American Economic Association

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Source: *American Economic Journal: Economic Policy*, Vol. 2, No. 4 (November 2010), pp. 26-57
Published by: American Economic Association
Stable URL: http://www.jstor.org/stable/25760084
Accessed: 11-08-2016 18:41 UTC

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American Economic Journal: Economic Policy 2 (November 2010): 26–57
http://www.aeaweb.org/articles.php?doi=10.1257/pol.2.4.26

Detecting Illegal Arms Trade†

By Stefano DellaVigna and Eliana La Ferrara*

We propose a method to detect illegal arms trade based on investor knowledge. We focus on countries under arms embargo and identify events that suddenly increase or decrease conflict intensity. If a weapon-making company is trading illegally, an event that increases the demand for arms may increase stock prices. We find positive event returns for companies headquartered in countries with high corruption and low transparency in arms trade. We also suggest a method to detect potential embargo violations based on chains of reactions by individual stocks. The presumed violations positively correlate with the number of UN investigations and Internet stories. (*JEL* D74, F13, G14, K42, L64)

Armed conflict is a leading cause of poverty and death in developing countries. In the Democratic Republic of Congo alone, violent conflict is considered responsible for about four million deaths since 1998 (Small Arms Survey 2005). To curb the extent of conflict, the United Nations has increasingly resorted to the imposition of arms embargoes, alongside peacekeeping operations and humanitarian interventions. Arms embargoes are viewed as "smart sanctions" since they target only the arms sector; hence, they are less likely to harm the victims of warfare, unlike general trade sanctions. Yet, illegal arms trade undercuts the effectiveness of the embargoes, as argued in investigative reports by advocacy groups such as Amnesty International and Human Rights Watch.

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The case-by-case evidence in these investigative reports, however, accounts only for a limited fraction of the illegal arms trade, and mostly concerns brokers in arms deals. More generally, quantitative information on the nature of this trade is hard to come by. The most basic questions are still unanswered. Which groups of countries illegally export weapons in areas of civil conflict? Which types of companies are involved? How profitable is the trade of illegal arms? A better answer to these questions is a pre-condition for effective policies.

In this paper, we propose a method to provide initial answers to these questions. We detect illegal arms trade based on the investor knowledge embedded in financial markets. We rely on the fact that company insiders and well-informed investors are likely to be aware of illegal trades, even if the general public is not. We focus on eight countries that were under UN arms embargo in the period 1990–2005: Angola, Ethiopia, Liberia, Rwanda, Sierra Leone, Somalia, Sudan, and Yugoslavia. In these countries, we identify eighteen events during the embargo that suddenly increase or decrease conflict intensity. To select the events, we use historical information and counts of newswire stories in the event days.

We identify weapon-making companies using the standard industrial classification (SIC) code information in the Datastream-Worldscope dataset, supplemented with a list of the top-100 weapons companies (J. Paul Dunne and Eamon Surry 2006). For these 153 companies, we consider the abnormal returns in the three days surrounding the events. If a company is not trading or trading legally, an event increasing the hostilities should not affect its stock price or should affect it adversely, since it delays the removal of the embargo and hence the re-establishment of legal sales. Conversely, if a company is trading illegally, the event should increase its stock price, since it increases the demand for illegal weapons.

We separate companies on the basis of proxies for the legal and reputational costs of illegal arms sales. We expect the cost of embargo violations to be lower in countries with higher corruption and lower transparency of arms sales. Further, we expect that lack of membership in a large organization like the Organisation for Economic Co-operation and Development (OECD), lower press freedom, higher bribe-paying, and lower participation by minority shareholders would also lower the cost of illegal arms trading.

We find that, for companies head-quartered in low-corruption countries, an event increasing conflict is associated with a decrease of 0.4 percentage points in 3-day abnormal stock returns. For companies in high-corruption countries, instead, an event increasing conflict is associated with over 1 percent increase in 3-day abnormal stock returns. These effects are statistically significant after allowing for arbitrary correlation of errors within an event date. We find similar results for the measures of transparency in arms sales and membership in the OECD, and weaker evidence using measures of press freedom, bribe-payment, and shareholder protection.

When considering the results event-by-event, we find the same pattern in 13 to 14 of the 18 events, indicating that the results are not due to a single event. The event returns are larger for events that are more unexpected or more significant according to news counts. The effect for companies in high-corruption countries occurs for the most part on the day of the event, suggesting that the event date is plausibly accurate. We present placebo specifications on leads and lags of stock returns in

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the 200 days before and after the event, as well as placebo specification on returns in other industries. On both accounts, we find no evidence of event returns in the placebo regressions. We also consider the impact of firm size and type of arms produced. The effects are stronger for smaller companies, for which the arms sales in countries under embargo are likely to constitute a larger share of sales, and somewhat larger for companies producing small arms and ammunitions, missiles, and explosives.

Our interpretation of these findings is that companies head-quartered in high-corruption countries are more likely to play a role in illegal arms trade, and hence benefit from the increase in hostilities. Companies in low-corruption countries are more likely to engage in legal arms trade, and are hurt by increases in hostilities that delay the re-establishment of legal trade. We formalize this interpretation with a simple model of conflict, embargo imposition, and firm competition with barriers to entry. We assume two states of conflict, an Embargo state—with high intensity of conflict—and a Non-Embargo state—with low intensity. Arms-producing companies differ in the cost of violating an embargo. High-cost companies do not sell arms in the Embargo state. As a consequence, given the barriers to entry, profits for the low-cost companies are higher in the Embargo state. In the model, increases in conflict have two effects: (i) they increase the contemporaneous demand for arms, and (ii) they increase the future likelihood of the Embargo state. While we cannot measure directly (i), we document (ii) showing that events increasing conflict are associated with a 10 percentage point increase in the probability of embargo the following year.

The model rationalizes the two main findings. First, increases in conflict during the embargo hurt companies with high legal and reputational cost of violation, lowering their market value. These companies do not benefit from the increased demand (since they are not trading), and are hurt by the increased probability of the Embargo state in the future. Second, increases in conflict during the embargo substantially benefit companies with low cost of violation. The value of these companies increases because of the current increase in demand, and because of the future increase in the likelihood of the Embargo state. A calibrated version of the model using the event returns yields estimates for the yearly profits for trade under embargo between \$1m and \$3m for the median firm. The implied industry-level yearly profits are in the order of hundreds of millions of dollars for a conflict.

This interpretation is subject to three caveats: we estimate average effect across companies, not responses company-by-company; we rely on the assumption of well-informed investors, which we cannot test; we cannot distinguish between direct violations of an embargo and arms sales to intermediaries which themselves violate the embargo, though we note that indirect violations, like direct ones, can also have legal and reputational costs for the companies exporting arms.

We consider alternative interpretations based on depletion of the stock of old arms, composition of arms produced, input and product mix, and regional instability. In contrast to our preferred explanation, most of these interpretations predict that the pattern of event returns should be similar for conflict events outside the embargo. Instead, for events outside the embargo we find no differential response for companies in high-and low- corruption countries, supporting our interpretation.

Next, we consider whether it is possible to detect individual firms violating the embargo. We conduct separate event studies for each company-event pair, and

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analyze cases in which a company has a *chain* of multiple significant reactions consistent with embargo violation within the same conflict. We identify 23 such chains, corresponding to 19 different companies. Three companies display chains of reactions for more than one conflict. While this evidence on detection is indirect and therefore not directly employable for forensic purposes, it can be used as a screening tool to identify targets of direct investigations. We relate these detection results to external sources in a validation exercise. We detect more predicted violations in conflicts with more documents on embargo enforcement by the UN Panels of Experts and Monitoring Groups. Also, we find more predicted violations for companies whose name appears more often in association with the word "embargo" on the Internet.

This paper is related to several strands of the literature. First, it contributes to the policy literature on arms embargoes (Loretta Bondi 2004; Brian Wood and Johan Peleman 1999; Control Arms Campaign 2006). This literature typically relies on legal analysis (e.g., the identification of pitfalls in export laws) or direct investigations (e.g., capture of illegal arms shipments) to denounce the limited effectiveness of embargoes and call for policy change. Our results suggest that violations spread well beyond the list of actors identified by the UN Sanctions Committees and by advocacy groups such as Amnesty International. However, our findings also suggest that the embargoes are, at least partially, effective in constraining arms trade. The negative returns for events during the embargo of companies in countries with low corruption and high transparency in arms export procedures indicate that the embargoes did limit sales from these countries: if the sanctions were completely ineffective, these companies should not be hurt by events increasing conflict.

Our paper is also related to the literature on the determinants and consequences of violence and conflict in developing countries (Paul Collier and Anke Hoeffler 1998; Edward Miguel, Shanker Satyanath, and Ernest Sergenti 2004; José G. Montalvo and Marta Reynal-Querol 2005). We suggest a return-based methodology to measure the illegal trade of arms, a (proximate) determinant of conflict.

The paper also relates to the studies of event returns for political events affecting political connections (Brian E. Roberts 1990; Raymond Fisman 2001), the party in power (Seema Jayachandran 2006; Erik Snowberg, Justin Wolfers, and Eric Zitzewitz 2007), legislative decisions (François-Xavier Delaloye, Michel Habib, and Alexandre Ziegler 2006), and conflict (Alberto Abadie and Javier Gardeazabal 2003; Gerald Schneider and Vera E. Troeger 2006; Massimo Guidolin and Eliana La Ferrara 2007; Arindrajit Dube, Ethan Kaplan, and Suresh Naidu 2008).

Our paper is also related to the literature on forensic economics, including the detection of teacher cheating (Brian A. Jacob and Steven D. Levitt 2003), tax evasion (Fisman and Shang-Jin Wei 2004; Justin Marion and Erich Muehlegger 2008), and corruption in sports (Mark Duggan and Levitt 2002; Wolfers 2006). Most closely related is Chang-Tai Hsieh and Enrico Moretti (2006), who use time-series changes in oil prices to infer whether the Iraq regime violated the oil-for-food program. Our aggregate results highlight features of the environment (e.g., corruption, low transparency in arms exports) that are correlated with embargo violations. Our company-level results seek to identify individual violators, although they are not sufficiently precise to be used as conclusive evidence.

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I. Conceptual Framework

In this section, we discuss qualitative predictions of the model, presented in detail in Section V. Events that change the demand for arms, such as the worsening of a conflict, have two effects: they affect the demand for arms, but also the likelihood of an arms embargo. For example, a sudden coup against a legitimate government signals both an increase in hostilities (and hence heightened demand for arms) and a likely embargo imposition.

The imposition of arms embargoes matters for firm value because embargoes raise the cost of selling arms. We allow for heterogeneity across firms in the cost of violating embargoes (e.g., because of high legal or reputation costs). Firms with high cost of violating the embargo stand too much to lose from the possible sanctions and do not sell arms during the embargo period. Firms with low cost of violation, instead, sell also during the embargo.

We model the industrial structure assuming some barriers to entry: only a fixed number of firms can enter the market. This assumption implies that the extra profits that the low reputations firms make during the embargo period are not eroded by entry of new firms.[1]

The value of a firm in a given period is the sum of current profits and the (discounted) expected continuation payoff. Positive shocks to the demand for arms increase current profits for both types of firms, but through their effect on the probability of the Embargo state, they have a heterogeneous impact across types of firms.

