Detecting Illegal Arms Trade*

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Abstract

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.

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1 Introduction

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 3.8 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.

The case-by-case evidence provided 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. How effective are arms embargoes in preventing illicit trade in arms? Which countries illegally export weapons in areas of civil conflict? Which types of companies are involved? A better answer to these questions is a pre-condition for effective policies.

In this paper, we propose a method that can 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 examine the stock returns of companies producing weapons in a window around these events. We identify weapon-making companies using the SIC code information in the Datastream-Worldscope data set, supplemented with a list of the top-100 weapons companies (Dunne and Surry, 2006). For these 153 companies, we consider the abnormal returns in the 3 days surrounding the events. If the companies are not trading or trading legally, an event increasing the hostilities should not affect stock prices or should affect them adversely, since it delays the removal of the embargo and hence the re-establishment of legal sales. Conversely, if the companies are trading illegally, the event should increase stock prices, since it increases the demand for illegal weapons.

In Section 4 we present the results on average event returns. Over the whole sample, we find no significant stock response to the events. The aggregate null effect, however, may mask heterogeneity in the event returns. Hence, 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 where corruption is more commonplace and where transparency of arms sales is lower. Further, we expect that lack of membership in a large organization like the OECD, lower press freedom, higher bribe-paying, and lower participation by minority shareholders would also lower the cost of illegal arms trading.

We find support for these predictions. Over the subset of companies head-quartered in low-corruption countries, an event increasing conflict is associated with a decrease 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. These findings suggest 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 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.

We examine alternative specifications, as well as the effects by sub-groups of companies. 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 an outlier. 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 our identification of the event date is plausibly accurate. We use placebo specifications on stock returns in the period before and after the event to argue that the effect is unlikely to be spurious. We also consider the impact of two firm characteristics, 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. Across types of arms produced, the result is generally found in all categories, but is largest for companies producing small arms and ammunitions, missiles, and explosives.

In Section 5, we present a calibration of the findings and interpretations. Our benchmark interpretation is based on a simple model of conflict, embargo imposition, and firm competition with barriers to entry, presented in Section 2. 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, 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. 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.

The model also makes a second set of predictions. Events increasing hostility in the non-Embargo state have an ambiguous effect on high-cost firms, since they increase contemporaneous profits but they hurt future profits through the increased likelihood of embargo imposition. We test this prediction using events that occur before, or after, the imposition of an embargo, as well as events in countries experiencing hostilities but no arms embargo. We find that events increasing conflict have a small positive (not significant) effect on the returns of high-cost (low-corruption) companies, with no difference between low- and high-corruption countries. These findings are consistent with a calibrated version of the model.

We also consider alternative interpretations. The stock return effect could be due to an increase in the worldwide demand for weapons. However, this interpretation does not explain the difference in event returns for events under embargo and for events not under embargo. The difference in the event returns between low- and high-cost companies could be due to differences in arms produced: low-cost companies may produce more of the weapons that are demanded in the embargoes countries. This does not explain, though, why the returns of high-cost companies respond negatively to increases in conflict, nor why increases in demand for arms by the same countries before or after the embargo does not generate similar reactions. We also discuss interpretations based on regional instability, product mix, and investor information. Finally, we cannot distinguish between direct violations of an embargo and arms sales to intermediaries which themselves violate the embargo. We note, however, that indirect violations, like direct ones, can also have legal and reputational costs for the companies exporting arms.

In Section 6 we consider whether it is possible to detect *individual* firms violating the embargo. We conduct separate event studies for each company-event pair, and isolate events in which the abnormal returns of a company are statistically different from zero, in a direction consistent with illegal arms trade. To reduce the number of false positives, we 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.

In Section 7, we use external sources to validate the detection results. 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 (Bondi, 2004; Wood and Peleman, 2006; 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. We offer a systematic quantitative evaluation of the extent of embargo violations, as well as insights on the types of arms producers (not just brokers) involved. 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 (Collier and Hoeffler, 1998; Miguel, Satianath and Sergenti, 2003; Montalvo and Reynal-Querol, 2005). We suggest a methodology that exploits investor information to measure the illegal trade of arms, a (proximate) determinant of conflict.

The paper also relates to the event studies of the effect of political events on stock prices. These studies have explored the economic effects of political connections (Roberts, 1990; Fisman, 2001), the party in power (Jayachandran, 2006; Wolfers and Zitzewitz, 2007), legislative decisions (Delaloye, Habib and Ziegler, 2006), and conflict (Abadie and Gardeazabal, 2003; Schneider and Troeger, 2006; Guidolin and La Ferrara, 2007; Dube, Kaplan and Naidu, 2008). A difference relative to this literature is that we do not know ex ante which types of companies are affected by the event and use the stock response to determine it.

Finally, our paper is also related to the literature on forensic economics. Papers in this literature use large data sets to detect patterns of cheating and corruption. Examples include detecting teacher cheating (Jacob and Levitt, 2003), tax evasion (Fisman and Wei, 2004; Marion and Muehlegger, 2008), and corruption in sports (Duggan and Levitt, 2002; Wolfers, 2006). Most closely related is Hsieh and Moretti (2006), who use time-series changes in oil prices to infer whether the Iraq regime violated the oil-for-food program. Compared to these papers, we rely on investor information, rather than on behavior of the agents committing the crime. 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.

2 Model

We present a simple model of conflict, embargo imposition, and firm value. The model provides testable predictions on the impact of events affecting the demand for arms on firm value. We distinguish between periods of arms embargo, characterized by high conflict and by prohibition to sell arms, and other periods, characterized by lower hostilities and unrestricted arms sales. We also distinguish between companies that stand to lose more from violating embargoes (e.g., because of high legal or reputation costs), and companies that stand to lose less. We derive predictions for events occurring during and outside the embargo for the two types of firms.

We consider an infinite-period model in which in every period firms produce arms and sell them in a market with stochastic demand. There are two sources of stochasticity. First, there are two states of the world—Embargo E and Non-Embargo N. The Embargo state E is characterized by fixed costs of firm entry and higher demand for arms, as detailed below. Second, within each state the demand for arms α is stochastic. The stochasticity in the demand for weapons captures the uncertainty regarding the evolution of a conflict.

We model the transition probability between states E and N as a Markov chain. If the country is in the Embargo state at time t, the probability to be in the Embargo state again at time $t+1$ is $P_{E,E}(\alpha_t)$; the probability of a Non-Embargo state at $t+1$ is $1-P_{E,E}(\alpha_t)$. The probability of embargo in the future depends positively on the current state of hostilities, that is, $P'_{E,E}(\alpha_t) > 0$. An embargo is more likely to persist if the hostilities worsen.

We model similarly the transition probability for the case of Non-Embargo. If the country is in the Non-Embargo state at t, the probability to transition to the Embargo state at $t+1$ is $P_{N,E}(\alpha_t)$ and the probability of the Non-Embargo state is $1-P_{N,E}(\alpha_t)$, with $P'_{N,E}(\alpha_t) > 0$. If hostilities increase, the transition to the Embargo state becomes more likely. 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, the probability of an embargo next period is higher if a country is currently under embargo.[1]

In each period t, there is a stochastic realization of the demand for arms α_t, distributed with c.d.f. F. The demand for arms 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. In addition, we make the simplifying assumption that, conditional on the state, the demand for arms α_t is i.i.d. over time. Hence, a higher demand for arms at time t increases the likelihood of the Embargo state at $t+1$ through $P_{E,E}$ and $P_{N,E}$, but, conditional on the state realization at $t+1$, it does not affect the realization of α_{t+1}.[2]

[1] A comprehensive model of embargo imposition and lifting goes beyond the scope of this paper. However, as we discuss in section 5, our simple model of UN behavior is consistent with what we see in our data. Ceteris paribus, increases in hostilities during an embargo increase the likelihood of embargo continuation by 10 percentage points, and outside an embargo they increase the likelihood of embargo imposition by 6 percentage points.

