Does political conflict hurt trade? Evidence from consumer boycotts

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ABSTRACT

I estimate the impact of international conflict on bilateral trade relations using several incidents of politically motivated boycotts: The boycott of Danish goods by Muslim countries following the Muhammad Comic Crisis in 2005/2006, the Chinese boycott of Japanese goods in response to the Senkaku/Diaoyu Island conflict in 2012, the boycott of French products in the US over the Iraq War in 2003, and Turkey's boycott of Israel over the Gaza conflict in 2014. The results from difference-in-differences regressions and the synthetic control group method show that boycotts can have strong negative effects on bilateral trade in both goods and services. I estimate an average one-year trade disruption of 18.8% in the case of Denmark, 2.7% for Japan, and 1.7% for French imports, where in the latter two cases this effect is only short term. For all boycott instances, this is only a minor share of overall exports of the boycotted country over the same period. For the Iraq and Gaza conflicts, there is a reciprocal negative effect on the boycotted countries' imports from the boycotter. Product-level results are in line with intuition: Boycotts are most effective for consumer goods, especially highly-branded signature export goods such as Japanese cars, while having at most a temporary effect on intermediates and capital goods. An event study on Japanese stock market returns suggests that the Chinese boycott depressed stock values of explicitly boycotted Japanese firms only temporarily.

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1. Introduction

Trade policy has long been a popular tool in relations between states. Trade agreements can strengthen inter-state relations and a large literature in political science has worked on international trade's role in promoting peace and interstate cooperation (e.g. Gartzke et al., 2001; Barbieri, 2002; Li and Reuveny, 2011; Massoud and Magee, 2012). At the same time, international trade can be used as a policy means in the case of conflict through sanctions, embargoes, and boycotts. Trade boycotts between countries are a special form of these policy tools. They have been used throughout history to punish or coerce specific behavior among trading partners. Examples of international conflicts where boycotts were used include the repeated boycotts of Japan by China throughout the 1930s in response to the Japanese invasion (Lauterpacht, 1933), the boycott of Israel by the Arab League after formation of the Jewish state in 1948, the worldwide boycott movement in protest of South Africa's apartheid system in the late 1950s, and the consumer boycott against French products over nuclear testing in the 1990s. Most recently, the importance of international trade boycotts has been highlighted by Russia's state-led import ban of agricultural products from Europe in response to sanctions over Russian interference in neighboring Ukraine.

These events share the common characteristic that they are not motivated by economic rationale, such as inferior product quality, but rather by political events and thus allow us to learn about how shocks to international relations affect trade. In contrast to the more frequent boycotts against specific firms, such as the boycott against Shell in 1995, they are directed against entire countries. They seem to become an option when other means of coercion, such as war or the severing of diplomatic relationships appear to be infeasible. The latter boycotts of the 21st century seem to be a simple continuation of earlier practices, but several developments portend an increase in the importance of boycotts as policy tools and warrant further research.

In a world characterized by less violence and decreasing tolerance for militarized conflict between states (Pinker, 2011), trade policy is the prevailing tool to carry out international disputes.[1] In addition, international trade has surged over the past decades, making boycotts potentially more harmful to trading partners. This is especially true since the nature of trade has changed from a simple exchange in final goods to a system of international production sharing. The advent of the internet has also changed international relations and the importance of governments. Being able to communicate and coordinate their actions online, consumer boycotts enable the public to become a political agent in international relations. In the case of the Chinese consumer boycott against Japan in 2012 that I study in this paper, the internet may have played a crucial role in organizing the boycott, with the Chinese government having limited control over the reaction on the

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[1] Besides boycotts, this also includes trade sanctions. While not the focus of this paper, the prevalence of trade policy in solving international conflicts is reflected in the recent economic sanctions against Iran and North Korea.

streets. This raises questions on how governments and the populace interact when it comes to foreign relations (Weiss, 2013, 2014) and how different regime types favor the emergence of consumer boycotts.

An important question is whether these new types of boycotts are effective. Aside from a reduction in import demand, international conflicts might hurt trade by putting business partners at personal risk when traveling, through latent government intervention or even through the boycotted country's refusal to export in response to the aggression. Similarly, boycotts can fail in many dimensions. At first, if the boycotted country's exporters can easily redirect their sales to domestic or other foreign markets, the potential economic loss may be small. Secondly, even if disrupted exports hurt the exporting country significantly, boycotts are a costly tool, since the boycotting country is also giving up on its gains from trade. This is even more true in a world characterized by increasing international integration of production, often within firms (Zeile, 1997). Today, trade is not primarily in final goods anymore, but the share of processing trade is rising. If production of the boycotting country depends heavily on imports from the boycotted country, this will raise the costs of the boycott, and it might render it an incredible threat. Furthermore, consumer-led trade boycotts rely on collective action that can be difficult to organize. Friedman (1999) and John and Klein (2003) study consumer boycotts and their inherent small-agent problem, i.e. the success of the boycott depends on a mass of participants, but every individual's impact and motivation to join in is low. To explain that consumer boycotts do happen, they propose a variety of psychological motivations, such as guilt and self-esteem or simply an exaggerated sense of one's own effectiveness. These theoretical studies suggest that consumer-organized boycotts are short-lived.

The empirical literature on the impact of boycotts on international trade has found contradicting results, mainly from boycotts in the aftermath of the Iraq War of 2003. Michaels and Zhi (2010) estimate that US–French trade deteriorated by about 9% in 2003 when France's favorability rating in the US fell sharply over its refusal to intervene in Iraq. Pandya and Venkatesan (2013), using supermarket scanner data, find that brands that are perceived as being French lose market shares in weeks with high media attention of the boycott. They estimate the implied costs of this boycott to be similar to the costs of an average product recall. Similarly, Chavis and Leslie (2009) find a 26% reduction in weekly sales of French wine in the US, but Ashenfelter et al (2007) attribute this decline to boycott-unrelated influences. Clerides et al. (2013) find a significant but short-lived drop in sales of US soft drinks in the Middle East, but cannot find a similar effect on other goods. These studies are based on local sales and do not investigate the effect on trade. Davis and Meunier (2011) study quarterly trade relationships between the US and France as well as between China and Japan for the years 1990–2006, thus including the boycott of French goods. They do not find any significant link between negative events involving these countries and the level of goods exchanged, but find that trade, as well as foreign direct investment, continued to grow sharply in the period studied.

Besides the focus on explicitly announced boycotts, there is a new literature studying the relationship between other political conflicts and international economic relations. Fuchs and Klann (2013) study countries' trade with China if they officially receive the Dalai Lama. China perceives any formal relations with the Tibetan spiritual leader as an interference into internal political affairs and threatens countries that do so with a reduction of trade. The authors find a significant negative short-term effect of state visits on trade volumes and confirm that, even though the effect dies out after one year, countries are willing to use trade as a tool to enforce their political will. Fisman et al (2014) and Govella and Newland (2010) study the effects of Sino–Japanese conflicts in the 21st century on the stock market value of Japanese firms using an event study approach. They find that stocks of Japanese companies with a high share of sales to China lose value compared to companies with a low exposure to China.

The aim of this paper is to evaluate the effectiveness of international trade boycotts, to quantify their impact, and thus to learn about the consequences of international conflict on trade relationships. The contributions that distinguish it from previous studies are manifold: At first, with the Mohammad Comics boycott I study an international conflict that was unexpected and plausibly exogenous to unobserved trade-related confounding effects, thus providing superior identification to study the impact of political conflict on bilateral trade. The previous literature has largely focused on the US boycott of French products, an incidence that might be confounded with other trade-related effects of the looming Iraq War. Secondly, monthly product-level data allows me to study the boycotts' complex short-term impacts which cannot be uncovered using only yearly or quarterly data. The availability of only low-frequency data might be the major reason why the previous literature came to contradicting results regarding the effect of consumer boycotts. Furthermore, the high frequency of the data enables me to extend the analysis to recent incidents that would be impossible to study with yearly data, such as the Chinese boycott of Japanese goods in the aftermath of the Senkaku/Diaoyu Island conflict in 2012, and Turkey's boycott of Israel over the Gaza conflict in 2014, thus expanding the set of conflicts to learn from considerably. In addition, the fine product disaggregation of the data allows me to estimate different impacts for consumer, intermediate, and capital goods based on the full range of traded products, rather than having to a priori choose boycott-prone products like French wine or US soft drinks. This dimension of the data offers insight into the main drivers behind the boycotts. Finally, I apply the synthetic control group methodology to construct data-driven counterfactuals showing that the results are robust to omitted variable bias.

