Recruitment wage offer (mean)	0	344828	5368	32416	1124	0.03	5388	32529	1158	0.00
Log wages	0	13	3.30	3.76	1124	0.03	3.07	3.66	1158	0.00
Sympathetic towards crime (index, six items)							-0.07	0.96	1158	0.00
Sympathetic: Econ insecurity	0	1	0.16	0.37	1123	0.03	0.15	0.36	1158	0.00
Sympathetic: Need money	0	1	0.12	0.33	1136	0.02	0.11	0.31	1158	0.00
Sympathetic: Adventure	0	1	0.06	0.23	1137	0.02	0.06	0.23	1158	0.00
Sympathetic: Security threats	0	1	0.28	0.45	1137	0.02	0.24	0.43	1158	0.00
Sympathetic: Rejected by fam/comm.	0	1	0.10	0.30	1127	0.03	0.09	0.29	1158	0.00
Sympathetic: Govt. reneges	0	1	0.18	0.38	1115	0.04	0.16	0.37	1158	0.00

Table E.4: Summary Statistics: Control Variables

		Min	Max	Sample Mean	SD	Count	% Missing	Population (missingness imputed) Mean	SD	Count	% Missing
Panel A: General controls											
(1)	Female	0	1	0.16	0.37	1158	0.00	0.17	0.38	1158	0.00
(2)	Non-white/mestizo	0	1	0.77	0.42	1148	0.01	0.80	0.40	1158	0.00
(3)	Educ level prior to joining	0	8	1.74	1.22	1158	0.00	1.79	1.18	1158	0.00
(4)	Age	19	79	33	7.62	1158	0.00	33	7.58	1158	0.00
(5)	Age joined armed group	5	70	20	7.12	1158	0.00	20	6.99	1158	0.00
(6)	Risk preferences	1	10	3.32	3.26	1153	0.00	3.53	3.31	1158	0.00
(7)	Time discounting	1	10	5.70	3.93	1151	0.01	5.69	3.90	1158	0.00
Panel B: Motivations for initially joining											
Family reasons (index)								0.01	0.98	1158	0.00
(8)	Family relations	1	4	1.90	0.94	1158	0.00	1.94	0.92	1158	0.00
(9)	Family problems	1	4	1.52	1.01	1147	0.01	1.50	1.00	1158	0.00
Grievances (index)								-0.05	0.85	1158	0.00
(10)	Government grievances	1	4	1.44	0.89	1156	0.00	1.41	0.83	1158	0.00
(11)	Joined for grievances	0	1	0.02	0.13	1158	0.00	0.01	0.11	1158	0.00
Ideological motivations (index)								0.00	0.95	1158	0.00
(12)	Support ideology	1	4	1.56	0.91	1157	0.00	1.57	0.89	1158	0.00
(13)	Joined for ideology	0	1	0.03	0.18	1158	0.00	0.03	0.17	1158	0.00
Economic welfare subj. (index)								0.04	0.97	1158	0.00
(14)	Economic situation	1	4	2.92	0.83	1147	0.01	2.94	0.82	1158	0.00
(15)	Money or other material	1	4	2.29	1.25	1158	0.00	2.29	1.25	1158	0.00
(16)	Escape poverty	1	4	2.28	1.24	1158	0.00	2.32	1.22	1158	0.00
(17)	Stable employment	1	4	2.36	1.28	1157	0.00	2.43	1.25	1158	0.00
(18)	Not afraid of losing money	0	1	0.81	0.39	1156	0.00	0.83	0.37	1158	0.00
(19)	Not afraid of losing emp	0	1	0.78	0.41	1152	0.01	0.80	0.40	1158	0.00
(20)	Not afraid of losing educ	0	1	0.41	0.49	1158	0.00	0.42	0.49	1158	0.00
(21)	Not afraid of losing a good life	0	1	0.39	0.49	1157	0.00	0.38	0.49	1158	0.00
Economic welfare obj. (index)								0.04	0.97	1158	0.00
(23)	Fridge	0	1	0.33	0.47	1142	0.01	0.34	0.47	1158	0.00
(24)	Dishwasher	0	1	0.09	0.28	1140	0.02	0.09	0.29	1158	0.00
(25)	Television	0	1	0.44	0.50	1142	0.01	0.46	0.50	1158	0.00
(26)	Motorcycle	0	1	0.09	0.28	1143	0.01	0.08	0.28	1158	0.00
(27)	Car	0	1	0.04	0.20	1143	0.01	0.04	0.20	1158	0.00
(28)	Savings account	0	1	0.07	0.25	1132	0.02	0.09	0.28	1158	0.00
(29)	Wall material	0	1	0.44	0.50	1157	0.00	0.47	0.50	1158	0.00
(30)	Running water	0	1	0.56	0.50	1155	0.00	0.57	0.49	1158	0.00
(31)	Sewage system	0	1	0.42	0.49	1154	0.00	0.45	0.50	1158	0.00
(32)	Electricity	0	1	0.71	0.45	1155	0.00	0.74	0.44	1158	0.00
(33)	Gas	0	1	0.10	0.30	1154	0.00	0.10	0.30	1158	0.00
Social reasons (index)								-0.03	0.99	1158	0.00
(34)	Knew people in the group	1	4	1.98	1.11	1158	0.00	1.94	1.07	1158	0.00
(35)	Joined for social network	0	1	0.04	0.18	1158	0.00	0.04	0.19	1158	0.00

Continued on next page

Table E.4: Summary Statistics: Control Variables

	Min	Max	Mean	SD	Count	% Missing	Mean	SD	Count	% Missing
			Sample				**Population (missingness imputed)**			
Status/power motivations (index)							-0.03	1.03	1158	0.00
(36) Social status	0	1	0.20	0.40	1157	0.00	0.21	0.40	1158	0.00
(37) Military life	0	1	0.45	0.50	1157	0.00	0.43	0.50	1158	0.00
(38) Adventure	0	1	0.36	0.48	1158	0.00	0.33	0.47	1158	0.00
(39) Joined for psych reasons	0	1	0.17	0.37	1158	0.00	0.17	0.37	1158	0.00
Security motivations (index)							-0.03	0.99	1158	0.00
(40) Protection	1	4	1.70	1.09	1158	0.00	1.65	1.07	1158	0.00
(41) Joined for security reasons	0	1	0.06	0.24	1158	0.00	0.06	0.24	1158	0.00
Coerced (index)							-0.02	0.98	1158	0.00
(42) Forced	1	4	1.52	1.03	1109	0.04	1.50	1.01	1158	0.00
(43) Joined because forced	0	1	0.09	0.29	1158	0.00	0.09	0.28	1158	0.00
Panel C: Conflict Experience										
Conflict exposure (index)							-0.06	0.98	1158	0.00
(44) Attacked	1	4	2.03	1.13	1150	0.01	1.95	1.09	1158	0.00
(45) Saw torture	1	4	1.80	0.99	1145	0.01	1.75	0.97	1158	0.00
(46) Forced to harm	1	4	1.26	0.66	1148	0.01	1.24	0.63	1158	0.00
(47) Dangerous situations	1	4	2.68	1.07	1156	0.00	2.65	1.09	1158	0.00
(48) Combat with enemy	1	4	2.56	1.18	1154	0.00	2.53	1.18	1158	0.00
Mid-level commander (index)							-0.09	0.90	1158	0.00
(49) Highest rank achieved was mando medio	0	1	0.16	0.36	1158	0.00	0.12	0.32	1158	0.00
(50) Had soldiers under command	0	1	0.12	0.33	1152	0.01	0.10	0.30	1158	0.00
(51) How many soldiers	0	1	0.11	0.32	1152	0.01	0.10	0.29	1158	0.00
High-level Commander (index)							0.00	1.00	1158	0.00
(52) Highest rank achieved was comandante	0	1	0.01	0.09	1158	0.00	0.01	0.10	1158	0.00
(53) Had soldiers under command	0	1	0.01	0.07	1152	0.01	0.01	0.08	1158	0.00
(54) How many soldiers	0	1	0.02	0.13	1152	0.01	0.01	0.12	1158	0.00
Unit cohesion (index)							0.09	0.99	1158	0.00
(55) Superiors cared	1	4	2.75	1.17	1148	0.01	2.85	1.17	1158	0.00
(56) Felt included	1	4	2.19	1.18	1146	0.01	2.22	1.20	1158	0.00
(57) Pride in unit	1	4	2.44	1.20	1142	0.01	2.53	1.22	1158	0.00
(58) Superiors were effective	1	4	1.61	0.79	1096	0.05	1.69	0.84	1158	0.00
(59) Trusted unit members with your life	1	4	2.89	1.23	1074	0.07	2.98	1.19	1158	0.00
Unit discipline (index)							-0.09	1.05	1158	0.00
(60) Punishment for drug use	0	5	4.03	1.35	1046	0.10	3.96	1.39	1158	0.00
(61) Punishment for harming someone	0	5	4.09	1.26	1052	0.09	3.95	1.31	1158	0.00
(62) Punishment for harming a civilian	0	5	4.38	1.24	968	0.16	4.27	1.34	1158	0.00
(63) Punishment for disobeying orders	0	5	3.93	1.29	1033	0.11	3.78	1.32	1158	0.00
Unit hierarchy (index)							-0.01	1.00	1158	0.00
(64) Comb. never selected targets	1	4	3.64	0.75	1078	0.07	3.68	0.70	1158	0.00
(65) Material goods all went to superiors	1	4	3.57	0.95	942	0.19	3.49	1.04	1158	0.00
Miscellaneous										
(66) Total time in groups	0.08	54	5.68	4.54	1158	0.00	5.36	4.39	1158	0.00
(67) Paramilitary (vs. guerilla)	0	1	0.59	0.49	1158	0.00	0.62	0.49	1158	0.00

