The Effect of the U.S.-China Trade War on U.S. Investment*

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Abstract

We develop a new method of quantifying the impact of policy announcements on investment rates that makes use of stock market data. By estimating the effect of U.S.-China tariff announcements on aggregate returns and the differential returns of firms exposed to China, we identify their effect on treated and untreated firms. We show theoretically and empirically that estimates of policy-induced stock-market declines imply lower returns to capital, which lowers investment rates. We estimate that the tariff actions through 2018 and 2019 will lower the investment growth rate of listed U.S. companies by 1.9 percentage points by the end of 2020.

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1 Introduction

What was the impact of the U.S.-China trade war on aggregate investment? A major challenge in addressing this question is that standard methodologies such as differences-in-differences regressions can only identify the differential effect of a policy announcement on the treated firms relative to the untreated. We overcome this limitation by developing a new methodology that enables us to estimate (rather than calibrate) the effect of an event on treated and untreated firms. Our contribution is to show that by using a variant of a stock-market event study, one can identify the impact of a policy announcement on aggregate stock market returns *and* the impact on the returns of firms that are differentially exposed to the announcement. We use these two estimates to exactly decompose an event's impact into two components: the "common effect" (how an event affected returns by changing factors or variables that matter for firms in general) and the "differential effect" (the additional effect on treated firms). We show theoretically and empirically that estimates of policy-induced stock-market declines cause lower returns to capital and reduce aggregate investment rates.

We apply this methodology to the data using U.S.-China trade war announcements in 2018 and 2019. As has often been noted, U.S.-China trade-war announcements have been associated with large stock-price declines. In our baseline specification, we find that U.S. and Chinese tariff announcements lowered U.S. aggregate equity prices in our sample of approximately 3,000 firms in the COMPUSTAT database by 6.0 percentage points. Given that our sample of firms had a market capitalization of $28 trillion, the aggregate lost firm value equals $1.7 trillion. We also show that while the trade war depressed equity values by lowering the returns of firms exposed to China, it had even larger negative effects by lowering the returns of firms more broadly. Of the 6.0 percent drop in equity prices, 3.4 percentage points can be attributed to the common effects and 2.6 percentage points can be attributed to the differentially poor performance of firms importing from, exporting to, or selling in China.

Our new methodology draws on the natural relationship between stock prices and market-to-book (MTB) values to estimate the aggregate investment effect. In the q theory of investment, which forms the basis of our link between firm values and investment, a firm's MTB value equals its expected return on capital. Thus, the lower stock prices arising from the U.S.-China trade war should reflect lower returns to capital (as measured by MTB values), which lowers the incentive to invest. We estimate that U.S.-China tariff announcements lowered the 4-quarter growth rates of listed firms by 0.3 percentage points in the fourth quarter of 2019 and will lower it by another 1.6 percentage points in the fourth quarter of 2020, resulting in a total decline of 1.9 percent.

Our decomposition builds on the work of Gabaix and Koijen (2019) and the factor-model approach of Bai and Ng (2002). These models posit that one can decompose firms' stock returns into movements related to a set of "common" factors that matter in general and an idiosyncratic firm-specific time-varying residual. Thus, any determinant of returns that matters in a non-trivial fraction of observations—e.g., shifts in macro variables like changes in expected growth rates—should be captured by one or more common factors, with the total number of estimated factors determined by some selection criterion. Importantly, these common factors can have heterogeneous effects on firms (captured by the factor loadings), so not all firms necessarily respond the same way to a change in expected economic conditions. Thus, a factor model should capture the impact of any model in which some set of variables systematically moves stock returns throughout the sample.

We show that any event study can be nested inside a factor model. An event study examines factors or variables that are not common—they do not matter in general, but they do matter in a short time period called an event window. In other words, a stock-market event study is an analysis of the residual of a factor model. Indeed, the differences-in-differences approach can be thought of as a means of using additional identifying assumptions in order to estimate the effects of a set of non-common factors. For example, the assumption that we know when the event-specific factor is relevant and when it is not, is tantamount to assuming that we do not need to estimate its value—we assume it can be proxied by an event indicator variable that equals one during the event window and zero otherwise. Similarly, the assumption that the impact of the event-specific factor can be captured by a constant times an observable treatment variable is isomorphic to assuming that the factor loadings cannot vary arbitrarily by firm but also must be proportional to the treatment variable. These assumptions dramatically reduce the number of parameters to be estimated but do not alter the fact that an event study can be written as a factor model in which a time-varying factor (the event indicator) has different effects on firms during an event window.

Since an event study can be nested into a factor model and factor models can be used to exactly decompose a dataset, we can decompose stock returns in an event window into the component explainable by the common effect, differential effect, and an idiosyncratic term. Importantly, we define the common effect to be the implied movement in equities due to the factor model plus a day fixed effect from the event study. This procedure enables us to allow for the possibility that in an event window, past relationships between returns and factors might break down, and an event-specific factor might move the returns of all firms for some other reason.

Our choice of which treatment variables to include in our event study is conventional. We follow Huang et al. (2019) in our selection of the observable exposure (or treatment) variables that matter when doing a stock-market event study of the U.S.-China trade war: whether the firm exports to or imports from China as well as the fraction of its revenues that it obtains from the Chinese market. This sales channel also turns out to be an important reason why the trade war hurt U.S. firms, perhaps reflecting the fact that U.S. tariffs not only likely provoked Chinese tariff and non-tariff retaliation, but also hurt U.S. multinationals by slowing their sales growth in China.[1]

One notable difference with other stock-market event studies that have focused on the trade war is that we use a systematic method to identify key trade war events to avoid potential biases arising from choosing events on an *ad hoc* basis. We argue that days

[1]While the U.S. only exported \$130 billion to China in 2017, sales by U.S. multinationals amounted to \$376 billion. Indeed, although the large bilateral deficit in 2017 was driven by the fact U.S. exports to China were only a quarter as large as Chinese exports to the U.S, total sales (exports plus multinational sales) by U.S. firms in China were only 11 percent lower than total sales by Chinese firms in the U.S. market. Sales in China by U.S. firms were \$505 billion and sales in the U.S. by Chinese firms were \$570 billion. Source: Bureau of Economic Analysis, U.S. Census.

in which significant information about the trade war was released are likely to be days in which there are likely to be peaks in the number of Google searches for the phrase "trade war." We use this method to identify eleven events between January 1, 2018 and December 31, 2019, which based on a reading of the newspapers on these days, can be further broken down into three subsets: four U.S. tariff announcements, three Chinese tariff announcements, and four other trade-war announcements. This last category contains announcements—e.g., tariff retaliation announcements by Mexico and the European Union—that are not directly related to bilateral U.S.-China tariffs. Having this last category of "other events" enables us to conduct a placebo test of whether firms exposed to China suffered negative returns when other countries retaliated against the U.S. or whether there was something specific about U.S. and Chinese tariff announcements that negatively affected these firms. Consistent with our identifying assumption, other trade-war announcements have no differential effect on firms exposed to China, but do have large, negative common effect that lowers returns for firms regardless of their China exposure.

We examine the impact of these trade war announcements using event windows extending from one to thirty trading days after the announcements. Cumulating the gains and losses across all seven U.S.-China tariff events, we find that U.S. equity prices fell 9.7 percent using a conventional 3-day event windows (the trading day before, the trading day of, and the trading day after an announcement). Stock prices partially rebounded, however, recovering close to half of their lost value—down only 4.3 percent—when we use 7-day windows starting the day before each event and extending five days afterwards. These results are consistent with models of stock-market overshooting in response to surprising news (e.g., De Bondt and Thaler (1985)). Our estimates of the total impact of the general and differential effects of tariff announcements on equity prices are fairly stable for a variety of window lengths: ranging from -6.0 percent in our benchmark specification based on a 7-day window to -8.3 percent when we use 3-day windows. Similarly, our estimate of the differential effect of the trade war on stock prices also does not vary much as we change the window length.

Interestingly, we estimate that most of the reduction in U.S. market value arose from *U.S.* tariff announcements; Chinese retaliation announcements mostly had small effects on U.S. stock prices. There are a number of plausible explanations for this result. First, markets may have viewed the surprising piece of information associated with the trade war was the U.S. decision to apply tariffs. Second, the value of U.S. exports to China was much lower than the value of Chinese exports to the U.S, so China's ability to retaliate was more limited. Third, Chinese retaliation may have been priced into the reactions to U.S. tariff announcements.

The last part of the paper embeds our estimates of the policy-induced reductions in firm market value into a model of investment. We show that reductions in share prices due to trade war announcements significantly lower firm-level investment rates four quarters later. Most of the 2019 effect is driven by the impact of tariffs on U.S. firms doing business with China, but the 2020 effects are driven more by the fact that tariff announcements drove down returns of firms regardless of their exposure to China, and this information lowered investment incentives. Since the baseline specifications employ conservative estimates of the impacts of the tariffs, alternative specifications tend to produce more negative effects on investment.

Our paper builds on a number of literatures. A large literature has developed that aims to measure the costs of protection using models in which adjustment is costless. For example, Amiti et al. (2019) and Fajgelbaum et al. (2019) estimate the costs of the trade war in conventional trade models. Caliendo and Parro (2015) estimate the gains from the North American Free Trade Agreement, and Ossa (2014) estimates the costs of a hypothetical trade war. Similarly, Baldwin and Forslid (1999) and Baldwin and Forslid (2000) develop a q-theory model of trade with *costless* adjustment, so firms can instantly adjust their capital stocks in response to any policy-induced change in prices. Our work differs in that we assume that there may be adjustment costs associated with a firm increasing or decreasing its capital stock, estimate these costs, and show that these matter for investment as is standard in the finance literature.

Our paper is also related to papers that use stock market event studies to evaluate the impact of trade on firms in a specific factors setting in which adjustment costs are infinitely high (c.f., Grossman and Levinsohn (1989), Fisman et al. (2014), Egger and Zhu (2019), Huang et al. (2019), Bianconi et al. (2019), and Greenland et al. (2019)). These models share the feature that trade-induced price changes affect firm value. We build on this insights by showing how one can nest an event-study setup into a factor model to not only identify a trade shock's differential effect on exposed firms, but also the aggregate effect. Moreover, rather than assuming that adjustment costs are infinite, we show how we can use these estimates to identify the key adjustment-cost parameter and then embed the estimates into a structural investment equation to estimate the impact of the shocks.

We are also related to studies of trade policy uncertainty. Important papers in this literature include Caldara et al. (2019), Handley and Limão (2015, 2017), Pierce and Schott (2016), Feng et al. (2017), Crowley et al. (2018), Crowley et al. (2019), and Steinberg (2019). We differ from all of these papers in that we decompose stock market returns in response to trade announcements to identify the differential and general impacts of tariff announcements (as opposed to trade-policy uncertainty) on firms and embed these estimates in a q-theory of investment with costly adjustment.

We are also related to a number of important papers in macro (c.f., Nakamura and Steinsson (2018) and Wolf (2019) that have used high frequency data and model-imposed restrictions to identify general equilibrium effects. Although our paper looks at a very different set of policies and mechanisms, we are similar in that we seek to identify common effects by using high-frequency data and explore the links between differences-in-differences estimators and aggregate effects.

Finally, our paper also builds off of the large finance literature that has developed regarding the estimation of q-theory models of investment. In particular, we use Hayashi (1982)'s insight about conditions when marginal and average q are the same, and employ the approach of Frank and Shen (2016) to construct our measure of MTB values. We also make use of results derived in Abel and Panageas (2020) to understand potential measurement error biases in investment regressions as well as work by Peters and Taylor (2017), Erickson and Whited (2012), and Erickson et al. (2014) that have developed sophisticated econometric techniques to estimate adjustment costs.

The structure of the paper is as follows. Section 2 presents the theory. We show how to map an event study into a factor model in Section 2.1 and present the decomposition of stock-market returns into the common effect and and the differential effect in Section 2.2. Section 2.3 shows how these estimates can be embedded in the q-theory model of investment. Section 3 discusses the data we use. Section 4 presents the results, and we conclude in Section 5.

2 Theory

Our theory will proceed in two steps. In sections 2.1 and 2.2, we show how one can use stock market data to exactly decompose aggregate returns into those caused by common and differential effects. In Section 2.3, we modify the standard q theory of investment so that we can write it in a form suitable for understanding how trade shocks affect investment in a model where capital adjustment is costly. We use the decomposition obtained in Section 2.2 to construct an instrumental variable that enables us to estimate the impact of trade shocks on investment and then use these estimates to predict the impact of the tariff announcements on investment.