The results show strong heterogeneity in the response among the boycotting countries, with an average one-year reduction in imports of about 18.8%, 2.7%, and 1.7% of total trade in the Muslim boycott case, Senkaku conflict, and the US consumer boycott against France respectively. I do not find a negative effect for Turkish imports from Israel following the Gaza war in 2014, but instead observe that Israel reduces its imports from Turkey by 12.3%. Product-level analysis shows that the impact is concentrated in consumer goods and especially in highly branded goods such as Japanese cars. I find only minor effects for intermediates and capital goods, being consistent with the notion that international trade boycotts are mainly carried out by consumers and not by firms or governments. This is confirmed by results from the multi-country Muhammad Comic boycott, where countries with higher press freedom boycott more, indicating that consumers find it easier to organize and participate in boycotts in open regimes. While the estimated disruption in imports from the boycotted country can be large, the reduction in total exports of the boycotted country is low in all boycott cases (0.4% for Denmark, 0.5% for Japan, and 0.4% for the US). This suggests that even though an individual firm of the boycotted country might be hit hard, the overall effect on the export sector is small. An event-study analysis based on time series variation does not hint towards substitution of imports or exports towards non-boycotting countries.

The paper is organized as follows: Section 2 provides background information on the events studied, while Section 3 outlines the empirical implementation. Section 4 presents the findings on both aggregate and product-level data. Section 5 concludes.

2. Background

In this section, I provide background information on the international conflicts used in the study and describe the events leading up to the boycotts as well as their political consequences.

2.1. Muhammad Cartoon Crisis

On September 30, 2005 the Danish newspaper Jyllands-Posten published a series of cartoons depicting Islamic prophet Muhammad in an unfavorable manner, the most striking one showing him with a bomb

in his turban.[2] Not only is the depiction of the prophet forbidden in several branches of Islam, but Muslims felt that the comics equated them to terrorists, thus the comics had a religious as well as political dimension. Even though Danish Muslims protested the publication from the very beginning, it was not until early 2006 that the controversy became international after the comics had been reprinted in Arabic newspapers. Violent protests sparked in many Middle Eastern countries, leading the ambassadors of several Muslim countries to unsuccessfully demand an official apology by the Danish government and prosecution of the cartoon artists.

The months of January and February 2006 saw further escalation of the conflict with Western embassies being attacked in Damascus, Beirut, and Tehran, leaving several dozen people dead. With the Danish government refusing an official apology, religious leaders in Saudi Arabia called for a boycott of Danish goods on January 26, 2006, publishing a boycott list of Danish firms.[3] Soon other Muslim countries joined the boycott. The French supermarket chain Carrefour preemptively removed Danish goods from its shelves in the Middle East and several Danish food producers, such as Arla Foods, reported large losses.[4] At the same time, a counter-boycott campaign called "Buy Danish" was called for, but it remains unclear whether this campaign gained enough media attention to have any large scale effects.[5]

The scandal about the Muhammad cartoons eventually lost public attention and the protests calmed down, though several incidents in later years were linked to the cartoons, e.g. the 2008 and 2010 attempts to assassinate the creator of the most controversial of the cartoons which could be prevented by police.

2.2. Senkaku/Diaoyu Islands conflict

The Senkaku (in Japanese) or Diaoyu (in Chinese) islands are a small group of islets unsuited for settlements in the East China Sea approximately 170 km North-East of Taiwan. In the aftermath of the First Sino–Japanese War (1884–85) and the subsequent invasion of Taiwan, Japan began to survey the islands and claimed them as its territory. After the Treaty of San Francisco formally established peace after World War II, Japan ceded all its claims to Taiwan and the nearby Okinawa islands came under US control. When the Okinawa islands were returned to Japan in 1972, it tacitly took control of the Senkaku islands as well and retains a military presence on the islands until today.

In 1968, possible oil reserves were found in the area surrounding the Senkaku/Diaoyu Islands leading to claims of both Mainland China and the Republic of China (Taiwan) to the islets that were rejected by Japan, leaving the territorial conflict remained unsolved. It was not until the 2000s when several incidents brought the Senkaku/Diaoyu conflict back to public attention. Between 2006 and 2011 several activist groups from Mainland China, Taiwan and Hong Kong arrived at the islands to proclaim Chinese sovereignty and were expelled by the Japanese navy immediately.

While these events worsened Japanese–Chinese relationships, the conflict only escalated after Japan announced to purchase the islands from their private owner in August 2012 and de facto established sovereignty over the archipelago. This led to anti-Japanese protests in several Chinese cities that later turned violent. Japanese businesses in China were attacked and protesters called for a boycott of Japanese goods. Japanese–Chinese relations deteriorated drastically when further naval standoffs near the disputed islands occurred, leading to worldwide fears over a military conflict. While the dispute has calmed down and lost media attention, the major issue is still unresolved and remains a major problem in Japanese–Chinese relations.

2.3. US boycott of France

The months preceding the invasion of Iraq by US-led forces in March 2003 caused widespread conflicts in international relations. While some European countries supported action against Saddam Hussein's regime, others, notably France and Germany, vocally opposed any intervention that was not backed by the UN. France's favorability ratings in the US began to plummet starting in February 2003 and conservative media outlets called for boycotts of French goods to punish the perceived betrayal of a supposedly close ally. Relations between the two states deteriorated so much that even Congress's food menu was officially relabeled French fries as "freedom fries".[6]

2.4. Turkey's boycott of Israel

On July 8th 2014, the long-lasting conflict between Israel and the Palestinians escalated again when Israeli military launched airstrikes on Gaza after heavy shelling of Israeli territory by Hamas. Two weeks later, the Israeli Defense Force led a ground invasion into the Gaza strip to destroy smuggling tunnels which resulted in the death of more than 2000 Palestinians, around 1500 of them being civilians.[7] Public outcry over the humanitarian toll of the conflict sparked anti-Israel protests in Turkey with Turkish prime minister equating Israel's actions to genocide. The Turkish trade union TESK launched a boycott call against Israel in late July. At the same time, polls in Israel showed that Israelis were boycotting Turkey and especially its holiday destinations.

3. Methodology

To evaluate the impact of the boycotts on trade, I estimate difference-in-differences models of logged exports from the boycotted country $Y_{j,t}$ to all its trading partners j at time t at monthly frequency. I determine treatment status by participation in the boycott and thus use non-boycotting countries as the control group. I include the typical gravity regressors GDP and distance provided by CEPII and control for a time trend and monthly fixed effects. The regression equation is given by

$$Y_{j,t} = \alpha + \beta_1 \ Treat_j + \beta_2 \ Post_t + \beta_3 \ Treat_j \times Post_t + \beta_4 \log GDP_t \\ + \beta_5 \log dist_j + \beta_6 t + \varepsilon_{j,t}. \tag{1}$$

The difference-in-differences approach might suffer from omitted variable bias if important determinants of trade are not controlled for. Despite the empirical success of parsimonious gravity equations in the cross-section, this relationship describes long-time averages and in the short-term, there may be many more unobserved confounding factors, such as a country's industry composition. To avoid this problem and to consistently construct a suitable control pool, I follow the synthetic control group method first used in Abadie and Gardeazabal (2003) and later further developed in Abadie et al. (2010, 2014). The synthetic control group method follows a pragmatic data-driven approach to choose the right control group by creating a weighted average of all the available control units. The weights are chosen such that the synthetic control group resembles the actual treatment unit in both the outcome variable as well as in any known explanatory characteristics in the pre-treatment period. The idea behind the method is to indirectly control for any unobserved factor by matching on previous outcomes. An estimate for the treatment effect can then be calculated by the difference

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[2] For a detailed narrative of the events, see Jensen (2008).

[3] Examples of these lists can be found on http://shariahway.com/boycott/index.htm.

[4] http://www.nytimes.com/2006/01/31/international/middleeast/31danish.html?_r=0.

[5] http://www.foxnews.com/story/2006/02/16/muslim-boycotts-hurt-danish-firms/.

[6] While US consumers were boycotting French products, the US itself became the victim of a boycott movement. The eventual invasion of Iraq triggered a boycott movement against US-American products in the Middle East. Clerides et al (2013) report the existence of boycott lists of American brands and find a negative effect of US softdrink sales in the Middle East, but are unable to detect a similar effect for detergents.

[7] Source: OCHAOPT (http://reliefweb.int/sites/reliefweb.int/files/resources/annual humanitarian overview 2014 english final.pdf).

between treatment unit and the synthetic control unit in the post-treatment period (for details, see the appendix).

