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On Target? The Incidence of Sanctions Across Listed Firms in Iran*

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Abstract

How successful are sanctions at targeting the economic interests of political elites in affected countries? We study the efficacy of targeting in the case of Iran, using information on the stock exchange-listed assets of two specific political entities with substantial influence over the direction of Iran's nuclear program. Our identification strategy focuses on the process of negotiations for sanctions removal, examining which interests benefit most from news about diplomatic progress. We find that the stock returns of firms owned by targeted political elites respond especially sharply to such news, though other listed firms unconnected to these elites also benefit from progress towards sanctions relief. These results indicate the 'bluntness' of sanctions on Iran, but also provide evidence of their effectiveness in generating economic incentives for elite policymakers to negotiate a deal for sanctions relief.

JEL Classification: F51

1 Introduction

Economic sanctions are an important tool of foreign policy, providing an instrument by which states may attempt to influence policies abroad without resorting to military force or covert action. In recent years, sanctions have been at the forefront of international responses to Russia's foreign policy decisions regarding the Ukraine and to Iran's program for the development of nuclear technology. Other prominent recent examples of sanctions have included measures leveled against Burma, Iraq, North Korea and Syria, among many others.

Once imposed, sanctions act as a 'carrot' for policymakers in the sanctioned country, as the actors imposing sanctions offer to remove them in exchange for policy reform. But if imposing sanctions is costly for the senders, then they will prefer to design this incentive as efficiently as possible. In theory, sanctions should therefore be focused on the sources of income most valued by those responsible for the key policy decisions, rather than the economy of the receiving country as a whole. In line with this logic, a central principle underlying the design of modern sanctions regimes is that they should be targeted as much as possible to the economic interests of elite decisionmakers.[1]

In practice, however, it may be difficult to design sanctions that stay 'on target'. This might be because the targeted group is particularly well placed to avoid even carefully designed sanctions, or is able to redistribute the effects of sanctions to less powerful groups within the sanctioned country.[2] Alternatively, the policy instruments used by the sender could be too blunt to target any particular group within the sanctioned country. While states are increasingly employing 'smart sanctions' instruments designed to affect only particular firms or individuals, many sanctions regimes also incorporate more comprehensive trade and financial restrictions, which may be difficult to selectively 'switch on and off' across different agents in a receiving country.[3]

In this paper, we consider the case of Iran, which was subject to multilateral sanctions related to its nuclear program until early 2016. Over several years of diplomatic negotiations

[1]For example, the European Commission (2008) asserts the principle that "[a]s a general rule, sanctions should target as closely as possible the individuals and entities responsible for the undesirable policies and actions, thus minimising adverse effects on others." An alternative form of targeting focuses on particular industries, as with arms embargoes (DellaVigna and La Ferrara 2010).

[2]For example, the UN's Oil-for-Food program in Iraq was designed to maintain an oil embargo on Iraq while still providing the country with access to humanitarian supplies, but there is strong evidence that the policy was circumvented, presumably to the benefit of Iraq's political elite (Hsieh and Moretti 2006).

[3]Such a problem of 'bluntness' leading to unintended consequences is not unique to sanctions policy and may also be present in strategies involving armed intervention (Dell and Querubin 2018) or military assistance (Dube and Naidu 2015).

ending in July 2015, Iran and its sanctioners came to an agreement in which Iran consented to changes in its nuclear policies in exchange for the lifting of economic sanctions. If these sanctions were successfully targeted, their removal should have benefited the policymaking elite, rewarding them for changing the policy for which sanctions were imposed.

Our paper exploits this insight about the returns to the lifting of sanctions in order to test for the effectiveness of the targeting of domestic political elites in the Iranian case. We consider two key actors who were targeted by the senders of sanctions because of their important roles in decisions about Iran's nuclear program: the Islamic Revolutionary Guard Corps (IRGC) and Iran's Supreme Leader, Ali Khamenei. Both of these actors reportedly control large conglomerates, and we check whether certain observable assets of those conglomerates – specifically, their holdings in firms listed on the Tehran Stock Exchange (TSE) – were positively affected by diplomatic progress towards a multilateral sanctions deal. We also compare the TSE returns of these 'target firms' to those of a set of unrelated firms with no known connection to the targeted conglomerates, so as to better understand the relative impact of sanctions relief across these two (TSE-listed) parts of the Iranian economy.

To accomplish this, we identify 'information shocks' regarding the progress of diplomatic negotiations, and compare contemporaneous shifts in firm-level TSE returns across target and unrelated firms. We first explore a compelling case study: the stock market reaction to a breakthrough in multilateral negotiations in Geneva between Iran and the main sanctioning countries in November 2013. This 'big bang' event is conveniently timed, with much information about the progress of negotiations accruing over the TSE's weekend break. Because of this, we are able to cleanly capture the TSE market response to the most important turning point in the negotiations for sanctions removal.

We then use high-frequency text-based measures of news about sanctions negotiations to capture information shocks covering the full period from the re-opening of serious negotiations in 2012 to agreement on a final deal in 2015. This allows us to test the sensitivity of different types of firms to news about possible progress towards the lifting of sanctions using a large collection of events. The measures we use – derived from the Factiva and GDELT (Global Database of Events, Language and Tone) databases – provide quantitative information on coverage based on an extensive library of news sources.[4]

Our results show a consistent pattern of responses by the two sets of firms to these information shocks. Specifically, in each of our empirical exercises, we find that the stock

[4]This aspect of our study builds on recent contributions that have incorporated text-based information on salient news shocks into stock market studies (e.g. Baker, Bloom and Davis 2016, Loughran and McDonald 2011, Tetlock 2007).

returns of firms owned by targeted political groups (our target portfolio) and companies unrelated to these groups (our non-target portfolio) both react positively to information indicating progress in diplomatic negotiations. However, the effects are significantly larger for the target portfolio. This pattern is compatible with a scenario in which sanctions rely on 'blunt instruments' and so affect the economy of the receiving country more widely than intended. But it also suggests that the TSE-listed assets of Iran's political elite were indeed affected by sanctions, and differentially so when compared with other listed firms.

We calculate total effect magnitudes across the full sample period using a specification focused on the top decile of sanctions news shocks. This indicates that around 17% to 25% of average daily returns of target firms, and 5% to 10% for the non-target group, can be assigned to major news shocks related to sanctions negotiations. We can therefore credit a non-trivial fraction of the positive returns that accrued to the target portfolio in the 2012-2015 period to news about the potential lifting of sanctions.

We also show that a later negative shock in expectations of the sanctions deal's continuation, provided by Donald Trump's surprise election victory, is similarly associated with differential stock returns (negative in this case) for target firms. Subsequent developments, again captured using daily news coverage measures, show no additional sensitivity for target portfolio returns, possibly implying that sanctions policies went 'off target' after Trump took power. However, the events captured by our coverage measures during the period after the US election (such as new sanctions in response to Iranian missile tests) are quite different from the diplomatic negotiations underlying our main results.

Finally, we show evidence that our main results are not driven by a potential alternative explanation: that diplomatic progress also changes the likelihood of conflict between Iran and other countries, which could itself have heterogeneous effects on target and non-target firms. We first observe that while both portfolios are sensitive to a political betting market measure of Iran's probability of direct military conflict with the US or Israel, the target portfolio is not differentially responsive to this information. We then find that the returns of arms industry firms listed on stock markets outside Iran show no significant co-movement with news of diplomatic progress towards a deal on Iran sanctions.

Overall, our finding of systematic comovement between target firm returns and events relating to sanctions removal provides evidence that multilateral sanctions against Iran succeeded in inducing some degree of 'income targeting' of the political elite. Even though we cannot say whether decisionmakers were explicitly driven by this incentive when negotiating the removal of sanctions, our empirical evidence suggests that such an incentive existed.

Related Literature. Our study makes a novel contribution to the substantial literature on the economics of sanctions. A large body of empirical work has studied the question of whether, and under what circumstances, sanctions accomplish the stated goals of the sender. Much of this work builds on the cross-country analysis and dataset of Hufbauer, Schott and Elliott (1990) and focuses on whether the offending policies of sanctioned countries are actually altered after sanctions are imposed. Very few papers zoom in to consider whether the incidence of sanctions within a given country is in line with the intentions of the sender.

Recent firm-level research by Ahn and Ludema (2017, 2019) finds that US and EU 'smart sanctions' on specific Russian companies, and their shareholders, managers and directors, have had substantial negative effects on those firms' performance. The key distinction between our studies is that Ahn and Ludema evaluate the efficacy of a set of specific sanctions instruments aimed at particular entities, while we assess the success of targeting in terms of the impact of the sanctions regime as a whole on the assets of influential political actors.[5]

Moreover, unlike the rest of the sanctions literature, our estimates are not based on the timing of the imposition of sanctions, but instead exploit news about progress towards their removal. It is this innovation that allows us to estimate the impact of the full suite of Iran sanctions rather than individual measures, because news about diplomatic progress relates to the lifting of the entire sanctions regime. It also means that we can use unexpected and precisely timed progress in diplomatic negotiations for identification.

Within the wider political economy literature, our paper is one of a number of recent studies exploiting variation in stock market returns within an event study framework. This methodology has been applied to topics such as the private benefits of civil conflict (Guidolin and La Ferrara 2007), the economic implications of covert foreign intervention (Dube, Kaplan and Naidu 2011) and the value of political connections (Fisman 2001, Faccio 2006, Coulomb and Sangnier 2014, Acemoglu, Hassan and Tahoun 2018). We also contribute to a growing literature on the political economy of the Middle East (e.g. Jaeger and Paserman 2008, Berman, Shapiro and Felter 2011, Durante and Zhuravskaya 2018).

Structure. The paper is organized in the typical way. Section 2 presents a conceptual framework to motivate our study and its research design. Section 3 provides background

[5]Some studies have considered the impact of sanctions on political outcomes (rather than regime assets): Marinov (2005) finds that leaders of countries subject to economic sanctions are more likely to lose power, while Allen (2008) notes a positive relationship between sanctions and antigovernment activity. Another paper considering the economic impact of sanctions on subgroups within a country is that of Neuenkirch and Neumeier (2016), who argue that US sanctions have led to an increase in poverty in sanctioned countries. Haidar (2017) finds heterogeneous effects of sanctions on Iran on exporting firms depending on characteristics such as exporter size.

information on the history of sanctions on Iran and the political entities targeted by these sanctions. Section 4 describes the data we use, and Section 5 then presents our empirical analysis. Section 6 assesses the possible alternative interpretation of our results in terms of changes in the risk of conflict. Section 7 offers concluding remarks.

2 Conceptual Framework

In this section, we first set out a simple static framework to clarify why countries might want to target sanctions, and why the effectiveness of targeting is an important economic question. As part of this, we frame the effects of sanctions as creating an incentive ('carrot') for policy changes in a sanctions-receiving country, distinguishing between targeted and non-targeted sub-groups within the receiver. We then discuss the effect of 'blunt instruments' on the realized incidence of sanctions and show that the cost of sanctions tends to increase under this scenario, due to the wider distribution of impacts across these sub-groups. Finally, we consider the implications of time-varying progress in negotiations towards sanctions relief, in order to motivate our empirical strategy and aid in the interpretation of our results. In particular, the spread of impacts suggested by our framework lets us tie our sanctions bluntness parameter β to the results of a difference-in-difference model that compares the asset values of targeted and non-targeted groups within the receiver over time.

2.1 Setup

We consider a scenario in which sanctions are imposed in order to induce changes in a particular policy in the sanctioned country, since this was the usual publicly stated goal of multilateral sanctions on Iran.[6] Consider two countries, a sender S of sanctions and a receiver R. Within R, a group p (the 'political elite') has control over a policy that, if in place, benefits p but imposes a cost on S. In response, S imposes economic sanctions on R, offering to lift these sanctions if R ends the policy. While sanctions may also be costly to the sender S (because of lost trade, transactions costs, or political considerations), we assume for simplicity that these costs are lower than the cost to S of the offending policy in place in receiver R.

Once sanctions are imposed, they work as a carrot rather than a stick: as long as the

[6]The framework in this section can easily be extended to accommodate other scenarios; for example, to show that the targeting of sanctions may also be desirable if the sanctioners' goal is to alter the balance of power between different actors within the sanctioned country, as in Kaempfer and Lowenberg (1988).

group p within R benefits from sanctions relief, the removal of sanctions is effectively a reward for ending the policy that harms S.[7] Say that the income cost of sanctions in R is y, and that this cost is spread additively across group p and the group o ('others'), constituting the rest of the population of R, so that $y_p + y_o = y$. Also assume that group p has a welfare function consisting of three additive terms: b_p, the benefit to p from the policy being in place; $Y^R = Y_p + Y_o$, the aggregate income of R; and an additional weight $\alpha - 1$ on Y_p, the income of group p. Then welfare of p is equal to $b_p + \alpha Y_p + Y_o$. The relevance of Y_o to the welfare of p might be due to altruism or 'good governance' by group p, or because of the need for support from the population as a whole in order to sustain the political regime. But as long as $\alpha > 1$, group p values increases in its own income more highly than similar increases in the income of the rest of the population.

To understand the potential attractiveness of targeting to S, assume that the cost of sanctions to S rises in y, so that increasing the cost of sanctions in the receiving country also increases the cost to the sender.[8] Then consider the scenario in which S has complete control over their incidence across o and p. In this case, S solves the problem:

$$\min_{y_p, y_o} \quad y_p + y_o \quad \text{subject to} \quad \alpha y_p + y_o \geq b_p, \quad y_p \geq 0, \quad y_o \geq 0$$

The sender must assure that the benefit to group p if the sanctions are lifted is at least as large as the cost of ending the policy, while minimizing the cost of sanctions to S itself. Given that $\alpha > 1$, the solution to this problem is to set $y_p = \frac{1}{\alpha} b_p$, $y_o = 0$. In other words, as long as each additional unit of income lost by p has a greater effect on the welfare of p than a unit of income lost by others in R, the minimum-cost sanctions policy is to target sanctions so that they affect the income of p exclusively.[9]

2.2 'Blunt Instruments'

Now imagine that S cannot control the incidence of sanctions within R, perhaps because of the bluntness of the available sanctions instruments, or the asymmetric capacity for sanctions

[7] Note that because of the empirical context we study (in which sanctions are in place and the sender and receiver are already negotiating sanctions relief), we abstract from the stage in which sanctions are threatened but not yet imposed; see Eaton and Engers (1992) for a model that incorporates this stage.

[8] A sanctions regime of higher intensity might, for example, lead to greater foregone gains from trade or increased transactions costs for the sender.

[9] Note that in this static framework, we implicitly assume that the costs imposed by the sanctions to the receiver at the point of introduction are symmetric with the benefits of removing the sanctions. This necessarily abstracts from more complex scenarios where the incidence of costs that arise when sanctions first hit is different from the incidence of benefits when sanctions are lifted.

avoidance across groups within R. In particular, say that an attempt at a 'fully targeted' regime of cost y actually leads to incidence $y_p = \beta y$, $y_o = (1-\beta)y$ for some $\beta \in [0,1]$ that is exogenous to S. The β parameter can therefore be seen as the share of income costs borne by the political elite group p within the receiver country. In simple terms, $\beta = 0$ corresponds to the elite group p escaping all of the income costs of sanctions, while $\beta = 1$ implies that the full income costs fall on the political elite only.

