Supply Chain Disruptions and Causal Outcomes: Evidence from the Bankruptcy of Hanjin Shipping

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Abstract

This paper exploits the insolvency announcement of Hanjin Shipping in 2016, to study the causal effects of supply chain disruptions on inventory investment, sourcing behavior and stock returns. I use a novel dataset that links 5 million shipments from U.S. Customs with real-time vessel movement data to identify impacted firms and the distribution of cargo delays to study their causal effects. I present four key findings. First, I show that disruptions to the flow of inventory — characterized by uncertainty in resolution — can result in excess inventory due to panic orders. Second, I provide novel evidence that pre-existing supply chain redundancies can benefit firms *during* disruptions as hedging tools. Third, I show that supply chain disruptions can cause firms to modify future sourcing strategies. Finally, instrumenting for the increases in inventory with disruption variables, I re-examine the link between excess inventory and abnormal stock returns. In contrast to prior work, I find no such associations.

Keywords: Supply Chain Disruptions, Empirical Operations, Supply Chain Management

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In recent decades, supply chain disruptions have become more frequent and costly. In 2016, one in three firms surveyed by the Zurich Insurance Group and the Business Continuity Institute reported losses in excess of $1 million due to supply chain disruptions in that year. But while there is a large body of theoretical research studying effective supply chain disruption management (e.g. Tang (2006) and Tomlin (2006)) and the association between sourcing strategies and disruption risk (Yang et al. (2009)), there is relatively little empirical work documenting the causal impact of disruptions on key firm outcomes such as inventory levels and stock returns.

Understanding causal relationships and their mechanisms can be valuable to firm managers who wish to make better investments in their supply chains to mitigate losses from future disruptions. Several high profile examples have hinted at the large losses associated with supply chain disruptions (Chopra and Sodhi, 2004) and in addition, a sequence of papers by Hendricks and Singhal have shown that disruption announcements are negatively associated with a multitude of firm outcomes: lower sales, higher growth in cost, higher growth in inventories and lower stock returns (Hendricks and Singhal, 2003, 2005, 2009).

However, causal evidence on the associations between supply chain disruptions and firm outcomes is sparse. Moreover, despite having strong theoretical foundations, there is relatively little empirical evidence for the value of well-known supply chain risk-management strategies (e.g. increasing supplier redundancies). This can be problematic for firm managers that are looking to perform accurate cost/benefit analyses of risk-reduction strategies. Indeed, the lack of causal evidence may be one explanation as to why firms under-invest in disruption risk-management strategies despite conducting extensive risk-assessment exercises (Rice and Caniato, 2003; Tang, 2006; Zsidisin et al., 2004).

Studying the causal outcomes and mechanisms of supply chain disruptions is challenging because supply chains are difficult to detail and deeply integrated into the overall economy. This makes the identification of the causal impacts of disruptions on firm outcomes difficult for at least two reasons.

First, selection bias is likely to arise because firm sourcing strategies and inventory management practices predispose firms to varying degrees of supply chain risk (Tomlin, 2006; Yang et al., 2009). In recent decades, the push for lean strategies such as single sourcing and just-in-time inventory management practices have reduced costs but may have caused firms to be more exposed to supply chain disruption risk (Tang, 2006). Therefore, causal inference is difficult without having detailed information on firms' supply chains.

Second, disruptions can result from underlying economic conditions or events that simultaneously impact firm outcomes. Consider a negative household demand shock that causes an upstream supplier to go bankrupt (Yang et al., 2015). Not only can this disruption depress downstream firms' sales via input shortages, but the original demand shock can also further amplify the depression in sales. Here, causal inference is difficult without having detailed knowledge on the nature of the disruption.

This paper addresses these challenges by exploiting the disruption resulting from the receivership announcement of Hanjin Shipping (henceforth, Hanjin) as a plausibly exogenous shock to the global supply chain. The receivership announcement of Hanjin in 2016 inflicted widespread uncertainty to the global supply chain as cargoes worth over $14 billion USD were stranded outside ports for upwards of two months. I use the variation in cargo delays as a source of exogenous variation to study how supply chain disruptions affect key indicators of firm performance and future sourcing strategies.

I present four key findings. First, I show that disruptions to the flow of inventory – characterized by uncertainty in resolution – can result in excess inventory due to the firms placing larger panic orders. In the case of Hanjin's insolvency announcement, this disruption caused impacted firms' inventories to grow by more than 50%. Second, I provide novel, empirical evidence that pre-existing supply chain redundancies can benefit firms *during* disruptions (i.e., before the disruption has resolved) as hedging tools. Impacted firms with larger supply-base redundancies are able to adopt a 'wait and see policy' in response to the uncertain cargo delays and accumulate less redundant inventory. Third, I show that supply chain disruptions can cause firms to modify future sourcing strategies. Impacted firms increased the number of redundant suppliers, increased geographic heterogeneity and transported their cargo using more carriers. Finally, instrumenting for the increases in inventory with the disruption, I re-examine the link between excess inventory and stock returns. In contrast to prior work, I find no such associations.

These findings result from a novel panel dataset that links over 5 million U.S Customs' shipping records of publicly traded U.S. firms with real-time vessel tracking data. Combined, each observation provides detailed information on each firm involved in the shipment (buyer, supplier, transporting carrier), the goods transacted (6 digit Harmonized Sales (HS) code and their weight) and the carrying vessel (location, speed, direction, name and International Maritime Organization (IMO) identifier). I compute the delay of cargo on every Hanjin vessel by subtracting the arrival date from the expected date of arrival that the captain of the vessel declared prior to departure. This data is then linked with financial and accounting variables from the CRSP/Compustat Merged database to identify the full distribution of micro-level disruptions and outcomes.

To argue that my estimates admit a causal interpretation, I complement facts of the stagnating maritime transport industry with two data-driven arguments that taken together, lend credibility to the plausible exogeneity of the shock. First, by examining shipping records prior to the date of receivership, I show that there are no significant deviations in the usage of Hanjin between treatment and control firms. Second, the data reveals a feature of the ocean carrier industry that limits how much control cargo owners have on *who* ultimately transport their goods. Because ocean carrier firms frequently share cargo space on their vessels to increase service and capacity (Slack et al., 2002), it is common that cargo contracted to one carrier is ultimately transported by a vessel owned and operated by another (Quartieri, 2017). Indeed, I show that the overwhelming majority of disrupted cargo on Hanjin vessels were actually *not* assigned to Hanjin as the transporting carrier. And because pricing and usage decisions are usually contracted with the assigned carrier (Quartieri, 2017), this means that even if a firm had knowledge of the impending bankruptcy, the firm would require detailed knowledge of Hanjin's strategic partnerships to avoid being impacted.

I argue that the results in this paper have high external validity and can be used to understand the impacts of other supply chain disruptions. At first glance, the bankruptcy of a large ocean carrier may seem to be a tail event. Indeed the last major bankruptcy was in 1986 by U.S. Lines and had limited impact on the global supply chain. However, at its core, this supply chain disruption resembles many others in the sense that it subjected firms to an unanticipated delay in the flow of goods – characterized by uncertainty in resolution. Labour disputes, political and armed conflicts, natural disasters, trade embargoes and product contamination can all potentially stall the flow of goods between firms for uncertain lengths of time and subject firms to uncertainty that can result in costly hedging decisions that are harmful, ex–post. Thus, the results from this paper can help firm managers to better understand the costs associated with other supply chain disruptions.

Beyond my key findings, I argue that my results offer a new perspective on how disruptions can negatively impact firms. Not only are there certain, direct costs associated with supply chain disruptions (e.g., disrupted production due to natural disasters, write offs due to spoiled inventory). But there can also be *indirect* costs resulting from how firms respond to the uncertainty generated by the disruption. The precautionary decisions that firms make under this uncertainty, albeit prudent, can come with significant costs depending on how quickly the uncertainty resolves and the total impact of the disruption. In the case of this paper, impacted firms hedged against the possibility of lost sales during key holiday periods by placing redundant orders. However, these prudent choices resulted in excess inventories as delayed cargo were all delivered prior to key holiday sales.

Literature

This paper contributes to a growing empirical literature examining the aggregate association between supply chain disruptions and firm outcomes. In a seminal paper, Hendricks and Singhal (2003) combine heterogenous supply chain glitch announcements from the Wall Street Journal and the Dow Jones News Service to show that disruption announcements are associated with negative stock returns. Subsequent papers using their methodology have shed further light on the pervasive and harmful nature of supply chain disruptions (Hendricks and Singhal, 2005, 2009; Schmidt and Raman, 2012). By estimating the costs associated with a diverse sample of disruptions, these papers provide insight on the value of reliable and robust supply chains.

This paper also contributes to the empirical literature examining the impact of excess inventory on firm outcomes. Hendricks and Singhal (2009) estimates a significantly negative association between stock returns and excess inventory announcements. In addition, Rumyantsev and Netessine (2007b) find that responsive inventory management is associated with higher firms earnings. In this paper, I exploit the increases in inventory resulting from Hanjin's insolvency announcement to study its effect on stock returns. In contrast to prior work, I find a null association.

To the best of my knowledge, this is the first paper in the empirical literature that claims to identify a *causal* link between supply chain disruptions and firm outcomes in an observational setting. In addition, while past work has examined the aggregate association between disruptions and firm outcomes, this paper studies a single supply chain disruption that affected many different firms to different degrees. One advantage of this approach is that I can more accurately study the mechanisms by which the disruption impacted firm outcomes. This is in contrast to using an aggregate set of disruptions where different disruptions can have heterogenous effects on firm outcomes.