One problem of the synthetic control group methodology is the inability to calculate standard errors. In practice, the fit between treatment group and the synthetic control group in the pre-boycott period will not be perfect, but subject to idiosyncratic shocks captured in the error term $\epsilon_{j,t}$. This will bring randomness into the estimate of the treatment effect β_t. The exact distribution of the estimate depends on the unobserved parameter vector λ_t and therefore cannot be computed. A pragmatic ad-hoc approach to evaluate the significance of the parameter estimates is to compare them to the prediction error in the pre-boycott period. The intuition is that if the synthetic control group fits the actual treatment unit poorly before the boycott happened, this would undermine the confidence in the estimate of the treatment effect. If the fit between the actual treatment country and its synthetic control, however, is close in the pre-boycott period, we can be more confident in assuming that any divergence after the treatment is actually caused by the boycott and not due to unrelated shocks.

I formalize this idea by testing for a structural break in the time series of the error term $\epsilon_{j,t}$ and test the model $\epsilon_{j,t} = \sum_{k=1}^{6} \rho_k \epsilon_{j,t-k} + \beta_{j,t} + u_{j,t}$ against the simple alternative $\epsilon_{j,t} = \sum_{k=1}^{6} \rho_k \epsilon_{j,t-k} + u_{j,t}$. The six-month autoregressive specification and inclusion of clustered standard errors allows for the possibility of a correlation over time and between countries. In specific, I report p-values of an F-test with the null hypothesis $H_0 : \sum_{t=T_0+1}^{T_0+d} \beta_t = 0$ where d denotes the horizon of the effect.

To complement the analysis, I also perform placebo tests that traditionally have been used in the context of synthetic control groups. There are two dimensions where a placebo test can detect wrongful inference: Within a single time series, a random assignment of a treatment time should not break the close fit between actual and synthetic control group and should not produce large estimates of the treatment effect. If both series deviate even though there is no boycott, then this should warn us that the synthetic control group is merely picking up unrelated idiosyncratic effects. Furthermore, we can estimate the same treatment effect for the control countries. If these countries are indeed unaffected by the boycott, the synthetic control group method should not find large treatment effects. If however the control countries seem to be negatively affected by the boycott, this would hint to mis-specification in the model and would greatly undermine our confidence in the method.

4. Results

This section presents the data sources, descriptive statistics, and the estimation results of both the difference-in-differences and synthetic control group methods for each boycott case.

4.1. Mohammad Cartoon Crisis

4.1.1. Data and descriptive statistics

I use data from the online portal of Statistics Denmark. This dataset covers Danish export values in local Danish krona (DKK) to virtually all trade partners at monthly frequency at the two-digit and five-digit SITC classification from the late 1980s onward. Unsurprisingly, being a small country, imports from Denmark make up only a small share of the Muslim world's total trade. On average, only 0.29% of all imported goods of the 34 countries with at least 75% Muslim population[8] stem from Denmark. Similarly, Danish exports to the Muslim world as a share of its total exports are relatively small accounting for 2.66% of Danish exports to all trading partners in 2004.

Even the biggest Muslim trading partner, Saudi Arabia, accounted for less than half a percent of Danish exports in 2004 (see Table A3 in the appendix).

Examining export values from Denmark to the boycotting countries shows that monthly trade data is characterized by high volatility, seasonal patterns, and possibly changing time trends. It is not uncommon that Danish exports to these countries increase by a multitude over one month or that trade completely collapses even in the pre-boycott period. The strong month-to-month swings are more prominent for the smaller export partners, so I exclude countries with zero values from the analysis.[9] A lowess plot of imports from Denmark by Muslim majority and minority countries in Fig. 1 reveals a pronounced dip for Muslim countries at the end of 2005 which is not present for non-Muslim countries. Table 1 summarizes the descriptive statistics of the time series for the three treatment countries with the largest imports from Denmark: Saudi Arabia, Turkey, and the United Arab Emirates.

4.1.2. Difference-in-differences results

Since the comics were published on the last day of September 2005 and a same-day effect is unlikely, I define October 2005 to be the first treatment period in the sample. This is considerably earlier than the official announcement of the consumer boycott in January 2006, but allows for undeclared boycotts as an immediate reaction to the insult. Instead of a binary treatment status, I use the share of Muslim population as a continuous treatment. Data on the Muslim population for each country is provided by the Pew Research Center. The results in Table 2 indicate that the treatment effect is negative and robust to including fixed effects. The coefficient on Post × Muslim suggests that a ten percent higher share of Muslim population reduces imports from Denmark by 3.7%. The elasticities with respect to GDP and distance have the expected positive and negative signs respectively. The negative coefficient on the share of Muslim population indicates that the treatment countries in general import less from Denmark than similar non-Muslim countries. Controlling for potentially endogenous exchange rate fluctuations, the treatment effect is still significant, but reduced to 2.2%.

Heterogeneity among the boycotting countries allows me to investigate the importance of different regime types for the effectiveness of the boycott. Consumer-organized boycotts are only possible if the populace is able to interact and draw masses to its cause. I estimate a triple difference model by interacting the treatment effect with a variable measuring the freedom of press as reported by Reporters Sans Frontières in the Quality of Government database. The coefficient on the triple interaction term in column (4) suggests that Muslim countries with a one-unit higher press freedom score reduce their imports from Denmark by an additional 1.18%. This suggests that more open countries allow for more organized action of their people and this strengthens the theory of the conflict being a consumer boycott.[10] In column (5), I interact the treatment effect with elasticities of substitution at the five-digit SITC level as measured by Broda and Weinstein (2006). This addition shows no significant effect on the boycott. If at all, highly substitutable goods are boycotted less, but the coefficient is imprecisely estimated.

To analyze the potentially heterogeneous effects on different product groups, I break up the analysis into three main product types: Consumer goods, intermediate goods, and capital goods.[11] Unlike consumer goods which merely reduce consumption, a boycott of intermediate and capital goods may have direct effects on the economy of the boycotting country if it depends heavily on foreign inputs. This drives up the cost of the boycott and we expect a weaker effect for these goods if countries choose their boycott strategy rationally. In addition, knowing which

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[8] For the exact list of these countries, see Table A2 in the appendix.

[9] For example, a complete disruption of trade with Kyrgyzstan would reduce Danish exports by only 0.004%.

[10] In regressions not reported here, I show that this result is robust against using alternative governance indicators such as the Polity IV score that ranks countries according to constitutional and practical criteria.

[11] Where available, I use the Broad Economic Categories (BEC) classification developed by the UN Statistics Department to categorize SITC5 codes. Trade codes that are not available in the BEC were coded by my own judgment in close concordance with the logic of the BEC classification. The complete conversion table can be found in the online appendix.
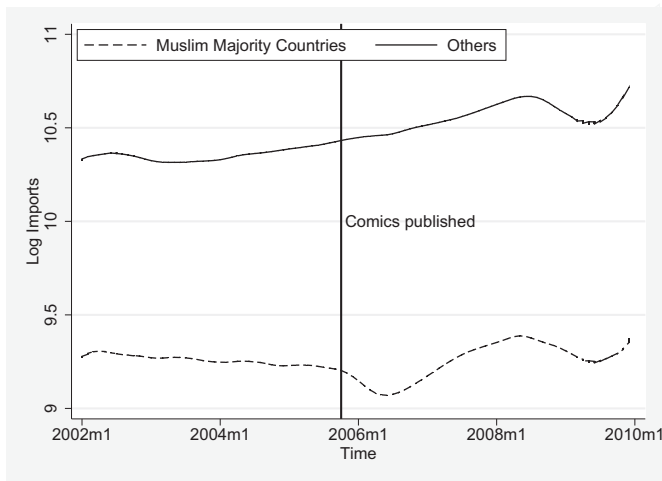
Fig. 1. Lowess plot (imports from Denmark).

goods are boycotted allows us to gain some insight about who is the main driver behind the boycott. If the boycott is mainly consumer-driven, we should expect a higher trade disruption in consumer goods as compared to non-consumer goods. A large effect for non-consumer goods would suggest that local producers engage in the boycott as well or that governments restrict imports indiscriminately.

The results show the heterogeneity of the treatment effect for the different product types. While there is no statistically significant treatment effect for intermediate goods, we observe that a ten percent higher Muslim population is associated with a 6.5% and 2.8% drop in consumer and capital goods imports from Denmark respectively. This confirms that the boycott was most effective for products that individual consumer purchase and suggests that while capital goods are also affected, the nature of the boycott is primarily a consumer boycott. The difference between capital and intermediate goods might be explained by the fact that capital goods tend to be branded and are thus easier to recognize as of Danish origin than intermediate goods. I also use yearly data from the International Trade Centre on trade in services and analyze its response to the boycott. The negative effect is very strong at 4.8% suggesting that Muslim countries readily reduced travel, communication, financial and other services from Denmark (Table 3). This is not surprising as compared to trade in goods, trade in services requires more personal interaction between people of the conflict parties.