[2] If we allowed for a positive correlation of demand across periods, increases in demand α_t would have the

This implies that the continuation payoff for the Embargo state V_E and for the Non-Embargo state V_N depend on time t only through the realization of the demand parameter α_t:

$$\begin{aligned} V_E(\alpha_t) &= \pi_E(\alpha_t) + \delta\left[P_{E,E}(\alpha_t) V_E + (1 - P_{E,E}(\alpha_t)) V_N\right]; \\ V_N(\alpha_t) &= \pi_N(\alpha_t) + \delta\left[P_{N,E}(\alpha_t) V_E + (1 - P_{N,E}(\alpha_t)) V_N\right]. \end{aligned} \tag{1}$$

The value of the firm in state i is the sum of current profit π_i 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 first expression in (1) with respect to dF_E and the second expression with respect to dF_N. We get

$$\begin{aligned} V_E &= E\pi_E + \delta\left[EP_{E,E} V_E + (1 - EP_{E,E}) V_N\right] \\ V_N &= E\pi_N + \delta\left[EP_{N,E} V_E + (1 - EP_{N,E}) V_N\right] \end{aligned} \tag{2}$$

where we define the expected profits $E\pi_E = \int \pi_E(\alpha)\, dF_E(\alpha)$ and $E\pi_N = \int \pi_N(\alpha)\, dF_N(\alpha)$, as well as the expected probabilities of transition $EP_{E,E} = \int P_{E,E}(\alpha)\, dF_E(\alpha)$ and $EP_{N,E} = \int P_{N,E}(\alpha)\, dF_N(\alpha)$. Subtracting the second equation in (2) from the first and solving for $V_E - V_N$, we obtain $V_E - V_N = (E\pi_E - E\pi_N)/\left[1 - \delta(EP_{E,E} - EP_{N,E})\right]$.

We now compute the derivatives of $V_E(\alpha_t)$ and $V_N(\alpha_t)$ with respect to the contemporaneous demand for weapons α_t. These derivatives capture the impact on the expected discounted value of the company of a demand shift $d\alpha_t$ due to a change in hostilities. Below, we relate these derivatives to the event returns for arms companies. Differentiating (1) and substituting in the expression for $V_E - V_N$, we obtain

$$\frac{\partial V_E(\alpha_t)}{\partial \alpha_t} = \pi'_E(\alpha_t) + \delta P'_{E,E}(\alpha_t) \frac{E\pi_E - E\pi_N}{1 - \delta(EP_{E,E} - EP_{N,E})} \tag{3}$$

$$\frac{\partial V_N(\alpha_t)}{\partial \alpha_t} = \pi'_N(\alpha_t) + \delta P'_{N,E}(\alpha_t) \frac{E\pi_E - E\pi_N}{1 - \delta(EP_{E,E} - EP_{N,E})} \tag{4}$$

A change in the demand for arms has two effects: (i) it alters the current profits, as captured by the first term ($\pi'_E(\alpha_t)$ and $\pi'_N(\alpha_t)$); (ii) it affects the 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.

additional effect of increasing future demand and hence the value V for all firms.

To evaluate these expressions, we derive predictions about the expected profits $E\pi_E$ and $E\pi_N$, and the derivative of profits $\pi'_E(\alpha_t)$ and $\pi'_N(\alpha_t)$ using a model of Cournot competition with barriers to entry, formalized in a Web Appendix to this paper. The key assumption is that there is a legal and reputational cost K to selling arms in the Embargo state. This cost does not apply to sales in the Non-Embargo state. We consider two types of firms with identical demand and identical (linear) production costs, but different legal and reputational cost K. For the high-cost firms H, the legal and reputational cost K^H 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 ($K^L = 0$).[3] We also assume that, due to barriers to entry, only a fixed number of firms can enter the market: at most N^H firms of the high-cost type and at most N^L firms of the low-cost type.

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 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. This is the basis for the empirical test of the response of stock returns to conflict events during the embargo. For high-cost companies, the expression for $\partial V_E^H(\alpha_t)/\partial\alpha_t$ follows from (3). For low-cost companies, we combine (3) and (4) and derive the expression for $\partial V_E^L(\alpha_t)/\partial\alpha_t - \partial V_E^H(\alpha_t)/\partial\alpha$. This matches the empirical test in Section 4 which involves an interaction term with proxies of low-cost of embargo violation.

$$\frac{\partial V_E^H(\alpha_t)}{\partial\alpha_t} = -\delta P'_{E,E}(\alpha_t)\frac{E\pi_N}{1-\delta(EP_{E,E}-EP_{N,E})} \leq 0; \tag{5}$$

$$\frac{\partial V_E^L(\alpha_t)}{\partial\alpha_t} - \frac{\partial V_E^H(\alpha_t)}{\partial\alpha_t} = \frac{\pi_E^L(\alpha_t)}{\alpha_t} + \delta P'_{E,E}(\alpha_t)\frac{E\pi_E^L}{1-\delta(EP_{E,E}-EP_{N,E})} > 0 \tag{6}$$

In the case of Embargo (expression (5)), an increase in demand α_t unambiguously lowers the value of high-cost companies. These companies do not reap the benefits of the increased demand during the embargo since they do not enter the market. In addition, they are hurt by the fact that increase in hostilities lower the probability that the embargo will be

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

lifted in the future. Compared to high-cost companies, low-cost ones respond substantially more positively to a demand shift during embargo (expression (6)). First, these companies benefit from a contemporaneous increase in profits, captured by $\pi_E^L(\alpha_t)/\alpha_t$. Second, they benefit from an increased probability of embargo in the future, which, unlike for high-cost companies, leads to higher profits. These results are summarized in Prediction 1, which we test in Tables 1-3.

Prediction 1 (Events in the Embargo State). *Increases in conflict intensity in the Embargo state (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).*

We then obtain the corresponding predictions for events in the non-Embargo state:

$$\frac{\partial V_N^H(\alpha_t)}{\partial \alpha_t} = \frac{\pi_N(\alpha_t)}{\alpha_t} - \delta P'_{N,E}(\alpha_t)\frac{E\pi_N}{1-\delta(EP_{E,E}-EP_{N,E})} \gtrless 0. \quad (7)$$

$$\frac{\partial V_N^L(\alpha_t)}{\partial \alpha_t} - \frac{\partial V_N^H(\alpha_t)}{\partial \alpha_t} = \delta P'_{N,E}(\alpha_t)\frac{E\pi_E^L}{1-\delta(EP_{E,E}-EP_{N,E})} > 0 \quad (8)$$

In the case of non-Embargo (expression (7)), an increase in demand α_t has two opposing effects on the value of high-cost companies: it increases current profits (as captured by $\pi_N(\alpha_t)/\alpha_t$), but it also increases the future likelihood of an embargo, reducing profits. The sign of expression (7) is therefore ambiguous. Compared to high-cost companies, low-cost companies have the same contemporaneous increase in profitability for events outside the embargo, and a positive future expected increase in profitability (expression (8)). These results are summarized in Prediction 2, which we test in Table 4.

Prediction 2 (Events in the Non-Embargo State). *Increases in conflict intensity in the Non-Embargo state (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).*

We now turn to a description of the data that will be used to test these predictions.

3 Background and Data

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 were imposed against twelve countries. The increased reliance on arms embargoes is 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 have to 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, so we can find stock price data for a significant number of arms producing companies. (ii) The embargo occurs in a country in which conflict took place, since our identification strategy relies on news regarding the evolution of the conflict. (iii) We can identify at least one salient and unexpected conflict event during the embargo period. (iv) No large-scale UN or US intervention occurred in the conflict, because we want to diminish the possibility that stock price effects reflect legal sales to these actors. [4] The final embargo data set includes seven African countries (Angola, Ethiopia and Eritrea, Liberia, Rwanda, Sierra Leone, Somalia, Sudan) and Former Yugoslavia.

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;

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

(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.[5] 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 only events for which the number of stories increases significantly on the event day (typically $s_t \geq 2$) and is relatively large (taking into account the limited news attention dedicated to these countries, typically $i_t \geq 10$). While the selection of the events in our benchmark specifications also takes into account qualitative factors, in Table 3 we examine the robustness of the result to the use of purely quantitative event selection procedures.

Table A2 in the Web Appendix lists the events that satisfy these criteria, including 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 4.

Companies. We use two sources of information on arms-producing companies. The first and main source is the matched Datastream-Worldscope data set of daily stock returns for companies traded in all major stock markets. We identify weapon-making companies as all companies with the primary or one of the seven secondary SIC-codes in the weapon-making range. We include the SIC codes 3482-3484, and 3489 (small arms and ammunitions), 3761, 3764, and 3769 (missiles), 3795 (tanks), and 2892 (explosives).[6]

The second source is a list of top-100 weapon-making companies published by the Stockholm International Peace Research Institute (SIPRI). This classification is based on sources such as company websites and annual reports, a SIPRI questionnaire, news from military journals and newspapers. We use the list compiled by Dunne and Surry (2006) for the year 2004 and include in the sample all the traded companies in this list that are available in

[5]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.