1 The Bankruptcy of Hanjin Shipping

1.1 Background

Hanjin Shipping, a subsidiary of one of Korea's conglomerates (Hanjin Group), was the seventh largest container carrier in the world. Hanjin's 141 vessels transported 100 million tons of cargo annually to 70 ports across 35 countries. The company held substantial shares in some of the world's largest and most lucrative trade routes including 8.37% of all trades between Asia and North America, 5% between Asia and northern Europe and 7.66% between Asian and the Mediterranean (Desormeaux, 2016). In addition to the physical transportation of goods, Hanjin owned and operated eleven port terminals across Belgium, Japan, South Korea, Spain, Taiwan and the Netherlands.

Despite its large presence in the global shipping industry and high operating revenues, Hanjin was not profitable and had not been profitable since the Financial Crisis of 2009. The company posted net losses of $757.9 million in 2011, $586.0 million in 2012, $630.8 million in 2013 and $386 million in 2014. By the end of 2015, the company had accrued debts in excess of $5 billion (*Journal of Commerce)*. The lion's share of Hanjin's debts were held by the Korean Development Bank (KDB) — a bank that was wholly owned by the Korean government. The KDB continued to support Hanjin's operations through the provision of liquid funds and subsidies. However, Hanjin's continued financial losses were deemed unsustainable and the KDB withdrew support of the struggling carrier on August 30, 2016. Due to the lack of liquid funds to continue operations, Hanjin proceeded to bankruptcy.

Despite Hanjin's poor financial indicators, predicting bankruptcy was difficult due to the struggles the ocean shipping industry was facing as a whole. High amounts of accrued debt and large operating losses were the status quo, and in the year Hanjin filed for receivership, the container line industry posted collective losses in excess of $3.5 billion (Waters, 2017). It was also unclear if the Korean government – who held the lion's share of Hanjin's debt – would axe their flagship carrier as they bailed out another ailing ocean carrier, Hyundai Merchant Marine Co. Ltd, three months earlier in a $570 million debt-for-equity swap (Lee, 2016).

1.2 Receivership Announcement

On August 31, 2016, Hanjin filed for receivership at the Seoul Central District Court. The primary effect of the receivership on the global supply chain was the immediate interruption and delay of inventory. Hanjin vessels with cargo on-board could not reach their destinations and unload cargo for two reasons. First, ports were rejecting Hanjin vessels in fears that they would not be paid for their services (Jun, 2016). Because Hanjin's creditors had rejected their restructuring plans, the company no longer had resources to pay for receiving and unloading fees. Secondly, Hanjin had intentionally prevented their own vessels from being unloaded in ports with creditors waiting to seize their assets (Nam, 2016). In total, an estimated $14 billion USD of cargo was tied up in approximately 90 Hanjin vessels involving 8,300 cargo owners (Hals, 2016).

A third effect on the global supply chain was the spillover onto ocean transportation costs. Due to Hanjin's receivership announcement, there was a sudden influx in demand for cargo delivery on routes that Hanjin could no longer service. According to the Shanghai Shipping Exchange, ocean transportation costs increased up to 50% after Hanjin filed for bankruptcy protection.

In response to the unanticipated delays, there is anecdotal evidence suggesting that impacted firms were placing redundant orders to hedge against the possibility that their cargo would arrive too late or not at all. Samsung announced that the company was transporting replacement cargo by air at "great costs" (Reynolds, 2016), and Maersk, the largest carrier in the world, revealed that they were shipping replacement orders for impacted firms (Nyshka, 2016).

2 Data

The analysis of this paper combines data from three different sources: U.S. Customs import data obtained from Panjiva/S&P Global Market Intelligence. AIS vessel tracking data from VesselFinder and financial data from the CRSP/Compustat merged database. This section describes each dataset and defines how I measure the disruption magnitude of firms in my sample.

2.1 Sources

I identify shipments that were delayed by Hanjin's receivership announcement by examining Panjiva's historical shipment database, which contains detailed U.S. Customs bill of lading manifests of public and private U.S. firms. Panjiva tracks companies in global trade and receives shipment records directly from U.S. Customs and the Department of Homeland Security. Due to access limitations, this paper focuses only on public U.S. firms and constructs a panel dataset between July 1, 2007 and Dec 31, 2017 containing sea imports unloaded at U.S. ports using shipment's bill of lading manifests. This manifest acts as a receipt issued by the carrier to the shipper confirming that goods are on board for transport to the consignee (receiver).

For each transaction, I observe the date the shipment arrived in the United States, the consignee (name, address, stock ticker), the shipper (name, address), cargo information (number of containers, 6 digit Harmonized Sales (HS) Code, country of origin, written description, weight), transportation carrier and vessel information (name and International Maritime Organization number). The vessel IMO is a unique identifier for ships issued by the International Maritime Organization, United Nations agency. I also observe the names of the port the cargo was picked up and unloaded.

An important benefit of the panel structure of my data is that it allows me to observe pre and post disruption shipping behavior of firms. This is critical in my analysis for several reasons. First, I can mitigate the problem of selection bias arising from heterogeneous sourcing strategies through the inclusion of supply chain proxies in my analyses. Secondly, I can provide empirical justification for the exogeneity of the supply chain disruption by examining the usage of Hanjin in months leading up to the disruption. If the data does not indicate large deviations ins usage by the treatment or control groups, exogeneity of the shock is more plausible. In addition, because I am able to examine firms' post-disruption sourcing patterns, I can see if the disruption had any impact on firms' sourcing strategies.

I approximate the delay each Hanjin vessel experienced by examining vessel movement data tracked by terrestrial Automatic Identification System (AIS) receivers supplied by VesselFinder. AIS receivers are fitted on every vessels carrying more than 300 gross tons through international waters. These trackers provide information on the vessel's position (latitude, longitude), speed, course and heading. To approximate the delay, I subtract the number of days between the arrival date indicated on the shipment record from the estimated time of arrival, imputed by the master (captain) of the vessel prior to departure. Shipment data and AIS tracking data are easily matchable due to the unique IMO identifier associated with each vessel. All delayed shipments in my sample were matched to AIS data.

For validation purposes, I check to see if the affected vessels and delays are consistent with analyst coverage of the disruption. In particular, I use analyst reports from the *Journal of Commerce* (IHS Markit), an online newspaper that covers international logistics and shipping and news reports from Alphaliner, another industry research organization. I find that the names of affected vessels and estimated delays match with several reports from these two sources.

Lastly, I obtain stock price data from the Center for Research in Security Prices (CRSP) and firm-level financial/accounting data on the sample firms through the CRSP/Compustat Merged database. The data is matched to firms from the shipment data through the stock ticker and consignee name.

In sum, the total number of shipments from publicly traded companies (AMEX, NYSE and NASDAQ) between July 1, 2007 and Dec 31, 2017 is in excess of 5 million. In my analysis, I examine shipments between Jan 1, 2014 to Dec 31, 2017. I define the *treatment group* as any firm that had at least one shipment delayed due to Hanjin's receivership announcement. The *control group* is simply the set difference between all importing firms and the treatment group in the analyzed time frame. The vessel AIS data includes over 7,000 position records at a 24-hour resolution (one position per day) for 110 Hanjin Vessels between August 31, 2016 and December 31, 2016.

2.2 Shock Variables

I use two shock variables to estimate the treatment effects of the supply chain disruption. The first is an indicator variable that is equal to 1 for treatment firm and 0 otherwise. In my regressions, the coefficient associated with this variable will capture the average difference in outcomes between the two groups.

The second treatment variable exploits the variation in cargo delays to compute a statistic that captures the average number of days delayed (across all cargo) experienced by each firm. Specifically:

$$AvgDaysDelayed_i = \frac{1}{|M_i|} \sum_{g \in M_i} Delay_{i,g} \tag{1}$$

where M_i is the set of delayed shipments for firm i, $|\cdot|$ is the cardinality of the set, and $Delay_{i,g}$ is the delay of the shipment.

For firms with only one impacted shipment, this value will simply be the representative delay the firm experienced. These firms are approximately 40% of the treatment group. However, for firms that have had multiple shipments delayed, this variable takes the average of the delays across these shipments and the interpretation becomes less straightforward.

Across all delayed shipments, the average and standard deviation of delays (in days) are 16 and 10, respectively. At the firm level, the average and standard deviation of $AvgDaysDelayed$ is 5 and 8, respectively. As a robustness check, I replace the mean number of days with the median in an attempt to control for the possibility of outliers in the distribution of cargo delays for each firm. I find that this modification has no material impact on my results.

2.3 Summary Statistics

I focus on a sample of 1,466 public U.S. firms (AMEX, NYSE, Nasdaq) with at least one maritime import between January 1st, 2014 and Dec 31st, 2017. Although treatment firms comprise of only 12% of the number of total firms, they are responsible for more than 50% of observed shipments. This makes intuitive sense as the likelihood of having at least one shipment impacted by Hanjin's receivership announcement is (weakly) increasing in the firm's number of imports. The opposite is true of firms that import less frequently. Treatment firms have approximately 10 times more observed shipments, six times more suppliers, five times more HS codes imported, 3 times more sourcing countries and their shipments are transported on approximately 6.5 times more vessels.

For operational outcomes, treatment firms also appear to be larger than their control counterparts. On average, they have larger inventories (1.5x), higher cost of goods sold (1.6x), net income (1.7x), operating income (1.4x) and sales (1.5x). For financial variables, treatment firms are larger in size (log of market capitalization), but do not greatly differ in book to market ratio, trading volume or previous year's stock return (momentum). I report full summary statistics of both shipping behavior and financial/accounting variables summary statistics can be found in the Online Appendix.

The large distributional differences in observed covariates suggests a systematic difference between treatment and control firms. Not accounting for these differences will hinder causal analysis if these covariates are simultaneously correlated with treatment variables and firm outcomes, Finding ways to control for the large differences in the observed covariates will be the focus of Section 3 and various robustness checks are performed in the main analysis to attenuate potential selection bias.