2.1 Event Studies and Stock Market Returns

We define r_{ft} as the percentage change in the firm's share price on *day* t We ignore all days in which markets are closed from the sample, so that day $t+1$ is the *trading* day after day t. We assume that the percentage change in a firm's share price can be written as a function of $K \geq 1$ unspecified common factors (δ_{kt}) and the idiosyncratic or abnormal return (ϵ_{ft}):

$$r_{ft} = \alpha_f + \sum_{k=1}^{K} \beta_{kf}\delta_{kt} + \epsilon_{ft}, \tag{1}$$

where α_f is the underlying rate of return for the firm, and β_{kf} is the loading on the factor k. An important feature of factor models is that the factors (δ_{kt}) do not have a firm subscript, so these factors capture the impact of *common* (i.e., not firm-specific) information released on day t. Thus, while a factor (δ_{kt}) may affect firms differently (if $\beta_{kf} \neq \beta_{kf'}$ for some f and f'), it may nonetheless be common in the sense that it matters in general.[2] All firm-specific information is therefore embodied in the error term, ϵ_{ft}.

This approach nests a number of popular ways of describing stock markets. For example, the CAPM is a special case in which the econometrician makes a number of identifying assumptions: $K = 2$, δ_{1t} equals the risk-free rate, and δ_{2t} equals a stock-market index (e.g., the S&P 500). Similarly, the Fama-French factor models can also be thought of as special cases in which additional factors are assumed to be equal to certain variables (e.g., the differential returns of small firms relative to large firms). We will work with this more general setup to avoid making the results dependent on a particular model of

[2]Following Bai and Ng (2002) and the factor model literature, a necessary condition for a factor to be common is that $\lim_{T\to\infty} \frac{1}{T}\sum_{t=1}^{T}\left(\delta_{kt} - \overline{\delta_k}\right)^2 > 0$. This condition will be violated for any factor that is only non-zero for a finite number of days (e.g., only during an event window), since the limit will be zero. Bai and Ng (2002) also formalize a second condition on the factor loadings (β_{kf}) that ensures that the factors contribute to a non-trivial share of the variance. When we say a factor is "common" or matters "in general," we assume that it satisfies these two conditions.

asset pricing. The decision about which factors are relevant can be determined by estimating equation (1) over some horizon and choosing a set of relevant factors (variables) that satisfy some selection criterion.

An event study can be thought of as a situation in which the econometrician has some extra information about some factor or variable that is not relevant in general (as defined in footnote 2), but does matter during some finite "event window". Thus, if we were to include it in equation (1), the extra factor would not satisfy the selection criterion, but we still might have prior information that the factor indeed matters on certain days. In our study, we consider events that involve U.S. or Chinese tariff events. We define the set of U.S. events as Ω^U, the set of Chinese events as Ω^C and the set of U.S. and Chinese events as $\Omega^{UC} = \Omega^U \bigcup \Omega^C$. We assume that information about this event may start affecting returns up to one day before event j and up to w days after the event. In this case, our event window can be defined as $t \in [j-1, j+w]$, where $w+2$ is the length of the event window. We define D^w_{jt} to be an indicator variable that is one if day t falls within the event window for event j and zero otherwise. During the event window, we assume that there is a set of treatment variables Z_{if} ($i \in \{1, ..., N\}$) that identify whether a firm is an importer from China, an exporter to China, or the share of its revenues that accrues from China. We can rewrite the error term in equation (1) as

$$\epsilon_{ft} = \theta_t D_t + \sum_{j \in \Omega^{UC}} \sum_{i=1}^{N} \gamma_{ij} Z_{if} D^w_{jt} + \nu_{ft} \quad \forall j, t \text{ s.t. } \sum_{j \in \Omega^{UC}} D^w_{jt} > 0, \tag{2}$$

where θ_t is a parameter that captures how events on day t moved the market differently from how the factor model would have predicted; γ_{ij} is a parameter that indicates how much treatment variable Z_{if} affected the abnormal return of firm f during event j; and ν_{ft} is an i.i.d. error that is distributed $N(0, \sigma)$. An interesting feature of the structure of a standard event study (equation (2)) is that it is isomorphic with that of the factor model given in equation (1) with some additional identifying restrictions. For example, the first factor is θ_t and we assume its factor loading is one. Similarly, we assume we know that the value of the next N factors equal the indicator variables D^w_{jt} and that their factor loadings are constrained to be proportional to the treatment variables (i.e., $\sum_{i=1}^{N} \gamma_{ij} Z_{if}$). Nevertheless, the structure is the same.

In order to estimate all the event-study parameters, we use the standard two-step procedure—we estimate the factor model in equation (1), and then we use the estimated abnormal returns ($\hat{\epsilon}_{ft}$) as the dependent variable when estimating (2). An important feature of our approach is that we do not pool across events. Thus, if event j contains important information about stock returns and event j' does not, we will have $E(\hat{\gamma}_{ij}) \neq 0$ and $E(\hat{\gamma}_{ij'}) = 0$. As we will see below, this feature of the estimation procedure will ensure that our decomposition and investment results are not biased even if we mistakenly include events that do not matter for our event study.

2.2 Decomposing Returns

Our next task is to understand how an event affects firms irrespective of their relative exposure to China. After estimating equations (1) and (2), we can decompose the firm's rate of return on any day *within an event window* as

$$r_{ft} = \hat{\alpha}_f + r_{ft}^C + r_{ft}^D + \hat{\nu}_{ft}, \tag{3}$$

where

$$r_{ft}^C \equiv \sum_{k=1}^{K} \hat{\beta}_{kf}\hat{\delta}_{kt} + \hat{\theta}_t \quad \text{and} \quad r_{ft}^D \equiv \sum_{j\in\Omega^{UC}} \sum_{i=1}^{N} \hat{\gamma}_{ij} Z_{if} D_{jt}^w. \tag{4}$$

r_{ft}^C captures how information released on day t *within an event window* affects the return of a firm through two channels: the amount that the factor model would have predicted ($\sum_{k=1}^K \hat{\beta}_{kf}\hat{\delta}_{kt}$) based on movements of common factors, and the average deviation from this amount ($\hat{\theta}_t$). This last term captures the impact of any factor that is not common in equation 1 but is relevant during the event window affecting all firms. Implicitly, we are assuming that within the event window, movements in the common factors are determined by the event. The variable r_{ft}^D captures the differential impact of treatment during the event window. Finally, we assume that outside of the event window returns (r_{ft}) are determined by the factor model given in equation (1) instead of (3), so we set $r_{ft}^C = r_{ft}^D = \hat{\nu}_{ft} = 0$ outside of the event window

We can obtain intuition for how this decomposition works by considering a few examples. If policy uncertainty rose on an announcement date t, and the role of policy uncertainty is captured by some factor (δ_{kt}), then $\hat{\beta}_{kf}\hat{\delta}_{kt}$ would capture how much this rise in policy uncertainty should affect firm f's return. On the other hand, if policy uncertainty is not a variable that matters much outside the event window (so no factor in the factor model captures its role) but then starts to matter in the event window, $\hat{\theta}_t$ would be our estimate of how this event-specific factor moves returns in general. Finally, if policy uncertainty does not matter outside the event window and only affects firms that are directly exposed to China in some way, it will be captured by r_{ft}^D. Obviously, these are stark examples, and in practice one should expect all three channels to capture some of the variation. However, to the extent that a firm's stock movement can be explained by some general relationship ($\sum_{k=1}^K \hat{\beta}_{kf}\hat{\delta}_{kt}$) or some event-specific factor that moved all firm returns ($\hat{\theta}_t$), it will be captured by r_{ft}^C. On the other hand, differences from this prediction that are correlated with some treatment variable (Z_{if}) during the event window will be captured by r_{ft}^D.

We now use equations (3) and (4) to decompose aggregate market returns. The market return on a day (R_t) is the market-capitalization weighted average of the return of each individual stock, i.e., $R_t \equiv \sum_f S_{f,t-1} r_{ft}$, where $S_{f,t-1}$ is the share of total market capitalization in period $t-1$ accounted for by firm f, and so $\sum_f S_{f,t-1} = 1$. Substituting equation (4) into equation (3), multiplying both sides by $S_{f,t-1}$, and summing across all firms yields

$$R_t = \underbrace{\sum_f S_{f,t-1}\hat{\alpha}_f}_{R_t^\alpha} + \underbrace{\sum_f S_{f,t-1} r_{ft}^C}_{R_t^C} + \underbrace{\sum_f S_{f,t-1} r_{ft}^D}_{R_t^D} + \underbrace{\sum_f S_{f,t-1}\hat{\nu}_{ft}}_{R_t^I}, \tag{5}$$

where R_t^α is the typical daily return of the market in the absence of any new information; R_t^C captures market movements due to factors that matter in general or moved all returns in general; R_t^D captures how the differential returns of firms exposed to China moved aggregate returns on day t; and R_t^I is the idiosyncratic component of stock market returns. The amount the announcement moved the market in aggregate is therefore given by $\left(R_t^C + R_t^D\right)$. If we want to see how an event j affected market abnormal returns over some event window of length $w+2$, we simply sum across the days in the window to obtain:

$$R_j(w) \equiv \sum_{\ell=-1}^{w} R_{j+\ell} \text{ and } R_j^X(w) \equiv \sum_{\ell=-1}^{w} R_{j+\ell}^X \text{ for } X \in \{\alpha, C, D, I\}. \tag{6}$$

In this decomposition $R_j^\alpha(w)$ captures the the stock-market movement that we should have expected during the event window due to the underlying upward drift in stocks; $R_j^C(w)$ is the impact of the common effect on aggregate stock-market returns; $R_j^D(w)$ captures how the differential returns of treated firms affected aggregate stock-price movements; and $R_j^I(w)$ is a residual that captures the impact of idiosyncratic firm-specific abnormal returns that cannot be explained by any common factors or treatment variable during the event. As we show in the appendix, this decomposition can also be amended for cases in which we want to compute the impacts of different classes of events (e.g., U.S. and Chinese tariff announcements).

2.3 A q-Theory of Investment

As we show in the online appendix, the q theory of investment can be used to derive the following equation linking a firm's investment level in quarter s (I_{fs}) relative to its initial capital stock (K_{fs}) to a firm-specific depreciation rate (ρ_f), the cost of investment goods (p_s), the shadow value of return to capital (q_{fs}), and a parameter that governs the magnitude of capital adjustment costs (ψ):

$$\frac{I_{fs}}{K_{fs}} = \rho_f - \frac{p_s}{\psi} + \frac{q_{fs}}{\psi}. \tag{7}$$

If $\psi = 0$, we are in a frictionless world in which capital stocks adjust instantly to the optimal level, so the shadow value of capital equals the cost of investment of goods (i.e., $q_{fs} = p_s$). In contrast, if ψ is infinite, firms are unable to adjust their capital stocks (as in the specific-factors model), and movements in the returns to capital will not affect investment. In general, we expect that $0 < \psi < \infty$. Thus, as long as we are not in a world of zero or infinite adjustment costs, a reduction in the shadow value of capital (q_{fs}) will also reduce the optimal investment rate.

We next make use of the Hayashi (1982) result that if the capital adjustment cost is homogeneous of degree one in investment and capital[3] and production is constant returns to scale, we can write $V_{fs}/K_{fs} = q_{fs} + \psi\chi_{fs}$, where V_{fs}/K_{fs} is the market-to-book (MTB) ratio and χ_{fs} is a term related to measurement error. Making this substitution yields the following investment equation:

$$\frac{I_{fs}}{K_{fs}} = \rho_f - \frac{p_s}{\psi} + \psi^{-1}\frac{V_{fs}}{K_{fs}} - \chi_{fs}. \tag{8}$$

[3]This condition is satisfied in our derivation of equation (7) in the online appendix.