[6]One limitation of the data is that the data set does not include a dynamic SIC code; hence, we classify companies based on their SIC codes in 2005.

Datastream.[7]

Tables A3 and A4 in the Web Appendix contain details on our sample. Table A3 presents a list of the countries in which the companies in the sample 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.

Measures of Cost of Embargo Violation. We collect information on company characteristics that affect the cost of embargo violation: the ease with which companies may circumvent 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.

First, we use 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 constructed standardizing the time-averaged index to mean zero and standard deviation one. For ease of interpretation, we use the indicator variable as our benchmark measure, but also examine the robustness to using the continuous variable.

Second, as an alternative measure of corruption, we use the index of *Control of corruption* (CC) proposed by Kaufmann, Kraay and Mastruzzi (2006) for the years from 1994 to 2004. This index captures 'the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as capture of the state by elites and private interests' (Kaufmann et al. (2006), p.4). Compared to the CPI index, the CC index is estimated from a larger number of data sources and only uses data for the current year (as opposed to the current year and two previous years as the CPI). As for the CPI index, we use a time-average to form an above-the-median indicator and a continuous standardized

[7] An accessory source of data is a list of 1,160 small-arm producing companies published by the non-profit organization NISAT. Within this list, we identify the 53 publicly traded companies present in Datastream. Some of them overlap with the sample constructed through SIC codes or SIPRI, others do not. Since NISAT does not publish the exact criteria used to produce this list, we employ it only for a robustness check in Table 5.

measure.

Third, we use the *Small Arms Trade Transparency Barometer* produced by the Small Arms Survey over the years 2004-2006. This index measures the extent to which a country provides transparent information on small arms exports. It 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, and we average it across the years 2004-2006. We use this average score to construct both a discrete and a continuous measure of low transparency (low cost of embargo violation). This variable is closely linked to transparency in the arms sector, but it is not available for some of the countries in our sample.

Fourth, we identify the countries that did not belong to the OECD in 1985. Membership in an international organization is likely to raise the reputational costs of violating international rules on arms embargo.

Fifth, we use the measure of press freedom provided by *Freedom House* for the years 1994-2004. Countries with a less free press are less likely to monitor illegal transactions conducted by companies head-quartered in their country. We average the measure across the years and define an indicator for below-median press freedom and standardize the continuous variable.

A sixth measure, also produced by *Transparency International*, is the *Bribe Payers Index* (BPI). This index ranks the top 30 exporting countries according to their propensity to bribe abroad, and is constructed from the opinions of business executives. We use the most accurate and comprehensive definition of the index, that is the 2006 BPI.[8] While the CPI measures the likelihood that firms corrupt officials in their own countries (e.g., to obtain licenses), the BPI captures the likelihood that firms bribe the officials of importing countries (either the conflict countries or some third, transit country). Unfortunately, the BPI index covers only a subset of the countries in our sample. We define a discrete and continuous variable using the same methodology as for the corruption variable.

Seventh, we use the self-dealing index of Djankov et al. (2006) as a measure of protection of small shareholders. In countries where small shareholders have fewer control rights (high self-dealing), they are also less likely to have access to information about illegal behavior by

[8]We do not average this measure with the previous years because the measure for 2006 is not comparable with the measure for the previous years.

the managers. We define a discrete and continuous variable of high self-dealing.

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

Returns. For both the Datastream-Worldscope sample and the SIPRI sample, we download the daily return data from Datastream for the years 1985-2005. We drop penny stocks defined as stocks with price of less than 2 units in the local currency unit. We also trim the top and bottom 2/10,000th of returns to avoid extreme outliers.[9] Finally, we drop returns that are zero for ten consecutive days, since this likely indicates a stale price series.

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

$$r_{i,t} = \alpha_i + \beta_i r_{m(i),t} + \varepsilon_{i,t}, \tag{9}$$

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 returns, since the exact day of the event is sometimes hard to determine. As an additional reason to use a 3-day window, while we can measure when a piece of information emerges in the news wires, we do not observe when the marginal investor learns the information, which could occur earlier, or later. We compute the 3-day return $e_{i,t}^{(-1,1)}$ as the cumulative abnormal return: $e_{i,t}^{(-1,1)} = e_{i,t-1} + e_{i,t} + e_{i,t+1}$. 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 $e_{i,t}$.

Match events-returns. We match the events to returns on the same day.[10] For events occurring in the weekend, we shift the event date to the Monday following the weekend.

[9] The results are similar if we do not remove penny stocks or trim outliers.

[10] In Column (5) of Table 1 we show that 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).

4 Event Studies

In this Section we use an event study methodology on the full sample of firms and years to estimate whether on average conflict events affect stock returns for arms companies. In section 6 we conduct event studies on individual firms, using shorter estimation windows around event dates.

Graphical evidence. In Figure 1a, we plot the average (equal-weighted) abnormal 3-day return $e_{i,t}^{(-1,1)}$ on days in which an event during an embargo diminishes the hostilities, in which no event occurs, or in which an event during an embargo increases the hostilities. The number of observation refers to the number of non-missing return observations.

To reflect the opposing effects on companies expected to trade arms legally and illegally, outlined in Section 2, we split the sample in two groups of companies and estimate the returns separately. Because we do not know ex ante which companies trade legally and which ones do not, we rely on variables that are likely to be correlated with the cost of performing illegal trades and of violating the arms embargo, as presented in Section 3. In particular, we separate companies by whether the market where they are headquartered is in a country whose corruption level is above or below the median in our sample according to the Corruption Perceptions Index. In below-median corruption countries, such as USA, most of Western Europe, or Australia, the legal and reputation cost of illegal trades is likely to loom larger than in above-median-corruption countries, such as Italy, Japan, China or South Africa.

For the companies in low-corruption countries, the 10 events diminishing hostilities have a (significantly) positive impact on returns (.32 percentage points, 709 observations), while the events increasing hostilities are associated with -.54 percentage point lower returns (576 observations). On the remaining trading days, returns are precisely estimated to be zero, as one would expect given that the returns are market-corrected. The data provides some support for the hypothesis 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. This is consistent with the predictions of the model for companies with high cost of violating the embargo (Prediction 1.(i)).

For companies in high-corruption markets, the results are very different. The events diminishing the hostilities are associated with a -.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, 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 cease-fire takes place. Overall, this evidence is consistent with the predictions of the model for companies with low cost of violating the embargo (Prediction 1.(ii)).

In Figure 1b we present evidence on the returns to events occurring in *non-embargo* periods. The sample of events includes fourteen events occurring in the 8 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 and the events increasing the hostilities are associated with a slight increase in returns. The patterns do not differ for countries with corruption above and below the median. The flatter response compared to the response to events inside the embargo 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.

So far, we have aggregated all the events that increase or decrease the intensity of the conflict. For the events occurring during the embargo, we now present the disaggregated event-by-event returns. For ease of interpretation, we separate the 8 events increasing conflict (Figure 2a) from the 10 events decreasing conflict (Figure 2b). Remarkably, for 7 out of 8 events increasing conflict (Figure 2a) the abnormal returns are negative for companies in low-corruption countries, and positive for companies in high-corruption countries. Among the 10 event decreasing conflict (Figure 2b), there is a correspondent, though less regular, pattern: 7 out of 10 events are associated with positive returns among the low-corruption countries, and 6 out of 10 events with negative returns among the high-corruption countries. These results indicate that the aggregate event returns in Figure 1b are not due to an outlier, but instead hold for the large majority of events. For the companies in low-corruption countries, in 14 out of 18 events the sign of the event returns is consistent with Prediction 1.(i). Using a binomial test, the probability of 14 or more consistent signs under the null of equal probability of positive or negative signs is .0154, suggesting that this pattern is unlikely to be due to chance. Similarly, for the companies in high-corruption 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 .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.

