Appendix I. Additional robustness tests

1. Firm entry, relocation, and firm exit

Firms strategically choose to locate their facilities and are likely to favor stable regions without war. Also, they decide when to enter or exit the market and are more likely to exit (or less likely to enter) when the conflict is intense. Whereas this type of self-selection could bias our diff-in-diffs estimates, it is not a serious issue in our sample because 95% of the firms operated in the same location during the entire sample timespan. Nonetheless, we test the impact of this type of selection bias by rerunning our analysis with a panel that includes only those firms that operated in the same location during the entire sample timespan. Column I in Table A.1 shows that our results are practically unchanged.

2. Extending the pre-treatment period

Our baseline regression restricts the pre-treatment interval—from 2010 to late 2012—because Colombia changed governments in 2010. This restriction allows us to circumvent potential confounding shocks that give rise to changes in the political map. However, we gauge the impact of this modeling choice by extending the pre-treatment period. We run three additional regressions by adding one, two, and three years to the pre-treatment data. Columns II–IV in Table A.1 show that our insights are robust across all specifications.

Table A.1 Firm entry, relocation, and exit and extended pre-treatment period

Robustness check	Entry, Exit, and Relocation	Extending Pre-treatment Period 2009-2013	2008-2013	2007-2013
Specification	I	II	III	IV
FARC·AFTER	0.66***	0.81**	0.73**	0.69**
	(0.24)	(0.33)	(0.34)	(0.35)
FARC	−0.43	0.31	0.44	0.54
	(0.93)	(1.11)	(1.07)	(1.07)
AFTER	−0.88***	−1.54***	−1.63***	−1.70***
	(0.14)	(0.24)	(0.27)	(0.28)
Dependent variable		*log (inventory-to-assets)*		
Controls	Yes	Yes	Yes	Yes
Observations	16,428	57,211	64,758	71,821

Notes. The table provides the results for the "Entry, Exit, and Relocation" robustness test, in Column I, and the results for the "Extending Pre-treatment Period" robustness test, in Columns II-IV. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Cash, and Return on Investment. All coefficients are multiplied by 10.

3. Conflict intensity

The violence rate in FARC-dominated regions was heterogeneous before the conflict, meaning that more violent regions witnessed a more drastic violence-rate reduction. We divide the sample into five groups, according to the quintiles of the conflict intensity distribution (the corresponding thresholds are 5, 25, 105, and 185 deaths per 100,000 inhabitants). In Table A.2, we show that the treatment effect monotonically increases along the quintiles—a more intense conflict translating into more extreme inventory change—with the effect being four times larger in the highest quintile than in the lowest.

Table A.2 Robustness checks by quintile of conflict intensity

Specification	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV
FARC·AFTER	0.79^{*}	1.03^{***}	1.15^{***}	1.10^{***}	3.78^{**}	0.03	0.05	0.01	−0.06	0.00	0.48^{***}	0.57^{***}	0.68^{***}	0.69^{**}	2.13^{*}
	(0.46)	(0.39)	(0.40)	(0.40)	(1.89)	(0.03)	(0.04)	(0.04)	(0.05)	(0.10)	(0.18)	(0.18)	(0.21)	(0.27)	(1.11)
FARC	0.04	1.04	1.520	0.92	−2.16	-0.04^{*}	-0.05^{**}	-0.06^{**}	0.01	-0.18^{*}	0.66	0.60	1.08^{**}	0.55	1.52
	(1.51)	(1.29)	(1.21)	(1.44)	(2.94)	(0.02)	(0.02)	(0.03)	(0.04)	(0.10)	(0.47)	(0.49)	(0.52)	(0.58)	(1.15)
AFTER	-1.43^{***}	-1.40^{***}	-1.39^{***}	-1.41^{***}	-3.62^{*}	0.35^{***}	0.35^{***}	0.35^{***}	0.38^{***}	0.35^{***}	-0.50^{***}	-0.50^{***}	-0.50^{***}	-0.54^{***}	−1.76
	(0.23)	(0.21)	(0.22)	(0.21)	(1.85)	(0.03)	(0.03)	(0.03)	(0.02)	(0.08)	(0.14)	(0.14)	(0.16)	(0.17)	(1.09)
Dep. variable	*log (inventory-to-assets)*					*log (assets)*					*log (inventory)*				
Quintile	First	Second	Third	Fourth	Fifth	First	Second	Third	Fourth	Fifth	First	Second	Third	Fourth	Fifth
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892	36,892

Notes. The table shows how firms change their inventory-to-assets, assets, and inventory holdings by quintiles of conflict intensity. We measure conflict intensity as the battle-related deaths per 100,000 inhabitants. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Cash, and Return on Investment. All coefficients are multiplied by 10.

4. Alternative dependent variables

We rerun our analysis with two alternative inventory metrics: Inventory Turnover (IT) and the Gross Margin Return on Inventory (GMROI). Alan et al. (2014, Section 2.1) define these metrics:

$$IT_{it} = \frac{COGS_{it}}{INV_{it}} \text{ , and}$$
$$GMROI_{it} = \frac{Sales_{it} - COGS_{it}}{INV_{it}},$$

where $COGS_{it}$ is the cost of goods sold and $Sales_{it}$ is the firm's recorded sales at time t. Table A.3 shows that both metrics significantly decreased after the peace process. This result is consistent with our main result: IT and $GMROI$ move in opposite directions, relative to the inventory-to-assets ratio. We also measure the effect of the conflict on the firm's production efficiency, using three proxies for productivity: profit margins, total factor productivity, and return on investment. Table A.3 shows that, indeed, the peace process made firms more efficient; we observe an increase in these three metrics. These effects, however, lack enough statistical significance, meaning the change in inventory didn't make a drastic enough change to explain the observed shifts in inventory (at least in the short term). Similarly, we estimate the effect on revenue and net income (in Columns VI and VII). However, it would be an overreach to link our baseline results to changes in profitability, revenue, and income. War affects profitability for reasons unrelated to inventory—that is, by increasing insurance rates and transportation costs, decreasing access to external markets, or, simply put, by creating frictions in the operational environment. For this reason, our empirical framework doesn't allow us to disentangle the effect of inventory from these confounders. Also, research shows that whereas firms are swift to adapt their operations to war—see Amodio and Di Maio (2017), Petracco and Schweiger (2012), and Klapper et al. (2013)—the effect on income is slow to materialize—Humphreys (2003) and Murdoch and Sandler (2002) show that war affects income only two to four years after a violence rate change. Accordingly, we would need a longer time span, and a different identification approach, to link changes in inventory to changes in profitability.

5. Alternative measures of firm assets

In the baseline model, we used total fixed assets to estimate inventory, which includes all fixed assets held by the firm. In Table A.4, however, we re-estimate our model using three alternative definitions of

Table A.3 Alternative dependent variables

Specification	I	II	III	IV	V	VI	VII
FARC·AFTER	-0.09^{***}	-0.13^{**}	0.01	0.02	0.18	0.10^{*}	0.02
	(0.03)	(0.05)	(0.01)	(0.01)	(0.19)	(0.06)	(0.03)
FARC	0.04	0.16	0.01	−0.01	−0.09	-0.21^{**}	−0.04
	(0.16)	(0.28)	(0.01)	(0.01)	(0.19)	(0.10)	(0.05)
AFTER	−0.01	-0.09^{**}	0.02^{***}	−0.02	-0.54^{***}	0.07	0.28^{***}
	(0.02)	(0.04)	(0.00)	(0.02)	(0.12)	(0.05)	(0.03)
Dependent variable	*log (IT)*	*log (GRMOI)*	*log (profit margin)*	*log (TFP)*	*log (ROI)*	*log (revenue)*	*log (net income)*
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,588	34,588	45,585	16,428	49,190	63,108	50,253

Notes. IT: Inventory Turnover. GRMOI: Gross Margin Return on Inventory. TFP: Total Factor Productivity. ROI: Return on Investments. Controls: Altitude, Location, Surface Area, Industry, Assets, and Financial leverage. All coefficients are multiplied by 10.

assets in the denominator: (i) Short-term assets (cash, inventory, marketable securities trades accounts receivable, inventories, and prepaid expenses), (ii) Long-term assets (property, plant, equipment, long-term investments, trademarks, and goodwill acquired), and (iii) Equity. In all cases, the results hold at the 1% level.