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Table E.4: Summary Statistics: Control Variables

			Sample					Population (missingness imputed)			
		Min	Max	Mean	SD	Count	% Missing	Mean	SD	Count	% Missing
(68)	Group freq. indoctrinated	1	4	2.30	1.16	1142	0.01	2.39	1.16	1158	0.00
(69)	Income while in group (000'000)	0	180	4.12	8.48	1157	0.00	4.10	9.73	1158	0.00
(70)	Combatant	0	1	0.86	0.35	1152	0.01	0.85	0.36	1158	0.00
Panel D: Demobilization experience											
(71)	Year of demob.	2003	2012	2006	1.88	1158	0.00	2006	1.81	1158	0.00
(72)	Individual Demob	0	1	0.55	0.50	1158	0.00	0.56	0.50	1158	0.00
(73)	Demob coerced	0	1	0.23	0.42	1148	0.01	0.23	0.42	1158	0.00
(74)	Discontent w/ demob.	0	2	0.21	0.54	1156	0.00	0.20	0.52	1158	0.00
(75)	Minor of age	0	1	0.07	0.26	1158	0.00	0.08	0.27	1158	0.00
(76)	Disabled	0	1	0.12	0.33	1158	0.00	0.14	0.35	1158	0.00
(77)	Settled where operated	0	1	0.14	0.35	1158	0.00	0.15	0.36	1158	0.00
Panel E: Reintegration controls (one year following demob.)											
Particip. in reint prog. (index)								0.12	0.88	1158	0.00
(78)	Duration of participation	0	10.25	5.14	2.40	1086	0.06	5.49	2.19	1158	0.00
(79)	Intensity of participation	1	3	2.73	0.54	1145	0.01	2.77	0.48	1158	0.00
(80)	Participating in reint program	0	1	0.93	0.25	1144	0.01	0.94	0.24	1158	0.00
(81)	Cumulative benefits received	0	7	3.95	1.48	1062	0.08	3.79	1.56	1158	0.00
Registered with the state (index)								0.00	0.99	1158	0.00
(82)	Bank account	0	1	0.54	0.50	1152	0.01	0.56	0.50	1158	0.00
(83)	Certificate of citizenship	0	1	0.45	0.50	1145	0.01	0.44	0.50	1158	0.00
(84)	Health services	0	1	0.85	0.36	1153	0.00	0.85	0.36	1158	0.00
Political voice (index)								-0.06	1.02	1158	0.00
(85)	Speak against government	1	4	1.81	1.03	1143	0.01	1.74	1.00	1158	0.00
(86)	Can change the country	0	1	0.56	0.50	1149	0.01	0.55	0.50	1158	0.00
Shame about group (index)								-0.11	1.01	1158	0.00
(87)	Not proud	0	1	0.70	0.46	1156	0.00	0.69	0.46	1158	0.00
(88)	Ashamed	0	1	0.52	0.50	1156	0.00	0.46	0.50	1158	0.00
(89)	Guilty	0	1	0.55	0.50	1158	0.00	0.49	0.50	1158	0.00
Confident gov. will not renege (index)								0.03	1.01	1158	0.00
(90)	Confident in gov. promises (at demob. time)	0	10	5.89	3.40	1138	0.02	5.85	3.42	1158	0.00
(91)	Confident in gov. promises (1 year later)	0	10	6.15	3.25	1155	0.00	6.24	3.18	1158	0.00
(92)	Overall satisfaction with reintegration prog	1	4	2.83	0.74	1062	0.08	2.81	0.77	1158	0.00
Perceptions of gov. capacity (index)								0.01	0.97	1158	0.00
(93)	National government confidence	0	10	6.15	3.17	1155	0.00	6.22	3.12	1158	0.00
(94)	Local government confidence	0	10	4.19	3.23	1098	0.05	4.18	3.08	1158	0.00
(95)	Confidence in the defensoria del pueblo	0	10	5.92	3.35	976	0.16	5.88	3.30	1158	0.00
(96)	Confidence in police	0	10	4.79	3.47	1154	0.00	4.83	3.41	1158	0.00
Insecurity (index)								-0.08	0.94	1158	0.00
(97)	Moved for security	0	1	0.21	0.40	1153	0.00	0.17	0.38	1158	0.00
(98)	Been in a fight	0	2	0.14	0.43	1157	0.00	0.13	0.38	1158	0.00
(99)	Feel at risk	1	4	2.17	1.24	1154	0.00	2.08	1.22	1158	0.00
(100)	Lost security	0	1	0.16	0.37	1155	0.00	0.16	0.37	1158	0.00

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Table E.4: Summary Statistics: Control Variables

| | | Min | Max | Sample | | | | Population (missingness imputed) | | | |
				Mean	SD	Count	% Missing	Mean	SD	Count	% Missing
(101)	Biggest loss after leaving	0	1	0.09	0.29	1158	0.00	0.09	0.29	1158	0.00
(102)	Gained security or protection after leaving	0	1	0.40	0.49	1155	0.00	0.38	0.49	1158	0.00
Family support (index)								0.04	0.96	1158	0.00
(103)	Family supported demob.	1	5	4.48	1.05	1027	0.11	4.44	1.09	1158	0.00
(104)	Family helps out often	1	4	2.86	1.11	1155	0.00	2.87	1.12	1158	0.00
(105)	Gained ability to be with family after demob.	0	1	0.92	0.28	1156	0.00	0.92	0.27	1158	0.00
(106)	Family was biggest gain after demob.	0	1	0.38	0.49	1158	0.00	0.40	0.49	1158	0.00
(107)	Did not lose family after demob.	0	1	0.94	0.23	1157	0.00	0.95	0.21	1158	0.00
Community support (index)								-0.01	1.00	1158	0.00
(108)	Community knows	0	1	0.44	0.50	1140	0.02	0.43	0.49	1158	0.00
(109)	Positive feelings	1	5	2.46	1.22	1061	0.08	2.50	1.22	1158	0.00
(110)	Protectors	0	1	0.17	0.38	1083	0.06	0.17	0.38	1158	0.00
(111)	Belong	0	1	0.41	0.49	1098	0.05	0.42	0.49	1158	0.00
(112)	Threat	0	1	0.45	0.50	1106	0.04	0.47	0.50	1158	0.00
(113)	No discrimination against excom employment	1	4	1.47	0.86	1134	0.02	1.52	0.89	1158	0.00
(114)	No discrimination in housing	1	4	1.72	1.01	1129	0.03	1.75	1.01	1158	0.00
(115)	No discrimination in starting a business	1	4	2.01	1.09	1121	0.03	2.02	1.08	1158	0.00
(116)	Participation in community organization	0	1	0.33	0.47	1155	0.00	0.30	0.46	1158	0.00
Depression/PTSD (index, six items, does not include anger)								-0.04	1.02	1158	0.00
(117)	Feel sad	1	4	1.99	1.07	1156	0.00	1.94	1.07	1158	0.00
(118)	Feel like hurting yourself or others	1	4	1.23	0.63	1158	0.00	1.23	0.63	1158	0.00
(119)	Feel unimportant	1	4	1.66	0.95	1158	0.00	1.66	0.95	1158	0.00
(120)	Have nightmares or bad memories	1	4	2.08	1.09	1158	0.00	2.04	1.09	1158	0.00
(121)	Avoid places or people	1	4	2.11	1.13	1156	0.00	2.08	1.12	1158	0.00
(122)	Uncertain about the future	1	4	1.67	1.00	1157	0.00	1.64	0.95	1158	0.00
(123)	Anger (separate covar not in index)	0	1	0.04	0.18	1158	0.00	0.04	0.19	1158	0.00

F Types of Crime

Table F.1 provides more detail on the types of crime that ex-combatants self-reported on the survey. Summary statistics are presented for both the sample and the population. For those ex-combatants who self-reported criminal activity, the most common types of crime included criminal intent, carrying or trafficking arms, robbery, physical assault, and drug trafficking.