Consider first events which occur during an embargo. In this case an increase in the demand for arms unambiguously lowers the value of companies with high reputation costs. These companies do not reap the benefits of the increased demand during the embargo since they do not enter the market, and are hurt by the decreased probability that the embargo will be lifted in the future. In comparison, companies with low reputation costs benefit both from a contemporaneous increase in profits, and from an increased probability of future embargo. These results are summarized in Prediction 1, which we test in Tables 1–4.

PREDICTION 1 (*Events during Embargo*): *Increases in conflict intensity during the Embargo*

(i) *cause a decrease in value for companies with high cost of embargo violation;*

(ii) *cause an increase in value for companies with low cost of embargo violation (compared to the high-cost companies).*

Outside the Embargo, an increase in demand for arms has two opposing effects on the value of companies with high reputation costs. It increases current profits, but it also increases the future likelihood of an embargo, thus reducing profits. The total

[1] Considering the legal, ethical and logistical obstacles that firms have to face to export arms illegally to a country under embargo, the assumption that there exists a limited number of firms that are willing or able to overcome these obstacles seems realistic.

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effect is ambiguous. In comparison companies with low reputation costs have the same contemporaneous increase in profitability, but also a positive future expected increase in profitability. These results are summarized in Prediction 2, which we test in Table 5.

PREDICTION 2 (*Events outside the Embargo*): *Increases in conflict intensity outside the Embargo*

(*i*) *have an ambiguous effect on the value of companies with high cost of embargo violation;*

(ii) *cause an increase in value for companies with low cost of embargo violation (compared to the high-cost companies).*

II. Background and Data

A. *Arms Embargoes*

The imposition of arms embargoes is a relatively recent form of UN sanctions. In its first forty-five years, the Security Council only introduced an arms embargo twice: against South Africa and Southern Rhodesia. Starting in 1990, however, UN embargoes have been increasingly used, largely a result of the dissatisfaction with the humanitarian consequences of other forms of sanctions. Arms embargoes are viewed as "smart sanctions" since they target only the arms sector; hence, they are less likely to harm the victims of warfare, unlike general trade sanctions. Under article 41 of the UN Charter, states are legally obliged to comply with arms embargoes that the Security Council imposes and to implement policies such that individuals within their jurisdictions also comply with the embargo.

Still, the imposition of arms embargoes is an imperfect policy tool. Investigations point to several instances of violations of the embargoes (Control Arms Campaign 2006). The violations are partly a consequence of imperfections in the way international legislation concerning embargoes is translated into national laws, but are also a result of the difficulty of detecting illegal arms transactions. The bodies that investigate the violations—the UN Sanction Committees—have very limited power, and rely on the voluntary collaboration of national governments in providing information. As a consequence, systematic and quantitative evidence of arms violations is lacking (Bondi 2004). The lack of direct evidence on these trades is a motivation for this paper. We suggest that the indirect evidence stemming from our methodology can usefully complement the limited direct evidence from investigations.

We start by considering all arms embargoes imposed by the UN Security Council between 1975 and 2005, as listed in Table A1 of the Web Appendix. We then restrict our attention to embargoes satisfying four criteria: (i) The embargo imposition dates after 1980, to guarantee overlap with the return data; (ii) We can identify at least one salient and unexpected conflict event during the embargo period; (iii) No large-scale UN or US intervention occurred in the conflict, to diminish the importance of legal

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sales to these actors.[2] The final dataset includes eight African countries (Angola, Ethiopia and Eritrea, Liberia, Rwanda, Sierra Leone, Somalia, Sudan) and former Yugoslavia.

B. *Events*

For each of these eight countries we search for events affecting the intensity of conflict, occurring both inside the embargo and outside the embargo. We follow three criteria: (i) the event is important enough to attract the interest of media and investors; (ii) the event is, to a first approximation, unanticipated; (iii) the event unambiguously increases or diminishes the intensity (and expected duration) of the conflict. To select the events, we combine a qualitative reading of the history with a quantitative evaluation of criteria (i) and (ii). We count the newswire stories in Lexis-Nexis that mention the name of the country under embargo in the days surrounding the event.[3] As a measure of (i), we define the Event Importance i_t as the average of the news stories on the day of and the day after the event: $i_t = (n_t + n_{t+1})/2$, where n_t is the number of stories on day t, and t is the event day. As a measure of (ii), we define the Event Surprise s_t as the ratio of the Event Importance to the average daily number of stories in the four days preceding the event: $s_t = [(n_t + n_{t+1})/2]/[(n_{t-1} + n_{t-2} + n_{t-3} + n_{t-4})/4]$. We keep events that are sufficiently important (taking into account the limited news attention dedicated to these countries, typically $i_t \geq 10$) and surprising (typically $s_t \geq 2$). While the selection of the events also takes into account qualitative factors, in Table 3 we examine the robustness of the result to a purely quantitative event selection procedure.

Table A2 in the Web Appendix lists the events and the measures of Event Surprise and Event Importance. The eighteen events occurring during the embargo period are emphasized. We also list the fourteen events occurring outside the embargo, which we use in Table 5.

C. *Companies*

The main source of information on arms-producing companies is the matched Datastream-Worldscope dataset of daily stock returns for companies traded in all major stock markets. We include companies with the primary or one of the seven secondary SIC codes in the SIC groupings: 3482–3484, and 3489 (small arms and ammunitions), 3761, 3764, and 3769 (missiles), 3795 (tanks), and 2892 (explosives).[4] A second source is a list of top-100 weapon-making companies published by the Stockholm International Peace Research Institute (SIPRI) and compiled by Dunne and Surry (2006) for the year 2004. This classification is based on sources such as company Web sites and annual reports, a SIPRI questionnaire, news from military journals and newspapers. We include all the traded companies in either source available in Datastream.

[2] From the initial full list of embargoes, criterion (i) eliminates South Africa, criterion (ii) eliminates Haiti and Libya, and (iii) eliminates Afghanistan and Iraq.

[3] For robustness, we also run searches in which we specify both the country name and a name for the event (such as Attack, Fighting, and Peace), resulting in similar measures.

[4] Since the dataset does not include a dynamic SIC code, we classify companies based on their SIC codes in 2005.

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Table A3 in the Web Appendix presents a list of the countries in which the companies are head-quartered, as well as the number of companies in each country. Table A4 reports the full list of companies with the number of non-missing observations and the source of data.

D. *Measures of Cost of Embargo Violation*

We collect information on company characteristics that affect the cost of embargo violation: the ease of circumventing international restrictions on the flow of arms, the likelihood that companies may be caught breaching the embargo, and the monetary and reputational costs of an embargo violation. Lacking company-level information, we rely on indices pertaining to the countries where the companies are head-quartered, since the countries are responsible for monitoring the companies.

The first benchmark measure is the *Corruption Perception Index* (CPI) of *Transparency International* for the years 1995–2005. This index draws on expert surveys to measure the perception of corruption of public officials and politicians in a country. We use a time-average of this index to construct a discrete measure and a continuous measure of corruption (low cost of embargo violation). The discrete measure is an indicator variable for a value of the corruption index above the median. The continuous variable is the time-averaged index standardized to mean zero and standard deviation one. We use the indicator variable as our benchmark measure, but also examine the robustness to using the continuous variable.

The second benchmark measure is the *Small Arms Trade Transparency Barometer* produced by the Small Arms Survey over the years 2004–2006 for most of the countries in our sample. This index measures the extent to which a country provides transparent information on small arms exports and is based on export reports by exporting countries as well as international customs data. The index evaluates the timeliness, access, clarity, and comprehensiveness of the information provided by countries regarding their exports of small arms. In addition, it also verifies the information provided on granted and denied licences, and on actual deliveries. We use the overall score that takes into account all these components, average it across the years 2004–2006, and construct both a discrete and a continuous measure of low transparency (low cost of embargo violation).

As additional measures (detailed in the Web Appendix), we also use (i) the index of Control of corruption (CC) proposed by Daniel Kaufmann, Aart Kraay, and Massimo Mastruzzi (2006); (ii) membership in the OECD in 1985; (iii) a measure of press freedom provided by Freedom House; (iv) the Bribe Payers Index (BPI), also produced by *Transparency International*; (iv) the self-dealing index of Simeon Djankov et al. (2008) as a measure of protection of small shareholders.

In Table A3 in the Web Appendix we separate companies into OECD and non-OECD markets, and we indicate whether the countries where the companies are head-quartered belong to countries with low cost of embargo violation according to the measures above.

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E. *Returns*

For both the Datastream-Worldscope sample and the SIPRI sample, we use the daily return data from Datastream for the years 1985–2005. We drop penny stocks defined as stocks with price of less than two units in the local currency unit. We also trim the top and bottom 2/10,000th of returns to avoid extreme outliers. Finally, we drop returns that are zero for ten consecutive days, since this likely indicates a stale price series.[5]

For our main specification, we correct for correlation with market returns using a market model. For each year, we estimate the market model

$$r_{i,t} = \alpha_i + \beta_i r_{m(i),t} + \varepsilon_{i,t}, \tag{1}$$

where $r_{i,t}$ is the (unlogged) return of company i on day t and $r_{m(i),t}$ is the (unlogged) return of the value-weighted market index for the country in which company i is traded. We then generate abnormal returns $e_{i,t} = r_{i,t} - \hat{\alpha}_{ti,t} - \hat{\beta}_{i,t} r_{m(i),t}$ where $\hat{\alpha}_{i,t}$ and $\hat{\beta}_{i,t}$ are estimated on data for the previous year, requiring a minimum of 40 return observations. In most specifications, we focus on 3-day cumulative abnormal returns ($e_{i,t}^{(-1,1)} = e_{i,t-1} + e_{i,t} + e_{i,t+1}$), since the exact day of the event is sometimes hard to determine and we do not observe when the marginal investor learns the information. We show that the results are robust to using 3-day cumulative raw returns ($r_{i,t}^{(-1,1)} = r_{i,t-1} + r_{i,t} + r_{i,t+1}$) and 3-day cumulative excess returns ($r_{i,t}^{(-1,1)} - r_{m,t}^{(-1,1)}$). We also show that our results are similar when we employ one-day abnormal returns $e_{i,t}$. Finally, we match the events to returns on the same day.[6] For events occurring in the weekend, we shift the event date to the Monday following the weekend.

III. Event Studies

In this section we use an event study methodology to estimate whether on average conflict events affect stock returns for arms companies. We start by presenting a graphical analysis of event returns during and outside embargo periods. As suggested by Prediction 1, we analyze separately companies with high and low cost of embargo violation, as captured by the corruption level in the country. We then present a regression analysis with our benchmark estimates as well as additional results on: (i) the robustness to alternative indicators of legal and reputational costs; (ii) the selection of events; (iii) the timing of stock reactions; (iv) placebo treatments, and (v) heterogeneity by firm characteristics (size and the type of arms produced). We postpone further discussion of the event returns to events outside the embargo to Section IV. Finally, in Section VI we conduct event studies on individual firms, and we provide some external validation of our return-based detection methodology.