Table A.4 Model estimates: Alternative assets definitions

Specification	I	II	III
FARC·AFTER	1.36^{***}	0.96^{***}	2.99^{***}
	(0.44)	(0.35)	(1.07)
FARC	1.95	0.47	−1.62
	(1.60)	(1.15)	(3.44)
AFTER	-2.04^{***}	-1.43^{***}	-2.95^{***}
	(0.28)	(0.23)	(0.57)
Dep. variable	*log (inventory-to-assets)*		
Assets measure	Short-term	long-term	Equity
Controls	Yes	Yes	Yes
Observations	67,803	67,948	67,982

Notes. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: 1%***, 5%**, 10%*. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment. All coefficients are multiplied by 10.

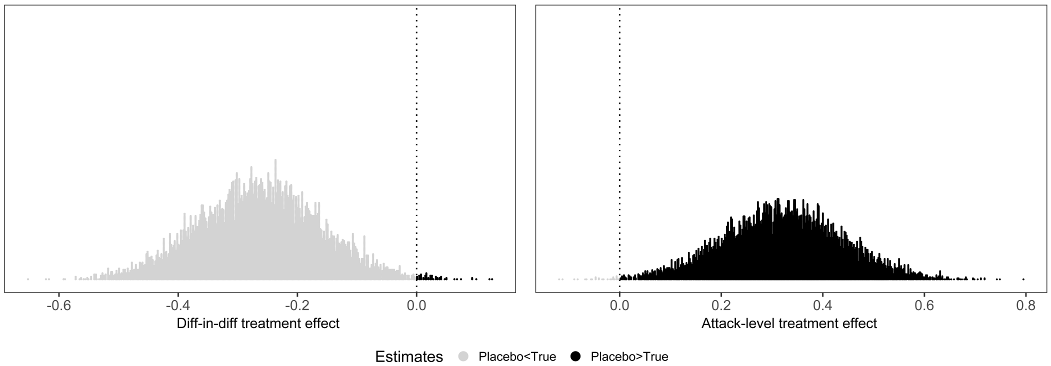
6. Cross-sectional placebo test

In diff-in-diffs models, standard errors are vulnerable to cross-correlation between the treated subjects, which could make our estimates spurious. One way to gauge the impact of this cross-correlation is via a cross-sectional placebo test. In this test, we replicate our analysis 10,000 times; each time, we create a synthetic dataset that randomly assigns observations to the treatment and control group (in the same proportion as in the true sample). We re-estimate our diff-in-diffs analysis and attack-level analysis for each of these 10,000 synthetic samples. We then compare the results of the 10,000 placebo estimates with our true estimates. If our true estimates were artifacts of cross-correlation, they would not stand out relative to the placebo estimates. But Figure A.1 shows that our baseline results do stand out—less than 0.68% of the placebo estimates have more extreme values than our true estimates.

7. Alternative specifications

We present 17 additional specifications to check the robustness of our baseline model. Table A.5 shows the coefficients of our baseline diff-in-diffs model using (i) unwinsorized samples, (ii) alternative standard error specifications, and (iii) fixed and random effects. Collectively, we conclude these specifications are consistent with our baseline results.

Figure A.1 Placebo simulations



Notes. We run regressions for 10,000 synthetic datasets. We construct these datasets by randomly assigning observations to treatment and control groups in the same proportion as in our baseline estimates. We then plot the distribution of the ratio of the placebo estimates to our actual estimates. We normalize our true estimates to zero. We benchmark the placebo results to the true estimates.

Table A.5 Regressions with alternative standard error specifications

Specification	I	II	III	IV	V	VI	VII	VIII
FARC·AFTER	0.50***	0.49***	0.92***	0.93***	0.93**	0.93***	0.92***	0.93***
	(0.19)	(0.17)	(0.29)	(0.29)	(0.38)	(0.38)	(0.34)	(0.34)
FARC	−0.52	0.61	−0.90**	0.27	−0.90	0.27	−0.90***	0.27
	(0.60)	(0.45)	(0.39)	(0.48)	(0.54)	(0.54)	(0.24)	(0.28)
AFTER	−0.48***	−0.49***	−1.41***	−1.41***	−1.41	−1.41	−1.41***	−1.40***
	(0.15)	(0.14)	(0.21)	(0.21)	(1.87)	(1.86)	(0.25)	(0.25)
Dep. variable	*log (inventory-to-assets)*							
Model	Clustered errors							
Standard errors	Municipality	Municipality	Firm	Firm	Industry	Industry	Robust	Robust
Clusters	269	269	12,510	12,510	31	31	-	-
Time effects	No	No	No	No	No	No	No	No
Unit effects	No	No	No	No	No	No	No	No
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	49,191	49,191	49,191	49,191	49,191	49,191	49,191	49,191

Specification	IX	X	XI	XII	XIII	XIV
FARC·AFTER	0.93***	0.85**	0.85**	0.93**	0.63***	0.64***
	(0.31)	(0.33)	(0.33)	(0.37)	(0.21)	(0.21)
FARC	0.27	-	-	0.27	0.09	0.09
	(1.23)	-	-	(0.55)	(0.33)	(0.33)
AFTER	-	−1.34***	-	−2.10	0.41	1.12***
	-	(0.24)	-	(2.81)	(0.38)	(0.22)
Dep. variable	*log (inventory-to-assets)*					
Model	Fixed effects					
Standard errors	Municipality	Municipality	Municipality	Industry	Industry	Industry
Clusters	269	269	269	31	31	31
Time effects	Yes	No	Yes	Yes	No	Yes
Unit effects	No	Yes	Yes	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	49,191	49,191	49,191	49,191	49,191	49,191

Specification	XV	XVI	XVII
FARC·AFTER	0.87***	0.87**	0.87***
	(0.33)	(0.33)	(0.33)
FARC	−0.68	−0.67	−0.68
	(1.90)	(1.90)	(1.90)
AFTER	−2.01***	−1.34***	−2.01***
	(0.45)	(0.24)	(0.45)
Dep. variable	*log (inventory-to-assets)*		
Model	Random effects		
Standard errors	Municipality	Municipality	Municipality
Clusters	269	269	269
Time effects	Yes	No	Yes
Unit effects	No	Yes	Yes
Controls	Yes	Yes	Yes
Observations	49,191	49,191	49,191

Notes. The Column I–II shows diff-in-diffs estimates of models using unwinsorized samples and clustering errors by municipality. The Column III–VI shows estimates of models with alternative standard error specifications, changing the clustering unit from the municipality level to the industry and firm level. Namely, the Column III–IV shows diff-in-diffs estimates clustering errors by firm; the Column V–VI shows estimates clustering errors by industry. The Column VII–VIII shows estimates using no clusters but robust standard errors. The Column IX–XVII shows estimates of models using fixed and random effects and clustering errors by municipality or by industry. Specifically, the Column IX–X shows estimates of one-way models (including time or unit effects) with errors clustered by municipality. The Column XI shows estimates of two-way fixed effects models (including time and unit effects) with errors clustered by municipality. The Column XII-XIII shows estimates of one-way models with fixed effects at the industry level and errors clustered at the industry level; the Column XIV shows estimates of a two-way model. Finally, the Column XV-XVII shows diff-in-diffs estimates of models with random effects and using errors clustered by municipality. Specifically, the Column XV–XVI shows estimates of one-way models and the Column XVII of a two-way model. Collectively, we conclude these specifications are consistent with our baseline results. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment. All coefficients are multiplied by 10.