In the 'bluntness' case where $\beta < 1$, the chosen sanctions regime will be costlier than in the scenario where S can control sanctions incidence. In particular, the solution for S is now to set $y = \frac{1}{\alpha\beta+(1-\beta)}b_p$, which is a greater total cost than under the targeted regime, where $y = \frac{1}{\alpha}b_p$. In this model, both S and R (in aggregate) therefore benefit from the ability of S to target sanctions. The effectiveness of targeting – here quantified as the magnitude of β – is thus an interesting economic question.

2.3 Negotiations for Sanctions Removal

Once a given sanctions regime is imposed, the above static framework requires that a cost-benefit analysis by group p should lead to an immediate decision to remove or continue the policy. In practice, however, a sender and receiver of sanctions can engage in a period of negotiations over the actual concessions to be made by the receiver, and the sender might escalate or ease the sanctions regime during this period. Based on the progress of negotiations, the expected discounted costs of sanctions and benefits from the offending policy may therefore evolve over time, due to changes to the current situation as well as expected future events. For example, if S and R take a step towards a deal to exchange policy reform for sanctions relief, this will lead to a decrease in the expected (discounted) future costs of sanctions to both p and o and the expected future benefit of the policy to p.

Therefore, if we are interested in learning about β using information from progress in political negotiations, we need an empirical strategy that separates the effects of these negotiations on the costs of sanctions to the targeted political elite p and to the non-elite o. In this study, we use stock returns data to examine the evolution of the asset values of groups corresponding to p and o during negotiations between Iran and its sanctioners.

If the values of assets of p comove positively with news of progress in negotiations, and these asset values are unrelated to any benefits from the policy itself, then we interpret this comovement as evidence that $\beta \neq 0$. Practically, this implies that the group p benefits from expected sanctions relief because it has not escaped bearing an income cost from the sanctions. If the values of assets of the non-elite group o move together with these shocks,

we similarly consider this to be evidence that $\beta \neq 1$; that is, the elite group does not bear the full costs and there is some sharing of costs between p and o. Finally, we derive further information about β by comparing the relative magnitudes of these changes in the values of the two groups' assets. As we outline in Section 5, this maps into our empirical difference-in-difference model so that we are able to provide a broad empirical assessment of the success of sanctions policy in terms of the targeting bluntness parameter β.

3 Background

3.1 Sanctions on Iran and Political Negotiations

While the United States has maintained economic sanctions on Iran since soon after its 1979 revolution, robust multilateral sanctions prompted by Iran's nuclear program were imposed only from the mid-2000s.[10] The United Nations Security Council first passed a resolution threatening Iran with sanctions in July 2006, in reference to International Atomic Energy Agency (IAEA) reports stating that the IAEA was unable to determine that Iran's nuclear program had no military dimension. The resolution called for Iran to suspend enrichment and reprocessing activities associated with its nuclear program. Sanctions were then imposed in December 2006 and tightened in two subsequent resolutions in 2007 and 2008, with these resolutions specifying that the sanctions would be removed once Iran met requirements set by the IAEA and the Security Council itself.

The sanctions put in place during this period constituted both narrowly focused 'smart sanctions' and more comprehensive measures. For instance, the Security Council resolutions asked states to freeze the assets of specific firms, individuals and other organizations directly involved in Iran's nuclear program, and also to "exercise vigilance" over the foreign activities of Iranian financial institutions in general. Sanctions were publicly portrayed as targeting the economic interests of actors with political influence inside Iran, by cutting Iran off on margins that would specifically affect those interests.[11] However, some observers argued that sanctions were ineffective, or even strengthened defenders of the nuclear program within Iran:

[10] US sanctions on Iran have included a variety of measures relating not only to weapons proliferation, but also to terrorism and human rights.

[11] For example, after the US ended access to the American financial system for a major Iranian bank, the US Treasury's Undersecretary for Terrorism and Financial Intelligence argued that "[w]hile those who are currently benefitting from Iranian integration into the global economy are the ones who will feel this isolation the most, they are also in the best position to persuade the regime that its current track will undermine the future of the Iranian people." (Wall Street Journal 2006)

for example, the Wall Street Journal suggested in 2006 that US pressure on Iranian banks "ended up boosting the very hard-line forces there that the US wants to curb."[12]

Several rounds of international negotiations were held alongside this gradual tightening of sanctions in the wake of the initial 2006 Security Council resolution. The talks with Iran were led by the 'P5+1' group of countries: the five permanent members of the UN Security Council (China, France, Russia, the UK and the US) as well as Germany. This process failed to reach agreement on sanctions relief in exchange for policy changes in Iran.

From 2010 through early 2012, the sanctions regime was greatly intensified, both due to a new UN Security Council resolution in mid-2010 and through various additional measures taken by individual actors including the US and EU. These new sanctions imposed severe limitations on Iran's international financial access; for example, the Belgium-based organization in charge of international banking transactions (SWIFT) removed a number of Iranian banks from its system in early 2012, after pressure from the EU. At the same time, new restrictions were placed on imports of various goods from Iran, including oil, by the EU and others. Meanwhile, the pace of diplomatic negotiations slowed; the only round of high-level multilateral talks during this period, in late 2010 and early 2011, did not produce a breakthrough.

After a hiatus of more than a year, the P5+1 negotiations on Iran's nuclear program resumed with a meeting in Istanbul in April 2012, and were characterized as successful by both sides (BBC 2012). After a series of meetings over the following months, the first major diplomatic breakthrough occurred in Geneva in November 2013, when the parties agreed to a framework agreement. This deal was followed by lengthy negotiations on a final agreement to lift sanctions in exchange for concessions related to Iran's nuclear program. The interim agreement reached at Geneva, which was originally due to expire in July 2014, was extended twice, with an eventual final deadline of June 2015. The framework of a final agreement was reached during high-level negotiations in Lausanne in April 2015, and the details of this deal were eventually concluded on July 14, 2015. Finally, after the agreement was officially approved by all parties and the IAEA reported that Iran had met its commitments under the deal, multilateral sanctions were lifted in January 2016.

However, the situation changed when in November 2016, an opponent of the agreement, Donald Trump, won the US presidential election. While the other members of the P5+1 group maintained their support for the accord, the US withdrew from the deal and began

[12]The article suggests that financial sanctions negatively affected firms unconnected to the regime, creating opportunities for hard-liners to purchase such companies; this could be interpreted as an especially problematic case of the 'blunt instruments' issue we have outlined.

reimposing sanctions in May 2018. US nuclear-related sanctions on Iran were fully restored in November 2018. These new US sanctions were not accompanied by high-level political negotiations with Iran over conditions for their removal.

3.2 Targeted Political Entities

Throughout the development of multilateral sanctions on Iran, the UN, EU and US have publicly linked sanctions policies to specific entities who have a major political influence over the direction of Iran's nuclear strategy, but also hold substantial economic assets. We discuss the nature of each of these actors' influence over the nuclear program and the structure of their economic interests in turn.

3.2.1 Islamic Revolutionary Guard Corps (IRGC)

The IRGC is a military organization with the explicit political role of guarding the 1979 Islamic revolution and promoting it outside Iran. The IRGC therefore officially functions as a branch of Iran's armed forces alongside its regular military, but is also known to be influential in politics, and former IRGC members such as Mahmoud Ahmedinejad (president of Iran between 2005 and 2013) frequently serve in prominent political roles.

The US, EU and the UN Security Council have all claimed the existence of close links between the IRGC and Iran's nuclear program in official documents. For example, in the 2010 resolution mentioned earlier, the UN Security Council noted "with serious concern the role of elements of the [IRGC] ... in Iran's proliferation sensitive nuclear activities and the development of nuclear weapon delivery systems" (UN 2010). In a document from the same year, the EU characterized the IRGC as "responsible for Iran's nuclear program" (EU 2010).

As noted above, the multilateral sanctions regime included instruments intended to influence political actors through comprehensive restrictions (e.g. on financial transactions), as well as smart sanctions incident on specific entities. The IRGC's extensive economic presence in Iran – originating from its participation in military industries and provision of services for current personnel and veterans, but now covering a wide range of industries – makes it potentially vulnerable to sanctions of both types. Indeed, beginning with the first UN Security Council resolution imposing sanctions in 2006, individuals and firms associated with the IRGC have been the subject of a large number of smart sanctions. For instance, the Fordow fuel enrichment plant became a focus of nuclear diplomacy after its disclosure in September 2009, and the UN Security Council's 2010 resolution on Iran included the im-

position of smart sanctions on a group of IRGC-owned firms that had been involved in its construction.

3.2.2 Supreme Leader and Setad

While Iran's political system includes an elected president as well as a parliament, it is the Supreme Leader, Ali Khamenei, who is effectively its most powerful figure. This includes ultimate political control of its nuclear program, as recognized by Wendy Sherman, the lead negotiator for the US in the Iran talks, in a Congressional hearing in December 2013: "The Supreme Leader is the only one who really holds the nuclear file, [and] makes the final decisions about whether Iran will reach a comprehensive agreement to forgo much of what it has created in return for the economic relief it seeks" (Kerr 2018).

As with the IRGC, the Supreme Leader's economic interests have been recognized and targeted by the senders of sanctions on Iran. The EU imposed sanctions on Mohammad Mokhber in 2010, characterizing him as "President of the Setad Ejraie Foundation, an investment fund linked to Ali Khamenei, the Supreme Leader" (EU 2010). In June 2013, the United States Treasury announced sanctions on "a major network of front companies controlled by Iran's leadership" (US Treasury 2013). The Treasury identified 37 firms in a network associated with the organization EIKO (the Execution of Imam Khomeini's Order), also known as Setad, which it claimed were "generating billions of dollars in profits for the Iranian regime each year". While the US Treasury did not provide further details on Setad's political ties, a Reuters investigative report on Setad later in 2013 identified it as a conglomerate controlled by Supreme Leader Khamenei, and noted that Setad's revenues allowed for Khamenei's financial independence from Iran's parliament and national budget process (Reuters 2013).

A report in 2015 revealed that the targeting of the Supreme Leader's economic base in Setad was an explicit political strategy on the part of sanctioners. Officials quoted by Reuters (2015) indicated that the organization was hit with smart sanctions in 2013 because the US "saw it as close to Khamenei and believed that the sanctions might induce him back to serious nuclear negotiations". Another official also noted that at that time, nuclear talks were deadlocked and action was felt to be needed: "The reason why we dropped the hammer on them when we did is because we were attempting to put pressure on the Supreme Leader (to agree to a deal)" (Reuters 2015). This report also suggested that the lifting of sanctions yielded tangible economic benefits for the conglomerate, such as the facilitation of international business deals. In Section 5.2, we take a closer look at the potential relevance

to Setad of the 2013 US smart sanctions relative to other (more comprehensive) sanctions instruments that were already in place.

4 Data

4.1 Stock Returns Data

We collect information on stock returns of listed Iranian firms by web scraping daily data from the web site of the Tehran Stock Exchange (TSE). As background, it should be noted that while the TSE is not as deep and sophisticated as many North American and Western European stock exchanges, it provides a suitable setting for conducting an event study. According to the World Bank's Global Financial Development database (see Čihák et al. 2012), the TSE puts Iran in the second (from top) quartile of financial access (share of companies outside the top 10 in market capitalization), the third quartile of financial depth (market capitalization as a share of GDP) and the second quartile of financial efficiency (shares traded as a proportion of total market capitalization), relative to other countries.[13] The TSE trades for three hours per day (9:00 to noon), five days per week (Saturday through Wednesday).

Our main sample period covers the revived multilateral negotiations leading up to the final deal. The sample therefore begins with the first day of negotiations in Istanbul (which happened to fall on the first day of the TSE's trading week) on Saturday, April 14, 2012, and ends on Wednesday, July 15, 2015 (the last day of the TSE's trading week), the day after a final agreement was reached in Vienna. In brief, we choose this window because April 2012 represents the beginning of an uninterrupted phase of negotiations (that is, a period when there was no major breakdown or hiatus in talks) and because the majority of sanctions were in place by early 2012. As we outline in Section 5.2, this latter point facilitates an event study research design centered on the effects of fully lifting sanctions.

As of the beginning of our sample period, there were 325 firms listed on the TSE. As discussed in the next subsection, our sample is made up of two firm portfolios, which together include 138 of these 325 firms. Daily returns for each firm are calculated by subtracting a stock's closing price for the previous day from its closing price for the current day and

[13]The TSE ranks very similarly to Egypt – the context studied by Acemoglu, Hassan and Tahoun (2018) – by these measures, based on data from the Egyptian Exchange. A study of market efficiency (Jahan-Parvar and Mohammadi 2013) indicates that the TSE is CAPM-efficient at the monthly level and displays patterns of international integration comparable to Middle Eastern markets that are considered to be open, such as Bahrain, Israel and Turkey.

dividing by the closing price for the previous day. We multiply daily returns by 100 so that they are expressed as percentages. We also exclude the top and bottom 1% of all observed returns from our sample, so that our results are not driven by outliers due to measurement error or very large positive or negative returns.[14]

For our exercises relating to the 2016 US election and subsequent events, we instead work with a sample period beginning sixty trading days before the US election and ending on the day when news of the US withdrawal from the deal reached the TSE. This period covers August 13, 2016 to May 9, 2018. As with our main sample period, we drop the top and bottom 1% of observed returns over this time.

4.2 Ownership Data

Our goal is to assemble a portfolio of firms listed on the TSE that were assets of the IRGC and/or Setad during the sample period in order to gather evidence on whether the political actors controlling these conglomerates may have gained from evolving progress towards sanctions relief. We do so by using information on firms explicitly identified as IRGC and Setad assets by the main 'senders' of sanctions (the US, UN and EU). We then identify the TSE-listed assets of these members of the IRGC and Setad conglomerates.

We provide a graphical summary of our procedure for classifying firms in Figure A1. As we discuss below, an important feature of our approach is that we identify not only the set of assets on the TSE that are most closely linked to the key targeted conglomerates, but also the TSE firms that are most removed from these conglomerates with no known affiliation via ownership. This is highly valuable for our research design since this set of 'most removed' firms are likely those that the senders of sanctions were least interested in targeting. Hence they provide the cleanest possible TSE-based comparison group for measuring the *de facto* efficacy of targeting of the assets of the IRGC and Setad.

4.2.1 Target Portfolio

The initial 'universe' of firms that we consider are those that were listed on the TSE at the start of our main sample period on April 14, 2012, representing a total of 325 firms. As a first step, we exclude firms with business operations directly relating to Iran's nuclear program, as identified by the senders of sanctions, with prominent examples being Iran's national maritime carrier, IRISL Shipping Lines, and several large banks. These firms are dropped in

[14] Our main results are similar if the data is instead winsorized; results are available on request.

order to avoid confounding the expected effects of sanctions relief with anticipated changes in the business of supplying goods or services for the nuclear program.[15]

In the second step, we begin defining the target group of firms. This is challenging because to do so, we require reliable information on the assets of the IRGC and Setad conglomerates, which is not available on a systematic basis in documents such as annual reports. However, as discussed in Section 3.2, Iran's sanctioners have often imposed smart sanctions on individual entities linked with the IRGC or Iran's Supreme Leader. In each case, the rationale for the imposition of sanctions has been laid out in an official document produced by the sanctioner. We can therefore use these documents to identify entities that the UN, EU or US have explicitly stated are owned or controlled by the IRGC or Setad.