[5] The results are similar if we do not remove penny stocks or trim outliers. They are also robust to excluding companies that are thinly traded (Web Appendix Table A5).

[6] The results are similar if we shift the event date by one day for companies traded in stock markets with more than an 8-hour difference (such as Asian markets or Australia) (Table A5 of the Web Appendix).

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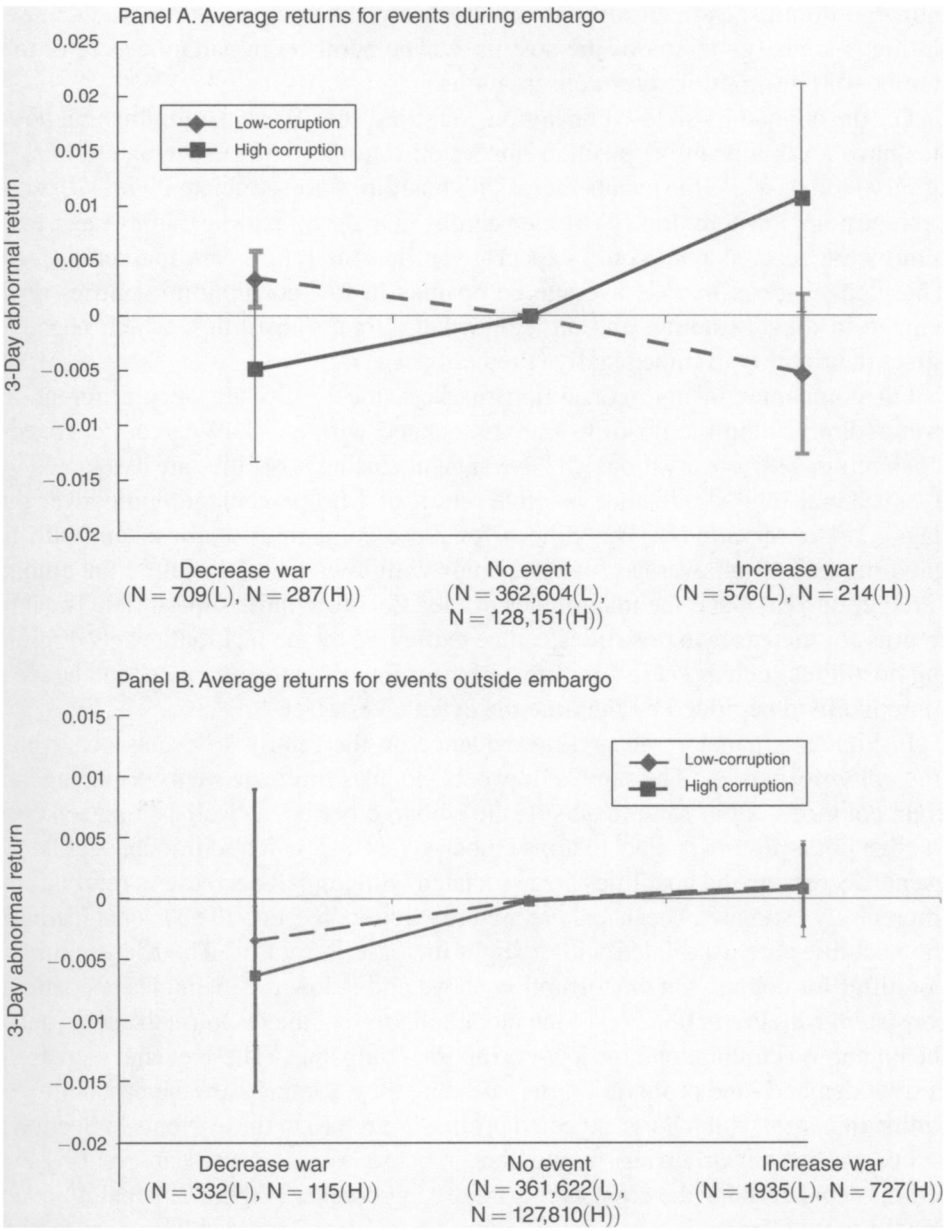

FIGURE 1

Notes: Figure 1, panel A and panel B display average 3-day abnormal cumulative returns and 95 percent confidence bars separately for days with events decreasing hostilities, no events, and events increasing hostilities. The figures also report the number of company-day observations over which the return is computed.

A. *Graphical Evidence*

In Figure 1, panel A, we plot the average (equal-weighted) abnormal 3-day return $e_{i,t}^{(-1,1)}$ (with 95 percent confidence interval) on days in which an event during an

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embargo diminishes the hostilities, in which no event occurs, or in which an event during an embargo increases the hostilities. The numbers in parenthesis refer to the number of non-missing return observations.

For the companies in low-corruption countries, the 10 events diminishing hostilities have a (significantly) positive impact on returns (0.32 percentage points, 709 observations), while the events increasing hostilities are associated with −0.54 percentage point lower returns (576 observations). On the remaining trading days, events returns are zero, as one would expect given that the returns are market-corrected. The data suggests that on average companies in low-corruption countries do not engage in illegal trading, and are somewhat hurt by hostilities, which negatively affect their ability to trade legally (Prediction 1(i)).

For companies in high-corruption markets, the results are very different. The events diminishing the hostilities are associated with a −0.49 percent decrease in stock return (287 observations). The events increasing hostilities are associated with a substantial (and significant) positive return of 1.06 percentage points over three days (214 observations). The pattern for these companies is consistent with illegal arms trading on average for companies with low cost of violating the embargo (Prediction 1(ii)), and the magnitudes of the effects are quite substantial. The larger returns for increases in hostilities can be explained by the fact that events diminishing hostilities such as cease-fires are easier for investors to anticipate, and hence are more likely to be priced by the time the event takes place.

In Figure 1, panel B we present evidence on the returns to events occurring in *non-embargo* periods. The sample of events includes fourteen events occurring in the eight countries of our sample outside the embargo period, as well as nineteen events in other countries not subject to arms embargo (see below for additional details). The events decreasing the hostilities are associated with a small decrease in returns, albeit imprecisely estimated because there are only five such events. The events increasing the hostilities are associated with a slight increase in returns. The event returns do not differ for countries with corruption above and below the median. This pattern is consistent with Prediction 2(i) of the model: the sign of the response to events outside the embargo is ambiguous for low-corruption companies. These events increase the current demand (and profits) of arms sales, but they also increase the probability of a future embargo, which hurts expected profits. We return to these events in Section IV.

For the events occurring during the embargo, we now present event-by-event returns separately for the eight events increasing conflict (Figure 2, panel A) and for the ten events decreasing conflict (Figure 2, panel B). Remarkably, for seven out of eight events increasing conflict (Figure 2, panel A) the abnormal returns are negative for companies in low-corruption countries, and positive for companies in high-corruption countries. Among the ten events decreasing conflict (Figure 2, panel B), there is a correspondent, though less regular, pattern: seven out of ten events are associated with positive returns among the low-corruption countries, and six out of ten events with negative returns among the high-corruption countries. Overall, for the companies in low-corruption countries, the sign of the event returns is consistent with Prediction 1(i) in 14 out of 18 events. Using a binomial test, we can reject the null that negative and positive returns are equally likely with a p-value of 0.0154, suggesting that this pattern is unlikely to be due to chance. Similarly, for the companies in high-corruption

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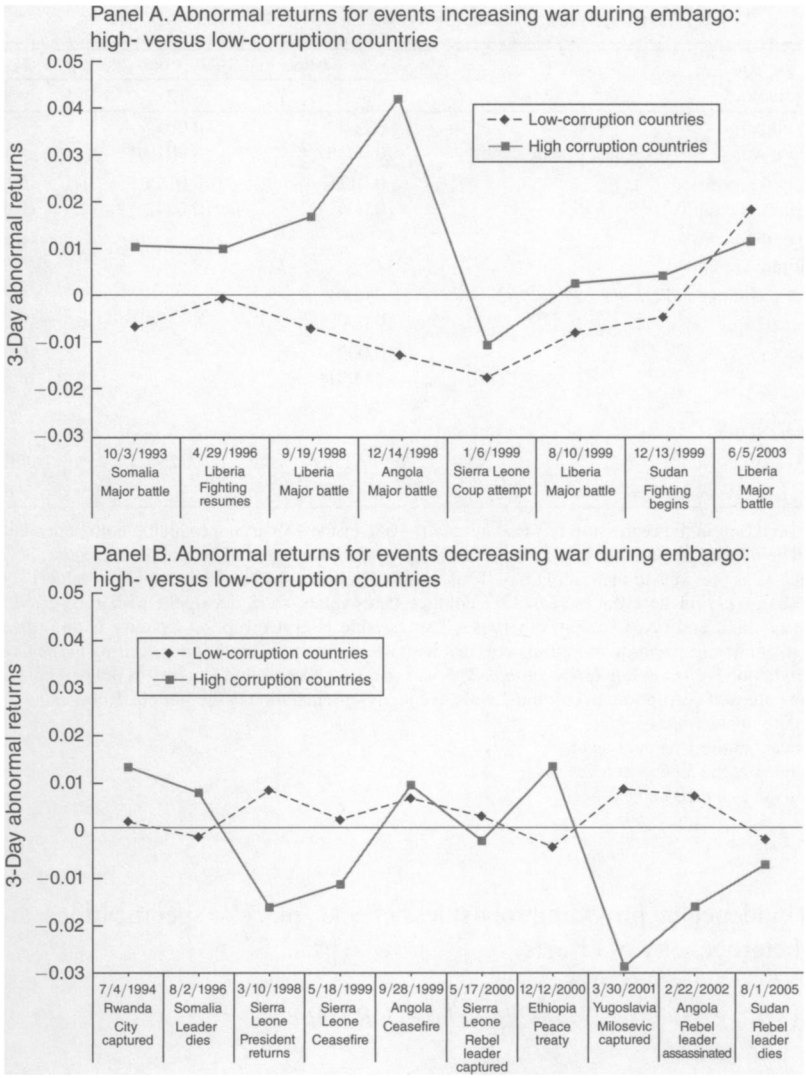

FIGURE 2

Notes: Figure 2 panel A and panel B display average 3-day abnormal cumulative returns separately for each event. The events are unexpected, significant occurrences affecting the hostilities during the arms embargo period in one of the eight countries in the sample. The list of events is in Appendix A2. The figure presents the returns separately for companies headquartered in countries with corruption above- and below-median according to the Corruption-Perceptions Index of *Transparency International.*

countries, in 13 out of 18 events the sign of the returns is consistent with Prediction 1(ii). The probability of 13 or more consistent signs is 0.0481, again a pattern unlikely to be random. In the remainder of the paper, to increase power we pool the events and consider aggregate event returns.