Appendix II. Testing the parallel-trends assumption

A. The parallel trend assumption for the baseline model

Our diff-in-diffs model's key identifying assumption is that, absent treatment, both groups would have the same trend. We probe this assumption by inspecting the pre-treatment trends, in Figure 4. Visually, these trends seem to drift in the same direction, supporting our identifying assumption. But we stamp out any possible doubt with two numerical tests: (i) a pre-treatment placebo test and (ii) a lead-lag test.

1. Pre-treatment placebo test

To perform a "pre-treatment placebo" test, we begin by removing all data points observed after the treatment date and, using the left-over data, run a diff-in-diffs regression with a fake (or placebo) treatment period. Specifically, we run six regressions: each regression has a different placebo treatment years that span 2006 to 2012. Our first regression estimates (shown in Column I of Table A.7) are obtained after letting 2007 be the placebo treatment year, meaning that 2006 and 2007-12 make up the pre-treatment and post-treatment periods, respectively; our second regression estimates (Column II) are obtained by letting 2008 be the placebo treatment year, meaning that 2006-2007 and 2008-12 constitute the pre-treatment and post-treatment periods; etc.

If the diff-in-diffs coefficient of the placebo regression is significant, either our experiment is contaminated with pre-treatment effects or the parallel trend assumption is invalid. But the Columns I-VI of Table A.7 show that the diff-in-diffs coefficients are insignificant—across all specifications.

2. Lead-lag test

To test the parallel-trends assumption, Autor et al. (2003) adapt Granger's Causality test to a diff-in-diffs setting, using a specification that interacts each period dummy with the treatment group's dummy. To this end, we consider m pre-treatment periods (leads) and n post-treatment treatment periods (lags) and follow Autor et al.'s specification:

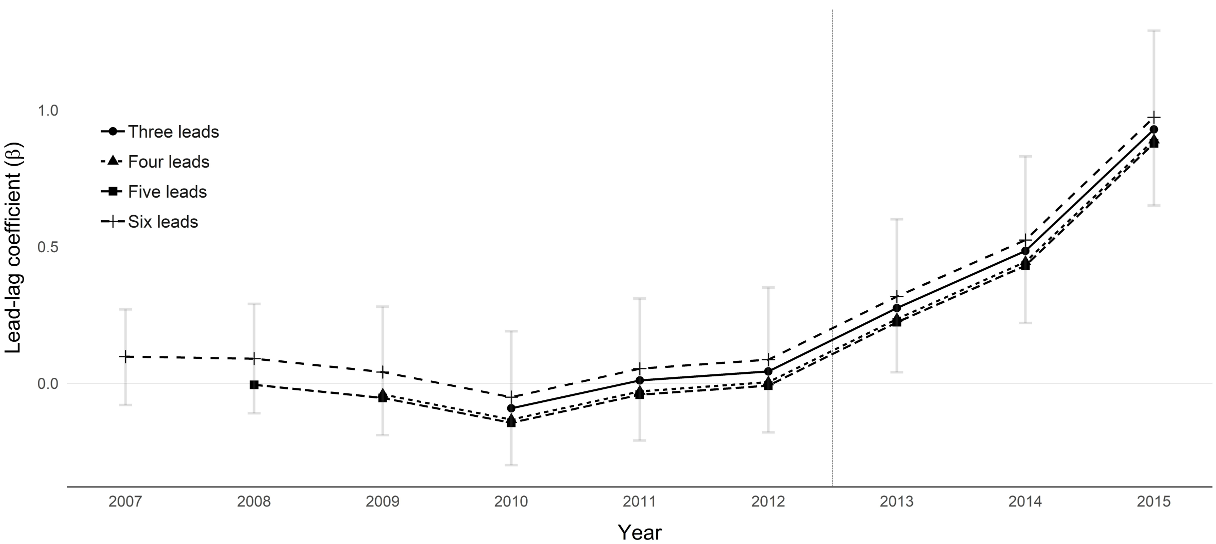
$$Inv_{it} = \alpha + \underbrace{Year_t}_{\text{period indicator}} + \underbrace{FARC_i}_{\text{treatment indicator}} + \underbrace{\omega X_{it}}_{\text{controls}} \tag{12}$$

$$+ \underbrace{\sum_{j=-m}^{0} \beta_j YEAR_{j+2013}\, FARC_i}_{\text{leads}} + \underbrace{\sum_{j=0}^{n} \beta_j YEAR_{j+2013}\, FARC_i}_{\text{lags}} + \varepsilon_{it}. \tag{13}$$

In our main model, the panel spans 2010 to 2016, giving us three leads and three lags. But for the purpose of making the test more robust, we run three additional regressions, extending the pre-treatment period by one, two, and three years.

If the parallel-trends assumption is satisfied, the lead's coefficients should equal zero (i.e., $\beta_{-m}=$ ....$=\beta_{-1}= 0$), meaning that the treatment group's trend moves in the same direction as the control group's trend. Thus, all lead coefficients should be statistically insignificant. Figure A.2 and Table A.6 suggest that the treatment and control groups have parallel trends across all four specifications.[15]

Figure A.2 Lead-lag analysis



Note. The figure shows the 95% confidence bands of the specification with six lags.

Table A.6 Lead-lag test

Specification	$m=6$	$m=5$	$m=4$	$m=3$
	leads			
β_{-6}	0.10			
	(0.09)			
β_{-5}	0.09	−0.01		
	(0.10)	(0.10)		
β_{-4}	0.04	−0.05	−0.04	
	(0.12)	(0.12)	(0.10)	
β_{-3}	−0.05	−0.14	−0.13	−0.09
	(0.12)	(0.13)	(0.11)	(0.08)
β_{-2}	0.05	−0.04	−0.03	0.01
	(0.13)	(0.13)	(0.12)	(0.11)
β_{-1}	0.09	−0.01	0.00	0.04
	(0.14)	(0.14)	(0.13)	(0.20)
	lags			
β_0	0.32**	0.22	0.23*	0.28**
	(0.14)	(0.15)	(0.13)	(0.13)
β_1	0.52***	0.43***	0.44***	0.48***
	(0.15)	(0.16)	(0.15)	(0.14)
β_2	0.97***	0.88***	0.89***	0.93***
	(0.16)	(0.17)	(0.16)	(0.15)
Dependent variable	*log (inventory-to-assets)*			
Controls	Yes	Yes	Yes	Yes
Observations	78,533	71,830	64,764	57,217

Table A.7 Pre-treatment placebo test

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.05	−0.01	−0.03	−0.03	0.04	0.05
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)
FARC	0.65***	0.70***	0.71***	0.70***	0.69***	0.69***
	(0.16)	(0.46)	(0.15)	(0.15)	(0.15)	(0.14)
AFTER	−0.07	−0.05	0.04	−0.03	−0.23***	−0.226***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Dependent variable	*log (inventory-to-assets)*					
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,569	53,569	53,569	53,569	53,569	53,569

Notes. The table on the left presents results for the lead-lag test employing Equation 12 and 13, using firms in ELN territories (control group), and firms in FARC territories (treatment group). Controls: Altitude, Location, Surface Area, Industry, Assets, Cash, and Return on Investment.