I now examine the distribution of delayed goods. Figure 1 (a) presents the histogram of all delayed goods and a cursory glance indicates that this distribution is far from uniform. The majority of delayed goods fell under four HS code 'chapters' (classification at the 2 digit level): Chapter 84 (*Nuclear reactors, boilers, machinery and mechanical appliances*), Chapter 85 (*Electrical machinery and equipment and parts thereof*), Chapter 39 (*Plastics and articles thereof* and Chapter 95 (*Toys, games and sports requisites*) with approximately 200, 130, 90 and 65 shipments, respectively. Chapter 95 is consistent with cargo being shipped for holiday sales and the other chapters are consistent with weight and height restrictions associated that preclude the transport of such cargo via airplanes.

The importance of understanding the distribution of delayed cargo becomes more apparent in Panel (b), (c) and (d). These histograms reveal that goods are associated with varying number of unique suppliers, carriers and countries. At the extremes, HS Code 2909.11 (Diethyl ether) was sourced from one supplier and one country across all public firms in 2016. Whereas 9403.60 (other wooden furniture) was sourced from 648 different suppliers across 54 countries.

These differences may contribute to the differential risk profiles of industries and hence, differential behavioral responses to supply chain disruptions. For example, the scarcity of suppliers may limit bargaining power of importing firms, increase search costs associated with finding new suppliers and cause large disruptions if suppliers become unavailable (Crook and Combs, 2007). Furthermore, the risk of unstable currency fluctuations and political instability can also be a non-trivial source of risk if inputs can only be sourced from a limited number of countries (Sodhi and Tang, 2012) or from 'high risk' countries. These concerns are primarily addressed through the inclusion of good, firm and industry fixed effects when applicable.

3 Sourcing Strategies and Disruption Propensity

The stark differences in shipping covariates between treatment and control firms warrant a closer look on how these covariates are correlated with treatment status and firm outcomes. Even after controlling for the frequency of shipments, there are also large variations across firms in terms of how many suppliers they contract with, how many countries their imports are sourced from and how many transportation carriers are contracted to transport their cargo.

To control for the potential confounding effects of these differences, I exploit the panel structure of my data to examine the shipping patterns of all firms in my sample prior to the disruption. This data is then used to construct measures that serve as proxies for redundancies across suppliers, countries and shipping carriers. These measures are also consistent with the theoretical literature on supply chain strategies that prescribe diversification and redundancies as ex–ante hedges against disruptions (Tang, 2006). I include these measures in all my analyses to attenuate potential omitted variable bias resulting from unobserved treatment selection.

3.1 Measuring Supply Chain Resilience

For each firm, I define three measures of redundancies. Each measure is simply the weighted average of the number of unique entities (suppliers, countries, carriers) associated with each 6 digit HS code, where the weight is the number of shipments for each good. Formally:

$$\varphi_{i,t}^{\text{Entity}} = \sum_{g \in HSCode_i} \frac{\text{No. Shipments}_{i,g,t}}{\text{Total Shipments}_{i,t}} \times \text{Unique Entity}_{i,g,t} \tag{2}$$

for firm i, good g in period t and $Entity \in \{Suppliers, Carriers, Countries\}$.

The main appeal of this measure is that it is simple to interpret and is broadly applicable to firms that have varying numbers of shipments across different goods. One major drawback is these measures fall prey to the distributional variations in entities across goods that make comparisons between firms in different industries difficult. For example, if the universe of apple suppliers and furniture suppliers are 5 and 100, respectively, then firms in the fruit business will likely have smaller supplier redundancies than firms dealing furniture solely due to the fact that there are fewer apple suppliers. However, this does not necessarily mean that firms in the fruit business have greater supply chain redundancies than firms in the furniture industry.

The high variance in the number of different entities is also evidenced by panels (b) through (d) in Figure 1. I attempt to mitigate these effects by including industry fixed effects in regressions whenever possible and by constructing these measures at different HS code lengths. Increasing the length of the HS code increases the specificity of the good, which in turn decreases the number of available suppliers.

These measures also appear to have sensible qualities. I estimate strong pairwise correlations among these measures indicating that firms with large redundancies in one dimension of their supply chain (e.g. suppliers) are also likely to have high redundancies in other dimensions (e.g. countries). Moreover, these measures are positively correlated with cost of goods sold but uncorrelated with inventory indicating that increasing redundancies in the supply chain comes at a cost. I also estimate highly statistically significant correlations between these measures and treatment status and magnitude. Finally, these measures are also correlated with sales, cost of goods sold but are uncorrelated with levels of inventory. I present a full correlation matrix is presented in the Online Appendix.

These empirical results corroborate past theoretical work associating sourcing strategies and inventory management practices with disruption risk. They also raise concerns about the validity of traditional event study disruption methodology where disruption-stricken firms are matched with controls based on firm size, SIC codes or other observables from financial data – but not on supply chain characteristics.

4 Plausible Exogeneity

In previous sections, I presented several facts that lend credibility to the assumption that the shock was exogenous to firms, conditional on observed covariates. First, Hanjin's financial struggles were consistent with industry competitors. Second, Hyundai Merchant Marine, another ailing South Korean carrier was bailed out by the same state-run creditor only three months prior. Third, Hanjin's transporting volume *increased* 7.2% in the quarter before the bankruptcy filing. Finally, $14 billion of cargo was willingly placed by shippers on Hanjin vessels – near full capacity of Hanjin vessels. These facts do not square with the notion that the industry was anticipating a large scale collapse of the world's seventh largest carrier. However, it is still possible that an unknown subset of firms were able to correctly anticipate the insolvency of Hanjin and stop using Hanjin as their transportation carrier. If this is true, then a causal interpretation of my estimates is less tenable because the shock could vary systematically with attributes of this unknown subset of firms and the outcomes of interest.

In this section, I present two distinct empirical facts that alleviate these concerns. First, I show that even if some unknown subset of firms were able to correctly anticipate the disruption, their ability to control their exposure to the disruption is severely constrained due to prevalence of vessel-sharing arrangements in the maritime shipping industry. Next, I show that leading up to the receivership announcement, no significant deviations in Hanjin usage can be detected between the treatment and control groups.

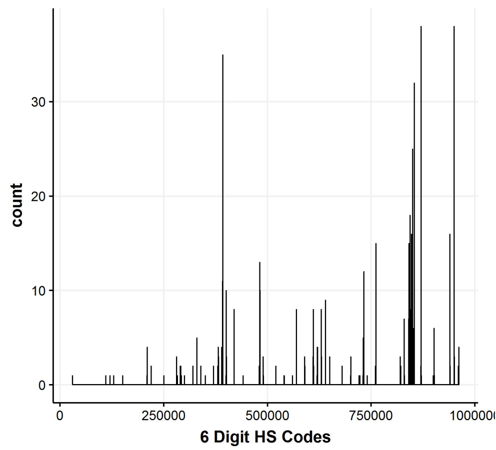
4.1 Carrier Strategic Alliances

Most, if not all large shipping companies are involved in some strategic agreement to share cargo room aboard vessels. This allows carriers to transport cargo across shipping lines that are not profitable enough to operate using their own vessels and allows carriers to increase service frequency and increase geographical coverage (Caschili et al., 2014; Slack et al., 2002).

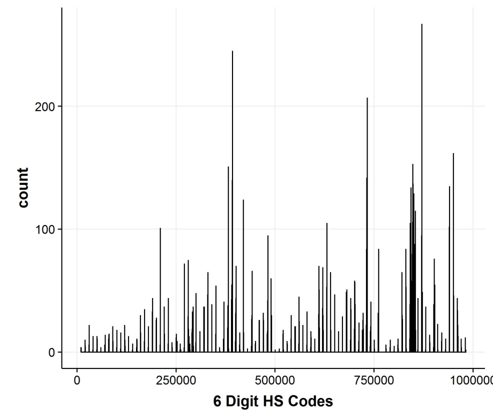
These arrangements generally come in the form of slot charter agreements, vessel–sharing agreements (VSA) or carrier alliances. Slot charter agreements involve purchases of limited space on another carrier's vessel. VSA's are agreements where multiple carriers jointly operate one or more lines by sharing vessel resources. Finally, carrier alliances involve the joint operation of multiple lines. The main distinction between these strategic partnerships and an outright merger is that individual carriers make independent sales and pricing decisions (Quartieri, 2017). In addition, members do not typically pay each other for these slots and capacity sharing is generally done on a quid pro quo basis.

The VSAs made it difficult, but not impossible, for firms to select into *and* out of their cargo being transported on a Hanjin vessel. A fortuitous cargo owner may not have known that Hanjin would go under, but by virtue of the VSAs, escaped any unanticipated delays to their cargo. Conversely, an informed firm may have been exposed to the disruption even though it was attempting to strategically avoiding Hanjin as the transporting carrier. The extent of how much control firms have in selecting vessels and further details on the nature of vessel sharing agreements have been studied extensively and can be found in the transportation literature (e.g, Panayides and Wiedmer

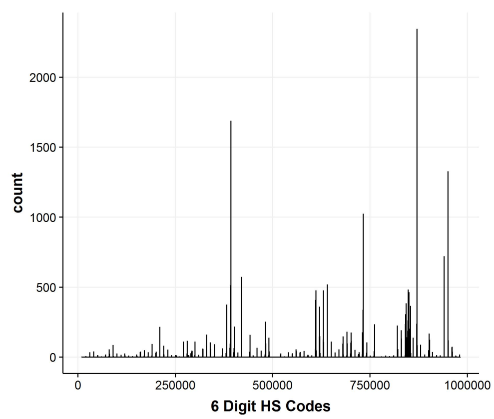
(2011)).