We next turn to a regression analysis. In order to gain more power, we impose the restriction, not rejected by the data, that increases and decreases in conflict intensity have symmetric effects (of opposite sign). The regression results complement the

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TABLE 1—STOCK MARKET REACTION TO WAR EVENTS: BENCHMARK EFFECTS

	Abnormal 3-day stock return (−1, 1)		
Dependent variable	(1)	(2)	(3)
Event during embargo	−0.0042	−0.0042	
(1 = increase war, −1 = decrease, 0 = no event)	(0.0018)**	(0.0019)**	
Event during embargo*	0.0115	0.0115	0.0073
(High-corruption country)	(0.0041)***	(0.0042)**	(0.0034)**
Event during embargo*			−0.0042
(Low-corruption country)			(0.0018)**
High-corruption country indicator	−0.0001		−0.0001
	(0.0002)		(0.0002)
Constant	−0.0001		−0.0001
	(0.0001)		(0.0001)
Include only event days		X	
Observations	492,569	1,786	492,569

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985–2005. The dependent variable is the abnormal 3-day cumulative return. The market correction is computed on the calendar year previous to the trading day. The variable Event During the Embargo takes value 1 if on day t, during the embargo period, an event increases the conflict, takes value −1 if, during the embargo period, an event decreases the conflict, and takes value 0 otherwise. The variable High-Corruption Country is an indicator variable indicating companies head-quartered in countries with above-median corruption according to the Corruption-Perceptions Index of *Transparency International*. The variable Low-Corruption Country is defined conversely for below median values of corruption. In column 2, only event days are included in the sample. Robust standard errors clustered by date in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

graphical evidence by providing robustness checks, placebo specifications, and estimates of heterogeneity of effects.

B. *Benchmark Results*

In Table 1, we present our main results for the event returns during the embargo, as in Figure 1, panel A. In column 1 we estimate the benchmark specification

$$e_{i,t}^{(-1,1)} = \alpha + \gamma Emb_t + \alpha_D D_i + \gamma^D Emb_t D_i + \eta_{i,t} \tag{2}$$

where $e_{i,t}^{(-1,1)}$ is the 3-day abnormal return for company i on date t; Emb_t is a variable that equals 1 if an event increasing conflict occurs during embargo at time t, −1 if an event decreasing conflict occurs during embargo at time t, and 0 otherwise. The variable D_i is an indicator for whether the company is head-quartered in a high-corruption country, or for other proxies of low cost of embargo violation. The standard errors are robust to heteroskedasticity and clustered by date, so as to allow for arbitrary correlation of returns within a date across companies. This clustering essentially counts each of the 18 events as one observation.

The estimates $\hat{\alpha} = -0.0001$ and $\hat{\alpha}_D = -0.0001$ indicate that, in absence of events, the average return is zero for both types of companies, as it should be, given

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the use of abnormal returns. An event raising hostilities during embargo lowers stock returns significantly by 0.42 percentage points ($\hat{\gamma} = -0.0042$) for companies in low-corruption countries, and the converse for an event decreasing hostilities. Relative to the effect in low-corruption countries, the effect of an event increasing hostilities in high-corruption countries is 1.15 percentage points higher ($\hat{\gamma}^D = 0.0115$), a significant difference. The coefficient estimates $\hat{\gamma}$ and $\hat{\gamma} + \hat{\gamma}^D$ capture the impact of events occurring during embargoes for the two types of companies, as in Figure 1, panel A.

In column 2, we estimate specification (2) only on event days; this requires setting $\alpha = \alpha^D = 0$. We obtain essentially identical point estimates and standard errors for both coefficients of interest, γ and γ^D. This is not surprising, since both α and α^D are estimated to be essentially zero. In the rest of the paper we use the whole sample, since this allows us to test that returns are on average zero on non-event days.

In column 3 we test if the overall effect for high corruption countries (and not just the differential one) is significant. We estimate

$$e_{i,t}^{(-1,1)} = \alpha + \gamma Emb_t\,(1 - D_i) + \alpha_D D_i + \gamma^D Emb_t\, D_i + \eta_{i,t},$$

so that γ^D captures the *overall* return for high-corruption countries (that is, not compared to low-corruption countries). The estimate $\hat{\gamma}^D = 0.0073$ is positive and significant. Hence, we detect significant evidence consistent with illegal arms trading also when we consider directly the impact in low-corruption countries, as opposed to the differential response of firms in high- and low-corruption countries.

In Table A5 of the Web Appendix we present additional robustness checks: (i) controlling for the per-capita GDP of the country producing arms does not affect the results; (ii) accounting differently for the time difference between the country of the event and the stock market where the company is traded does not change the estimates; (iii) the standard errors are somewhat smaller when we cluster by company, allowing for time-series correlation; (iv) adopting a more conservative approach to deal with stale price series leaves the results unaffected; (v) using the two-day abnormal returns $e_{i,t}^{(0,1)}$ (instead of $e_{i,t}^{(-1,1)}$) reduces the estimated γ for companies in low-corruption countries, but has little effect on the estimated γ^D for companies in high-corruption countries; (vi) the results do not depend on the market correction, since we obtain similar results using raw returns ($r_{i,t}^{(-1,1)}$) or returns net of the market ($r_{i,t}^{(-1,1)} - r_{m,t}^{(-1,1)}$).

Overall, our evidence suggests that on average investors expect arms companies in low-corruption countries to trade legally, but firms in high corruption countries to trade illegally.

C. *Measures of Cost of Embargo Violation*

So far, we examined the impact of corruption. In Table 2, we re-estimate specification (2) using alternative measures of the cost of embargo violation, presented in Section II. In panel A we employ discrete measures D_i, while in panel B we estimate the specification $e_{i,t}^{(-1,1)} = \alpha + \gamma Emb_t + \alpha_D S_i + \gamma^D Emb_t S_i + \eta_{i,t}$, where S_i is a continuous measure of the costs of embargo violation, standardized across countries with mean zero and standard deviation one (see Section II). Higher values indicate lower costs of embargo violation.

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TABLE 2—STOCK MARKET REACTION: MEASURES FOR COST OF EMBARGO VIOLATION

Dependent variable	Abnormal 3-day stock return (−1, 1)						
Measure of cost of embargo violation	High corruption percept. index (1)	Control of corruption index (2)	Low transparency of arms trade (3)	Non-OECD member (4)	Low press freedom (5)	High bribe-payer index (6)	High self-dealing index (7)
Panel A. Indicators for cost of embargo violation							
Event during embargo (1 = increase war, −1 = decrease, 0 = no event)	−0.0042 (0.0018)**	−0.0042 (0.0019)**	−0.0043 (0.0020)**	−0.0031 (0.0017)*	−0.0023 (0.0017)	−0.0027 (0.0017)	−0.0025 (0.0016)
Event during embargo* (Low cost of embargo violation, indicator)	0.0115 (0.0041)***	0.0117 (0.0043)***	0.0114 (0.0042)***	0.015 (0.0056)***	0.0061 (0.0045)	0.0058 (0.0046)	0.0055 (0.0040)
Low cost of embargo violation—Indicator	−0.0001 (0.0002)	−0.0002 (0.0002)	−0.0001 (0.0002)	−0.0002 (0.0003)	−0.0002 (0.0002)	0 (0.0002)	−0.0002 (0.0002)
Constant	−0.0001 (0.0001)	−0.0001 (0.0001)	−0.0001 (0.0001)	−0.0001 (0.0001)	−0.0001 (0.0001)	−0.0001 (0.0001)	0 (0.0001)
Panel B. Standardized continuous variables for cost of embargo violation							
Event during embargo (1 = increase war, −1 = decrease, 0 = no event)	0.0013 (0.0018)	0.0018 (0.0019)	0.0025 (0.0025)	— —	0.0008 (0.0015)	0.0007 (0.0018)	−0.0005 (0.0017)
Event during embargo* (Low cost of embargo violation, continuous)	0.0066 (0.0028)**	0.0072 (0.0029)**	0.0048 (0.0019)**	— —	0.0039 (0.0023)*	0.005 (0.0026)*	0.0016 (0.0017)
Low cost of embargo violation—continuous	−0.0002 (0.0001)	−0.0002 (0.0001)	0 (0.0001)	— —	0 (0.0001)	0 (0.0001)	−0.0001 (0.0001)
Constant	−0.0002 (0.0001)	−0.0002 (0.0001)	−0.0001 (0.0001)	— —	−0.0001 (0.0001)	−0.0001 (0.0001)	−0.0001 (0.0001)
Source of measures of cost of embargo violation:	Transparency International	Kaufmann et al. (2006)	Small arms Survey	OECD	Freedom House	Transparency International	Djankov et al. (2008)
Observations	492,569	492,569	475,128	492,569	492,569	477,908	492,569

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985–2005. The dependent variable is the abnormal 3-day cumulative return. The market correction is computed on the calendar year previous to the trading day. The variable Event During the Embargo takes value 1 if on day t, during the embargo period, an event increases the conflict, takes value −1 if, during the embargo period, an event decreases the conflict, and takes value 0 otherwise. In columns 1–6 we use six different measures of the reputational and legal costs of violating an embargo for the country where the company is headquartered (see Section III in the text). OECD membership is defined as of 1995, the first year of the sample. Panel A uses an indicator variable for below-median cost of embargo violation, while panel B uses a standardized version of the continuous variable. Higher values indicate lower cost of embargo violation. Robust standard errors clustered by date in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

In column 1, panel A, we reproduce the baseline effect of Table 1. In panel B, we obtain consistent results using the continuous standardized measure of corruption. A one-standard deviation increase in corruption significantly increases the return response to a war event by 0.66 percentage points ($\hat{\alpha}_D = 0.0066$). We obtain very similar results using the alternative corruption index proposed by Kaufmann et al. (2006) (column 2).

In column 3, we consider a measure that is more directly tied to arms production, the index of transparency of small arms trade collected by the Small Arms Survey. The more easily available is information on arms exports, the more difficult it is for a company to conceal illegal arms trades. While the indicator D_i for low transparency is correlated with the indicator of corruption, the two variables differ in 7 of the 23 countries for which the transparency data is available. We find that companies in countries

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with less transparent arms reports display 1.14 percentage points more reaction to the events during an embargo ($\hat{\gamma} = 0.0114$), a significant difference. The effect replicates using the continuous measure (panel B). This suggests that availability of information about arms trade is likely to be a determinant of embargo violations.

We then present the results using the additional measures. In column 4 we show that stock returns for non-OECD companies respond significantly more to conflict events during an embargo. This is consistent with membership in an international organization raising the reputation costs of a violating an embargo. In column 5, we use the measure of press freedom: the results are directionally similar, but the estimates are smaller and marginally significant only with the continuous variable. We obtain similar results using a measure of propensity of managers to pay bribes (column 6). Finally, in column 7 we use the Djankov et al. (2008) measure of the control powers of minority shareholders. To the extent that some minority shareholders are aware of and disagree with illegal arms trades, this measure captures their ability to question and block the arms trade. We do not find a significant impact, although the point estimate for γ^D is positive.

In the rest of the paper, we use the discrete measure D_i of corruption as the benchmark measure, supplemented by the discrete measure of transparency of arms trade in some of the specifications. The findings in the paper are similar using the continuous measure of corruption, the arms transparency proxy (discrete or continuous), and the measure of membership in the OECD.

D. *Event Selection*

As we discussed in Section II, the selection of events is based on a qualitative evaluation of the history of the conflicts, complemented by quantitative information on the number of news wire stories on days surrounding the events. In Table 3, we consider alternative definitions of the events. These results provide a test of the robustness of our selection criteria, and also allow us to assess the potentially heterogeneous impact of different types of conflict events (e.g., some events are more important than others).

