POL

Detecting Illegal Arms Trade*

Stefano DellaVigna
UC Berkeley and NBER
sdellavi@berkeley.edu

Eliana La Ferrara
Universita' Bocconi and IGIER
eliana.laferrara@unibocconi.it

This version: July 10, 2007.

Abstract

Illegal arms are responsible for thousands of deaths in civil wars every year. Yet, their trade is very hard to detect. We propose a method to statistically detect illegal arms trade based on the investor knowledge embedded in financial markets. We focus on eight countries under UN arms embargo in the period 1990-2005. We consider events during the embargo that suddenly increase or decrease conflict intensity, and examine the contemporaneous stock returns of weapon-making companies. If the companies are not trading or are trading legally, an event worsening the hostilities should not affect stock prices or affect them adversely, since it delays the removal of the embargo. Conversely, if the companies are trading illegally, the event may increase stock prices, since it increases the demand for illegal weapons. We detect a large and significant positive reaction for companies trading in markets where the legal and reputation costs of illegal trades are likely to be lower. The results hold using measures of corruption and transparency in arms trade, or membership in OECD. We also suggest a method to detect potential embargo violations based on stock reactions by individual companies, including chains of reactions. The presumed violations are higher for conflicts with more UN investigations and for companies with more Internet stories regarding arms and embargo. Our analysis suggests that investors believe some companies are selling arms that will ultimately reach countries under embargo.

*Keith Chen, Lauren Cohen, Raymond Fisman, Ben Hermalin, Ethan Kaplan, Lawrence Katz, Debraj Ray, Jesse Shapiro, Uri Simonsohn, and audiences at the Catholic University of Milan (Italy), Columbia University, Princeton University, Stockholm (SITE), UC Berkeley, the University of Maryland-College Park, the University of Rome (Tor Vergata, Italy), the University of Toulouse, ULB-Bruxelles, Yale University (SOM), the BREAD-CESIfo conference in Venice, the NBER Labor Studies Spring Meeting, the NEUDC conference at Cornell, and the Inequality and Development conference at Namur provided useful comments. We thank Loretta Bondi and Ollie Sprague for helpful discussions on arms embargoes. Maria Aleksynska, Scott Baker, Thomas Barrios, and Silvia Redaelli provided excellent research assistance.

1 Introduction

Armed violence around the world is estimated to be responsible for at least 100,000 deaths just in the year 2003. (ControlArms, 2006) A significant portion of these deaths is believed to be due to arms that are imported illegally. (Small Arms Survey, 2006)

Given the seriousness of the problem, policy-makers have taken steps to limit this trade. The United Nations has imposed arms embargoes on countries involved in several of the most severe conflicts, including Angola, Sudan, and Yugoslavia.

Unfortunately, because of the illegal nature of these transactions, little quantitative information exists on the nature of illegal arms trade. The most basic questions are still unanswered. How wide-spread is this trade? Which countries illegally export weapons in areas of civil conflict? A better answer to these questions is a pre-condition for effective policies.

In this paper, we propose a method that can provide some 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 identify the events, we use historical information and counts of newswire stories in the event days. An example of an event increasing hostilities is an unsuccessful coup attempt by the rebels in Sierra Leone on January 6, 1999.

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. 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 significant 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 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. The event returns are larger for events that are more unexpected or more significant according to news counts. The stock return effect 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 strongest 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 and firm profitability, 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.

The model rationalizes the two main findings of the first part of the analysis. First, increases in conflict during the embargo hurt high-cost companies. 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 benefit substantially low-cost companies. 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 \$2m 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 may benefit high-cost firms, since they increase contemporaneous profits (though they hurt future profits). In addition, these events should benefit the low-cost firms somewhat more. We test these predictions using events that occur before, or after, the imposition of an embargo in the eight countries of our sample. We also consider events affecting the intensity of conflict in countries experiencing hostilities but no arms embargo, such as Algeria, Congo, and Venezuela. We find some support for the predictions of the model. Events increasing conflict do not hurt (or benefit) high-cost companies, as measured by corruption indicators. However, we find no difference in the event returns between low- and high-corruption countries.

We also consider alternative interpretations. The stock return effect could be due to an increase in the worldwide demand for weapons, for example if the increases in conflict during the embargo lead to depletion of old stocks of weapons. This interpretation does not explain, though, the difference in event returns for events under embargo and events not under embargo. The difference in the event returns between low- and high-cost companies could also be due to differences in arms produced: low-cost companies may produce more of the weapons that are demanded in the embargoes countries. This, however, does not explain why the returns of high-cost companies respond negatively to increases in conflict. We also discuss interpretations based on regional instability, product mix, and misinformed investors.

While the event studies identify average differences in returns across groups of companies, 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. We find some evidence, as in the previous results, that companies in countries with low cost of embargo are more likely to be detected as engaging in illegal arms trade. We also identify 23 chains of multiple significant reactions within a conflict by the same company. Three companies display chains of reactions for more than one conflict. Still, these results should be considered as suggestive, because the uncertainty in the estimates is such that the detection remains subject to high error margins.

In Section 7, we use external sources to validate the detection results. Unfortunately, there is very little direct evidence from the UN investigations: the few detected violators do not include publicly traded companies. (In fact, the lack of direct evidence is a motivation for our return-based detection procedure) We can, however, use indirect evidence. We detect more predicted violations in conflicts with more UN investigations, such as in Liberia, and for companies with more Internet stories regarding arms and embargo.

The paper is related to the literature on forensic economics. Papers in this literature take advantage of large data sets to detect patterns of cheating and corruption. Examples include detecting teacher cheating (Jacob and Levitt, 2003) and pinpointing corruption in sports (Duggan and Levitt, 2002; Wolfers, 2006). Most closely related is Hsieh and Moretti (2006), who use time-series movements in oil prices to infer whether the Iraq regime violated the oil-for-food program. Compared to these papers, we rely on investor information to detect illegal behavior, rather than on behavior of the agents committing the crime.

The paper also relates to the event studies literature, in particular the studies of the effect of political events on stock prices. These studies have explored the impact of political connections (Roberts, 1990; Fisman, 2001) and of civil conflict (Guidolin and La Ferrara, forthcoming). A key difference relative to this literature is that we do not know ex ante which companies are affected by the event, but rather use the stock response to determine it.

The 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). We suggest a methodology that exploits the investor information to measure the illegal trade of arms, a (proximate) determinant of conflict.

2 A Simple Model

We present a simple model of conflict, arms production, and firm profitability. The model focuses on the implications for firm value of shifts in the demand for arms. We interpret the shifts in demand as due to increases or decreases in conflict. 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.

2.1 Conflict

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 probabilities 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 transition to 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 $P_{E,E}(\alpha_t) > P_{N,E}(\alpha_t)$ for all α. This captures a form of state dependence: for given hostilities α, the probability of an embargo next period is higher if a country is currently under embargo.

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: we assume that the demand for arms in the Embargo state first-order stochastically dominates the distribution 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}. This assumption, while not very realistic, simplifies the solution without, we argue below, substantial loss of generality.

We can now write the continuation payoff for the Embargo state and for the Non-Embargo state. These continuation payoffs depend on time t only through the realization of the demand parameter α_t. The value of the firm in state $i = E, N$ is

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

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 $E\pi_N = \int \pi_N(\alpha)\, dF_N(\alpha)$ and 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, we obtain $V_E - V_N = E\pi_E - E\pi_N + \delta\left[(EP_{E,E} - EP_{N,E})(V_E - V_N)\right]$ which can be solved for $V_E - V_N$ as follows:

$$V_E - V_N = \frac{E\pi_E - E\pi_N}{1 - \delta\left(EP_{E,E} - EP_{N,E}\right)}$$

The difference in company value between the Embargo state and the Non-Embargo state is increasing in the difference between the average profits $E\pi_E$ and $E\pi_N$ in the two states.

In addition, given that $E\pi_E > E\pi_N$ (as we show below), the difference $V_E - V_N$ is larger if the difference between $EP_{E,E}$ and $EP_{N,E}$ is larger. If the probability of transitioning to the Embargo state is higher under the Embargo state ($EP_{E,E}$) than under the non-Embargo state ($EP_{N,E}$), the Embargo state is more persistent and hence more valuable.

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

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

In order to evaluate these expressions, we need to measure the expected profits in the state of Embargo ($E\pi_E$) and in the state of Non-Embargo ($E\pi_N$), as well as the derivative of profits in either state with respect to the demand shift α_t ($\pi'_E(\alpha_t)$ and $\pi'_N(\alpha_t)$). We derive predictions about these variables using a simple model of competition with barriers to entry.

2.2 Arms Production and Firm Profits

We model the competition among firms as a two-stage game with an entry decision and Cournot competition, taking place within each period t. In the first stage of period t, the potential entrants observe the realization of the state–Embargo or not Embargo–, and then decide to enter or not enter the market. The firms that enter pay a fixed cost K which depends on the type of firms and on the state of the world, as detailed below. In the second stage of period t, the N_t firms that entered the market observe the demand realization α_t and choose production levels q_t in a Cournot game. We rule out repeated game strategies and assume that, in each time period t, firms play a static equilibrium.

We consider two types of firms, firms with high cost of embargo violation (H) firms and firms with low cost of embargo violation (L). The two types of firms have identical demand and identical linear costs of production $c(q_t) = cq_t$, with $c > 0$, but different entry costs K. The high-cost firms H pay an entry cost K_E^H for selling arms in the Embargo state. This captures the legal and reputational cost of violating an embargo. The entry cost is instead zero for the low-cost (L) firms, that is, $K_E^L = 0$. These firms stand less to lose from embargo violations. Finally, there is no cost of entry in the Non-Embargo state, that is, $K_N^H = K_N^L = 0$. We also assume that, due to barriers to entry, in any period t 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.

We first analyze the competition in the second stage, and then characterize the entry decision in the first stage.

Competition. We consider symmetric equilibria in the second stage of period t where all firms choose the same quantity q_t. Hence, aggregate supply Q_t in period t is equal to $N_t q_t$. The aggregate demand function for the market is $\alpha_t D(P_t)$ where α_t is a demand shift capturing shifts in demand due to changes in conflict. We write the equilibrium inverse demand function $P_t = P\left[N_t q_t/\alpha_t\right]$. We assume that P is twice-differentiable, with $P'(.) < 0$ and $P''(.) \leq 0$, and $\lim_{Q\to\infty} P\left(Q\right) < c < P\left(0\right)$. The assumption $P'(.) < 0$ is simply a requirement that demand curves be downward-sloping. The other assumptions guarantee the existence and the uniqueness of the solution to the profit-maximization problem in the second stage.

Let $\bar{q}_t$ be the average production level of the $N-1$ competitors, then the second-stage maximization problem for the firm is

$$\max_{q_t} \Pi(q_t|\alpha_t, N_t) = P\left[\frac{(N_t-1)\bar{q}_t + q_t}{\alpha_t}\right] q_t - cq_t.$$

The first order condition for each firm in a symmetric equilibrium is:

$$P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{q_t^*}{\alpha_t} + P\left[\frac{N_t q_t^*}{\alpha_t}\right] - c = 0. \tag{4}$$

For a given α_t and N_t, equation (4) has one and only one solution q_t^*. This follows because the left-hand-side of equation (4) is decreasing in q_t^*, is positive for $q_t^* = 0$, and is negative in the limit as $q_t^* \to \infty$. Given a solution q_t^*, we define the equilibrium profits $\Pi^*(\alpha_t, N_t) = \Pi(q_t^*|\alpha_t, N_t)$.

We now characterize the comparative statics of the equilibrium profits $\Pi^*(\alpha_t, N_t)$ with respect to demand shifts α_t and with respect to the number of firms N_t. First, we consider the impact of an increase in demand α_t for given N_t. In a similar set-up, DellaVigna and Pollet (forthcoming) show that, with constant marginal cost, production increases proportionally with the demand shift: $\partial q_t^*\left(\alpha_t\right)/\partial\alpha_t = q_t^*\left(\alpha_t\right)/\alpha_t$. Using this property, we solve for the derivative of equilibrium profits $\Pi^*(\alpha_t, N_t)$ with respect to the demand shift α_t:

$$\begin{aligned}\frac{d\Pi^*(\alpha_t, N_t)}{d\alpha_t} &= P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\left(\frac{\partial q_t^*}{\partial\alpha_t} - \frac{q_t^*}{\alpha_t}\right)\left(\frac{N_t q_t^*}{\alpha_t}\right) + \left(P\left[\frac{N_t q_t^*}{\alpha_t}\right] - c\right)\frac{\partial q_t^*}{\partial\alpha_t} = \\ &= \left(P\left[\frac{N_t q_t^*}{\alpha_t}\right] - c\right)\frac{q_t^*}{\alpha_t} = \frac{\Pi^*\left(\alpha_t, N_t\right)}{\alpha_t},\end{aligned} \tag{5}$$

where in the second step we used the property $\partial q_t^*\left(\alpha_t\right)/\partial\alpha_t = q_t^*\left(\alpha_t\right)/\alpha_t$ and in the third step we substituted the definition of $\Pi^*(\alpha_t, N_t)$. *Hence,* the derivative of profits with respect to a demand shift α_t is increasing in the level of profits $\Pi^*(\alpha_t, N_t)$, a property we use below.