Benchmark Results. In Table 1, we present our main results for the event returns during the embargo, as in Figure 1a. In Column (1) we estimate the benchmark specification

$$e_{i,t}^{(-1,1)} = \alpha + \gamma Emb_t + \alpha_D D_i + \gamma^D Emb_t * D_i + \eta_{i,t} \tag{10}$$

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 $\hat{\alpha} = -.0001$ and $\hat{\alpha}_D = -.0001$ indicates that, in absence of events, the average return is zero for both types of companies, as it should be, given the use of abnormal returns. An event raising hostilities during embargo lowers stock returns significantly by .42 percentage points ($\hat{\gamma} = -.0042$) 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 ($\hat{\gamma}^D = .0115$), a significant difference. The coefficient estimates $\widehat{\gamma}$ and $\widehat{\gamma} + \widehat{\gamma}^D$ capture the impact of events occurring during embargoes for the two types of companies, as in Figure 1a, except for the additional restriction that increases and decreases in conflict intensity have symmetric effects. This restriction, which we impose to increase the power of the estimation, is not rejected by the data (see Figure 1a).

In Column (2), we estimate specification (10) 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. Since the results are identical, 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 $\hat{\gamma}^D = .0073$ 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 which we considered above.

In Column (4) we test to what extent these results are due to differences in income, which is correlated with corruption. We add real per-capita GDP in $ in 2000 constant prices (from the Penn World Tables) as an additional control to specification (10), both in levels and interacted with the embargo event variable. The effect of the corruption measure remains essentially unaltered while the effect of GDP is insignificant, though in the expected direction. This suggests that corruption, rather than income, is a stronger determinant of illegal arms trades.

In Column (5) we examine the role of the time difference between the country of the event and the stock market where the company is traded. We shift forward (or backward) by one day events for companies that are traded in countries with a time difference of more than eight hours in either direction. The results are very similar. In Column (6) we examine the role of time-series correlation and show that the standard errors are somewhat smaller when we cluster by company, and hence allow for arbitrary autocorrelation within a company over time. Given this finding, in the next specifications we use the more conservative standard errors and cluster standard errors by date. Then, we show that the results do not depend on the market correction, since we obtain similar results using raw returns ($r_{i,t}^{(-1,1)}$, Column (7)) or using returns net of the market ($r_{i,t}^{(-1,1)} - r_{m,t}^{(-1,1)}$, Column (8)). Finally, the results are comparable in the two (overlapping) samples of arms-producing companies: the companies in Worldscope and the companies identified by SIPRI (not shown).

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.

Measures of Cost of Embargo Violation. So far, we examined the impact of above-median corruption. In Panel A of Table 2, we re-estimate specification (10) using alternative discrete measures D_i of low cost of embargo violation, presented in Section 3. In addition, in Panel B of Table 2, we estimate the alternative 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 3). Higher values indicate lower costs of embargo violation.

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 .66 percentage points ($\hat{\alpha}_D = .0066$). 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 with less transparent arms reports display 1.14 percentage points more reaction to the events during an embargo ($\hat{\gamma} = .0114$), a significant difference. The effect replicates using a continuous measure of transparency in arms trade (Panel B). This suggests that availability of information about arms trade is likely to be a determinant of embargo violations.

In Column (4) we use membership in the OECD in 1985, the beginning of the sample, on the grounds that membership in an international organization may raise the reputation costs of a violation of an embargo. Indeed, stock returns for non-OECD companies respond significantly more to conflict events during an embargo.

In Column (5), we attempt to capture the role of the media using the measure of press freedom provided by Freedom House. The results for the low press freedom variable are directionally similar as for the previous three measures, but the estimates are smaller and not significant (marginally significant with the continuous variable). We obtain similar results using a measure of propensity of managers to pay bribes (see Column 6).

Finally, in Column (7) we use the Djankov et al. (2006) 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 the extent to which these minority shareholders may be able to question and block the arms trade. We do not find a significant impact of this measure, although the point estimate for γ^D is positive as for the other measures.

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 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.

Event Selection. As we discussed in Section 3, the selection of events during the embargo 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.

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 set 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 results depend to a large extent on the events included in the core, narrow definition. As expected, the core set of events appears to capture larger changes in the demand for arms (α_t in the model), and hence larger impacts on firms value.

In Columns (4)-(7) we evaluate the results using a quantitative definition of the events. We employ the measures of Event Importance i_t (number of news stories) and of Event Surprise s_t (*increase* in the number of news stories around the event), defined in Section 3. We estimate the specification of Column (2) using the broad sample of 35 events, but we weigh the estimates by the Event Importance (Column (4)) and by the Event Surprise (Column (5)). In both specifications, the point estimate of the effect of high-corruption countries γ^D is larger than in the unweighted regression (Column (2)). In Column (6) and (7) we estimate the impact of events using an automated definition of events based on Event Importance i_t and Event Surprise s_t. Out of the broad sample of events, in Column (6) we use the 21 events with $i_t \geq 10$ and $s_t \geq 2$, and in Column (7) 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 (7), 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.

We also investigated the exact timing of the stock price reactions using as dependent variable 1-day abnormal returns at different windows around the event (Table A5 of the Web Appendix). Half of the impact of the events for the high-corruption countries (γ^D) occurs on the day of the event, and about one fourth of the impact each occurs on the day before and the day after the event. This suggests that the coding of the date is fairly accurate.

Events Outside the Embargo. The model in Section 2 also yields predictions on the returns associated with events outside the embargo. We thus estimate the following augmented version of equation (10):

$$e_{i,t}^{(-1,1)} = \alpha + \alpha^D D_i + \gamma Emb_t + \gamma^D Emb_t * D_i + \delta Out_t + \delta^D Out_t * D_i + \eta_{i,t}. \qquad (11)$$

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 affecting conflict for countries which experienced conflict but not an arms embargo: Algeria, Haiti, Venezuela, Tajikistan, Central African Republic, Ivory Coast, Democratic Republic of Congo, and Togo.[11] We denote this second set of events as "Events in countries without embargo".

The results are displayed in Table 4. In Column (1) we estimate specification (11) in the aggregate, without distinguishing between high- and low-cost companies (that is, we set $\alpha^D = \gamma^D = \delta^D = 0$). We find no effect for events occurring in embargoed countries, and we find a small positive (not significant) effect for events in non-embargoed countries. The effect does not differ between companies with high and low cost of embargo breach (Columns (2) and (3)). The small effect for firms with high cost of embargo violation is consistent with Prediction 2.(i)) of the model.

Most importantly, as we discuss in section 5 these results help us rule out several potential explanations of our main result, such as the possibility that the higher demand for arms for low-cost companies is the result of generalized political instability in the region, of the depletion of old stocks, or of increased input prices. If this were the case, we should find similar effects for events occurring outside the embargo. Table 4 shows that this is not the case.

Placebos. An alternative interpretation of our results is that an omitted variable induces a correlation between the events and stock returns. While it is not clear why the omitted variables would produce an effect only for companies in high-corruption markets, we address

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

this concern directly by presenting regressions at horizons for which, if the model is correctly specified, we should observe no effect. Columns (1) through (6) of Table 5 present the results of these placebo regressions. The specification is as in (10), except that the dependent variables are 3-day abnormal stock returns at horizons (-10,-8), (-7,-5), (-4,-2), (2,4), (5,7), and (8,10) around the event. At these horizons the events should have no effect on stock returns. For example, Column (1) of Table 5 tests whether an increase in conflict on day t affects stock returns between date $t-10$ and date $t-8$, which clearly should not be the case. The data passes this placebo test. We reject the null hypothesis of no effect at the 10 percent level for only one of twelve estimates for γ and γ_D in Columns (1) through (6).

In Column (7) we present another placebo treatment based on different industry codes. We replicate the specification (10) with, as dependent variable, the 3-day return 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. (Note that the stock markets chosen are the ones in which the arms-producing companies are traded, not the ones in which the war events occur.) We do not find any significant effect of the war events.

Firm Characteristics. We now estimate how the event returns depend on firm size and type of arms produced. In Columns (1) and (2) of Table 6 we split the sample into small and large firms. This analysis addresses whether the positive returns for the companies in high-corruption countries are mostly due to differences in firm size. Larger firms are less likely to display significant event returns, since the profits from these trades are likely to be a smaller share of the balance sheets. We define as "small" firms those in the bottom quartile of annual revenue (in US dollars) in any given year. The remaining firms are classified 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. Size therefore does not explain the results, though it affects them.

Next, we estimate which types of weapons are mostly responsible for the results. We estimate specification (10) separately 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)). In each specification, we include companies that have one of the eight SIC codes in the required range; hence, the samples in Columns (3) through (6) are not mutually exclusive. 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 this type of conflicts. Beyond small arms and ammunitions, the estimate for γ_D is largest for consumable arms—explosives and missiles.