It is straightforward to think about how an announcement will affect investment in this setup. First, If we totally differentiate equation (8), we obtain

$$dI_{fs} = \frac{I_{fs}}{K_{fs}} dK_{fs} - \frac{dp_s}{\psi} K_{fs} + \psi^{-1} K_{fs} d\frac{V_{fs}}{K_{fs}} - K_{fs} d\chi_{fs}, \tag{9}$$

where the third term on the right hand side tells us how much an event affects the firm's investment growth rate, through its effect on MTB. Second, we assume the impact of an event on MTB value is proportional to its impact on share prices. It will prove useful to measure these impacts by converting the key stock market variables in equation (3) from the daily frequency to the quarterly frequency as follows:

$$\bar{X}_{fs} \equiv \sum_{t=F(s)}^{L(s)} X_{ft}, \text{ for } X \in \left\{ r_{ft,} r_{ft}^C, r_{ft}^D, \hat{\nu}_{ft}, \hat{\epsilon}_{ft} \right\}, \tag{10}$$

where $F(s)$ is the first day in quarter s and $L(s)$ is the last day. We can write the movement in MTB due to share price changes as

$$d\frac{V_{fs}}{K_{fs}} = \lambda_C \bar{r}_{fs}^C + \lambda_D \bar{r}_{fs}^D + \lambda_\nu \bar{\nu}_{fs} + \lambda_r \left(\bar{r}_{fs} - \bar{r}_{fs}^C - \bar{r}_{fs}^D - \bar{\nu}_{fs} \right), \tag{11}$$

where λ_C, and λ_D are parameters that capture how share price movements due to the common and differential effects moved MTB values during the event window; λ_ν informs us about how idiosyncratic share-price movements map into changes in MTB values; and the λ_r terms tell us how share price movements affect MTB values outside of the event window.[4]

If all of these λ's were equal, then simply knowing the magnitude of a share-price movement would be a sufficient statistic for how much a policy induced movement in share prices affects MTB values. However, they need not be equal. For example, a movement in share prices due to an idiosyncratic piece of information about a firm is likely to only move a firm's market value because the firm's share price changed. However, a movement in share prices due the common effect ($\bar{r}_{fs}^C$) might not only affect the MTB value of a firm because its share price changed, but also because it caused changes in the values of other assets owned by the firm.

We can use equations (9) and (11) to express the impact of an event on investment rates:

$$dI_{fs}^E \equiv \psi^{-1} K_{fs} \left(\lambda_C \bar{r}_{fs}^C + \lambda_D \bar{r}_{fs}^D \right) \tag{12}$$

Summing this expression across all firms and dividing both sides by aggregate investment (I_s) gives an expression for how the event affected aggregate investment through each of these channels:

[4]Note that in general, $\bar{r}_{fs} - \bar{r}_{fs}^C - \bar{r}_{fs}^D - \bar{\nu}_{fs} \neq \alpha_f \left(\sum_{\ell=s-3}^{s} T_s - \sum_{\ell=s-7}^{s-4} T_s \right)$ because $\bar{r}_{fs}^C$, $\bar{r}_{fs}^D$, and $\bar{\nu}_{fs}$ are only non-zero during an event window. Since $\alpha_f \approx 0$ and there is very little variation in the number of trading days between any two 4-quarter periods, we will assume that $\alpha_f \left(\sum_{\ell=s-3}^{s} T_s - \sum_{\ell=s-7}^{s-4} T_s \right) = 0$ to avoid notational clutter. In practice, our estimates are only trivially affected by the inclusion of this term. As long as some days in a quarter are not in an event window, we will have $\bar{r}_{fs} \neq \bar{r}_{fs}^C - \bar{r}_{fs}^D - \bar{\nu}_{fs}$.

$$\frac{dI_s^E}{I_s} \equiv \frac{\psi^{-1}\lambda_C \sum_f \bar{r}_{fs}^C K_{fs}}{I_s} + \frac{\psi^{-1}\lambda_D \sum_f \bar{r}_{fs}^D K_{fs}}{I_s} \tag{13}$$

2.3.1 Estimating Equation

In order to implement this equation, we need to obtain estimates for ψ^{-1}, λ_C, and λ_D. We will explore a number of ways of estimating the impact of MTB value on investment (ψ^{-1}). The simplest is OLS estimation of equation (8). In order to take equation (8) to the data, we need to be precise about when a period begins and ends. Usually, this equation is estimated with annual data, so the relevant MTB value (V_{fs}/K_{fs}) one year earlier is used to explain the investment that occurs in the following year. In order to be consistent with this work, we need to lag the MTB values by four quarters, so that our estimates reflect how MTB values four quarters ago affect current investment.[5] We therefore rewrite our empirical implementation of equation (8) as

$$\frac{I_{fs}}{K_{f,s-4}} = \rho_f - \frac{p_s}{\psi} + \psi^{-1}\frac{V_{fs-4}}{K_{fs-4}} - \chi_{fs}. \tag{14}$$

A large literature has developed in finance that concerns possible biases in estimates of ψ^{-1} that might arise from the estimation of equation (14). Abel and Panageas (2020) show that in the presence of financial constraints, estimates of ψ^{-1} will be biased upwards due to non-classical measurement error, but they will be biased downwards if one also includes a cash-flow control variable in the regression. Thus, the true value is partially identified and should lie between these bounds. We will make use of this result and consider the sensitivity of our results to the presence of non-classical measurement error.

Second, it is possible that $\text{cov}\left(\frac{V_{f,s-4}}{K_{f,s-4}}, \chi_{fs}\right) \neq 0$ due to classical measurement error or for some other reason. In order to deal with this potential problem, we need an instrument that is correlated with MTB values but not correlated with measurement error or shifts in tax treatment of past investments. One plausible instrument set comprises the various components of abnormal returns due to exposure to China. However, since abnormal returns are expressed in percent changes, the impact of protection on firm stock prices is more likely to explain changes in MTB values than levels, so in order to use this instrument set, we take 4-quarter differences of equation (14):

$$\Delta^4\left(\frac{I_{fs}}{K_{fs}}\right) = \Delta^4\frac{p_s}{\psi} + \psi^{-1}\Delta^4\left(\frac{V_{fs-4}}{K_{fs-4}}\right) - \Delta^4\chi_{fs}, \tag{15}$$

where Δ^4 is a 4-quarter difference operator, and $\Delta^4\frac{p_s}{\psi}$ is a term that we can estimate by using a time fixed effect. Our instrument set will therefore be

$$\tilde{X}_{fs} \equiv \sum_{\ell=s-3}^{s} \bar{X}_{f\ell} \quad \text{for} \quad X \in \left\{r, r^C, r^D, \nu\right\} \tag{16}$$

These variables can be used as instruments for $\Delta^4\left(\frac{V_{f,s-4}}{K_{f,s-4}}\right)$, and the fact that we have more than one instrument for this variable lets us use overidentification tests to verify their validity.

[5] This also helps avoid issues with seasonality.

We construct an instrument that is correlated with movements in MTB values between eight and four quarters before period s by leveraging our estimates of how protection and idiosyncratic returns affected daily returns. These variables can be used as instruments for $\Delta^4\left(\frac{V_{f,s-4}}{K_{f,s-4}}\right)$ under the assumption that policy announcements that affect share prices affect MTB values but do not have an independent effect on investment. Similarly, idiosyncratic variation in firm prices is also a potentially valid instrument. Because we have several instruments for a change in MTB value, we can run an over-identification test to check the plausibility of these assumptions. A second advantage of this approach is that we can use the first-stage equation to obtain an estimate for how the various components of stock price movements affect MTB values (i.e., λ_C, λ_D, λ_ν, λ_r). We take a discrete approximation of equation (11)

$$\Delta^4\left(\frac{V_{fs}}{K_{fs}}\right) = \eta_f + \eta_t + \lambda_C \tilde{r}^C_{fs} + \lambda_D \tilde{r}^D_{fs} + \lambda_\nu \tilde{\nu}_{fs} + \lambda_r \dot{r}_{fs} + \zeta_{fs}, \tag{17}$$

where $\dot{r}_{fs} \equiv \tilde{r}_{fs} - \tilde{r}^C_{fs} - \tilde{r}^D_{fs} - \tilde{\nu}_{fs}$; η_f and η_t are firm and time fixed effects that capture different intercepts for each term and any time varying component of higher order terms; and ζ_{fs} is an error term that captures the impact of higher order terms. Finally, we can write the discrete, empirically implementable version of equation (13) as:

$$\frac{\Delta^4 I^P_s}{I_{s-4}} \equiv \frac{\widehat{\psi^{-1}}\hat{\lambda}_C \sum_f \tilde{r}^C_{f,s-4} K_{f,s-4}}{I_{s-4}} + \frac{\widehat{\psi^{-1}}\hat{\lambda}_D \sum_f \tilde{r}^D_{f,s-4} K_{f,s-4}}{I_{s-4}}. \tag{18}$$

This will provide us with an estimate of the aggregate effects of the US-China trade war announcements on U.S. aggregate investment.

3 Data

Our analysis requires data on stock returns, balance sheet items, exposure to China, and event dates. Our stock return data are from the Center for Research in Security Prices (CRSP) provided by Wharton Research Data Services (WRDS), for every trading day in 2016-2019. The balance sheet data (e.g., investment, cash flows, MTB, etc.), covering the same period but at the quarterly frequency, are from the CRSP/Compustat Merged (CCM) data also provided by WRDS. When we merge the Compustat data with the CRSP data for a balanced panel of firms that report stock returns on every day, we obtain a sample of 2,864 firms that cover all sectors. We use the same procedure as in Frank and Shen (2016) to construct our measures of the variables we use in the investment regressions. The 2,864 listed firms that we use in our sample have investment rates that are highly correlated with investment rates in national accounts. The correlation between the growth rate of aggregate tangible fixed capital formation in Compustat data and the growth rate of fixed capital in national accounts data is 0.92.[6] Thus investment patterns in our sample track national ones well. We report sample statistics and a mapping between Compustat variable codes and our variables in the appendix.

[6]National accounts data is from FRED is Real Gross Private Domestic Investment: Fixed Investment: Nonresidential: Equipment, Percent Change from Quarter One Year Ago, Quarterly, Not Seasonally Adjusted.

We consider three ways in which firms are exposed to China: importing, exporting, and foreign sales (either through exporting or subsidiaries). It is important to capture indirect imports that are ultimately purchased by U.S. firms because many firms do not import directly from China but instead obtain Chinese inputs through their subsidiaries or the U.S. subsidiaries of foreign firms. For example, Apple Computer's exposure to China can arise through direct imports, imports obtained by its subsidiary (Beats Electronics), or from the purchase of iPhone's from the U.S. subsidiary of Foxconn. In order to identify the supply chains, we merge the Compustat data with S&P Capital IQ data using DUNS numbers to create the network of firm suppliers that potentially import from China. We define "China Revenue Share" to be the share of a firm's revenues in 2018 (either obtained through sales of subsidiaries or exports) that arise from sales in China as reported in FactSet. Finally, we also used data on firm employment from FactSet.

We also merge the firm names from Compustat, S&P Capital IQ, and FactSet with Datamyne data to identify which firms are trading with China directly or indirectly through their network or suppliers. The Datamyne data has a number of limitations. First, it only covers seaborne trade. Fortunately, U.S. Census data reveals that in 2017, 62 percent of all imports from China and 58 percent of exports to China are conducted by sea, so we capture over half of the value of U.S. China trade. A second problem is that while some of the Datamyne data is at the Harmonized System (HS) 6-digit level, much of it is at the HS2-digit level. Since U.S. tariffs are set at the Harmonized Tariff System 8-digit level, it is not possible to know what share of a firm's trade was affected by tariffs. We therefore opt to use a binary exposure measure. Our "China Import" dummy is one if the firm or its supply network imported from China in 2017 and zero otherwise. We also construct a "China Export" dummy analogously for exports.

Table 1: China Trade Exposure of Listed U.S. Firms

	Mean
Firm Imports from China	0.07
Firm or Subsidiary Imports from China	0.23
Firm, Subsidiary, or Supplier Imports from China	0.27
Firm Exports to China	0.01
Firm or Subsidiary Exports to China	0.04
Firm Exposed to China	0.46
Number of Firms: 2,864	

Note: This table reports the means of indicator variables that are one if a firm satisfies the listed criterion.

These data show that the supply chain information is critical in understanding firms' exposure to international trade. From Table 1, we see that only about 7 percent of the firms in our sample import directly from China, and only 1 percent export directly to China. However, if we take into account subsidiaries, these numbers rise to 23 and 4 percent, respectively. When we add in imports by all firms in the supply chain, we see that 27 percent of all listed firms in the U.S. import directly or indirectly from China. While only 4 percent of all firms export to China, the average firm in our sample obtained

2.3 percent of its revenue from China. In the last row of the table, we construct a variable, "Firm Exposed to China" if any firm in the firm's network exported to or imported from China or if the firm had positive revenues from China. We see that 46 percent of all firms were exposed to China through one of these channels. Consistent with prior work, we find that all of our China exposure measures are positively correlated ($\rho_{Imp,Exp} = 0.26$, $\rho_{Imp,Rev} = 0.24$, and $\rho_{Exp,Rev} = 0.08$), which suggests that firms that are affected by U.S. tariffs are also likely to be firms that are more exposed to Chinese retaliation on exports to or sales in China.