Similarly, we can derive the comparative statics of the profits with respect to the number of firms N_t, for a given α_t. In Appendix A, we provide bounds on the response of q_t^* to changes in N_t: $-q_t^*/N < \partial q_t^*/\partial N_t < 0$. As the number of firms increases, the production of each firm decreases ($\partial q_t^*/\partial N_t < 0$), but not so much that total production $N_t q_t^*$

may fall ($\partial q_t^*/\partial N_t > -q_t^*/N_t$). (The response of total production to an increase in q_t^* is $\partial(N_t q_t^*(N_t))/\partial N_t = N_t\left[\partial q_t^*/\partial N_t + q_t^*/N_t\right]$). Using these results, we establish that equilibrium profits $\Pi^*(\alpha_t, N_t)$ are a decreasing function of the number of firms N_t, a property we use below:

$$\frac{d\Pi^*(\alpha_t, N_t)}{dN_t} = P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\left(\frac{\partial q_t^*}{\partial N_t} + \frac{q_t^*}{N_t}\right)\left(\frac{N_t q_t^*}{\alpha_t}\right) + \left(P\left[\frac{N_t q_t^*}{\alpha_t}\right] - c\right)\frac{\partial q_t^*}{\partial N_t} < 0. \tag{6}$$

The inequality follows since both terms in expression (6) are negative, the first because $\partial q_t^*/\partial N_t > -q_t^*/N_t$ and the second because $\partial q_t^*/\partial N_t < 0$.

Entry. Going back to the first stage of period t, we consider the entry decision. In the non-Embargo state, there are no fixed costs of entry. If all firms enter, that is, $N_t = N^H + N^L$, the firms earn expected profits $E\pi_N = \int \Pi^*\left(\alpha_t, N^H + N^L\right) dF_N(\alpha)$. We assume $E\pi_N \geq 0$, that is, firms earn non-negative profit in the case of full entry. This implies full entry: $N_t^* = N^H + N^L$. The profits $E\pi_N$ are the same for high- and low-cost firms.

In the Embargo state, instead, the entry costs differ across the two types of firms. We assume that the cost K_E^H is high enough to deter entry of high-cost firms, that is, $K_E^H > E\pi_E = \int \Pi^*\left(\alpha_t, N^L\right) dF_E(\alpha)$. The high-cost firms, hence, earn zero profits in the Embargo state: $E\pi_E^H = 0$. The low-cost firms, instead, face no costs of entry, and find it optimal to enter, that is, $N_t^* = N^L$. The profits $E\pi_E^L$ under Embargo are higher than the profits under Non-Embargo $E\pi_N$ for two reasons: (i) the demand for arms α_t in the Embargo state first-order stochastically dominates the demand for arms in the Non-Embargo state; (ii) entry N_t in the Embargo state is lower than in the Non-Embargo state. Since higher demand α_t and lower entry N_t both raise profits, $E\pi_E^L > E\pi_N$ follows.

This identifies the parameters $E\pi_E$ and $E\pi_N$ in expressions (3). To obtain the parameters $\pi_N'(\alpha_t)$ and $\pi_E'(\alpha_t)$, we use expression (5). In the case of Non-Embargo, we obtain $\pi_N^{L\prime}(\alpha_t) = \pi_N^{H\prime}(\alpha_t) = \pi_N(\alpha_t)/\alpha_t > 0$: both types of firms have an equal and positive derivative of profits with respect to demand shifts. In the case of Embargo, the high-cost firms do not produce ($\pi_E^{H\prime}(\alpha_t) = 0$), while the low-cost firms produce and earn profits: $\pi_E^{L\prime}(\alpha_t) = \pi_E(\alpha_t)/\alpha_t$. Given $\pi_E(\alpha_t) > \pi_N(\alpha_t)$, it follows that $\pi_E^{L\prime}(\alpha_t) > \pi_N^{L\prime}(\alpha_t)$ for all α_t. Hence, the profits for low-cost companies are more responsive to demand shifts under an embargo than outside of an embargo.

2.3 Predictions

We combine equations (3) with the expressions for π_E and π_N to derive separate prediction for the high-cost and low-cost companies. For high-cost companies, since $\pi_E^{H\prime}(\alpha_t) = 0$ for all α_t and $E\pi_E^H = 0$, we obtain

$$\frac{\partial V_E^H(\alpha_t)}{\partial \alpha_t} = -\delta P_{E,E}'(\alpha_t)\frac{E\pi_N}{1-\delta\left(EP_{E,E} - EP_{N,E}\right)} \leq 0; \tag{7}$$

$$\frac{\partial V_N^H(\alpha_t)}{\partial \alpha_t} = \frac{\pi_N(\alpha_t)}{\alpha_t} - \delta P_{N,E}'(\alpha_t)\frac{E\pi_N}{1-\delta\left(EP_{E,E} - EP_{N,E}\right)} \gtrless 0. \tag{8}$$

In the case of Embargo (expression (7)), an increase in demand α_t unambiguously lowers the value of arms companies in 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 lifted in the future. In the case of non-Embargo (expression (8)), instead, the increase in demand has two opposing effects: it increases the current demand and hence the current profits (as captured by $\pi_N(\alpha_t)/\alpha_t$), but, as in the case of Embargo, it also increases the future likelihood of an embargo, reducing profits. The sign of expression (8) is therefore ambiguous.

We then obtain the corresponding predictions for low-cost companies. We express the results for low-cost companies as differences from the corresponding values for high-cost companies. This corresponds to the empirical test in Section 4. Using equations (3), we obtain

$$\frac{\partial V_E^L(\alpha_t)}{\partial \alpha_t} - \frac{\partial V_E^H(\alpha_t)}{\partial \alpha_t} = \frac{\pi_E^L(\alpha_t)}{\alpha_t} + \delta P'_{E,E}(\alpha_t)\frac{E\pi_E^L}{1-\delta\left(EP_{E,E}-EP_{N,E}\right)} > 0 \quad (9)$$

$$\frac{\partial V_N^L(\alpha_t)}{\partial \alpha_t} - \frac{\partial V_N^H(\alpha_t)}{\partial \alpha_t} = \delta P'_{N,E}(\alpha_t)\frac{E\pi_E^L}{1-\delta\left(EP_{E,E}-EP_{N,E}\right)} > 0 \quad (10)$$

Compared to high-cost companies, low-cost ones respond substantially more positively to a demand shift during embargo (expression (9)). 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 same companies respond positively, but less markedly, to shifts in demand in the Non-Embargo state (expression (10)). 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.

We summarize the main testable predictions, assuming that the shift in demand for arms α_t is due to shifts in the intensity of conflict.

Prediction 1 (Events in the Embargo State). *Increases in the intensity of a conflict (higher α) 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).*

Prediction 2 (Events in the Non-Embargo State). *Increases in the intensity of a conflict (higher α) 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 test these predictions using stock returns for arms-producing companies as the measure for changes in the value of the companies. Before we do that, we discuss the robustness of these predictions issues to changes in the model.

Robustness. We have assumed that the low-cost firm pays no cost for entry in the

Embargo state. More generally, the cost of entry K_E^L could be positive. This would lower the total profits from the embargo for the low-cost firms, but it would not affect Predictions 1 and 2 as long as the entry cost is smaller than the profits $E\pi_E^L$.

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.

Despite the increased use over time, arms embargoes are still uncommon compared to the scale of contemporary conflicts. According to the *Stockholm International Peace Research Institute* (SIPRI), there were fifty-seven major armed conflicts in the decade 1990-2001, yet an arms embargo was imposed only in eight of them.[1]

The imposition of arms embargoes is an imperfect policy tool. Investigations point to several instances of violations of the embargoes (Wood, 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. The limited powers of the UN in this regard also imply that 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 indirect evidence on embargo violations that relies on investor knowledge can usefully complement the limited direct evidence from investigations.

We start by considering all arms embargoes imposed by the UN Security Council after 1975, as listed in Table 1. We then restrict our attention to embargoes satisfying four criteria: (i) The embargo imposition must date after 1980, so we can find stock price data for a significant number of arms producing companies. (ii) The embargo must occur in a country in which conflict took place. This is because our identification strategy relies on news regarding the evolution of the conflict. (iii) The embargo must last long enough that we can identify at least one salient and unexpected conflict event during the embargo period. (iv) No massive

[1] The number of embargoes here is lower than the total number imposed by the UN Security Council, as some of the embargoes imposed where not on countries in which "major armed conflicts" were taking place according to SIPRI's definition.

UN or US intervention must have occurred in the conflict, because we want to rule out the possibility that stock price effects reflect legal sales to these actors. [2] The final embargo data set includes seven African countries (Angola, Ethiopia and Eritrea, Liberia, Rwanda, Sierra Leone, Somalia, Sudan) and Former Yugoslavia, as listed in table 1.

Events. For each of these eight countries, we search for events affecting the intensity of conflict, occurring both inside the embargo and outside the embargo. We follow three criteria: (i) the event is important enough to attract the interest of media and investors; (ii) the event is, to a first approximation, unanticipated; (iii) the event unambiguously increases or diminishes the intensity (and expected duration) of the conflict. To select the events, we combine a qualitative reading of the history with a quantitative evaluation of criteria (i) and (ii). We count the newswire stories in Lexis-Nexis that mention the name of the country under embargo in the days surrounding the event.[3] As a measure of (i), we define the Event Importance i_t as the average of the news stories on the day of and the day after the event: $i_t = (n_t + n_{t+1})/2$, where n_t is the number of stories on day t, and t is the event day. As a measure of (ii), we define the Event Surprise s_t as the ratio of the Event Importance to the average daily number of stories in the four days preceding the event: $s_t = [(n_t + n_{t+1})/2]/[(n_{t-1} + n_{t-2} + n_{t-3} + n_{t-4})/4]$. We keep 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 takes into account qualitative factors, in Table 5 we examine the robustness of the result to the use of purely quantitative event selection procedures.

Appendix Table A1 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 (mostly, before) the embargo, which we use in Table 6. As an example, we consider the case of Sierra Leone, a country under arms embargo from October 8, 1997. Three significant events occurred in the pre-embargo period: two coup attempts and an important election. In the embargo period, we identify four events. First, on March 10, 1998, the elected president of Sierra Leone, Ahmad Kabbah, returned to his country after being forced out by a coup. This event is likely to diminish the hostilities. A few months later, on January 9, 1999, an unsuccessful coup attempt signalled an aggravation of the hostilities. Third, on May 18, 1999, the government forces and the rebels signed a cease-fire agreement, decreasing the hostilities. Finally, on May 17, 2000, the leader of the rebels was captured, leading to diminished hostilities.

Companies. We use two sources of information on arms-producing companies. The first

[2] From the full list of embargoes shown in table 1, criterion (i) eliminates South Africa, criterion (ii) eliminates Libya, criterion (iii) eliminates Haiti, and (iv) eliminates Afghanistan and Iraq.

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

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), 3761, 3764, and 3769 (missiles), 3795 (tanks), and 2892 (explosives).[4]

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 Datastream.[5]

Table 2 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. Appendix Table A2 reports the full list of companies with the number of non-missing observations and the source of data.

Measures of Cost of Embargo Violation. Following the model, we collect information on company characteristics that affect the cost of embargo violation. In particular, we collect proxies for the ease with which companies may circumvent international restrictions on the flow of arms, for the likelihood that companies may be caught breaching the embargo, and for 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. We use this information to construct measures of low cost of embargo violation.

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 of "corrupt" countries 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. We use the indicator variable as our benchmark measure, but also examine the robustness to using the continuous variable.

Second, 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

[4] One limitation of the data is that the data set does not include a dynamic SIC code; hence, we classify companies based on the most recent SIC code.

[5] 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 publicly traded companies present in Datastream. Some of them overlap with our 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 9.

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. As for the other variables, we use this average score to construct both a discrete and a continuous measure of low transparency (low cost of embargo violation). This variable has the advantage of being closely linked to transparency in the arms sector, and the disadvantage of not being available for some of the countries in our sample.