Table F.1: Types of Crimes Committed

		Min	Max	Sample Mean	SD	Count	Population Mean	SD
Panel A: Self-Reported Criminals Only								
1	Intent	0	1	0.31	0.47	305	0.35	0.48
2	Arms trafficking	0	1	0.30	0.46	305	0.28	0.45
3	Robbery	0	1	0.25	0.43	305	0.24	0.43
4	Causing physical harm	0	1	0.20	0.40	305	0.15	0.36
5	Drugs trafficking	0	1	0.17	0.37	305	0.15	0.36
6	Extortion	0	1	0.13	0.34	305	0.09	0.29
7	Rebellion	0	1	0.04	0.20	305	0.05	0.22
8	Terrorism	0	1	0.03	0.18	305	0.03	0.17
9	Fabrication	0	1	0.02	0.14	305	0.01	0.11
10	Recruitment	0	1	0.01	0.11	305	0.01	0.11
11	Conspiracy	0	1	0.01	0.11	305	0.01	0.12
12	Money laundering	0	1	0.01	0.10	305	0.01	0.08
13	Murder	0	1	0.01	0.08	305	0.01	0.10
Panel B: Full Ex-combatant Sample								
1	Intent	0	1	0.08	0.28	1158	0.07	0.26
2	Arms trafficking	0	1	0.08	0.27	1158	0.06	0.23
3	Robbery	0	1	0.07	0.25	1158	0.05	0.22
4	Causing physical harm	0	1	0.05	0.23	1158	0.03	0.17
5	Drugs trafficking	0	1	0.04	0.21	1158	0.03	0.17
6	Extortion	0	1	0.03	0.18	1158	0.02	0.14
7	Rebellion	0	1	0.01	0.11	1158	0.01	0.10
8	Terrorism	0	1	0.01	0.09	1158	0.01	0.08
9	Fabrication	0	1	0.01	0.07	1158	0.00	0.05
10	Recruitment	0	1	0.00	0.06	1158	0.00	0.05
11	Conspiracy	0	1	0.00	0.06	1158	0.00	0.05
12	Money laundering	0	1	0.00	0.05	1158	0.00	0.04
13	Murder	0	1	0.00	0.04	1158	0.00	0.04

G Using the Survey to Study a Hidden Population

Comparing our administrative and survey data sheds light on the size of the 'hidden' population of criminals and the extent to which the survey succeeded in eliciting admissions from respondents outside of the criminal justice system. Our main coding of criminality defines an ex-combatant as criminal if they were convicted of a crime in the administrative data or self-reported criminal activity in the survey. As Table G.1 shows, 186 respondents who admit to criminal behavior on the survey have not been convicted. Of that 186, 113 were not in prison at the time of the survey. This suggests that the survey identified a sub-population of ex-combatants who self-report criminal activity but who have not been identified by the state.

Table G.1: Comparing criminality in the admin. and survey data (sample)

| | | | Criminal activity (survey) | | | |
			No	Yes	% Missing	*Total:*
	No	n	723	186	70	979
Convicted of		%	62	16	6	85
a crime						
(admin data)	*Yes*	n	48	119	12	179
		%	4	10	1	16
	Total:	$\boldsymbol{n}$	771	305	82	1158
		%	67	26	7	100

Results for the sample (missingness not imputed)

If we were to make the (questionable) assumption that all ex-combatants who are charged with crimes are also criminal, then the survey identifies 97 respondents who are not recognized as criminal in the administrative data. Finally, 76 respondents admit to criminal behavior on the survey but have not been charged with or convicted or a crime *and* are not in prison, implying that they are truly 'hidden' to the criminal justice system. Overall, these breakdowns suggest that—even using different coding schemes—the survey succeeded in detecting a hidden population of criminals when employing a number of different ways of classifying an ex-combatant as criminal.

Table G.2: Hidden population

Panel A: Assuming all convicted and charged are criminal

| | | | Criminal activity (survey) | | | |
			No	Yes	% Missing	Total:
	No	n	540	97	51	688
Convicted of or charged with a crime		%	47	8	4	59
(admin data)	Yes	n	231	208	31	470
		%	20	18	3	41
	Total:	n	771	305	82	1158
		%	67	26	7	100

Panel B: Assuming all convicted, charged, *and* in prison are criminal

| | | | Criminal activity (survey) | | | |
			No	Yes	% Missing	Total:
	No	n	519	76	51	646
Convicted of or charged with a crime or in prison		%	44.82	7	4	56
(admin data)	Yes	n	252	229	31	512
		%	22	20	3	44
	Total:	n	771	305	82	1158
		%	67	26	7	100

Results for the sample (missingness not imputed)

H Control Variables and the Correlates of Criminality

As discussed in Section 4, our analysis makes use of 123 controls obtained from the enumerated survey to mitigate concerns about omitted variable bias driving any observed correlation between our explanatory variables and crime. We select control variables that may be correlated with either economic or social conditions following demobilization and also with criminal activity (to address concerns about confounding), or that may be direct correlates of crime. To reduce the number of covariates we use in the regressions we again use inverse covariance weighting to combine controls where possible into indices, leaving us with a final set of 25 control indices and 20 individual covariates.

We note that while this means that we still include a relatively large number of controls, we do this because there are many potential confounding variables that could introduce spurious correlation between our key independent variables—wartime ties, economic opportunity costs—and crime, and it was not clear *a priori* what we should exclude.[8] Having such a large number of controls would only be a problem if we thought they were introducing bias, but we do not think this is the case for two reasons. First, we run all analysis with and without controls and in no case is a basic finding sensitive to the choice of controls. Second, we might be concerned if we thought that including controls was introducing post-treatment bias (Rosenbaum, 1984), but we are not concerned with this as we took care to ensure that all controls were measured 'pre-treatment by measuring everything one-year following demobilization. All in all, in light of these two factors, we did not feel there was a reason to reduce the number of variables that we use as controls in the analysis beyond what we are already doing by creating indices. This Appendix explains our choice of control variables; below we analyze how our controls correlate with criminality. Summary statistics for all control variables can be found in Appendix E.

Central factors that likely determine the strength of social ties following de-mobilization are the characteristics of the conflict group itself during wartime and the type of individuals who choose to join it. We first control for the reported reasons for initially joining an armed group, whether those reasons were material, due to grievances or insecurity, or attributable to ideological convictions. Following Weinstein (2007), we control for material versus non-material reasons for recruitment on the logic that those who were more materially motivated might be less likely to retain their allegiance to their former comrades after demobilizing. We also control for whether an individual initially joined an armed group for network or status reasons to ensure that our results are not driven by ex-combatants who are social 'types'.

We also include several controls that capture characteristics of the armed group itself. We

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[8]Also, as noted in the main text, we reduce the dimensionality of controls by creating 25 indices using inverse covariance weighting, as in Anderson (2008).

expect that armed groups characterized by stronger ideology and commitment to long-term organizational goals might create more enduring networks insofar as they devote extended time to "shaping identities, mobilizing networks and building ideologies" (Weinstein, 2007, 52). Accordingly, we include measures of the frequency with which the fighting group indoctrinated and socialized its members (Wood, 2008). Additionally, we include a number of controls that capture whether a unit was cohesive, disciplined, and hierarchical and the extent to which combatants were exposed to conflict: an important proxy for the intensity of fighting (and social bonding) conditions.

The manner in which a combatant demobilized could also be a significant determinant of the strength of wartime ties in the reintegration period. Ex-combatants who demobilized individually, as was the case for the vast majority of guerrillas until the 2016-2017 peace process, made a voluntary decision to put down their arms. They also did so at great risk as armed groups like the FARC often punished defection with death. In contrast, those who demobilized collectively never decided to disarm; rather, they demobilized because they were ordered to. All but 3,000 paramilitaries surrendered in this fashion. We anticipate that collective demobilization is more conducive to the maintenance of strong wartime networks than individual demobilization, an implication of our argument that we examine empirically.

We also expect that the reintegration environment contributes to the maintenance of wartime networks following demobilization. Specifically, we expect that ex-combatants who enjoy greater community acceptance and family support and those that are more integrated into state institutions will have civilian ties that can dilute the strength of wartime attachments (de Vries and Wiegink, 2011; Annan and Cutter, 2009; Kaplan and Nussio, 2018). Indeed, studies in criminology suggest that such 'positive' social forces act as counterweights to the pull of group or gang-related crime (Warr, 2002; Hirschi, 1969). We therefore include control indices that capture the extent of family support, community acceptance, and integration into the state. We also control for the geography of post-war migration: whether the ex-combatants remained in the zone in which they operated militarily, a strong predictor of the durability of wartime networks (Daly, 2016). In addition to these controls, we include measures for characteristics like age, gender, marital status, the age at which an individual first entered an armed group (which proxies for the strength of their pre-war social ties), and duration in an armed group, all of which could influence the extent of the ex-combatants socialization into civilian networks versus conflict ones.

Many of these controls also help address confounding due to the omission of factors that lead to both negative economic welfare and criminality after demobilization. Including material motivations for joining helps control for ex-combatants pre-war sensitivity to opportunity costs. Family support, community acceptance, and integration into the state likely influence ex-combatants access to productive resources and to the legal labor market. Remaining

where one fought is likely to provide better economic opportunities than displacing to a new place. Younger age of entry into an armed group, greater time in arms, and higher intensity conflict exposure should cause greater disruption to ex-combatants accumulation of material or human capital and prevent them from establishing themselves in the licit economic sectors (Tajima, 2009). We also control for whether the individuals are disabled (and restricted in their economic possibilities) and the amount of their wartime incomes (to account for their material capital at the time of demobilizing). We also control for exposure to Colombia's reintegration program, which focused heavily on providing economic and material benefits to former combatants.