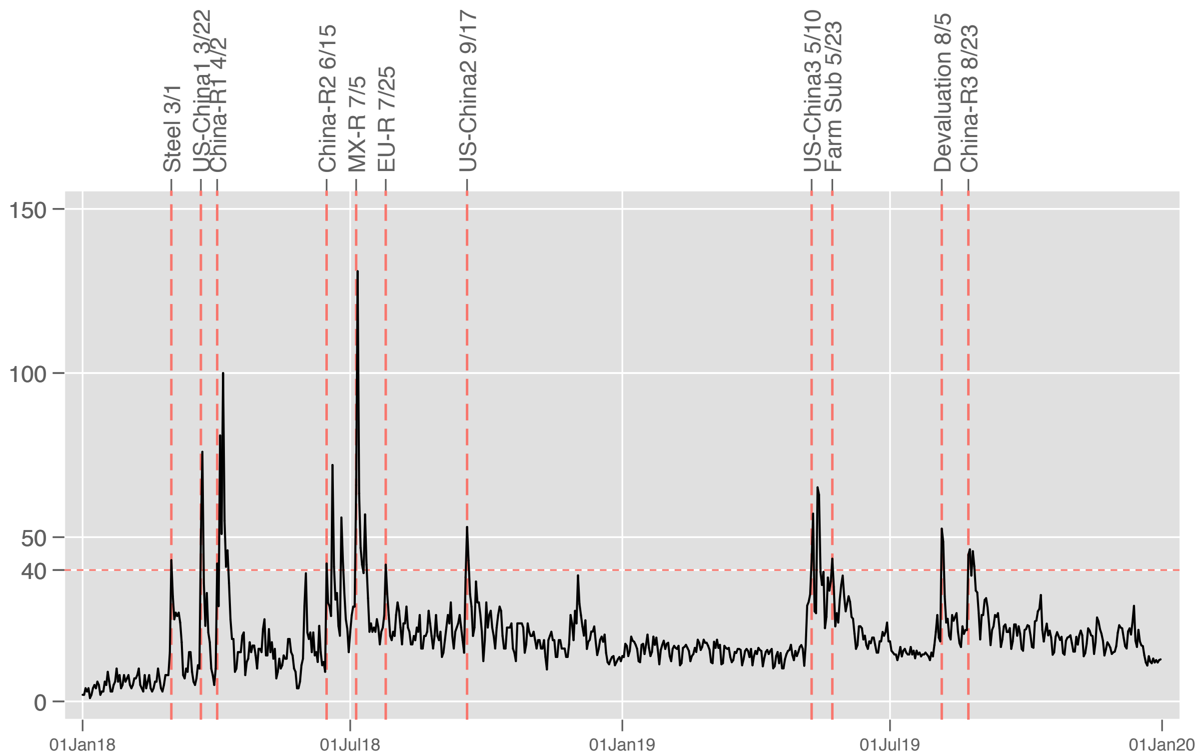
There were a large number of trade war announcements that occurred during the course of the first two years of the U.S.-China trade war, so the choice of which announcements are significant is not obvious *ex ante.* For example, Huang et al., 2019 focus much of their event study analysis on one important event: the March 22, 2018 announcement that the U.S. was proposing tariffs an a large fraction of Chinese imports, although they consider a few other dates as well.[7] By contrast, Egger and Zhu (2019) use twelve U.S. events based on their assessment of the importance of the information on those days and excluding announcements that did not specifically target China. In order to prevent the possibility that we inadvertently pick dates that match stock movements consistent with our priors, we use a systematic method of identifying the key event dates in the trade war. We select our event dates based on Google Trends data. Google Trends provides daily searching frequency on a term for periods of up to six months, and the reported value is normalized based on all searches that occurred in the period.[8] In particular, we argue that when real information about the trade war enters the market, it is likely to prompt people to search for the term "trade war." Figure 1 presents the results from this exercise. Google does not release the actual number of searches, so we do not know how many searches a value of 100 implies, but we do know that it is twice as many searches as a value of 50. A second feature of this graph is that it makes clear that after searches spike, it often takes a few days for the rate of searching to fall back to its prior level. Based on this, we define the start date of an event as the day on which the value of Google Trends for "trade war" exceeds 40, and we do not allow a second event to begin until five calendar days have passed after the first event.

Our method identifies 11 candidate events, which turn out to be easily classifiable into three categories: U.S. tariff events, Chinese retaliation events, and other trade war related events that do not involve U.S. or Chinese tariffs. Our first event is the March 1, 2018 announcement of steel and aluminum tariffs (which also targeted China). We classify this event along with the March 22, 2018 event, the September 17, 2018 announcement of tariffs on $200 billion of Chinese imports, and the May 10, 2019 announcement of the increase in tariffs of those imports from 10 to 25 percent as "U.S. tariff" events. Our "China

[7]They also consider the April 3, 2018 list of targeted Chinese products, the conclusion of U.S.-China trade talks on January 9, 2019, and the May 5, 2019 tweeted threat of an increase in U.S. tariffs as other events.

[8]To splice together Google Trends series in different periods, we made five downloads for 2018-2019: each download is a series for up to six months and an overlapping month with its previous period. We start with values for the first six-month period (2018/01-06), regress the second period on it, and use the predicted values for the second period. Then we regress the third period on the predicted values of the second period, and repeat this process until all series are spliced together.

Figure 1: "Trade War" Searches in Google Trend



Note: The figure reports the frequency of google searches for the phrase "trade war." "Steel 3/1" is the March 1, 2018 announcement of steel and aluminum tariffs (which also affected Chinese imports); "US-China1 3/22" is the March 22, 2018 announcement that the U.S. was proposing tariffs on a large fraction of Chinese imports; "China-R1 4/2" is the April 2, 2018 announcement of Chinese retaliation on 128 categories of U.S. exports; "China-R2 6/15" is the announcement that China was going to retaliate against $50 billion of U.S. exports; "MX-R 7/5" is the July 5, 2018 Mexican announcement that they were going to retaliate in response to the steel and aluminum tariffs; "EU-R 7/25" is the July 25, 2018 announcement that the European Union was preparing retaliatory tariffs on $20 billion of U.S. exports; "US-China2 9/17" is the September 17, 2018 announcement of tariffs on $200 billion of Chinese imports; "US-China3 5/10" is the May 10, 2019 announcement of the increase in tariffs of those imports from 10 to 25 percent; "Farm Sub 5/23" is the May 23, 2019 announcement of $16 billion of farm subsidies to help farmers affected by the trade war; "Devaluation 8/5" is the August 5, 2019 announcement that the People's Bank of China was devaluing the yuan; and "China-R3 8/23" is the August 23, 2019 announcement that China was going to raise tariffs on U.S. soybean and auto exports.

Retaliation" events consist of the April 2, 2018 announcement of Chinese retaliation on 128 categories of U.S. exports, the June 15, 2018 announcement that China was going to retaliate against $50 billion of U.S. exports, and the August 23, 2019 announcement that China was going to raise tariffs on U.S. soybean and auto exports. Finally, we identify four "other" events that correspond to trade war related actions not involving the U.S. and China: the July 5, 2018 Mexican announcement that they were going to retaliate in response to the steel and aluminum tariffs, the July 25, 2018 announcement that the European Union was preparing retaliatory tariffs on $20 billion of U.S. exports, the May 23, 2019 announcement of $16 billion of farm subsidies to help farmers affected by the trade war, and the August 5, 2019 announcement that the People's Bank of China was devaluing the yuan, which did not have a differential effect on U.S. firms relative to other foreign firms.

Table 2 presents the stock market return on each of these event dates. We see that the stock market fell on six of the seven U.S. or China event dates, with a total drop of 8.9 percent over all of the events. Interestingly, the data provides some evidence of overshooting—if we start the event window one day before the event and extend the end of event window (w) to five trading days after the event as in the next column (typically seven calendar days), the total drop falls in magnitude to only 3.0 percent, and we find that the stock market actually rose on four out of the seven events. These results suggest caution about the choice of event dates and the length of the event window. This sensitivity motivates our decision to present results for all event dates (as well as individual event dates) and all reasonable event windows up to 30 trading days after an event.

Table 2: Stock Returns on Event Dates

Event Group	**Event Date**	R_t **(%)**	$\sum_{t-1}^{t+5} R_t$ **(%)**	**Description**
US	01Mar18	-1.16	0.28	US announces steel and aluminum tariffs
US	22Mar18	-2.48	-2.70	US orders identification of Chinese products for tariffs
CHN	02Apr18	-2.25	0.39	China to impose tariffs on 128 US exports
CHN	15Jun18	-0.10	-0.53	China retaliates on $50 bn of US imports
OTH	05Jul18	0.88	2.61	Mexico imposes retaliatory tariffs on dozens of US goods
OTH	25Jul18	0.84	-0.08	EU prepares retaliatory tariffs on $20 bn in US goods
US	17Sep18	-0.67	0.32	US announces tariffs on $200 bn goods from China
US	10May19	0.39	-0.70	US raises tariffs from 10 to 25 percent on $200 bn of Chinese imports
OTH	23May19	-1.29	-4.08	US announces $16 bn bailout to farmers hurt by trade war
OTH	05Aug19	-3.00	-2.48	Chinese currency fell to the lowest point since 2008
CHN	23Aug19	-2.60	0.03	China raises tariffs on soy and autos
US+CHN	all	-8.87	-2.93	

Note: This table shows market returns on and around trade-war announcements that resulted in large numbers of google searches. "US" refers to events involving an announcement of US tariffs on China; "CHN" refers to events involving Chinese retaliatory tariffs; and "OTH" refers to trade-war events not concerning U.S. or Chinese tariffs. R_t is the market return (in our sample of firms) on the day of the announcement. $\sum_{t-1}^{t+5} R_t$ is the cumulative market return over a 7-day window beginning on the trading day before the announcement and extending five trading days after the announcement. The total 7-day return for the U.S. and Chinese events in this table does not exactly equal the value in subsequent tables because we are presenting raw data in this table and double count one day that appears in two event windows.

Another noteworthy characteristics of the data is that large U.S. firms tend to have much higher exposure to China than small firms, and they tended to perform substantially worse when the U.S. and China announced tariffs. Table 3 presents data on how the China exposure variables differ for large firms, where we define large to be the 25, 50, or 100 largest firms in terms of market capitalization. While only 27 percent of all firms import directly or indirectly from China, 60 of the top 100 firms do. Similarly, while only 4 percent of listed firms export to China, 16 out of the top 100 firms do. Finally, we see a similar pattern in terms of how dependent firms are on the Chinese market for sales. While on average only 2.3 percent of all firm revenues of listed firms come from the Chinese market, 6.1 percent of revenues for the largest 100 firms come from China. Since these 100 firms account for 56 percent of total U.S. market capitalization, the size weighted average of exposure to China is much larger than the simple average.

Table 3: Large Firms Import from, Export to, and Sell More in China

	All Firms	Top 25	Top 50	Top 100
Cumulative Share of Market Capitalization	1	0.31	0.43	0.56
Average China Import Dummy	0.27	0.72	0.60	0.60
Average China Export Dummy	0.043	0.20	0.18	0.16
Average China Revenue Share	0.023	0.066	0.086	0.061
Average Non-China Revenue Share	0.16	0.34	0.38	0.34

Note: This table shows selected sample statistics for all firms in our sample as well as firms that are ranked in the top 25, top 50, and top 100 of all firms in terms of market capitalization.

4 Results

In this section, we first present the results from estimating our factor model and event study. We then present our estimates of how these trade events affected investment.