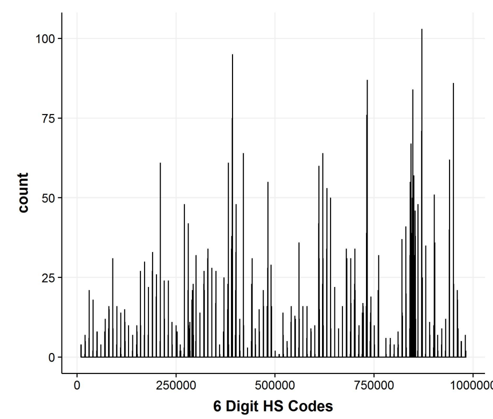
(a) Delayed Cargo



(b) Unique Carriers



(c) Unique Suppliers



(d) Unique Countries

Figure 1: **Histograms** (a) Distribution of delayed cargo. (Bin size = 1). (b) Distribution of unique carriers associated with each HS Code (Bin size = 5) (c) Distribution of unique suppliers associated with each HS Code (Bin size =5). (d) Distribution of unique countries associated with each HS Code (Bin size = 5).

My data is consistent with these claims. Table 10 presents the carrier breakdown of all shipments delayed by the disruption. Close to 400 carriers are listed as the handling carrier of delayed cargo. And only 22% of the shipments on delayed Hanjin vessels were actually contracted to Hanjin as the handling carrier. Hanjin's large market share, fleet size and the sheer number of observed non-Hanjin carriers in Table 10 suggests that manipulating shipments to completely avoid the use of Hanjin vessels would have been a very difficult feat.

4.2 Pre-Receivership Shipping Behavior

Another way to argue for the shock's exogeneity is to analyze shipping patterns prior to the bankruptcy to see if there are discernible divergences in shipping behavior between the control and treatment group leading up to the disruption. To this end, I exploit the panel structure of my data and construct two measures of Hanjin usage prior to the bankruptcy for the control and treatment groups:

$$\theta_s^{vessel} = \frac{\text{\# HJ Vessels}_s}{\text{Total Shipments}_s}, \quad \theta_s^{carrier} = \frac{\text{\# HJ Carriers}_s}{\text{Total Shipments}_s}, \quad \theta_s^{alliance} = \frac{\text{\# HJ Alliances}_s}{\text{Total Shipments}_s}$$

with $s \in \{Treatment,\ Control\}$. The first measure is the share of shipments that were transported by Hanjin vessels (independent of the assigned carrier), the second is the share of shipments that were assigned to Hanjin as the carrier and the last measure is the share of shipments that were assigned to Hanjin's alliance (Cosco Container Lines, "K" Line, Yang Ming Line, Hanjin Shipping, and Evergreen Line). I include the latter measure because carriers share vessels frequently within alliances and thus, an informed firm may have avoided the alliance altogether.

If the bankruptcy and subsequent disruption was unanticipated by firms, we would expect to see relatively constant values between the treatment and control with no significant deviations prior to the disruption. In contrast, if firms in the control group had prior knowledge on Hanjin's insolvency and able too manipulate their shipments onto other vessels, we should see a sharp decline in θ_s^{vessel} and $\theta_s^{carrier}$ for control firms.

But this is not what we see. Figure 3 presents the time series for all three measures. The solid dot and triangle indicate the values of each measure for the control and treatment groups, respectively. I use six month intervals leading up to the receivership announcement to plot the measures. In general, we do not see any large deviations in usage in the two or so years leading up to the insolvency announcement. Additional time intervals (3 months and 12 months) also show no significant deviations and can be found in the Online Appendix.

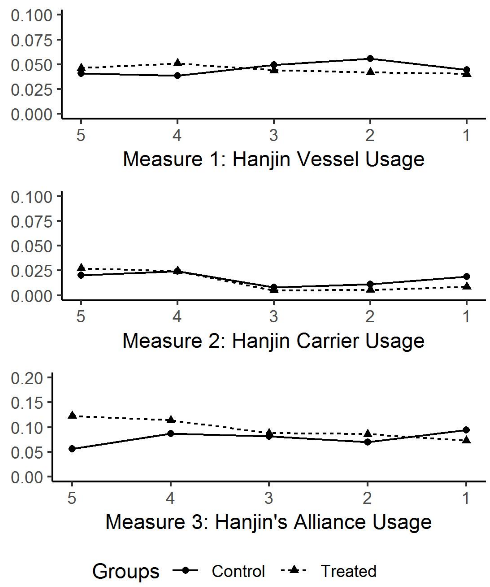


Figure 2: Time series using 6 month intervals. Horizontal axis corresponds to the six month intervals leading up to Hanjin's insolvency announcement. For example, a value of 1 corresponds to Feb 28, 2016 – August 31. 2016, 2 corresponds to August 30, 2015 – Feb 27, 2016 and so on.

5 Impact on Inventory

I now examine the impact of the shock on firms' inventory levels in the cross-section. The underlying hypothesis here is that the uncertainty in the disruption's resolution could cause firms to place redundant orders to meet key sales forecasts (Nyshka, 2016).

As a first exercise, I examine the percentage increase in firms' fourth quarter inventory levels between 2015 and 2016. Because the receivership announcement occurred towards the end of the third fiscal quarter and completely resolved before the end of the fourth quarter, all redundant orders (if any) would show up in fourth quarter inventory levels (2016).

5.1 Inventory: Baseline Results

The model to test the impact of the shock on inventory can be written as follows:

$$\frac{y_{i,Q4}^{2016} - y_{i,Q4}^{2015}}{y_{i,Q4}^{2015}} = \alpha + \beta Shock_i + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\rho} \boldsymbol{\Phi}_i + \ \boldsymbol{\eta} Z_i + \varepsilon_i \tag{3}$$

where the dependent variable is the percentage increase in fourth quarter inventory levels for firm i between 2015 and 2016. The explanatory variables include $Shock$ which is either $AvgDaysDelayed$ (Subsection 2.2) or a dummy variable that is equal to 1 for treatment firms and 0 otherwise; $\boldsymbol{X}$ are sourcing proxies; $\boldsymbol{\Phi}$ are additional controls that may affect the change in inventory levels (e.g. the percentage increase in 4Q sales between 2015 and 2016); Z_i are Fama-French 17 industry fixed effects and ε_{it} is a random noise term. The main coefficient of interest, β, captures the causal impact of the shock on Q4 inventories between 2015 and 2016.

For control variables, I include the percentage change in fourth quarter sales as the change in inventory could be attributed to demand side fundamentals (i.e., lower sales relative to expectations). I also include supply chain proxies from the 2016 calendar year which includes firms' total shipments, total number of imported HS codes (6 digits) and their supply chain redundancy (Subsection 3.1). These variables attenuate any selection bias arising from differential sourcing behavior or inventory management practices (Tomlin, 2006). As a proxy for firm size, I include firms' 2016 Q4 total assets and 2016 Q3 inventory levels. I include the former because larger firms may have more resources to deal with unanticipate delays in inventory. For the latter, firms with larger pre-existing inventories may be less impacted by the shock as some of their stored inventory could be used instead of placing redundant orders.

The error ε_i is firm varying and assumed to be distributed independently of Z_i. However, these errors may be correlated across industries due to differences in inventory management and sourcing practices. For example, Rumyantsev and Netessine (2007a) show that there are observable differences in inventory holdings, demand uncertainty and lead times across industries. All of these characteristics can cause correlation in the error term across industries. Therefore, to avoid potential biases in the estimation of standard errors, I estimate standard errors clustered at the industry level.

Finally, I note that by deploying industry fixed effects, we are addressing a host of potential issues of endogeneity resulting from unobserved industry characteristics. First, some industries may have been more (or less) affected due to differences in how much inventory firms hold across industries (Rumyantsev and Netessine, 2007a) and how substitutable their products are. The more inventory they hold and the more substitutable their products, the less likely they would need to place redundant orders. Second, we saw in Figure 1 that the variance in the number of available suppliers across different goods is quite large. Firms in industries where there are few available suppliers may be less impacted because their suppliers' capacity constraints prevented them from placing redundant orders. Lastly, differences in fourth quarter inventory levels may arise due to favorable or unfavorable demand realizations that differ across industries. Thus, industry fixed effects control for situations where impacted firms placed large quantities of redundant orders but due to favorable industry demand realizations, accrued very little excess inventory.

In this model, β is the estimate of the (average) effect of the shock variable on inventory changes between 2015 and 2016. A key assumption in this framework is that the percentage change in inventories of firms do not exhibit differential trends prior to the receivership announcement (parallel trends). In Subsection 5.4, I verify the validity of this assumption by re-estimating Equation 3 with the dependent variable moved back in one year increments (i.e., percentage increase in Q4 inventories between 2013 – 2014 and 2014–2015).

In addition, we saw in Subsection 2.3 that control firms and treatment firms exhibit large differences in sourcing behavior. Besides the inclusion of control variables, another way I address this concern is to create a matched sample of control firms that more closely resembles the treatment group in Subsection 5.3. If estimates from the full and matched samples are similar, then we can be more confident that my estimates are not driven by the large differences in observed shipping and financial covariates. In fact, I find that the point estimates are nearly identical and also estimate smaller standard errors.

The hypothesis that the shock may have caused firms to place redundant orders is corroborated by Table 1. Columns (1) – (3) are estimates corresponding to the full sample of firms. After controlling for fourth quarter sales and sourcing strategies, I find that on relative to the control group, firms in the treatment group had fourth quarter inventory levels grow by 53% between 2015 and 2016. Then, using cargo delays a second source of variation, I find that the impact of a one day increase in $AvgDaysDelayed$ corresponds to a 5% increase in inventory levels that is also statistically significant at the 5% level. For firms that have only one shipment impacted, this would correspond to a 5% increase for each day delayed. Without clustering standard errors, both shock variables are significant at the 1% level.

5.2 Redundant Suppliers

I test if pre-existing supply base redundancies modulated the impact of the shock on firms' inventory levels using interaction effects. I posit that when suppliers have long lead times and are constrained in capacity, firms with low redundancies need to place 'panic' orders with haste if they expect to meet demand forecasts. On the other hand, firms with larger redundancies are afforded the ability to 'wait and see' if Hanjin's receivership problems would self-resolve. This is because larger redundancies implies more suppliers are available to replace trapped inventory later on in a shorter time period.