5 Calibration and Explanations

Calibration. Under a set of assumptions, we can use the event returns to calibrate the implied profits from legal and illegal arms trading. This calibration uses expressions (5)-(8) which involve five types of parameters: (i) the event returns ($\partial V/\partial \alpha_t * d\alpha_t$); (ii) the average transition probabilities between embargo and non-embargo ($EP_{E,E}; EP_{N,E}$); (iii) the change in transition probabilities induced by conflict events ($P'_{E,E}(\alpha_t)\, d\alpha_t; P'_{N,E}(\alpha_t)\, d\alpha_t;$); (iv) the change in the demand for arms induced by the event ($d\alpha_t/\alpha_t$); and (v) the discount rate (δ). We estimate the first three types of parameters and make assumptions about the last two. We provide details of the calibration exercise in the Web Appendix and discuss here only the estimation of (iii).

Parameter (iii) speaks to a key assumption of our model: an increase in the demand for arms due to a conflict event should increase the probability of embargo in the next period. To estimate this, we computed the fraction of countries under embargo in year t as a function of whether there was an event during the embargo in year $t-1$. The fraction under embargo is .778 if one of the 9 events diminishing hostilities occurred in year $t-1$. The fraction is higher, .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 $P(d_{\text{Embargo},j,t} = 1) = \Phi\left(\alpha + \gamma Emb_{j,t-1}\right)$, where $d_{\text{Embargo},t}$ 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 .100 (s.e. .055): on average an event during the embargo affects the probability of embargo by 10 percentage points. We repeated a similar exercise for events outside the embargo and found that on average an conflict-increasing event outside the embargo increases the probability of embargo in the next period by .063 (s.e. .032).

The results of our calibration indicate that 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. 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. The second number is smaller than the first because the market capitalization of companies in high-corruption countries is substantially 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 where defense expenditure is a large share of the economy.

Explanations. ILLEGAL TRADE. Our interpretation is that the abnormal returns in the event window are evidence of profits due to legal and illegal arms trade. We should point out that the findings up to this point only show an *average* effect across the companies. They do not show that all, or even most, arms companies in high-corruption countries trade illegally. They also do not rule out that some companies in low-corruption countries trade illegally. Moreover, our interpretation does not imply that arms companies in high-corruption countries violate the embargo *directly*. It is possible that the trade of arms flows through an intermediary, in a way that still leaves the original company a substantial profit margin. Notice, however, that 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) In addition to legal costs, the reputational costs from re-export can be substantial. For example, 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).

DEPLETION OF OLD ARMS. An 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. This interpretation, however, does not explain why the effect of the events is significant under embargo, but not outside the embargo, nor the difference in response between companies with low- and high-cost of embargo violation.

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, nor why companies in high and low-cost countries do 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. This would predict, though, a similar finding for the events outside the embargo, which we do not observe.

Regional Instability. The impact of events under the embargo may be due to the 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, the fact that we find a different impact for events occurring *outside* the embargo is harder to reconcile—unless one posits that events inside the embargo are more significant.

Investor beliefs. 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., Maloney and 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. For examples, investors in high corruption markets may have relatively high priors on the likelihood of company involvement in embargo breaches, while investors in low corruption markets may be genuinely misinformed. 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 a test that does not rely on investor knowledge would be preferable, it is highly unlikely that systematic, direct evidence of illegal arms trade (e.g., based on trade data) will ever be available, given the weak requirements of the existing regulations and the sensitivity for national security of the arms trade data. As a result, the investor-based evidence is likely to be the best source of information.

6 Detecting Individual Violations

Our final set of empirical evidence is in the spirit of the detection of illegal behavior of the forensic economics literature (Duggan and Levitt, 2002; Jacob and Levitt, 2003; Wolfers, 2006; Hsieh and Moretti, 2006). While in Section 4 we examined *average* event returns across groups of companies, in this Section we consider each company and event in isolation. We record each 'anomalous' reaction (see below for a definition) and generate a list of companies and events that the returns suggest may be embargo violators. While we do not single out individual firms, we illustrate the characteristics of the firms that this methodology identifies. A caveat is that, since we only observe a small number of events, this detection procedure remains subject to substantial error margins. Nevertheless, it provides an idea of 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 (9) with an estimation window of 100 trading days.[12] 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 of Campbell et al. (1997) with a 10 percent significance threshold.

Individual reactions. We first analyze reactions to individual events, and then combine individual reactions into *chains* of reactions. As suggested by the model in Section 2, we isolate three types of reactions to individual events. The first type, denoted as "*Illegal_React*", is that of 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*", has reversed signs. It occurs when the return of the company 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)). Finally, the third type, labelled as "*Outside_React*", indicates companies that display

[12] This procedure differs slightly from the market correction procedure adopted in the previous Section where, for computational reasons, the market model is estimated over the previous year of data, instead of over the previous 100 days. Because of this difference, the number of non-missing return observations for events during the embargo is 1,838, compared to 1,786 in the previous Section (see Column (2) in Table 1). The results in this Section are essentially identical if we restrict the sample to the 1,786 events.

a statistically significant positive (negative) return when conflict increases (decreases) *outside* the embargo. This pattern is consistent with the company selling arms to the country (Prediction 2.(ii)). Table A6 in the Web Appendix provides an example of our categorization.

Chains of Reactions. Because isolated reactions may be the result of noise, we 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.

Over the whole sample, we find a total of 23 company-country pairs with a chain of illegal reactions (Column (1) of Table 7). These chains pertain to 19 different companies, as two companies display chains in two embargoes and one company in three embargoes. To evaluate the frequency of these chains, we compare it to the number of all possible combinations of events within a company-country pair that could have led to identifying a chain (Column (2)). For example, in a country with three events occurring during an embargo, events 1,2, and 3, a chain requires at least two significant reactions in the illegal directions: either events 1-2, or 1-3, or 2-3. For a company with non-missing price data for all three events, there are three possibilities of chain; for a company with missing returns in event 1, instead, there is only one possibility. In general, let $n_{i,j}$ be the number of events inside the embargo with non-missing returns for company i in country j. Similarly, let m_i the number of events outside the embargo with non-missing returns for company i in country j. The number of possible chains for the country-company pair is $n_{i,j}^2/2! + m_{i,j}n_{i,j}$, where the first addendum corresponds to sequences of two illegal reactions inside the embargo and the second to sequences of one reaction outside and one illegal reaction inside. Column (3) reports the percent of possible chains (Column (2)) that are *actual* chains (Column (1)), which is 0.6 percent.

In the rest of Table 7 we provide aggregate statistics by sub-groups. Since the sample size of potential violators is small, this evidence should be considered as suggestive. The country with the greatest number of violations is Liberia, where 8 companies displayed a chain of reactions consistent with embargo violation, that is, 1.5 percent of the potential chains. Sudan follows with 7 chains of reactions (1.46 percent incidence). Next are Angola with 3 chains (0.64 percent incidence) and Sierra Leone with 4 chains (0.24 percent incidence).

Next, we evaluate the relationship with our proxies of the cost of embargo violation. In absolute levels, there is a higher number of violators among companies located in low-corruption (that is, high-cost) countries: 14 against 9. However, once we correct for differences in the number of return observations, the incidence of chains is higher in high-corruption countries, as expected: 0.88 percent versus 0.50 percent. The pattern is similar using the arms transparency index. The percentage of significant illegal chains is instead about the same for low- and high-cost companies when we use the other proxies. The results using the corruption index and the arms transparency index indicate a two-fold pattern of results. First, companies in low-cost countries appear more likely to engage in illegal arms trading, consistent with Prediction 1.(ii) and with the findings in Section 4. Second, a sizeable number of companies from high-cost countries are identified as potential violators too. This clarifies that our earlier findings did not imply that *only* companies from high-corruption countries were detected as violating embargoes.

7 External Validation

In the spirit of the forensic economics literature, we would like to compare the list of detected companies based on returns to outside evidence on legal and illegal arms trade. Unfortunately, direct evidence on violations of arms embargoes is very hard to come by. (The lack of such evidence is, in fact, a motivation for this study). A first source is the United Nations itself. The UN attempts to monitor violations of the arms embargo for each conflict. The known violations are organized in three main sources: the Reports of Panel of Experts, the Reports of the Monitoring Groups, and Selected Documents.[13] For all the eight countries in the sample, we examined the three sets of documents. The violators named in the reports are mostly brokers and intermediaries, and no traded company in our sample is mentioned in these reports. We interpret this as evidence that detection of trades by larger companies is more difficult, and perhaps the political will for detection weaker.