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

Fourth, we use the measure of press freedom provided by *Freedom House* over the years 1994-2004. Countries with a less free press will be 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 variable for below-median press freedom and standardize the continuous variable.

A fifth 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.[6] 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). One shortcoming of the BPI is that it is only available for 30 countries and does not cover some of the countries in our sample. We define a discrete and continuous variable using the same methodology as for the corruption variable.

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

In Table 2 we list separately the companies in OECD markets and non-OECD markets, and we indicate whether the countries where the companies are head-quartered belong to countries with low cost of embargo violation according to the measures above.

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

[6]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.

top and bottom 2/10,000th of returns to avoid extreme outliers. Finally, we drop returns that are zero for ten consecutive days, since this likely indicates a stale price series.

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

$$r_{i,t} = \alpha_i + \beta_i r_{m(i),t} + \varepsilon_{i,t}, \tag{11}$$

where $r_{i,t}$ is the return of company i on day t and $r_{m(i),t}$ is the 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: $e_{i,t}^{(-1,1)} = e_{i,t-1} + e_{i,t} + e_{i,t+1}$. For a robustness check, we also use 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.[7] For events occurring in the weekend, we shift the event date to the Monday following the weekend.

4 Event Studies

In this Section, using an event study methodology, we estimate whether on average conflict events affect stock returns for all arms companies and for companies of a particular type.

Graphical evidence. In Figure 1a, we plot the average 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.

In correspondence of the 10 events (996 return observations) diminishing hostilities, stock returns increase on average by .03 percentage points, a small change. In correspondence of the 8 events (790 return observations) increasing hostilities, stock returns diminish by on average .05 percentage points. On the remaining trading days, returns are precisely estimated to be zero, as one would expect given that the returns are market-corrected. Overall, therefore, arms-producing companies do not appear to respond to events affecting conflicts during embargoes. Prima facie, we find no evidence on aggregate of investor-backed illegal arms trading.

[7] In Appendix Table A3 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).

This pattern, however, could simply reflect the opposing effects outlined in Section 2. Companies expecting to trade arms legally after the end of the embargo are hurt by increases in conflicts, while companies trading illegally are benefited. To the extent that the expected profits and gains average approximately to zero across the two types of companies, we would indeed expect to find no impact of the events on stock returns.

If we knew which companies trade legally and which ones do not, of course, we would simply separate them into two groups, and estimate the returns separately. Since the identity of the companies exporting illegally (if any) is unknown, 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 with above- or below-median corruption level 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 large that in above-median-corruption countries, such as Italy, China, or South Africa.

Figure 1b presents the results of Figure 1a separately for the two groups of companies. For the companies in low-corruption countries, the events diminishing hostilities have a positive impact on returns (.32 percentage points, 709 observations), while the events increasing hostilities are associated with -.54 percentage point lower returns (576 observations). 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 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 fact that the returns are larger for an increase 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)).

Finally, in Figure 1c 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 (Appendix Table A1), 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.

Benchmark Results. In Table 3, we present the results in a format that allows us to evaluate the statistical significance of the findings on the events inside the embargo. Column (1) and (2) of Table 3 replicate the content of Figures 1a and 1b. We estimate the specification

$$e_{i,t}^{(-1,1)} = \alpha + \gamma_1 \mathbf{1}_{\{Emb_t=1\}} + \gamma_{-1} \mathbf{1}_{\{Emb_t=-1\}} + \alpha_D D_i + \gamma_1^D \mathbf{1}_{\{Emb_t=1\}} D_i + \gamma_{-1}^D \mathbf{1}_{\{Emb_t=-1\}} D_i + \eta_{i,t}. \tag{12}$$

where $e_{i,t}^{(-1,1)}$ is the 3-day abnormal return for company i on date t, and 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, the measure of low cost of embargo violation. The standard errors are robust to heteroskedasticity and clustered by company, so as to allow for arbitrary correlation of returns over time within a company.[8]

In Column (1) we estimate specification (12) imposing no differential effect for companies in high- and low-corruption countries ($\gamma_1^D = \gamma_{-1}^D = \alpha_D = 0$), as in Figure 1a. The constant $\hat{\alpha} = -.0001$ indicates that, in absence of events, the average return is zero. Compared to this return, events increasing hostilities slightly lower returns ($\hat{\gamma}_1 = -.0009$) and events decreasing hostility slightly raise returns ($\hat{\gamma}_{-1} = .0010$), but neither estimate is significant. Across all companies, there is no significant response to events affecting hostilities during an arms embargo.

In Column (2) of Table 3 we allow for a differential effect in high- and low- corruption countries, as in Figure 1b. For companies in low-corruption countries, events increasing war significantly lower returns by .53 percentage points ($\hat{\gamma}_1 = -.0053$) and events decreasing war raise returns by a (marginally significant) .33 percentage points ($\hat{\gamma}_{-1} = .0033$). Compared to these estimates, in companies with high-corruption countries events that increase war raise returns significantly by an additional 1.62 percentage points ($\hat{\gamma}_1^D = .0162$); doing the same comparison, events that decrease war lower returns by a (marginally significant) .79 percentage points ($\hat{\gamma}_{-1}^D = .0079$). The patterns in Figure 1b are, therefore, (mostly) statistically significant.

These estimates also indicate that we cannot reject the hypothesis that the response to events increasing and decreasing hostilities is symmetric: $\gamma_1 = -\gamma_{-1}$ and $\gamma_1^D = -\gamma_{-1}^D$. To gain additional power, in the following specifications we impose these restrictions. In Column (3) we estimate the benchmark specification

$$e_{i,t}^{(-1,1)} = \alpha + \gamma Emb_t + \alpha_D D_i + \gamma^D Emb_t D_i + \eta_{i,t}. \tag{13}$$

[8] As we show in Appendix Table 3, the standard errors are similar if we instead cluster the results by date.

The parameter γ captures the average effect of an event during embargo for companies in low-corruption countries, while the parameter γ^D captures the differential effect for companies in high-corruption countries. An event raising hostilities during embargo lowers stock returns significantly by .42 percentage points for companies in low-corruption markets ($\hat{\gamma} = -.0042$), 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.

An alternative estimation procedure for event studies includes only event days. To test whether this alternative procedure affects the results, in Column (4) we estimate specification (13) only on event days; this requires setting $\alpha = \alpha^D = 0$. We obtain 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. This allows us to test that the returns are on average zero on non-event days.

Finally, in Columns (5) and (6) we test whether the effect of corruption is limited to less developed countries. We measure development with OECD membership and estimate specification (13) separately for companies in OECD and non-OECD countries. In the OECD sample (Column (5)), returns are significantly negative ($\hat{\gamma} = -.0043$) for low-corruption countries, and significantly more positive ($\hat{\gamma}^D = .0064$) for the high-corruption countries (Japan, Italy, Spain, Belgium, and Greece). In the non-OECD sample (Column (6)), there is no negative effect for low-corruption countries ($\hat{\gamma} = .0001$), and the effect is larger for high-corruption countries ($\hat{\gamma}^D = .0131$), though the estimates are imprecise: in this group of countries only Israel, Chile, and Singapore are classified in the low-corruption group.

Overall, we find evidence suggesting that, on average, investors expect arms companies in low-corruption countries to trade legally, but firms in high corruption countries to trade illegally. The magnitudes of the coefficients are economically quite large.

Measures of Cost of Embargo Violation. So far, we examined the impact of above-median corruption. In Panel A of Table 4, we re-estimate specification (13) using alternative discrete measures D_i of low cost of embargo violation, presented in Section 3. In addition, in Panel B of Table 4, 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 the corruption measure of Table 3. In Panel B, we obtain similar 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 ($\hat{\alpha}_D = .0066$).

In Column (2), 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 (Table 2). As Column (2) in Table 4 shows, 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 (3) we use membership in the OECD in 1985 as an alternative proxy. Membership in an international organization like the OECD[9] is likely to 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. Illegal arms trade appears to be more common on average for companies in non-OECD countries.

In Column (4), 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). Taken at face value, this suggests that the role of freedom of the media may not be as important, though we cannot reject estimates of the size of Columns (1)-(3). We obtain similar results using a measure of propensity of managers to pay bribes that we employ in Column (5).

Finally, in Column (6) 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, 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 surrounding the events. In Table 5, we examine the robustness of the results to the definition of the events.

[9]We take the OECD countries as of 1985, the year of the beginning of sample.

In Column (1) of Table 5 we reproduce the benchmark results using the standard set of 18 events during the embargo. In Column (2) we present the results for a specification that uses a broader set of 35 events during the embargo. This set includes, in addition to the 18 events in the standard definition, 17 other events 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 estimate the impact of the events including 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 more significant events. These events lead to 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_i (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 weight 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_i. Out of the broad sample of events, in Column (6) we use the 21 events with $i_t \geq 10$ and $s_i \geq 2$, and in Column (7) the 10 events with $i_t \geq 20$ and $s_i \geq 3$. 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. Hence, we use the core set of 18 events in the remainder of the paper.

Robustness. In Appendix Table A3 we consider a series of robustness checks of the benchmark results. In Column (1) we examine the role of cross-sectional correlation and show that the standard errors are very similar when we cluster by date, and hence allow for autocorrelation across companies on any date. Since the results are similar, in the next specifications we cluster standard errors by company. In Column (2) 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. Then, we show that the results are comparable in the two (overlapping) samples of arms-producing companies: the companies in Worldscope (Column (3)) and the companies identified by SIPRI (Column (4)). Finally, 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 (5)) or using returns net of the market ($r_{i,t}^{(-1,1)} - r_{m,t}^{(-1,1)}$, Column (6)).

Events Outside the Embargo. The model in Section 2 yields predictions on the returns associated with events outside the embargo. The response to events increasing hostilities outside an embargo is ambiguous for high-cost companies (Prediction 2.(i)): these events lead to a contemporaneous increase in profits, but also to an expected loss from the increased likelihood of embargo imposition. Compared to the effect for high-cost companies, the effect for low-cost companies is positive (Prediction 2.(ii)): low-cost companies have more to gain from the increased likelihood of embargo imposition. However, this latter effect is arguably likely to be small, since changes in the probability of embargo imposition are likely to be small.

To test these predictions we estimate the following augmented version of equation (13):

$$e_{i,t}^{(-1,1)} = \alpha + \alpha^D D_i + \gamma Emb_t + \gamma^D Emb_t * D_i + \delta Out_t + \delta^D Out_t * D_i + \eta_{i,t}. \quad (14)$$

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 occurring outside the embargoes: (i) 14 events occurring outside the embargo period for the same eight countries in which embargoes were eventually imposed (Appendix Table A1); (ii) 19 events affecting conflict for countries which experienced conflict but not an arms embargo: Algeria, Haiti[10], Venezuela, Tajikistan, Central African Republic, Ivory Coast, Democratic Republic of Congo, and Togo. We denote this second set of events as "Events in countries without embargo".

The results are displayed in Table 6. In Column (1) we estimate specification (14) on the aggregate, without distinguishing between high- and low-cost companies (that is, we set $\alpha^D = \gamma^D = \delta^D = 0$). We do not find a significant effect for events occurring in embargoed countries, and we find a small positive effect on profits for events in non-embargoed countries. Once we distinguish between companies with high and low cost of embargo breach (Columns (2) and (3)), the effect of events outside the embargo is not significant with either data set of events. The lack of a significant effect for firms with high cost of embargo violation is consistent with Prediction 2.(i)) of the model. As we discussed above, events outside the embargo have two opposing effects on profits, which can cancel out. The model, however, also predicts a more positive response for firms with low costs of embargo violation (Prediction 2.(ii)), which we do not find. However, the difference between high- and low-cost firms is likely to be small, so that we may not be able to detect it empirically.

These results help us rule out one potential explanation of our main result, i.e. the possibility that the higher demand for arms for low-cost companies was generated not as a consequence of embargo violations but rather from the demand of neighboring countries responding to generalized political instability in the region. If this were the case, we should find similar effects for events occurring outside the embargo. Table 6 shows that this is not the case.

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

Timing. In Table 7, we return to our main specification and investigate on which day the stock returns incorporate the information of the event. We run specifications as in (13), except that the dependent variable is a 1-day abnormal return at different windows around the event.

We find that 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 suggest that the coding has a fair degree of accuracy. Somewhat surprisingly, the impact for the low corruption countries (γ) occurs more on the day before than on the other days.

Placebos. An alternative interpretation of the results above 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 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 8 present the results of these placebo regressions. The specification is as in (13), 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 any effects on stock returns. For example, Column (1) of Table 8 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. Across the six specifications in Column (1) through (6), we cannot reject the null hypothesis of no effect on the coefficient γ^D. We can reject instead the hypothesis of no effect for the coefficient γ in Columns (2) and (6), albeit in opposite directions.