Entities on which US smart sanctions were imposed due to their links with the IRGC may be found using the Department of the Treasury's Specially Designated Nationals and Blocked Persons list (SDN list), in which these organizations are assigned a specific 'IRGC' identifier. We draft a list of all entities that have been tagged with this identifier and subsequently cross-check these with US Treasury press releases that provide further details on the reasons for their inclusion in the SDN list. We add entities into our target group only if the relevant press release states that the entity is owned or controlled by the IRGC.[16] On the other hand, entities sanctioned by the US due to their links with Setad do not have a separate identifier on the SDN list. However, US smart sanctions on Setad assets were imposed in a single episode in June 2013, and so we use a detailed US Treasury press release from this episode to identify these firms.

Similarly, European Council decision 2010/413/CFSP and its amendments provide lists of EU-sanctioned entities, while UN Security Council resolutions identify organizations designated for smart sanctions by the UN. Both sets of documents provide rationales for these decisions. Again, we only include entities that these records state are owned or controlled by the IRGC or Setad.[17]

[15] We define this set of firms using a similar approach to the one we use to identify IRGC and Setad assets, discussed in the next paragraph. Specifically, we exclude any firm on which smart sanctions were imposed by the US, EU and UN for involvement in supplying the nuclear program, according to official documents. In terms of the conceptual framework, we do this so as to isolate changes in asset values that are unrelated to any benefits from the policy itself; see Section 2.3.

[16] Sanctions have been imposed on some entities for providing support or services to the IRGC. For example, Iran's national air carrier, Iran Air, was sanctioned for the transportation of military-related equipment on behalf of the IRGC.

[17] In 2010, the European Union also sanctioned Sina Bank, with the rationale that it was "very close to the interests of the Daftar (Office of the Supreme Leader). It contributes in this way to funding the regime's strategic interests" (EU 2010). Because Sina Bank was not described as a Setad asset, we have not included it in the target portfolio. However, the main results are robust to adding Sina Bank and its TSE-listed assets

The third step in defining the target portfolio involves identifying all TSE-listed assets of the IRGC and Setad entities defined in the process just outlined. Both the IRGC and Setad conglomerates are comprised of a mixture of stock market listed and private, unlisted companies. Thus, although information from the US, EU and UN allows us to identify 75 firms and other entities partly or fully owned by the IRGC and/or Setad, only six of these are themselves listed on the TSE as of the beginning of the sample. To deepen our coverage of IRGC and Setad assets, we therefore use shareholder information as of the beginning of the sample period, extracted from the TSE website, to identify firms in which at least one of the 75 targeted entities is a shareholder.[18] In this step, we identify an additional 44 TSE-listed firms in which the IRGC and/or Setad have ownership stakes, resulting in a target portfolio of 50 firms in total.

4.2.2 Non-Target Portfolio

We next construct a portfolio composed of the listed firms that we can most confidently assume are not assets of either the IRGC or Setad. Our starting point is the 260 listed firms that were not identified by the classification procedure documented in the previous subsection. However, while this excludes the 50 firms with the most direct known ownership connection to the IRGC or Setad, there are two additional groups of firms with more tenuous or less certain connections to these two conglomerates, and we drop each of these from the non-target portfolio.

The first group of companies that we drop are those with what could be termed 'second-degree' connections to the IRGC or Setad. These are the 144 listed firms that are two or more ownership layers below the 75 entities identified as IRGC or Setad assets by the US, EU and UN. We then eliminate a second group of firms that sources other than the US, EU or UN suggest might be assets of the IRGC or Setad.[19] This eliminates another 28 firms from the initial pool, leaving 88 firms in our non-target portfolio.[20]

as target firms.

[18]We are interested in all direct assets of these entities, and therefore do not use a cutoff for their observed share of ownership. Our data on the size of IRGC and Setad ownership shares is anyway likely to be very conservative due to our narrow definition of firms owned directly by these two conglomerates (i.e. identification as such by the US, EU or UN). In Section 5.2.2, we investigate the heterogeneity of our main results depending on observed IRGC or Setad ownership share.

[19]These sources are a 2013 Reuters investigative report on Setad (Reuters 2013) and a 2010 American Enterprise Institute report on the IRGC's involvement in the Iranian economy (Alfoneh 2010). We begin by dropping listed firms that are identified in these sources but are not in our target portfolio. We then use the TSE shareholder data again to construct layers of ownership below each of the entities named in these additional sources, and remove all firms within this ownership structure from the non-target portfolio.

[20]When we estimate our main results with the 172 excluded firms inserted into the sample as a separate

Table 1 shows summary statistics for firms in the target and non-target portfolios. According to data from the TSE, these two groups of firms make up a similar share of total TSE market capitalization: between one fifth and one quarter in each case. The mean and standard deviation of the daily return variable by firm-day are also very similar across the two portfolios. Accounting data from Orbis for 2012, which is available for only a subset of these firms, suggests that there are no statistically significant differences between the mean turnover, assets and labour force of the firms in each portfolio. The industrial composition of the two groups is somewhat different: for instance, a larger proportion of target firms operate in the financial sector. We account for these differences in sectoral composition in some of our empirical specifications below.

4.3 News Coverage and Event Data

As part of our research design, we construct daily measures of news coverage of sanctions-related negotiations involving Iran. The main aim of these coverage variables is to provide consistent measures of sanctions-related events that may be salient to TSE market actors during this period of diplomatic progress towards a sanctions deal.

Factiva Measure. The first measure we construct is derived from the Factiva news archive database, a business information tool marketed by Dow Jones and Company. Factiva covers around 33,000 global media sources, including most leading newspapers as well as influential multimedia content. We perform a full-text search on Factiva's media archives and retrieve all English-language articles which mention Iran and at least one of the P5+1 countries alongside the word 'sanctions'.[21] The output retrieved from this query is used to create a daily index of sanctions-related news calculated as the total raw article count, standardized within the sample period.[22] Because our goal is to measure TSE investors' reactions to the events covered in these articles, we assign all articles published over the TSE's weekend (Thursday and Friday) or during holidays to the subsequent trading day.[23]

portfolio, we find that their returns respond to sanctions-related events very similarly to firms in the non-target portfolio. Our main results are thus similar if these firms are included in the non-target group.

[21]More specifically, the Boolean query we use is as follows: (Iran) and (US or USA or United States or Russia or China or France or UK or United Kingdom or Germany) and (sanctions).

[22]For each of the two measures discussed in this section, we also create similar measures that are standardized within the alternative sample period used for the analysis of post-deal events in Section 5.3.

[23]Baker, Bloom and Davis (2016) use a similar approach to construct a monthly measure of economic policy uncertainty by calculating the frequency of articles containing a combination of three words related to uncertainty, drawing on a sample of ten leading US newspapers. In an extensive audit study, they evaluate the performance of this simple text retrieval algorithm by comparing its results with a human-coded index

GDELT Measure. Our second event measure is derived from the Global Data on Events, Location and Tone (GDELT) database. The GDELT initiative uses machine learning and natural language processing (NLP) tools to create a high-frequency open-source database of political events based on the text of news articles covering more than 37,000 online news sources in multiple languages (including Persian). A key innovation of GDELT, compared to other news databases, is that it provides information at the event level, where the events are automatically defined via NLP algorithms.[24]

GDELT defines an event as an action undertaken by an actor upon another actor. Actors can be national, subnational (e.g. rebel groups) or transnational (e.g. United Nations). Any event recorded with exactly the same date, subject actor, object actor and event type is treated as a single event and given the same unique event identifier in the GDELT database. This allows a given event to be tracked over multiple articles and days. To measure how much coverage a certain event receives, GDELT reports a count of all source articles mentioning the event and attributes this to the calendar date on which the event first appeared in the news. Each event is also classified into a four-digit category (e.g. '0341 - Express intent to change policy') based on another NLP procedure (Schrodt 2012).

For our analysis, we extract all political events stored in GDELT for which one of the actors is Iran and the other actor is one of the P5+1 countries. We condition further on events that lie in the two-digit categories that are most likely to contain diplomatic negotiation episodes: '03 - Express intent to cooperate', '04 - Consult' and '05 - Engage in diplomatic negotiations'.[25] In line with our news coverage variable sourced from Factiva, we measure events on an intensive margin using GDELT's count of source articles that mention a certain event, which can be interpreted as a proxy for the event's importance. Again, we assign all events occurring over the TSE's weekend or during holidays to the following trading day.

After calculating the total number of relevant articles for each day, we standardize this measure across all of the days in our sample. Before standardizing, we drop all events in February to August 2013, during which time there was a steep drop in the number of articles collected by GDELT, due to technical issues associated with a transition between article collection systems.

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and find the two indices to be highly correlated.

[24] See Manacorda and Tesei (2018) for a recent study that makes extensive use of GDELT data.

[25] For our 2016-2018 sample period, when many of the key sanctions-related events did not involve diplomatic negotiations, we also include the categories '10 - Demand', '11 - Disapprove', '12 - Reject' and '13 - Threaten', along with '16 - Reduce relations' (which includes the subcategory 'Impose embargo, boycott or sanctions').

Evolution of News Coverage. Figure 1 displays the variation in both of our event measures over the course of the main sample period.[26] The top six distinct events identified by each measure, highlighted in the time-series plots in Figure 1, are all related to the sanctions negotiations. Both measures spike at the times when the final framework agreement and final official agreement were completed in Lausanne and Vienna respectively, when the Geneva deal was reached and when US president Obama made a call to Iran's president, Hassan Rouhani. Our Factiva event measure also clearly identifies a round of negotiations in Vienna that ended in agreement to extend the deadline for a final deal, as well as the release of an International Atomic Energy Agency report in 2012. The GDELT measure instead places more importance on early negotiations in Istanbul and Moscow. This suggests that these measures vary similarly but not identically over time; indeed, the correlation coefficient between the two is 0.72.[27]

4.4 Other Data

In order to test whether our results could be driven by an alternative war-related explanation (since diplomatic negotiations may also have changed the likelihood of conflict between Iran and other countries), we rely on two additional data sources. First, we extract price data for betting contracts from Intrade, a large online betting market, in order to construct a measure of the probability of military conflict between Iran and Israel or the United States.[28] The betting contract we use for this exercise is specified as "US and/or Israel execute an overt airstrike against Iran by December 31, 2012". The contract was to have paid $10 if an airstrike occurred before December 31, 2012, and zero otherwise. During the trading period, the contract traded in the range of 0 to 100, where 1 point equals $0.10. We use the contract price to calculate the daily arrival probability of an airstrike that is implied by the contract, under a set of simple assumptions about the conditions in this market.[29] Figure A2 shows

[26] Variation in news coverage measures for our 2016-2018 sample period is discussed in Section 5.3.

[27] The top fifteen days by each measure may be found in Appendix Table A1; eight of these are shared across the two measures.

[28] Intrade was an online futures exchange where predictions about events ranging from Oscar nominations to presidential election outcomes were traded as futures contracts. Trading volumes on Intrade reached over one million annually in 2012. Intrade was established in 1999, but in the context of a conflict with the US Commodity Futures Trading Commission, suspended the accounts of its US members in December 2012 and ended trading in early 2013.

[29] Specifically, let $strike_t$ be the number of times an airstrike occurs between day t and December 31, 2012 (which we call day T). On any given day t, investors can buy a contract at price p_t that will be worth $10 if $strike_t > 0$ and $0 if $strike_t = 0$. Assume that the process is Poisson, investors are risk-neutral and do not discount the future, and the Poisson parameter λ_t prevailing at a given moment is known but can change over time due to information shocks. Then we can back out λ_t from the closing price of the contract, since

the evolution of the contract's daily price and the implied daily arrival probability of an airstrike from the beginning of the sample period (April 14, 2012) through the end of 2012.[30]

Second, we use the Datastream database to extract stock price data for a set of firms for which profit expectations were likely to be sensitive to the odds of war in the Middle East. In particular, we include all publicly traded firms in the Stockholm International Peace Research Institute (SIPRI) 2012 Arms Industry Database. The SIPRI database identifies the world's hundred largest arms-producing and military services companies in 2012, but since some of these firms are privately owned, we observe stock price data for 66 of the companies on this list as of the beginning of our sample period.

5 Empirical Analysis

5.1 Case Study: Geneva Deal

To more clearly introduce our event study strategy and interpretation of our results, we begin by studying the effects of a single major event: the interim agreement negotiated in Geneva in November 2013. We examine this event both because the Geneva deal was perhaps the largest breakthrough in Iran's negotiations with the P5+1 countries, and because the timeline of this agreement (displayed in Figure 2) delivers some useful advantages for our analysis. The final round of negotiations leading to the interim agreement began at 17:00 on Wednesday November 20, after the TSE closed at noon on the same day. The TSE was then closed for two days as per the usual structure of the Iranian working week. During this time, encouraging reports emerged about the progress of talks, which continued in Geneva over both Thursday and Friday.[31] When the TSE re-opened at 9:00 on Saturday November 23, there was thus a large amount of information on which the market could act. The final agreement was then officially announced at 5:30 Tehran time on Sunday November 24, in advance of the TSE opening again at 9:00 on the same day.

this price should be equal to $\$10 * P(strike_t > 0)$, and $P(strike_t > 0) = 1 - e^{-\lambda_t(T-t)}$. Therefore, we can calculate $\lambda_t = (-1/(T-t))\ln(1-(p_t/10))$. We multiply λ_t by 100 so that it represents a probability in percentage terms.

[30] We do not display data from Intrade after December 22, 2012, since US trading on Intrade was suspended as of December 23. Our regressions using this data similarly use a sample period before the suspension of US trading.

[31] For example, as shown in Figure 2, an English-language live blog from Nasim Online news agency cited several positive quotes from parties to the negotiations ("positive atmosphere", "talking details", "room for optimism"), and reported agreement on the issue of nuclear enrichment, during the TSE's weekend break (Nasim Online 2013).

The positive information shocks before each of these two trading days provide us with an opportunity to use this progress in political negotiations to study the impact of sanctions relief. Specifically, we formulate a difference-in-difference model based on our definitions of the target and non-target portfolios and the timing of news from Geneva. Our baseline model is structured as follows:

$$R_{ijt} = \alpha_i + \phi Geneva_t + \theta Target_i * Geneva_t + u_{ijt} \tag{1}$$

where i indexes firms, j represents industries and the time index t is at the day level. Here, R_{ijt} represents raw returns, $Target_i$ is a indicator variable for firms identified as IRGC and/or Setad assets, and $Geneva_t$ is a dummy for the two days November 23 and 24, 2013.[32]

We argue that the good news about progress towards a sanctions deal in Geneva should imply higher future income for firms that benefit from sanctions relief, because the expected future duration of sanctions falls. If investors hold this belief, then the stock prices of these firms should be bid up upon the arrival of this news. Meanwhile, firms unaffected by sanctions removal should not see abnormal returns associated with this event.

The estimated values of θ and ϕ are then informative about the targeting success of sanctions across the listed firms we study. A scenario where $\phi = 0$ but the target group interaction coefficient $\theta > 0$ would represent full success: elite assets would be affected without any implied effects for the non-target assets. This would correspond to the 'minimum-cost sanctions policy' in our conceptual framework with bluntness parameter $\beta = 1$, reflecting complete targeting within our sample of firms.