B. The parallel trend assumption for alternative models

In Section 8, we performed two additional diff-in-diffs analyses: one that includes firms in peaceful territories as an additional control group and one that uses demand as the dependent variable. To show that the analysis for these two diff-in-diffs specifications is valid, we need to test the parallel-trends assumption.

[15] We let the first observation of the pre-treatment period be the excluded dummy.

To this end, we begin by running a lead-lag test for these two models, in Table A.8. The estimates from Columns I and II show results for the specification from the peaceful-territories specification: Column I's estimates use firms in peaceful territories models as the control group, whereas Column II's uses both firms in ELN and peaceful territories as the control group. The Specifications in Columns III–VII contain a lead-lag test for the demand analysis from Section 8.3, using an aggregate sample (Column III) and by dissecting the sample across commodity producers, manufacturers, wholesalers, and retailers (Columns IV, V, VI, and VII, respectively).

Table A.8 Lead-lag test for additional analyses

Specification	I	II	III	IV	V	VI	VII
			leads				
β_{-6}	−0.01	−0.05	−0.19	-0.40	−0.10	−0.18	−0.09
	(0.112)	(0.12)	(0.12)	(0.50)	(0.17)	(0.18)	(0.27)
β_{-5}	0.55	0.53	−0.07	−0.65	0.08	−0.05	0.07
	(0.49)	(0.48)	(0.12)	(0.50)	(0.17)	(0.18)	(0.27)
β_{-4}	0.08	−0.24	−0.01	−0.50	0.15	0.02	−0.15
	(0.21)	(0.36)	(0.12)	(0.49)	(0.16)	(0.18)	(0.27)
β_{-3}	0.04	−0.04	0.12	−0.13	−0.01	0.15	0.36
	(0.11)	(0.11)	(0.12)	(0.50)	(0.16)	(0.18)	(0.27)
β_{-2}	0.15	−0.04	−0.08	−0.33	0.01	0.10	−0.36
	(0.12)	(0.16)	(0.12)	(0.48)	(0.16)	(0.18)	(0.27)
β_{-1}	0.10	0.09	0.09	0.36	0.05	0.02	0.01
	(0.15)	(0.14)	(0.12)	(0.47)	(0.16)	(0.18)	(0.27)
			lags				
β_0	0.42^{**}	0.41^{**}	0.10	0.24	0.09	0.13	−0.07
	(0.19)	(0.18)	(0.12)	(0.47)	(0.16)	(0.18)	(0.27)
β_1	0.53^{**}	0.48^{***}	0.09	0.11	0.05	0.09	0.10
	(0.23)	(0.22)	(0.12)	(0.47)	(0.16)	(0.19)	(0.26)
β_2	0.73^{***}	0.64^{***}	−0.12	−0.65	−0.11	−0.05	0.01
	(0.23)	(0.21)	(0.12)	(0.48)	(0.17)	(0.19)	(0.27)
Dependent variable	*log(inventory-to-assets)*				*log (orders)*		
Test	Peaceful territories		Aggregate	Commodities	Manufacturing	Wholesale	Retail
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	178,454	178,454	137,159	20,649	43,711	50,594	22,205

Notes. The table shows estimates of a lead-lag test to support the parallel trend assumption employed in Section 8. We run a lead-lag test with $m = 6$, employing Equation 12 and 13. Column I's estimates use firms in peaceful territories models as the control group, whereas Column II's uses firms in ELN and peaceful territories as the control group. The results in Columns III–VII present results for the lead-lag test employing Equation 12 and 13, $m = 6$, and using a log-linearized version of demand as the dependent variable. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment. All coefficients are multiplied by 10.

In addition, we perform pre-treatment placebo tests: In Tables A.9, we present estimates for the placebo test of the peaceful-territories analysis (Section 8.2); in Tables A.10–A.14, we present estimates for the pre-treatment placebo tests of the Demand analysis (Section 8.3).

Table A.9 Pre-treatment placebo test using firms in peaceful territories

Specification	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
FARC·AFTER	0.41	0.47	−0.12	−0.47	−0.56	−0.59	0.04	0.08	−0.24	−0.06	−0.02	0.03
	(0.33)	(0.33)	(0.31)	(0.30)	(0.40)	(0.40)	(0.14)	(0.16)	(0.22)	(0.16)	(0.15)	(0.14)
FARC	0.24	0.24	0.68	0.84	0.81	0.72	−0.20	−0.22	−0.01	−0.13	−0.15	0.05
	(0.74)	(0.67)	(0.63)	(0.59)	(0.58)	(0.55)	(0.34)	(0.34)	(0.39)	(0.42)	(0.39)	(0.24)
AFTER	0.17	−0.06	0.09	0.53^{*}	0.74^{*}	0.89^{**}	0.53^{***}	0.33^{***}	0.21	0.12	0.19^{*}	0.24^{***}
	(0.32)	(0.31)	(0.26)	(0.29)	(0.40)	(0.30)	(0.10)	(0.11)	(0.13)	(0.12)	(0.11)	(0.09)
Dependent variable						*log (inventory-to-assets)*						
Beginning treatment	2007	2008	2009	2010	2011	2012	2007	2008	2009	2010	2011	2012
Control group			*Firms in peaceful territories*						*Firms in peaceful and ELN-dominated territories*			
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	145,534	145,534	145,534	145,534	145,534	145,534	145,534	145,534	145,534	145,534	145,534	145,534

Notes. The table shows the results of the placebo test using firms in peaceful territories as the control group. Columns I-VI present the results using firms in peaceful territories only, and Columns VII-XII present the results using firms in peaceful and ELN-dominated territories. With these tests, we provide support to the parallel-trends assumption implied in Section 8. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment. All coefficients are multiplied by 10.

Table A.10 Placebo test: Aggregate

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.05	0.05	0.04	0.11	-0.06	-0.04
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.05)
FARC	-0.06	-0.06*	-0.04	-0.02	-0.01	-0.02
	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
AFTER	0.09**	0.04	0.05	0.06**	0.27***	0.27***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Dep. variable			*log (orders)*			
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	92,855	92,855	92,855	92,855	92,855	92,855

Table A.11 Placebo test: Commodities

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.03	-0.02	-0.07	-0.06	-0.14	-0.18
	(0.16)	(0.12)	(0.10)	(0.10)	(0.11)	(0.14)
FARC	0.12	0.17	0.19**	0.18*	0.20***	0.18***
	(0.15)	(0.11)	(0.09)	(0.08)	(0.07)	(0.06)
AFTER	0.20	0.24**	0.24*	0.23*	0.16	0.21
	(0.14)	(0.11)	(0.10)	(0.09)	(0.10)	(0.13)
Dep. variable			*log (orders)*			
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,149	12,149	12,149	12,149	12,149	12,149

Table A.12 Placebo test: Manufacturing

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.07	0.08	0.07	-0.01	0.01	0.02
	(0.08)	(0.06)	(0.05)	(0.05)	(0.06)	(0.07)
FARC	-0.17**	-0.16***	-0.14***	-0.10**	-0.11***	-0.11***
	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)
AFTER	0.03	-0.07	-0.06	-0.01	0.22***	0.21***
	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
Dep. variable			*log (orders)*			
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30,717	30,717	30,717	30,717	30,717	30,717