In column 1 of Table 3 we reproduce the benchmark results using the standard set of 18 events. In column 2, we use a broader set of 35 events. This includes 17 additional events occurring during the embargo that, while significant for the history of the conflict, were not evaluated to be sufficiently unexpected or sufficiently salient. The results are qualitatively similar to the ones in the benchmark specification, but the point estimates are only about half as large. In column 3 we include variables for both definitions. The effect depends to a large extent on the events included in the core definition, which likely captures larger unexpected changes in the demand for arms.

In columns 4 and 5, we use a purely automated definition of events based on our proxies for Event Importance i_t (number of news stories) and Event Surprise s_t (increase in the number of news stories around the event), defined in Section II. Out of the broad sample of events, in column 4 we use the 21 events with $i_t \geq 10$ and $s_t \geq 2$, and in column 5 the 10 events with $i_t \geq 20$ and $s_t \geq 3$. As expected, the estimates of the coefficient γ^D using these cutoffs are larger than the estimates in the broad sample (column 2) and, using the more restrictive set of events in column 5,

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TABLE 3—STOCK MARKET REACTION: EVENT SELECTION

	Abnormal 3-day stock return (−1, 1)				
Dependent variable	(1)	(2)	(3)	(4)	(5)
Event during embargo	−0.0042		−0.0036		
(1 = increase war, −1 = decrease, 0 = no event)	(0.0018)**		(0.0026)		
Event during embargo* (High-corruption country)	0.0115		0.0096		
	(0.0041)***		(0.0048)**		
Event during embargo (broad definition)		−0.0024	−0.0005		
(1 = increase war, −1 = decrease, 0 = no event)		(0.0014)*	(0.0019)		
Event during embargo (broad definition)* (High-corruption country)		0.0069	0.0019		
		(0.0026)***	(0.0024)		
Event during embargo (automatic definition)				−0.0031	−0.0049
(1 = increase war, −1 = decrease, 0 = no event)				(0.0013)**	(0.0021)**
Event during embargo (automatic definition) (High-corruption country)				0.0086	0.0104
				(0.0029)***	(0.0045)**
Indicator for high-corruption country	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	−0.0001	−0.0001	−0.0001	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Set of events	Core set of events	Broad set of events	Broad set of events	Events with surprise>=2 import.>=10	Events with surprise>=3 import.>=20
Number of events	18	35	35	21	10
Observations	492,569	492,569	492,569	492,569	492,569

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985–2005. The dependent variable is the abnormal 3-day cumulative return. The market correction is computed on the calendar year previous to the trading day. The variable Event During the Embargo takes value 1 if on day t, during the embargo period, an event increases the conflict, takes value −1 if, during the embargo period, an event decreases the conflict, and takes value 0 otherwise. The variable High-Corruption Country is an indicator variable indicating companies head-quartered in countries with above-median corruption according to the Corruption-Perceptions Index of *Transparency International* . In column 1 we replicate the benchmark specification using the core set of 18 events occurring during the embargo period. In columns 2 and 3, we use a broader set of 35 events occurring during the embargo period. This broad definition includes some events that we do not categorize as sufficiently unexpected or sufficiently important to be included in our core set of events. The measures of event importance and of event surprise are based on the number of news stories containing the country name in the days surrounding the event. The event importance is the average daily number of news hits in the day of and the day after the event. The event surprise is the ratio of the event importance and the average daily number of news hits in the four days preceding the event. In column 4 we use the subset of broad events with event surprise >=2 and event importance >=10. In column 5, we use the subset of broad events with event surprise >=3 and event importance >=20. Robust standard errors clustered by date in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

close to the estimates with the core events (column 1). The fact that the estimates are largest using the core sample of events suggests that the qualitative information used to choose the core events is informative.

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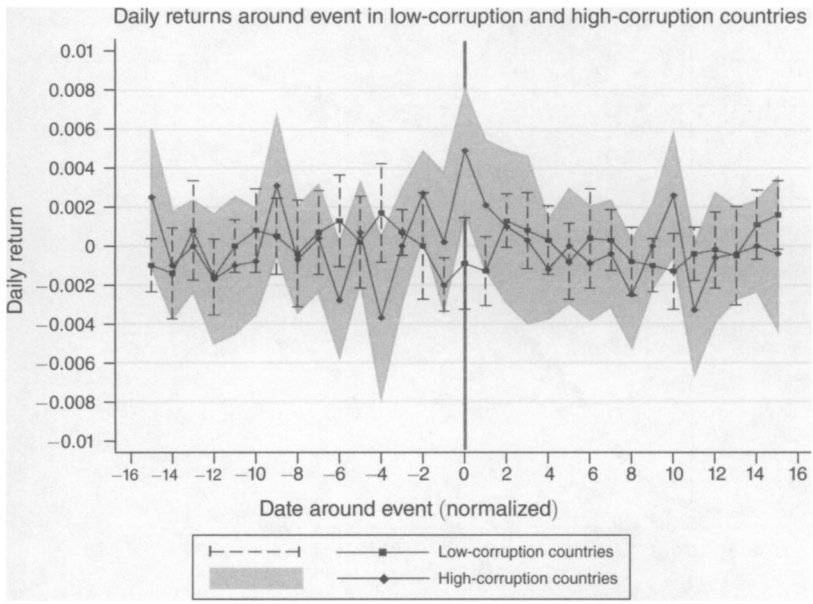

FIGURE 3. ABNORMAL RETURNS AROUND THE EVENT DATES

Note: Figure 3 displays average 3-day abnormal cumulative returns and 95 percent confidence bars separately for companies in high- and low-corruption countries in the 30 trading days around the event date.

E. *Timing*

We investigate the timing of the stock price reactions including a set of dummies for 30 trading days around the event date (15 before and 15 after the event):

$$(3) \qquad e_{i,t} = \alpha + \sum_{j=-15}^{+15} \gamma_j Emb_{t+j}(1 - D_i) + \alpha_D D_i + \sum_{j=-15}^{+15} \gamma_j^D Emb_t\, D_i + \eta_{i,t}.$$

Figure 3 displays the estimated coefficients $\hat{\gamma}_{-j}$ for low-corruption countries (square symbol and dashed line for 95 percent confidence intervals) and $\hat{\gamma}_{-j}^{D}$ for high corruption countries (diamond symbol and shaded for 95 percent confidence intervals). For example, point 2 on the x axis refers to returns two days after the event. As expected, the largest differences between the two return series (and the only statistically significant one) occurs around the event date.

F. *Placebos*

A possible concern is that our results could be due to an omitted variable inducing a correlation between the events and stock returns. While it is not clear why the omitted variables would produce a differential effect for companies in high-corruption markets, we address this concern directly by presenting two falsification tests, the first based on leads and lags, the second on placebo industries. In the first, we estimate a series of placebo regressions as in equation (2) with, as dependent variable, 3-day future abnormal stock returns $e_{i,s}^{(-1,1)}$, with $s = t + 3, t + 4, \ldots, t + 200$; similarly, we estimate

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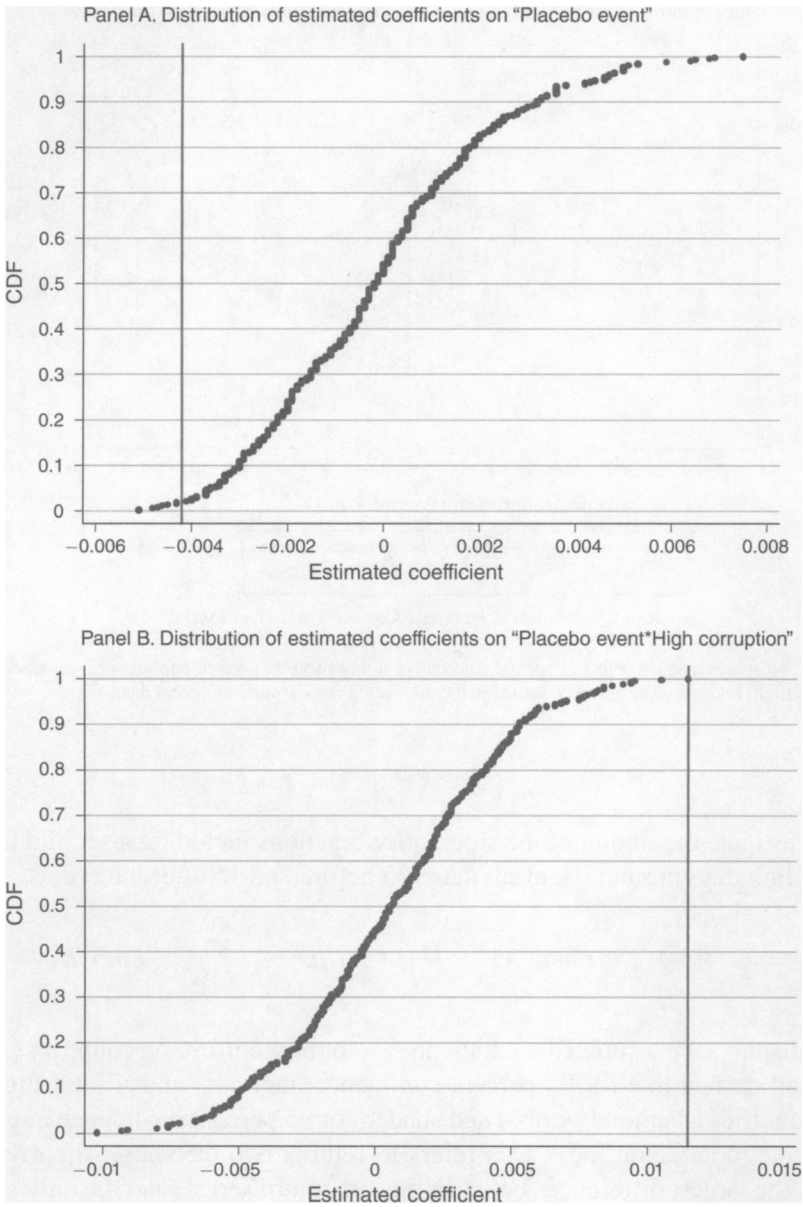

FIGURE 4

Notes: Panel A and panel B display the empirical c.d.f. of the placebo estimates γ and γ^D from a set of regressions that use as dependent variables 3-day future abnormal stock returns $t+3,\ldots,t+200$ and lagged past returns $t-3,\ldots,t-200$. The vertical line indicates our benchmark estimates from column 1 of Table 1.

placebo regressions for lagged past returns, with $s = t-3, t-4, \ldots, t-200$, for a total of 396 placebo regressions. Figure 4, panels A and B show the empirical c.d.f. of the placebo estimates γ and γ^D, respectively, compared to the benchmark estimates from column 1 of Table 1 (the vertical lines). In the top panel, 7 out of 396 coefficients, i.e., 1.8 percent, are smaller or equal to our estimate $\hat{\gamma} = -0.0042$. In

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the bottom panel, only 1 out of 396 coefficients, i.e., 0.2 percent, is greater or equal to our estimate $\hat{\gamma}^D = 0.0115$. These results increase our confidence that our estimated effects are not spurious.