4.1 Event Study Results

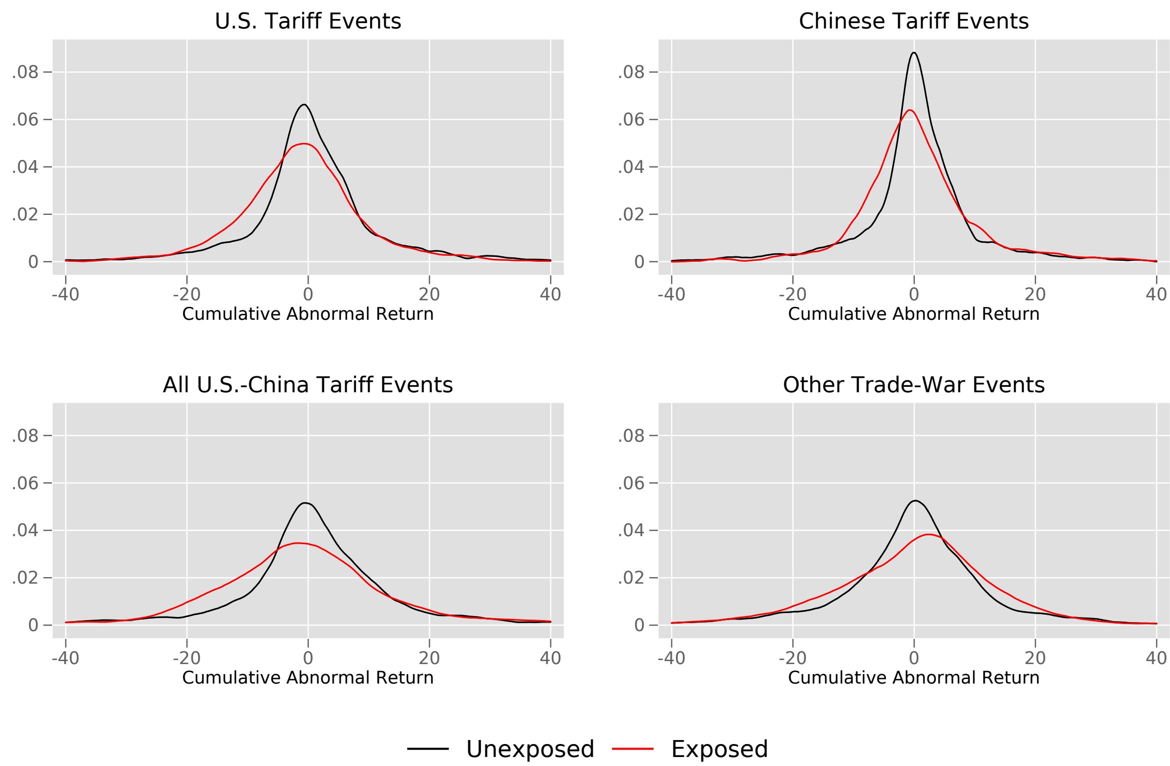
Using daily stock returns for all trading days between January 1, 2016 and December 31, 2019, we first estimate the number of factors (K) and the resulting factor model in equation (1) following the approach of Bai and Ng (2002) and Bai and Ng (2013). We follow Bai and Ng (2002), who recommend choosing the number of factors to minimize the following loss function when errors may be cross-sectionally correlated:

$$IC(K) = \ln(\mathcal{L}(K)) + K\left([F+T-K]\frac{\ln(FT)}{FT}\right),$$

where F is the number of firms; $\mathcal{L}(K)$ is the log likelihood function based on the estimation of equation (1); N is the number of firms; and T is the number of days. Each additional factor adds 2,864 β_{kf} parameters (one for each firm). The first factor is similar to what would be obtained in a classic CAPM setup—its correlation with the total market return in our data is 0.89. As expected, the typical factor loadings for the first factor are close to one ($\overline{\beta_{1f}} = 0.89$, with the inter-quartile range between 0.60 and 1.14). Similarly, our estimates of α_f are close to zero ($\overline{\alpha_f} = 0.001$, with a standard deviation of 0.001). The first factor accounts for 10.4 percent of the variance, but additional factors account for much less, with the next four factors accounting for 2.0, 1.6, 1.5, and 1.4 percent of the variance, respectively. Based on the Bai and Ng (2002) loss function, we use two factors in our baseline, but also do robustness checks with five factors.

In order to visualize how the various events affected exposed and unexposed firms (as defined in Table 1), we plot the kernel densities of the 7-day abnormal returns ($\epsilon_{fj}^{CAR} \equiv \sum_{t=j-1}^{j+5} \hat{\epsilon}_{ft}$) (multiplied by 100) for both sets of firms in Figure 2. On the top left panel, we see that the distribution of CARs for firms exposed to China over the week following U.S. tariff announcements is shifted to the left relative to firms that were not exposed. Similarly, we see that announcements of Chinese tariff retaliation produce a similar pattern, with the distribution of abnormal returns for exposed firms lying to the left of the distribution for unexposed firms. The first plot on the second row pools together all of the U.S.-China tariff events, which is even more shifted to the left. Finally, in

Figure 2: Dispersion in Returns (7-Day Windows)



Note: This figure portrays kernel density plots of cumulative abnormal returns of firms exposed to China (red) and unexposed (black) during 7-day windows around trade-war announcements. Exposed firms are firms that export to, import from, or have positive revenues in China.

the lower right panel, we present the densities for other trade war announcements that did not involve U.S. or Chinese tariff announcements as a placebo test. As we mentioned in the data section, these events are composed of Mexican or E.U. retaliatory tariff announcements, the announcement of U.S. farm subsidies, and the Chinese devaluation of the yuan. We see that on these events exposed firms are associated with somewhat greater dispersion in CARs, but there is no clear pattern of underperformance. These results suggest that the U.S.-China tariff announcements shifted the distribution of CARs to the left for firms exposed to China only when the announcements contained information about U.S. or Chinese tariffs.

We identify the event-study effects on abnormal returns by estimating equation (2), with $w = 5$ as our baseline, i.e. a 7-day event window, so that we reduce the scope for stock-market overshooting to affect our results.[9] Table 4 presents the results from regressing the cumulative abnormal return (CAR) on the China exposure variables for each of the four U.S. tariff events. The estimated coefficients under each event date correspond to $\hat{\gamma}_{ij}$ in equation (2), and we report the average value of these estimated coefficients across all events in the first column. The coefficients should be interpreted as the aver-

[9]We discuss alternative window lengths ranging from three to thirty days as robustness checks later in the paper and show that these do no not qualitatively change the main results.

age daily effect of the announcement on the returns of exposed firms during the event window relative to unexposed firms. For example, the coefficient of -0.252 on the China importer dummy in column 2 implies that during the 7-day event window around the March 1, 2018 steel and aluminum announcement, firms that imported from China had abnormal returns that were on average 0.252 percentage points lower than other firms *every day* within this 7-day period. Thus, their *cumulative* abnormal return was -1.764 ($= 7 \times 0.252$) percent. Similarly, since the average estimates in column 1 tell us about four events each composed of seven days, the differential impact of being an importer from China on stock returns cumulated over all four events is 28 times larger: -2.072 percentage points ($= -0.074 \times 4 \times 7$).

Table 4: Impact of U.S. Tariffs Announcements (7-Day Windows)

Dep. Var.: $\hat{\epsilon}_{ft}$	Average	01Mar18 Steel and Aluminum Announcement	22Mar18 China Target Announcement	17Sep18 \$200 Billion Announcement	10May19 10-25% Tariff Increase Announcement
China Importer	-0.074***	-0.252***	0.083**	-0.025	-0.102**
	(0.021)	(0.044)	(0.038)	(0.042)	(0.047)
China Exporter	-0.023	-0.116*	-0.045	0.095	-0.027
	(0.035)	(0.067)	(0.064)	(0.085)	(0.062)
China Revenue Share	-0.619***	0.253	-0.903***	0.775***	-2.601***
	(0.146)	(0.264)	(0.279)	(0.240)	(0.370)
Decomposition of Market Return in Percent					
Market Return	-3.50	0.28	-3.39	0.32	-0.70
Differential Effect	-2.13	-0.99	-0.04	0.26	-1.36
Common Effect	-2.69	2.15	-2.62	-0.60	-1.62
Total Event Effect	-4.82	1.16	-2.66	-0.34	-2.98

Note: This table presents the results from estimating equation (2). The number of observations is 137,472. The dependent variable ($\hat{\epsilon}_{ft}$) is the abnormal return obtained from estimating the factor model (equation 1) with two factors. China Importer is a dummy that equals one if the firm or any of its subsidiaries or suppliers imports from China. China Exporter is a dummy that equals one if the firm or its subsidiaries export to China. China Revenue Share is the share of the firm's revenue that comes from sales in China. Column 1 presents the the average of the coefficients on each of the event days. Standard errors are in parentheses. In the lower panel of the table, we report the cumulative market effects as well as the differential and common effects as defined in equation (6). In this lower panel, the first column is the total (not the average) for all the events.

Table 4 also reveals the important heterogeneity of effects across different event dates based on estimation of equation (2). This heterogeneity reflects the potential pitfall of assuming that all tariff announcements should have similar effects. For example, the March 1, 2018 announcement (widely seen as the start of the trade war) and the May 10, 2019 announcement (which more than doubled tariffs against China) had significant negative effects on the abnormal returns of importers from China. Other U.S. tariff announcements had insignificant or even positive effects on importers, perhaps revealing that the announcements were less protectionist than anticipated.

We also see that while there is no robust relationship between U.S. tariff announcements and the abnormal return of firms that exported to China, firms that obtained larger shares of their revenues from China did have lower returns than other firms. This negative coefficient on the China Revenue Share variable is likely due to three (not mutually exclusive) reasons. The first is that the the China revenue share variable (which includes the share of revenues due to exports) may be picking up the impact of announcements on exporters. Second, market participants may have anticipated that U.S. tariffs may provoke Chinese retaliatory tariffs *and* non-tariff barriers that could lower revenues obtained either by exporting or multinational sales. Third, it is also possible that U.S. tariffs weakened the Chinese economy, which could lower profits for U.S. firms selling there. The average estimated effect is also economically significant. For example as we saw in Table 3, the average share of revenues from China for a firm in the top 100 was 0.061. The coefficient on China revenue share in the first column of Table 4 implies that these firms had an average abnormal return of -0.038 ($= -0.619 \times 0.061$) every day during all of the U.S. tariff events. Therefore, the cumulative abnormal return for these firms was 28 times higher: -1.06 percentage points.

Table 5: Impact of Chinese Tariff Announcements (7-Day Windows)

Dep. Var.: $\hat{\epsilon}_{ft}$	Average	02Apr18 China \$128 Bln Announcement	15Jun18 China \$50 Bln Announcement	23Aug19 China Soy/Auto Announcement
China Importer	0.046*	0.027	-0.046	0.158***
	(0.025)	(0.039)	(0.044)	(0.044)
China Exporter	-0.056	0.059	-0.183***	-0.045
	(0.037)	(0.057)	(0.063)	(0.069)
China Revenue Share	-0.825***	-0.417	-1.699***	-0.360
	(0.144)	(0.270)	(0.244)	(0.230)
Decomposition of Market Return in Percent				
Market Return	-0.80	-0.29	-0.53	0.03
Differential Effect	-0.48	0.01	-0.95	0.46
Common Effect	-0.67	-0.44	0.73	-0.95
Total Event Effect	-1.15	-0.43	-0.23	-0.50

Note: This table presents the results from estimating equation (2). The number of observations is 137,472. The dependent variable ($\hat{\epsilon}_{ft}$) is the abnormal return obtained from estimating the factor model (equation 1) with two factors. China Importer is a dummy that equals one if the firm or any of its subsidiaries or suppliers imports from China. China Exporter is a dummy that equals one if the firm or its subsidiaries export to China. China Revenue Share is the share of the firm's revenue that comes from sales in China. The average is the the average of the coefficients on each of the event days. Standard errors are in parentheses. In the lower panel of the table, we report the cumulative market decline as well as the differential and common effects as defined in equation (6). In this lower panel, the first column is the total (not the average) for all the events.

Table 5 presents analogous regressions estimated using data for the three Chinese tariff retaliation events. As with the U.S. tariff events, we also find substantial heterogeneity in the effects of the events on the returns for firms. We see no significant negative effects from Chinese retaliation on U.S. importers and mixed impacts on exporters and firms selling in China. Not surprisingly, announcements of Chinese retaliation are not associated with lower abnormal returns for U.S. importers, but we do see consistent negative abnormal returns for firms selling in China when China announces retaliation. These returns are significant and negative on average as one can see in the first column. The average daily effect (-0.825) implies that the one hundred largest firms in terms of market capitalization (which had a China Revenue Share of 0.061) experienced a -2.5 percentage point abnormal return due to their exposure to the Chinese market. Interestingly, only the June 15, 2018 Chinese retaliation appears to have significantly lowered the returns for firms exporting to China.

In the lower panel of the tables, we use equation (6) to decompose the aggregate market return into the differential and common effects, with the results in the first column corresponding to cumulative (not average) effect over all events. As one can see from Table 4, the U.S. stock market fell a total of 3.5 percent during the four 7-day event windows around the U.S. tariff announcements about China. The differential returns of firms exposed to China account for a 2.13 percentage point decline, and the common effect drove markets down another 2.69 percentage points on those days. These two declines total 4.82 percentage points, which is 1.32 percentage points larger than the aggregate, which implies that idiosyncratic factors uncorrelated with the information in the announcements pushed markets up by 1.32 percentage points. Much of this difference is driven by the underlying market trend in the market ($R^{\alpha}(5)$). The period between the first trading day in 2016 and the last trading day in 2019 was one of remarkable stock price increases with the Wilshire 5000 rising 66 percent. The four events with 7-day windows in our sample contain 28 trading days, which on average experienced a substantial upward drift as measured by $R^{\alpha}(5)$. In our sample, this drift amounts to 2.00 percentage points, which implies that if one had randomly picked 28 days in the sample with no information or events perturbing the market (i.e., $\delta_{kt} = Z_{if} = \theta_t = 0$), one should have expected the stock market to have risen by 2.00 percentage points. The fact that the market only rose 1.32 percentage points after netting out the impact of the tariff announcements, implies that the time-varying idiosyncratic firm-specific component ($R^{I}(5)$) only amounted to -0.68 percentage points. ($= 1.32 - 2.00$). Thus, virtually all of the stock market movement during these event windows can be accounted for by the underlying market drift ($R^{\alpha}(5)$), the differential effect ($R^{D}(5)$), and the common effect ($R^{C}(5)$).