Table A.13 Placebo test: Wholesale

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.01	0.03	0.02	0.02	-0.09	-0.06
	(0.08)	(0.06)	(0.05)	(0.05)	(0.06)	(0.07)
FARC	-0.04	-0.06	-0.05	-0.04	-0.02	-0.03
	(0.07)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
AFTER	0.09	0.03	0.05	0.01	0.35***	0.36***
	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)
Dep. variable			*log (orders)*			
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,852	34,852	34,852	34,852	34,852	34,852

Table A.14 Placebo test: Retail

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.05	-0.01	0.03	-0.02	-0.10	-0.07
	(0.12)	(0.09)	(0.08)	(0.08)	(0.09)	(0.11)
FARC	-0.06	-0.01	-0.04	-0.01	0.01	-0.01
	(0.11)	(0.08)	(0.07)	(0.06)	(0.05)	(0.05)
AFTER	0.21*	0.21**	0.23***	0.27***	0.34***	0.33***
	(0.11)	(0.08)	(0.08)	(0.08)	(0.08)	(0.10)
Dep. variable			*log (orders)*			
Beginning treatment	2007	2008	2009	2010	2011	2012
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,137	15,137	15,137	15,137	15,137	15,137

Notes. The tables show the results of the pre-treatment placebo test employed to support the parallel-trends assumption of the demand analysis presented in Section 8.3. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment. All coefficients are multiplied by 10.

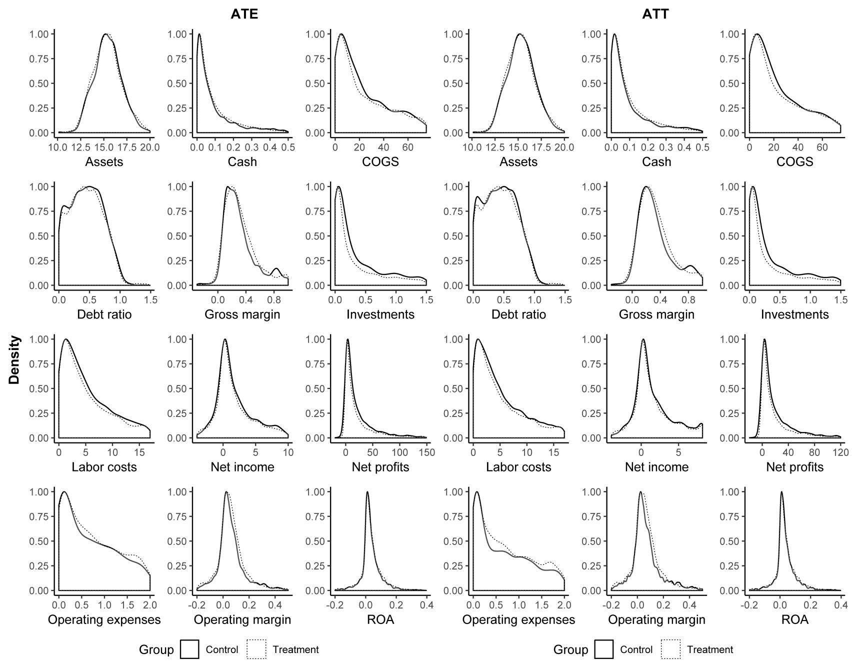
Appendix III. Attack-level analysis: Distributional balancing and the impact of unobserved confounders

A. Distributional Balancing. Propensity score matching depends on the assumption that the treatment and control samples are identically distributed across all covariates (i.e., the distributions are "balanced"). If the distributions are unbalanced—as it's often the case—we can adjust them by reweighting each observation. So, whereas it is acceptable to have unbalanced distributions at the outset, our analysis will fail if we can't find a suitable weight vector to rebalance them. Fortunately, the unadjusted distributions in our sample are remarkably balanced—and become practically identical after we re-weight them. A visual inspection of the adjusted distributions—in Figure A.3—confirms that the density functions of the treatment and control samples are almost indistinguishable. We provide more accurate evidence with three statistics that measure the balance of the distributions' means, variances, and cumulative distribution functions.

1. *Balance of means.* We measure the standardized bias for each variable to determine the balance of the distributions' first moment. To calculate the standardized bias of covariate x, we measure the difference of the means, $\mu_x^{treatment} - \mu_x^{control}$, and divide it by (i) the treatment group's standard deviation, when measuring the Average Treatment on the Treated (ATT), or (ii) the pooled standard deviation, when measuring the Average Treatment Effect (ATE). Figure A.4 and Table A.15 illustrate the standardized bias for each covariate, before and after reweighing the distributions. As a rule of thumb, the adjusted standardized differences should be smaller than 0.1. Our sample's reweighed distributions comfortably satisfy this requirement.
2. *Balance of variances.* We estimate the balance of the distributions' second moment with the treatment and control group's variance ratio. By convention, we place the largest variance in the numerator, so the ratio is bounded below by one. A ratio of one means that the variances are perfectly balanced and, as a rule of thumb, a ratio below two is acceptable after adjusting the distributions. Our sample's variance ratios are all significantly smaller than two (most are below 1.1).
3. *Balance of the cumulative distribution function.* The Kolmogorov-Smirnov statistic measures the maximum distance between the cumulative distribution functions. This statistic ranges from zero (perfect balance) to one (full imbalance). By convention, a value below 0.05 is recommended after adjusting. Table A.15 shows that all our adjusted covariates meet this recommendation. In fact, a visual inspection of the adjusted distributions, in Figure A.3, confirms that the density functions of the treatment and control samples are almost indistinguishable.

B. Assessing the impact of unobserved confounders in the attack-level analysis. Our propensity score matching estimates could be subject to unobserved confounders. But to what extent? To

Figure A.3 Covariates density functions



Notes. The graph shows the covariates' density functions of the reweighted distributions. The x-axis is scaled in millions of Colombian Pesos ($1 USD≈$3,400 COP).

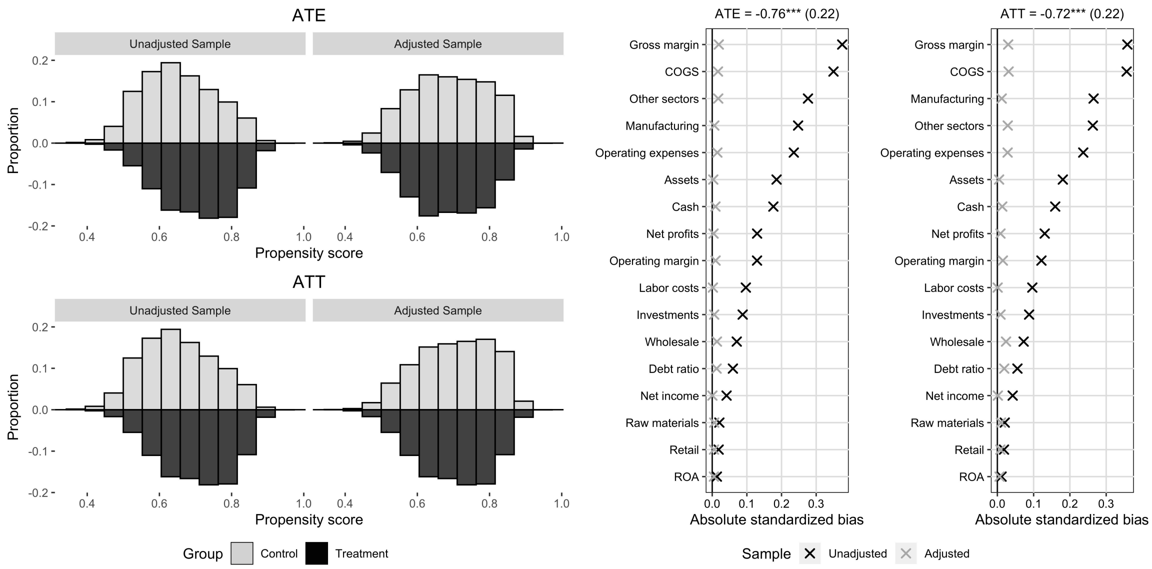
Table A.15 Statistics for the distributional balancing