Our second placebo treatment is based on industry classifications. We replicate specification (2) with a different dependent variable, namely the 3-day return $r_{i,t}^{(-1,1)}$ around the event for the stock market index of the market in which each company is traded. Since arms-producing companies are a small share of the stock market capitalization, this tests that war events do not affect stock valuations in sectors other than arms production, like the food, engineering, and service sectors. Our estimated regression (with standard errors in parenthesis) is:

$$r_{m(i),t}^{(-1,1)} = \underset{(0.0002)}{0.0012} + \underset{(0.0026)}{0.0001}\, Emb_t - \underset{(0.0002)}{0.0004}\, D_i + \underset{(0.0032)}{0.0003}\, Emb_t\, D_i.$$

The lack of significant effect of the war events suggests that our results are not driven by unobserved shocks to the stock markets where the arms-producing companies are traded.

G. *Firm Characteristics*

In Table 4 we estimate how the event returns depend on firm size and type of arms produced. We split the sample into small and large firms, defining as small firms those in the bottom quartile of annual revenue (in US dollars) in any given year, and the remaining firms as large. We find that both the response of low-corruption countries (γ) and the differential response of high-corruption countries (γ^D) are substantially higher (in absolute value) for small firms. Company size therefore does not explain the results, though it affects them: smaller firms are more likely to display significant event returns, since the profits from these trades are likely to be a larger share of the balance sheets.

Next, we estimate the event returns separately depending on the type of weapons produced, i.e., for companies with SIC codes in the range 3482–3484, and 3489 (small arms and ammunitions, column 3), 3761, 3764, and 3769 (missiles, column 4), 3795 (tanks, column 5), and 2892 (explosives, column 6). The samples in columns 3 through 6 are not mutually exclusive, since we include companies with at least one of the eight SIC codes in the required range. The estimate for γ_D is positive in all types of arms, and it is marginally significant for companies producing small arms and ammunitions, a category likely to be heavily used in these conflicts. Beyond small arms and ammunitions, the estimate for γ_D is largest for consumable arms—explosives and missiles.

IV. Interpretation

Our interpretation of these results is that the abnormal event returns are evidence of profits due to legal and illegal arms trade, and that companies located in countries with higher corruption are more likely to violate the arms embargo.

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TABLE 4—STOCK MARKET REACTION BY FIRM CHARACTERISTICS (*firm size and type of arms*)

Dependent variable	Abnormal 3-day stock return (−1, 1)					
	Firm size		Type of arms produced			
Firm characteristics	Small firms (1)	Large firms (2)	Small arms & Ammunitions (3)	Missiles (4)	Tanks (5)	Explosives (6)
Event during embargo	−0.01	−0.0024	−0.0048	−0.0057	−0.0049	−0.0077
(1 = increase war, −1 = decrease,	(0.003)***	(0.0018)	(0.0035)	(0.0036)	(0.0042)	(0.0049)
0 = no event)						
Event during embargo*	0.02	0.0075	0.0099	0.029	0.0046	0.0137
(high-corruption country)	(0.0052)***	(0.0042)*	(0.0056)*	(0.0186)	(0.0046)	(0.0084)
Indicator for high-corruption	−0.0003	0	−0.0001	0.0004	0.0001	−0.0002
country	(0.0004)	(0.0002)	(0.0004)	(0.0007)	(0.0005)	(0.0004)
Constant	−0.0001	−0.0001	−0.0002	−0.0001	−0.0001	−0.0001
	(0.0003)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Same of companies	All	All	Worldscope	Worldscope	Worldscope	Worldscope
Observations	132,713	355,909	133,323	114,005	43,061	58,399

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985–2005. The dependent variable is the abnormal 3-day cumulative return. In columns 1–2, we estimate separately the results for small and large firms. We define as small firms those in the bottom quartile of annual revenue (in US dollars) in a given year. The remaining firms are classified as large. In columns 3–6, the sample includes only companies with one of the 8 SIC codes in the range of a particular type of arms, that is, 3482–3484, and 3489 for small arms and ammunitions, 3761, 3764, and 3769 for missiles, 3795 for tanks, and 2892 for explosives. The variable Event During the Embargo takes value 1 if on day t, during the embargo period, an event increases the conflict, takes value −1 if, during the embargo period, an event decreases the conflict, and takes value 0 otherwise. The variable High-Corruption Country is an indicator variable indicating companies head-quartered in countries with above-median corruption according to the Corruption-Perceptions Index of *Transparency International*. Robust standard errors clustered by date in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

This interpretation is subject to several caveats. First, these findings show an average effect across companies: they do not imply that all, or even most, arms companies in high-corruption countries trade illegally, nor do they rule out that some companies in low-corruption countries trade illegally.

Second, our paper is based on the assumption of well-informed investors. Indeed, investors are often informed even when the general public is not (e.g., Michael T. Maloney and J. Harold Mulherin 2003). It is however possible that there is no illegal arms trading, but the marginal investor is mis-informed, and reacts as if there were trade. Alternatively, it is possible that countries differ in the extent to which investors are informed about "illegal" dealings of listed companies, and that our results reflect this differential. While we cannot test for (differences in) investor rationality and information, it is plausible that investors close to the top management in any country would know if illegal arms trade takes place, and they would have strong incentives to trade in the days of conflict events. In addition, while direct evidence from actual trade flows would be preferable to indirect, return-based evidence, the weak reporting requirements on arms trade due to national security concerns make a direct test based on arms imports and exports very unreliable. Hence, investor-based evidence is likely the best source of systematic information on illegal arms trade.

Third, our interpretation does not imply that arms companies violate the embargo *directly*. It is possible that the trade of arms flows through an intermediary, in a

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way that still leaves the original company a substantial profit margin. However, even in this latter case the original company may bear legal or reputation costs. The US law, for example, is explicit about the legal responsibility for re-exports in its International Traffic in Arms Regulations (ITAR):

> the country designated as the country of ultimate destination on an application for an export licence [...] must be the country of ultimate end-use. [...] Exporters must ascertain the specific end-use and end-user prior to submitting an application to the Office of Munitions Control or claiming an exemption under this subchapter. End-use must be confirmed and should not be assumed. (Section 123.9)[7]

A corollary of this point is that the effect we find may partly reflect differences in the distribution of rents along the supply chain, if firms with high reputation costs rely more on intermediaries during embargoes (and retain relatively less profits) than firms in high corruption countries.

In what follows we discuss some alternative interpretations of our findings, and present some evidence to assess them.

Depletion of Old Arms.—A first alternative interpretation is that the event returns indicate increases in the world demand for arms due to depletion of old stocks. Even if the countries under embargo do not import new weapons but just deplete existing ones, this will generate a positive demand shift for weapon companies in the future, when the depleted stock will have to be replenished. According to this interpretation, conflict events should have a significant effect on returns both under embargo and outside the embargo. Also, the effects should not differ across companies in low- and high-corruption countries.

Composition of Arms Production.—The difference in results between companies with low and high cost of embargo violation may be due to differences in the type of arms they produce. Companies in high-cost countries may be less likely to produce arms used in developing countries, and hence respond less to conflict events in these countries. This, however, does not explain why companies in high-cost countries respond negatively to increases in conflict. Also, this interpretation predicts that companies in high and low-cost countries should not respond differentially to events outside the embargo.

Input and Product Mix.—An event may cause an increase in demand not only for the weapons produced by low-cost companies, but also for the inputs used in the production of arms in high-cost companies. Even if these latter companies do not trade in the conflict zone, their returns may respond negatively, as we observe empirically. Again, this would predict a similar finding for events outside the embargo.

[7] The reputational costs from re-export can also be substantial. A recent Amnesty International report names EU and US companies that produced components for military helicopters that could allegedly be exported from India to Myanmar, a country covered by EU and US sanctions (Amnesty International 2007).

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Regional Instability.—The impact of events under the embargo may be due to their destabilizing impact on neighboring countries. The impact on profits could then be due not to illegal arms trades, but to legal arms trades to neighboring countries. However, such destabilizing effect should be present also when the conflict event occurs outside an embargo, unless one posits that events inside the embargo are more significant.

These alternative interpretations make one common prediction: the differential impact of conflict events across companies with high and low cost of embargo violation should hold also for events occurring outside the embargo. We now provide evidence on this point and estimate the following augmented version of equation (2):

$$(4) \quad e_{i,t}^{(-1,1)} = \alpha + \alpha^D D_i + \gamma Emb_t + \gamma^D Emb_t D_i + \delta Out_t + \delta^D Out_t D_i + \eta_{i,t}.$$

The variable Out_t equals 1 if an event increasing conflict occurs outside embargo at time t, -1 if an event decreasing conflict occurs outside embargo at time t, and 0 otherwise.

We construct the variable Out_t using two sets of events: (i) 14 events occurring outside the embargo period for the same eight countries in which embargoes were eventually imposed (Table A2 in the Web Appendix); (ii) 19 events in countries which experienced conflict but not an arms embargo: Algeria, Haiti, Venezuela, Tajikistan, Central African Republic, Ivory Coast, Democratic Republic of Congo, and Togo.[8] We denote this second set of events as Events in countries without embargo.

The results are displayed in Table 5. In column 1 we estimate specification (4) without distinguishing between high- and low-cost companies (that is, we set $\alpha^D = \gamma^D = \delta^D = 0$). We find no significant effect for events either during the embargo or outside. We then allow for a differential effect for companies high corruption countries (column 2) and for companies in low arms transparency countries (column 3). We find no difference between the two types of companies in the response to events outside the embargo. This helps us rule out that the above alternative interpretations alone can account for the effects we find.

V. Model and Calibration

The event returns can be used to compute, under a set of assumptions, the implied profits from legal and illegal arms trading. In order to do so, we present a model that formalizes the framework presented in Section I, and we calibrate this model to the data.

A. *Model*

We consider an infinite-period model in which in every period arms producing firms face two sources of uncertainty: the state of the world—Embargo E and Non-Embargo N, and, within each state, the demand for arms α.

[8] While Haiti was subject to arms embargo in 1993 and 1994, the events we identify occur outside this period.