The heterogeneity of the estimated common and differential effects will play an important role in understanding the impact of the trade war on investment. Consider the first two U.S. tariff announcements which occurred within the first quarter of 2018. We estimate that jointly they drove down stock prices by 3.22 percentage points. Of this drop, 1.03 percentage points can be attributed to the differential lower returns of firms exposed to China. However, the common effects of these announcements almost completely cancel (2.15-2.62=-0.47). Thus, when we turn to estimating the impact of the trade war on investment, we should only expect to see the differential effect having much of an impact from those two announcements. Similarly, the absence of a large differential or common effect following the September 17, 2018 announcement, means that we shouldn't expect much of an impact from this announcement on investment either. By contrast, the May 10, 2019 announcement appears to have had economically large impacts on equity prices due to the common effect and by driving down the relative share prices of exposed firms. This feature of our methodology highlights the fact that we do not bias our results by erroneously including irrelevant events because irrelevant events should yield estimated differential and common effects that are close to zero.

Turning to the aggregate estimated stock market impacts of Chinese retaliation in Table 5, we see similar heterogeneity. Overall, the markets declined by only 0.8 percent during these three event windows, and the differential and common effects both only account for a fraction of a percentage point movement. The only Chinese tariff retaliation event that seems to have moved the market substantially was the June 15, 2018 announcement. While the common effect pushed the market up by 0.73 percent following this announcement, differentially poor abnormal returns by firms exposed to China dragged overall returns down by 0.95 percentage points following the retaliation. Thus, the net effect is small.

Table 6: Decomposition by Length of Event Window and Type of Announcement

	All Events			U.S. Events			China Events		
w	$R(w)$	$R^C(w)$	$R^D(w)$	$R(w)$	$R^C(w)$	$R^D(w)$	$R(w)$	$R^C(w)$	$R^D(w)$
1	-9.74	-6.57	-1.75	-8.63	-5.58	-1.31	-1.11	-0.99	-0.44
		(0.34)	(0.34)		(0.28)	(0.28)		(0.21)	(0.19)
5	-4.29	-3.36	-2.61	-3.50	-2.69	-2.13	-0.80	-0.67	-0.48
		(0.44)	(0.47)		(0.34)	(0.36)		(0.28)	(0.27)
10	-2.24	-4.50	-1.76	-5.16	-5.19	-3.25	2.92	0.68	1.49
		(0.70)	(0.59)		(0.44)	(0.51)		(0.47)	(0.40)
30	-5.74	-18.22	-0.76	-10.53	-14.94	-3.55	5.75	-2.41	2.79
		(0.97)	(0.70)		(0.61)	(0.82)		(0.60)	(0.77)

Note: $R(w)$, $R^C(w)$, and $R^D(w)$ equal the total stock-market percentage decline, the decline due to the common effect, and the decline due to differential effects as defined in equation (6). Since the event window always starts one day before the event the length of the event window is $w+2$, where w is the number of days after the event included in the window. These effects are defined in equation (6). Bootstrapped standard errors are in parentheses.

Thus far, we have not addressed the statistical significance of the estimated impact of the common and differential effects given in equation (6). Because these estimates are based on thousands of coefficients arising from the factor model, it is difficult to compute them analytically. We therefore estimate them by bootstrapping the sample of firms 200 times, and then recomputing all parameters and estimates for a variety of event window lengths. We present these results in Table 6. The first set of three columns of this table presents the results for all U.S. and China tariff events, and the other sets of columns separates the events into those corresponding to U.S. and Chinese tariff announcements. The first column in each set of events ($R(w)$) gives us the cumulative market movement during the event window. In all cases we see that $R(1)$ is less than $R(5)$, which is consistent with the overshooting we saw when just comparing 3- and 7-day event windows. When we look at events extending 30 days past the announcement ($R(30)$), we see that returns drift even more negatively for U.S. tariff announcements and positively for Chinese tariff announcements, but on net the fall is quite similar to what we see using a 7-day window ($R(5)$).

The estimates for the differential effect ($R^D(w)$) are fairly stable for all window lengths as we saw in Figure 3. For values of w ranging from 1 to 10 (i.e., event window lengths of three to 12 days), the differential effect ranges between -1.75 and -2.61 when we include all events. It also is significantly negative for all values of w through 10. U.S. tariff announcements have significant negative differential effects on exposed firms for all event windows. As before, we see that Chinese tariff announcements have small differential effects, and these are not always significant and can flip sign when we have event windows exceeding five trading days after an announcement. Not surprisingly, the event window length seems to matter the most when estimating the common effect since stock-market movements over long horizons are likely to also reflect information unrelated to the trade war. Since the sum of the common and differential effects are more negative on average when we use event windows of 3 or 12 days ($w = 1$ or 10), we focus on our most conservative estimate of 7 days ($w = 5$) in the next section, noting that we can estimate more negative impacts if we chose event windows of 1, 10, or 30 days after an announcement. We will continue to make our baseline $w = 5$—i.e., a 7-day window—for three reasons. First, this choice maintains a short enough event window so that we can plausibly argue that the major news was due to the event. Second, it is long enough to limit the potential impact of stock-market overshooting. And third, it enables us to be conservative about the total effect of the U.S.-China tariff announcements.

4.1.1 Robustness

Next, we explore the robustness of these results. First, we explore how the positive correlation of the China revenue share variable with the import and exporting dummies that we discussed in Section 3 affects the results. We re-estimate our baseline specifications without the China revenue share variable and report the coefficient averages for the U.S. and Chinese events in columns 1 and 2 of Table 7. Dropping the revenue share variable increases the magnitude of the estimated negative effect that U.S. tariffs have on U.S. importers and Chinese tariffs have on U.S. exporters. Shutting down this channel of exposure reduces the magnitude of the estimated differential effect of the stock-market decline from 2.61 percentage points to 1.47 percentage points (when we sum across all U.S. and China events), respectively, but increases the magnitude of the decline due to the common effect from 3.36 percentage points to 3.84 percentage points. Overall, though, the aggregate impact of the tariff announcements on stock prices is little changed: the total effect falls in magnitude from -5.97 percent in our preferred specification to -5.31 percent when we shut down the differential impact of the trade war on firms selling in China.

Second, we consider the effect of changing the event window from seven to three days (i.e., the day before, the day of, and the day after). A shorter event window is more common in the literature and constitutes our main alternative specification. In order to save space, we do not report the results for each event date—they are presented in the appendix—and instead present the averages of the coefficients in columns 3 and 4 of Table 7. We see two important differences with the baseline results. First, the market moved much more in 3-day windows around events relative to 7-day windows, presumably because short-lived uncertainty or other factors caused the market to overshoot after the initial announcements. The cumulative market decline for the seven events in Tables 4 and 5 equals 9.74 percentage points: more than two times larger than the cumulative decline we measured using 7-day windows (4.3 percentage points). The results also provide an explanation for why the market partially recovered after the initial announcements: the downward effect of the common effect was much smaller. While the common effect drove markets down by 3.36 percentage points when using a 7-day window—2.69 percentage points on U.S. announcement days and 0.67 percentage points on Chinese announcement days—it drove markets down by 6.57 percentage points when we shift to 3-day windows (as one can see by summing together the common effect estimates in columns 3 and 4 of Table 7). By contrast, the differential effect becomes slightly less im-

Table 7: Robustness Tests: Collinearity, Event Windows, and Placebo Tests

	Average of Coefficients					
	(1)	(2)	(3)	(4)	(5)	(6)
China Importer	-0.094***	0.020	-0.076**	0.008	-0.038	-0.063***
	(0.021)	(0.024)	(0.034)	(0.038)	(0.023)	(0.017)
China Exporter	-0.027	-0.061*	-0.080	-0.074	0.014	-0.018
	(0.035)	(0.037)	(0.056)	(0.050)	(0.041)	(0.025)
China Revenue Share			-1.097***	-0.868***	0.253	0.481***
			(0.207)	(0.246)	(0.171)	(0.108)
N	137,472	137,472	60,144	60,144	80,192	100,240
w	5	5	1	1	5	5
Events	U.S.	China	U.S.	China	Other	5 Largest Declines 2017
Decomposition of Market Return in Percent						
Market Return	-3.49	-0.81	-8.63	-1.11	-4.04	-7.90
Differential Effect	-1.56	0.09	-1.31	-0.44	-0.19	-0.49
Common Effect	-2.93	-0.91	-5.58	-0.99	-7.64	-9.61
Total Event Effect	-4.50	-0.83	-6.89	-1.43	-7.83	-10.10

Note: This table presents the average coefficient on each of the event days obtained from estimating equation (2) with two factors. Coefficients for each event corresponding to the specifications in columns 1 and 3 can be found in Appendix Tables A.2 and A.3. The dependent variable ($\hat{\epsilon}_{ft}$) is the abnormal return obtained from estimating the factor model (equation 1). China Importer is a dummy that equals one if the firm or any of its subsidiaries or suppliers imports from China. China Exporter is a dummy that equals one if the firm or its subsidiaries export to China. China Revenue Share is the share of the firm's revenue that comes from sales in China. Standard errors are in parentheses. In the lower panel of the table, we report the cumulative market decline as well as the differential and common effects as defined in equation (6). In this lower panel, the first column is the total (not the average) for all the events.

portant as we shorten the event window. Firms exposed to China had stock returns that were 1.75 percentage points lower than unexposed firms using a 3-day window, but their returns were 2.61 percentage points lower when we shift to 7-day windows. In order to be conservative and not allow this overshooting to cause us to overstate the impact of the tariffs, we therefore will focus on the 7-day windows, but we estimate even larger effects using 3-day windows as a robustness check.

Third, we conduct a series of placebo tests to make sure that the effects we identify do not appear when we estimate the model using non-tariff events. We first check that there is no differential effect on China-exposed firms during trade war announcements unrelated to China. We rerun our event study using the four trade-war announcements listed in Table 2 that are not associated with escalating U.S. or Chinese tariffs, but instead due to events like Mexico or EU retaliation. The results, presented in column 5, indicate that none of our China exposure variables are significantly associated with abnormal returns arising from events not linked to U.S.-China tariff announcements. The differential effect accounts for only 0.19 percentage points of the 4.04 percentage point drop in the market around these days. However, we do see a large negative effect arising from the common effect. This result is consistent with the hypothesis that escalations in the trade war with countries other than China did lower markets due to heightened policy uncertainty and other common factors, but these announcements did not have any differential effect on firms exposed to China.

We run a second placebo test to examine the hypothesis that our selection of announcements was driven by large stock-market declines. If this were true, one should expect to see differential effects in response to other large, negative movements in stock markets. In the last column of Table 7, we run our specification using the five largest one-day declines in 2017 as our event dates. While there is evidence that importers on average performed significantly worse on these days, there is no significant association with exporters, and the firms selling in China actually performed better. On net, these effects tend to cancel producing only a small differential effect. The cumulative decline on these days was 7.90 percent, but the contribution of the event component was only -0.49 percentage points: only 6 percent of the total. The small contribution of the event component indicates that differential returns of firms exposed to China did not contribute substantially to overall market declines when the market moved substantially for reasons other than trade policy. Similarly, the large negative value for the common effect also makes sense—if we pick events on the basis of sharp market movements on those days, we are likely picking events in which some common factor depressed stock prices.

Table 8 presents results for a number of additional robustness checks. First, it is possible that import-competing firms in protected industries benefited from the tariffs. To test this hypothesis, we include a dummy variable in the regression that is one if the U.S. applied a tariff in the North American Industrial Classification System (NAICS) 6-digit sector listed for the firm.[10] The results, reported in column 1, indicate that there is no significant relationship between stock returns and being in a protected industry. These results are largely in line with Egger and Zhu (2019) who using a different sample of events and different method of constructing abnormal returns also found weakly negative relationships between U.S. protection and abnormal returns of U.S. firms.