Statistic	Stand. bias		Variance ratio		Kolmogorov-Smirnov		Stand. bias		Variance ratio		Kolmogorov-Smirnov	
Covariate	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.
Cash	0.18	-0.01	1.58	1.09	0.08	0.03	0.16	-0.01	1.58	1.10	0.08	0.03
Operating margin	0.13	-0.01	1.31	1.05	0.09	0.03	0.12	-0.02	1.31	1.04	0.09	0.03
ROA	-0.01	0.01	2.05	1.40	0.04	0.03	-0.01	0.01	2.05	1.41	0.04	0.03
Assets	-0.19	0.00	1.13	1.09	0.10	0.03	-0.18	-0.01	1.13	1.07	0.10	0.03
Debt ratio	-0.06	0.01	1.39	1.20	0.05	0.02	-0.06	0.02	1.39	1.25	0.05	0.02
Net profits	-0.04	0.00	1.08	1.09	0.05	0.02	-0.04	0.00	1.08	1.10	0.05	0.02
Net income	-0.13	0.00	1.03	1.02	0.08	0.02	-0.13	0.01	1.03	1.03	0.08	0.02
Labor costs	-0.10	0.00	1.03	1.02	0.07	0.01	-0.10	0.00	1.03	1.01	0.07	0.01
Operating expenses	-0.24	0.02	1.02	1.01	0.12	0.02	-0.24	0.03	1.02	1.02	0.12	0.02
COGS	-0.35	0.02	1.07	1.01	0.17	0.02	-0.36	0.03	1.07	1.01	0.17	0.03
Investments	-0.09	-0.01	1.01	1.02	0.06	0.03	-0.09	-0.01	1.01	1.02	0.06	0.03
Gross margin	0.38	-0.02	1.21	1.07	0.18	0.03	0.36	-0.03	1.21	1.07	0.18	0.04
Propensity score	0.48	-0.01	1.07	1.10	0.21	0.04	0.49	-0.03	1.07	1.10	0.21	0.05
Effect	Average treatment effect						Average treatment effect on the treated					

Notes. The table shows the statistics for the unadjusted (Unadj.) and adjusted (Adj.) distributions. We present three statistics: Standardized bias (Stand. bias), variance ratio, and Kolmogorov-Smirnov.

answer this question, we apply the sensitivity analysis in Rosenbaum (2002), which reports the amount of hidden bias required to explain estimates' significance. This analysis relies on the principle of randomization inference by evaluating the treatment effect bounds after inserting a degree of "hidden bias." Parameter Γ measures the amount of bias by setting the odds of differential assignment to treatment due to uncontrolled factors. For instance, if (i) $\Gamma = 1$, we assume no unobserved bias; (ii) if $\Gamma = 1.5$, we assume that hidden bias affects the odds of treatment by 1.5 (a 50% shift); and (iii) if $\Gamma = 2$, we assume it affects the treatment odds by 2 (a 100% shift). After fixing Γ, we obtain two bounds: the Hodges-Lehmann Bounds (which express the bounds of the treatment effect's point estimate) and the Wilcoxon Signed Rank Bounds (which express the bounds of the corresponding p-values). If our results

Figure A.4 Results of the distributional balancing analysis

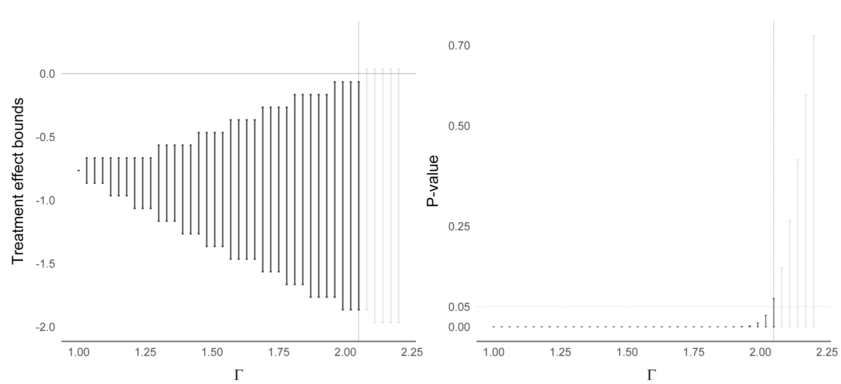


Notes. Left: We present histograms showing the propensity score distribution before (unadjusted) and after (adjusted) reweighting the raw data using the ATE and ATT propensity score matching. Right: We provide the absolute standardized bias statistics.

vary dramatically for small values of Γ, our estimates are highly sensitive to unobserved confounders, but if they remain significant for large values of Γ, it is unlikely that our results are an artifact of uncontrolled factors. Table A.16 and Figure A.5 show that it is almost impossible for our results to be driven by unobserved bias. In fact, under a value of $\Gamma = 2$, our estimates would still have a p-value below 0.05 and remain negative. To put this result in perspective: even if we're assuming that unobserved bias shifts the likelihood of treatment by 100%—a colossal degree of hidden bias—our results would still be significant at the 95% level.

Another potential concern is that attack types could be clustered in territories. In our sample, however, this isn't the case— Figure A.7 shows that all regions were afflicted by all attack types. Specifically, the average municipality experienced 18 attacks, and 94.2% of these municipalities experienced at least one attack of each type.

Figure A.5 Treatment effect bound and p-value from the Rosenbaum analysis



Note. The figure shows the treatment effect bound and p-value for different values of Γ, ranging from $\Gamma = 1$ to $\Gamma = 2.25$.

Table A.16 Rosenbaum sensitivity analysis

Γ	Hodges-Lehmann		P-value	
	Lower	Upper	Lower	Upper
1.00	-0.77	-0.77	0.00	0.00
1.10	-0.97	-0.67	0.00	0.00
1.20	-1.07	-0.67	0.00	0.00
1.30	-1.17	-0.57	0.00	0.00
1.40	-1.27	-0.57	0.00	0.00
1.50	-1.37	-0.47	0.00	0.00
1.60	-1.47	-0.37	0.00	0.00
1.70	-1.57	-0.27	0.00	0.00
1.80	-1.67	-0.27	0.00	0.00
1.90	-1.77	-0.17	0.00	0.00
2.00	-1.87	-0.07	0.00	0.01
2.05	-1.87	-0.07	0.00	0.07
2.10	-1.97	0.04	0.00	0.22
2.15	-1.97	0.04	0.00	0.47

Note. The table shows the lower (Lower) bound and upper (Upper) bound of the Hodges-Lehmann point estimate (Hodges-Lehmann) and the Wilcoxon signed rank p-value (P-value).