To test this hypothesis, I construct the following measure for each firm:

$$SupplierRedundancies_i = \frac{1}{|M_i|} \sum_{g \in M_i} UniqueSuppliers_{i,g} \tag{4}$$

where $UniqueSuppliers_{i,g}$ is the number of unique suppliers associated with good g and firm i in the year leading up to the disruption. M_i is the set of firm i' s delayed HS codes. This variable captures the average, pre-existing supplier redundancies for the goods that were delayed and I interact this measure with $AvgDaysDelayed$ and include it in Equation 3.

Columns (4) and (5) in Table 1 report these results. I focus on the sample of impacted firms and find that for each increase in $SupplierRedundancies$, inventories fall by 2%. Moreover, I find that the coefficient on $AvgDaysDelayed \times SupplierRedundancies$ is negative and statistically significant at the 5% level highlighting that the impact of the shock is modulated by the degree of redundancies in the firm's supply base. Specifically, the impact of the shock variable, $AvgDaysDelayed$, decreases by 1% for each increase in $SupplierRedundancies$.

Redundancies in firms' supply bases are often considered a robust strategy that provide benefits to firms under normal circumstances and after a major disruption (Tang, 2006). The estimates in this section suggest however, that supply base redundancies provide an additional benefit: buffers against taking costly hedging actions *during* disruptions.

5.3 Propensity Score Matching

A major concern is the large observed differences between the treatment and control group in Section 2.3. As a robustness check, I re-do the analysis on a matched sample that better resembles the treatment group. If estimates from using the matched sample are similar to those with the full sample, we can have more confidence that my results are not being solely driven by the large differences in financial and shipping covariates.

To do so, I apply the Covariate Balancing Propensity Score (CBPS) method proposed by Imai and Ratkovic (2013). The CBPS procedure is an empirical likelihood method that models treatment assignment based on pre-treatment covariates and minimizes their differences between the treatment and control group. I use this method due to the large observed differences in importing behavior between the treatment and control group (Subsection 2.3) that may not be sufficiently addressed by other propensity score methods. The CBPS procedure also appears to be more robust in terms of balancing covariates and reducing bias compared when compared with other misspecified (logistic) propensity score methods (Wyss et al., 2014).

Table 1: Impact of Shock on Inventory (Full Sample)

	Dependent Variable: Δ % Q4 Inventory				
	(1)	(2)	(3)	(4)	(5)
Dummy (Treatment = 1)	0.53^{**}		-0.99^{**}		
	(0.23)		(0.47)		
Avg. Days Delayed		0.05^{**}	0.09^{**}	0.07^{**}	0.21^{**}
		(0.02)	(0.04)	(0.04)	(0.09)
Supplier Redundancies				-0.02^{**}	0.09^{**}
				(0.01)	(0.04)
Avg. Days Delayed x Supplier Redundancies					-0.01^{**}
					(0.00)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes	Yes	Yes
Observations	1,180	1,180	1,180	174	174

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

I estimate the covariance-balanced propensity score of each firm in my sample with the binary treatment variable and the following pre-treatment covariates: all sourcing proxies from Section 3.1, total shipments, total HS codes, and fourth quarter inventory levels in 2015 (size proxy). I then match each firm in the treatment group with a firm in the control group based on their estimated propensity scores (nearest neighbor). Before matching, the (standardized) mean differences in pre-treatment covariates ranged from -0.10 to 0.75. After matching, these values were all reduced to be within 0.03. Details on the matching procedure and the resulting pre-treatment covariate/CBPS distributions are relegated to the Online Appendix.

Table 2: Impact of Shock on Inventory (Matched Sample)

	Dependent Variable: Δ % Q4 Inventory		
	(1)	(2)	(3)
Dummy (Treatment = 1)	0.58***		−0.92*
	(0.20)		(0.48)
Avg. Days Delayed		0.05***	0.09**
		(0.02)	(0.04)
Sourcing Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Observations	350	350	350

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Table 2 reports estimates for Equation 3 using the matched sample. We can see that the point estimates are very similar to those from the full sample: 58% versus 53% for the dummy shock variable and identical coefficient for $AvgDaysDelayed$. We see however that both these coefficients are now significant at the 1% level rather than at the 5% level with the full sample.

5.4 Placebo Tests

I conduct placebo tests to see if large deviations in fourth quarter inventory levels occur for the treated group in years leading up to the receivership announcement. Finding significant volatile changes in the treatment group could suggest that my results are an artifact of a volatile treatment group. To do so, I leave the treatment variables of $Shock$ and $AvgDelay$ unchanged in Equation 3 but move the dependent and other independent variables backwards in year increments (to remain at the year end). For example, the placebo test for one year prior before would have the dependent variable of $\frac{y_{i,Q4}^{2015} - y_{i,Q4}^{2014}}{y_{i,Q4}^{2014}}$. Sourcing proxies and the other control variables would also be moved back to 2014.

Table 3 presents placebo tests for the two years leading up the receivership announcement. There is no evidence of any differences in inventory increases prior to the receivership announcement. In fact, the average difference between treatment and control groups for 2013–2014 (Column 1) and 2014–2015 (Column 4) are very close to zero. Collectively, these estimates suggest a strong association between the treatment variables and the inventory increases *only* during the year Hanjin announced insolvency.

Table 3: Impact of Shock on Inventory (Placebo Tests)

	Dependent Variable: Δ % Q4 Inventory					
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy (Treatment = 1)	0.00		0.09	0.03		−0.13
	(0.02)		(0.10)	(0.05)		(0.14)
Avg. Days Delayed		−0.00	−0.01		0.00	0.01
		(0.00)	(0.01)		(0.00)	(0.01)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,161	1,161	1,161	1,189	1,189	1,189

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

5.5 Other Outcomes

I examine the scope of the disruption by examining its effects on other outcomes. In particular, I examine the disruption's impact on two other key operational outcomes – sales and cost of goods sold. To do so, I estimate Equation 3 and modify the dependent variable with the percentage increase in Q4 sales or cost of goods sold between 2015 and 2016.

Table 5: Impact of Shock on Sales and COGS (Full Sample)

	Dependent variable:			
	Δ % Q4 Sales		Δ % Q4 COGS	
	(1)	(2)	(3)	(4)
Treatment/Control	0.08		0.02	
	(0.07)		(0.04)	
Avg. Days Delayed		0.01		0.00
		(0.01)		(0.00)
Sourcing Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes	Yes
Observations	1,180	1,180	1,180	1,180

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

These estimates are presented in Table 5. I find that the disruption had no impact on sales which is consistent with the fact that all cargo was received by firms before key holiday sales. I also do not find an impact on cost of goods sold. This is also expected. Even if firms were placing redundant orders at a high markup, this would be not be reflected in the 2016 Q4 balance sheet as these orders went unsold (indicated by the null effect on sales) and would remain as inventory.

5.6 External Validity

At first glance, the underlying supply chain disruption studied in this paper may seem to be a rare event that has limited external validity. Indeed, the last time a major ocean carrier went bankrupt was United States Lines in 1986. However, at heart, this disruption resembles many others in the sense that it subjected firms to a delay in the flow of goods with unknown resolution time. Labour disputes, political and armed conflicts, natural disasters and product contamination can all stall the flow of goods between firms for uncertain lengths. In such situations, firms are confronted with the same dilemma as the treatment group in this paper: Should they adopt a 'wait and see' policy and wait until all uncertainty is resolved? Or, should they make a decision that hedges the possibility of greater loss, but at a cost?

Another issue related to external validity is that the firms in my sample are only public firms. The implications of this are twofold. First, systematic differences in sourcing strategies may exist between public and private firms that can either make the above estimates too conservative or too large. Using the universe of Colombian imports and exports, Kwon (2018) presents evidence that suggests public firms invest more into their supply chains and have higher redundancies. This might stem from public firms being more sensitive to disruptions if there is a chance that the disruption may impact future earnings and stock price. In the case of this paper, the possibility that delayed inventory may lead to depressed sales and lower earnings may have provided further incentives for public firm managers to place panic orders.

6 Impact on Future Sourcing Strategies

I now move on to studying whether the shock impacted future sourcing strategies. Although the primary channel of impact was the delay on cargo, I hypothesize that broader changes to a firm's short-run sourcing patterns are plausible due to the increased cognizance of their supply chain vulnerabilities. My motivations are grounded upon the observation that firms invest little time and resources to managing supply chain risk despite conducting extensive risk-assessments (Rice and Caniato (2003), and Zsidisin et al. (2004)). Experiencing a disruption may cause changes to sourcing strategies as firms update their beliefs on the likelihood and costs of disruptions (Tang, 2006). This also allows firms to perform a more accurate cost/benefit analysis of implementing risk-reduction strategies.

For each 6 digit HS Code, I examine the impact of the shock on the number of redundant suppliers, the number of carriers used and the number of countries the good originates from. To do so, I estimate variants of the following equation using OLS:

$$UniqueVar_{i,g} = \alpha_i + \psi_g + \lambda_t + \beta\, Delay_{i,g,t} + \gamma\, TotalShipments_{i,g} + \varepsilon_{i,g,t} \tag{5}$$

where the dependent variable is the number of unique suppliers, carriers or countries of origin for a 6 digit HS-code g belonging to firm i; α_i, φ_g and λ_t are firm, good and period fixed effects, respectively; $Shock_{i,g,t}$ is the treatment variable; $TotalShipments_{i,g}$ is the total number of shipments for that good for firm i in that period and $\varepsilon_{i,g,t}$ is a random noise term. All regressions are estimated with heteroscedasticity-robust standard errors clustered at the firm level.