Additionally, to improve precision, we include a number of controls that could be directly correlated with criminality. We control for the length of time since demobilization to account for the fact that some respondents are being asked to recall more recent history. We also include covariates that capture personality type since it could be the case that factors like risk preferences, time discounting, and initially joining an armed group for adventure are all associated with criminality (Theidon, 2009). Furthermore, we control for psychological well-being following demobilization since general strain theory posits that individuals will engage in crime when they harbor powerful emotions such as anger, sadness, depression, or post-traumatic stress disorder (PTSD) (Agnew, 1992; Elbogen et al., 2012; ?; Agnew, 2005; Froggio, 2007). We control for demographic factors like age and having children, which are both associated with lower risks of criminal behavior (Nussio, 2018). Finally, we use five measures from the survey to control for the possibility that personal insecurity following demobilization is associated with crime. This would be the case if insecurity induces former combatants to illegally carry arms for self-protection or to commit crimes in self-defense if facing threats (Bøås and Hatløy, 2008; Nussio, 2011).

H.1   Regression results

To provide a more complete picture of our data and the correlates of criminality, Table H.1 presents estimates from regressions of our binary measure of 'proven' criminality on our full set of control variables. We group the control variables into five 'families' and report the unadjusted p-values as well as FDR adjusted q-values for each family of variables as they are added into the regression to account for multiple hypothesis testing (Anderson, 2008).

This analysis reveals a small handful of control variables with significant explanatory power after the multiple hypothesis correction. Of our demographic factors, women are significantly less likely to engage in crime than men following demobilization (Panel A). There is a strong association between conflict exposure and criminality (Panel C), which is notable because of its link to several of our hypotheses. For instance, ex-combatants with more conflict exposure could also be more likely to have skills or abilities that are

valuable to the labor market for illicit activities or to have the most durable conflict networks. Controlling for conflict exposure thus allows us to account for an important source of spurious association. Other conflict factors, like rank, unit hierarchy, and fighting group interestingly have no significant relationship with criminality when using the corrected p-values.

Of those variables associated with demobilization and reintegration experience (Panels D and E), exposure to the reintegration program is negatively associated with the propensity to engage in criminal behavior. This could reflect the fact that ex-combatants who engage in crime are more likely to withdraw from the reintegration program. Alternatively, it could be that the reintegration program succeeded in mitigating criminal propensities, for instance through its provision of economic assistance.

Other notable results include a strong positive association between personal insecurity and criminality. This could reflect the fact that insecurity induces former combatants to illegally carry arms for self-protection, to commit crimes in self-defense if facing threats of retaliation, or to join criminal gangs for the social protection that they afford. Interestingly, we find no significant association between criminality and the strength of community and family ties. This suggests that these other social ties are not serving as a counter-veiling force that helps to keep ex-combatants out of crime following demobilization.

Table H.1: Correlates of criminality: all control variables

	1		2		3		4		5		P-value	Q-value
Panel A: General controls											For column 1	
Female	-0.15***	(0.03)	-0.14***	(0.04)	-0.12**	(0.04)	-0.11**	(0.04)	-0.11**	(0.04)	0.000	0.001
Non-white/mestizo	-0.05	(0.04)	-0.06	(0.04)	-0.07	(0.04)	-0.07	(0.04)	-0.07	(0.04)	0.258	0.348
Educ level prior to joining	0.03*	(0.01)	0.03*	(0.01)	0.03*	(0.01)	0.03*	(0.01)	0.03*	(0.01)	0.028	0.049
Age	-0.01**	(0.00)	-0.01**	(0.00)	-0.01**	(0.00)	-0.01**	(0.00)	-0.01**	(0.00)	0.008	0.025
Age joined armed group	0.00	(0.00)	0.00	(0.00)	0.01	(0.00)	0.01	(0.00)	0.01	(0.00)	0.557	0.804
Risk preferences	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.817	0.824
Time discounting	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.846	0.824
Panel B: Motivations for initially joining											For column2	
Family reasons (index)			-0.01	(0.02)	-0.01	(0.02)	-0.01	(0.01)	-0.01	(0.01)	0.391	1.000
Grievances (index)			0.01	(0.02)	0.01	(0.01)	0.01	(0.01)	0.02	(0.01)	0.415	1.000
Ideological motivations (index)			0.02	(0.02)	0.01	(0.01)	0.01	(0.01)	0.01	(0.01)	0.242	1.000
Economic welfare obj. (index)			0.04*	(0.02)	0.03	(0.02)	0.03	(0.02)	0.04*	(0.02)	0.033	0.423
Economic welfare subj. (index)			-0.02	(0.02)	-0.02	(0.02)	-0.02	(0.02)	-0.03	(0.02)	0.236	1.000
Social reasons (index)			0.00	(0.01)	0.00	(0.01)	0.00	(0.01)	0.01	(0.01)	0.732	1.000
Status/power motivations (index)			0.01	(0.02)	0.00	(0.02)	0.00	(0.02)	0.00	(0.02)	0.642	1.000
Security motivations (index)			0.01	(0.01)	-0.01	(0.01)	-0.01	(0.01)	-0.02	(0.01)	0.674	1.000
Coerced (index)			0.02	(0.02)	0.01	(0.02)	0.02	(0.02)	0.01	(0.02)	0.399	1.000
Panel C: Conflict experience											For column 3	
Conflict exposure (index)					0.06**	(0.02)	0.06**	(0.02)	0.05*	(0.02)	0.003	0.035
Mid-level commander (index)					0.03	(0.02)	0.03	(0.02)	0.03	(0.02)	0.061	0.256
High-level Commander (index)					0.03*	(0.02)	0.03*	(0.01)	0.03*	(0.01)	0.040	0.250
Combatant					-0.07	(0.04)	-0.06	(0.04)	-0.05	(0.04)	0.107	0.273
Unit cohesion (index)					-0.02	(0.02)	-0.02	(0.02)	-0.01	(0.02)	0.352	0.433
Unit discipline (index)					0.01	(0.02)	0.02	(0.02)	0.00	(0.02)	0.412	0.448
Unit hierarchy (index)					0.02	(0.02)	0.03	(0.02)	0.03	(0.02)	0.242	0.320
Total time in groups					0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.550	0.502
Paramilitary (vs. guerilla)					0.09	(0.05)	0.01	(0.08)	0.00	(0.07)	0.082	0.258
Group freq. indoctrinated					0.00	(0.02)	0.00	(0.02)	0.00	(0.02)	0.873	0.798
Income while in group (000'000)					0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.557	0.502
Panel D: Demobilization experience											For column 4	
Year of demob.							0.00	(0.01)	-0.01	(0.01)	0.802	1.000
Individual Demob							0.10	(0.07)	0.06	(0.07)	0.194	0.478
Demob coerced							-0.03	(0.04)	-0.02	(0.04)	0.460	0.828
Discontent w/ demob.							0.00	(0.03)	-0.04	(0.03)	0.933	1.000
Minor of age							-0.05	(0.07)	-0.09	(0.07)	0.469	0.828
Disabled							-0.07*	(0.03)	-0.09**	(0.03)	0.024	0.197
Settled where operated							0.08*	(0.04)	0.08	(0.04)	0.047	0.197
Panel E: Reintegration controls											For column 5	
Particip. in reint prog. (index)									-0.07***	(0.02)	0.000	0.001
Registered with the state (index)									0.02	(0.01)	0.085	0.247
Political voice (index)									0.01	(0.02)	0.507	0.568
Shame about group (index)									0.01	(0.02)	0.709	0.650
Confident gov. will not renege (index)									0.00	(0.02)	0.913	0.710
Perceptions of gov. capacity (index)									-0.03	(0.02)	0.110	0.247

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Insecurity (index)						0.06***	(0.02)	0.001	0.006
Family support (index)						0.02	(0.02)	0.146	0.281
Community support (index)						0.01	(0.02)	0.611	0.618
Depression/PTSD (index)						0.03	(0.02)	0.090	0.247
Anger						-0.02	(0.08)	0.819	0.694
Constant	0.47***	(0.08)	0.47***	(0.08)	0.47***	(0.10)	4.64	(16.53)	15.74	(16.54)
N	1158		1158		1158		1158		1158

* p < .05, ** p < .01, *** p < .001.
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for municipality clustering.
Column 7 reports unadjusted two-sided p-values. Column 8 reports FDR q-values.

I Estimation and inference

We estimate the following regression specification for individual i in municipality j:

$$Y_{ij} = E'_{ij}\delta + T'_{ij}\beta + X'_{ij}\gamma + \mu_j + \epsilon_{ij} \tag{14}$$

where Y_{ij} is one of our crime measures, E'_{ij} is the vector of economic variables, with corresponding coefficients δ, and T'_{ij} is the vector of social ties indices, with corresponding coefficients β.[9] X'_{ij} is the vector of other controls described above. Finally, μ_j denotes the municipality fixed effects and ϵ_{ij} is individual level random error. We fit the model using weighted least squares where we use the known survey weights to account for variation in the probabilities of selection into the sample due to stratification. We use least squares because of its robustness for fixed effects regressions (Beck, 2015). To address a small amount of item-level missingness that nonetheless would have resulted in dropping a substantial number of observations, we perform ten rounds of predictive-mean-matching imputation for missing data. Our standard errors are consistent for sampling variability given our sampling design and account for the fact that our sample was stratified by municipality and clustered by neighborhood groupings within each municipality. We test our hypotheses on the effects of individual economic conditions (as measured by δ) and then the joint effect of vertical ties and horizontal ties (as measured by β) using joint F-tests.

