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The Global Impact of Brexit Uncertainty

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ABSTRACT

We propose a text-based method for measuring the cross-border propagation of large shocks at the firm level. We apply this method to estimate the expected costs, benefits, and risks of Brexit and find widespread reverberations in listed firms in 81 countries. International (i.e., non-U.K.) firms most exposed to Brexit *uncertainty* (the second moment) lost significant market value and reduced hiring and investment. International firms also overwhelmingly expected negative first-moment impacts from the U.K.'s decision to leave the European Union (EU), particularly related to regulation, asset prices, and labor market impacts of Brexit.

BREXIT, THE U.K.'S MOMENTOUS DECISION to leave the European Union (EU), exemplifies how political and economic shocks in one country can propagate to affect firms in other countries and across the globe. How exactly these shocks transmit through the world economy, however, is an open question, not least to the policymakers and politicians struggling to find an

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appropriate response.[1] Systematic examination of the impact of events such as Brexit faces the challenge of measuring the extent to which individual firms are exposed to *specific* shocks. We propose to construct such a measure from transcripts of earnings conference calls between firm management and financial analysts when they talk about Brexit, or more generally, any other specific shock (e.g., the Fukushima nuclear disaster, as we briefly illustrate in the Internet Appendix).[2] We demonstrate how a text-based approach can capture a given firm's exposure to the shock and provide a way to decompose the firm's exposure into expected costs, benefits, and risks as assessed by the firm's management and analysts. We then illustrate our approach with a comprehensive empirical analysis of how U.S. and other international (i.e., non-U.K.) firms responded to the Brexit referendum shock and provide direct evidence of the global repercussions of Brexit uncertainty.

Several recent papers aim to estimate the effects of Brexit on U.K.-based firms (e.g., Sampson (2017), Graziano, Handley, and Limão (2021), Broadbent et al. (2019)).[3] Notably, Bloom et al. (2019) use detailed surveys among U.K. businesses to isolate the impact of Brexit uncertainty within the United Kingdom. However, absent the ability to conduct similar surveys at thousands of international firms, considerable challenges confront attempts to quantify the effects of Brexit uncertainty on firms outside the U.K. (and, for that matter, the cross-border propagation of any shock).

First, the intricacies of global trade relations complicate measurement of any firm's exposure to a shock in this interweaved network. Regulatory hurdles, barriers to product market access, and frictions in managing relationships with customers and suppliers, may be among the many ways a shock in one country can impact firms in other geographies. Financial statements and other corporate regulatory filings may not reflect many of these economically significant but potentially indirect exposures. Second, while stock returns in response to a given event (such as the outcome of the Brexit vote) can hint at winners and losers, asset prices are generally silent on *why* a particular firm is affected by the event. Firms' stock prices may respond to increases in risk (the second moment) in the same way they respond to bad news (the first moment), preventing researchers from teasing out the effects of the uncertainty generated by Brexit. This distinction is crucial, however, for formulating an effective policy response. Third, a firm's exposure to Brexit is not constant over time. When researchers infer exposure from isolated events, they are likely to

[1] Witness, for example, President Macron's comment that he would rather have a "no-deal" Brexit than continued uncertainty troubling the French economy (Waterfield, Wright, and Zeffman (2019)).

[2] The Internet Appendix is available in the online version of the article on *The Journal of Finance* website.

[3] Other papers documenting the negative impact of Brexit on U.K. investment, employment, wages, trade, lending, and competition include Born et al. (2019), Berg et al. (2021), Van Reenen (2016), Breinlich et al. (2018), Davies and Studnicka (2018), Dhingra et al. (2017), Garetto, Oldenski, and Ramondo (2019), Costa, Dhingra, and Machin (2019), McGrattan and Waddle (2020), and Steinberg (2019).

fall short in understanding the dynamics of Brexit's impact. As a case in point, the prolonged political process stemming from the 2016 referendum yielded a series of potential negotiation outcomes, with the implications of each varying across firms. Put differently, a firm may appear to be a Brexit "winner" one day, only to find itself in a disadvantaged position the next. Isolating the global impact of Brexit uncertainty thus requires separately tracking international firms' first- and second-moment exposures over time.

Our study addresses each of these challenges. Using natural language processing, we propose a general text-based method for isolating first- and second-moment impacts stemming from specific shocks. Our approach identifies the exposure of firms to a given shock (in this case, Brexit) by counting the number of times the event is mentioned in a given firm's quarterly earnings conference call. These earnings calls usually occur together with an earnings release and are an opportunity for management to describe the company's current affairs. After the management's presentation, analysts query management on challenges the firm is facing. We expect managers and analysts to devote more time to events of greater importance to the firm, in which case the time spent discussing an event is a powerful measure of a firm's exposure to it. Further, since participants in these earnings calls are arguably among the foremost experts on the firm, any significant impact of Brexit on a firm's financial, product, and labor markets will likely come up in these calls. Thus, earnings calls allow us to identify market-assessed, over-time variation in Brexit exposure from the moment that talks of a Brexit referendum began (before 2016) until the present. Indeed, our method allows us to track changes in firm-level Brexit exposure (due to, e.g., developments in EU-U.K. negotiations) without needing to conduct surveys of executives in multiple countries. Finally, we adapt the method developed by Hassan et al. (2019) to separate our measure of Brexit exposure into its first moment (*BrexitSentiment*) and second moment (*BrexitRisk*). Specifically, we rely on call participants' use of "risk" and "uncertainty" synonyms near the term "Brexit" to measure *BrexitRisk* and their use of positive- and negative-tone words near "Brexit" to capture *BrexitSentiment*. By disentangling risk and sentiment, we take an important first step in providing evidence on the mechanisms at play in a firm's response to a significant shock, that is, on the extent to which first- versus second-moment effects explain firm policy outcomes. Further, our text-based approach allows us to investigate the nature of Brexit-related impacts by identifying the exact concerns that call participants raise when discussing Brexit.