To answer the question whether the boycott announcement caused a two-way trade disruption, that is whether Danish consumers retaliated against the Muslim states, I apply the above methodology to Danish import data. The results in Table A6 indicate that the Danish did not boycott. While the estimates indicate a reduction in imports from Muslim countries after the comics were published, the standard errors

are too high to conclude that there was a significant effect. Anecdotal evidence from newspaper articles also suggests that the boycott was a one-way trade disruption.

Table 1
Descriptive statistics (log Danish exports).

Country	Saudi Arabia	Turkey	UAE	Aggregate
Mean (in DKK)	176,710	205,965	129,765	1,036,268
Standard deviation	31,738	87,048	37,468	188,399
Std dev as mean	18.0%	42.3%	28.9%	18.2%
Minimum	100,143	77,810	82,677	710,530
Maximum	272,301	462,941	422,072	1,596,562
Min % change	−33.0%	−60.2%	−54.4%	−29.1%
Max % change	67.8%	92.6%	194.7%	46.9%
Seasonality p-value	0.37	0.32	0.68	N/A

Statistics over the pre-boycott period October 2000 to September 2005.
Seasonality p-value is the p-value of a F-test testing for joint significance of monthly indicator variables in a linear time series regression.

Table 2
Muhammad Comic Crisis: results.

Dependent variable: log Danish exports					
	(1)	(2)	(3)	(4)	(5)
Log GDP	0.950***	0.455***	0.511***	0.491***	0.258*
	(0.0366)	(0.115)	(0.111)	(0.112)	(0.147)
Log distance	−0.880***				
	(0.0787)				
Post	0.043	0.065*	0.049	−0.166	0.691***
	(0.0389)	(0.0376)	(0.037)	(0.129)	(0.072)
Muslim	−0.106				
	(0.179)				
Post × Muslim	−0.370***	−0.302***	−0.217**	0.204	−0.457***
	(0.098)	(0.091)	(0.090)	(0.215)	(0.175)
log exchange rate			−0.015		
			(0.085)		
Press freedom				−0.0024	
				(0.0047)	
Post × Press freedom				0.0039**	
				(0.0019)	
Press freedom × Muslim				−0.0006	
				(0.0116)	
Post × Press freedom × Muslim				−0.0118**	
				(0.0049)	
Elasticity (in 100)					−0.00801
					(0.00509)
Post × Elasticity					0.00298
					(0.00487)
Elasticity × Muslim					0.00595
					(0.0138)
Post × Elasticity × Muslim					0.0159
					(0.0155)
Country fixed effects	No	Yes	Yes	Yes	Yes
Trend and month FE	Yes	Yes	Yes	Yes	Yes
N	13,518	13,518	10,267	12,954	5,037,984
Adj. R²	0.857	0.944	0.959	0.943	0.113

Standard errors are in parentheses (clustered at country level).
* p < 0.10
** p < 0.05
*** p < 0.01

Table 3
Muhammad Comic Crisis: results by product type.

Dependent variable: log Danish exports				
	Consumer	Intermediate	Capital	Services
Log GDP	0.043	0.437***	0.637***	0.528***
	(−0.183)	(−0.116)	(−0.136)	(0.151)
Post	0.039	0.041	−0.055	0.464
	(−0.067)	(−0.053)	(−0.064)	(0.06)
Post × Muslim	−0.558***	−0.073	−0.221**	−0.481***
	(−0.173)	(−0.094)	(−0.11)	(0.166)
Constant	11.57***	9.393***	7.767***	−22.61
	(−1.083)	(−0.736)	(−0.859)	(24.73)
Country fixed effects	Yes	Yes	Yes	Yes
Trend and month FE	Yes	Yes	Yes	Yes
Frequency	Monthly	Monthly	Monthly	Yearly
N	16,149	16,942	15,243	1527
Adj. R²	0.862	0.909	0.841	0.951

Standard errors are in parentheses (clustered at country level).
* p < 0.10.
** p < 0.05.
*** p < 0.01.

4.1.3. Synthetic control group results

The synthetic control group method requires a binary treatment status, so I assign all countries that have a share of Muslim population of the total population of more than 75% into the treatment group. Conversely, I assign countries for which this share is less than 10% into the control group and drop all other countries to avoid contamination of the control group.[12] This leaves me with 34 countries in the treatment group and 100 countries in the control group (see Table A2 in the appendix).

Since the number of potential control units is large, I restrict the pool of controls to countries that are close in both distance and GDP in the month prior to the boycott. In specific, I allow the GDP to differ by 100% in both directions and distance to deviate by 4000 km. This avoids that the relatively small and close economies of the Middle East are replicated by large and distant countries like Japan and the US. While these restrictions seem arbitrary, they shrink the pool of control countries to an average of no more than ten units, a reasonable number to avoid overfitting 60 pre-boycott time periods. Experimenting with different specifications, the results tend to be fairly robust to these restrictions.

To calculate the value of the foregone trade, I simply add up the treatment effects of all treatment countries for each month and calculate the percentage loss as a share of total trade levels. Table 4 shows the estimated aggregate percentage reduction for a period of three, twelve, and 24 months. The results indicate that there was a statistically significant fall in imports from Denmark in the treatment countries which is robust to changes in the specification of the control group and sampling frequency. My preferred estimate in column (1) with all three predictors for the 19 treatment countries that take up at least 0.02% of all Danish exports shows that the short-term reduction in imports reaches 12.4% after three months. The boycott then intensifies to an 18.8% trade loss within one year; after which the impact is reduced to 14.7% after 24 months.

Including the ten smaller Muslim countries introduces more noise to the analysis without changing the results much. Releasing the restrictions on the control pool significantly increases the average number of control countries from 9.5 to 14.7 and consequently the average pre-period correlation, leading to slightly lower estimates of the treatment effect. Using quarterly instead of monthly data, the reduction in noise leads to a similarly high pre-treatment fit. While the short-term estimates are slightly higher, the treatment effect after 24 months remains basically the same. To further strengthen the robustness of the results, I shut down one predictive variable (GDP, distance, previous trade levels) at a time in columns (5)–(7). While the short-term results differ slightly, the long-term effects after 24 months are close to the baseline result of −14.7%.

The results by country depicted in Table A4 in the appendix show strong heterogeneity between the different Muslim countries. Some larger export partners like Algeria, Egypt, Kuwait, and Saudi Arabia see a strong and persistent negative effect, while some countries even show a positive reaction. Most notably, the second and third largest Danish trading partners Turkey and UAE show no reaction to the boycott at any time horizon.

Adding up the estimates for all countries, I calculate the total disruption of trade due to the boycott to be about 0.51 billion DKK after three months, 2.86 billion DKK after twelve months, and 4.28 billion DKK after two years. The US-Dollar equivalents after taking into account fluctuations of the exchange rate are 198 million USD after three months, 444 million USD after one year, and 758 million USD after two years.

While the percentage loss for all the Muslim countries combined is sizable, this loss is marginal when compared to the total exports of

Denmark. Over the period from October 2005 to September 2007, Danish exports to all its trading partners summed to 1.08 trillion DKK (185 billion USD). The implied overall disruption of trade caused by the boycott is then only 0.4% of all Danish exports during this period. While the boycott might have hit individual Danish companies hard, the effect on the total Danish export sector is negligible.

4.1.3.1. Product-level results. To assess the treatment effect by product type, I first add up the Danish exports to all the treatment countries and then separate them by product type. Fig. 2 shows the realized and counterfactual log Danish exports of each classification. Consistent with the boycott being consumer-driven, I see the largest relative decline in consumer goods with long-term reductions in this category of 27.5% and 24.8% after one and two years respectively (Table 5). This suggests that the publication of the comics itself did not cause a major consumer reaction, but only after the official boycott announcement did imports from Denmark decline.

For non-consumer goods, the reaction is less strong and in many cases not statistically significant. Danish capital goods exports to the Muslim world seem to decline marginally in the short and medium run, but the large prediction errors render this result statistically insignificant. Over two years, this decline is reduced to less than 2%. For intermediate goods, we do see a significant reduction in imports from Denmark of about 9.0% and 10.9% after 3 and 12 months respectively. The reduction for these goods is reduced to 1.7% after two years. This is inconsistent with the idea of a pure consumer boycott and could be explained by nationalistic sentiment of business owners or official trade restrictions such as complicating the processing of imports at custom offices.

4.1.3.2. Placebo tests. To check whether the results depend strongly on the parametrization of the synthetic control group, I assign placebo treatment times and estimate the trade disruption for these false boycott instances. I restrict the robustness checks to the three largest export partners Saudi Arabia, Turkey, and the United Arab Emirates. I estimate the cumulative treatment effect over six months for the 30 months preceding the publication of the comics. 25 of these placebo treatment times are not related to the boycott, but the five 6-month estimates prior to the actual treatment month will contain at least one of the actual treatment months respectively. Fig. A1 in the appendix shows the distribution of the estimated treatment effects. Some of the placebo treatments do create negative treatment effects, but in general are of smaller magnitude and not as persistent as the estimated trade disruption of the actual treatment. For Saudi Arabia and UAE, all six-month estimates including the actual treatment month are negative and large. For Turkey, the estimate of the actual treatment is still negative, but at a much smaller scale especially compared to previous large negative and positive effects. These random fluctuations are in line with Turkey's estimated, non-significant effect of about 0%.