While in Columns (1) through (6) we exploit the timing of the effects to generate placebo treatments, in Column (7) we exploit market returns to present another placebo treatment. We replicate the specification (13) 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 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. The coefficient γ_1 is essentially zero, suggesting no different response in high- and low-corruption countries to events during embargo in this placebo treatment.

While one can never reject the possibility that an omitted variable is causing the findings in Table 3, a systematic mis-specification of the model does not appear to be responsible for the results.

Firm Characteristics. We now estimate how the event returns depend on two firm characteristics: firm size and type of arms produced. In Columns (1) and (2) of Table 9 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 a return response to events occurring in countries under embargo, 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 (13) separately for companies with SIC codes in the range 3482-3484, and 3489 (small arms, 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 result is generally present in all types of arms, but is strongest for companies producing small arms and ammunitions, missiles and explosives.

In Column (5), we provide a second test of the impact of war events for small arms. We re-estimate specification (13) on the sample of companies that the non-profit organization NISAT identifies as responsible for small arms sales. (Additional details are in the Section 3.) The sample overlaps partially with the sample in Column (1), but it also includes additional companies. Over this sample, the estimate of γ_1 is positive, but not significant ($\hat{\gamma}_1 = .0050$).

5 Calibration and Explanations

Calibration. The event returns can be used to compute, under a set of assumptions, the implied profits from legal and illegal arms trading. Before we proceed, we should note that the calibration depends on parameters that are hard to estimate, such as the change in the demand for arms. As such, the calibrated profits come with substantial uncertainty and are meant to indicate an order of magnitude of the effects, as opposed to precise magnitudes.

Expressions (7)-(10) indicate the change in company value due to infinitesimal changes in the demand of arms α_t. Since the observed events involve discrete, as opposed to infinitesimal, changes $d\alpha_t$, we use a linear approximation to obtain the resulting discrete change in value dV: $dV = \partial V/\partial \alpha_t * d\alpha_t$. We note that the following calculations do not assume that companies only sell arms to countries under conflict. We can write the total company value V^{ALL} as the sum of V (the relevant value) and V^{OTH} (the other company value). To the extent that V^{OTH} is orthogonal to shifts in demand due to conflict events (that is, $\partial V^{OTH}/\partial \alpha_t = 0$), $\partial V^{ALL}/\partial \alpha_t$ equals $\partial V/\partial \alpha_t$. Expressions (7) and (9) for the value under embargo V_E imply

$$dV_E^H = -\delta \left(P'_{E,E}(\alpha_t)\, d\alpha_t \right) \frac{E\pi_N}{1 - \delta\left(EP_{E,E} - EP_{N,E}\right)}, \quad (15)$$

$$dV_E^L - dV_E^H = \pi_E^L(\alpha_t)\frac{d\alpha_t}{\alpha_t} + \delta\left(P'_{E,E}(\alpha_t)\,d\alpha_t\right)\frac{E\pi_E^L}{1-\delta\left(EP_{E,E}-EP_{N,E}\right)}. \quad (16)$$

We assume time periods t corresponding to one year and a yearly discount factor $\delta = .95$. We compute $EP_{E,E}$ as the fraction of countries under arms embargo in (at least a part of) year t that is still under arms embargo in year $t+1$. In our core sample of eight countries over the period 1985-2004, we obtain $E\hat{P}_{E,E} = .93$. Similarly, we compute $EP_{N,E}$ as the fraction of countries that are not under arms embargo in (at least part of) year t but that are under arms embargo in year $t+1$: $E\hat{P}_{N,E} = .12$.

The impact of conflict events on the transition probabilities, $P'_{E,E}(\alpha_t)\,d\alpha$ and $P'_{N,E}(\alpha_t)\,d\alpha$, varies substantially across events. The death of a rebel leader in battle, such as the death of Savimbi in Angola, can reduce the probability of embargo $P_{E,E}$ by .5-.7 to close to zero (in Angola, a cease-fire was signed within a month of Savimbi's death after 10 years of civil war, and the embargo was removed within a year). Other events, such as a major battle, may change the probability by a more modest .05 or .10. Events outside the embargo are likely to yield smaller changes, given that the UN is generally reluctant in imposing arms embargoes. As an approximation, we assume $P'_{E,E}(\alpha_t)\,d\alpha_t = .2$ and $P'_{N,E}(\alpha_t)\,d\alpha_t = .05$. (For simplicity, we develop the calibration assuming events increasing conflict, that is, $d\alpha_t > 0$.) Finally, we assume $d\alpha_t/\alpha_t = .2$, that is, events on average cause a 20 percent change in demand for arms.

Given these parameters, and imposing $\pi_N(\alpha_t) = E\pi_N$ and $\pi_E^L(\alpha_t) = E\pi_E^L$, expressions (15) and (16) reduce to $dV_E^H = -.82E\pi_N$ and $dV_E^L - dV_E^H = 1.02E\pi_E^L$. We estimate the changes in value dV_E^H and $dV_E^L - dV_E^H$ using the event returns with corruption as a proxy of the cost of embargo violation (Column (3) of Table 3). The estimated change in value $d\hat{V}_E^H(\alpha_t)$ equals the observed return $-.0042$ for the companies in low-corruption countries, multiplied by the market capitalization, which we measure as the median among the companies in low-corruption countries[11], \$408m: $d\hat{V}_E^H(\alpha_t) = -\$1.71m$. This implies an estimate of the expected yearly profit in the non-Embargo state $E\hat{\pi}_N = \$2.08m$. According to this calibration, hence, the median company in a low-corruption country reaps on average two million dollars of profits yearly for arms trade to a developing country with sustained conflict in a non-embargo period.

Similarly, we calibrate the profits in the Embargo state. The estimated differential change in value $d\hat{V}_E^L - d\hat{V}_E^H$ equals the return .0115 multiplied by the median market capitalization among the companies in high-corruption countries, \$150m: $d\hat{V}_E^L - d\hat{V}_E^H = \$1.72m$. This implies $E\hat{\pi}_E^L = \$1.68m$, that is, the median company in a high-corruption country earns on average 1.7 million dollars of profits for arms trade in defiance of an arm embargo. This estimate is smaller than the estimate of the profits $E\hat{\pi}_N$ outside the embargo simply because the market

[11] This figure is the median market capitalization among all 363,807 return observations for companies in low-corruption countries, expressed in 1982-84 dollars. We use the median rather than the mean since the distribution of the market capitalization is very skewed, and the results of Table 9 indicate that the results are stronger for small firms.

capitalization of companies in high-corruption countries is over two times 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.

As a consistency check, we use these parameters and the estimated values for $E\hat{\pi}_N$ and $E\hat{\pi}_E^L$ to predict the returns to events outside the embargo, estimated in Table 6. Multiplying expressions (8) and (10) by $d\alpha$ and evaluating them at the parameters yields $dV_N^H = -.01E\pi_N$ and $dV_N^L - dV_N^H = .21E\pi_E^L$. Using the estimated $E\hat{\pi}_N$ and $E\hat{\pi}_E^L$ and the same market capitalization figures as above, the predicted returns are $\hat{r}_N^H = 0$ and $\hat{r}_N^L - \hat{r}_N^H = .0023$. We compare these to the estimated returns in Table 6, Column (2). The returns to the two sets of events outside the embargo in high-cost (low-corruption) companies are .0003 and .0023, none of which is significantly different from $\hat{r}_N^H = 0$. The differential returns for low-cost (high-corruption) companies are -.0008 and .0008, none of which is significantly different from $\hat{r}_N^L - \hat{r}_N^H = .0023$. The parameters estimated from the response to events during the embargo, therefore, are broadly consistent with the observed response to events outside the embargo.

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 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 rate.

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 are not importing any new weapons but just depleting existing ones, the increased depletion will generate a positive demand shift for weapon companies at some point in the future, when the depleted stock will have to be replenished. The future purchases may take place once the embargo is over. This explanation, however, does not explain why the effect of the events is significant under embargo, but not outside the embargo. This story would predict that demand shifts would increase demand in a similar way. It also does not explain the difference in response between companies with low- and high-cost of embargo violation.

Investor beliefs. Our detection-procedure is based on the assumption that the marginal investor is well-informed. It is possible that there is no illegal arms trading, but the marginal investor is mis-informed, and reacts as if there were trade. While we cannot test for investor rationality, it is plausible that investors close to the top management would know if illegal arms trade takes place, and they would have strong incentives to trade in the days of conflict events.

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.

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.

Input and Product Mix. An event may cause an increase in demand not only for the weapons produced in 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.

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 (Jacob and Levitt, 2003; Duggan and Levitt, 2002; 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 time an 'anomalous' reaction occurs and generate a list of companies and events that the returns suggest may be embargo violators. While we do not reveal the identity of individual firms, we illustrate the characteristics of the firms that this methodology singles out. Before detailing the procedure, we should note 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 $e_{i,t}^{(-1,1)} = e_{i,t-1} + e_{i,t} + e_{i,t+1}$. To obtain the abnormal returns $e_{i,t}$, we estimate the market model (11) 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 a measure of chains of reactions. As suggested by the model in Section

[12]This procedure differs slightly from the market correction procedure adoped in the previous Section where, for computational reasons, the market model is estimated over the previous year of data.

2, we isolate three types of reactions to individual events. The first two types of reaction are to events inside the embargo. The first type of reaction 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)). We denote this type of reaction as "*Illegal_React*". The second type of reaction has reversed signs, and occurs when the return of the company is significantly negative (positive) in correspondence of events that increase (decrease) conflict intensity. We denote this reaction as "*Legal_React*" because it is consistent with a company expecting to sell arms legally to the country after the embargo is lifted (Prediction 1.(ii)). Finally, we identify companies that display a statistically significant positive (negative) return when conflict increases (decreases) outside the embargo. This pattern is consistent with the possibility that the company is selling arms to the country (Prediction 2.(ii)). We label this type of reaction as "*Outside_React*".

Table 10 illustrates this categorization for Company A, a company from our sample producing small arms. The first four columns show the company, the countries, and the events considered. Column (5) indicates whether the event occurs during an embargo. Column (6) shows our classification of the effect that the event had on the intensity of conflict, with "+" indicating an increase in conflict and "-" a decrease. Column (7) reports the event return $e_{i,t}^{(-1,1)}$, while Column (8) reports the p-value of the test that $e_{i,t}^{(-1,1)} = 0$ against the one-sided alternative. In Column (8) we classify the type of detected reaction (if any) using the relationship between the sign of conflict intensity (Column (6)) and that of the event return (column (7)), and the presence of embargo (Column (5)).

Company A displays a significant positive abnormal return in correspondence of the first event for Ethiopia, an event where conflict intensity increased before the embargo. We categorize this reaction as *'Outside_React'* (Column (9)). Subsequently, this company stock reacts negatively to the news of the peace treaty signed by Ethiopia and Eritrea, an event lowering conflict during the embargo. This reaction is categorized as *'Illegal_React'*, since it is consistent with illegal arms sales. Interestingly, the same company displays reactions consistent with illegal arms trades also for the conflict in Former Yugoslavia: its abnormal returns are negative when Milosevic is arrested and positive later on when the prime minister of Serbia, Zoran Djindjic, is assassinated.

In a parallel analysis to the one in Section 4, in Table 11 we test whether proxies for the cost of embargo violation predict legal and illegal reactions to the events under embargo. In Panel A we estimate the linear probability model[13]

$$Illegal\_React_{i,t} = \alpha + \alpha_D D_i + \eta_{i,t} \tag{17}$$

where t corresponds to an event occurring during an embargo, i indicates a company, and D_i is a proxy for low cost of embargo violation. We use all the six proxies employed in Table 4.

[13]The results are virtually identical if we use a Probit model.

Using all six measures, we find $\alpha_D > 0$, indicating that illegal reactions are more likely to occur for companies located in countries with low costs of embargo violations, consistently with our previous analysis. However, the estimated coefficients are not statistically significant. In Panel B, we estimate a parallel specification to (17), with a dummy for legal reactions, *'Legal_React'*, as dependent variable. We find that $\alpha_D < 0$, indicating that companies in countries with low cost of violation are less likely to display the behavior that we associate with legal arms trade. This is consistent again with the predictions. The results, however, are significant in only one specification. The lower significance level of these results, compared to the results in Table 4, is likely due to two factors. First, we examine one-sided reactions, pooling no reactions and reactions in the opposite direction in the same category of the dependent variable. Second, we only exploit information on the existence of a reaction, and not on the size of the abnormal return. While the second feature is intrinsic to this procedure, we can address the first issue by combining legal and illegal reactions in the same dependent variable. In Panel C of Table 11, we estimate the OLS specification (17) with a dependent variable equal to +1 if $Illegal\_React = 1$, -1 if $Legal\_React = 1$, and 0 otherwise. Consistent with the predictions of the model, we find $\alpha_D > 0$; the difference between low- and high-cost companies is now significant for the corruption measure and the OECD measure.