The next scenario to consider is $\phi + \theta = 0$, which would imply that the targeted elite assets in our sample escape all of the effects of sanctions, mapping to full bluntness $\beta = 0$ in our conceptual framework. The rejection of both of these hypotheses, however, would provide evidence in favour of the third scenario, in which both the targeted elite group as well as non-elite interests in the sample benefit from sanctions relief. This would imply that the costs of sanctions were shared as a result of the 'blunt instruments' problem. The value of θ, the additional effect of Geneva on firms that are assets of targeted political actors, would then provide suggestive evidence of the magnitude of the bluntness parameter β.

Baseline Results. With these interpretations in mind, we present the results of estimating equation (1) for a sample period that includes the two days November 23 and 24, 2013 and

[32]This model can be interpreted as a variant of the constant mean return model in the event study literature (MacKinlay 1997) because of the inclusion of firm fixed effects. This is because we are effectively demeaning returns on a firm-specific basis, analogous to an abnormal return.

the sixty trading days beforehand. We use a sixty-day pre-period for our baseline estimates because this is a standard estimation window used in the event study literature.[33] As shown in column (1) of Table 2, we find that for non-target firms, the Geneva deal was associated with a daily return that was 0.648 percentage points above the mean return for those firms, with an additional effect of 1.256 percentage points for target firms. Both of these estimates are statistically significant when standard errors are clustered by firm.[34]

We next check whether the effects we estimate are concentrated in either of the two trading days in which the market was hit by positive news about negotiations in Geneva. Column (2) of Table 2 displays the results of regressions in which dummies and interactions for November 23 ('day 1') and November 24 ('day 2') are included separately. The estimated impact is similar across the two days for both the target and non-target groups. Because November 23 and 24 are at the beginning of the trading week, when stock returns might differ systematically from other days, we also rerun our baseline regression substituting firm-day-of-week fixed effects for firm fixed effects, and find very similar results (see column (3)).

Another potential issue with our baseline specification is that firms in the target and non-target groups are somewhat unevenly distributed across industries (see Table 1). Along with the promise of potential changes in the future evolution of sanctions, the Geneva deal also suspended some industry-specific sanctions (on crude oil, petrochemicals, the automotive industry and precious metals). Our results might thus be capturing an asymmetric industry-level effect of the Geneva deal itself rather than the impact of the multilateral sanctions regime as a whole. However, in Table 2 column (4), we add industry-Geneva interactions to our baseline specification in order to control for industry-specific effects of this event, and find that our estimate of the differential impact of Geneva on firms in the target portfolio is almost unchanged.

These results suggest that both target and non-target firms benefited from sanctions relief, but that the impact on target firms was significantly larger. The estimated average effect of each day of the Geneva event on non-target firms is approximately 32% of the size of the standard deviation of returns across all firm-days in the sample. However, the estimated abnormal returns for target firms are approximately 62% of a standard deviation higher than this. This is evidence in favour of the possibility that targeting of sanctions was neither a

[33] Our results are robust to using other windows, including thirty-day or ninety-day periods.

[34] Using the much stricter strategy of clustering by the 15 industries in our sample, and using the wild bootstrap approach of Cameron, Gelbach and Miller (2008) due to the small number of clusters, produces p-values of 0.088 for our estimated difference-in-difference coefficient and 0.182 for the coefficient on the Geneva dummy.

complete success nor a complete failure, but that firms owned by targeted political entities were more affected by sanctions than other listed firms.[35]

Placebo Analysis. The statistical significance of this difference between the returns of target and non-target firms suggests that such a difference in returns between these groups is an unusual occurrence. However, one common concern in stock market event studies is the fact that daily returns do not tend to be normally distributed. Although we have dropped large outliers, we might nonetheless question whether our p-values underestimate the probability that we would observe such a large difference even if the interaction term coefficient $\theta = 0$.

To address this issue, we estimate a series of regressions, each with a dummy for a different two-day period, an interaction of this two-day dummy with our target variable, and firm fixed effects. The two-day dummies cover our entire sample period, from April 14-15, 2012 to July 14-15, 2015. We then plot the distribution of the estimated coefficient on the interaction term from each of these 388 regressions in Figure 3, highlighting the 'true' Geneva result. It is apparent from this figure that our estimate of the differential effect of the Geneva deal on target firms is in the far right tail of this empirical distribution. In other words, over the time period around Geneva, it was highly unusual for the two-day returns of target and non-target firms, conditional on firm fixed effects, to differ from one another to the extent we observe in Table 2.

Daily Effects - Geneva Sample Period. Finally, we take a closer look at day-by-day differences between the returns of target and non-target firms in the period around the Geneva deal. To do this, we return to the sample period from our baseline regression, but add the eight trading days after the end of our two-day Geneva event, so that we may also observe information on the full two weeks after the Geneva shock. We then estimate a series of regressions as described in the previous paragraph, this time instead using one-day dummies for each day in the six TSE trading weeks from Saturday, October 26 to

[35]Note that the size of our point estimates is realistic as compared to estimates relating to other types of major economy-wide policy shifts, such as the effect of changes of government. A useful benchmark here is the study by Snowberg, Wolfers and Zitwewitz (2007). They provide a two to three percentage point estimate of the impact of a Republican presidency on stock prices across historical elections. This includes an 1.5 to two percentage point impact in the 2004 Bush-Kerry contest where they are able to leverage sharp, well-measured shifts in the probability of Bush winning as part of their empirical strategy. While we are not able to directly measure market expectations of the probability of near-term sanctions relief, it is credible to think that this probability jumped sharply over the two-day Geneva window and that our total 1.9 percentage point effect for target firms (column (1) of Table 2) is a realistic movement in returns.

Wednesday, December 4, 2013. We plot the estimated coefficients on the target * one-day dummy interactions, along with their 95% confidence intervals, in Figure 4.

Throughout the two-week period starting November 23 (to the right of the vertical line in Figure 4), the difference-in-difference coefficient for each day is positive, though not always statistically significant. This implies that the differential effect of Geneva on target firms was not eliminated by movement in the other direction soon after the deal was reached. At the same time, there is a noticeable dip in the coefficients in the weeks preceding the deal, including three days with statistically significant negative estimates. This might have been due to a temporary surge of pessimism about the potential future incidence of sanctions during the pre-deal negotiations.

However, it is also possible that our positive difference-in-difference estimate of the impact of Geneva is instead driven by mean reversion for target firms after an unrelated negative shock. This uncertainty about interpretation is a limitation of our analysis of a single event. Our next empirical strategy is therefore to use a continuous measure of information shocks, defined over a much longer time period, as outlined in the next section.

5.2 News Coverage Analysis

5.2.1 Basic News Coverage Model

For our main empirical exercise, we extend our sample period to the full range of dates discussed in Section 4.1: April 14, 2012 to July 15, 2015.[36] Our period of study begins with the April 2012 Istanbul meeting between Iran and the P5+1 for two main reasons. First, this was the beginning of a diplomatic process with no long-term breakdowns, unlike the several-month gap in talks that preceded Istanbul. Because relevant events occurred throughout the sample, we have sufficient power to identify our parameters of interest using comparisons within shorter subperiods (quarters). Second, significant changes were made to the multilateral sanctions regime over the several months before April 2012, but the majority of sanctions were in place by the time of the Istanbul meeting. This facilitates an identification strategy based on progress towards full sanctions relief instead of the imposition or removal of particular instruments, allowing us to relate our estimated effects to the sanctions regime as a whole rather than any single sanctions instrument. We end our sample period on July 15, 2015, the day after the final agreement was reached in Vienna.

[36] As discussed in Section 4.3, we exclude February to August 2013 for the GDELT news coverage measure because of a technical issue with GDELT article collection during this period.

To capture progress in diplomatic negotiations over the course of this period, we alternately use the daily event measures sourced from the Factiva and GDELT datasets.[37] In both cases, our measure of progress is a standardized variable based on the number of news articles about relevant events, as described in Section 4.3. We amend the model of equation (1) by replacing our Geneva dummy with this continuous news coverage variable as follows:

$$R_{ijt} = \alpha_i + \phi Coverage_t + \theta Target_i * Coverage_t + u_{ijt} \tag{2}$$

where again, i represents firms, j indexes industries and t indexes days, and the return and target variables are as above.

Column (1) of Table 3 Panels A and B displays the results of estimating this specification using the Factiva and GDELT measures, respectively. The Factiva estimates imply that an increase in our news coverage measure by one standard deviation is associated with a return that was 0.055 percentage points above the mean return for non-target firms, and an additional 0.065 percentage points above the mean for target firms. Both of these estimates are statistically significant at the 1% level. Our results using the GDELT measure are similar in magnitude and statistical significance: in this case, when news coverage is higher by one standard deviation, we see average abnormal returns of 0.069 percentage points in the non-target portfolio and 0.064 percentage points in addition to this for the target portfolio.[38]

In the remaining columns of Table 3, we refine these initial results by relying on variation within subperiods and/or industries. First, we note the possibility that our news coverage measure tends to rise over the course of our three-year sample period for reasons other than progress in negotiations (such as better measurement), which could bias our estimates upward if average stock returns also increase. Therefore, in column (2) we use within-quarter variation to arrive at our estimates. To do this, we substitute firm-quarter fixed effects for the firm fixed effects in equation (2), where 'quarter' henceforth refers to a unique quarter and year.[39] This has a relatively small effect on the estimated coefficients and standard errors.

[37] Of course, some of the events captured by these news coverage measures, such as the failed June 2012 negotiation round in Moscow, may represent setbacks rather than progress. However, because our sample period began with much doubt about whether a deal could ever be reached, and ended with a deal actually agreed, we argue that these measures capture forward progress on average.

[38] Clustering at the industry level using the wild bootstrap approach of Cameron, Gelbach and Miller (2008) yields p-values of 0.002 for our estimate of ϕ for both the Factiva and GDELT measures, and 0.002 (using Factiva) and 0.012 (using GDELT) for our estimate of θ.

[39] Our sample ends on July 15, 2015 and thus includes only ten trading days from the third quarter of 2015. In our regressions, we include these ten days in the second quarter of 2015.

We next consider the issue that both news coverage measures and stock returns may differ systematically between different days of the week. Given that we have chosen to aggregate measured news coverage over weekends and allocate this to the first day of the trading week (as discussed in Section 4.3), this is a real concern. In column (3), we further refine our fixed effects to the firm-quarter-day-of-week level, so that we are, for example, making comparisons between the returns of each firm on Saturdays in the fourth quarter of 2012 with higher and lower values of our coverage variable. This is our preferred specification, and we continue to use firm-quarter-day-of-week fixed effects in all subsequent specifications. Our estimate of the association between sanctions-related news coverage and non-target firms' returns is similar to before, but this new specification somewhat increases the estimated additional returns of firms in the target portfolio, to 0.092 percentage points in the case of the Factiva measure and 0.066 for the GDELT measure.

Finally, in column (4) of Table 3, we add interactions of our coverage measure with industry dummies to the model of column (3). As in column (4) of Table 2, this provides us with an indication of whether the additional effect on target firms is driven by the different industrial composition of the firms in the target and non-target portfolios. However, as in the previous section, we find that our estimates are not substantially affected by this change to the specification.

5.2.2 Heterogeneity and Robustness

Analysis by Sub-Period. While our news coverage regressions expand our identification strategy beyond the November 2013 Geneva agreement, they nonetheless still include this event. It is thus possible that our findings in Table 3 are driven by the same diplomatic progress in late 2013 that underpinned the results in Section 5.1. To check that our conclusions do not depend only on the Geneva event, we rerun the regression of column (3) of Table 3 for a period that excludes the entire sample frame used in Section 5.1 (the two Geneva event days and the sixty previous trading days). The results, displayed in column (2) of Table 4, are highly reassuring: our estimated coefficients are unsurprisingly somewhat smaller in magnitude (in comparison to the full-sample estimate in column (1), reproduced from Table 3 column (3)), but they remain positive and statistically significant.

As noted above, one key advantage of beginning our sample frame in April 2012 is that the full sanctions regime was essentially in place in its final form by this time, after significant changes over the two preceding years. However, a formal freeze on nuclear-related sanctions by the UN, EU and US was not in place until November 2013, when it was

agreed as part of the Geneva deal. The 'cleanest' test of the effects of diplomatic progress towards comprehensive sanctions relief, in a period without the imposition of new sanctions instruments, might therefore exclusively use post-Geneva variation in sanctions-related news.

In columns (3) to (5) of Table 4, we decompose the full-sample effect in column (1) into separate estimates for three nonoverlapping periods: the Geneva deal sample frame used in the previous subsection, and all trading days before and afterwards. The relationship between each news measure and returns is again statistically significant for non-target firms and significantly larger for target firms in the period between Geneva and the negotiation of a final agreement (column (5)).[40] Notably, though, the estimated coefficient on the target interaction is not statistically significant for the pre-Geneva period (column (3)); we will analyze this result more carefully below.

Additional Checks. Table 5 provides a number of additional robustness checks of our main results in column (3) of Table 3 (which we reproduce in Table 5 column (1)).[41] First, in column (2) of Table 5, we assess the robustness of our estimates to a change in our definition of the news coverage variables. Rather than using a continuous measure to proxy for diplomatic progress, we return to an event study model in which 'event days' are assigned a value of one and all other days receive a value of zero. Specifically, we define only the top 10% of observations as events, which corresponds (for both measures) approximately to a dummy for days with a number of relevant news articles at least one standard deviation above the mean. The estimates reported in column (2) show that returns of non-target firms are 0.072 or 0.145 percentage points higher on these event days, and those of target firms are an additional 0.153 or 0.196 percentage points larger, using GDELT and Factiva respectively; all of these estimated coefficients are statistically significant.

We next consider the possibility that the effects of sanctions news are different for larger firms, or for those firms with greater market capitalization. In Table 5 column (3), we control for the interaction of the logarithm of market capitalization and our news coverage measure. In column (4), we retain this new control variable, and also include controls interacting

[40]Recall that to construct the target and non-target portfolios, we use shareholder data from April 14, 2012, the first day of our sample period. To explore the implications of potential changes in ownership over time, we re-estimate the regression in Table 4 column (5) using target and non-target portfolios that are instead constructed via TSE shareholder data from November 20, 2013 (the final trading day before the Geneva deal). Results using the Factiva measure remain of similar magnitude and statistical significance, but in the case of the GDELT measure, the estimated coefficient on the interaction term is smaller by half and has a p-value of 0.19.

[41]Similar robustness checks of the findings relating to the Geneva deal in column (1) of Table 2 may be found in Appendix Table A2.

coverage with log turnover, log assets and log employees; the sample in this case is restricted to firms for which we have Orbis data in 2012. In both cases, our estimate of the coefficient on the interaction term does not change substantially from our baseline regression.[42]

The final two columns of Table 5 deal with institutional features of TSE trading. In column (5), we account for holidays other than two-day weekends, by adding fixed effects for the number of days off immediately before the trading day, as well as interactions of these with our target portfolio dummy variable. Meanwhile, in column (6), we deal with the possible concern that our results are driven by thinly traded stocks, by dropping the bottom 25% of thinly traded firms from the sample. This is the quartile of firms with the smallest number of days in which we observe any trading, over the course of the sample period. The stocks remaining in the sample are all traded on at least 87.5% of sample days. Neither of these robustness checks yields results that are substantially different from the baseline.