I use calendar years as my time intervals and the sample describes all shipments from the beginning of 2014 until the end of 2017. This means that I compare the number of suppliers, say, for HS Code 1234.56 belonging to firm X in 2016 with the number of suppliers in 2014, 2015 and 2017.

The inclusion of the various fixed effects in Equation 5 removes the potentially confounding effects of time-invariant variables that may lead to differential sourcing strategies in response to the shock. These unobserved differences across goods, firms and years can generate biased estimates if we estimate Equation 5 using cross-sectional data excluding fixed effects.

At the firm level, my hypothesis that firms' sourcing behavior can be influenced by past disruptions necessarily implies that previous supply chain disruptions other than the one examined in this paper will affect the dependent variable. Moreover, firm managers can have different levels of risk aversion and respond differently to disruptions. Finally, the firm fixed effect can also capture average differences stemming from variable managerial compensation.

At the good level, some goods may be more difficult than others to search for new suppliers and may not be available in multiple countries (See Section 2.3 and the histograms in Figure 1).

Lastly, year fixed effects capture the influence of possible time trends. Most importantly, these time trends can arise from maritime transportation costs that have been fluctuating widely between

2014 and 2017 (United Nations Conference on Trade and Develop). Including year fixed effects also control for potential changes in trade policy, the value of the dollar, and U.S. household demand that may shape firms' international sourcing behavior.

The estimates for Equation 5 are presented in columns 1–3 in Table 6. I find that a 1 day increase in cargo delay is associated with a 0.18, 0.06 and 0.07 increase in the number of suppliers, carriers and countries. Given that the mean of delays was approximately 16 days with a standard deviation of 5.78, an alternative interpretation of these results could be stated as following in terms of standard deviations, e.g., a 1 standard deviation increase in delays is associated with 1.04 increase in suppliers. Smaller magnitudes are found for the number of countries and carriers associated with the impacted goods. These results are robust to any combination of good and firm fixed effects and if clustered standard errors are replace with White's robust standard errors.

I also test for overall changes to the impacted firms' sourcing strategies. Specifically, I test if impacted firms change sourcing behavior for *all* HS Codes post-disruption. To do so, I replace the *Delay* variable in Equation 5 with an indicator function that equals 1 for goods belonging to firms that were impacted and 0 otherwise. To be precise, shipments that were not involved in the delay, but are being imported by firms in the treatment group, will have values of 1 in the calendar year of 2017.

The results for this specification are found in columns 4–6 in Table 6. In contrast to columns 1–3, I do not find any statistically significant point estimates to reject a zero coefficient. In sum, while the shock had an impact on future sourcing strategies for delayed goods, but I do not find any spill-over effects onto other goods for treated firms.

Tang (2006) summarizes three reasons why firms may perceive serious supply chain risk but fail to take commensurable actions: (i) Inaccurate assessment of risks (ii) Unfamiliarity of risk management strategies (iii) Difficult to perform cost/benefit analysis to justify risk reduction strategies. The evidence from this paper is consistent with (i) and (iii). That is, the disruption likely updated firms' prior beliefs on the likelihood of disruptions and their costs. However, the quick changes to firms' sourcing strategies suggests that firms were aware of what robust strategies to implement.

Due to data limitations, it is unclear if these changes were transient or persistent. Thus, these results only suggest *short-run* effects and follow-up analyses would be required to test for long run changes.

Table 6: Impact on Sourcing Strategies

	Dependent variable:					
	Suppliers	Carriers	Countries	Suppliers	Carriers	Countries
	(1)	(2)	(3)	(4)	(5)	(6)
Delay	0.18***	0.06***	0.07***			
	(0.05)	(0.01)	(0.02)			
Dummy (Treatment = 1)				0.01	0.004	−0.02
				(0.09)	(0.04)	(0.04)
Total Shipments	0.03***	0.004***	0.01***	0.03***	0.004***	0.01***
	(0.01)	(0.001)	(0.001)	(0.01)	(0.001)	(0.001)
Good Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE (Firm Level)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,756	129,756	129,756	129,756	129,756	129,756

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). Columns (1) – (3) correspond to estimates from Equation 5. Columns (4) – (6) correspond to estimates also from 5 but use an indicator variable that equals 1 if good belongs to a firm that is in the treatment group. The dependent variable is the number of unique entity (suppliers, carriers or countries) at the good level.

7 Excess Inventory and Stock Returns

The exogenous increases to firms' inventories provides me a valuable opportunity to examine the relationship between excess inventories and abnormal stock returns. My motivation here is guided by several papers examining the link between firm performance and excess inventory (Chopra and Sodhi, 2004; Naranayan and Raman, 2004), the negative association between excess inventory announcements and stock prices (Hendricks and Singhal, 2009) and the positive association between earnings and responsive inventory management (Rumyantsev and Netessine, 2007b). My primary objective is consistent with the overall theme of this paper: to provide causal evidence.

To study this relationship, I construct Buy and Hold Abnormal Returns (BHAR) and use Two Stage Least Squares (2SLS). I instrument the percent change in fourth quarter inventory levels between 2015 and 2016 with the magnitude of the shock and apply 2SLS. Because none of the firms in my sample made excess inventory announcements, these estimates are not confounded by potential announcement effects (Zhang, 2006) or managerial bias associated with disruption announcements (Schmidt and Raman, 2012). Outside of the increases to firms' inventories, it is also unlikely that cargo delays affected firms' stock prices because the disruption originated from a decision made by a foreign government entity. Collectively, these reasons suggest that the exclusion restriction for my instruments hold.

7.1 Construction of Abnormal Returns

I construct Buy and Hold Abnormal Returns (BHAR) for sample firms at various time horizons beginning Jan 1st, 2017. To do so, I compute abnormal returns using a four-factor model that includes three Fama-French factors (Fama and French, 1993) and a momentum factor (Carhart, 1997). I then compute BHAR's by computing the geometric sum of these abnormal returns:

$$BHAR_{0,t} = \prod_{j=1}^{t} AbReturns_j \tag{6}$$

where t is the horizon in terms of the number of trading days (dropping the i subscript for notational clarity). Details on how I construct BHARs and cumulative abnormal returns (CAR) for my robustness check are provided in the Online Supplement.

7.2 Instrumental Variables Estimation

I estimate the impact of the increase of inventories on abnormal returns in the cross-section for various horizons. The specification is as follows:

$$BHAR_{i,t} = \alpha + \beta InvIncrease_i + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\pi} \boldsymbol{\Sigma}_i + \varepsilon_i \tag{7}$$

where $BHAR_{i,t}$ is the Buy and Hold Abnormal Return for firm i in t trading days; $InvIncrease_i$ is the fourth quarter change in inventories between 2015 and 2016; $\boldsymbol{X}$ are sourcing proxies; $\boldsymbol{\Sigma}_i$ are financial explanatory variables and ε_i is a random noise term. The financial explanatory variables includes the past 3 month averages for log size, trading volume and a momentum term for the firms' return over the past 3 months. These controls are standard asset pricing variables that are well known predictors of the cross-section of firm returns (Lee and Swaminathan, 2000; Fama and French, 1993; Jegadeesh and Titman, 1993).

I estimate Equation 7 using 2SLS. I first instrument $InvIncrease$ with a dummy variable that equals 1 for treatment firms and 0, otherwise. I also instrument $InvIncrease$ with $AvgDaysDelayed$, the average delay in days that each firm in my sample experienced.

7.3 Results and Discussion

My 2SLS estimates presented in Table 7 where the instrument is a dummy variable that equals 1 for treatment and 0 otherwise. Across all time horizons, the point estimates are negative. However, I cannot reject the null hypothesis that the effect of the inventory increases on stock returns is non-zero at the 5% significance level. Thus, these results do not suggest a relationship between excess inventories and abnormal stock returns at any time horizon.

These results hold true under the following robustness checks: (i) Replacing standard errors with robust, or clustered by industry. (ii) Winsorizing BHAR at the (1%,99%) threshold. (iii) Using $AvgDaysDelayed$ as the instrument (Online Appendix). (iv) Replacing BHARs with cumulative abnormal returns (Online Appendix). Moreover, a weak instruments test (F-test of the first stage) does not indicate that these estimates suffer from a weak instruments problem (p-value of 0.065). Finally, directly regressing the shock variables on abnormal returns also produces a null result.

There are several explanations for this null result. Hendricks and Singhal (2009) finds that announcements of excess inventory resulting from the demand side lead to more negative stock returns. This is consistent with the results found in this paper as the cause of excess inventories was due to an exogenous decision from a foreign government and had no impacts on other operational outcomes such as sales. In conjunction, these results suggest that excess inventory may only be detrimental to stock returns if they result from a demand-supply mismatch resulting from weak consumer demand or firm production. Still, another possibility is that investors are particular averse to hearing news of excess inventories (Zhang, 2006) rather than the excess inventories having a major impact on stock returns or other firm outcomes.

8 Conclusion and Future Research

Measuring the causal impacts of supply chain disruptions on firm outcomes is critical for firm managers who wish to optimally invest in their supply chains to mitigate the frequency and magnitude of future disruptions. I address the identification problem resulting from the highly endogenous nature of supply chains and their disruptions by exploiting the variation in cargo delays resulting from Hanjin's receivership announcement in 2016. I identify the distribution of cargo delays by analyzing over 5 million U.S. Customs' bill of lading manifests and linking them with real-time vessel tracking data. I complement industry facts with data-driven arguments to argue that the delays to firms' cargoes were exogenous to firms conditional on observed covariates.

Using regression analysis, I find that this disruption lead to large increases in inventory and changes to impacted firms' future sourcing strategies. I also find that pre-existing redundancies can attenuate the impact of disruptions characterized by uncertainty by affording firms the ability to 'wait and see' until the uncertainty is fully (or partially) resolved. Finally, instrumenting cargo delays for the increases in inventory, I find no evidence that excess inventory levels lead to abnormal stock returns for impacted firms.