Chains of Reactions. Because isolated reactions may be the result of noise, we look for systematic patterns of reactions that are less likely to occur randomly. We focus on 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. For example, in Table 10, Company A has a chain of illegal reactions both in Ethiopia and in Yugoslavia.

Over the whole sample, we find a total of 23 company-country pairs with a chain of illegal reactions (Column (1) of Table 12). 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, the identification of 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 0.60 percent.

Although we do not report the identity of individual company-country pairs detected as potentially illegal, in the rest of Table 12 we provide aggregate statistics by sub-groups. Since the sample size of potential violators is small, this evidence should be considered as suggestive. First, we report in which conflicts these violations occurred to a greater extent. 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 (but only 0.24 percent incidence).

Next, we evaluate whether companies head-quartered in countries with low cost of embargo violation are more likely to belong to the set of potential violators. In absolute levels, there is a higher number of violators among companies located in low-corruption (that is, high-cost) countries: 14 against 9 in high-corruption countries. However, this does not correct for differences in the number of non-missing return observations. Once we take this into account, 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 incidence of chains is instead about the same for low- and high-cost companies when we use the other indices of cost of embargo violation. The results using the corruption index and the arms transparency index indicate a two-fold pattern of the results: (i) 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; (ii) 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.

Finally, we present the results by type of arms produced. The group of arms with highest incidence is tanks (0.95 percent), followed by explosives (0.61 percent) and small arms (0.57%).

7 External Validation

Our interpretation of the above results is that the abnormal returns provide information on the incidence of legal and illegal arms trade, and on the types of companies most likely to engage in this trade. 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 reported in three main sources: the Reports of Panel of Experts, the Reports of the Monitoring Groups, and Selected Documents. For all the eight countries in the sample, we examined the three sets of reports. The violators named in the reports are mostly brokers and intermediaries, such as Pecos (from Guinea) for Liberia (Report 1015e) and Kas Engineering (Gibraltar) for Angola (Report 363e). No traded company in our sample is mentioned in these reports. We interpret this as evidence that detection of trades by larger companies is harder, 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 a given conflict is likely to reflect the concerns about the seriousness of the violations in that conflict. We thus construct two quantitative measures of the number of UN Reports. We group together the reports by the Panel of Experts and by the Monitoring Group, since these are less frequent, and define $MGPE_j$ the total number of these Reports concerning country j, divided by the number of years of the embargo. The average incidence of $MGPE$ across 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 across the eight countries is 1.07, with a minimum of 0 (Rwanda, Somalia, and Sudan) and a maximum of 3 (Liberia). The information refers to the embargoes for which information is available on the UN website.[14]

In Table 13, we use this information to test if, in conflicts with higher incidence of UN reports, companies are more likely to be detected as reacting to the conflict events. Similarly to Table 11, in Panel A of Table 13 we follow specification

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

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 significantly increases the likelihood of an illegal reaction. In Panel B, we examine whether the incidence of UN reports also affects the detection of legal reactions. 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 the 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, depending on the specification, 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 measure of severity of violations.

In addition to official UN documents, unofficial documents and journalistic pieces may also provide evidence of arms trading. We take advantage of information on the Internet and

[14] The variables and the number of years of embargo covered by the data are: Angola ($MGPE = .66$, $SE = 1$, 9 years), Ethiopia/Eritrea ($MGPE = 0$, $SE = 2$, 1 year), Liberia ($MGPE = 1.45$, $SE = 3$, 11 years), Rwanda ($MGPE = 0$, $SE = 0$, 2 years), Sierra Leone ($MGPE = .33$, $SE = 1.89$, 9 years), Somalia ($MGPE = .57$, $SE = 0$, 14 years), Sudan ($MGPE = 3$, $SE = 0$, 1 year), and Yugoslavia ($MGPE = 0$, $SE = .67$, 3 years).

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 Saiz and Simonsohn (2007), who examine the case of measures of corruption and show that Internet-based measures correlate well with standard measures. The advantage of Internet counts is that they can be constructed even when standard measures do not exist, such as for arms trade. 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.[15] Finally, among all the companies with at least 100 hits ($n_i > 100$), we define an indicator variable for the companies (or company-country combinations in the case of $confl_{i,j}$) in the top 10 percent.[16]

In Panel A of Columns (3)-(5) of Table 13 we present the results of regressions of $Illegal\_React_{i,t}$ on the top-10 Google indicators; in Panel B we present the results with $Legal\_React_{i,t}$ as dependent variable. Companies with high arms-related Google counts are more likely to display what we detect as illegal reactions. The pattern is strongest for the counts using the word "Embargo", which is arguably the wording most closely tied to embargo violations. These findings provide some external validation to the return-based detection, albeit an indirect one, since we do not examine the Internet content directly. We do not find any significant evidence of an effect on the detection of legal reactions (Panel B). While this is expected for the "Embargo" measure, it is somewhat surprising for the "arms" measure and the conflict measure.

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 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. We focus on eight countries under UN arms embargo in the period 1990-2005. We consider events during the embargo that suddenly increase or decrease conflict intensity, and examine the contemporaneous stock returns of weapon-making companies. As our simple model predicts, for companies trading legally, an event worsening the hostilities should affect stock prices adversely, since

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

[16]We do not use the continuous variable because it is highly skewed.

it delays the removal of the embargo. Conversely, for companies trading illegally, the event should increase stock prices, since it increases the demand for illegal weapons.

We estimate a large and significant positive reaction for weapon-making companies headquartered in countries where the legal and reputational costs of illegal trades are likely to be lower. We interpret this as evidence that investors believe that some of these latter companies are engaging in illegal arms trades. An event study analysis of individual companies and events points to potential embargo violators, including companies displaying systematic patterns of reactions. While the detection of individual companies is subject to substantial uncertainty, the results of the detection procedure are corroborated by outside evidence from UN reports and Google counts.

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

9 Appendix A

Claim. *For a given α_t, $-q_t^*/N < \partial q_t^*/\partial N_t < 0$* holds.

Proof. *In the unique equilibrium, condition (4) must hold. We can then use the implicit function theorem to obtain $\partial q_t^*/\partial N_t$.* We obtain:.

$$\frac{\partial q_t^*}{\partial N_t} = -\frac{q_t^*}{N_t} \frac{P''\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{q_t^*}{\alpha_t^2} + P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{1}{\alpha_t}}{P''\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{q_t^*}{\alpha_t^2} + P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{1}{N_t \alpha_t} + P'\left[\frac{N_t q_t^*}{\alpha_t}\right]\frac{1}{\alpha_t}},$$

where we have collected q_t^* in the numerator and N_t in the denominator. The condition $\partial q_t^*/\partial N_t < 0$ follows given $P'' < 0$ and $P' < 0$. The condition $\partial q_t^*/\partial N_t > -q_t^*/N$ follows since the second fraction is smaller than 1. **Q.E.D.**

References

[1] Bondi, Loretta. "Externalities of the Arms Trade", in Passas N. and Goodwin N. (eds), *It's Legal but It Ain't Right*, University of Michigan Press, 2004.

[2] Campbell, John, Andrew Lo, and C., MacKinlay, *The Econometrics of Financial Markets*, Princeton University Press, 1997.

[3] Collier, Paul, and Anke Hoeffler. "On the Economic Causes of Civil War", *Oxford Economic Papers,* 50, 563-573, 1998.

[4] DellaVigna, Stefano and Joshua Pollet. "Demographics and Industry Returns" *American Economic Review,* forthcoming.

[5] Djankov, Simeon, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. "The Law and Economics of Self-Dealing", mimeo, 2006.

[6] Duggan, Mark and Steven Levitt. "Winning Isn't Everything: Corruption in Sumo Wrestling." *American Economic Review*, Vol. 92, pp. 1594-605, 2002.

[7] Dunne, J.Paul and Eamon Surry. "Arms production", *SIPRI Yearbook 2006*, Oxford: Oxford University Press, pp. 419-427, 2006.

[8] Fisman, Raymond. "Estimating the Value of Political Connections," *American Economic Review*, Vol. 91, pp. 1095-1102, 2001.

[9] Guidolin, Massimo, and Eliana La Ferrara. "Diamonds Are Forever, Wars Are Not. Is Conflict Bad for Private Firms?", *American Economic Review*, forthcoming.

[10] Hsieh, Chang-Tai, and Enrico Moretti. "Did Iraq Cheat the United Nations? Underpricing, Bribes, and the Oil for Food Program", *Quarterly Journal of Economics*, Vol. 121, 2006.

[11] Jacob, Brian A and Steven Levitt. "Rotten Apples: An Investigation of the Prevalence and Predictors of Teacher Cheating." *Quarterly Journal of Economics*, Vol. 118, pp. 843-77, 2003.

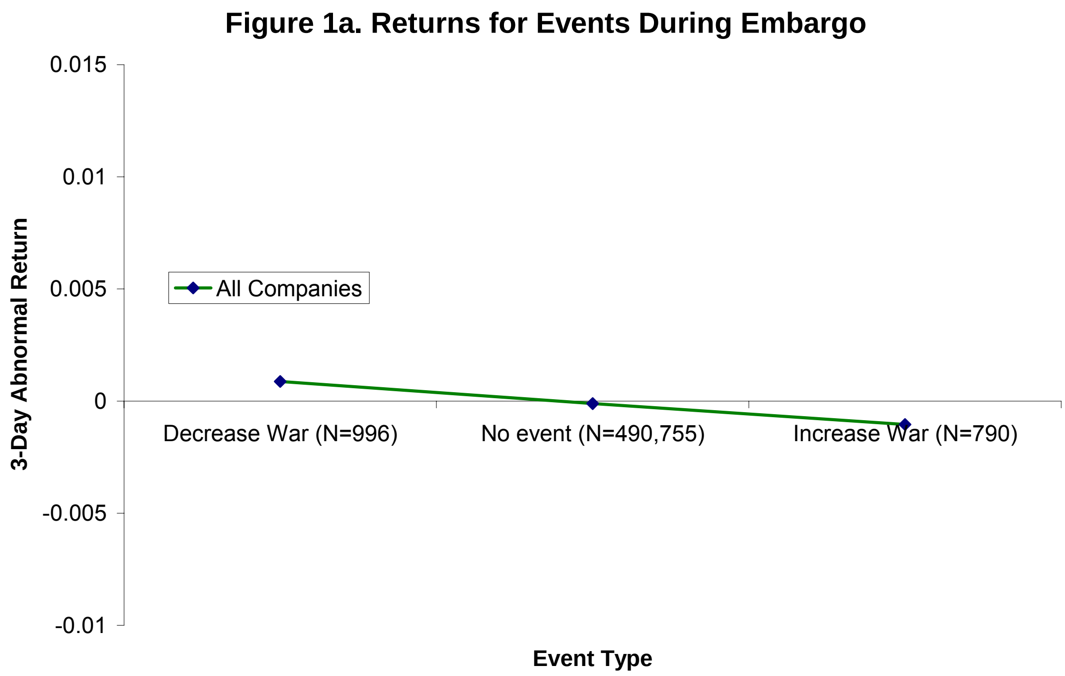
[12] Johnson, Simon and Todd Mitton. "Cronyism and Capital Controls: Evidence from Malaysia," *Journal of Financial Economics,* 67, pp. 351-382, 2003.