Potential Role of Smart Sanctions. Finally, we briefly explore the role of 'smart sanctions' instruments, as opposed to more comprehensive instruments such as restrictions on Iran's entire financial sector, in driving the targeting effects we observe.[43] We first return to the finding in Table 4 column (3) that target portfolio returns are not differentially sensitive to sanctions news before the beginning of our Geneva sample period; i.e. before mid-2013. This suggests the possibility that although most sanctions instruments were already in place before this time, the imposition of smart sanctions on Setad assets by the US Treasury in June 2013 (see Section 3.2.2) may have had an important effect. In Table A3, we look at sensitivity to sanctions news measures separately by conglomerate, before and after mid-2013. While returns of IRGC assets are more responsive to sanctions-related news than returns of non-target firms throughout the sample period, analogous effects for Setad-owned firms are apparent only after smart sanctions were imposed. This is suggestive evidence of the efficacy of these particular sanctions instruments.[44]

Our second exercise compares the target firms that were directly subject to smart sanctions with the other IRGC and Setad assets in the target portfolio. As reported in Table

[42]Note that the coefficient on news coverage is not of primary interest (since it now represents the association between news coverage and returns when all covariates equal zero), and so we do not display it in columns (3) and (4).

[43]Note that even in the absence of smart sanctions, more comprehensive instruments could generate the results we observe if their impacts on IRGC and Setad assets were stronger than their effects on other firms.

[44]An event study using the date of announcement of the Setad sanctions does not turn up significant related short-run movements in returns (results available upon request). This might be because the sanctions were anticipated by investors, but unfortunately, we are not aware of whether information about these sanctions became available before they were officially announced.

A4 columns (1) and (3), the estimated impact of progress towards a diplomatic agreement is very similar for each of these subgroups.[45] This result appears to indicate that smart sanctions had no additional effect on directly affected target firms.

However, rather than providing evidence against smart sanctions' effectiveness, the latter finding might instead suggest that their impacts spill over to other related firms. Although smart sanctions were imposed on only six of the target firms in our sample, each of the 44 other firms in the target portfolio had at least one owner that was itself subject to smart sanctions.[46] The study of sanctions on Russia by Ahn and Ludema (2017) notes the phenomenon of 'de-risking', in which potential business partners shy away from transactions with even an indirect connection to a firm subject to smart sanctions. To measure the strength of this connection in our context, we further subdivide indirectly affected target firms according to the ownership share of parents subject to IRGC or Setad-related smart sanctions. We find that firms with above-median shares by this measure are significantly more sensitive to sanctions news, though only when we use the Factiva coverage measure (see Table A4 columns (2) and (4)). Overall, these exercises suggest that smart sanctions might have been an important driver of our main results, but the evidence is not conclusive.

5.2.3 Magnitude of Effects

The 'event day' specification in the previous subsection provides a simple way of calculating a back-of-the-envelope estimate of the magnitude of the effect of diplomatic negotiations on firm returns. Specifically, say that diplomatic progress only occurred in days in the top decile of the news measure, and that articles identified by the Factiva or GDELT measures on all other days constitute irrelevant noise. Then we can simply multiply the estimates in column (2) of Table 5 by 0.1 (the number of days in the top decile as a share of the total number of trading days) in order to answer the question: what is the daily average return over the sample period that can be assigned to progress in sanctions negotiations?

Applying this methodology to the Factiva measure, non-target firms are estimated to have had a daily average return of 0.0145 due to progress in sanctions negotiations, which, according to Table 1, is just over 10% of the actual average daily return of firms in this portfolio over the period. The corresponding figure using GDELT is 0.0072, which is closer to 5% of the observed average daily return. For target firms, using Factiva, the daily average return due to diplomatic progress during the sample period is 0.034 (based on the sum of

[45] This conclusion is based on the full sample period, but also holds when we consider only the period after the US imposed smart sanctions on Setad.

[46] This is because of how we identify target firms, as discussed in Section 4.2.1.

our estimates of ϕ and θ), or approximately 25% of the average daily return of target firms over the sample period. Using GDELT, this is instead 0.023, or 17% of the observed average daily return of these firms. Based on this back-of-the-envelope method, we can thus credit a non-trivial fraction of the positive returns of the target portfolio over this period to progress in sanctions negotiations.

5.3 2016 US Election and Subsequent Events

We next examine the impact of the 2016 US election result on the abnormal returns of target and non-target firms. When the TSE opened on November 9, 2016, it was apparent that Donald Trump had very likely been elected US president, a surprising shift from the probabilities prevailing at the time of the market's closure on the previous day.[47] Because Trump had signaled his opposition to the Iran deal during the 2016 election campaign, while Hillary Clinton was a supporter of the agreement, this event provides us with an opportunity to study an exogenous shift in the probability of sanctions being reimposed by at least one of the deal's signatories.

As shown in column (1) of Table 6, the TSE's response to this shock is a mirror image of the estimated impact of the Geneva deal in column (1) of Table 2 (from which we reproduce the same specification, including a sixty-day pre-period and firm fixed effects). Average returns for non-target firms are 1.667 percentage points below their mean, while the abnormal return for target firms is even more negative: an additional 1.296 percentage points below average.[48] Figure 5 presents daily event study evidence similar to that in Figure 4, with the day of the election standing out as displaying significant negative effects for target group firms relative to the non-target portfolio. This response to the US election surprise is consistent with our earlier conclusions regarding the targeting of nuclear-related sanctions across listed firms in Iran.

In a final exercise, we investigate the responses of target and non-target firms to subsequent sanctions-related events, using our Factiva and GDELT news coverage measures. We study a sample period starting on the first trading day after the election event (November 12, 2016) and ending with the US withdrawal from the Iran deal (May 9, 2018). In columns

[47] See Silver (2016) for a discussion of expectations and predictions ahead of the 2016 US election. Silver's FiveThirtyEight model had the highest (29%) probability of Trump winning amongst a range of polling-based predictions, typically in the 1% to 15% band. In addition, Silver notes that betting markets put Trump's odds at 18% on the eve of the election.

[48] Appendix Table A5 presents robustness checks of this result based on the specifications in Table A2 and columns (3) and (4) of Table 2. Clustering by industry using the wild bootstrap yields p-values of 0.002 for the coefficient on the election dummy and 0.032 for the interaction term.

(2) and (3) of Table 6, we see that the estimated coefficient for non-target firms is positive; as shown in column (4), this is driven by news involving the non-US signatories to the sanctions agreement, who maintained support for the deal despite US opposition.[49] However, there is no longer a differential effect on target firms: the response of their stocks to these events is not statistically different from that of the non-target portfolio.

In contrast to the previous result, this suggests that senders' sanctions policy changes drifted 'off target' at some point after the election. However, while intriguing, both of the main results in this subsection should be treated with some caution. First, the post-2015 variation in our news coverage measures does not isolate shocks related to the multilateral nuclear deal as cleanly as before. As seen in Table A7, which lists the top fifteen event days for each measure during this period, we now also capture events such as Iranian missile tests and a confrontation at sea between the US and Iran. We therefore cannot interpret the findings in Table 6 columns (2) to (4) as relating to the expected incidence of sanctions as confidently as in Section 5.2.[50] At the same time, the election of Trump rather than Clinton represents a choice between a whole suite of likely policies, and we cannot separate the effect of a possible shift in US sanctions policy from impacts of other potential changes – though we will consider one key factor, conflict risk, in the next section.

6 Alternative Interpretation: Conflict Risk

The analysis above rests on the assumption that the content of our event dummies and news coverage measures consists of 'clean' information that is predominantly about sanctions. But progress in diplomatic negotiations may have had other implications for the foreign policy environment faced by Iran, specifically the probability of military conflict between Iran and other countries. We perform two exercises to assess the potential implications of this issue for the interpretation of our results, focusing alternatively on Iranian and non-Iranian firms.

[49] To define separate regressors for news involving the US and other countries in the P5+1 group, we use the fact that in the GDELT data, each event is identified with only one country other than Iran.

[50] In Table A6, we study a more precisely defined set of events based on the requirement for the US president to renew a waiver on most US sanctions every 120 days. This waiver, which was first signed by Barack Obama for the implementation of the 2015 Iran deal, was renewed by Donald Trump in May and September 2017 and January 2018, but not in May 2018, when Trump instead withdrew from the deal. Unfortunately, news reports suggest that each of these decisions was expected in advance. Nonetheless, we show the results of event studies based on Trump's three renewals (columns (1) to (4)) and the US withdrawal (column (5)). We observe impacts of both sets of events on non-target firms – positive when the waiver was renewed and negative when it was not – but again see no differential impact on target firms.

6.1 TSE Firm Returns and Conflict Risk

Our first exercise looks at the relative sensitivity of target and non-target firms to the risk of direct conflict. In order to measure the probability of conflict, we use political betting market prices for a contract regarding the likelihood that the US and/or Israel would "execute an overt airstrike against Iran by December 31, 2012", as discussed in Section 4.4. We then estimate the relationship of the stock returns of firms in each portfolio with the change in the daily arrival probability of an airstrike implied by these contract prices in Panel A of Table 7. To do this, we use the same set of specifications as in Table 3, with the day-to-day change in the probability of conflict replacing our news coverage measure, using data from the first day of our sample period to the end of 2012.[51]

The results indicate that increases in conflict probability, inferred from rises in contract prices, negatively affect stock returns for firms in our sample. In the specification with firm-quarter-day-of-week fixed effects in column (3), we find that an increase in the daily arrival probability of an airstrike that is higher by 0.1 percentage points (which is approximately three times the standard deviation of this variable) is associated with a statistically significant fall in returns of 0.158 percentage points. However, there is no differential sensitivity between the two portfolios, since the estimated coefficient on the interaction of our target group dummy with the change in arrival probability is statistically insignificant in all specifications. In short, while conflict risk appears to have a negative effect on firm returns across the TSE, there is no indication that the target group is more economically vulnerable to this risk.

6.2 Information Content of Sanctions-Related News Measures

In our second exercise, we examine the possibility that our news coverage measures contain confounding information about the probability of direct military conflict. The ideal sample for this exercise would be a group of firms that are 'war-sensitive' but not exposed to the effects of sanctions relief. Our approach to approximating this 'ideal set' of firms is to identify a group of companies on stock markets outside Iran who are sensitive to conflict risk (benefiting from increased odds of conflict) but, as non-Iranian firms, are not directly exposed to the local effects of sanctions.

As discussed in Section 4.4, we use a group of 66 firms from the 2012 SIPRI Arms Industry Database, which identifies the world's largest arms-producing and military services

[51] Ideally, we would also like to use information from similar contracts later in our sample period. However, as noted earlier, Intrade suspended its US accounts in late December 2012, and all of its activities in early 2013.

companies. We first confirm that this portfolio is sensitive to the odds of Iran-related conflict, since these firms' daily stock returns are strongly related to the change in the daily arrival probability of an airstrike implied by Intrade contract prices.[52] This allows us to use them as a placebo group to investigate whether conflict risk and sanctions news are confounded.

In columns (1) and (3) of Table 7 Panel B, we regress the daily returns of arms industry firms on each of our news coverage measures and firm-quarter-day-of-week fixed effects, as in the specification in column (3) of Table 3, using data from the full 2012-2015 sample period. The estimated coefficient on the coverage variable is statistically insignificant in each case. We then refine our exercise to check whether firms that are more sensitive to the odds of conflict between Iran and other countries respond differently to these news shocks. For each of the firms in the sample, we run a regression of daily returns in the quarter before the sample period (January to March 2012) on changes in airstrike arrival probability. We then rank the firms according to the size of the estimated coefficient on airstrike arrival probability (from most positive to most negative) and add the interaction between this variable and our sanctions news measure to our regressions. This again yields statistically insignificant results.

In Table A8 Panel A, we repeat this exercise using a Geneva event dummy rather than our news coverage measures, and similarly find no evidence that the returns of our sample of arms industry firms were affected by this sanctions-related event. In the same table, we show that the 2016 US election was followed by a surge in the stock market returns of these firms. Gains in defense stocks after Trump's victory were well-documented in media reports at the time, which cited expected rises in military spending under Trump (e.g. Forbes 2016, Washington Post 2016). However, we do not find evidence that this post-election bump was linked to heightened risk of conflict with Iran in particular, as firms ranking more highly in the sensitivity measure introduced above did not experience differential gains.[53]

7 Conclusion

In this study, we test a central plank of international sanctions policy: the efficacy of the targeting of elite decision-makers within a sanctioned country. We consider the case of Iran,

[52] More specifically, we find that a specification with daily return as the dependent variable and the change in the daily arrival probability of an airstrike implied by Intrade contract prices as the regressor, along with firm-quarter-day-of-week fixed effects, yields an estimated coefficient of 2.768 and a standard error of 0.361.

[53] In contrast, the interaction of each of our 2016-2018 news coverage measures with this firm-specific measure of sensitivity to Iran-related conflict does have a statistically significant estimated coefficient (see Table A8 Panel B). This again suggests that the 2016-2018 versions of our Factiva and GDELT measures may be capturing additional information other than sanctions news.

examining the response of its stock market to information indicating progress in diplomatic negotiations towards an agreement on sanctions removal. We find evidence that listed firms owned by key groups within Iran's political system were differentially sensitive to news about potential sanctions relief leading up to the final deal. Importantly, this sensitivity suggests that political elites within Iran faced a tangible economic incentive to negotiate the removal of sanctions.

The change in US policy towards Iran after the 2016 election raises the question of whether the response of Iranian firms to sanctions-related news has continued to follow a pattern consistent with successful targeting. Our analysis indicates that while target firms responded more sharply to Donald Trump's election, such differential returns are not evident for later events leading up to the 2018 US withdrawal from the Iran deal. However, news about sanctions policy is more difficult to interpret from 2016 onwards relative to the 2012-2015 period, when members of the P5+1 presented a unified front and news coverage was a reasonable proxy for forward progress in negotiations.

It should also be noted that while our focus on listed firms has made our study possible by allowing us to use high-frequency news shocks for identification, there are other important dimensions of the Iranian economy that we do not measure here. First, our findings exclude the costs from sanctions that were incurred by households. These are likely to have been considerable: in 2012, when the intensity of the sanctions regime reached its peak, Iranian GDP declined by 7.4%, while inflation exceeded 25% (World Bank 2019). Our results for listed firms therefore need to be put into the context of other likely economic and social costs. Second, we do not observe assets of the IRGC and Setad that are not listed on the TSE. Our results do, however, provide evidence that sanctions were 'on target' in the sense that sources of income for elite policymakers were positively affected by their removal. The evidence we present here therefore indicates that a 'complete policy failure' scenario – where Iran's political elites fully escaped direct negative income effects from sanctions – can be ruled out.