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TABLE 5—STOCK MARKET REACTION TO EVENTS OUTSIDE THE EMBARGO

	Abnormal 3-day stock return (−1, 1)		
Dependent variable	(1)	(2)	(3)
Event during embargo (1 = increase war, −1 = decrease, 0 = no event)	−0.001	−0.0042	−0.0043
	(0.0015)	(0.0018)**	(0.0020)**
Event during embargo* (low cost of embargo violation)		0.0115	0.0114
		(0.0041)***	(0.0042)***
Event outside embargo (1 = increase war, −1 = decrease, 0 = no event)	0.0001	0.0003	0
	(0.0020)	(0.0026)	(0.0025)
Event outside embargo* (low cost of embargo violation)		−0.0008	0.0005
		(0.0038)	(0.0031)
Event in countries without embargo (1 = increase war, −1 = decrease, 0 = no event)	0.0025	0.0023	0.0023
	(0.0018)	(0.0022)	(0.0023)
Event in countries without embargo* (low cost of embargo violation)		0.0008	0.0001
		(0.0029)	(0.0030)
Proxy for low cost of embargo violation—indicator variable		−0.0001	−0.0001
		(0.0002)	(0.0002)
Constant	−0.0001	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)
Proxy measure—indicator variable for low cost of embargo violation		High Corruption	Low Transparency of Arms Trade
Observations	492,569	492,569	475,128

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985–2005. The dependent variable is the abnormal 3-day cumulative return. The market correction is computed on the calendar year previous to the trading day. The variable Event During the Embargo takes value 1 if on day t, during the embargo period, an event increases the conflict, takes value −1 if, during the embargo period, an event decreases the conflict, and takes value 0 otherwise. The variable High-Corruption Country is an indicator variable indicating companies head-quartered in countries with above-median corruption according to the Corruption-Perceptions Index of *Transparency International*. The variable Low-Transparency of Arms Trade Robust is an indicator variable indicating companies head-quartered in countries with below-median transparency in arms trade according to the *Small Arms Survey*. Robust standard errors clustered by date in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

We model the transition probability between states E and N as a Markov chain. Denote with $P_{i,j}(\alpha_t)$ the probability of transitioning from state $i \in \{E, N\}$ at time t to state $j \in \{E, N\}$ at $t + 1$. The probability of embargo in the future depends positively on the current state of hostilities, that is, $P'_{E,E}(\alpha_t) > 0$ and $P'_{N,E}(\alpha_t) > 0$. We also assume a form of state dependence: $P_{E,E}(\alpha_t) > P_{N,E}(\alpha_t)$ for all α_t. For given hostilities α_t, the probability of an embargo next period is higher if a country is currently under embargo.

The demand for arms, drawn in each period t from c.d.f. F, depends on the state at time t: the demand in the Embargo state first-order stochastically dominates the demand in the Non-Embargo state: $F_E(\alpha_t) \leq F_N(\alpha_t)$ for all α_t. We make the simplifying assumption that, conditional on the state, the demand for arms α_t is i.i.d. over time.[9]

[9] If we allowed for a positive correlation of demand across periods, increases in demand α_t would have the additional effect of increasing future demand and hence the value V for all firms.

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The continuation payoffs for the Embargo state V_E and the Non-Embargo state V_N are:

$$(5) \qquad V_j(\alpha_t) = \pi_j(\alpha_t) + \delta[P_{j,E}(\alpha_t)V_E + (1 - P_{j,E}(\alpha_t))V_N] \quad j \in \{E, N\}.$$

The value of the firm in state j is the sum of current profits π_j and the (discounted) expected continuation payoff, which itself depends on the realized state in period $t + 1$. We model profits π_E and π_N below. The expected continuation payoffs V_E and V_N are defined as $V_E = \int V_E(\alpha)\, dF_E(\alpha)$ and $V_N = \int V_N(\alpha)\, dF_N(\alpha)$. To solve for the unconditional continuation payoffs V_E and V_N, we integrate the expression for $V_E(\alpha_t)$ in (5) with respect to dF_E and the expression for $V_N(\alpha_t)$ with respect to dF_N. We get

$$(6) \qquad V_j = E\pi_j + \delta[EP_{j,E}V_E + (1 - EP_{j,E})V_N] \quad j \in \{E, N\}$$

where we define the expected profits $E\pi_j = \int \pi_j(\alpha)dF_j(\alpha)$ and the expected probabilities of transition $EP_{j,E} = \int P_{j,E}(\alpha)\, dF_j(\alpha)$. Using (6) we obtain $V_E - V_N = (E\pi_E - E\pi_N)/[1 - \delta(EP_{E,E} - EP_{N,E})]$.

We then compute the derivatives of $V_E(\alpha_t)$ and $V_N(\alpha_t)$ with respect to the demand for weapons α_t. Differentiating (5) and substituting in the expression for $V_E - V_N$, we obtain

$$(7) \qquad \frac{\partial V_j(\alpha_t)}{\partial \alpha_t} = \pi_j'(\alpha_t) + \delta P_{j,E}'(\alpha_t)\frac{E\pi_E - E\pi_N}{1 - \delta(EP_{E,E} - EP_{N,E})} \quad j \in \{E, N\}.$$

A change in the demand for arms has two effects: (i) it alters current profits, as captured by the first term $\pi_j'(\alpha_t)$; (ii) it affects expected future profits through the probability of the Embargo state, as captured by the second term. The latter effect is positive for companies which are more profitable under embargo ($E\pi_E > E\pi_N$), and negative otherwise.

To evaluate these expressions, we derive predictions about $E\pi_E$, $E\pi_N$, $\pi_E'(\alpha_t)$, and $\pi_N'(\alpha_t)$, using a model of Cournot competition with barriers to entry, formalized in the Web Appendix. We consider two types of firms with identical demand and identical (linear) production costs, but different legal and reputational cost to selling arms in the Embargo state. This cost does not apply to sales in the Non-Embargo state. For the high-cost firms H, the legal and reputational cost is high enough that these firms do not sell arms in the Embargo state. For the low-cost firms L, instead, the cost is zero.[10] We also assume that, due to barriers to entry, at most N^H firms of the high-cost type and at most N^L firms of the low-cost type can enter the market.

As we show in the Web Appendix, in the Non-Embargo state, the profits for the two types of firms are the same: $\pi_N^H = \pi_N^L = \pi_N \geq 0$. In the Embargo state, high-cost firms do not sell and have $\pi_E^H = 0$, while low-cost firms earn profits that are higher than in the non-Embargo state ($\pi_E^L > \pi_N$). In addition, the model yields

[10] More generally, we can allow the cost of entry K^L to be positive, but smaller than K^H. This does not affect our Predictions as long as the entry cost is smaller than the expected profits under embargo $E\pi_E^L$.

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an expression for the derivative of profits with respect to the demand for arms: $\pi'(\alpha) = \pi(\alpha)/\alpha$.

We can thus obtain expressions for the change in company value in response to changes in the demand for arms occurring during the Embargo. For high-cost companies, the expression for $\partial V_E^H(\alpha_t)/\partial\alpha_t$ follows from (7). For low-cost companies, we derive the expression for $\partial V_E^L(\alpha_t)/\partial\alpha_t - \partial V_E^H(\alpha_t)/\partial\alpha$. These derivatives form the basis of Predictions 1 and 2 in Section I, and match the empirical tests in Section III.

In order to calibrate the model, we use a linear approximation to express the change in company value in response to discrete (as opposed to infinitesimal) changes in the demand for arms, i.e., $dV = (\partial V/\partial\alpha_t)\, d\alpha_t$. The expressions are for the Embargo state:

$$dV_E^H = -\delta(P'_{E,E}(\alpha_t)d\alpha_t)\frac{E\pi_N}{1-\delta(EP_{E,E}-EP_{N,E})} \leq 0; \tag{8}$$

$$dV_E^L - dV_E^H = \pi_E^L(\alpha_t)\frac{d\alpha_t}{\alpha_t} + \delta(P'_{E,E}(\alpha_t)d\alpha_t)\frac{E\pi_E^L}{1-\delta(EP_{E,E}-EP_{N,E})} > 0 \tag{9}$$

and for events in the Non-Embargo state:

$$dV_N^H = \pi_N(\alpha_t)\frac{d\alpha_t}{\alpha_t} - \delta(P'_{N,E}(\alpha_t)d\alpha_t)\frac{E\pi_N}{1-\delta(EP_{E,E}-EP_{N,E})} \gtrless 0. \tag{10}$$

$$dV_N^L - dV_N^H = \delta(P'_{N,E}(\alpha_t)d\alpha_t)\frac{E\pi_E^L}{1-\delta(EP_{E,E}-EP_{N,E})} > 0. \tag{11}$$

B. *Calibration*

We assume that the time periods t correspond to one year with a yearly discount factor $\delta = 0.95$. A key set of parameters for the calibrations are the yearly transition probabilities ($EP_{E,E}$ and $EP_{N,E}$) and the changes in transition probabilities induced by conflict events ($P'_{E,E}(\alpha_t)\, d\alpha_t$ and $P'_{N,E}(\alpha_t)\, d\alpha_t$).

We estimate these parameters using the broad sample of countries used for Table 5 over the period 1985–2006. We estimate $EP_{E,E}$ (the probability of transition from Embargo to Embargo) as the fraction of countries under arms embargo in (at least a part of) year t that is still under arms embargo in year $t+1$; we obtain $E\hat{P}_{E,E} = 0.928$, indicating a high likelihood that the embargo will be persistent. Similarly, we estimate $EP_{N,E}$ as the fraction of countries that are not under arms embargo in year t but that are under arms embargo in year $t+1$; we obtain $E\hat{P}_{N,E} = 0.043$, consistent with a low baseline probability of embargo imposition.

Turning to $P'_{E,E}(\alpha_t)d\alpha_t$, we estimate how the fraction of countries under embargo in year t is affected if there was a conflict event during the embargo in the country

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in year $t-1$. To illustrate this, the fraction of countries under embargo is 0.778 if one of the 9 events diminishing hostilities occurred in year $t-1$. The fraction is higher, 0.941, for the 68 country-year observations with no events during the embargo and yet higher, 1, for the 7 events increasing the hostilities. Formally, to evaluate $P'_{E,E}(\alpha_t)\,d\alpha$ we estimate the probit $P(d_{\text{Embargo},j,t}=1)=\Phi(\alpha+\gamma Emb_{j,t-1})$, where $d_{\text{Embargo},j,t}$ is an indicator for embargo in country j and year t, and $Emb_{j,t-1}$ equals 1 if an event increasing conflict occurs during the embargo in country j and year $t-1$, -1 if an event decreasing conflict occurs during the embargo in country j and year $t-1$, and 0 otherwise. The marginal impact of $Emb_{j,t-1}$ is 0.100 (standard error 0.055): on average an event during the embargo affects the probability of persistence of embargo by 10 percentage points. To estimate $P'_{N,E}(\alpha_t)d\alpha_t$, we repeat a similar exercise for events outside the embargo and find that on average a conflict-increasing event outside the embargo increases the probability of embargo in the next period by 0.063 (standard error 0.032). Embargo imposition and renewal are responsive to hostilities, consistent with our model.

The final parameter needed for the calibration is $d\alpha_t/\alpha_t$, the percent increase in demand for arms induced by a conflict event. Since we do not have any measure of this parameter, we consider a benchmark calibration of $d\alpha_t/\alpha_t=0.4$, that is, events on average cause a 40 percent change in demand for arms, and an alternative lower calibration of $d\alpha_t/\alpha_t=0.2$.

Given these parameters, and imposing $\pi_N(\alpha_t)=E\pi_N$ and $\pi_E^L(\alpha_t)=E\pi_E^L$, expressions (8) and (9) reduce to $dV_E^H=-0.594\,E\pi_N$ and $dV_E^L-dV_E^H=0.994\,E\pi_E^L$ ($0.794\,E\pi_E^L$ in the alternative calibration). The estimated $d\hat{V}_E^H$ equals the event return -0.0042 for the companies in low-corruption countries (column 1 of Table 1), multiplied by the market capitalization, which we measured as the median among the companies in low-corruption countries (in 1984 dollars), \$408m: $d\hat{V}_E^H(\alpha_t)=-\$1.71m$. The estimated expected yearly profit in the Non-Embargo state $E\hat{\pi}_N$ is $-\$1.71m/(-0.594)=\$2.88m$. (This estimate does not depend on $d\alpha_t/\alpha_t$, and is thus the same in the alternative calibration.) According to this calibration, hence, the median company in a low-corruption country reaps on average 2.88 million dollars of profits yearly for arms trade to a country in our sample during a non-embargo period.