We also considered robustness to various other omitted variables. For example, firms exposed to China tend to be large, multinationals, so it is possible that these characteristics of the firms are driving the results. In columns 2 and 3, we re-estimate our specifications for U.S. and Chinese tariff events with a "Large Company" dummy that is one if the company has more than 1000 employees. As one can see from the table, the estimates of the coefficients of interest are only slightly affected and the coefficient on size is insignificant. It also could be the case that our China Revenue Share variable is picking up an effect arising from being a multinational, not just a firm that sells in China. If the U.S.-China trade war affected all multinationals equally, we would expect that including the share of a firm's sales arising from sales in foreign countries other than China to matter in our event study. We therefore report the results from this specification in columns 4 and 5 of the table. As one can see from these results, multinationals that had sales to countries

[10]We set the industry protected dummy equal to 1 if a 6-digit NAICS industry was subject to a new tariff. We identified these industries for each of the four U.S. tariff announcement events combining information on the HTS10 tariff affected industry with a new tariff with a mapping from HTS10 to NAICS. The affected industries were easily identified for all of the US tariff announcements except the 3/22/18 event which was the announcement that US orders identification of Chinese products for tariffs. For this event, we set the dummy equal to one for all 6-digit NAICS industries where the 2017 share of China imports in total consumption was at least 10 percent, and zero otherwise. Note there was no shipments data available for NAICS industries starting with 11 (logging) and special codes (starting with 9) so these are set to zero.

Table 8: Robustness Tests: Omitted Variables and Factors

	Average of Coefficients						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
China Importer	-0.064***	-0.071***	0.036	-0.068***	0.035	-0.054**	0.025
	(0.023)	(0.022)	(0.025)	(0.022)	(0.026)	(0.021)	(0.024)
China Exporter	-0.017	-0.018	-0.061	-0.019	-0.065*	-0.014	-0.078**
	(0.035)	(0.035)	(0.037)	(0.035)	(0.037)	(0.035)	(0.036)
China Revenue Share	-0.556***	-0.633***	-0.870***	-0.557***	-0.956***	-0.608***	-0.805***
	(0.153)	(0.149)	(0.146)	(0.157)	(0.159)	(0.145)	(0.143)
Industry Protected	-0.065						
	(0.074)						
Large Company		-0.032	-0.043				
		(0.035)	(0.047)				
Non-China Revenue Share				-0.048	0.100*		
				(0.051)	(0.060)		
N	137,472	121,392	121,392	137,472	137,472	137,472	137,472
Events	U.S.	U.S.	China	U.S.	China	U.S.	China
Number of Factors	2	2	2	2	2	5	5
Decomposition of Market Return in Percent							
Market Return	-3.19	-2.24	-1.00	-3.53	-0.76	-3.51	-0.79
Differential Effect	-1.86	-2.09	-0.66	-2.33	-0.17	-1.77	-0.77
Common Effect	-2.53	-1.74	-0.59	-2.58	-0.85	-3.11	-0.35
Total Event Effect	-4.39	-3.84	-1.25	-4.92	-1.02	-4.88	-1.12

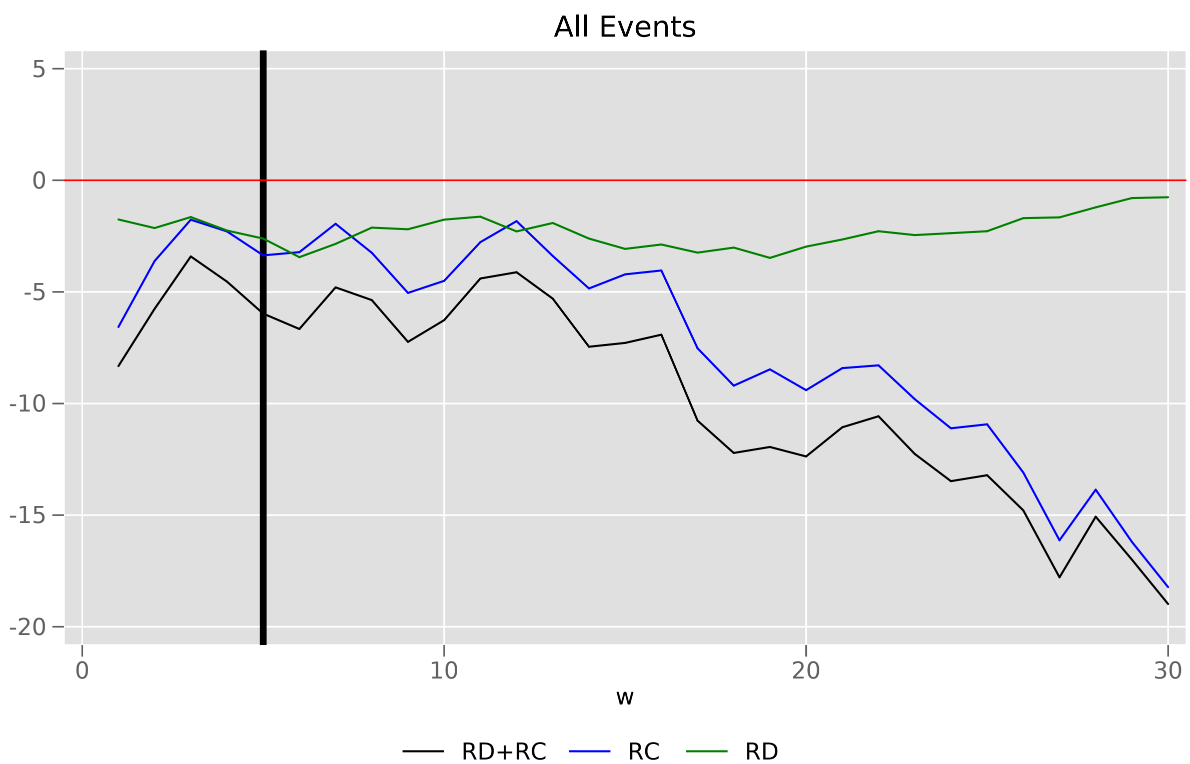
Note: This table presents the average coefficient on each of the event days obtained from estimating equation (2). The dependent variable ($\hat{\epsilon}_{ft}$) is the abnormal return obtained from estimating the factor model (equation 1). China Importer is a dummy that equals one if the firm or any of its subsidiaries or suppliers imports from China. China Exporter is a dummy that equals one if the firm or its subsidiaries export to China. China Revenue Share is the share of the firm's revenue that comes from sales in China. Large Company is a dummy that equals 1 if the firm has 1000 or more employees. Non-China Revenue Share is the share of the firm's revenues that comes from foreign companies other than China. Industry Protected is a dummy equal to one in the 6-digit NAICS industry that has a new tariff applied in the event window. Standard errors are in parentheses. In the lower panel of the table, we report the cumulative market decline as well as the differential and common effects as defined in equation (6). In this lower panel, the first column is the total (not the average) for all the events.

other than China did not fare significantly worse than firms with no sales outside the U.S, which indicates that the key driver of the negative CARs is the firm's exposure to China.

So far, all of the results have been drawn on a two factor model that was based on the Bai and Ng (2002) procedure, but it is possible that we may have underestimated the number of relevant factors. To check the sensitivity of our results to the number of factors, we re-estimated the factor model using five factors and report the results in the last two columns. Using more factors seems to slightly strengthen the results: yielding a negative and significant relationship between Chinese retaliation an export status and slightly raising the estimated common effect.

Finally, we check how robust our results are to varying the length of the event window. To do this, we recomputed the decomposition given in equation (6) for every value of w between 1 and 30 (corresponding to event windows ranging between 3 and 32 trading days). We present these results in Figure 3. The first panel in the figure shows the results of using all of the U.S.-China tariff events. As before, we see that the common effect is negative and large in magnitude in short event windows, reflecting the sharp overall drop in the market around the announcements. However, the common effect rises in magnitude to -5 percentage points about two days after the event and stays at approximately this level until about 15 days after the event. Thereafter, it drifts off, which probably reflects the fact that our identifying assumption—that the tariff announcements were the main source of information about stock prices—is less plausible as we move several weeks away from any announcement. Interestingly, the differential contribution is remarkably stable over all window lengths. This stability implies that firms that were exposed to China experienced differentially lower stock returns following U.S.-China tariff announcements and these persisted for at least 30 trading days afterwards.

Figure 3: Decomposition by Length of Event Window



Note: This figure shows the estimated percentage point decline overall market returns for various event window lengths due to the common effect of U.S.-China tariff announcements ($R^C(w)$) and information that differentially affected the returns of exposed firms ($R^D(w)$). Since the event window always starts one day before the event the length of the event window is $w+2$, where w is the number of days after the event included in the window. These effects are defined in equation (6).

In sum, our stock-market decomposition reveals that U.S. tariff announcements that targeted China are associated with *persistent* declines in the stock returns of firms that were exposed to China. Importantly, these results only reflect the impact of the unanticipated component of the announcement. To the extent that markets anticipated the trade war announcements, we omit them from our analysis. Thus, the aggregate effect may be even larger. Finally, although we do not see much evidence of Chinese retaliation announcements substantially affecting U.S. market values, the U.S. events are large enough to have driven substantial declines in overall U.S. market value. We explore the impact of these effects on q and investment in the next section.

4.2 Investment Results

In this section, we show a causal effect of the trade war on U.S. investment of listed firms, using the event component of the stock-price changes. To this end, we begin this analysis by reproducing the standard IK result in levels (equation (8)) using ordinary least squares (OLS) with firm and time fixed effects to allow for differences in the rates of firm-level depreciation and changes in the cost of capital. In column 1 of Table 9, we report results based on our structural equation and see that lagged MTB values are significantly linked to movements in investment rates when estimated using the same time period as in our factor model (2016Q1-2019Q4). The coefficient on our proxy for q, the market-to-book value, is 0.013 and statistically significant. It is standard in the literature to also include cash flow relative to the lagged capital stock to the regression to account for possible non-classical measurement error in our measure of q or the possibility that some firms may be credit constrained. We do so in column 2 and subsequent specifications. Controlling for cash flow lowers the coefficient on MTB to a statistically significant estimate of 0.012. These results are consistent with the Abel and Panageas (2020) finding that in the presence of non-classical measurement error and financial constraints, the coefficient on MTB is biased upwards when one does not control for cash flow and biased downwards when one does. Thus, the true estimate should lie between these two bounds. [11]

We use equations (15) and (17) to estimate ψ^{-1} in the case of either classical measurement error or an endogeneity bias arising from a correlation between MTB value and the error term. This requires us to run the investment rate specification in differences. Because our instrument is lagged four quarters, we can only run the instrumental variables specification over the period 2017Q4-2019Q4. In column 3, we show that a 4-quarter differenced OLS specification (equation 15) run over this shorter sample period results in similar estimates of the coefficient on MTB value ($\widehat{\psi^{-1}}$) as in the levels specification. The next column of Table 9 presents the results from estimating ψ^{-1} using instrumental variables, where we use equation (16) to construct our instruments: the lagged common and differential effects of the firm's stock return in a quarter ($\tilde{r}^G_{f,s-4}$ and $\tilde{r}^D_{f,s-4}$), the lagged idiosyncratic component ($\tilde{\nu}_{f,s-4}$), and the lagged component of returns not explained by the event ($\dot{r}_{f,s-4}$). Comparing the IV estimate of ψ^{-1} in column 4 with the OLS levels specification in column 2 or the first-differenced specification in column 3 reveals that all specifications yield similar estimates of ψ^{-1}.