4.2. Senkaku Island conflict

4.2.1. Data and descriptive statistics

The data for the Senkaku/Diaoyu conflict comes from the Monthly Comtrade dataset that, in addition to the standard Comtrade data, reports trade flows at monthly frequency for all Harmonized System (HS) product codes. The very fine disaggregation of the data is however offset by very limited availability of trade from January 2010 to January 2014 only. Data prior to 2010 is at the moment only available at annual frequency.

Unlike the Danish–Muslim boycott where all the boycotting countries take up only a small share of total exports, the People's Republic of China is the largest export partner for Japan in the pre-boycott period from January 2000 to August 2012. China alone accounts for 19.23% of all Japanese exports (Table 6). The Special Administrative Region of

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[12] The distribution of Muslim percentage between countries is bimodal and most countries exhibit either a very high or very low Muslim population. Only few countries fall between the thresholds and the results are robust to changes in the cutoffs.

Table 4
Estimated treatment effect on log Danish exports (synthetic control).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
3 months	**−12.4%**	**−11.8%**	**−9.1%**	**−16.6%**	**−15.1%**	**−13.7%**	**−8.7%**
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
Months	**−18.8%**	**−17.6%**	**−16.0%**	**−19.3%**	**−20.9%**	**−19.3%**	**−18.5%**
	(.0000)	(.000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
Months	**−14.7%**	**−13.7%**	**−11.1%**	**−14.6%**	**−16.1%**	**−15.2%**	**−13.2%**
	(.0019)	(.0000)	(.0000)	(.0001)	(.0000)	(.0000)	(.0000)
GDP	100%	100%	None	100%	100%	100%	100%
Distance	4000 km	4000 km	None	4000 km	4000 km	4000 km	4000 km
Frequency	Monthly	Monthly	Monthly	Quarterly	Monthly	Monthly	Monthly
Excluded	None	None	None	None	GDP	Distance	Lags
Controls	9.5	8.1	14.7	8.0	9.4	9.4	23.2
Correlation	40.2%	32.7%	62.7%	62.8%	38.3%	36.7%	5.9%
CV	40.8%	67.8%	36.2%	26.6%	41.2%	41.5%	50.5%
Countries	19	29	19	19	19	19	19

Excluded: Variable excluded from the matching procedure.
Controls: Average number of control countries per treatment country.
Correlation: Average value of correlation coefficient in the pre-treatment period between treatment and synthetic control.
CV: Average value of coefficient of variation.
Countries: Number of treatment countries.
p-Values are in parentheses (estimates with p-values below 0.01 are in bold).

Hong Kong and Taiwan[13] report separate trade statistics. Including the trade with these entities, the total percentage of exports to the Chinese-speaking world amounts to 30.8%

For the Japanese–Chinese trade data, the month-to-month fluctuations are lower but can still reach percentage changes of more than 30% in either direction. The time series is marked by a stark drop in March 2011, the effect of the devastating Tohoku earthquake and tsunami that resulted in more than 50,000 deaths. Seasonality might be an issue especially in the winter months in which trade appears to slow down and the F-test testing for the joint significance of the monthly indicator variables suggests seasonal patterns.

4.2.2. Difference-in-differences results

For the Senkaku Island Crisis case, I identify three political entities that are potentially affected by the boycott announcement: The People's Republic of China, its Special Administrative Region (SAR) Hong Kong and the Republic of China (Taiwan). All these entities claim sovereignty of the Diaoyu Islands and sent activists to them. I estimate the model in Eq. (1) where $Chinese_j$ is an indicator variable that takes the value of one if the country is either China, Taiwan, or Hong Kong.

The results in Table 7 show that the treatment effect is negative in all specifications and is estimated to be −12.3% when including country fixed effects. As before, the coefficients on GDP and distance have the expected signs and positive results for *Chinese* indicate that Japan exports more to the treatment countries than to similar non-Chinese countries to begin with. To analyze heterogeneity in the response to the boycott, I re-estimate the model above for each Chinese country separately. The estimates for the individual countries indicate that the results are mainly driven by the PR China with a strong negative estimate of 29% whereas the Taiwan and Hong Kong show smaller estimates of −6.4% and −5.7%. In the opposite direction, I do not find any effect for Japan boycotting imports from China as seen in Table A6.

4.2.3. Synthetic control group results

The nature of Japanese trade with Mainland China creates challenges with the synthetic control group method. As discussed above, Mainland China is not only Japan's largest export partner over the pre-treatment period but it is also geographically close. It is thus at the end of the distribution of both outcome as well as explaining variables and it is impossible to replicate its imports from Japan with a weighted average. The

Table 5
Treatment effect by product type (synthetic control).

Period	Consumer	Intermediate	Capital
3 months	1.5%	−9.0%***	−13.4%
	(.9894)	(.0001)	(.6646)
6 months	−27.5%***	−10.9%***	−12.0%
	(.0006)	(.0002)	(.8539)
12 months	−24.8%**	−1.7%	−1.7%
	(.0337)	(.1502)	(.8518)

p-Values are in parentheses.

other treatment units, Taiwan and Hong Kong, have smaller shares of 6.2% and 5.2% respectively, but there are still only two control countries that import more from Japan (USA and Korea). I therefore relax the conditions of the weights to be in the unit interval and instead allow for arbitrary weights.

To avoid overfitting, I restrict the number of control units to countries that have a similar GDP.[14] In general, a small number of countries are able to replicate the Chinese trade patterns rather well according to the correlation coefficients in Table 8.

Fig. 3 shows the realized and counterfactual exports from Japan to China on a log scale. The strong decline in realized exports for about six months after the boycott is easily visible and trade levels even fell below those that followed the devastating earthquake in 2011. Yet Chinese imports from Japan were on a downward trend and only a portion of the decline can be attributed to the boycott, as the counterfactual trade figures implied by the synthetic control group decline as well. The effect seems to die out after half a year and then trade values catch up with the control unit. Table 8 shows this short term effect with a highly significant three and six month effect that is not statistically significant at 12 months anymore. The total reduction in Japanese exports within one year of the boycott amounts to 2.69% and is equivalent to 3.48 billion USD. This estimated trade disruption amounts to a share of 0.5% of total Japanese exports over the same time period. As in the case of the Muhammad Comic boycott, this is a rather small percentage of the total Japanese export economy.

For the other Chinese entities there is no significant negative effect, but Hong Kong and Taiwan experience a positive reaction to the boycott. This hints towards substitution of exports from Mainland China towards these entities, significantly reducing the overall negative impact

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[13] For political reasons, monthly trade data for Taiwan is not officially available in the Comtrade Monthly dataset, but can be inferred from the country code 490, "Other Asia, nes".

[14] In specific, the replicating country's GDP should have at least 20% of GDP of the treatment country and it should not exceed it by the factor 1.8. While arguably arbitrary, this creates control pools of around 10 control countries.
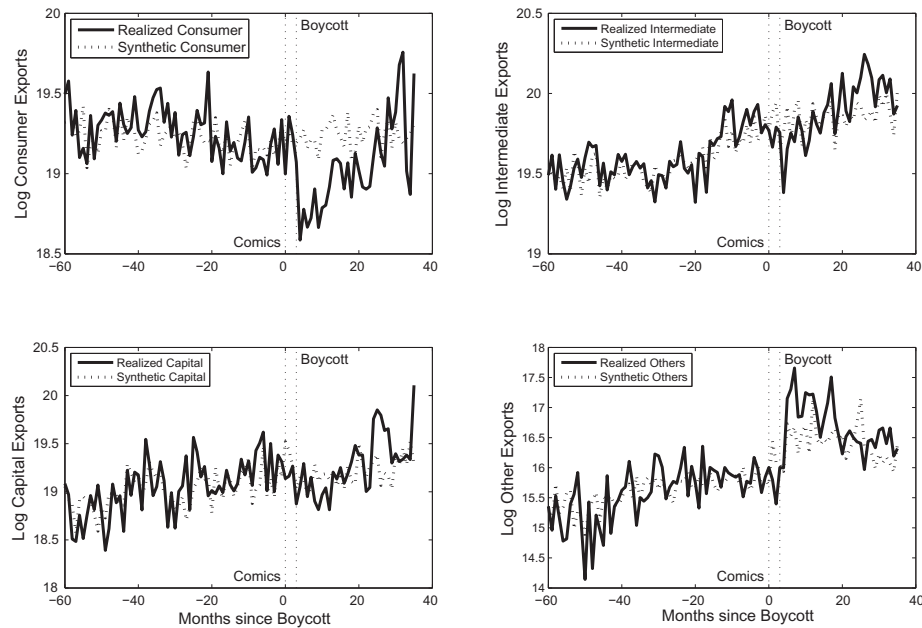
Fig. 2. Realized and counterfactual log Danish exports by class.

of the Mainland boycott. One can conclude that the boycott was effective only in Mainland China and that the movement was unable to encourage Chinese people in Taiwan and Hong Kong to participate in the boycott.