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[9]We also check the robustness of all results to excluding the economic variables in case they introduce post-treatment bias—the results are unchanged.

J Robustness Checks

J.1 Alternative DV codings

We check the robustness of our main results presented to two alternative codings and classifications of the dependent variable. First, to guard against endogeneity, we measured all independent variables in the survey *one year following demobilization* (see Section 4). There were, however, 44 respondents who reported having committed crimes within the first year of demobilization. We therefore check the robustness of results to dropping these 44 ex-combatants from the analysis. As can be seen in Tables J.1 and J.2, all results are similar to what is reported in the main text Tables II and VI, respectively. The only result that loses statistical significance at the 95 percent confidence letter is the coefficient on *Horiz. ties* in column two (it is still significant at the 90 percent level). The coefficient is almost identical to the main tables, suggesting the difference just reflects a loss in power.

Table J.1: Wartime Ties and Crime (excluding those who committed crimes within one year)

	'Proven' criminal		Violent crime	
	(1)	(2)	(3)	(4)
Panel A: Main Results				
Vert. ties	0.06**	0.05*	0.11**	0.08**
	(0.02)	(0.02)	(0.03)	(0.03)
Horiz. ties	0.05*	0.05	0.13***	0.13**
	(0.02)	(0.02)	(0.04)	(0.04)
Panel B: Interaction Regressions				
Vert. ties	0.06**	0.05*	0.09**	0.07*
	(0.02)	(0.02)	(0.03)	(0.03)
Horiz. ties	0.05*	0.05	0.13***	0.12**
	(0.02)	(0.03)	(0.04)	(0.04)
Horiz. × vert. ties	0.00	0.01	0.05*	0.04*
	(0.01)	(0.01)	(0.02)	(0.02)
Social F-test (pvalue)	0.00	0.00	0.00	0.00
Observations	1114	1114	1114	1114
Clusters	532	532	532	532
Econ controls	Yes	Yes	Yes	Yes
Other controls	No	Yes	No	Yes

Standard errors in parentheses
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for clustering by survey sampling blocks.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table J.2: Interaction of Economic and Social Factors (excluding those who committed crimes within one year)

	'Proven' criminal				Violent crime			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed	-0.01	-0.00	-0.00	-0.00	-0.06	-0.05	-0.05	-0.05
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Econ welfare obj. (index)	0.02	0.02	0.02	0.02	0.04	0.04	0.04	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Econ welfare subj. (index)	-0.00	-0.00	-0.00	-0.00	0.02	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Education	0.01	0.01	0.01	0.01	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Vert. ties	0.05**	0.05**	0.05**	0.05**	0.08***	0.08***	0.08***	0.08***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Horiz. ties	0.09**	0.05*	0.05*	0.04	0.18***	0.13***	0.13***	0.08
	(0.04)	(0.03)	(0.02)	(0.05)	(0.07)	(0.04)	(0.04)	(0.08)
Emp X Horiz. ties	-0.06				-0.07			
	(0.05)				(0.08)			
Econ obj. X Horiz. ties		-0.00				0.02		
		(0.02)				(0.04)		
Econ subj X Horiz. ties			0.01				0.02	
			(0.02)				(0.03)	
Educ X Horiz. ties				0.00				0.02
				(0.01)				(0.03)
Observations	1114	1114	1114	1114	1114	1114	1114	1114
Econ_ties_F_test_p	0.62	0.92	0.89	0.92	0.57	0.60	0.61	0.56
Clusters	532	532	532	532	532	532	532	532
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for clustering by survey sampling blocks.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table J.3 checks the robustness of the main results to an alternative way of coding 'proven' criminality in the survey. As described in Section 4 we code an ex-combatant as criminal according to the survey data if they self-report criminal behavior as an individual or with a gang, or if they report the types of crimes that they have been involved in. As can be seen in Table I in the main paper, nearly 20 percent of our ex-combatant population report the type of crimes that they have committed. This high level might raise concerns that ex-combatants interpreted this question to mean crimes that they had committed *before* demobilizing, despite repeated enumerator prompts to recall criminal activity committed *since* demobilization. We therefore test the robustness of our results to excluding the *type of crimes committed* measure from our coding of 'proven' ex-combatant criminality. If we exclude this component then about 17 percent (rather than 24 percent) of our ex-combatant

population has engaged in criminal activity following demobilization when we combine the administrative and survey data as described in the main paper. As can be seen in Table J.3, however, when using this revised version of 'proven' criminality, the main results are just as strong as in the main text when focusing on the full sample (columns 1-2) as well as the sample that excludes those committed crimes within the first one year (columns 3-4). The interaction between horizontal and vertical ties is no longer robust, however, suggesting little support for the prediction on theorized interactive effects for those readers who prefer this outcome measure.

Table J.3: Main results (excludes total crimes from coding of DV)

	Full sample		Excluding those who committed crimes within one year of demob.	
	(1) Criminal (alt)	(2) Criminal (alt)	(3) Criminal (alt)	(4) Criminal (alt)
Panel A: Main Results				
Vert. ties	0.06***	0.05*	0.06**	0.05*
	(0.02)	(0.02)	(0.02)	(0.02)
Horiz. ties	0.06***	0.05**	0.04**	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Interaction Regressions				
Vert. ties	0.06**	0.04*	0.06**	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
Horiz. ties	0.06***	0.04**	0.04**	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
horizXvert_ties	0.01	0.02	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Social Ftest (p-value)	0.00	0.00	0.00	0.00
Observations	1158	1158	1114	1114
Clusters	570	570	532	532
Covariates	No	Yes	No	Yes
Excludes oneyear	No	No	Yes	Yes

Standard errors in parentheses
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for clustering by survey sampling blocks.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

J.2 Controlling for *bloque* and coca

It could be that individuals that belonged to units that were more criminal during war were more likely to keep their networks and to engage in criminality after demobilizing. We show that our results hold with municipal fixed effects and with controls for whether the recruits were more criminal or ideological in their reasons for joining and for whether they reported belonging to a unit that was more criminal or more political in nature. To further isolate variation across individuals within units that varied in their levels of engagement in criminality during the conflict, we check the robustness of our results to controlling for fighting unit fixed effects.

Some armed groups were highly criminal before demobilizing whereas others were deemed more political or ideological. For example, Vencedores de Arauca was a paramilitary franchise bought by two notorious drug trafficking twin brothers, "los Mellizos." In contrast, Autodefesas Campesinas de Ortega maintained a more counter-insurgent self-defense logic. The same variation can be found among the FARC *bloques*. If more criminal units during the more are both more likely to become criminal after and to bring their fighters with them, then what is doing the explanatory work are the conditions favoring wartime criminality. We therefore seek to see if the results hold controlling for the bloques that the individual combatants belonged to.

To address this, we use questions from the survey that asked respondents to list the *frentes* and *bloques* to which they belonged during the conflict. In addition to incorporating *bloque* fixed effects, we control for access to criminal opportunities during the conflict by including an indicator for whether the respondent operated in a municipality with the presence of drug cultivation. For this, we use coca data derived from the Centro de Datos of El Centro de Estudios sobre Desarrollo Económico (CEDE) of Universidad de los Andes. The source of this data is the Sistema Integrado de Monitoreo de Cultivos Ilícitos (SIMCI). We use a binary variable, which assumes a value of "1" if the municipality had coca cultivation between 2000-2012; "0" otherwise.

We present the results of this analysis in Table J.4, in which we replace municipality fixed effects with *bloque* fixed effects and add the coca cultivation control to our vector of controls for the analysis in columns two and four. The results show that vertical and horizontal wartime ties remain strong predictors of ex-combatant demobilization following criminality even when estimated within fighting units that were more or less engaged in crime during the conflict.