Appendix IV. Additional tables and figures

1. Changes in inventory and assets

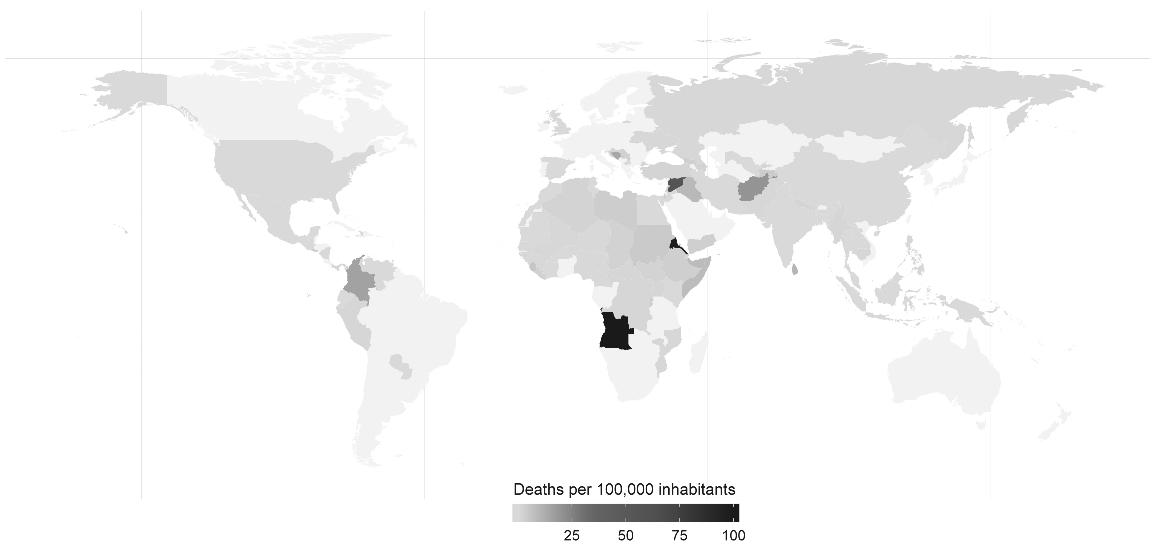
Table A.17 Model estimates: Inventory and assets

Specification	I	II	III	IV	V	VI
FARC·AFTER	0.71***	0.66***	0.69***	0.03	0.01	0.01
	(0.19)	(0.2)	(0.21)	(0.04)	(0.04)	(0.04)
FARC	1.21**	1.26**	1.16**	0.01	0.02	0.02
	(0.48)	(0.50)	(0.50)	(0.03)	(0.03)	(0.03)
AFTER	−0.50***	−0.50***	−0.50***	0.34***	0.34***	0.34***
	(0.14)	(0.16)	(0.16)	(0.03)	(0.03)	(0.03)
Dependent Variable		*log (inventory)*			*log (assets)*	
War threshold	Low	Medium	High	Low	Medium	High
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,892	32,579	31,542	36,892	32,579	31,542

Notes. The table shows diff-in-diffs estimates using log(inventory) and log(assets) as the dependent variable. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: 1%***, 5%**, 10%*. Controls for Columns I -III: Altitude, Location, Surface Area, Industry, Assets, Cash, and Return on Investment; controls for Columns IV-VI do not include Assets. All coefficients are multiplied by 10.

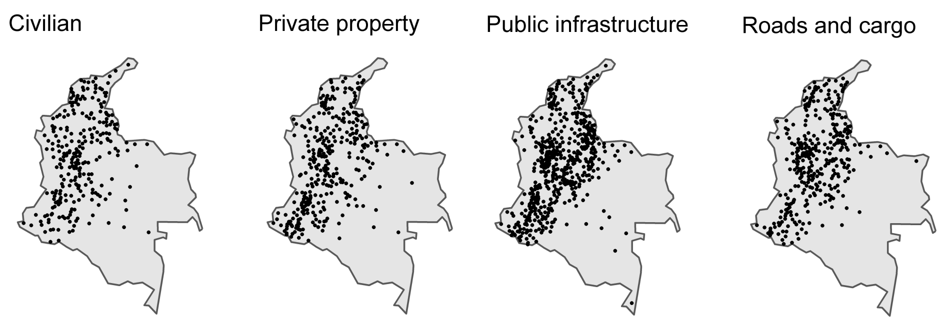
2. Global warfare

Figure A.6 Conflict intensity map



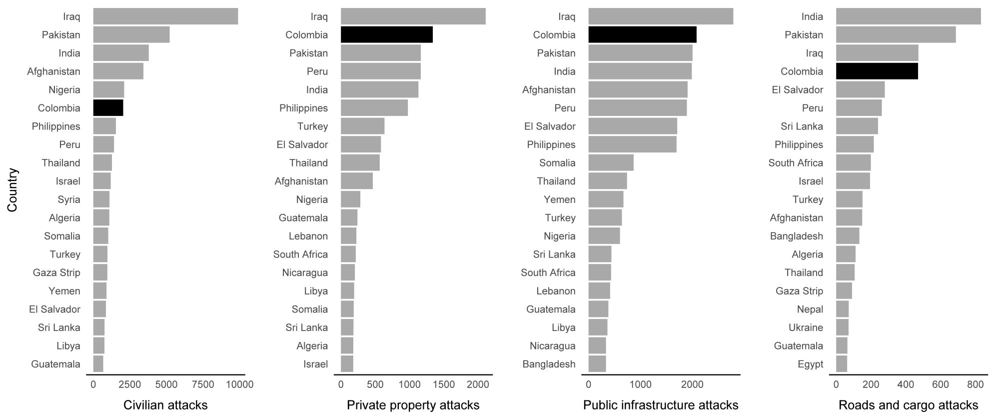
Notes. The figure shows the average number of battle-related deaths per year using data from 1989-2018 (all data available). Data source: Pettersson et al. (2019).

Figure A.7 Attack by type



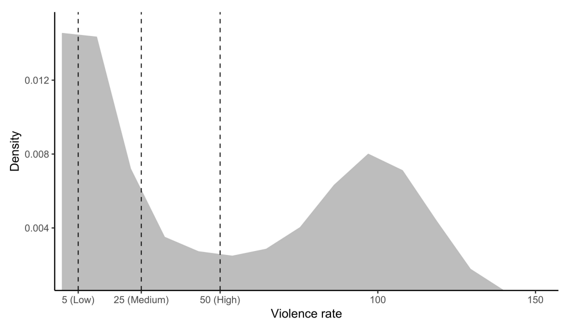
Note. The figure displays attack events by type in Colombia from 1995 to 2015.

Figure A.8 Attacks by country and type



Notes. The figure displays the number of attacks, by type and country (for the top twenty countries). We only count war-related crimes. Source: University of Maryland's Global Terrorism Database.

Figure A.9 Violence rate distribution in Colombia



Note. The figure displays the violence rate distribution (battle-related death per 100,000 inhabitants) across Colombia's municipalities.