The results also indicate that our instruments are strong and valid. From the middle panel, we see that the first stage *F*-statistic is 1,635, which is significant at all conventional levels, indicating that we have strong instruments. Since we have four instruments for the change in MTB, we can test the validity of our instrument set. The instruments pass the over-identification test. Moreover, the coefficients on the common and differential effects

[11] For example, in their classic paper, Fazzari et al. (1988) obtained estimates of ψ^{-1} ranging from 0.0008 and 0.0046 depending on the firm type. More recently, Peters and Taylor (2017) using annual data from 1975 to 2011, obtain estimates for ψ^{-1} ranging from 0.017 to 0.035

Table 9: Investment Rate Regression (7-Day Window)

Dep. Var.	$\frac{I_{f,s}}{K_{f,s-4}}$ OLS (1)	$\frac{I_{f,s}}{K_{f,s-4}}$ OLS (2)	$\Delta^4\left(\frac{I_{f,s}}{K_{f,s-4}}\right)$ OLS (3)	$\Delta^4\left(\frac{I_{f,s}}{K_{f,s-4}}\right)$ IV (4)	$\Delta^4\left(\frac{I_{f,s}}{K_{f,s-4}}\right)$ IV (5)	$\frac{I_{f,s}}{K^T_{f,s-4}}$ Cumulant (6)
$\text{MTB}_{f,s-4}$	0.013***	0.012***				0.015***
	(0.001)	(0.001)				(0.003)
$\text{Cashflow}_{fs}/K_{f,s-4}$		0.004***				0.229***
		(0.000)				(0.041)
$\Delta^4\text{MTB}_{f,s-4}$			0.012***	0.009***	0.008***	
			(0.001)	(0.002)	(0.002)	
$\Delta^4\left(\text{Cashflow}_{fs}/K_{f,s-4}\right)$			0.008***	0.008***	0.008***	
			(0.001)	(0.001)	(0.001)	
N	30,780	29,698	16,522	14,390	14,390	21,615
Time FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Overid J-testχ^2				5.87		3.51
[p value]				[0.12]		[0.06]
Weak IV F-test				1,635.0	6,504.7	
First Stage				$\Delta^4\text{MTB}_{f,s-4}$	$\Delta^4\text{MTB}_{f,s-4}$	
$\tilde{r}^C_{f,s-4}$				1.255**		
				(0.542)		
$\tilde{r}^D_{f,s-4}$				3.207***		
				(0.470)		
$\tilde{\nu}_{f,s-4}$				1.001***		
				(0.063)		
$\dot{r}_{f,s-4}$				0.969***		
				(0.012)		
$\tilde{r}_{f,s-4}$					0.973***	
					(0.012)	
First stage F-test				1,635	6,505	
[p-value]				[0.00]	[0.00]	

Notes: $I_{f,s}/K_{f,s-4}$ is the firm's quarterly capital expenditures (investment) relative to its 4-quarter lagged capital stock; $\Delta^4(I_{f,s}/K_{f,s-4})$ is the 4-quarter change in this variable, MTB_{fs} is the firm's market-to-book value, and $\text{Cashflow}_{f,s}/K_{f,s-4}$ is the firm's cash flow divided by its lagged capital stock. Variables are cleaned on top and bottom 1 percentiles. As defined in equation (16), $\tilde{r}^G_{f,s-4}$ and $\tilde{r}^D_{f,s-4}$ are the lagged 4-quarter movement in returns due to the common and differential effects; $\tilde{\nu}_{f,s-4}$ is the lagged 4-quarter movement in returns due to idiosyncratic shocks; and $\dot{r}_{f,s-4}$ is defined in equation (17); and $\tilde{r}_{f,s-4}$ is the lagged 4-quarter movement in returns. The coefficient on cashflow in first stage is not reported. Column 6 implements the Peters and Taylor (2017) estimation procedure in which $K^T_{f,s-4}$ incorporates tangible and intangible capital and a cumulant estimator is used.

($\tilde{r}^{G}_{f,s-4}$ and $\tilde{r}^{D}_{f,s-4}$), which correspond to our estimates of λ_C and λ_D in equation (17), are precisely estimated, indicating that the stock price movements induced by the various U.S.-China tariff actions affected exposed firms' MTBs. The point estimates for λ_C and λ_D are 1.25 and 3.21, respectively, which indicates that for a firm with the mean value of MTB (1.82) that a one percent drop in stock prices due to the common effect lowers MTB values by 0.69 percent while a one percent drop in share prices due to the differential effect lowers MTB values by 1.7 percent. This establishes that movements in stock returns due to the trade war affect firms' MTB, which in turn affects firms' investment rates. In column 5, we estimate an alternative specification in which we use the abnormal return, i.e. the error from the factor model ($\tilde{r}_{f,s-4}$), as an instrument. This also yields a very similar point estimate for $\widehat{\psi^{-1}}$ as we obtained before.[12]

Column 6 of the table replicates the method introduced by Peters and Taylor (2017) to estimate ψ^{-1}. This approach introduces a number of innovations relative to standard ones. First, the dependent variable is no longer investment divided by the lagged book value of property, plant and equipment, but is now investment divided by physical and intangible capital, which incorporates intellectual property investments (e.g., R&D) patents, goodwill, etc. as intangible assets.[13] In addition, this specification also follows Peters and Taylor (2017) in using the cumulant estimator proposed by Erickson and Whited (2002) and Erickson et al. (2014)). The results from this procedure are presented in column 6 of Table 9. As one can see, the estimate of ψ^{-1} is larger, but the difference with the previous estimates is not always significant. However, this higher point estimate in combination with the fact that the sum of physical and intangible capital exceeds the value of just physical capital, would result in a larger aggregate investment impact using equation (18) Therefore, in order to remain conservative, about the impact, we will continue to base our estimate of ψ^{-1} on the value in column 4.

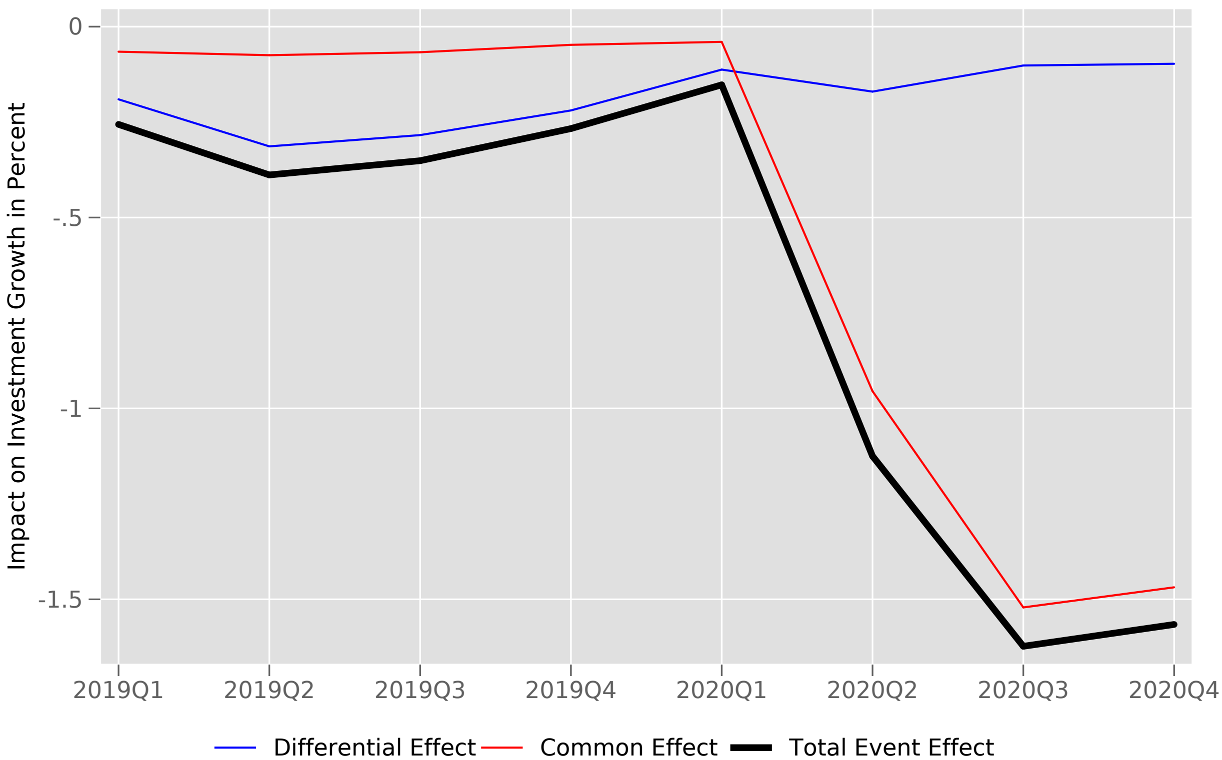
We use equation (18) to compute the implied aggregate effect of these declines as a share of total investment by listed firms and present the results in Figure 4. The plot shows the results of estimating the investment impact given in equation (18) using our baseline estimates for ψ^{-1}, λ_C, and λ_D from column 4 of Table 9. The effect of the trade war does not reach its maximal impact until the end of 2020. In our estimation framework, the impact of tariff announcements on stock prices affects investment four quarters later. Thus, the opening salvos of the trade war in March of 2018 would not have affected investment until the first quarter of 2019. As we saw in Tables 4 and 5, the initial two March announcements of U.S. tariffs produced significant negative differential returns for firms exposed to China, but the common effects almost completely canceled each other. Similarly, Chinese retaliation in April and the U.S. announcement in September of that year had little effect on the market, and we estimate only modest impacts of these announcements on the returns of firms in general or those exposed to China. Thus, most of the impact of the trade war on returns to capital in 2018 was driven by the two March 2018 U.S. tariff announcements, and these mainly affected equity markets by driving down

[12] We also checked for heterogeneous $\widehat{\psi^{-1}}$ across different sized firms, but did not find any significant differences.

[13] We measure intangible capital using the same method as the one described in Appendix B of Peters and Taylor (2017).

the returns of firms exposed to China. Four quarters later, the lower returns to capital for exposed firms reduced their incentives to invest and this fact accounts for why the figure shows a drop in 4-quarter investment growth in the fourth quarter of 2019 of 0.3 percentage points.

Figure 4: Effect of U.S.-China Tariff Announcements on Investment Growth



Note: The figure shows the estimated 4-quarter percentage point decline in investment growth due to the differential and common effect of the trade war based on the estimates from Table 9, column 4, and equation (18).

The larger decline at the end of 2020 can be understood by referring back to our event study results in Tables 4 and 5 where we identified two important escalations in 2019: the May 2019 increase in U.S. tariffs and the Chinese tariff retaliation announcement in August of 2019. The estimates in Table 4 column 5 indicate that the U.S. announcement caused the U.S. market to fall by 2.98 percentage points over a one week window: 1.62 percent due to the common effect and 1.36 percent due to differentially poor performance of firms exposed to China. We estimate that the impact of this announcement did not appear in investment numbers until the second quarter of 2020, which accounts for the steep drop in our estimated impact of the tariff announcements on investment in the latter half of 2020. Similarly, the August 2019 Chinese retaliation announcement was associated with another large negative common effect (and a mild positive differential effect), which we estimate will further depress the rate of investment growth by close to two percentage points in the third and fourth quarters of 2020. By the the fourth quarter of 2020, when managers will have had a chance to fully revise their investment decisions in light of all of the 2019 announcements, we estimate that investment growth will be another 1.6 percentage points lower than without the tariff-induced market movements of 2019. Combining the effects of the 2018 and 2019 announcements implies that the U.S.-China trade war will lower investment in our sample of firms by a total of 1.9 percentage points by the fourth quarter of 2020.[14]

5 Conclusion

This paper develops a method of quantifying the impact of policy announcements on investment rates that makes use of stock market data. We use the fact that by using short event windows around trade policy announcements, we can both identify the total effect of an announcement and the differential effect on exposed firms to show how we can decompose the impact of an announcement into the "common effect" (i.e., the contribution explainable by factors that matter in general) and the differential effect (the contribution explainable by the differential behavior of treated firms). When we apply this theory to the U.S.-China trade war announcements in 2018 and 2019, we find that U.S. and Chinese tariff announcements lowered U.S. aggregate equity prices in our sample of close to 3,000 listed firms by 6.0 percentage points: a $1.7 trillion reduction in market value for our sample of listed firms.

We embed these estimates into a q theory of investment setup in which market-to-book values equal the shadow value of capital. We show that policy-induced reductions in firm share prices lowered U.S. MTB values and these lowered investment rates in our sample of firms. Adverse common factors (e.g., lower macroeconomic growth rates, heightened policy uncertainty, etc.) accounted for half of this fall and the other half was due to the differentially poor performance of firms exposed to China.

Our q model implies that the effect of these declines in market values take time to appear in investment numbers. Since many of the most significant market declines due to the trade war did not appear until the second and third quarters of 2019, their effects will only appear in the middle of 2020. Our estimates indicate that investment growth among listed firms was lowered by 0.3 percentage points in the fourth quarter of 2019, but the depressing effect of the the U.S.-China will shave another 1.6 percentage points off the investment growth rate of listed firms, resulting in a 1.9 percentage point decline over the two years.

There are several caveats to this analysis that should be mentioned. The first is that we only have data for *listed* firms. This means that the national impact might be more negative if unlisted firms, e.g. farmers, were also adversely affected on average or less negative if the tariffs caused new entry into protected sectors like steel and aluminum. The second is that we only have data for *U.S.* firms. A large amount of investment in the U.S. is conducted by foreign multinationals that are listed on other exchanges, which are therefore excluded from our analysis. Many of these foreign multinationals were also adversely affected by the trade war, but these effects, which would matter for the U.S. as a whole, were left out the analysis. Third, we can only estimate the impact of the unan-

[14]Switching to 3-day window produces a larger estimated effect in 2019 and a smaller one in 2020, but the cumulative decline over 2019 and 2020 is actually slightly larger—a decline of 2.0 percentage points by the fourth quarter of 2020.

ticipated component of the announcements. This means that to the extent that markets anticipated the trade war, we are likely to have underestimated the effects. Finally, our analysis only focuses on trade-war announcements related to U.S.-China tariffs largely because we were constrained to focus on seaborne trade. This means that we do not include other important trade war announcements (e.g., retaliation by countries other than China or actions against specific Chinese companies like Huawei). We leave addressing these additional effects to future research.

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