The short pre-boycott period does not allow for a sensible placebo assignment of the treatment time. I instead estimate the treatment effect for the control countries that should not be affected by the boycott. I calculate the percentage losses of Japanese imports to the countries of France, Germany, Russia, India, Thailand, the UK, and the US which are all major trading partners of Japan. The results in Table 8 show that for the majority of the controls, the boycott did not have a significant effect on imports from Japan. Russia is the exception as it shows a significant negative impact over a 6-month period. This effect however disappears at the one-year window. The US and Thailand show a positive reaction to the Chinese boycott, suggesting that the Japanese exporters substituted their goods towards these countries.

4.2.4. Identifying consumer industries

Beyond dividing trade into consumer, intermediate, and capital goods the data allows me to look at a more detailed product level to trace out the effect of the boycott for six-digit HS categories.[15] I make use of publications of the Chinese boycott movement itself to identify consumer goods that are most prone to the boycott, i.e. goods that can be clearly identified by Chinese consumers as being Japanese. These publications are two flyers that were circulated on the internet at the height of the conflict and contain pictures of Japanese brands that Chinese consumers should avoid (see Fig. 4 in the appendix). I report the brand names and their industry in Table A9 in the appendix. Most of these firms are concentrated in a few industries, namely automotive, consumer electronics, foods, clothing, and cosmetics, while the remaining companies engage in industries as diverse as toys, cigarettes, and airline services.

I searched through the companies' internet representations and identify the brands' major export products. I then classify these products into the corresponding HS codes using the official description and

the commercial website http://hs.e-to-china.com/ that allows searching for keywords and outputs the relevant HS code. These signature products can be subsumed into seven product codes which show a significant amount of trade between Japan and China. These codes contain highly branded goods such as passenger cars, make-up and beauty articles, foods, and a variety of consumer electronics such as cameras and video recording devices.

I estimate the impact of the boycott on these consumer goods and Table 9 summarizes the results. The category that sees the most drastic decline in trade is unsurprisingly 8703 which includes passenger cars. Fig. 4 shows the realized and counterfactual log trade levels for Mainland China. Clearly visible is the massive drop in car imports and although they catch up to the control group after about nine months, Japanese car exports to China drop by a 32.3% within a single year. While the effect of the boycott is very clear for vehicles, evidence for other product codes is not obvious. The estimated percentage disruption in trade in highly-branded goods like beverages, beauty products, and cameras is large, but in absolute values the estimates of a mere 12, 14, and 4 million USD for these categories are dwarfed by the huge trade disruption of almost two billion USD for passenger cars. The other product categories seem not to be negatively affected by the boycott.

Notable is the lack of a negative reaction in the area of consumer electronics, even though 34 Japanese companies in this sector were

Table 6
Descriptive statistics (Japanese exports).

Country	PR China	Taiwan	Hong Kong
Mean (in USD)	12,800,000	4,175,000	3,502,000
Standard Deviation	1,378,000	363,400	362,700
Std dev as mean	10.8%	8.7%	10.4%
Minimum	9,626,000	3,090,000	2,674,000
Maximum	15,420,000	4,770,000	4,149,000
Min % change	−30.4%	−25.2%	−29.4%
Max % change	32.6%	22.9%	39.2%
Seasonality p-value	0.01	0.00	0.00
Share of Japanese exports	19.2	6.2	5.2
Japanese share of total imports	11.8	19.6	8.8

Statistics over the pre-boycott period January 2010 to August 2012.
Seasonality *p*-value is the *p*-value of a F-test testing for joint significance of monthly indicator variables in a linear time series regression.

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[15] Product-class series for China will suffer from the same problem as the total trade values as they will be the largest and cannot be reproduced without non-negative weights. This problem is less severe for product-level HS6 codes, as China is not be the biggest export market for all of them.

Table 7
Senkaku Crisis: results.

Dependent variable: log imports from Japan					
Countries	All	All	PR China	Hong Kong	Taiwan
Log GDP	1.068*** (0.0516)	1.419*** (0.255)	1.427*** (0.256)	1.441*** (0.257)	1.443*** (0.256)
Log distance	−1.091*** (0.217)				
Post	−0.131*** (0.0316)	−0.117*** (0.0313)	−0.120*** (0.0317)	−0.120*** (0.0316)	−0.120*** (0.0315)
Chinese	1.016 (0.719)				
Post × Chinese	−0.123** (0.0596)	−0.138** (0.0689)	−0.292*** (0.0347)	−0.0647** (0.0284)	−0.0573 (0.0348)
Constant	1.621 (2.461)	−15.95*** (5.263)	−16.22*** (5.277)	−16.54*** (5.289)	−16.56*** (5.285)
Country fixed effects	No	Yes	Yes	Yes	Yes
Trend and month FE	Yes	Yes	Yes	Yes	Yes
N	5760	5760	5664	5664	5664
Adj. R^2		0.965	0.963	0.962	0.962

Standard errors are in parentheses (clustered at country level).

  * $p < 0.10$.
 ** $p < 0.05$.
*** $p < 0.01$.

mentioned on the flyers. One explanation could be the outsourcing of Japanese firms' production to China. As it is well known, China is the "world's workshop" and produces consumer electronics under foreign brands (Feenstra and Wei, 2009). If this is true, even though sales of Japanese-branded products in China might decline, this will not show up in the trade data as these products are manufactured in China.
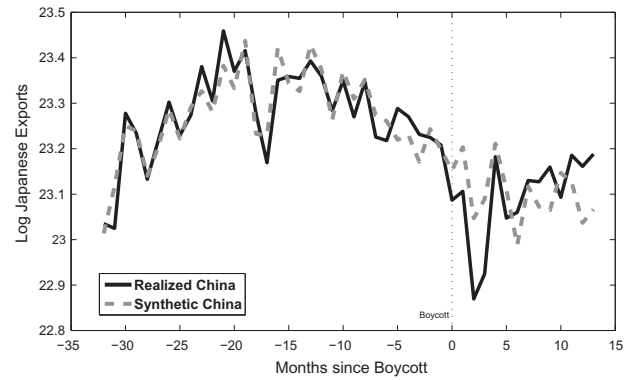
4.2.5. Event study

Even though a boycott does not appear in the trade data, it should affect companies' profits and thus be reflected in their stock prices. Following Govella and Newland (2010), I implement an event study for different incidents with daily market prices of 23 boycotted firms and

Table 8
Senkaku crisis: Estimated trade disruption.

Country	Correlation	3 months	6 months	9 months	12 months
PR China	0.913	−10.39%*** (.0021)	−9.09%** (.0147)	−3.59%* (.0854)	−2.69% (.1376)
Taiwan	0.840	0.87% (.2363)	6.75%*** (.003)	13.41%** (.0388)	10.20%* (.0648)
Hong Kong SAR	0.917	5.15%*** (.0030)	3.00%*** (.0057)	9.28%*** (.0007)	7.03%*** (.0022)
Placebo					
France	0.860	−2.48% (.3527)	−5.40% (.1029)	1.43% (.958)	1.09% (.7423)
Germany	0.860	3.57% (.0757)	1.47% (.2998)	0.20% (.7073)	0.15% (.6525)
Russia	0.861	−10.59%*** (.0001)	−7.88%** (.0199)	−0.19% (.2628)	−0.15% (.2901)
India	0.845	−1.02% (.6073)	4.76%** (.032)	−3.42% (.5129)	−2.56% (.2121)
Thailand	0.883	8.78%*** (.0003)	11.30%*** (.006)	6.94%* (.051)	5.21% (.1534)
UK	0.730	3.30% (.7451)	7.12% (.9986)	−4.25% (.3841)	−3.16% (.2811)
USA	0.963	3.93%** (.039)	7.18%*** (.0012)	11.60%** (.0163)	8.62%* (.0886)

Correlation is the pre-treatment correlation coefficient between treatment and synthetic control unit.
p-Values are in parentheses.

  * $p < 0.10$.
 ** $p < 0.05$.
*** $p < 0.01$.

Fig. 3. Realized and counterfactual Japanese exports to China.