Table A.18 Classification of war by type

Name	Location		Intensity		Conduct		
	Interstate	Intrastate (civil)	Total	Limited	Conventional	Unconventional	Asymmetric
1989 Paraguayan coup d'état		x		x		x	x
2004 Haitian coup d'état		x		x		x	x
Abkhaz–Georgian conflict		x	x		x		x
Cenepa war	x			x	x		
Colombian civil war		x		x			x
CPP–NPA–NDF rebellion		x		x		x	x
Eritrean–Ethiopian war	x			x	x		
Ethiopian civil war		x	x			x	
Gaza–Israel conflict		x	x			x	x
Heglig crisis	x			x	x		
India–Pakistan border skirmishes	x			x	x		
Indonesian invasion of East Timor		x		x	x		x
Insurgency in Aceh		x		x		x	x
Insurgency in Laos		x		x		x	x
Invasion of Kuwait	x		x		x		
Iraq war	x		x		x		
Iraqi civil war		x	x				x
Karen conflict		x		x			x
Libyan civil war		x		x			x
Punjab insurgency		x		x			x
Second Chechen war		x		x			x
Somali civil war		x	x				x
Sri Lankan civil war		x	x				x
Syrian civil war		x	x				x
War in Afghanistan		x	x				x
War in Donbass (Ukraine)	x		x				x
Western Iran clashes		x		x			x
Western Sahara war				x			x
Yemeni civil war		x	x				x
Yugoslav wars		x	x			x	

Note. The table classifies world conflicts using the UCDP/PRIO Armed Conflict Dataset, version 18.1.

3. Specifications by conflict intensity

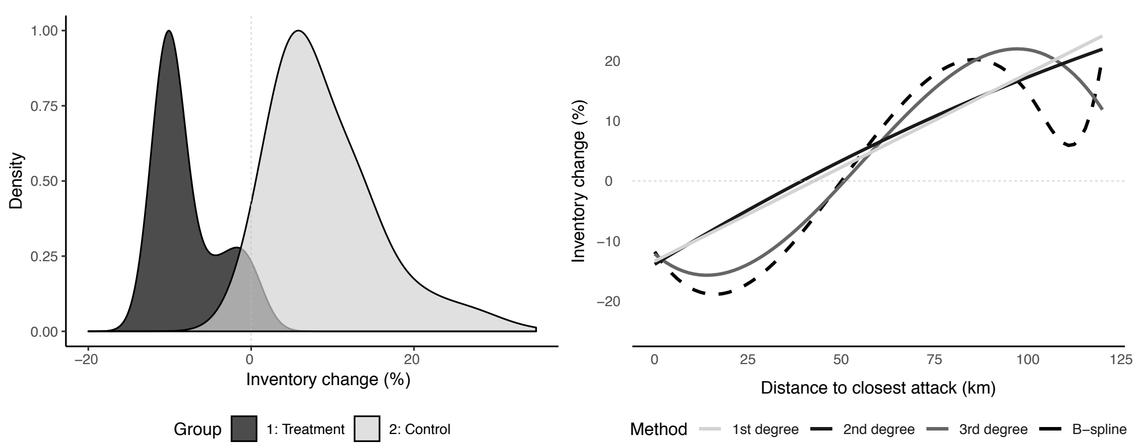
Table A.19 Inventory response by attack type

Specification	I	II	III	IV	V	VI
Civilian	-0.03^{***}	-0.03^{***}	-0.03^{**}	0.01	-0.01^{***}	-0.02^{***}
	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)	(0.00)
Private property	0.01^{*}	0.01^{**}	-0.05^{***}	-0.05^{***}	-0.01^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Public infrastructure	-0.01	-0.01^{***}	0.02^{***}	0.02^{**}	0.01^{**}	0.01^{*}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Roads and cargo	0.02^{**}	0.02^{***}	0.02	0.01	0.01^{*}	0.01^{*}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Dependent variable			*log (inventory-to-assets)*			
Attack intensity	Lowest tertile		Medium tertile		Highest tertile	
Controls	No	Yes	No	Yes	No	Yes
Observations	163,325	163,325	17,891	17,891	16,476	16,476

Notes. The table measures how firms change their inventory following four attack types. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$. Controls: Altitude, Location, Surface Area, Industry, Assets, Financial leverage, and Return on Investment.

4. Inventory change and firm's distance to attacks

Figure A.10 Inventory change as function of a firm's distance to the closest attack



Notes. The figures describe the firm's inventory change one year after an attack. The figure on the left shows the inventory change distribution by firms that suffered an attack within 50 km from their location (treatment group) and firms that suffered an attack beyond 50 km from their location (control group). The figure on the right shows evidence of a positive concave relationship between distance to the closest attack and inventory change. Estimates are scaled by 10,000.

5. Test of Sample differences: FARC vs. ELN and FARC vs. Peace Regions

Table A.20 Municipality characteristics

	GDP per capita		Population	
Group	FARC vs. ELN	FARC vs. Peace	FARC vs. ELN	FARC vs. Peace
Difference	0.04	-0.60^{***}	0.10	-2.57^{***}
	(0.08)	(0.11)	(0.20)	(0.28)

Notes. The table presents the results of two-sample t-tests with unequal variances, where Ho: difference = 0 and Ha: difference $\neq$ 0. The tests evaluate the average differences of (i) GDP per capita and (ii) population between the group of municipalities in FARC-dominated territories vs. ELN-dominated territories or peaceful territories. Standard errors (in parentheses) are robust and clustered by municipality. Significance levels: $1\%^{***}, 5\%^{**}, 10\%^{*}$.

Table A.21 Firm and asset composition by industry

	Firm composition		Asset composition	
Group	FARC vs. ELN	FARC vs. Peace	FARC vs. ELN	FARC vs. Peace
Commodities producers	0.01	0.03	0.01	0.03
Manufacturers	0.03	0.04	0.02	0.04
Wholesalers	0.02	0.02	0.02	0.02
Retailers	0.01	0.01	0.01	0.01
Other	0.01	0.10	0.02	0.10
Total Absolute Deviation	0.08	0.20	0.08	0.20

Notes. The table presents the absolute differences in the distribution of assets and firms in FARC-dominated territories vs. ELN-dominated territories or peaceful territories. All differences are in percent points.

6. Summary statistics by sector

Table A.22 Summary statistics by sector

Variable	Commodities		Manufacturing		Wholesale		Retail	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Total Inventory	8.62	6.14	12.66	3.68	12.33	4.04	12.27	3.69
Finished Inventory	7.08	6.34	10.08	5.38	11.62	4.77	11.83	4.23
Work-in-process Inventory	3.82	5.65	7.96	6.04	3.43	5.55	2.15	4.55
Raw Inventory	1.96	4.34	10.54	4.92	1.3	3.8	0.98	3.27
Cash-to-assets	0.82	4.68	0.84	3.71	1.64	5.58	2.72	7.62
Equity	14.69	1.82	14.62	1.83	13.94	1.59	13.61	1.54
Assets	15.29	1.71	15.36	1.71	14.91	1.51	14.53	1.47
Net Income	11.05	3.11	11.92	2.3	11.52	2.04	11.19	1.95
Cost of Goods Sold	11.51	5.91	14.41	3.41	14.15	3.79	12.87	3.49
Profit Margin	-1.36	0.96	-1.31	0.66	-1.45	0.73	-1.30	0.69
Sales	14.47	2.04	15.36	1.78	15.26	1.74	14.92	1.70
Return on Assets	-3.94	1.61	-3.36	1.23	-3.42	1.22	-3.34	1.17
Financial Leverage	0.33	0.34	0.40	0.25	0.45	0.23	0.44	0.22
Revenue	11.42	4.83	13.72	2.84	13.39	2.93	13.28	2.71
Payroll	8.12	4.77	9.62	3.91	8.61	4.04	8.41	4.07
Firm-year observations	51,982		59,932		62,509		28,967	

Notes. All variables are in thousands of Colombian Pesos (COP). In June 2019, US\$1≈COP\$3400. Total Inventory, Finished Inventory, Work-in-process Inventory, Equity, Assets, Net Income, Cost of Goods Sold, Sales, Revenue, and Payroll are expressed in logarithms. Financial leverage = total debt (short-term debt + long-term debt) / total assets. St Dev: Standard deviation.