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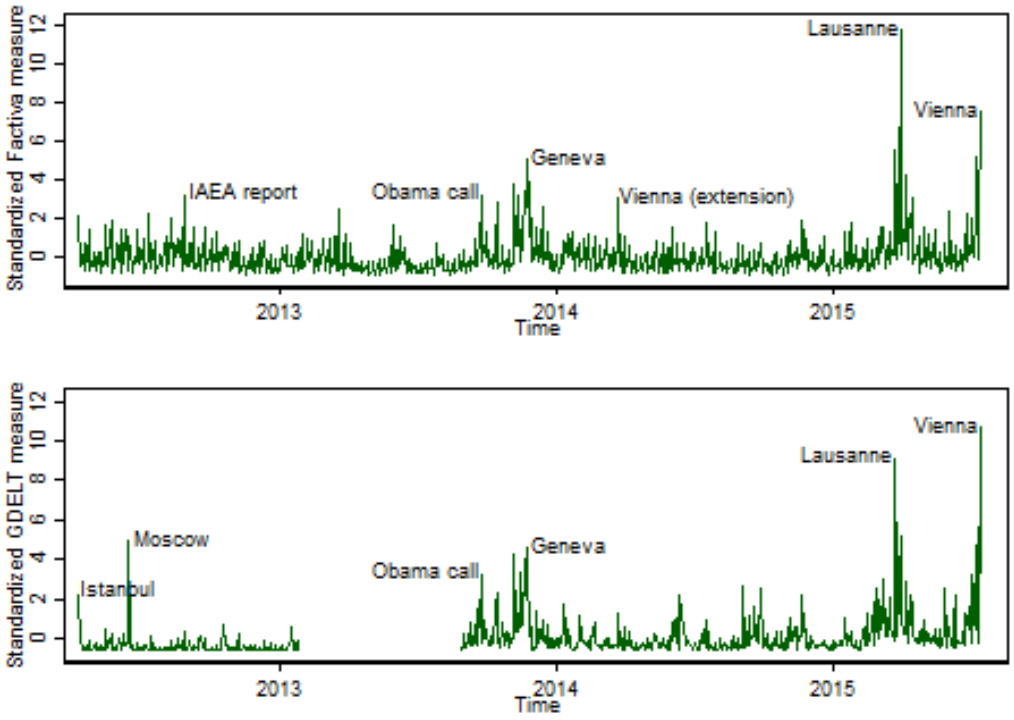

Figure 1: Value of Daily News Coverage Measures, April 14, 2012 to July 15, 2015

This figure shows the evolution, over the sample period April 14, 2012 to July 15, 2015 (excluding February to August 2013 for the GDELT measure), of a standardized count of the number of articles on relevant events identified in the Factiva and GDELT datasets, as discussed in Section 4.3. The standardized Factiva measure is displayed in the top panel and the standardized GDELT measure is shown in the bottom panel.

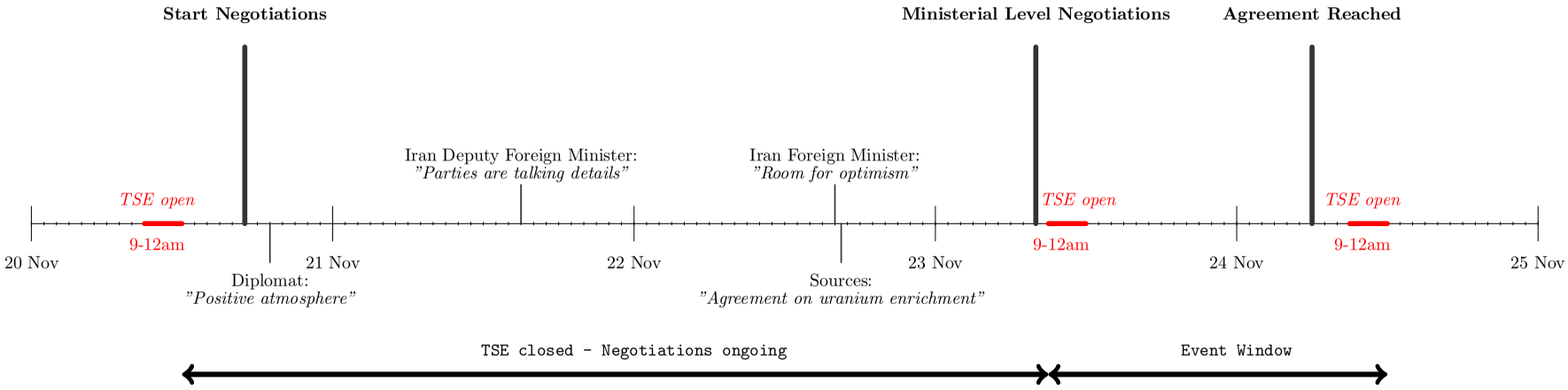

Figure 2: Timeline of November 2013 Geneva Event (With Quotes from Nasim Online (2013) Live Blog)

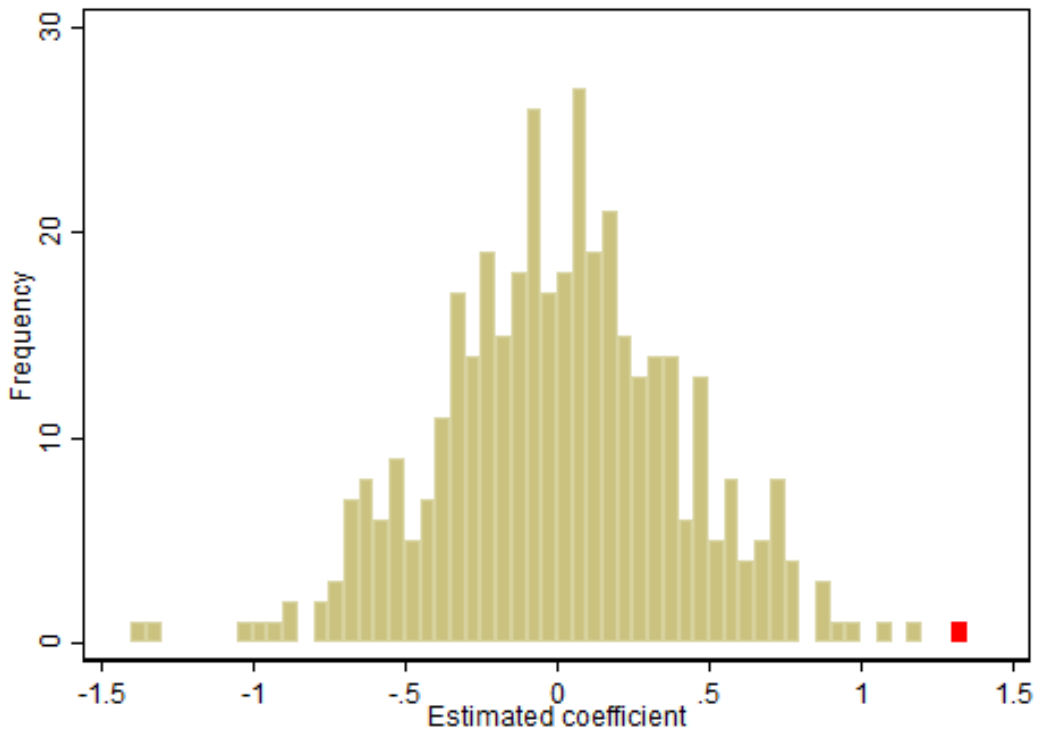

Figure 3: Distribution of Estimates Across Two-Day Periods (Geneva Deal Highlighted)

In this figure, we use data for the period from April 14-15, 2012 until July 14-15, 2015 to estimate a series of models, each with a dummy for a different two-day interval, the interaction of this with our target portfolio dummy, and firm fixed effects. Implementation of this procedure for the full set of consecutive two-day intervals corresponds to 388 regressions, and the histogram reports the distribution of estimated coefficients on the interaction term. The two-day Geneva event (November 23-24, 2013) is highlighted (the largest estimated coefficient).

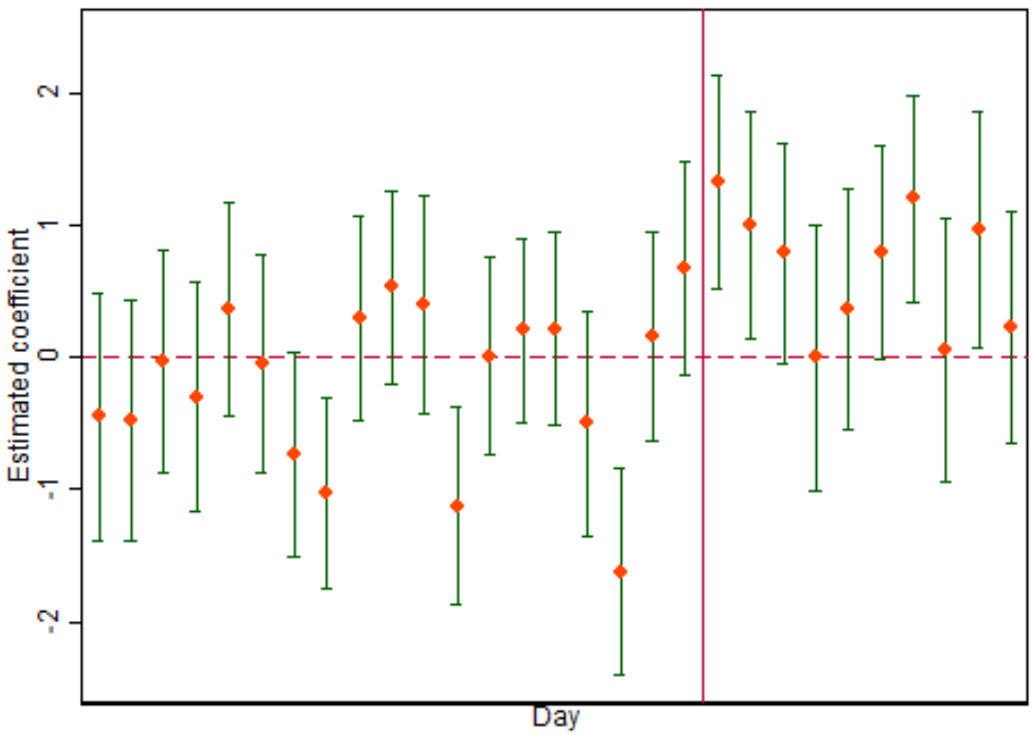

Figure 4: Daily Estimated Target-Day Interaction Coefficients Around Geneva Deal

In this figure, we use data for the 60-day estimation window used in Table 2 and the subsequent two weeks to estimate a series of models, each with a dummy for a different day, the interaction of this day dummy with our target portfolio dummy, and firm fixed effects. The figure reports the estimated coefficient on the interaction term and its 95% confidence interval for each trading day in the last four weeks of the 60-day estimation window and the following two weeks. The vertical line is drawn immediately to the left of the first day of the Geneva event (November 23, 2013). Standard errors are clustered by firm.

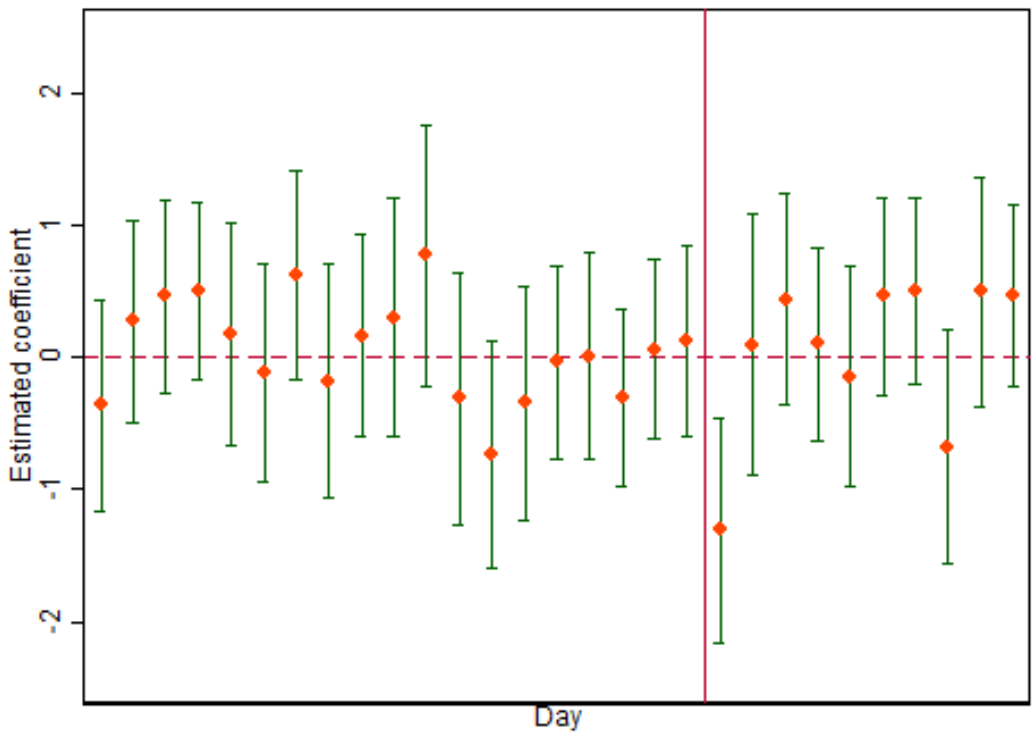

Figure 5: Daily Estimated Target-Day Interaction Coefficients Around 2016 US Election

In this figure, we use data for a period including the 60-day estimation window used in Table 6 column (1) and the subsequent two weeks to estimate a series of models, each with a dummy for a different day, the interaction of this day dummy with our target portfolio dummy, and firm fixed effects. The figure reports the estimated coefficient on the interaction term and its 95% confidence interval for each trading day in the last four weeks of the 60-day estimation window and the following two weeks. The vertical line is drawn immediately to the left of the US election event (November 9, 2016). Standard errors are clustered by firm.

Table 1: Summary Statistics – Sample Firms

	Target firms	Non-target firms
Number of firms	50	88
Daily return	0.134	0.136
	(2.01)	(2.05)
Market capitalization	12,357.47	7,908.29
	(31,273.68)	(22,461.24)
Share of TSE market capitalization	21.53	23.32
Share of firms by industry:		
Mining	2.0	4.6
Food products	4.0	6.8
Wood/paper/textiles	2.0	3.4
Coke/refined petroleum	4.0	1.1
Chemicals	2.0	10.2
Pharmaceuticals	6.0	10.2
Rubber/plastic/mineral products	28.0	18.1
Basic metals	10.0	10.2
Metal products	2.0	1.1
Electronics/electrical equipment	4.0	8.0
Machinery	4.0	2.3
Motor vehicles	12.0	8.0
Transportation/telecom	2.0	1.1
Finance	16.0	8.0
Construction/real estate	2.0	6.8
Orbis data:		
Number of firms	32	57
Turnover	3,566.48	5,034.33
	(8,899.41)	(23,743.09)
Assets	5,674.74	3,482.93
	(16,403.98)	(9,583.67)
Employees	1,679.71	785.21
	(3,451.24)	(1,269.74)

Daily stock return is in percent; its mean and standard deviation (in parentheses) are by firm-day, omitting the top and bottom 1% of observed returns. Market capitalization, turnover and assets are in billion rial. Market capitalization (from TSE data) is as of March 10, 2014. Total TSE market capitalization is calculated for firms listed on the TSE as of April 14, 2012, omitting two firms for which data is unavailable. We use a listing of firms by sector in TSE (2011) to classify firms into industries based on two-digit NACE codes such that there are no industries in which only one of the two portfolios is represented. Data from Orbis on turnover, assets and employees is from 2012, and is only available for a subset of firms. The number of firms with available data on turnover and assets is listed in the table; employee data is missing for an additional nine firms (four target firms and five non-target firms).