While we cannot use the UN reports to validate the detection of individual companies, we can use them to measure the seriousness of embargo violations for the 8 conflicts in the sample. The number of UN reports devoted to embargo enforcement in a given conflict is likely to reflect the seriousness of the violations in that conflict. We thus construct two quantitative measures of the number of UN documents. We group together the reports by the Panel of Experts and by the Monitoring Group, since these are comparable in nature and less frequent, and define $MGPE_j$ as the total number of these reports concerning country j, divided by the number of years of the embargo. The average incidence of $MGPE$ across

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

the eight countries is .75, with a minimum of 0 (Ethiopia, Rwanda, and Yugoslavia) and a maximum of 3 (Sudan). Similarly, SEL_j is the number of Selected Documents concerning country j, divided by the number of years of the embargo. The average incidence of Selected Documents is 1.07, with a minimum of 0 (Rwanda, Somalia, and Sudan) and a maximum of 3 (Liberia). The information refers to the years of embargoes for which information is available on the UN website.

We test if, in conflicts with higher incidence of UN reports, companies are more likely to be detected as reacting to the conflict events. In Panel A of Table 8 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, we find that 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. Official reports by Panel of Experts and Monitoring Groups appear to be more informative of the seriousness of embargo breaches. 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. These findings are consistent with our predictions: in conflicts with a higher share of reports, and hence a likely higher number of violations, we detect a higher frequency of illegal trades and a lower frequency of legal trades. The return-based detection and the measures based on the number of UN reports are consistent. We should, however, 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 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 Saiz and Simonsohn (2007) used to measure corruption. For each company i, we record four counts of Google hits: (i) n_i for 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 (i.e., "Sudan"). 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

[14]Two full searches were conducted by two independent teams of research assistants; we take the average

for 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 8). 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 of an effect on the detection of legal reactions (Panel B). These findings provide some external validation to the return-based detection, albeit an indirect one, since we cannot examine the Internet content directly given the 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.

8 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.

of the fractions computed according to each team's counts.

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Figure 1a. Return for Events During Embargo

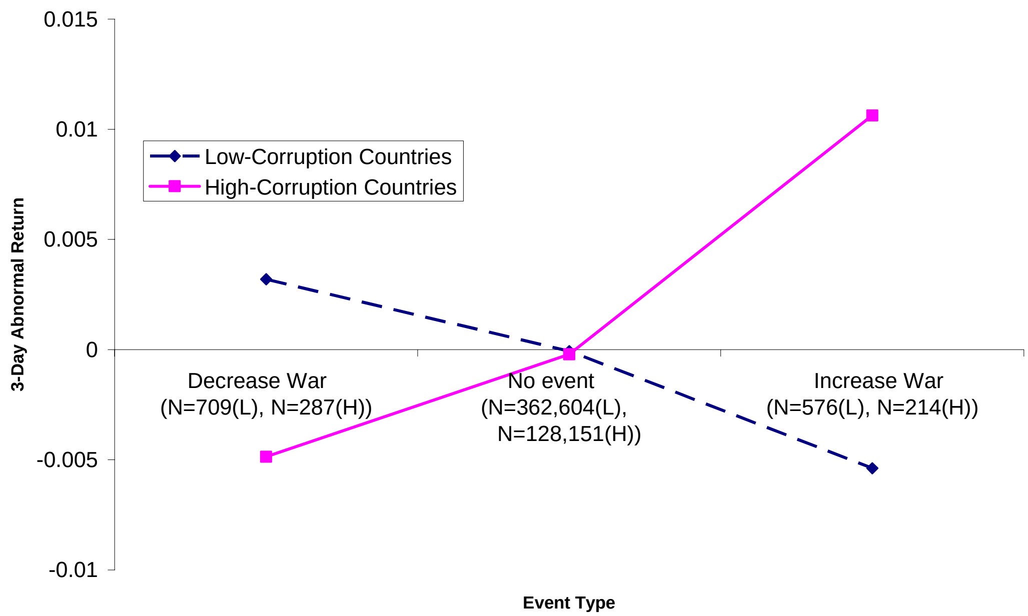
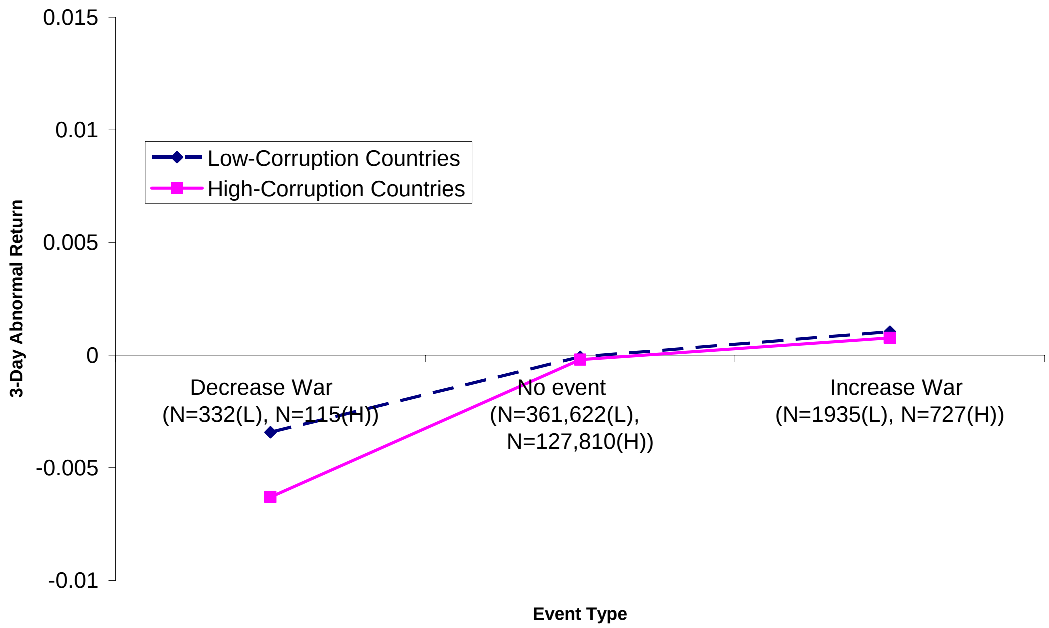


Figure 1b. Return for Events Outside Embargo



Note. Figures 1a and 1b display average 3-day abnormal cumulative returns 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. The average returns in Figure 1a are 0.0032(0.0013)**, -0.0001(0.0001), and -0.0054(0.0037) for companies in low-corruption countries and -0.0049(0.0043), -0.0002(0.0002), and 0.0106(0.0053)** for companies in high-corruption countries. The average returns in Figure 1b are -0.0034(0.0062), -0.0001(0.0001), and 0.001(0.0017) for companies in low-corruption countries and -0.0063(0.0029)**, -0.0002(0.0002), and 0.0008(0.0020) for companies in high-corruption countries.

Figure 2a. Abnormal Returns For Events Increasing War During Embargo: High- vs. Low- Corruption Countries