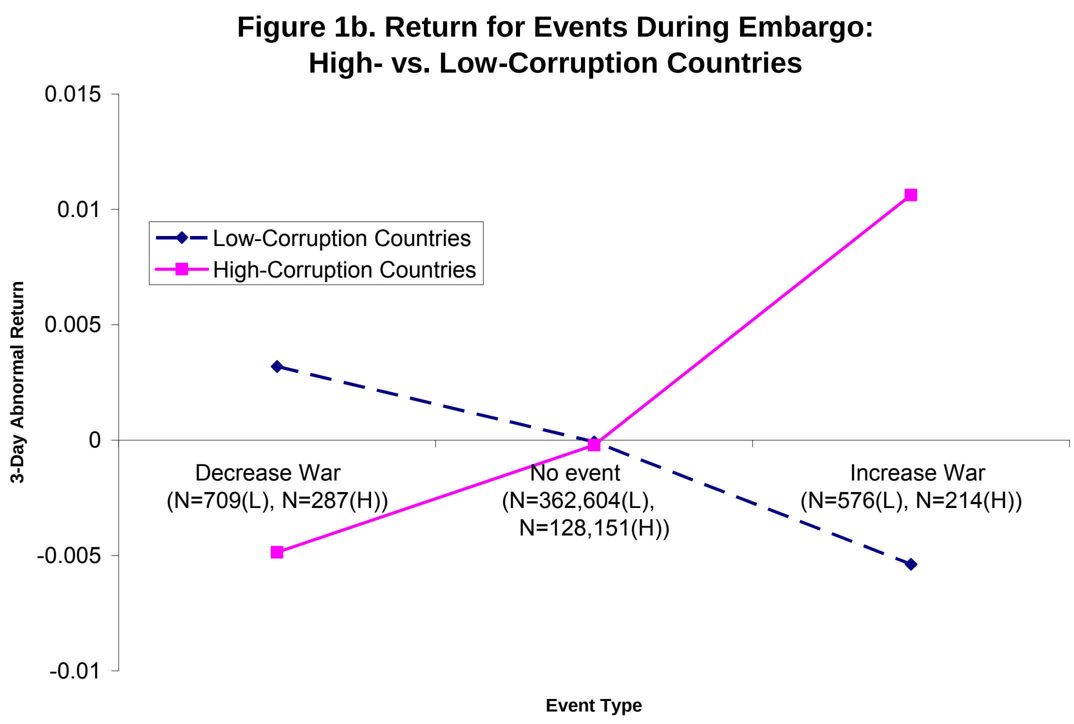
[13] Miguel, Edward, Shanker Satyanath, and Ernest Sergenti, "Economic Shocks and Civil Conflict: An Instrumental Variables Approach", *Journal of Political Economy*, Vol. 112, pp. 725-753, 2004.

[14] Roberts, Brian E. “A Dead Senator Tells No Lies: Seniority and the Distribution of Federal Benefits,” *American Journal of Political Science,* 34, pp. 31-58, 1990.

[15] Saiz, Albert and Uri Simonsohn. “Downloading Wisdom from Online Crowds,” Working paper, 2007.

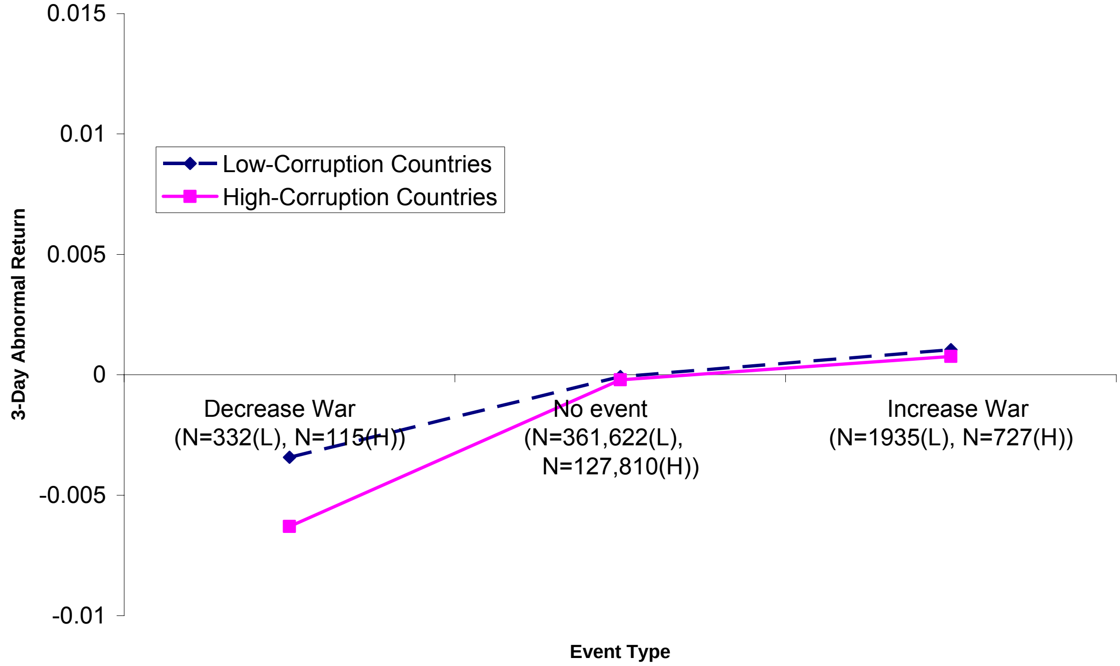
[16] Wolfers, Justin. “Point Shaving: Corruption in NCAA Basketball”. *American Economic Review, Papers and Proceedings,* Vol. 96, 2006.





Note. Figures 1a and 1b display average 3-day abnormal cumulative returns separately for days with events decreasing hostilities, for days with no events, and for days with events increasing hostilities. The events are unexpected, significant occurrences affecting the hostilities during the arms embargo period in one of the 8 countries in the sample. The Figures also report the number of company-day observations over which the return is computed. Figure 1b presents the returns separately for companies headquartered in countries with corruption above- and below-median according to the Corruption-Perceptions Index of *Transparency International*.

Figure 1c. Return for Events Outside Embargo: High- vs. Low-Corruption Countries



Note. Figure 1c displays average 3-day abnormal cumulative returns separately for days with events decreasing hostilities, for days with no events, and for days with events increasing hostilities. The events are unexpected, significant occurrences affecting the hostilities outside the arms embargo period in one of the 8 countries in the sample, or in one of 6 other countries with no arms embargo. The Figures also report the number of company-day observations over which the return is computed. 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. List of Countries with Arms Embargoes

Country	Embargo Target	Date Imposed	Date Lifted	Res. No.	By	Included
(1)	(2)	(3)	(4)	(5)	(6)	(7)
South Africa		11/4/1977	5/24/1994	UNSCR 418	UN	No (too early)
Iraq		8/6/1990	--	UNSCR 661	UN	No (Gulf War)
Former Yugoslavia		9/25/1991	10/1/1996	UNSCR 713	UN	Yes
		3/31/1998	9/10/2001	UNSCR 1160	UN	
Somalia		1/23/1992	--	UNSCR 733	UN	Yes
Libya		3/31/1992	12/9/2003	UNSCR 748	UN	No (no war)
Liberia		11/19/1992	--	UNSCR 788	UN	Yes
Haiti		10/13/1993	10/15/1994	UNSCR 841	UN	No (no event during embargo)
Angola	UNITA	9/15/1993	12/9/2002	UNSCR 864	UN	Yes
Rwanda		5/17/1994	8/16/1995	UNSCR 918	UN	Yes
	Rebels	8/16/1995	--	UNSCR 1011	UN	
Sudan		3/15/1994	9/28/2001	94/165/CFSP	UN	Yes
		7/30/2004	--	UNSCR 1556	UN	
Sierra Leone		10/8/1997	6/5/1998	UNSCR 1132	UN	Yes
	Rebels	6/5/1998	--	UNSCR 1171	UN	
Ethiopia & Eritrea		5/17/2000	5/16/2001	UNSCR 1298	UN	Yes
Afghanistan	Taliban	12/19/2000	--	UNSCR 1267	UN	No (Afghan War)

Notes: The Table lists all embargoes imposed from 1975 on by the United Nations. Column (1) and (2) list the country affectd and the embargo target, if different from the whole nation. Columns (3) and (4) report the date the Embargo was imposed and the date the embargo was lifted, if any. Columns (5) and (6) report the resolution number and the organization issuing the embargo. Finally, Column (7) states whether the embargo is included in the data set in this paper, and if not why.

Table 2. Measures of Cost of Embargo Violation for Countries of Headquarter of Arms Companies

OECD Countries

Country	Number Of Companies Headquart.	High Corruption	Low Transp.	Low Press Freedom	High Bribe-Payer	High Self-Dealing
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Usa	53	No	No	No	No	No
France	14	No	No	Yes	Yes	Yes
Japan	14	Yes	Yes	No	No	No
Uk	10	No	No	No	No	No
Germany	7	No	No	No	No	Yes
Australia	6	No	No	No	No	No
Italy	4	Yes	No	Yes	Yes	Yes
Canada	3	No	No	No	No	No
Switzerland	3	No	Yes	No	No	Yes
Norway	2	No	No	No		Yes
Spain	2	Yes	No	No	Yes	Yes
Austria	1	No	Yes	No	No	Yes
Belgium	1	Yes	Yes	No	No	No
Greece	1	Yes		Yes		Yes
Netherlands	1	No	No	No	No	Yes
Sweden	1	No	No	No	No	Yes
	123					

Non-OECD Countries

Country	Number Of Companies Headquart.	High Corruption	Low Transp.	Low Press Freedom	High Bribe-Payer	High Self-Dealing
(8)	(9)	(10)	(11)	(12)	(13)	(14)
China	5	Yes	Yes	Yes	Yes	No
Brazil	4	Yes	Yes	Yes	Yes	Yes
South Korea	4	Yes	Yes	Yes	Yes	Yes
Malaysia	3	Yes		Yes	Yes	No
South Africa	3	Yes	Yes	Yes	Yes	No
Czech Rep.	2	Yes	No	No		Yes
India	2	Yes		Yes	Yes	No
Israel	2	No	Yes	Yes	Yes	No
Peru	2	Yes		Yes		Yes
Chile	1	No		Yes		No
Russia	1	Yes	Yes	Yes	Yes	No
Singapore	1	No	Yes	Yes	No	No
	30					

Notes: The Table lists all the countries in which the arms-producing companies are head-quartered. Columns (1) and (2) list the countries, and the number of companies in each country, for the OECD countries (OECD membership is defined as of 1985, the beginning of our sample). Columns (3) through (7) present information on whether the country is above the median in the corruption level (according to the CPI index), Column (3)), in low transparency (Column (4)), in low press freedom (Column (5), in high payment of bribes (BPI Index, Column (6)), in high self-dealing (Column (7)). Columns (8) though (14) present the same information for the non-OECD countries. The measures of corruption are defined in the text.

Table 3. Stock Market Reaction to War Events. Benchmark Effects

Dep. Var.:	**Abnormal 3-Day Stock Return (-1,1)**					
	(1)	(2)	(3)	(4)	(5)	(6)
Event Increasing War During Embargo	-0.0009 (0.0021)	-0.0053 (0.0021)**				
Event Decreasing War During Embargo	0.001 (0.0018)	0.0033 (0.0018)*				
Event Increasing War * (High-Corruption Country)		0.0162 (0.0054)***				
Event Decreasing War * (High-Corruption Country)		-0.0079 (0.0046)*				
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)			-0.0042 (0.0013)***	-0.0042 (0.0013)***	-0.0043 (0.0014)***	0.0001 (0.0022)
Event During Embargo* (High-Corruption Country)			0.0115 (0.0036)***	0.0115 (0.0036)***	0.0064 (0.0027)**	0.0131 (0.0065)*
Indicator for High-Corruption Country		-0.0001 (0.0002)	-0.0001 (0.0002)		0 (0.0003)	-0.0004 (0.0007)
Constant	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)		-0.0001 (0.0001)	0 (0.0005)
Include Only Event Days				X		
Sample of Companies	All	All	All	All	OECD	Non-OECD
N	492541	492541	492541	1786	436939	55602

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 (4) only event days are included in the sample. In Column (5) only companies head-quartered in an OECD country are included in the sample. In Column (5) only companies head-quartered in a non-OECD country are included in the sample. OECD membership is defined as of 1995, the first year of the sample. Robust standard errors clustered by company in parentheses.

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

Table 4. 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*	*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)
Panel A -- Indicators for Cost of Embargo Violation						
Event During Embargo	-0.0042	-0.0043	-0.0031	-0.0023	-0.0027	-0.0025
(1=Increase War, -1=Decrease, 0=No Event)	(0.0013)***	(0.0014)***	(0.0012)**	(0.0012)*	(0.0012)**	(0.0013)*
Event During Embargo*	0.0115	0.0114	0.015	0.0061	0.0058	0.0055
(Low Cost of Embargo Violation, Indicator)	(0.0036)***	(0.0038)***	(0.0057)***	(0.0049)	(0.0050)	(0.0041)
Low Cost of Embargo Violation -	-0.0001	-0.0001	-0.0002	-0.0002	0	-0.0002
Indicator	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0002)
Constant	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001	0
	(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	0.0013	0.0025	.	0.0008	0.0007	-0.0005
(1=Increase War, -1=Decrease, 0=No Event)	(0.0020)	(0.0023)	.	(0.0019)	(0.0023)	(0.0016)
Event During Embargo*	0.0066	0.0048	.	0.0039	0.005	0.0016
(Low Cost of Embargo Violation, Continuous)	(0.0029)**	(0.0017)***	.	(0.0021)*	(0.0028)*	(0.0015)
Low Cost of Embargo Violation -	-0.0002	0	.	0	0	-0.0001
Continuous	(0.0002)	(0.0001)	.	(0.0002)	(0.0002)	(0.0001)
Constant	-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	Small Arms Survey	OECD	Freedom House	Transparency International	Shleifer et al.
N	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 company in parentheses.