Table 2: Baseline Results – Geneva Deal

	(1) Firm FEs	(2) Day-by-day estimates	(3) Firm-day-of-week FEs	(4) Industry interactions
Geneva	0.648**		0.731**	
	(0.277)		(0.286)	
Day 1		0.548*		
		(0.303)		
Day 2		0.748**		
		(0.342)		
Target * Geneva	1.256***		1.212***	1.270***
	(0.364)		(0.383)	(0.400)
Target * Day 1		1.414***		
		(0.417)		
Target * Day 2		1.110**		
		(0.445)		
Observations	6,587	6,587	6,587	6,587
Number of firms	129	129	129	129

This table displays estimated effects of the Geneva deal on returns of target and non-target firms. The dependent variable is daily stock return in percent. 'Geneva' is defined as the two days Saturday November 23 and Sunday November 24, 2013. In column (2), 'day 1' is November 23, 2013 and 'day 2' is November 24, 2013. The sample period is Geneva and the previous sixty trading days. Columns (1), (2) and (4) include firm fixed effects, and column (3) includes firm-day-of-week fixed effects. Column (4) includes industry * Geneva interactions for fifteen industries. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table 3: Baseline Results – News Coverage

	(1) Firm FEs	(2) Firm-quarter FEs	(3) Firm-quarter-day-of-week FEs	(4) Industry interactions
Panel A - Factiva measure				
Coverage	0.055***	0.082***	0.077***	
	(0.010)	(0.010)	(0.013)	
Target * coverage	0.065***	0.058***	0.092***	0.099***
	(0.014)	(0.015)	(0.019)	(0.019)
Observations	75,021	75,021	75,021	75,021
Number of firms	138	138	138	138
Panel B - GDELT measure				
Coverage	0.069***	0.068***	0.065***	
	(0.011)	(0.011)	(0.013)	
Target * coverage	0.064***	0.054***	0.066***	0.075***
	(0.016)	(0.016)	(0.017)	(0.018)
Observations	62,129	62,129	62,129	62,129
Number of firms	138	138	138	138

This table displays estimated effects on returns of target and non-target firms from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable is daily stock return in percent. The variable 'coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (Panel A) or GDELT (Panel B) data, as discussed in Section 4.3. Column (1) includes firm fixed effects, column (2) includes firm-quarter fixed effects and columns (3) and (4) include firm-quarter-day-of-week fixed effects. Column (4) includes industry * coverage interactions for 15 industries. Here, 'quarter' refers to a unique quarter and year. The sample period is from April 14, 2012 to July 15, 2015 (excluding February to August 2013 in Panel B). Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table 4: Heterogeneity – Varying Sample Period

	(1) Full sample	(2) No Geneva	(3) Pre-Geneva	(4) Geneva	(5) Post-Geneva
Panel A - Factiva measure					
Coverage	0.077***	0.070***	0.129***	0.261***	0.062***
	(0.013)	(0.012)	(0.035)	(0.052)	(0.013)
Target * coverage	0.092***	0.055***	-0.019	0.302***	0.067***
	(0.019)	(0.018)	(0.052)	(0.076)	(0.021)
Observations	75,021	68,434	28,759	6,587	39,675
Number of firms	138	138	138	129	131
Panel B - GDELT measure					
Coverage	0.065***	0.062***	0.042	0.150***	0.064***
	(0.013)	(0.012)	(0.039)	(0.038)	(0.013)
Target * coverage	0.066***	0.045**	0.030	0.152***	0.048**
	(0.017)	(0.018)	(0.052)	(0.051)	(0.020)
Observations	62,129	55,947	16,272	6,182	39,675
Number of firms	138	138	138	129	131

This table displays estimated effects on returns of target and non-target firms from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable is daily stock return in percent. The variable 'coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (Panel A) or GDELT (Panel B) data. In Panel A, the sample period in column (1) is from April 14, 2012 to July 15, 2015, in column (2) it is from April 14, 2012 to July 15, 2015 excluding August 26, 2013 to November 24, 2013, in column (3) it is from April 14, 2012 to August 25, 2013, in column (4) it is from August 26, 2013 to November 24, 2013 and in column (5) it is from November 25, 2013 to July 15, 2015. In Panel B, the sample period in all columns excludes February to August 2013. All columns include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table 5: Additional Robustness Checks – News Coverage

	(1) Baseline	(2) Top 10% dummy	(3) Market cap control	(4) All size controls	(5) Days off controls	(6) Thin trading
Panel A - Factiva measure						
Coverage	0.077***	0.145***			0.051***	0.092***
	(0.013)	(0.041)			(0.016)	(0.013)
Target * coverage	0.092***	0.196***	0.090***	0.067**	0.110***	0.082***
	(0.019)	(0.062)	(0.019)	(0.026)	(0.023)	(0.019)
Observations	75,021	75,021	75,021	45,065	75,021	66,022
Number of firms	138	138	138	80	138	103
Panel B - GDELT measure						
Coverage	0.065***	0.072**			0.060***	0.083***
	(0.013)	(0.033)			(0.016)	(0.013)
Target * coverage	0.066***	0.153***	0.058***	0.070***	0.075***	0.045**
	(0.017)	(0.050)	(0.018)	(0.022)	(0.021)	(0.018)
Observations	62,129	62,129	62,129	37,241	62,129	54,605
Number of firms	138	138	138	80	138	103

This table displays estimated effects on returns of target and non-target firms from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable is daily stock return in percent. In all columns, the variable 'coverage' is based on the number of articles on a relevant event identified in the Factiva (Panel A) or GDELT (Panel B) data. This is a dummy for the top 10% of observed values in column (2) and a standardized count in all other columns. Column (3) includes interactions of log market capitalization as of March 10, 2014 with the coverage variable. Column (4) includes interactions of log market capitalization as of March 10, 2014, and log turnover, log assets and log employees from 2012 (if this data is available from Orbis), with the coverage variable. Column (5) includes fixed effects for the number of TSE days off immediately before the trading day and interactions of these fixed effects with the target dummy. Column (6) drops the bottom 25% of firms from the sample according to the number of days in the sample period that their stocks were traded on the TSE. All columns include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. The sample period is from April 14, 2012 to July 15, 2015 (excluding February to August 2013 in Panel B). Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table 6: Post-Deal Events – Potential Restoration of US Sanctions

	(1)	(2)	(3)	(4)
	US election	Factiva measure	GDELT measure	
	Nov 2016	post-election	post-election	
Election	-1.667***			
	(0.305)			
Target * election	-1.296***			
	(0.434)			
Coverage		0.040**	0.051***	
		(0.017)	(0.016)	
Target * coverage		-0.031	-0.036	
		(0.021)	(0.023)	
US coverage				0.015
				(0.016)
Target * US coverage				-0.007
				(0.023)
P4+1 coverage				0.044**
				(0.019)
Target * P4+1 coverage				-0.035
				(0.027)
Observations	6,346	36,557	36,557	36,557
Number of firms	120	123	123	123

This table displays estimated effects of the 2016 US election and subsequent sanctions-related events on returns of target and non-target firms. 'Election' is defined as the event day Wednesday November 9, 2016. 'Coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (column (2)) or GDELT (columns (3) and (4)) data. 'US coverage' is a standardized count of the number of articles on a relevant event in the GDELT data in which the US is the country other than Iran identified by the GDELT dataset, while 'P4+1 coverage' is an analogous measure for the other members of the P5+1 group. The dependent variable is daily stock return in percent. In column (1), the sample period is the election event day and the previous sixty trading days. In columns (2), (3) and (4), the sample period is from November 12, 2016 to May 9, 2018. Column (1) includes firm fixed effects and columns (2), (3) and (4) include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table 7: Alternative Explanation – Conflict Risk

Panel A - Intrade airstrike arrival probability				
	(1) Firm FEs	(2) Firm-quarter FEs	(3) Firm-quarter-day-of-week FEs	(4) Industry interactions
Change in arrival probability	-1.405**	-1.494**	-1.580**	
	(0.609)	(0.602)	(0.606)	
Target * change in arrival probability	-0.819	-0.860	-0.783	-0.696
	(0.863)	(0.845)	(0.844)	(0.938)
Observations	14,012	14,012	14,012	14,012
Number of firms	137	137	137	137

Panel B - Arms industry firms				
	(1)	(2)	(3)	(4)
	Factiva measure		GDELT measure	
Coverage	-0.00005	-0.012	0.008	0.009
	(0.007)	(0.014)	(0.006)	(0.014)
Sensitivity rank * coverage		0.0004		0.00003
		(0.0004)		(0.0004)
Observations	50,587	50,587	41,519	41,519
Number of firms	66	66	66	66

This table displays the results of regressions exploring the possibility that the main results are driven by changes in conflict risk. Panel A displays correlations between returns of target and non-target firms and the day-to-day change in the arrival probability of a US or Israeli airstrike against Iran, calculated from the price of the Intrade contract "US and/or Israel to execute an overt airstrike against Iran by December 31, 2012". Panel B displays estimated effects on returns of non-Iranian firms in the arms industry from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable in both panels is daily stock return in percent. 'Change in arrival probability' represents the change in the arrival probability of a US or Israeli airstrike against Iran, during the same day, in percent. 'Coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (columns (1) and (2)) or GDELT (columns (3) and (4)) data. 'Sensitivity rank' is the rank of each firm based on the responsiveness of its stock return to the change in the airstrike arrival probability in the first quarter of 2012, as discussed in Section 6.2. In Panel A, column (1) includes firm fixed effects, column (2) includes firm-quarter fixed effects, columns (3) and (4) include firm-quarter-day-of-week fixed effects and column (4) adds industry * coverage interactions for fifteen industries. All columns in Panel B include firm-quarter-day-of-week fixed effects. Here, 'quarter' refers to a unique quarter and year. In Panel A, the sample period is from April 14, 2012 to December 22, 2012, and in Panel B, it is from April 16, 2012 to July 15, 2015. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

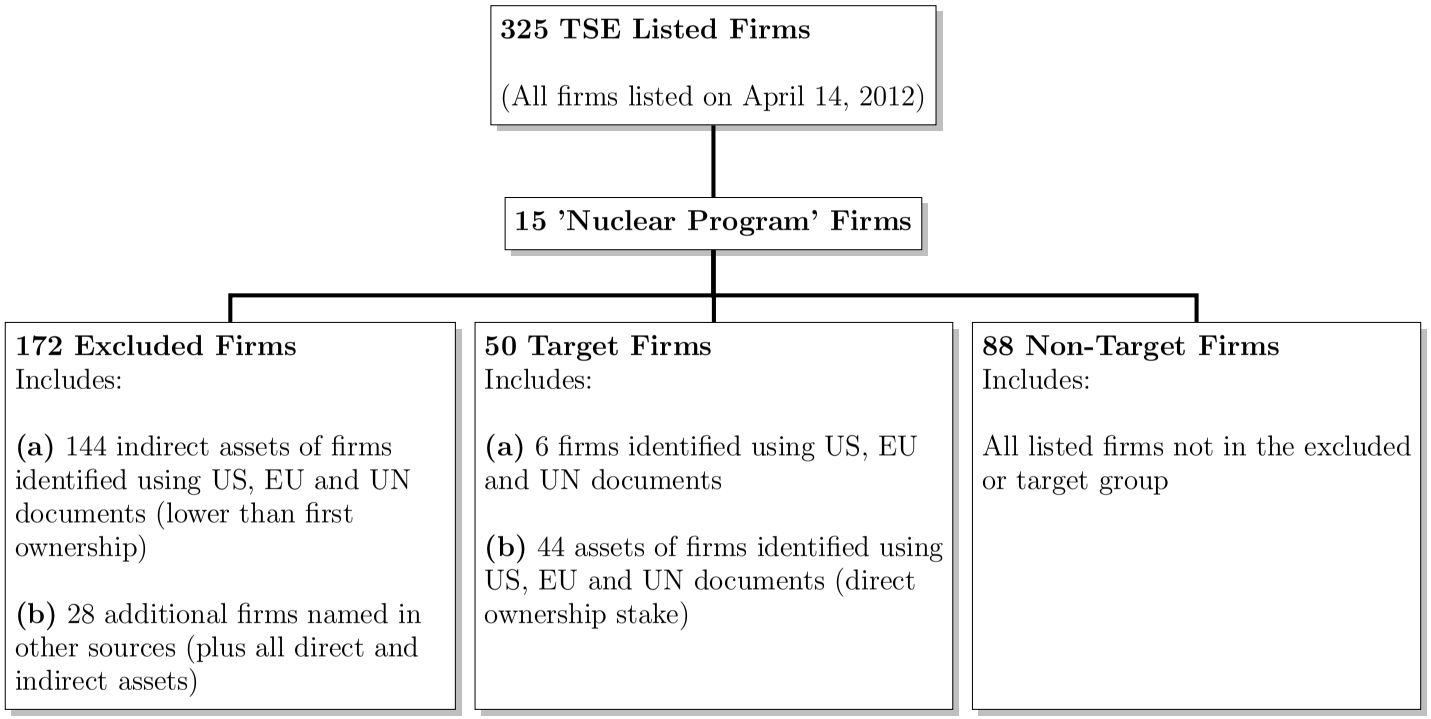

FIGURE A1: Outline of Firm Classification Procedure

This diagram outlines our procedure for classifying listed firms into the target and non-target portfolios used in our empirical analysis.

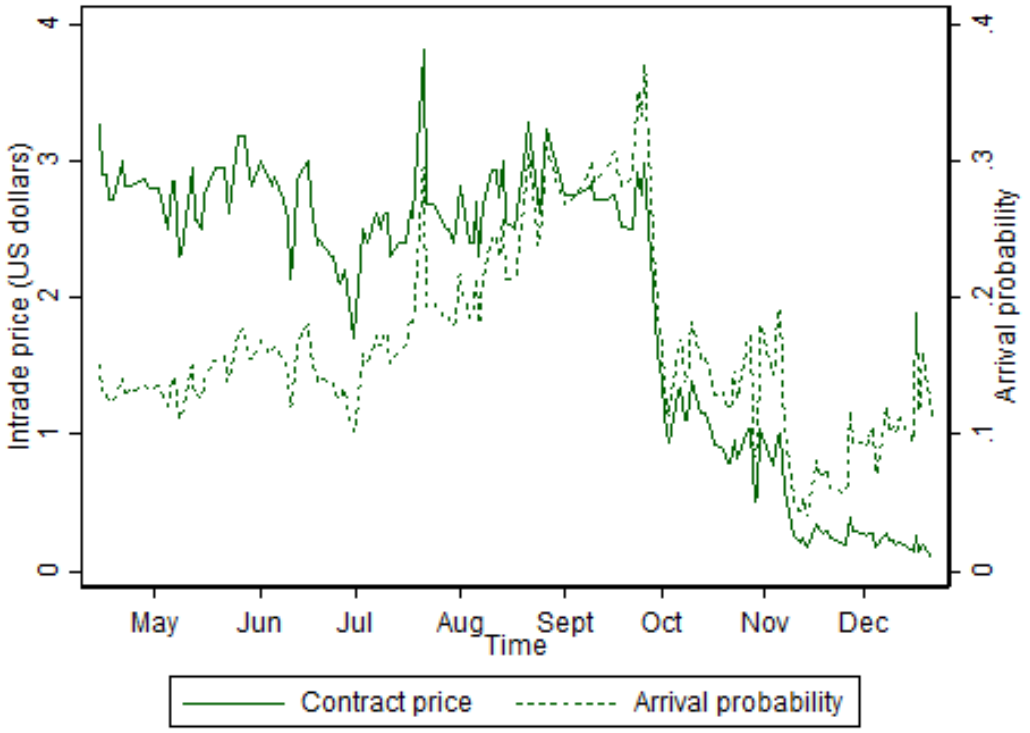

Figure A2: Daily Price of Intrade Contract and Implied Airstrike Arrival Probability, April 14, 2012 to December 22, 2012

This figure shows the evolution of Intrade contract prices (in current US dollars) for the betting contract specified as "US and/or Israel execute an overt airstrike against Iran by December 31, 2012", and the implied daily arrival probability (in percent) of an airstrike, as discussed in Section 4.4. The contract was to have paid $10 if an airstrike occurred before December 31, 2012, and zero otherwise. The period plotted is April 14, 2012 to December 22, 2012 (the day before US Intrade trading was suspended).