12 domestic control firms using the market model with the Nikkei225 index as the market proxy (MacKinley, 1997).[16]

The event study results indicate that none of the events mentioned above had a statistically significant effect on the Japanese firms who were mentioned in the boycott calls. Neither the start of the protests on August 20 nor the nationalization of the Senkaku Islands, which is believed to be the main cause of the boycott, do affect stock market prices significantly. This surprising result is most likely due to the fact that the treatment countries make up a big share of the market index. Plotting the cumulative abnormal returns in Fig. A2 in the appendix for both the treatment and control country and the market return shows that abnormal returns do not deviate much from zero until about seven trading days after the boycott announcement, when both groups start to diverge. This coincides with negative returns of the market index which tracks the treatment group pretty well. The cumulative abnormal returns however start to converge again after 20 trading days and eventually are close to zero. This suggests that the boycott did have a lagged effect not only on the stock prices of Japanese firms mentioned in the boycott flyers but also on the whole market index. This effect however is temporary and dies out after roughly one month.

4.3. US–French boycott

Data for the trade boycott that resulted from the Iraq War are taken from the US Census Bureau's Foreign Trade database that provides SITC-denoted import and export data at monthly frequency. US–French trade relations are unbalanced: While France's share of US imports and exports amounts to only 2.4% and 2.8%, US shares of France's trade is much higher with 8% of imports and 8.1% of exports as measured in 2002.

For the French case, I estimate Eq. (1) for both US imports and exports, thus I now use data sources from the boycotting country. I set the treatment timing to February 2003, a period when public opinion polls detected a sharp fall in positive sentiment of France in America. I exclude Middle Eastern states from the control group to avoid contamination as there is mild evidence that some of these countries might have been involved in a boycott of US product over the Iraq War (Clerides et al, 2013). The results show that imports from France fell by almost 20% in the post-treatment period, a result that is robust to the inclusion of country fixed effects. The effect is slightly smaller for

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[16] The events include the announcement of the nationalization of the islands by the Japanese government on 11 September 2012 that most likely caused the boycott. I also use the onset of the actual protests on September 17 as the beginning of the event period. In addition, I analyze the effect of the protests in front of the Japanese embassy in Beijing on 15 August and a protest call by Chinese internet users on 20 August. The control countries are taken from "Japan's Best Domestic Brands 2012", published by Interbrand (2012), and have less than 10% foreign sales each. Most of these firms are engaged in non-traded services like broadband telecommunication, construction, and real estate.
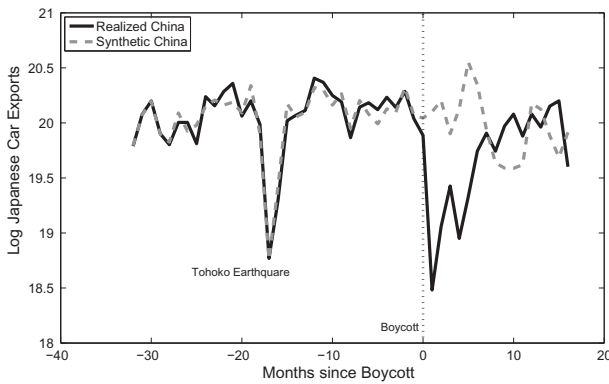
Fig. 4. Realized and counterfactual Japanese exports (HS8703 motor cars).

Table 9
Senkaku crisis: Estimated one year trade disruption by HS code (synthetic control).

HS code	Description	In USD	In %	*p*-Value
1902	Pasta	550,166	124.6%	0.1736
22	Beverages	−12,884,956	−26.5%	0.2679
3304	Make-up	−14,596,832	−7.7%**	0.0286
8508	Electromechanical tools	1,066,317	40.4%*	0.0855
8521	Video recording apparatus	5,677,785	32.1%	0.9600
8703	Motor cars	−1,888,019,883	−31.8%**	0.0295
9006	Still cameras	−4,630,183	−26.1%**	0.0286

exports to France, which fell by about 16% indicating that the Iraq conflict did not merely lead to a unilateral boycott, but that it seriously hurt trade relations in both directions (Table 10).

The results however are not robust in the synthetic control group specification, where we see much higher drop in US exports to France than in imports from it (Table 11). Fig. 5 shows the gap in exports to France compared to the synthetic control group[17] in the year following the boycott announcement. This sums up to a 15% trade disruption equivalent to 3.020 billion USD, a share of 0.4% of all US exports during that time. The reduction in imports instead is short-term and amounts to only 4.3% within the first three months. The one-year estimate of −2.8% (or 493 million USD) is not statistically significant. This suggests that the consumer boycott in the US was only temporary shocks to imports, but that it severely harmed exports through other channels. The possibilities include a consumer boycott in France or governmental intervention. That the US administration is willing to use commerce to punish can be seen by the policy to exclude French and other nations' firms from reconstruction contracts in Iraq.[18]

4.4. Gaza

I use data for Turkish imports and exports from Comtrade Monthly with availability until January 2015. In 2013, Israel only made up a small share of Turkish trade with 1% of imports and 1.7% of exports. Turkey had a higher share with 3.3% and 3.8% of Israeli imports and exports respectively.

The difference-in-differences results for the Gaza boycott in Table 12 show a surprising uptick in Turkish imports from Israel that is robust to the inclusion of fixed effects. The synthetic control group approach confirms the positive effect, although statistical significance holds only for the short period of the first three months after the Gaza war. Fig. 6 shows imports well above the synthetic control group until the very last period of available data when imports drop sharply. In summary, there is no evidence of any reduction of Turkish imports from Israel due to the crisis for a horizon of at least half a year and only with future availability of data will one be able to draw a conclusion. Instead, we observe a pronounced drop in exports to Israel of around 7.5% in the post-treatment period and the synthetic control group confirms the strongly significant effect. Israeli consumers appear to be more efficient at carrying out boycotts which again is confirmed by the synthetic control group approach.

4.5. Substitution effects

Applying the difference-in-differences methodology implicitly assumes that the control group is not affected by the boycott, i.e. that there is no substitution of exports from boycott to non-boycott countries. If these substitutions happen, the control group will be affected positively and the treatment effect will be upward biased in absolute values. However, the estimated treatment effect can still be interpreted as an upper bound of the true causal effect of the boycott on bilateral trade. Alternatively, we can interpret the estimate as the combination of the import-reducing direct effect of the boycott and the substitution effect, where the latter can at most be one half of the former.

Substitution can happen on both ends: The boycotted country might shift its export to other trading partners and thus alleviate the negative impact of the boycott, while the boycotting country might import the same products from other sources. Either way, this substitution is likely to be costly as importers source from the cheapest supplier and exporters supply to the country with the highest willingness to pay. The issue is loosely related to the literature on trade creation and trade diversion (Krueger, 1999; Magee, 2008) which distinguishes between substitutions of domestic production with trade as well as substitution of trade between trading partners, and suffers from similar identification problems.

To explore the issue of substitution, I use an event study approach and estimate a post-boycott period dummy for all countries separately according to Eq. (2), controlling for GDP and time trends. The advantage is that I do not have to specify a control group, but relying on only the time series variation results in imprecise estimates. I estimate Eq. (2) for Danish exports and then correlate the post-boycott dummies with the share of Muslim population. I find that on average, a 10% higher Muslim population is associated with a 3% drop of Danish exports to this country, a result which is very close to the initial difference-in-differences findings. Furthermore, the results suggest that a hypothetical country with zero Muslim population imports a statistically insignificant 6.5% more from Denmark (Table 13). While this does not completely rule out substitution effects, it shows that there is no obvious export substitution to non-boycotting countries.

$$Y_{j,t} = \alpha_j + \beta_1\ Post_{j,t} + \beta_2 \log GDP_t + \beta_3\ t + month_t + \epsilon_{j,t} \qquad (2)$$

To check for substitution on the import side, I run the same regression for US imports in the context of the Iraq War boycott. I find a stark (though statistically insignificant) drop in US imports from France which again is close to the difference-in-differences estimate. The average drop in imports for all non-boycotted countries is 3.9% which is not statistically significant from zero. Thus, again there is no clear evidence for substitution away from France to other trade partners.