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

Table 5. 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 (0.0013)***		-0.0036 (0.0016)**				
Event During Embargo * (High-Corruption Country)	0.0115 (0.0036)***		0.0096 (0.0044)**				
Event During Embargo (Broad Def.) (1=Increase War, -1=Decrease, 0=No Event)		-0.0024 (0.0010)**	-0.0006 (0.0012)	-0.002 (0.0013)	-0.0033 (0.0012)***		
Event During Embargo (Broad Def.) * (High-Corruption Country)		0.0069 (0.0023)***	0.0019 (0.0027)	0.0109 (0.0036)***	0.0083 (0.0027)***		
Event During Embargo (Autom. Def.) (1=Increase War, -1=Decrease, 0=No Event)						-0.0032 (0.0011)***	-0.0049 (0.0018)***
Event During Embargo (Autom. Def.) (High-Corruption Country)						0.0086 (0.0028)***	0.0104 (0.0047)**
Indicator for High-Corruption Country	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0007 (0.0008)	-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 (0.0003)	-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 company in parentheses.

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

Table 6. 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 (0.0014)	-0.0042 (0.0013)***	-0.0043 (0.0014)***
Event During Embargo * (Low Cost of Embargo Violation)		0.0115 (0.0036)***	0.0114 (0.0038)***
Event Outside Embargo (1=Increase War, -1=Decrease, 0=No Event)	0.0001 (0.0013)	0.0003 (0.0016)	0 (0.0017)
Event Outside Embargo * (Low Cost of Embargo Violation)		-0.0008 (0.0027)	0.0005 (0.0025)
Event in Countries without Embargo (1=Increase War, -1=Decrease, 0=No Event)	0.0025 (0.0012)**	0.0023 (0.0014)	0.0023 (0.0014)
Event in Countries without Embargo * (Low Cost of Embargo Violation)		0.0008 (0.0027)	0.0001 (0.0028)
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 company in parentheses.

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

Table 7. Timing of Stock Market Reaction

Dep. Var.:	Abnormal 1-Day Stock Return				
Timing relative to Event:	**(-2,-2)**	**(-1,-1)**	**(0,0)**	**(1,1)**	**(2,2)**
	(1)	(2)	(3)	(4)	(5)
Event During Embargo	0.0003	-0.0019	-0.0009	-0.0013	0.0015
(1=Increase War, -1=Decrease, 0=No Event)	(0.0007)	(0.0009)**	(0.0008)	(0.0009)	(0.0010)
Event During Embargo*	0.0023	0.0022	0.0058	0.0034	-0.0006
(High-Corruption Country)	(0.0022)	(0.0016)	(0.0018)***	(0.0019)*	(0.0019)
Indicator for High-Corruption	-0.0001	-0.0001	0	0	0
Country	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Constant	0	0	0	0	0
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
N	491433	492541	492541	492541	491433

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 1-day stock return for the five days surrounding the event day. 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* . Robust standard errors clustered by company in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. 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)	(0.0012)	(0.0012)*	(0.0012)**	(0.0016)	(0.0013)	(0.0014)**	(0.0003)
Event During Embargo*	0.0014	-0.0047	-0.0042	-0.0022	-0.0017	0.0022	0.0003
(High-Corruption Country)	(0.0027)	(0.0029)	(0.0025)*	(0.0029)	(0.0034)	(0.0029)	(0.0007)
Indicator for High-Corruption	-0.0001	-0.0002	-0.0002	-0.0001	-0.0001	-0.0001	-0.0004
Country	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0001)***
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)*	(0.0000)***
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 company in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. 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	Missiles	Tanks	Explosives	Small Arms NISAT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event During Embargo	-0.01	-0.0024	-0.0048	-0.0057	-0.0049	-0.0077	-0.0006
(1=Increase War, -1=Decrease, 0=No Event)	(0.0034)***	(0.0013)*	(0.0028)*	(0.0023)**	(0.0031)	(0.0073)	(0.0019)
Event During Embargo*	0.02	0.0075	0.0099	0.029	0.0046	0.0137	0.005
(High-Corruption Country)	(0.0057)***	(0.0048)	(0.0044)**	(0.0169)*	(0.0043)	(0.0089)	(0.0053)
Indicator for High-Corruption	-0.0003	0	-0.0001	0.0004	0.0001	-0.0002	0.0005
Country	(0.0005)	(0.0003)	(0.0005)	(0.0006)	(0.0006)	(0.0004)	(0.0004)
Constant	-0.0001	-0.0001	-0.0002	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0003)	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)
Sample of Companies	All	All	Worldscope	Worldscope	Worldscope	Worldscope	NISAT
N	132699	355898	133316	113998	43061	58395	141194

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 any 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, 3761, 3764, and 3769 for missiles, 3795 for tanks, and 2892 for explosives. In Column (7), the sample includes the public companies listed in the sample of small arms companies identified by NISAT. 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 company in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10. Detection methodology, An Example

Company (1)	Country (2)	Event Date (3)	Event Type (4)	UN Embargo (5)	Event and Conflict Intensity (6)	Cumulative 3-day Abnormal Return (7)	P-value of Test CAR=0 (8)	Detected Reaction (9)	Detected Chain Of Reactions (10)
Company A	Ethiopia	02/06/1999	Major Battle	No	+	+0.11	0.031	Outside_React	Chain of Illegal Reactions
Company A	Ethiopia	05/12/2000	Major Battle	No	+	+0.03	0.116	.	
Company A	Ethiopia	12/12/2000	Peace Treaty	Yes	-	-0.05	0.039	Illegal_React	
Company A	Yugoslavia	06/25/1991	Independence	No	+	-0.04	0.111	.	Chain of Illegal Reactions
Company A	Yugoslavia	03/30/2001	Leader Captured	Yes	-	-0.12	0.015	Illegal_React	
Company A	Yugoslavia	03/12/2003	Assassination	No	+	+0.12	0.015	Outside_React	
Company A	Yugoslavia	03/17/2004	Start Fighting	No	+	+0.03	0.161	.	

Notes: "Event and Conflict intensity" in Column (6) is coded as "+" when the event increases demand for arms and "-" when it decreases it. The cumulative 3-day abnormal return in Column (7) is calculated using an event window of (-1,+1) days around the event and an estimation window of 100 trading days. The p-value in Column (8) is computed using use the parametric tests of no abnormal returns of Campbell et al. (1997). In Column (9) we report whether the abnormal return leads to a detected reaction: 'Illegal_React' denotes the case in which the return significantly increases (decreases) at the 10 percent level when conflict increases (decreases) during the embargo; 'Legal_React' denotes the case in which the return significantly decreases (increases) when conflict increases (decreases) at the 10 percent level 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 (10) we identify Chains of Illegal reactions 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'.

Table 11. Detection: Illegal vs. Legal Reactions and Cost of Embargo Violation

Measure of Cost of Embargo Violation:	*High Corruption Perceptions Index* (1)	*Low Transparency of Arms Trade* (2)	*Non-OECD Member* (3)	*Low Press Freedom* (4)	*High Bribe-Payer Index* (5)	*High Self-Dealing Index* (6)
Panel A -- Dep.var.: 1 if illegal reaction; 0 otherwise						
OLS coefficients						
Low Cost of Embargo Violation (Indicator)	0.022	0.0167	0.0384	0.027	0.0222	0.0201
	(0.0164)	(0.0181)	(0.0245)	(0.0192)	(0.0199)	(0.0171)
Constant	0.0759	0.0768	0.0763	0.0758	0.0749	0.0766
	(0.0072)***	(0.0073)***	(0.0064)***	(0.0066)***	(0.0069)***	(0.0070)***
Panel B -- Dep.var.: 1 if legal reaction; 0 otherwise						
OLS coefficients						
Low Cost of Embargo Violation (Indicator)	-0.0238	-0.0299	-0.0407	-0.001	-0.0012	-0.0016
	(0.0149)	(0.0154)*	(0.0172)**	(0.0161)	(0.0164)	(0.0139)
Constant	0.0986	0.1016	0.0981	0.0921	0.0934	0.0923
	(0.0085)***	(0.0087)***	(0.0078)***	(0.0083)***	(0.0086)***	(0.0089)***
Panel C -- Dep.var.: +1 if illegal reaction, 0 if no reaction, -1 if legal reaction						
OLS coefficients						
Low Cost of Embargo Violation (Indicator)	0.0458	0.0466	0.0791	0.028	0.0234	0.0217
	(0.0230)**	(0.0244)*	(0.0316)**	(0.0264)	(0.0273)	(0.0224)
Constant	-0.0228	-0.0248	-0.0218	-0.0163	-0.0185	-0.0158
	(0.0110)**	(0.0112)**	(0.0098)**	(0.0106)	(0.0108)*	(0.0115)
Source of Measures of Cost of Embargo Violation:	Transparency International	Small Arms Survey	OECD	Freedom House	Transparency International	Shleifer et al.
N	1839	1749	1839	1839	1761	1839

Notes: An observation in the regression 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. The dependent variable in Panel C is equal to 1 if the event is of the type 'Legal_React', is equal to -1 if the event is of the type 'Legal_React', and 0 otherwise. In Columns (1)-(6) we use 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). Robust standard errors are clustered at the company level.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 12. Detection: Chains of illegal reactions

		# Illegal Chains of Reactions	# Possible Chains of Reactions	Percent of Chains of Illegal Reactions
		(1)	(2)	(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%
By Type of Arms	Small arms & ammur	6	1051	0.57%
	Missiles	2	783	0.26%
	Tanks	3	315	0.95%
	Explosives	3	489	0.61%

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 13. 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	0.0235	0.0134			
	(0.0064)***	(0.0061)**			
Indicator for High Arms-Related Google Hits By Company			0.0516	0.0449	0.0339
			(0.0196)***	(0.0157)***	(0.0184)*
N	1839	1839	1811	1811	1811
Panel B -- Dep.var.: 1 if legal reaction; 0 otherwise					
OLS coefficients					
Incidence of UN Reports on Embargo Violation By Conflict	-0.0176	0.003			
	(0.0078)**	(0.0054)			
Indicator for High Arms-Related Google Hits By Company			0.0202	-0.0115	0.0044
			(0.0257)	(0.0315)	(0.0197)
N	1839	1839	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. All regressions in Columns (1)-(5) include a constant, not reported. Robust standard errors are clustered at the company level.