Table A1: Top Fifteen Peaks in Sanctions News Coverage

Date	Value	Sanctions negotiations event
Panel A - Factiva measure		
Apr 4 2015*	**11.76**	**Framework agreement reached in Lausanne**
Jul 14 2015	**7.58**	**Final agreement reached in Vienna**
Jul 15 2015	**7.03**	**Final agreement reached on previous day in Vienna**
Mar 25 2015*	**5.60**	**Negotiation round ends in Lausanne**
Jul 11 2015*	**5.20**	**Negotiation round in progress in Vienna**
Nov 25 2013	5.07	Interim agreement reached on previous day in Geneva
Apr 11 2015*	4.26	Framework agreement reached in previous week in Lausanne
Nov 9 2013*	**3.74**	**Negotiation round ends in Geneva**
Nov 23 2013*	**3.40**	**Negotiation round in progress in Geneva**
Sep 2 2012*	3.23	France calls for stronger sanctions after IAEA report
Sep 28 2013*	**3.23**	**Presidents of Iran and US speak by phone**
Nov 16 2013*	3.13	Negotiation round forthcoming in Geneva
Apr 18 2015*	3.13	Framework agreement reached two weeks earlier in Lausanne
Mar 25 2014*	3.09	Negotiation round ends in Vienna
Nov 26 2013	2.85	Interim agreement reached two days earlier in Geneva
Panel B - GDELT measure		
Jul 14 2015	**10.66**	**Final agreement reached in Vienna**
Mar 25 2015*	**9.07**	**Negotiation round ends in Lausanne**
Apr 4 2015*	**5.16**	**Framework agreement reached in Lausanne**
Jun 19 2012*	4.97	Negotiation round ends in Moscow
Jul 11 2015*	**4.67**	**Negotiation round in progress in Vienna**
Nov 24 2013	4.55	Interim agreement reached in Geneva
Nov 9 2013*	**4.22**	**Negotiation round ends in Geneva**
Nov 23 2013*	**3.55**	**Negotiation round in progress in Geneva**
Nov 18 2013	3.33	Negotiation round about to begin in Geneva
Jul 15 2015	**3.26**	**Final agreement reached on previous day in Vienna**
Sep 28 2013*	**3.23**	**Presidents of Iran and US speak by phone**
Jul 4 2015*	3.21	Negotiation round in progress in Vienna
Mar 31 2015	3.17	Negotiation deadline extended in Lausanne
Mar 7 2015*	2.95	Several P5+1 members discuss negotiations
Mar 28 2015*	2.89	Negotiation round in progress in Lausanne

This table displays the top fifteen observations, within the sample period April 14, 2012 to July 15, 2015, of a standardized count of the number of articles on relevant events identified in the Factiva (Panel A) and GDELT (Panel B) datasets, as discussed in Section 4.3. For each of these observations, the table shows the relevant date, the value of the variable, and an event of that day that is relevant to diplomatic negotiations for sanctions relief. Entries that are among the top fifteen observations for both of the two measures are highlighted in bold. Dates are marked with stars when the observation also includes events from prior days because of weekends or holidays on the TSE.

Table A2: Additional Robustness Checks – Geneva Deal

	(1) Baseline	(2) Market cap control	(3) All size controls	(4) Days off controls	(5) Thin trading
Geneva	0.648**			0.630**	0.611*
	(0.277)			(0.274)	(0.323)
Target * Geneva	1.256***	1.260***	1.948***	1.226***	1.467***
	(0.364)	(0.372)	(0.468)	(0.362)	(0.422)
Observations	6,587	6,587	4,018	6,587	5,434
Number of firms	129	129	78	129	94

This table displays estimated effects of the Geneva deal on returns of target and non-target firms. The dependent variable is daily stock return in percent. 'Geneva' is defined as the two days Saturday November 23 and Sunday November 24, 2013. Column (2) includes interactions of log market capitalization as of March 10, 2014 with the Geneva variable. Column (3) includes interactions of log market capitalization as of March 10, 2014, and log turnover, log assets and log employees from 2012 (if this data is available from Orbis), with the Geneva variable. Column (4) includes fixed effects for the number of TSE days off immediately before the trading day and interactions of these fixed effects with the target dummy. Column (5) drops the bottom 25% of firms from the sample according to the number of days in the sample period that their stocks were traded on the TSE. The sample period is Geneva and the previous sixty trading days. All columns include firm fixed effects. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table A3: Heterogeneity by Conglomerate – IRGC and Setad Assets

	(1) Full sample	(2) Before Setad sanctions	(3) After Setad sanctions
Panel A - Factiva measure			
Coverage	0.077***	0.138***	0.066***
	(0.013)	(0.038)	(0.014)
IRGC * coverage	0.149***	0.115	0.145**
	(0.044)	(0.071)	(0.055)
Setad * coverage	0.085***	-0.075	0.106***
	(0.019)	(0.053)	(0.021)
Observations	75,021	23,544	51,477
Number of firms	138	138	132
Panel B - GDELT measure			
Coverage	0.065***	0.042	0.067***
	(0.013)	(0.039)	(0.013)
IRGC * coverage	0.077*	0.126*	0.072
	(0.041)	(0.072)	(0.045)
Setad * coverage	0.065***	0.015	0.070***
	(0.018)	(0.054)	(0.019)
Observations	62,129	16,272	45,857
Number of firms	138	138	131

This table displays estimated effects on returns of target firms (divided into those that are IRGC assets and those that are Setad assets) and non-target firms from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable is daily stock return in percent. The variable 'coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (Panel A) or GDELT (Panel B) data. In Panel A, the sample period in column (1) is from April 14, 2012 to July 15, 2015, in column (2) it is from April 14, 2012 to June 3, 2013 and in column (3) it is from June 8, 2013 to July 15, 2015. In Panel B, the sample period in all columns excludes February to August 2013. All columns include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table A4: Heterogeneity – Firms Directly and Indirectly Subject to Smart Sanctions

	(1)	(2)	(3)	(4)
	Factiva measure		GDELT measure	
Coverage	0.077***	0.077***	0.065***	0.065***
	(0.013)	(0.013)	(0.013)	(0.013)
Smart sanctions * coverage	0.102**	0.102**	0.071*	0.071*
	(0.042)	(0.042)	(0.042)	(0.042)
Other * coverage	0.090***		0.066***	
	(0.020)		(0.018)	
High share * coverage		0.118***		0.071***
		(0.026)		(0.022)
Low share * coverage		0.064***		0.060***
		(0.021)		(0.021)
Observations	75,021	75,021	62,129	62,129
Number of firms	138	138	138	138

This table displays estimated effects on returns of target and non-target firms from specifications that include a daily measure of news coverage related to diplomatic progress between Iran and the P5+1 countries. The dependent variable is daily stock return in percent. The variable 'coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (columns (1) and (2)) or GDELT (columns (3) and (4)) data. Columns (1) and (3) separate the target portfolio into two groups: firms ever subject to smart sanctions and other target firms. Columns (2) and (4) additionally classify the latter set of firms into two subgroups: those with above-median ownership by firms ever subject to IRGC or Setad-related smart sanctions ('high share') and those with below-median ownership by firms ever subject to IRGC or Setad-related smart sanctions ('low share'). All columns include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. The sample period is from April 14, 2012 to July 15, 2015 (excluding February to August 2013 in columns (3) and (4)). Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table A5: Additional Robustness Checks – Election Event

	(1) Baseline	(2) Firm-day-of-week FEs	(3) Industry interactions	(4) Market cap control	(5) All size controls	(6) Days off controls	(7) Thin trading
Election	-1.667***	-1.704***				-1.637***	-1.948***
	(0.305)	(0.319)				(0.302)	(0.340)
Target * election	-1.296***	-1.236***	-1.476***	-1.483***	-1.143*	-1.290***	-0.997**
	(0.434)	(0.434)	(0.446)	(0.428)	(0.599)	(0.431)	(0.473)
Observations	6,346	6,346	6,346	6,346	3,851	6,346	5,129
Number of firms	120	120	120	120	74	120	88

This table displays estimated effects of the 2016 US election on returns of target and non-target firms. 'Election' is defined as the event day Wednesday November 9, 2016. All columns except column (2) include firm fixed effects. Column (2) includes firm-day-of-week fixed effects. Column (3) includes industry * election interactions for fifteen industries. Column (4) includes interactions of log market capitalization as of March 10, 2014 with the election variable. Column (5) includes interactions of log market capitalization as of March 10, 2014, and log turnover, log assets and log employees from 2012 (if this data is available from Orbis), with the election variable. Column (6) includes fixed effects for the number of TSE days off immediately before the trading day and interactions of these fixed effects with the target dummy. Column (7) drops the bottom 25% of firms from the sample according to the number of days in the sample period that their stocks were traded on the TSE. The sample period is the election event day and the previous sixty trading days. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table A6: Post-Deal Events – Additional Specifications

	(1)	(2)	(3)	(4)	(5)
		US waiver renewals			US withdrawal
	May 2017	Sept 2017	Jan 2018	All three	May 2018
Renewal	1.590***	0.330	0.486**	0.807***	
	(0.243)	(0.256)	(0.243)	(0.163)	
Target * renewal	-0.187	-0.194	-0.156	-0.272	
	(0.388)	(0.319)	(0.284)	(0.210)	
Withdrawal					-0.633**
					(0.240)
Target * withdrawal					0.071
					(0.335)
Observations	6,116	6,119	6,554	18,789	6,485
Number of firms	121	122	121	123	121

This table displays estimated effects of sanctions-related events after the 2016 US election on returns of target and non-target firms. 'Renewal' represents an event day when US sanctions waivers were renewed: May 20, 2017, September 16, 2017 and/or January 13, 2018. Each renewal event is studied separately in columns (1) to (3), while column (4) jointly considers all three renewal events. 'Withdrawal' represents the US withdrawal from the sanctions deal on May 9, 2018. The dependent variable is daily stock return in percent. In columns (1), (2), (3) and (5), the sample period is the event day and the sixty preceding trading days. In column (4), the sample period consists of the three renewal event days and the sixty trading days prior to each of these event days. All columns include firm fixed effects. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).

Table A7: Top Fifteen Peaks in News Coverage Measures – 2016-2018 Sample Period

Date	Value	Event
Panel A - Factiva measure		
May 9 2018	**8.89**	**US withdraws from agreement on previous day**
May 8 2018	**6.68**	**US withdraws from agreement**
Oct 14 2017*	**4.83**	**US decertifies agreement**
Feb 4 2017*	**4.71**	**US imposes sanctions after missile test**
May 5 2018*	**4.04**	**US set to withdraw from agreement**
Jul 29 2017*	**3.97**	**US-Iran confrontation at sea**
Apr 28 2018*	3.95	US Secretary of State calls for new sanctions
Jan 13 2018*	**3.36**	**US renews sanctions waiver**
Dec 3 2016*	2.87	US extends Iran Sanctions Act
Aug 6 2017*	2.68	Some P5+1 members attend Iran presidential inauguration
Mar 25 2018*	2.63	US imposes sanctions after hacking episode
Jan 6 2018*	2.34	Demonstrations take place across Iran
Sep 23 2017*	**2.17**	**Iran tests missile, some P5+1 members support deal**
May 7 2018	1.84	US set to withdraw from agreement
Jul 26 2017	1.63	Iran imposes sanctions on US firms
Panel B - GDELT measure		
Oct 14 2017*	**7.54**	**US decertifies agreement**
May 9 2018	**5.40**	**US withdraws from agreement on previous day**
May 8 2018	**4.97**	**US withdraws from agreement**
Jan 30 2017	4.00	Iran tests missile on previous day
Jan 13 2018*	**3.87**	**US renews sanctions waiver**
Jul 29 2017*	**3.67**	**US-Iran confrontation at sea**
Feb 4 2017*	**3.54**	**US imposes sanctions after missile test**
Sep 23 2017*	**3.17**	**Iran tests missile, some P5+1 members support deal**
May 6 2017*	3.13	Agreement discussed at Iranian presidential debate
Jan 31 2017	2.80	Iran tests missile two days earlier
Dec 18 2016*	2.70	US confirms extension of Iran Sanctions Act
Jan 29 2017	2.52	Iran tests missile
May 5 2018*	**2.40**	**US set to withdraw from agreement**
Dec 20 2016	2.31	Iran meets Russia to discuss Syria
May 1 2018	2.23	Israel claims existence of Iran nuclear program

This table displays the top fifteen observations, within the sample period November 12, 2016 to May 9, 2018, of a standardized count of the number of articles on relevant events identified in the Factiva (Panel A) and GDELT (Panel B) datasets, as discussed in Section 5.3. For each of these observations, the table shows the relevant date, the value of the variable, and an event of that day that is relevant to relations between countries of the P5+1 and Iran. Entries that are among the top fifteen observations for both of the two measures are highlighted in bold. Dates are marked with stars when the observation also includes events from prior days because of weekends or holidays on the TSE.

Table A8: Conflict Risk – Additional Specifications

Panel A - Geneva and election event studies				
	(1)	(2)	(3)	(4)
	Geneva event		Election event	
Event	-0.003	-0.067	1.221***	1.646*
	(0.138)	(0.282)	(0.421)	(0.859)
Sensitivity rank		0.002		-0.012
* event		(0.008)		(0.023)
Observations	3,788	3,788	3,752	3,752
Number of firms	66	66	64	64
Panel B - Post-election news coverage				
	(1)	(2)	(3)	(4)
	Factiva measure		GDELT measure	
Coverage	0.029***	0.002	-0.024**	-0.062***
	(0.009)	(0.018)	(0.010)	(0.019)
Sensitivity rank		0.0008*		0.0011**
* coverage		(0.0005)		(0.0005)
Observations	22,516	22,516	22,516	22,516
Number of firms	64	64	64	64

This table displays estimated effects from specifications based on returns of non-Iranian firms in the arms industry. The dependent variable in both panels is daily stock return in percent. 'Event' is defined as Monday November 25, 2013 in columns (1) and (2) and Wednesday November 9, 2016 in columns (3) and (4). 'Coverage' is a standardized count of the number of articles on a relevant event identified in the Factiva (columns (1) and (2)) or GDELT (columns (3) and (4)) data. 'Sensitivity rank' is the rank of each firm based on the responsiveness of its stock return to the daily change in the airstrike arrival probability in the first quarter of 2012, as discussed in Section 6.2. All columns in Panel A include firm fixed effects, and all columns in Panel B include firm-quarter-day-of-week fixed effects, where 'quarter' refers to a unique quarter and year. In Panel A, the sample period is the event day (for the Geneva deal in columns (1) and (2) and for the 2016 US election in columns (3) and (4)) and the sixty previous trading days, and in Panel B, the sample period is from November 12, 2016 to May 9, 2018. Standard errors, clustered by firm, are in parentheses. Small p-values are represented by *** (below 1%), ** (below 5%) or * (below 10%).