Similarly, we calibrate the profits in the Embargo state. The estimated differential change in value $d\hat{V}_E^L-d\hat{V}_E^H$ equals the return 0.0115 multiplied by the median market capitalization among the companies in high-corruption countries, \$150m: $d\hat{V}_E^L-d\hat{V}_E^H=\$1.72m$. This implies $E\hat{\pi}_E^L=\$1.73m$, that is, the median company in a high-corruption country earns on average 1.73 million dollars of profits for arms trade in defiance of an arms embargo. (This figure is \$2.17m under the alternative calibration for $d\alpha_t/\alpha_t$.) Notice that the estimated profits under embargo are smaller than the estimated profits outside embargo because the market capitalization of companies in high-corruption countries is smaller.

Overall, these estimates imply yearly profits in the order of hundreds of millions of dollars for the worldwide sale of arms from traded companies to each of the eight countries in our sample. These are large numbers, but not inconceivable for economies with GDPs in the order of (tens of) billions of dollars, and with large defense expenditure.

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VI. Individual Detection and External Validation

A. *Detecting Individual Violations*

In this section we consider each company and event in isolation, and identify companies that the returns suggest may be embargo violators. A caveat is that, since we only observe a small number of events, this detection procedure remains subject to substantial error, and hence we do not single out individual firms. Nevertheless, it suggests a possible forensic application.

To estimate event reactions, we use cumulative abnormal 3-day returns $e_{i,t}^{(-1,1)} = e_{i,t-1} + e_{i,t} + e_{i,t+1}$ computed using the market model (1) with an estimation window of 100 trading days. For each company-event observation, we test the null that the event does not affect the abnormal returns of the company. We use the parametric tests under the assumption of joint normality of John Y. Campbell, Andrew W. Lo, and A. Craig MacKinlay (1997, page 160) with a 10 percent significance threshold.

We isolate three types of reactions. The first type, denoted as *Illegal_React*, indicates companies whose return significantly increases (decreases) when conflict increases (decreases) during the embargo—a behavior consistent with sales of arms in violation of the embargo (Prediction 1(i)). The second type of reaction, denoted as *Legal_React*, occurs when a return is significantly negative (positive) in correspondence of events that increase (decrease) conflict intensity during an embargo—consistent with a company expecting to sell arms legally after the embargo is lifted (Prediction 1(ii)). The third type, labelled as *Outside_React*, indicates companies that display a statistically significant positive (negative) return when conflict increases (decreases) *outside* the embargo, consistent with the company selling arms to the country (Prediction 2 (ii)). Table A7 in the Web Appendix provides an example of our categorization. Out of 145 companies, 64 never display a significant reaction consistent with illegal behavior, 32 display it once, 35 twice, 10 three times, 3 companies have four instances of *Illegal_React*, and one company has seven occurrences of *Illegal_React*.

Because isolated reactions may be due to chance, we also look for multiple reactions for a company *within* a conflict. We define a chain of illegal reactions as a sequence of at least two statistically significant reactions for the same conflict, either *Outside_React* and *Illegal_React*, or a sequence of multiple *Illegal_React* reactions. We find 23 company-country pairs with a chain of illegal reactions, with two companies displaying chains in two embargoes and one company in three embargoes. The country with the greatest number of violations is Liberia, where 8 companies displayed a chain of reactions. Sudan follows with 7 chains, Sierra Leone with 4 and Angola with 3. Regarding the location of the companies displaying these chains, 14 of them are in low corruption countries and 9 in high corruption ones.[11]

[11] This is not inconsistent with the results in Section III since the large majority of companies in our data are in low-corruption countries. In Table A8 of the Web Appendix we normalize these results taking into account the number of possible combinations.

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This clarifies that our earlier findings did not imply that only companies from high-corruption countries were detected as violating embargoes.

B. *External Validation*

Using these results, we compare the "detected violations" to two indirect sources of outside evidence on legal and illegal arms trade. A first source is the United Nations reports on monitoring arms embargoes: the Reports of Panel of Experts, the Reports of the Monitoring Groups, and Selected Documents.[12] The violators named in the reports are mostly brokers and intermediaries, and no traded company in our sample is mentioned in these reports, implying that we cannot use these reports to directly validate the detection of individual companies.[13] Still, as a measure of the seriousness of the violations in a conflict, we use the number of UN reports devoted to embargo enforcement. The first such measure, $MGPE_j$, is the number of reports by the Panel of Experts and by the Monitoring Group concerning country j, divided by the number of years of the embargo, and it varies from a minimum of 0 (Ethiopia, Rwanda, and Yugoslavia) to a maximum of 3 (Sudan). The second, SEL_j, is the number of Selected Documents reports concerning country j, divided by the number of years of the embargo, and varies from a minimum of 0 (Rwanda, Somalia, and Sudan) to a maximum of 3 (Liberia). The information refers to the years of embargoes for which information is available on the UN Web site.

In panel A of Table 6 we test if, in conflicts with higher incidence of UN reports, companies are more likely to be detected as reacting to the conflict events. We estimate

$$Illegal_React_{i,t} = \alpha + \alpha_D MGPE_j + \eta_{i,t}$$

in column 1 and a similar specification in column 2 using the incidence of Selected Documents SEL_j as independent variable. Using either measure, a higher incidence of UN reports increases the likelihood of an illegal reaction. The result is however significant at the 5 percent level only for the *MGPE* variable. In panel B, we find that the incidence of Panel of Experts and Monitoring Group Reports significantly lowers the detection of legal reactions, while the incidence of Selected Documents has no effect. The return-based detection and the measures based on the number of UN reports are consistent, though we should point out that the incidence of UN reports is a rough proxy for the severity of violations.

In a second exercise, we take advantage of information spread on the Internet and use counts of Google hits to provide a rough measure of the association of companies with embargoes, with arms trading, and with a specific conflict. We follow a methodology similar to the one Albert Saiz and Uri Simonsohn (2010) used to measure corruption. For each company i, we record four counts of Google hits: (i) n_i for

[12] The Selected Documents include for example letters written by local government authorities regarding allegations of embargo violation, but also generic communications on administrative procedures.

[13] We interpret this as evidence that detection of trades by larger companies is more difficult, and perhaps the political will for detection weaker.

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TABLE 6—EXTERNAL VALIDATION USING UN REPORTS AND GOOGLE HITS

Independent variable (measure of external validation)	Incidence of UN reports by monitoring group and panel of experts in conflict j (1)	Incidence of UN selected documents in conflict j (2)	Top 10 percent of Google hits using company name and "embargo" (3)	Top 10 percent of Google hits using company name and "arms" (4)	Top 10 percent of Google hits using company name and conflict name (5)
Panel A. Dependent variable: 1 if illegal reaction; 0 otherwise					
OLS coefficients					
Incidence of UN reports on embargo violation by conflict	0.0262	0.0138			
	(0.0093)**	(0.0093)			
Indicator for high arms-related Google hits by company			0.0516	0.0449	0.0313
			(0.0226)**	(0.0323)	(0.0243)
Constant	0.0582	0.0625	0.0763	0.0775	0.078
	(0.0140)***	(0.0159)***	(0.0102)***	(0.0115)***	(0.0115)***
Observations	1,838	1,838	1,811	1,811	1,811
Panel B. Dependent variable: 1 if legal reaction; 0 otherwise					
OLS coefficients					
Incidence of UN reports on embargo violation by conflict	−0.0162	0.0029			
	(0.0064)**	(0.0076)			
Indicator for high arms-related Google hits by company			0.0202	−0.0115	0.0129
			(0.0314)	(0.0274)	(0.021)
Constant	0.1068	0.0878	0.0903	0.0931	0.0909
	(0.0084)***	(0.0111)***	(0.0071)***	(0.0084)***	(0.0076)***
Observations	1,838	1,838	1,811	1,811	1,811

Notes: An observation in the OLS regressions is an event day for one of the 153 arms-producing companies in the years 1985–2005. Only events occurring inside the embargo are included in this Table. The dependent variable in panel A is equal to 1 if the event is of the type "*Illegal_React*" and 0 otherwise. "*Illegal_React*" denotes the case in which the return significantly increases (decreases) at the 10 percent level when conflict increases (decreases) during the embargo. The dependent variable in panel B is equal to 1 if the event is of the type "*Legal_React*" and 0 otherwise. "*Legal_Reac*t" denotes the case in which the return significantly decreases (increases) at the 10 percent level when conflict increases (decreases) during the embargo. In column 1 the regressor is the total number of Reports of the Monitoring Group and of the Panel of Experts concerning country j, divided by the number of years of the embargo. In column 2 the regressor is the number of Selected Documents concerning country j, divided by the number of years of the embargo. In column 3 the regressor is constructed using the ratio of the number of Google hits for searches of the company name AND "embargo," divided by the number of Google hits for the company name (if the latter hits are at least 100); the regressor is an indicator variable for the top 10 percent of the hits across companies. In column 4 the regressor is similarly constructed, except that the numerator of the ratio is the number of hits for the company name AND "arms." In column 5 the regressor is similarly constructed, except that the numerator of the ratio is the number of hits for the company name AND the name of the conflict to which the event refers. Robust standard errors are clustered by event date.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

searches of the company name; (ii) emb_i for searches of the company name AND "embargo"; (iii) arm_i for searches of the company name AND "arms"; (iv) $confl_{i,j}$ for searches of the company name AND the name of the country in conflict. We then compute the ratios of emb_i, arm_i, and $confl_{i,j}$ to the total number of hits n_i to obtain a variable that is, to a first approximation, independent of the scale of n_i.[14] Among the companies with at least 100 hits ($n_i > 100$), we define an indicator variable for

[14] Two full searches were conducted by two independent teams of research assistants; we take the average of the fractions computed according to each team's counts.

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the companies (or company-country combinations in the case of $confl_{i,j}$) in the top 10 percent. (We do not use the continuous variable because it is highly skewed.) Companies in the top-10 percent of arms-related Google counts are qualitatively more likely to display what we detect as illegal reactions (columns 3–5 of Table 6). The result is statistically significant for the counts using the word "Embargo", the wording most closely tied to embargo violations. We do not find any significant evidence for the detection of legal reactions (panel B). These findings provide some external validation, albeit an indirect one, since we cannot examine the Internet content directly given the high number of the searches.

Finally, as a last form of validation we considered using information from ComTrade on bilateral flows of goods categorized as arms. However, the ComTrade documentation warns that, due to specific provisions related to national security, the coverage of goods for military use is often not captured by customs authorities, and as such the data is less reliable.

VII. Conclusion

Can stock prices help to detect illegal transactions? We have proposed a method to detect illegal arms trade based on event returns for arms-producing companies.

While in this paper we have focused on detection of illegal arms trades, the approach used in this paper has broader applications. For example, it could be used to detect violators of other types of legislation. Unlike in most event studies that examine changes in legislation, the idea is to examine sudden events that affect the enforcement of existing legislation. We hope that follow-up work will pursue other examples of returns-based detection.

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