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

Appendix Table A1. List of Events (with Emphasis for Events under Embargo)

Country	Date Event	Type of Event	Event Description	Effect on Hostilities	Event Surpr.	Event Import.
Angola	12/22/1988	Peace Agreement	Angola, Cuba and, South Africa reach agreement. South Africa agrees to withdraw troops.	Decreases	2.84	32.00
	12/14/1998	Major Battle	UNITA attacks town of Cuito.	Increases	1.67	28.00
	09/28/1999	Ceasefire	Top UNITA general and 2,000 rebels surrendered in Bailundo.	Decreases	2.32	21.50
	02/22/2002	Assassination	Jonas Savimbi was killed on Feb. 22 by soldiers of the Angolan army.	Decreases	3.69	54.50
Ethiopia	02/06/1999	Major Battle	Fighting renews after a several month lull; heavy casualties.	Increases	3.27	63.00
	05/12/2000	Major Battle	Ethiopia launches major offensive against Eritrean positions.	Increases	2.69	58.50
	12/12/2000	Peace Treaty	Ethiopia and Eritrea sign a treaty formally ending their 2 year war.	Decreases	1.87	35.50
Liberia	04/29/1996	Fighting Resumes	Fighting resumes; Liberia's head of state, Wilton Sankawulo, and Charles Taylor flee.	Increases	6.95	36.50
	09/19/1998	Major Battle	Fighting erupts in Monrovia between government forces and partisans of former warlord Roosevelt Johnson.	Increases	4.59	19.50
	08/10/1999	Major Battle	Liberian president declares emergency. Fighting rages between government troops and forces who seized the key town of Kolahun in northwest. State of emergency declared.	Increases	3.68	28.50
	06/05/2003	Major Battle	LURD rebels launch attack on Monrovia, then withdraw. 300-400 people die and others fled.	Increases	1.44	39.50
Rwanda	10/21/1993	Coup	Burundi President Melchior Ndadaye executed by his captors after a coup.	Increases	22.00	16.50
	04/06/1994	Assassination	The airplane carrying President Habyarimana was shot down as it prepared to land at Kigali. Military and militia groups began rounding up and killing all Tutsis as well as political moderates irrespective of their ethnic backgrounds.	Increases	34.67	52.00
	07/04/1994	Major Battle	RPF capture Kigali.	Decreases	1.68	59.50
Sierra Leone	04/29/1992	Coup	Captain Valentine Strasser stages a coup and removes President Momoh from power.	Increases	17.33	13.00
	02/26/1996	Elections	Elections organized by the military junta give victory to the Sierra Leone People's Party.	Decreases	2.61	23.50
	05/25/1997	Coup	Major General Johnny Paul Koroma deposes President Kabbah in a military coup, suspends the constitution, bans demonstrations, and abolishes political parties. Kabbah flees to Guinea to mobilise international support.	Increases	16.67	50.00
	03/10/1998	Return to power	The elected president of Sierra Leone, Ahmad Tejan Kabbah, returns home - ten months after he was forced into exile by a military coup.	Decreases	2.67	40.00
	01/06/1999	Coup Attempt	Unsuccessful coup attempt by Revolutionary United Front.	Increases	6.25	86.00
	05/18/1999	Ceasefire	Tentative ceasefire between government forces and RUF.	Decreases	2.26	21.50
	05/17/2000	Leader Captured	Rebel leader Foday Sankoh captured. His capture came nine days after he had disappeared from his home where he had been detained under house arrest.	Decreases	1.97	107.50
Somalia	10/03/1993	Major Battle	Black Hawk Down Incident. 18 US troops killed leading immediately to increased troops levels.	Increases	4.28	131.50
	08/02/1996	Leader Dies	Aidid, a Somali politician and the leader of the Habr Gidr clan, dies. He had hindered international famine relief efforts in the early 1990s and challenged the presence of United Nations and United States troops in the country.	Decreases	8.70	43.50
Sudan	04/06/1985	Coup	Commander-in-Chief of the people's armed forces of Sudan, Abdel Rahman Mohamed Hassan Suwar al Dahab, terminated the constitution and proclaimed martial law in the country.	Increases	2.25	22.50
	06/30/1989	Coup	National Salvation Revolution takes over in military coup.	Increases	14.00	21.00
	12/13/1999	Fighting Begins	President Bashir dissolves the National Assembly and declares a state of emergency following a power struggle with parliamentary speaker, Hassan al-Turabi.	Increases	3.36	42.00
	07/20/2002	Peace Agreement	After talks in Kenya, government and SPLA sign Machakos Protocol on ending 19-year civil war. Government accepts right of South to seek self-determination after six-year interim period. Southern rebels accept application of Shariah law in North.	Decreases	2.03	31.50
	07/31/2005	Death	John Garang, leader of the rebel Sudan People's Liberation Army and Sudanese First-Vice President, is killed in a helicopter crash.	Decreases	5.71	155.50
Yugoslavia	06/25/1991	Independence	Croatia and Slovenia proclaim independence.	Increases	3.13	47.00
	03/30/2001	Leader Captured	Milošević arrested on charges of abuse of power and corruption	Decreases	4.90	277.00
	03/12/2003	Assassination	The prime minister of Serbia, Zoran Djindjic is assassinated.	Increases	7.19	93.50
	03/17/2004	Start Fighting	Mitrovica, in Kosovo, experiences the worst ethnic violence in the regions since the 1999 war. At least 22 people are killed, and another 500 are injured.	Increases	5.92	77.00

Notes: List of events affecting hostilities occurring inside the embargo period (emphasized) and outside the embargo period (not emphasized). The effect on hostilities is the presumed effect on hostilities of the event. 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.

Appendix Table A2: Arms-Producing Companies in the Sample

Company Name	Country	NoObs	Source	Company Name	Country	NoObs	Source
Advanced Tchn.Prds.	Usa	3367	W	Mitsubishi Plastics	Japan	5475	W
Alliant Technologies Ystems	Usa	3910	WS	Nec	Japan	5477	S
Allied Defense Group	Usa	5420	W	Ricoh Elemex Corporation	Japan	4410	W
Anteon International	Usa	781	S	Toshiba	Japan	5477	S
Armor Hdg.	Usa	2605	S	Alvis	Uk	4762	W
Ball	Usa	5475	W	Babcock International	Uk	706	S
Blount International	Usa	1548	W	Bae Systems	Uk	5472	W
Boeing	Usa	5475	WS	Cobham	Uk	5374	S
Caci International	Usa	5464	S	Gkn	Uk	5477	S
Computer Scis.	Usa	5474	S	Meggitt	Uk	5261	S
Cordant Technologies Inco Com	Usa	4010	W	Smiths Group	Uk	5477	S
Cubic	Usa	5477	S	Ultra Electronics Hdg.	Uk	2237	S
Curtiss Wright	Usa	5477	S	Vickers	Uk	3860	W
Diehl Graphsoft	Usa	1140	S	Vt Group	Uk	4198	S
Ducommun	Usa	5446	W	H & R Wasag	Germany	4365	W
Dynamic Materials	Usa	4028	W	Indus Holding	Germany	2606	W
Dyncorp	Usa	961	S	Krauss-Maffei	Germany	1082	W
Engd.Support Systems	Usa	5201	S	Renk	Germany	4364	W
Esco Technologies	Usa	1301	W	Rheinmetall	Germany	5475	W
Firearms Training Systems	Usa	1724	W	Rheinmetall Pref.	Germany	5341	W
Gencorp	Usa	5475	W	Thyssenkrupp	Germany	5477	S
General Dynamics	Usa	5475	WS	Electro Optic Systems	Australia	1027	W
General Electric	Usa	5477	S	Harrington Group Limited	Australia	881	W
Goodrich Corporation	Usa	5475	WS	Lomah Corporation	Australia	933	W
Grumman	Usa	2417	WS	Metal Storm	Australia	1558	W
Halliburton	Usa	5474	S	Orica	Australia	5475	W
Harris	Usa	5477	S	Transfield Services	Australia	1040	S
Hi Shear Technology	Usa	2836	W	Alenia	Italy	1619	S
Honeywell International	Usa	5216	S	Alenia (Dus)	Italy	519	S
Jacobs Engr.	Usa	5477	S	Breda	Italy	2078	W
Lockheed Martin	Usa	2606	WS	Ericsson	Italy	4505	S
Mantech International	Usa	778	S	Cae	Canada	5453	S
Martin Marietta	Usa	2659	W	Snc-Lavalin Group	Canada	4926	W
Mcdonnell Douglas	Usa	3281	W	Spar Aerospace	Canada	4425	W
Moog	Usa	5477	S	Compagnie Financiere Richemont (Ad	Switzerland	1910	W
Olin	Usa	5475	W	Ems-Chemie	Switzerland	4431	W
Orbital Sciences	Usa	3910	WS	Richemont	Switzerland	4431	W
Oshkosh Truck	Usa	5216	S	Dyno	Norway	3309	W
Primex Technologies	Usa	799	W	Kongsberg Gruppen	Norway	2866	W
Raytheon	Usa	611	S	Indra Sistemas	Spain	2402	S
Ride	Usa	983	W	Tubos Reunidos	Spain	486	W
Rockwell Collins	Usa	1042	S	Steyr-Daimler-Puch	Austria	3059	W
Rohr	Usa	3382	W	Sabca	Belgium	3099	W
Starmet	Usa	3869	W	Econ Industries	Greece	1071	W
Stewart & Stevenson Services	Usa	5477	S	Fokker	Netherlands	3382	W
Sturm Ruger & Co	Usa	5397	W	Saab Scania	Sweden	1679	W
Taser International	Usa	1040	W	Anhui Leimingkehua	China	246	W
Textron	Usa	5474	S	Guizhou Jihlian	China	232	W
Titan	Usa	5327	S	Jiangsu Gaochun Ceramics	China	518	W
Trw	Usa	4679	W	Shaanxi Aerospace Power High Techr	China	518	W
United Defense Industries	Usa	645	S	Wuhan Plastics Industrial Group	China	2015	W
United Technologies	Usa	5477	S	Amadeo Rossi Pn 1000	Brazil	696	W
Venturian	Usa	4001	W	Cbc Cartucho Pn	Brazil	38	W
Aerospatiale Matra	France	132	W	Embraer On	Brazil	1824	S
Charlatte	France	1824	W	Forja Taurus Pn	Brazil	2291	W
Cs Communication Systems	France	4648	W	Daewoo Precision Industries	Southkorea	1427	W
Dassault Aviation	France	4692	W	First Technologies	Southkorea	3873	W
Eads (Par)	France	1301	W	Hanwha	Southkorea	5454	W
Explosifs Et Produits Chimiques	France	3474	W	Samsung Techwin	Southkorea	4551	S
Geci International	France	1031	W	Rohas-Euco Industries	Malaysia	2544	W
Latecoere	France	4097	W	Sugar Bun Corporation Bhd.	Malaysia	1819	W
Matra	France	1657	W	Weida	Malaysia	866	W
Sagem	France	82	S	Aeci	Southafrica	4393	W
Snecma	France	256	W	Omnia	Southafrica	3101	W
Thales (Adr)	France	4890	W	Plessey	Southafrica	722	W
Thales (Ex Thomson-Csf)	France	4692	W	Aliachem	Czechrep	1138	W
Verney Carron	France	1396	W	Ceska Zbrojovka	Czechrep	1055	W
Asahi-Seiki Manufacturing	Japan	3742	W	Bharat Electronics	India	2085	S
Daicel Chemical Industries	Japan	5475	W	Ici India	India	3834	W
Daikin Industries	Japan	5475	W	Aryt Industries	Israel	1621	W
Hosoya Pyrotechnics	Japan	2772	W	Elbit Systems	Israel	1824	S
Howa Machinery	Japan	5475	W	Exsa	Peru	2088	W
Ishikawa Seisakusho	Japan	5475	W	Famesa Explosivos	Peru	1359	W
Japan Carlit	Japan	5159	W	Enaex	Chile	1774	W
Kanematsu Engineering	Japan	779	W	Irkut	Russia	205	S
Miroku	Japan	3313	W	St Electronic&Engr.	Singapore	1491	S
Mitsubishi Electric	Japan	5477	S				

Notes: Companies included in the sample of arms-producing companies. The Table reports the country in which the company is head-quartered, the number of observations in the sample, and the source of the data. The Source is coded as follows: "W" indicates that the company is identified as an arms-producing company using the Datastream-WorldScope data set; "S" indicates that the company is listed in the SIPRI list of top 100 arms-making companies; "WS" indicates that the company is present in both data sets.

Appendix Table A3. Stock Market Reaction. Robustness

Dep. Var.:	3-Day Stock Returns (-1,1)					
	Abnormal Returns				Raw Returns	Excess Returns
	(1)	(2)	(3)	(4)	(5)	(6)
Event During Embargo (1=Increase War, -1=Decrease, 0=No Event)	-0.0042	-0.0041	-0.0056	-0.0016	-0.0045	-0.0046
	(0.0018)**	(0.0014)***	(0.0024)**	(0.0024)	(0.0013)***	(0.0014)***
Event During Embargo* (High-Corruption Country)	0.0115	0.0118	0.0124	0.0105	0.012	0.0117
	(0.0041)***	(0.0039)***	(0.0043)***	(0.0054)*	(0.0039)***	(0.0036)***
Indicator for High-Corruption Country	-0.0001		-0.0001	-0.0001	-0.0004	0
	(0.0002)		(0.0002)	(0.0003)	(0.0003)	(0.0002)
Constant	-0.0001	-0.0001	-0.0001	0	0.0023	0.0011
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)***	(0.0001)***
Clustering of Standard Errors	By Date	By Company	By Company	By Company	By Company	By Company
Shift Date for Time Difference > 8 Hours		X				
Sample of Companies			Worldscope	SIPRI		
N	492541	492541	319078	202731	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) the standard errors are clustered by date rather than by company as in the other regressions. In Column (2) 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 Columns (3) and (4) we report separately the results for the (partially overlapping) subsets of arms-producing companies according to the SIC codes in the DataStream-Worldscope data set (Column (3)) or according to SIPRI (Column (4)). In Columns (5) and (6) we present the results with raw returns (Column (5)) and returns net of the market return (Column (6)), instead of beta-corrected returns. Robust standard errors clustered by company in parentheses.

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