Table J.4: Main results on criminality, social ties, and economic conditions

	(1) Criminal	(2) Criminal	(3) Violent	(4) Violent
Employed	0.03	0.05	-0.02	-0.03
	(0.04)	(0.04)	(0.06)	(0.06)
Econ welfare obj. (index)	0.03	0.04*	0.05	0.04
	(0.02)	(0.02)	(0.03)	(0.03)
Econ welfare subj. (index)	0.04*	0.01	0.09**	0.04
	(0.02)	(0.02)	(0.03)	(0.03)
Education	0.02	0.01	-0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.03)
Vert. ties	0.07***	0.06***	0.09**	0.07*
	(0.02)	(0.02)	(0.03)	(0.03)
Horiz. ties	0.07***	0.05*	0.14***	0.12***
	(0.02)	(0.02)	(0.03)	(0.03)
Observations	1150	1113	1150	1113
Econ_F_test_p	0.02	0.24	0.01	0.34
Social_ties_F_test_p	0.00	0.00	0.00	0.00
Clusters	568	541	568	541
Covariates	No	Yes	No	Yes

Standard errors in parentheses
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for clustering by survey sampling blocks.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

K Sensitivity Analysis (Omitted Variables)

We analyze sensitivity of the wartime ties and economic opportunity cost results to hidden confounding. The analysis is based on the ideas of Imbens (2003) and uses the simulation routine developed by Beber, Roessler and Scacco (2014), which extends Imbens's framework to continuous predictors. Conceptually, the analysis proceeds as follows. First, set an element of E_{ij} or T_{ij} from our regression equation (14) as the predictor of interest. Then, suppose the existence of an unmeasured confounder, U_{ij}, that is independent of all the control variables (X_{ij}) and all other elements of (E_{ij}, T_{ij}), but that is correlated with both the predictor of interest and the outcome. Then, suppose we could estimate the following extended regression,

$$Y_{ij} = E'_{ij}\tilde{\delta} + T'_{ij}\tilde{\beta} + X'_{ij}\tilde{\gamma} + \tilde{\mu}_j + \phi U_{ij} + \tilde{\epsilon}_{ij}. \tag{15}$$

We focus on the estimate for the element of $(\tilde{\delta}, \tilde{\beta})$ that corresponds to our predictor of interest. What we want to know is, *how strongly must U_{ij} be correlated with the outcome and the predictor of interest until our inference for the predictor of interest changes?* The coefficient estimates for the economic variables in Table VI are essentially zero. For these, we would want to know how strongly U_{ij} must correlate with the outcome and each of the economic variables until we get a negative and significant estimate. The coefficients for the social ties variables in Table II are positive and significant. For these, we would want to know how strongly U_{it} must correlate with the outcome and each of the social ties variables until we get an statistically insignificant estimate. In both cases, the concern is the possibility of positive bias in our estimates. For positive bias, U_{ij} would have to exhibit either positive correlation with both the outcome and predictors or interest or negative correlation with both (Angrist and Pischke, 2009, pp. 59-64). Following Beber, Roessler and Scacco (2014), we simulate U_{ij} values that are correlated with the outcome and predictor of interest in various ways, but are independent of the other variables. Then, we fit the extended regression and record whether the coefficient on the predictor of interest is statistically significant or not.

Figure L.1 shows the results for an analysis that focuses on our primary outcome variable, proven criminality. The title of each plot states the predictor of interest. Each point in the graph corresponds to a simulated U_{ij} variable, with the correlation with the predictor of interest indicated by the x-axis and the correlation with the outcome indicated by the y-axis. The large points correspond to correlation values that give rise to statistically significant estimates at the 95% confidence level. The small points correspond to correlation pairs that give rise to statistically insignificant estimates. Also plotted are correlation pairs for select control variables. The controls that were selected were the ones that served as significant predictors of the outcome. They are plotted as points of comparison.

The first four plots, from top to bottom, are for the economic variables. The correlation values that would overturn our results in favor of a negative effect are the ones indicated by the large black dots in the top right and lower left quadrants. We see that the large black dots reside at correlation values that are much more extreme than the selected control variables. This indicates that our findings are robust to even quite extreme degrees of hidden confounding.

The last two plots, at the bottom, are for the social ties variables. For vertical ties to commanders, the correlation values that would change our inference reside well beyond those of the selected control variables. For horizontal ties to peers, a hidden confounder that exhibited a correlation pattern as strong as for conflict exposure would render our findings insignificant. This suggests that the peer finding is less robust than the other findings, although the degree of confounding needed to overturn it is still very high relative to what the selected control variables indicate.
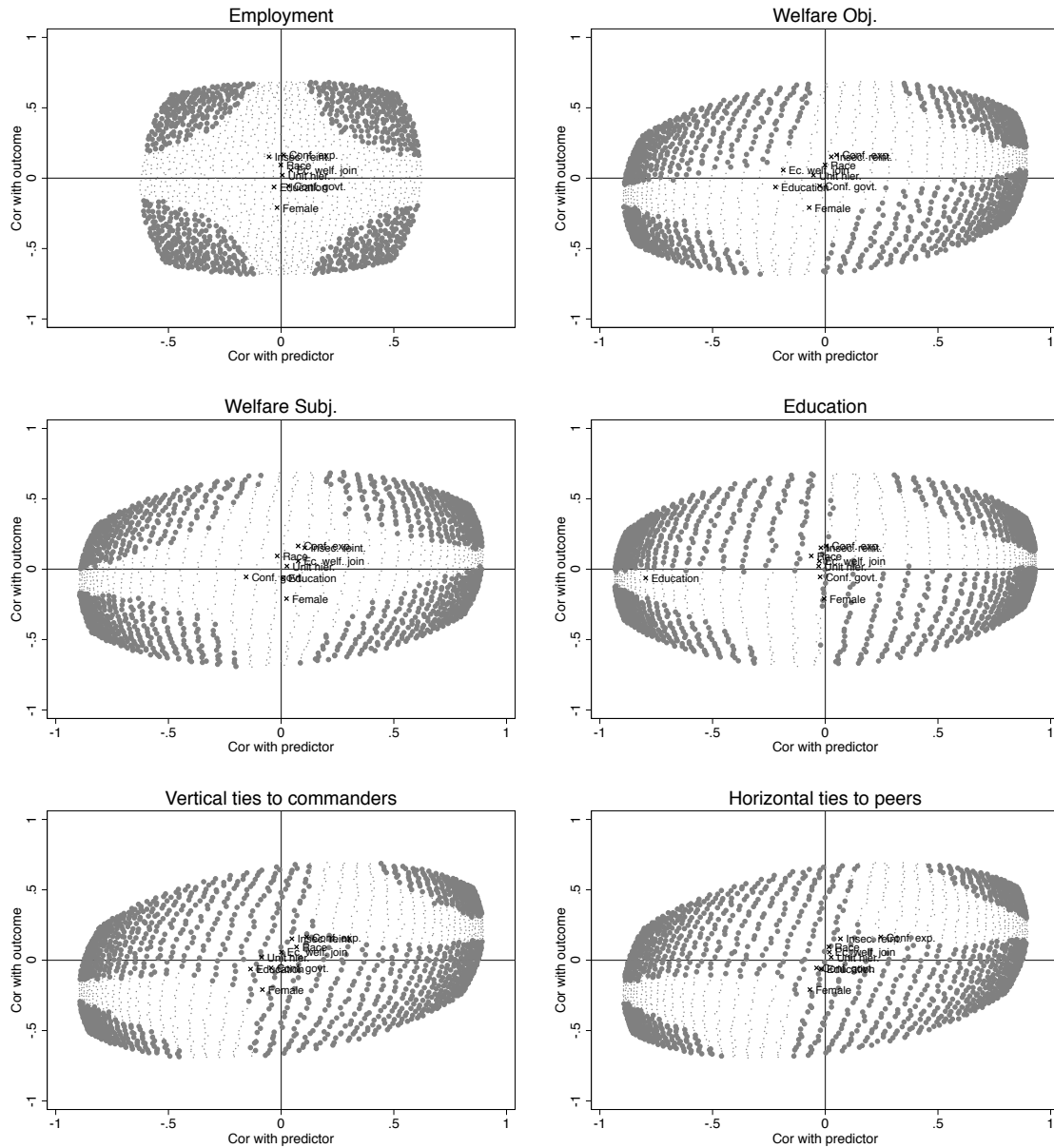
Figure K.1: Sensitivity analysis plots. *For each plot, the x-axis indicates correlation between a hidden confounder and the predictor of interest (as given by the plot title). The y-axis indicates correlation between a hidden confounder and the crime outcome variable. Each point in the plot shows the result of re-estimating the regression model given a hidden confounder with the corresponding correlations to the predictor and outcome. A dot is large if the resulting effect on the predictor is statistically significant at the 95% confidence level and small if not significant. Also plotted for comparison are correlations corresponding to significant control variables.*

L Sensitivity Analysis (Sampling Bias)

We carry out a formal sensitivity analysis to evaluate the potential for bias in our study due to the fact that a segment of the ex-combatant population remained non-locatable by the national reintegration office (the ACR) and thus "hidden" from our study. Administrative data from the ACR suggests that the size of the hidden population is about 9,922 ex-combatants, or about 19 percent of the population of 51,832 ex-combatants who were alive at the time of our study.[10]