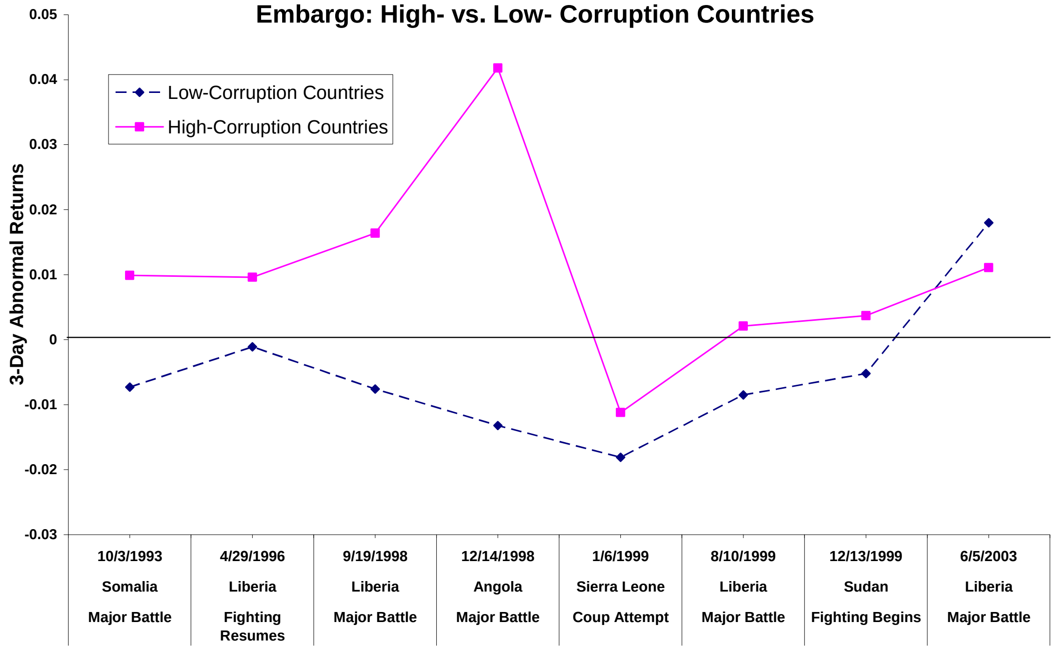
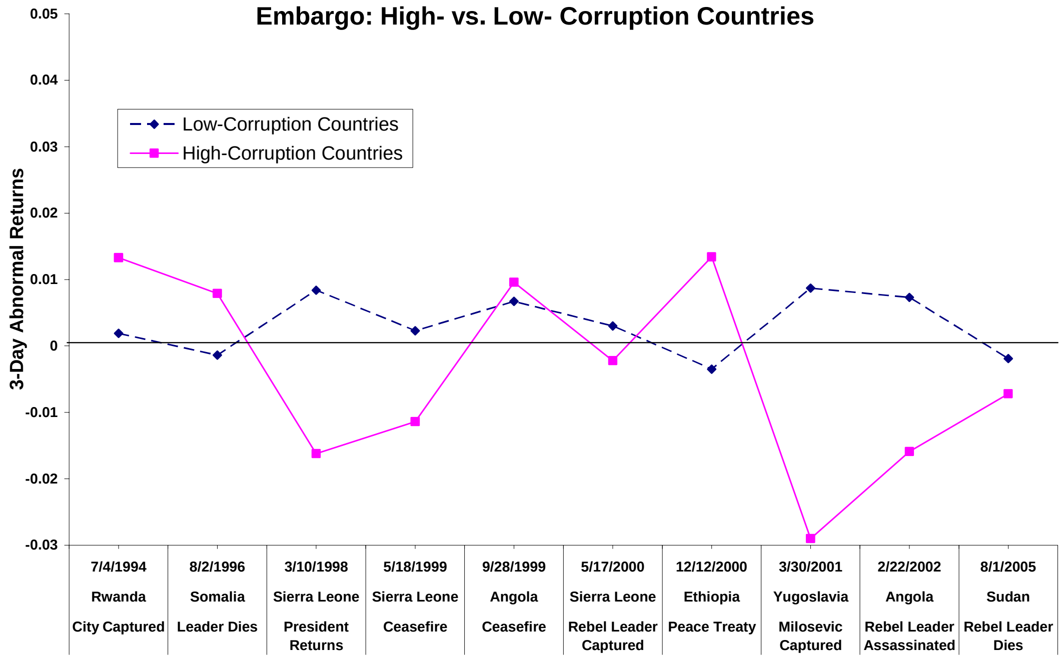


Figure 2b. Abnormal Returns For Events Decreasing War During Embargo: High- vs. Low- Corruption Countries



Note. Figures 2a-2b 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 8 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*.

Table 1. Stock Market Reaction to War Events: Benchmark Effects

Dep. Var.:	Abnormal 3-Day Stock Return (-1,1)						3-Day Raw Returns (-1,1)	3-Day Excess Returns (-1,1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.0042 (0.0018)**	-0.0042 (0.0019)**		0.0093 (0.0343)	-0.0041 (0.0014)***	-0.0042 (0.0013)***	-0.0045 (0.0024)*	-0.0046 (0.0022)**
Event During Embargo* (High-Corruption Country)	0.0115 (0.0041)***	0.0115 (0.0042)**	0.0073 (0.0034)**	0.0105 (0.0046)**	0.0118 (0.0039)***	0.0115 (0.0036)***	0.012 (0.0049)**	0.0117 (0.0040)***
Event During Embargo* Low-Corruption Country)			-0.0042 (0.0018)**					
Event During Embargo* (Log GDP in 2000$)				-0.0013 (0.0034)				
Low Cost of Embargo Violation - Indicator	-0.0001 (0.0002)		-0.0001 (0.0002)	-0.0001 (0.0002)		-0.0001 (0.0002)	-0.0004 (0.0002)	0 (0.0002)
Log GDP in 2000$				0.0001 (0.0002)				
Constant	-0.0001 (0.0001)		-0.0001 (0.0001)	-0.0009 (0.0023)	-0.0001 (0.0001)	-0.0001 (0.0001)	0.0023 (0.0002)***	0.0011 (0.0001)***
Include Only Event Days		X						
Shift Date for Time Difference > 8 Hours					X			
Clustering of Standard Errors	By Date	By Date	By Date	By Date	By Date	By Company	By Date	By Date
N	492541	1786	492541	492541	492541	492541	492541	492541

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. In Column (5) the event date is shifted by one day if the difference in time zones between the country of the event and the country where the company shares are traded is larger than 8 hours. In Column (6) the standard errors are clustered by date rather than by company as in the other regressions. In Columns (7) and (8) we present the results with raw returns (Column (7)) and returns net of the market return (Column (8)), instead of beta-corrected returns. Robust standard errors clustered by date in parentheses, except in Column (6).

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2. Stock Market Reaction: Measures for Cost of Embargo Violation

Dep. Var.:	Abnormal 3-Day Stock Return (-1,1)						
Measure of Cost of Embargo Violation:	*High Corruption Percept. Index*	*Control of Corruption Index*	*Low Transparency of Arms Trade*	*Non-OECD Member*	*Low Press Freedom*	*High Bribe-Payer Index*	*High Self-Dealing Index*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A -- Indicators for Cost of Embargo Violation							
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.0042	-0.0042	-0.0043	-0.0031	-0.0023	-0.0027	-0.0025
	(0.0018)**	(0.0019)**	(0.0020)**	(0.0017)*	(0.0017)	(0.0017)	(0.0016)
Event During Embargo* (Low Cost of Embargo Violation, Indicator)	0.0115	0.0117	0.0114	0.015	0.0061	0.0058	0.0055
	(0.0041)***	(0.0043)***	(0.0042)***	(0.0056)***	(0.0045)	(0.0046)	(0.0040)
Low Cost of Embargo Violation - Indicator	-0.0001	-0.0002	-0.0001	-0.0002	-0.0002	0	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Constant	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	0
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(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	0.0025	.	0.0008	0.0007	-0.0005
	(0.0018)	(0.0019)	(0.0025)	.	(0.0015)	(0.0018)	(0.0017)
Event During Embargo* (Low Cost of Embargo Violation, Continuous)	0.0066	0.0072	0.0048	.	0.0039	0.005	0.0016
	(0.0028)**	(0.0029)**	(0.0019)**	.	(0.0023)*	(0.0026)*	(0.0017)
Low Cost of Embargo Violation - Continuous	-0.0002	-0.0002	0	.	0	0	-0.0001
	(0.0001)	(0.0001)	(0.0001)	.	(0.0001)	(0.0001)	(0.0001)
Constant	-0.0002	-0.0002	-0.0001		-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. (2006)
N	492541	492541	475101	492541	492541	477881	492541

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 head-quartered (see Section 4 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 10%; ** significant at 5%; *** significant at 1%