5. Conclusion

The analysis of the case studies has shown that boycotts can have a significant effect on trade relations, and that political conflict has sizable spillovers to international trade. There is no such thing as a typical reaction to a boycott, but the impact on trade is very heterogeneous.
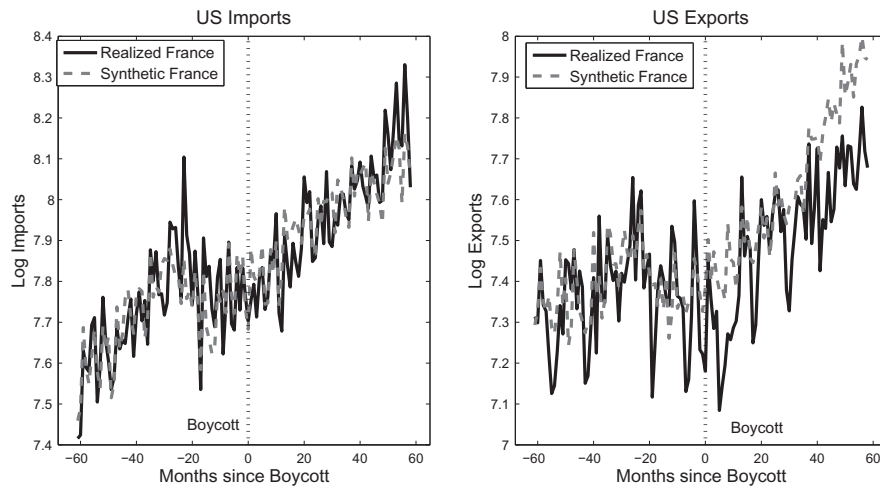
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[17] Notable is the high weight of Germany in the synthetic control group (Table A8.) The inclusion of Germany is somewhat questionable since the country itself was a major opponent of the Iraq War. However, Pandya and Venkatesan (2013) point out that the boycott was primarily directed against France and not against all the European countries that did not support the US in invading Iraq. In calculations not shown I find a similar effect if Germany is excluded from the controls.

[18] http://www.nytimes.com/2003/12/10/world/a-region-inflamed-the-reconstruction-pentagon-bars-three-nations-from-iraq-bids.html.

Fig. 5. Realized and counterfactual trade levels (US).

Estimates of trade reductions in the range of 30% and 40% show that some countries are very willing to carry out boycotts. However, there are also many countries that do not seem to boycott, but even increase their trade with the boycotted country. Comparing the results of the Danish–Muslim and the Japanese–Chinese boycotts, the similarities are that they both cause one-time reductions in trade, reverting to previous levels after several months. The boycotts are also impacting consumer goods much more than non-consumer goods and countries with more open political regimes tend to boycott more. Both findings are in line with the theory of boycotts being mainly consumer-driven with no or very moderate official government assistance. This suggests that agents do take the economic consequences of disrupting trade into account when choosing their conflict strategy, as boycotting consumer goods is plausibly less costly to the boycotter than stopping the import of intermediate and capital goods.

While the boycotts of Denmark and Japan appear to be one-directional with Danish and Japanese imports from the boycotting countries being mostly stable, the US–French dispute in 2003 and the Turkish–Israeli conflict in 2014 seem to have bilateral effects on both imports and exports. This is indicative of consumers in the boycotted country retaliating against the perceived aggression and the results suggest that the impact of this reverse boycott can be even larger than the

initial boycott, especially in the case of Israel. The US–French boycott nevertheless confirms the transient nature of the boycotts, while the poor data availability for the very recent boycott of Turkey prevents a clear conclusion on the nature of this event.

The boycotted countries in this analysis — Denmark, Japan, the US, and Israel — happen to be well-diversified export economies. In the case of Denmark, the Muslim countries that are prone to boycotting made up only 2.6% of total exports. This a priori limits the total impact of boycotts on the boycotted country's export sector. Even though the trade disruption was large for some boycotting countries, overall exports declined by only 0.4% in the case of Denmark. Very similar numbers hold true for the US where France's export share is 2.6% and the overall trade disruption is also 0.4%. Contrary to this is the boycott of Japan, where the boycotting country, China, makes up almost one fifth of total exports. The rather low estimate for the boycott effect however causes the total trade disruption to be only 0.5%. One explanation for this small estimate is that trade from Japan to China is still dominated by intermediate and capital goods. This suggests that for exporting countries that have a diverse range of export goods and destinations, the intended punishment effect of a boycott is not likely to have a major impact; however, a boycott could be potentially more harmful to more specialized countries. The inability of the empirical approach to account for substitution effects towards non-boycotting countries is likely further strengthening this argument.

This conclusion is not necessarily true for firms within the boycotted country. While the overall disruption of exports for both Denmark and Japan is low, some firms might have suffered heavily. The event study approach on individual firms' stock market prices, however, shows that explicitly boycotted Japanese firms did experience large negative abnormal returns compared to domestic firms during the boycott, but that these relative losses are reverted within a few weeks' time. It is likely that these large companies serve widely diversified markets with different products, so that a boycott by a single country is not overly damaging.

Table 10
US boycott: results.

Dependent variable	US imports from France		US exports to France	
	(1)	(2)	(3)	(4)
Log GDP	0.847***	0.748***	0.866***	0.794***
	(0.043)	(0.192)	(0.040)	(0.075)
Log distance	−0.545**		−1.284***	
	(0.218)		(0.203)	
Post	−0.106***	−0.078*	−0.073	−0.060
	(0.037)	(0.040)	(0.045)	(0.045)
France	−0.314		−0.366**	
	(0.198)		(0.172)	
Post × France	−0.197***	−0.225***	−0.164***	−0.177***
	(0.053)	(0.048)	(0.027)	(0.023)
Constant	0.754	−3.142	6.802***	−3.892***
	(1.941)	(1.996)	(1.688)	(0.814)
Country fixed effects	No	Yes	No	Yes
Trend and month FE	Yes	Yes	Yes	Yes
N	10,219	10,219	8924	8924
Adj. R^2	0.708	0.952	0.744	0.964

Standard errors are in parentheses (clustered at country level).
 * $p < 0.10$.
 ** $p < 0.05$.
 *** $p < 0.01$.

Table 11
Estimated trade disruption (synthetic control).

US	Imports	Exports	Turkey	Imports	Exports
3 months	−4.3%***	−9.1%***	3 months	14.5%	−10.5%
	(0.0399)	(0.0052)		(0.0550)	(0.0774)
6 Months	−1.7%	−15.0%***	6 months	11.1%	−12.3%***
	(0.3242)	(0.0001)		(0.6285)	(0.0000)
12 Months	−2.8%	−8.9%***			
	(0.2190)	(0.0021)			

p-Values are in parentheses.
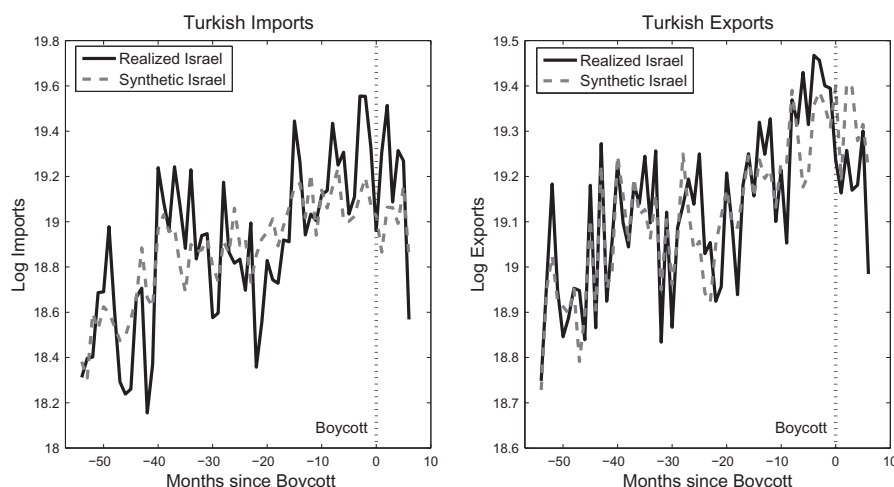
Fig. 6. Realized and counterfactual trade levels (Turkey).

Table 12
Gaza: results.

	Turkish imports		Turkish exports	
	(1)	(2)	(3)	(4)
Log distance	−0.198		−0.436***	
	(0.152)		(0.119)	
Post	−0.106***	−0.105***	−0.114***	−0.114***
	(0.0346)	(0.0351)	(0.0274)	(0.0276)
Israel	0.599***		1.387***	
	(0.189)		(0.129)	
Post × Israel	0.221***	0.218***	−0.0751***	−0.0751***
	(0.0394)	(0.0415)	(0.0248)	(0.0250)
Constant	17.02***	15.46***	15.76***	12.33***
	(1.487)	(0.793)	(1.165)	(0.573)
N	3717	3717	3904	3904
Adj. R^2	0.025	0.879	0.178	0.926

Standard errors are in parentheses (clustered at country level).

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Table 13
Substitution effects.

	(1)	(1)
	Danish exports	US imports
Muslim share	−0.308***	
	(0.086)	
France		−0.233
		(0.484)
Constant	0.065	−0.039
	(0.041)	(0.049)
N	122	96

Standard errors are in parentheses.

* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$

Acknowledgments

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jinteco.2015.11.008.

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