The formal sensitivity analysis is based on a decomposition of a population-level regression coefficient into subcomponents from the observed and hidden sub-populations. The analysis follows [[reference withheld to preserve anonymity]]. Define β_X as the population-level expected value of the regression coefficient on the variable X for a regression of Y on X along with other controls. Let $\tilde{Y}$ and $\tilde{X}$ refer to the residualized outcome and regressor of interest that one uses to express a coefficient from a multiple regression in terms of a bivariate residual-residual regression, as per the Frisch-Waugh-Lovell theorem. Let the indicator variable $R \in \{0, 1\}$ indicate that a unit in the population is available to be sampled ($R = 1$) or not ($R = 0$). Let $\pi = \Pr[R = 1]$. Then,

$$
\begin{aligned}
\beta_X &= \frac{\mathrm{E}\left[\tilde{Y}\tilde{X}\right]}{\mathrm{E}\left[\tilde{X}^2\right]} \\
&= \frac{\mathrm{E}\left[\tilde{Y}\tilde{X}|R=1\right]\pi + \mathrm{E}\left[\tilde{Y}\tilde{X}|R=0\right](1-\pi)}{\mathrm{E}\left[\tilde{X}^2|R=1\right]\pi + \mathrm{E}\left[\tilde{X}^2|R=0\right](1-\pi)} \\
&= \beta_{X(1)}\left(\frac{\pi}{\pi + \eta(1-\pi)}\right) + \beta_{X(0)}\left(\frac{1-\pi}{\frac{1}{\eta}\pi + (1-\pi)}\right).
\end{aligned}
\tag{16}
$$

where

$$
\eta = \frac{\mathrm{Var}\left(\tilde{X}|R=0\right)}{\mathrm{Var}\left(\tilde{X}|R=1\right)},
$$

where $\beta_{X(1)}$ is the expected value of regression coefficient for the subpopulation that we are able to sample, and $\beta_{X(0)}$ is the expected value of regression coefficient for the hidden subpopulation. The expression (16) shows that with an estimate $\hat{\beta}_{X(1)}$, along with stipulated π and η values, one can assess for what values $\beta_{X(0)}$ we would have that $\beta_{X(1)}$ and β_X differ in sign. Moreover, given the linearity in the relationship between $\beta_{X(1)}$ and β_X, one can easily establish the value of $\beta_{X(0)}$ that is minimally different from $\beta_{X(1)}$ but would nonetheless lead our results to be overturned were it that we had been able to include the hidden population

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[10]Calculations available from the authors upon request. The overall population of 55,511 includes 3,679 who were deceased at the time of our study. Counting these deceased ex-combatants as part of the hidden population does not substantially change the results here.

in our analysis.

Figure L.1 shows the results for estimates from the first column of our main results in Table II and VI. Specifically, we present results for the effects of horizontal and vertical ties (the analysis is the same, since the coefficients and standard errors were the same) and for the objective economic welfare index. The x-axis in each graph defines a possible value for $\beta_{X(0)}$. The horizontal dashed line shows the estimated $\hat{\beta}_{X(1)}$, for reference. The $y-axis$ shows what the resulting population β_X would be given $\beta_{X(0)}$ as well as stipulated π and η values. We set $\pi = 1 - 0.19 = 0.81$, as per the administrative data. Then, the solid diagonal lines show implied β_X at different potential values of η (that is, for $\eta = 1, 0.5, 0.25$). The assumption that $\eta = 1$ is a very conservative assumption — essentially saying that the degree of heterogeneity in X for the minority hidden subpopulation is as high as is the case for the rest of the population. This is unlikely to be true, but it establishes a standard against which bias might be most severe. (We think it is simply implausible for $\eta > 1$.) The horizontal dashed line shows the critical value, given $\eta = 1$, at which our results would be overturned in terms of finding a statistically significant effect in the opposite direction. What the graphs indicate is that for either of the results to be overturned, the effect in the hidden population would have to be the opposite and about an *order of magnitude* larger in terms of the strength. This is driven by the fact that the missing population constitutes a minority share of the population, and so the averaging-in of such units can only overturn the results from our sample if they differ very dramatically.

Figure L.1: Sensitivity analysis for hidden population

**Sensitivity analysis for
horizontal and vertical ties**

**Sensitivity analysis for economic
welfare obj. (index)**
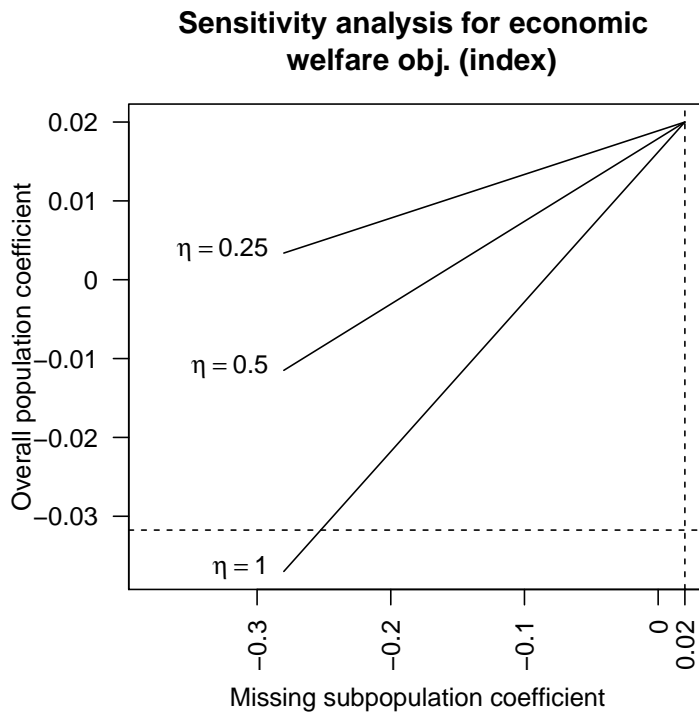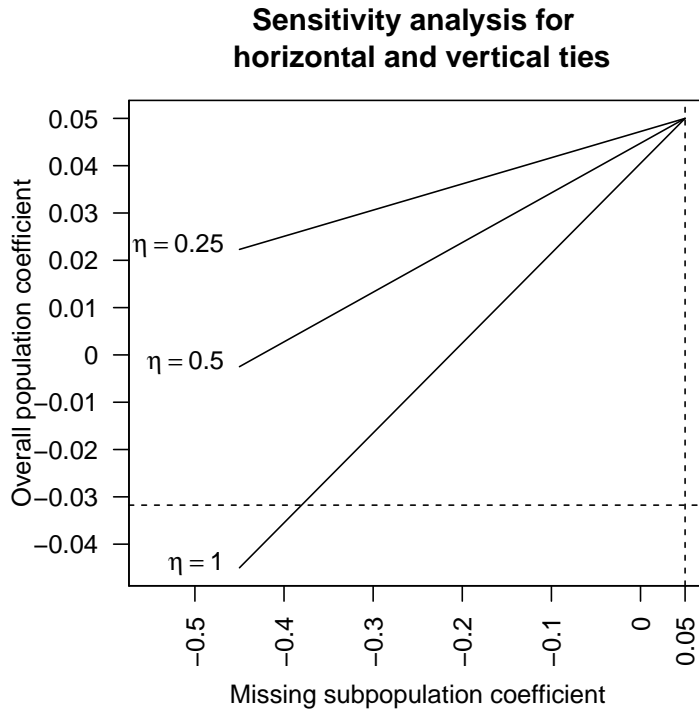
M Additional Analysis

M.1   Heterogeneous Effects by Fighting Group

We check to see whether the effects of wartime ties and economic opportunity costs vary by fighting group, as discussed in Section 5. As described in the main text, the results presented in Table M.1 show that higher levels of economic welfare are not associated with less criminal activity one year following demobilization, further indicating that economic opportunity costs are an important part of the crime story in the Colombia case. The results, however, provide further support for the social logic of crime. Ex-combatants with stronger vertical 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. This could reflect the fact that paramilitary networks are characterized either by stronger criminal capacities or pro-crime social norms. At the same time, the results underscore that horizontal ties do not always lead to more crime, as evidenced by the results for former guerrillas. 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.

Table M.1: Heterogeneous Effects by Fighting Group

	'Proven' Criminality (binary measure)	
	(1)	(2)
Employed	0.00	0.02
	(0.05)	(0.05)
Econ welfare obj. (index)	0.03	0.03
	(0.02)	(0.03)
Econ welfare subj. (index)	0.01	-0.01
	(0.02)	(0.02)
Education	0.02	0.01
	(0.02)	(0.02)
Vert. ties	0.09*	0.07*
	(0.04)	(0.04)
Horiz. ties	-0.00	-0.01
	(0.02)	(0.03)
Emp X paramil.	-0.00	-0.04
	(0.07)	(0.07)
Econ. obj. X paramil.	-0.01	-0.01
	(0.03)	(0.03)
Econ. subj. X paramil.	0.04	0.03
	(0.03)	(0.03)
Educ. X paramil.	0.00	0.01
	(0.02)	(0.02)
Vert. ties X paramil.	-0.04	-0.03
	(0.04)	(0.04)
Horiz. ties X paramil.	0.10**	0.09**
	(0.03)	(0.03)
Paramilitary (vs. guerilla)	0.07	0.05
	(0.08)	(0.11)
Social F-test (pval)	0.00	0.00
Econ F-test (pval)	0.17	0.71
Observations	1158	1158
Clusters	570	570
Covariates	No	Yes

Standard errors in parentheses
Weighted least squares with municipality FE and indiv. controls.
Standard errors account for clustering by survey sampling blocks.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

M.2 Drivers of Commander Criminality

The discussion in Section 2 suggests that mid-level commanders can play an important role in whether a wartime network reintegrates or engages in crime. This of course raises the question of why some commanders themselves decide to engage in crime following demobilization. Given that we only have about 210 commanders in our sample, we have limited power in our ability to explain what motivates some to engage in crime. Nevertheless, we analyze this to the best of our abilities here. In this analysis, we code as a commander anyone who reported command as one of their principal activities while with their fighting unit or who reported that their highest rank was *comandante de bloque o frente* (high-ranking commander) or *mando medio* (mid-ranking commander). We note that 88 percent of the commanders in our sample are *mandos medios* and only four percent were higher-ranking (the others were lower ranking).