Table 3. Stock Market Reaction: Event Selection

Dep. Var.:	Abnormal 3-Day Stock Return (-1,1)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.0042 (0.0018)**		-0.0036 (0.0026)				
Event During Embargo * (High-Corruption Country)	0.0115 (0.0041)***		0.0096 (0.0048)**				
Event During Embargo (Broad Def.) (1=Increase War, -1=Decrease, 0=No Event)		-0.0024 (0.0014)*	-0.0006 (0.0019)	-0.002 (0.0017)	-0.0033 (0.0015)**		
Event During Embargo (Broad Def.) * (High-Corruption Country)		0.0069 (0.0026)***	0.0019 (0.0024)	0.0109 (0.0041)***	0.0083 (0.0029)***		
Event During Embargo (Autom. Def.) (1=Increase War, -1=Decrease, 0=No Event)						-0.0032 (0.0013)**	-0.0049 (0.0021)**
Event During Embargo (Autom. Def.) (High-Corruption Country)						0.0086 (0.0029)***	0.0104 (0.0045)**
Indicator for High-Corruption Country	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0007 (0.0010)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)
Constant	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0004 (0.0005)	-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	Broad Set of Events	Broad Set of Events	Events with Surprise>=2 Import.>=10	Events with Surprise>=3 Import.>=20
Weighting	Unweighted	Unweighted	Unweighted	Weighted by Event Importance	Weighted by Event Surprise	Unweighted	Unweighted
Number of Events	18	35	35	35	35	21	10
N	492541	492541	492541	492541	492541	492541	492541

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)-(5) we use a broader set of 35 events occurring during the embargo period. This broad definition includes some events that we do no 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) the regression is weighted by the event importance (the importance is set to 1 for non-event days). In Column (5) the regression is weighted by the event surprise (the surprise is set to 1 for non-event days). In Column (6) we use the subset of broad events with event surprise >=2 and event importance >=10. In Column (7) 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 10%; ** significant at 5%; *** significant at 1%

Table 4. Stock Market Reaction to Events Outside the Embargo

Dep. Var.:	**Abnormal 3-Day Stock Return (-1,1)**		
	(1)	(2)	(3)
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.001 (0.0015)	-0.0042 (0.0018)**	-0.0043 (0.0020)**
Event During Embargo * (Low Cost of Embargo Violation)		0.0115 (0.0041)***	0.0114 (0.0042)***
Event Outside Embargo (1=Increase War, -1=Decrease, 0=No Event)	0.0001 (0.0020)	0.0003 (0.0026)	0 (0.0025)
Event Outside Embargo * (Low Cost of Embargo Violation)		-0.0008 (0.0038)	0.0005 (0.0031)
Event in Countries without Embargo (1=Increase War, -1=Decrease, 0=No Event)	0.0025 (0.0018)	0.0023 (0.0022)	0.0023 (0.0023)
Event in Countries without Embargo * (Low Cost of Embargo Violation)		0.0008 (0.0029)	0.0001 (0.0030)
Proxy for Low Cost of Embargo Violation - Indicator Variable		-0.0001 (0.0002)	-0.0001 (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
N	492541	492541	475101

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 10%; ** significant at 5%; *** significant at 1%

Table 5. Stock Market Reaction: Placebo Treatments

Dep. Var.:	Abnormal 3-Day Stock Return of Company j						Aggregate Stock Return in Market of Company j
Timing relative to Event:	(-10,-8)	(-7,-5)	(-4,-2)	(2,4)	(5,7)	(8,10)	(-1,1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event During Embargo	0.0003	0.0023	0.0027	0.0024	-0.0003	-0.0028	0.0001
(1=Increase War, -1=Decrease, 0=No Event)	(0.0021)	(0.0019)	(0.0020)	(0.0018)	(0.0018)	(0.0018)	(0.0026)
Event During Embargo*	0.0014	-0.0047	-0.0042	-0.0022	-0.0017	0.0022	0.0003
(High-Corruption Country)	(0.0036)	(0.0025)*	(0.0034)	(0.0034)	(0.0030)	(0.0027)	(0.0032)
Indicator for High-Corruption	-0.0001	-0.0002	-0.0002	-0.0001	-0.0001	-0.0001	-0.0004
Country	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)*
Constant	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	0.0012
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)*	(0.0002)***
N	484230	486666	489317	489317	486666	484230	492541

Notes: An observation in the regression is a trading day for one of the 153 arms-producing companies in the years 1985-2005. In columns (1)-(6), the dependent variable is the abnormal 3-day return for different windows around the event. The specifications in this Table are placebo specifications since events should not affect stock returns earlier than 2 days before the event, or later than 2 days after the event. In Column (7), the dependent variable is the 3-day stock return in the 3 days surrounding the event for the market index of the country in which the company is traded. This specification is a Placebo treatment because we do not expect the war events to affect the whole stock market, but only the weapon-making companies. 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 10%; ** significant at 5%; *** significant at 1%

Table 6. Stock Market Reaction by Firm Characteristics (Firm Size and Type of Arms)

Dep. Var.:	Abnormal 3-Day Stock Return (-1,1)					
Firm Characteristics:	Firm Size		Type of Arms Produced			
	Small Firms	Large Firms	Small Arms & Ammunitions	Missiles	Tanks	Explosives
	(1)	(2)	(3)	(4)	(5)	(6)
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.01 (0.0029)***	-0.0024 (0.0018)	-0.0048 (0.0035)	-0.0057 (0.0036)	-0.0049 (0.0042)	-0.0077 (0.0048)
Event During Embargo* (High-Corruption Country)	0.02 (0.0052)***	0.0075 (0.0042)*	0.0099 (0.0056)*	0.029 (0.0186)	0.0046 (0.0046)	0.0137 (0.0084)
Indicator for High-Corruption Country	-0.0003 (0.0004)	0 (0.0002)	-0.0001 (0.0004)	0.0004 (0.0007)	0.0001 (0.0005)	-0.0002 (0.0004)
Constant	-0.0001 (0.0003)	-0.0001 (0.0001)	-0.0002 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0003)
Sample of Companies	All	All	Worldscope	Worldscope	Worldscope	Worldscope
N	132699	355898	133316	113998	43061	58395

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 Column (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 10%; ** significant at 5%; *** significant at 1%

Table 7. Detection: Chains of illegal reactions

		# Illegal Chains of Reactions (1)	# Possible Chains of Reactions (2)	Percent of Chains of Illegal Reactions (3)
Full sample		23	3813	0.60%
In which conflicts?				
By Country Under Embargo	Angola	3	467	0.64%
	Ethiopia	1	184	0.54%
	Liberia	8	532	1.50%
	Rwanda	0	173	0.00%
	Sierra Leone	4	1643	0.24%
	Somalia	0	82	0.00%
	Sudan	7	479	1.46%
	Former Yugoslavia	0	253	0.00%
Which type of companies?				
By Corruption Perception Index	High corruption	9	1019	0.88%
	Low corruption	14	2794	0.50%
By Transparency of Arms Trade	Low Transparency	10	944	1.06%
	High transparency	13	2730	0.48%
By Membership in OECD	non-OECD	3	468	0.64%
	OECD	20	3345	0.60%
By Press Freedom	Low Press Freedom	4	767	0.52%
	High Press Freedom	19	3046	0.62%
By Bribe-Payer Index (BPI)	High BPI	4	755	0.53%
	Low BPI	19	2945	0.65%
By Self-Dealing Index	High self-dealing	7	980	0.71%
	Low self-dealing	16	2833	0.56%

Notes: In this Table we report in Column (1) all company-country observations for which we detect a Chain of Illegal reactions. A Chain of Illegal reactions occurs when a company within a conflict displays more than one reaction 'Illegal_React' or a combination of a reaction "'Illegal_React' and a reaction 'Outside_React'. 'Illegal_React' denotes the case in which the return significantly increases (decreases) at the 10 percent level when conflict increases (decreases) during the embargo; 'Outside_React' denotes the case in which the return significantly increases (decreases) at the 10 percent level when conflict increases (decreases) outside the embargo. In Column (2) we report the number of all possible combinations of events within a company-country pair that could have led to identifying a Chain. In Column (3) we present the fraction of Chains (Column (1) to possible Chains (Column (2)). We display the information by conflict, and using six different indicator variables of below-median cost of violating an embargo for the country where the company is head-quartered (see Section 4 in the text).

Table 8. 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 -- Dep.var.: 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	0.0339
			(0.0226)**	(0.0323)	(0.0209)
Constant	0.0582	0.0625	0.0763	0.0775	0.0757
	(0.0140)***	(0.0159)***	(0.0102)***	(0.0115)***	(0.0108)***
N	1838	1838	1811	1811	1811
Panel B -- Dep.var.: 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	0.0044
			(0.0314)	(0.0274)	(0.0178)
Constant	0.1068	0.0878	0.0903	0.0931	0.0915
	(0.0084)***	(0.0111)***	(0.0071)***	(0.0084)***	(0.0073)***
N	1838	1838	1811	1811	1811

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_React' 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 the 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 10%; ** significant at 5%; *** significant at 1%