Beyond these key findings, I argue that my results offer a new perspective on the mechanisms by which supply chain disruptions can negatively impact firms. Not only are there certain, direct costs associated with supply chain disruptions (e.g., disrupted production due to natural disasters, write offs due to spoiled inventory). But there can also be *indirect* costs resulting from how firms respond to the uncertainty generated by the disruption. The precautionary decisions that firms make under this uncertainty, albeit prudent, can come with significant costs depending on how quickly the uncertainty resolves and the ultimate realization of the disruptions' impact.In the case of this paper, impacted firms hedged against the possibility of lost sales during key holiday periods by placing redundant orders. However, these prudent choices resulted in excess inventories as delayed cargo were all delivered prior to key holiday sales.

The results from this paper are not all consistent with prior work. In particular, neither the disruption nor the excess inventories resulting from the disruption, lead to abnormal stock returns. l also find null effects on sales and cost of goods sold. However, this can easily be explained by the fact that supply chain disruptions are heterogenous and erupt for many different results. But it could also result from this paper being the first to explicitly control for firms' supply chain characteristics that may otherwise lead to selection bias. Therefore, additional plausibly exogenous supply chain disruptions coupled with detailed supply chain data will need to be studied to develop additional insights on the causal associations between supply chain disruptions and firm outcomes.

Table 7: IV Regressions: Buy and Hold Abnormal Returns (BHAR)

	Dependent variable:					
	BHAR 5	BHAR 15	BHAR 30	BHAR 60	BHAR 120	BHAR 200
	(1)	(2)	(3)	(4)	(5)	(6)
Δ % Q4 Inventory	-0.01	-0.02	-0.08	0.01	-0.07	-0.10
	(0.03)	(0.05)	(0.09)	(0.08)	(0.18)	(0.11)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Financial Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,151	1,150	1,150	1,151	1,148	1,142

Notes: 2SLS coefficient estimates associated with Equation 7 and (in parentheses) classic OLS standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). The instrumental variable for the increase in inventory is an indicator variable that equals 1 if the firm is in the treatment group and 0 otherwise. All regressions include as exogenous control variables: redundancy measures constructed in subsection 3.1, total shipments, total HS Codes, momentum, log (Size) and trading volume. The supplemental appendix provides details on how these financial variables are constructed. A test for weak instruments (F-test for first stage regression) yields p-values of approximately 0.065 in each column.

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Online Supplemental Appendix

Robustness to Outliers

Both *AvgDaysDelayed* and *SupplierRedundancies* constructed in Subsection 2.2 and Section 5.2 are computed using the mean of delays and the mean of pre-existing suppliers, respectively. To mitigate the impact of outliers, I replicate the estimates of Table 1 using the median rather than the mean when constructing these two measures.

Table 8: Impact of Shock on Inventory (Full Sample)

	Dependent variable:				
	Δ % Q4 Inventory				
	(1)	(2)	(3)	(4)	(5)
Dummy (Treatment = 1)	0.53^{**}		-0.47^{***}		
	(0.23)		(0.05)		
Med. Days Delayed		0.05^{***}	0.06^{***}	0.07^{**}	0.11^{**}
		(0.01)	(0.01)	(0.04)	(0.05)
Med. Supplier Redundancies				0.00^{**}	0.12^{**}
				(0.00)	(0.05)
Med. Days Delayed x Med. Supplier Redundancies					-0.01^{**}
					(0.00)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes	Yes	Yes
Observations	1,180	1,180	1,180	174	174

Notes: OLS coefficient estimates and (in parentheses) standard errors clustered by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed).

Replacing the mean with the median does not significantly alter the results (Table 8). In fact, we see that Columns (2) and (3) report similar point estimates that are now significant at the 1% level (rather than 5% level using the mean).

Summary Statistics

Table 9: Correlation Table

	$\varphi_{i,t}^{\text{Suppliers}}$	$\varphi_{i,t}^{\text{Countries}}$	$\varphi_{i,t}^{\text{Carriers}}$	HS Codes	Shipments	Inventory	COGS	Sales	Cargo Delay
$\varphi_{i,t}^{\text{Suppliers}}$									
$\varphi_{i,t}^{\text{Countries}}$	0.89***								
$\varphi_{i,t}^{\text{Carriers}}$	0.57***	0.71***							
HS Codes	0.51***	0.52***	0.40***						
Shipments	0.49***	0.53***	0.38***	0.77***					
Inventory	0.01	0.01	0.00	0.02	0.03				
COGS	0.21**	0.15***	0.08**	0.19***	0.21***	0.21***			
Sales	0.11***	0.12***	0.07*	0.19***	0.21***	0.23***	0.96***		
Cargo Delay	0.22***	0.27***	0.26***	0.41***	0.35***	0.05	0.04	0.05	
Treatment Dummy	0.29***	0.35***	0.30***	0.45***	0.34***	0.08	0.07*	0.08*	0.82***

Notes: ***, **, and * indicate statistical significance at the .1%, 1%, and 5% levels. This table reports the correlation table associated with the redundancy measures constructed in Section 3.1. Balance sheet variables are fourth quarter variables at the end of 2016. Shipping statistics are constructed from 2016-01-01 and end at 2016-12-31, i.e., the full calendar year.

Table 10: Delayed Cargo Breakdown

Carrier	Shipments	% Total Shipments
Hanjin Shipping Company Ltd	259	0.22
Expeditors International Of Washington Inc	155	0.13
BDP Tnsport, Inc	84	0.07
Kawasaki Kisen Kaisha Ltd	71	0.06
Danmar Lines Ltd	59	0.05
Cosco Shipping Lines Co Ltd	59	0.05
Ups Asia Group Pte Ltd	42	0.04
Evergreen Line	38	0.03
Yang Ming Marine Tnsport Corp	30	0.03
Blue Anchor America Line (Blue Anchor Line)	25	0.02
Christal Lines	17	0.01
Naca (Vanguard Logistics Services)	16	0.01
Pyramid Lines	16	0.01
Maersk Line	15	0.01
JHJ International Transportation Co Ltd	15	0.01
Other	376	0.31

Notes: This table presents the breakdown of all carriers associated with delayed Hanjin cargo. 'Other' includes carriers with smaller shares and observations with a missing 'carrier' entry. This constitutes about 5% of the delayed cargo.

Table 11: Firm Operational and Financial Summary Statistics

Variable	Status	N	Missing	Mean	SD	p.value
Momentum	0	1163	16	6.78	16.40	0.920
	1	174	2	6.89	14.04	
Book to Market	0	906	273	0.48	0.37	0.167
	1	152	24	0.44	0.34	
Log (Size)	0	1172	7	21.39	2.20	0.001
	1	174	2	21.95	1.95	
Trading Volume	0	1172	7	0.01	0.01	0.344
	1	174	2	0.01	0.01	
Inventory	0	1162	17	745.43	2065.15	0.031
	1	175	1	1148.87	2327.07	
Total Assets	0	1176	3	16608.97	54741.87	0.516
	1	175	1	14513.30	37048.38	
Cost of Goods Sold	0	1176	3	1120.13	2869.63	0.034
	1	175	1	1768.37	3872.47	
Net Income	0	1176	3	114.21	439.36	0.081
	1	175	1	192.24	563.21	
Operating Income	0	1176	3	231.71	620.55	0.167
	1	175	1	315.32	761.32	
Sales	0	1176	3	1782.88	4097.49	0.025
	1	175	1	2744.38	5420.44	

Notes: Summary statistics for financial and operational variables of treatment (status = 1) and control (status = 0) firms. Unless noted, these values are from firms' fourth quarter financial records in 2015. P-values in the last column correspond to a difference of means test between the control and treatment groups.

Table 12: Firm Shipping Summary Statistics

Shipping Statistic	Status	N	Mean	SD	p.value
Shipments	0	1281	637.70	3466.02	< 0.001
	1	185	6532.52	16171.75	
Unique Suppliers	0	1281	27.35	93.46	< 0.001
	1	185	160.12	243.85	
Unique Carriers	0	1281	11.10	15.57	< 0.001
	1	185	33.35	30.35	
Unique Vessels	0	1281	65.90	121.65	< 0.001
	1	185	425.27	321.84	
Unique Ports of Lading	0	1281	5.86	5.03	< 0.001
	1	185	13.28	7.19	
Unique Ports of Unlading	0	1281	10.91	13.42	< 0.001
	1	185	35.97	26.83	
Unique Countries	0	1281	7.83	9.61	< 0.001
	1	185	25.12	18.66	
Unique HS Codes	0	1281	33.94	71.81	< 0.001
	1	185	177.88	187.99	
Average Delay (Days)	0	1281	0.00	0.00	< 0.001
	1	185	15.83	6.86	

Notes: Shipping summary statistics for firms in treatment (status = 1) and control (status = 0). This sample includes shipments for all inbound maritime public U.S. firms between Jan 1st, 2014 and Dec 31st, 2017. Unique variables are computed by summing the number of unique entities for each firm across all goods. (E.g., the unique number of suppliers for firm X is simply the number of all their suppliers). P-values in the last column correspond to a difference of means test between the control and treatment groups.