Table M.2 presents the results from a regression of our proven crime indicator on the full suite of controls described in Section 4 using a LASSO-based variable selection model. The results show that paramilitary commanders are much more likely to engage in crime. There is some indication that those engaged in crime have lower trust in the state and lower sense of security. Maintaining vertical ties also predicts criminality among commanders, indicative of the importance of lines of command. Finally, it is those commanders who were relatively well off economically at the time of their demobilization that are more likely to engage in crime. This possibly reflects the fact that former commanders who are economically well-off following demobilization turn to crime to maintain a higher quality of life than can be sustained in peace-time. Another possibility is that wealth facilitates criminality for commanders if it helps them recruit foot-soldiers into criminal gangs.

Table M.2: Correlates of criminality for commanders

	Demographics			Joining			Conflict			Demob. experience			Reint. experience			w/ IVs		
	b	se	p	b	se	p	b	se	p	b	se	p	b	se	p	b	se	p
Female	-0.27	(.17)	0.116	-0.26	(.17)	0.133	-0.22	(.18)	0.227	-0.23	(.18)	0.196	-0.27*	(.16)	0.084	-0.34**	(.15)	0.033
Non-white/mestizo	-0.01	(.16)	0.947	-0.01	(.15)	0.940	-0.07	(.13)	0.600	-0.06	(.13)	0.609	-0.13	(.11)	0.263	-0.04	(.11)	0.696
Educ level prior to joining	0.07*	(.04)	0.054	0.06*	(.03)	0.074	0.03	(.03)	0.377	0.03	(.03)	0.353	0.04	(.03)	0.186	0.04	(.03)	0.223
Age	-0.01	(.01)	0.144	-0.01*	(.01)	0.094	-0.01*	(.01)	0.087	-0.01*	(.01)	0.091	-0.01	(.01)	0.302	0.00	(.01)	0.861
Family reasons (index)				-0.05	(.06)	0.408	-0.06	(.05)	0.202	-0.06	(.05)	0.212	-0.10**	(.05)	0.048	-0.11***	(.04)	0.009
Economic welfare subj. (index)				0.01	(.04)	0.824	0.00	(.04)	0.972	0.00	(.04)	0.975	0.00	(.04)	0.985	0.00	(.04)	0.890
Total time in groups							0.00	(.01)	0.801	0.00	(.01)	0.789	0.00	(.01)	0.848	-0.01	(.01)	0.304
Paramilitary (vs. guerilla)							0.35***	(.13)	0.008	0.33**	(.14)	0.020	0.24*	(.14)	0.075	0.14	(.13)	0.288
Year of demob.										-0.01	(.02)	0.632	-0.02	(.02)	0.409	-0.01	(.02)	0.594
Particip. in reint prog. (index)													-0.01	(.05)	0.823	-0.01	(.04)	0.766
Confident gov. (index)													0.00	(.05)	0.997	-0.01	(.05)	0.868
Gov. capacity (index)													-0.07	(.05)	0.133	-0.05	(.05)	0.250
Insecurity (index)													0.08*	(.04)	0.091	0.06	(.04)	0.156
Family support (index)													0.06	(.04)	0.127	0.05	(.04)	0.179
Depression/PTSD (index)													0.05	(.04)	0.220	0.07*	(.04)	0.066
Econ welfare obj. (index)																0.10**	(.05)	0.043
Vert. ties																0.10**	(.05)	0.039
Horiz. ties																-0.03	(.04)	0.540
Constant	0.58*	(.27)	0.031	0.61*	(.25)	0.015	0.58**	(.22)	0.009	21.40	(43.4)	0.623	34.12	(40.61)	0.402	23.36	(43.12)	0.589
N		210			210			210			210			210			210	

* p < .10, ** p < .05, *** p < .01. Weighted least squares with municipality FE and indiv. controls. Standard errors account for municipality clustering.
Independent variables are determined by a LASSO -based variable selection model. Unadjusted two-sided p-values are reported.

N Why the null effect for economic opportunity costs?

The analysis presented thus far suggests that the null effect on economic factors is not due to heterogeneity by fighting group or to omitted variable bias. We address five additional explanations for the null results, considering whether they could be due to attenuation bias, additional sources of heterogeneity, the effectiveness of the reintegration program, and our measurement of crime.

One possible explanation for the null economic results could be attenuation bias due to classical measurement error. We think this is unlikely because our economic measures are based on information that should be relatively easy for respondents to provide and our measures are based on modules commonly used on Colombia's census. Furthermore, while measuring income and wealth on surveys can produce noisy results, the use of indices helps to remove noise. Another potential source of attenuation bias is the "classic" selection problem, whereby selection on some variable attenuates its predictive power in the selected sample (Achen, 1986, 73-78). This could taint our results if it were the case that Table VI reported coefficients on respondents' economic conditions *prior* to joining an armed group. Our analysis, however, estimates coefficients on respondents' economic conditions after demobilizing, *controlling* for economic conditions prior to joining (see Appendix H).

Second, it is possible that economic conditions one year out might have a small effect on criminality because economic conditions change over time. Unlike social ties, which might not vary dramatically over time, economic conditions may be volatile and the effects of such economic conditions on criminality may be quite immediate. So, if employment conditions vary from year to year, then there may be cases of people who were employed one year after demobilization, but then subsequently lost their jobs and then engaged in crime. Without panel data we are not able to track how economic opportunity costs change over time. This might be one reason why we observe less evidence for economic opportunity costs than for wartime ties, which might be more stable over time.

A third explanation for the null economic results could be that the relationship between economic welfare and criminality is conditional on the presence of some other factor. While we find no evidence of a stronger association between economic factors and criminality conditional on fighting group, there are other sources of heterogeneity in the ex-combatant population that could moderate the relationship. We show in Table N.1 that there is also no evidence that the association between economic factors and criminality is greater for 'material types'—those who first joined an armed group for economic reasons.

Fourth, it could be the case that the employment and benefits component of Colombia's reintegration program succeeded in severing the link between economic insecurity and crime within the first year of demobilization. Indeed, the Colombian reintegration program has

witnessed certain successes—reflected in our survey data—with respect to the reincorporation of fighters into civilian life. Participation and receipt of reintegration benefits, at least in initial phases, was near universal, with virtually all respondents indicating that they had received assistance packages in their first year of demobilization. Sixty-seven percent of ex-combatants indicate being satisfied with their economic conditions in the year following their demobilization and 81 percent report having found employment in that time. Only 23 percent reported incomes that would translate to less than $10 per day (in purchasing power parity terms), indicating economic hardship. Our data indeed indicates that ex-combatants who participated in the reintegration program were significantly less likely to engage in crime (see Appendix H), possibly because of the economic benefits provided by the program.

A final possibility is that economic factors matter less for the binary decision of whether to engage in crime and more for how time is *allocated* between legal and illegal sectors. This is not something that we can investigate in our data, however. All in all, in light of the possible explanations for the null economic result elaborated here, we do not argue that economic conditions never matter for criminality. Nor do we interpret our results as a definitive challenge to the economic opportunity cost logic in light of our theory and the substantial quantity of empirical evidence to the contrary. This highlights the importance of considering how this might vary by context, as we discuss here and in Section 6. Nevertheless, our findings clearly show that enduring wartime ties—even when controlling for economic opportunity costs—play an important role in ex-combatant criminality following demobilization.

Table N.1: Economic conditions interacted with joining for material reasons

	(1) Criminal	(2) Criminal	(3) Violent	(4) Violent
Employed	0.02	-0.00	-0.07	-0.11
	(0.05)	(0.05)	(0.09)	(0.09)
Econ welfare obj. (index)	0.03	0.01	0.02	0.02
	(0.02)	(0.02)	(0.04)	(0.04)
Econ welfare subj. (index)	0.01	-0.02	0.03	-0.02
	(0.02)	(0.02)	(0.04)	(0.04)
Education	0.03	0.02	0.02	0.01
	(0.01)	(0.02)	(0.03)	(0.04)
Emp X joined material	-0.02	0.01	0.06	0.11
	(0.07)	(0.07)	(0.13)	(0.12)
Econ obj. X joined material	0.00	0.01	0.03	0.04
	(0.03)	(0.03)	(0.05)	(0.05)
Econ subj. X joined material	0.03	0.04	0.06	0.06
	(0.04)	(0.03)	(0.06)	(0.06)
Educ X joined material	-0.01	-0.01	-0.03	-0.03
	(0.02)	(0.02)	(0.04)	(0.04)
Joined for material reasons	0.12	0.09	0.05	-0.04
	(0.08)	(0.09)	(0.13)	(0.15)
Vert. ties	0.06***	0.04*	0.11***	0.08**
	(0.02)	(0.02)	(0.03)	(0.03)
Horiz. ties	0.07***	0.06*	0.17***	0.15***
	(0.02)	(0.02)	(0.04)	(0.04)
Observations	1158	1158	1158	1158
Econ_F_test_p	0.15	0.78	0.58	0.69
Social_ties_F_test_p	0.00	0.00	0.00	0.00
Clusters	570	570	570	570
Covariates	No	Yes	No	Yes

Standard errors in parentheses

Weighted least squares with municipality FE and indiv. controls.

Standard errors account for clustering by survey sampling blocks.

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

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