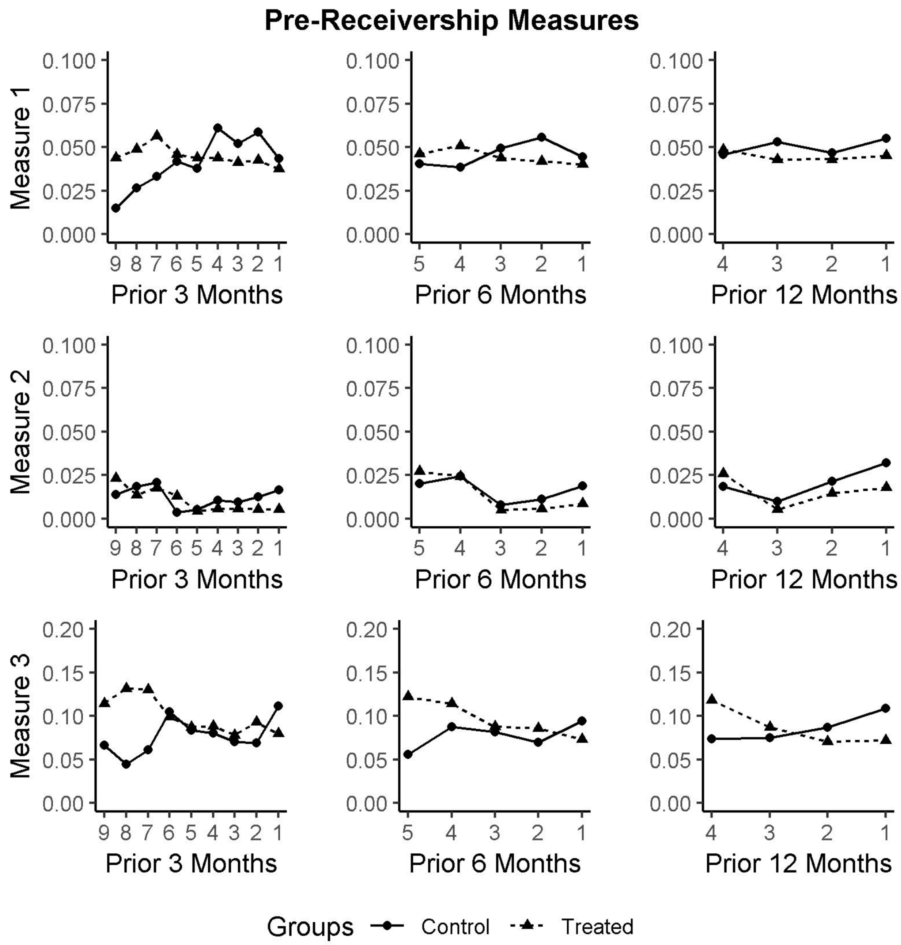


Figure 3: Measure 1 is the average proportion of shipments that were transported on a Hanjin vessel for treatment and control firms. Measure 2 is the average proportion of shipments that were assigned to Hanjin as the handling carrier. Measure 3 is the average proportion of shipments that were assigned to any of Hanjin's alliance members (including Hanjin itself). Treatment group includes public firms that had at least one shipment delayed by Hanjin's receivership announcement. Control group includes public firms that received at least one shipment between the year before and after the receivership announcement.

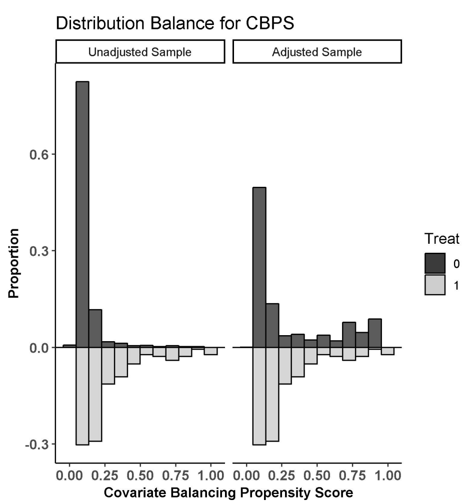
Covariate Balancing Propensity Score

Figure 4 plots the propensity score distribution and covariate balance resulting from the CBPS matching procedure. The full sample is referred to as the "Unadjusted sample" ($n = 1151$) whereas the matched sample is referred to as the "Adjusted sample' ($n = 350$).

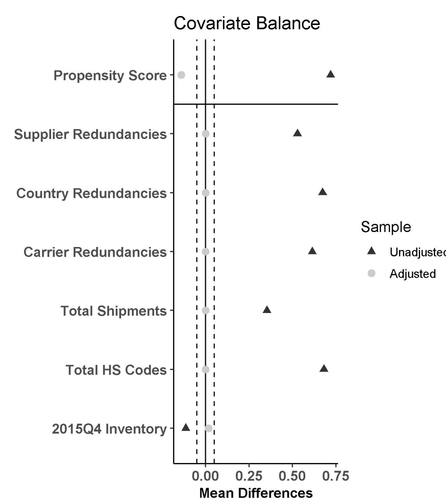
The dual purpose of the CBPS is evident in this figure. Examining panel (a), we see that the treatment and control group exhibits large distributional differences in propensity scores with the full sample. This is likely due to the fact that the firms in the control group source fewer goods and import less frequently (Table 12). In fact, the overwhelming majority of control group firms have a propensity score of less than 0.50. After applying the CBPS, however, these differences are largely attenuated.

We also see that the CBPS does a reasonable job of minimizing the (standardized) mean differences in pre-treatment covariates between the treatment and control group (Panel (b)). The standardized mean difference is computed as:

$$\text{Mean Diff (Standardized)} = \frac{\bar{X}_{treatment} - \bar{X}_{control}}{\sqrt{\frac{1}{2}(s^2_{treatment} + s^2_{control})}} \tag{8}$$



(a) Estimated CBPS Distributions



(b) Standardized Mean Differences in Pre-Treatment Covariates. The dashed line indicates a threshold of 0.05.

Figure 4

Abnormal Returns

To construct Buy and Hold Abnormal Returns (BHAR) and Cumulative Abnormal Returns (CAR), I first estimate the abnormal returns of firms in my sample susing a four–factor model that includes three Fama-French factors (Fama and French, 1993) in addition to a momentum factor (Carhart, 1997). Using OLS, I estimate the coefficients of the following regression:

$$R_{i,t} = \alpha + R_{f,t} + \beta_i(R_{m,t} - R_{f,t}) + \beta_{i,HML} SMB_t + \beta_{i,HML} HML_t + \beta_{i,MOM} MOM_t + U_{i,t} \quad (9)$$

where the dependent variable is stock returns of firm i prior to the disruption; $R_{m,t}$ is the CRSP value weighted market return; SMB_t is the differential return between portfolios of small versus big stocks; HML_t is the differential return between portfolios of low versus high book-to-market values and MOM_t is the momentum factor. The momentum factor is the lagged difference between the weighted average of the highest and lowest performing firms. Including the momentum factor allows me to better isolate the effect of the disruption on stock price performance by accounting for persistence in stock price movements generated by the disruption (if any) and other events.

Pre-disruption returns are obtained from CRSP and all four factors are obtained from Kenneth French's website. I estimate Equation 9 for each firm in my sample beginning 30 trading days prior to Jan 1st, 2017 for a total of 255 trading days. Abnormal returns are simply the fitted values resulting from the estimates of Equation 9:

$$BHAR_{0,t} = \prod_{j=1}^{t} AbReturns_j \qquad CAR_{0,t} = \sum_{j=1}^{t} AbReturns_j \quad (10)$$

where t is the horizon in terms of the number of trading days (dropping the i subscript for notational clarity).

The financial explanatory variables includes the past 3 month averages for log size and trading volume. I also include a momentum term for the firms' past 3 month return. I construct these variables as follows:

- log size: First, I compute firm size by multiplying the closing share price and the number of shares outstanding. I then take the natural logarithm,

- Trading volume: I compute trading volume as the percentage of shares outstanding that is traded on each trading day.

- Momentum term is the simple return of the share in the past three months.

Table 13: IV Regressions

	Dependent variable:					
	BHAR 5	BHAR 15	BHAR 30	BHAR 60	BHAR 120	BHAR 200
	(1)	(2)	(3)	(4)	(5)	(6)
Δ % Q4 Inventory	−0.02	0.06	−0.14	0.01	−0.18	−0.16
	(0.08)	(0.13)	(0.25)	(0.17)	(0.45)	(0.29)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Financial Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,151	1,150	1,150	1,151	1,148	1,142

Notes: IV coefficient estimates associated with Equation 7 and (in parentheses) classic OLS standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). The instrumental variable for the increase in inventory is *AvgDaysDelayed* (Section 2.2). All regressions include as exogenous control variables: redundancy measures constructed in subsection 3.1, total shipments, total HS Codes, momentum, log (Size) and trading volume. A test for weak instruments (F-test for first stage regression) yields p-values of approximately 0.398 in each column.

Table 14: OLS Regressions (BHAR)

	Dependent variable:					
	BHAR 5	BHAR 15	BHAR 30	BHAR 60	BHAR 120	BHAR 200
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy (Treatment = 1)	−0.00	−0.00	−0.01	0.00	−0.01	−0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Financial Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,151	1,150	1,150	1,151	1,148	1,142

Notes: OLS estimates where BHARs are directly regressed onto a dummy variable that equals 1 or treatment and 0 otherwise. Classic OLS standard errors are (in parentheses). ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). All regressions include as exogenous control variables: redundancy measures constructed in subsection 3.1, total shipments, total HS Codes, momentum, log (Size) and trading volume.

Table 15: IV Regressions: Cumulative Abnormal Returns (CAR)

	Dependent variable:					
	CAR 5	CAR 15	CAR 30	CAR 60	CAR 120	CAR 200
	(1)	(2)	(3)	(4)	(5)	(6)
Δ % Q4 Inventory	−0.01	−0.01	−0.08	0.03	−0.02	−0.05
	(0.03)	(0.05)	(0.10)	(0.14)	(0.15)	(0.15)
Sourcing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Financial Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,151	1,150	1,150	1,151	1,148	1,142

Notes: IV coefficient estimates associated with Equation 7 and (in parentheses) classic OLS standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels (two-tailed). The instrumental variable for the increase in inventory is a dummy variable that equals 1 for treatment and 0 otherwise. All regressions include as exogenous control variables: redundancy measures constructed in subsection 3.1, total shipments, total HS Codes, momentum, log (Size) and trading volume. A test for weak instruments (F-test for first stage regression) yields p-values of approximately 0.073 in each column.