Using these new measures, we document novel empirical findings on the impact of Brexit on firms in 81 countries. While these findings validate our measures of Brexit shock exposure, they are also significant in their own right. For example, not only do we document that concerns about Brexit became more pronounced for U.K. firms in the second quarter of 2019 when a no-deal Brexit became a possibility, but we also show that concerns about Brexit-related risks were widespread among non-U.K. firms. For instance, on average, Brexit risk is discussed significantly more for Irish firms than U.K. firms, and Brexit risk is a concern for firms as far afield as the United States, South Africa, and

Singapore. Also of note is the fact that U.K. and non-U.K. firms overwhelmingly expect negative consequences from Brexit. When we aggregate *BrexitSentiment* to the country level, no single country has a significantly positive average. Only in tax havens such as the Channel Islands is average sentiment toward Brexit positive, although the effect is not statistically distinguishable from zero.

Through a human audit of a large number of text fragments from earnings calls that mention Brexit, we next determine the content of the associated discussions. We find that the topics most frequently raised in discussions of Brexit risk relate to consumer confidence, logistics and transition costs, asset prices, trade deals, and regulatory issues. Interestingly, discussion of all of these topics is associated with a significantly negative tone, suggesting more losers than winners in each case, even among U.K. firms. The most negatively toned discussions focus on Brexit's impact on regulation, asset prices, and the labor market. In the most positively toned text fragments, managers express relief that their firm has little exposure to Brexit. Consistent with this generally negative outlook, we find little to no discussion about the major economic benefits touted by the Vote Leave campaign (such as looser regulation or better trade deals), even for U.K.-based firms.

We next examine how U.S. and other international firms respond to their Brexit shock exposure. Using our time-varying firm-specific measure, we show that, up to the end of our sample period in December 2019, Brexit exposure mostly affects firm-level actions through risk, as opposed to sentiment. Specifically, we document meaningful negative effects of *BrexitRisk* on firms' investment and employment decisions.[4] For example, we estimate that in response to Brexit risk, on average Irish firms decreased investment by 2.53% and net hiring by 3.75%, relative to the mean in each of the first three years after the Brexit referendum. For U.S.-based firms (which on average are as exposed to Brexit as Greek firms), reductions in average investment and net hiring rates are 0.33% and 0.86%, respectively.

Although we lack a formal instrument for Brexit exposure, we address the three most plausible challenges to a causal interpretation of these results. First, corporate executives may use Brexit and Brexit risk as an excuse to justify poor performance. Second, firms exposed to Brexit risk may also be more exposed to other types of risks that explain changes in firms' investment and employment. Third, firms doing business with the United Kingdom. may be systematically different from other firms. We investigate these alternative interpretations of our findings in a range of robustness checks and placebo experiments but find little supporting evidence. For example, our estimates remain unchanged when we control for measures of firms' current performance and hence executives' incentives to engage in "cheap talk" about Brexit. Similarly, our results remain unchanged when we control for the time spent discussing risks unrelated to Brexit and for the firm's exposure to trade policy

[4] Bansal et al. (2019) show that uncertainty shocks also entail a first-order negative impact on medium-term investment.

risk. Adding further controls for possibly unobserved heterogeneity correlated with Brexit and investment or hiring does not change our results.

We supplement these analyses with two additional pieces of evidence. First, we investigate how stock markets reacted to the outcome of the U.K.'s Brexit referendum on June 23, 2016. We show that *BrexitSentiment* is positively associated with stock returns in a narrow window around the referendum date, whereas the association with *BrexitRisk* is significantly negative. In other words, both first- and second-moment exposure to Brexit is quickly impounded into stock prices after the announcement of the referendum result. The market thus prices international firms' Brexit-related costs, opportunities, and risks.

Second, we examine whether the average Brexit exposure of firms in a given U.K. district is associated with the share of that district's electorate who voted to leave the EU in the 2016 referendum. Our findings show that constituents who live closer to the firms most negatively affected by Brexit tended to vote to remain in the EU.

Taken together, the evidence suggests that prior to the U.K.'s actual withdrawal from the EU in January 2020, the Brexit referendum vote mostly affected firm actions through increased uncertainty. While stock markets recognized and priced both Brexit sentiment and Brexit risk, the first-moment effects of Brexit on firm actions had not yet been realized. That is, firms' real decisions were predominantly a response to increased uncertainty rather than to changes in the mean of their exposure to the Brexit shock (i.e., whether the shock is good or bad news for the firm). In this sense, our analysis suggests that many of Brexit's (negative) first-moment effects will materialize over a longer period.

Related literature. Our work relates to a large literature on the spillover of shocks across borders and on "contagion." A long-standing idea in this literature is that an uncertainty shock from one region can affect valuations and investment across the world (Forbes and Warnock (2012), Rey (2015), Maggiori (2017), Colacito et al. (2018)). Our work documents a concrete and well-identified example of such a spillover, with uncertainty in the U.K. affecting valuations, investment, and other precautionary behavior in the United States and other countries. We believe that it represents the first example of such a transmitted shock to uncertainty identified in firm-level data.

Our work also relates to a wider literature that documents the transmission of specific natural disasters or credit supply shocks across borders using data on subsidiaries or customer-supplier networks (e.g., Schnabl (2012), Barrot and Sauvagnat (2016), Boehm, Flaaen, and Pandalai-Nayar (2019), Anderson, Du, and Schlusche (2019), Braggion, Manconi, and Zhu (2020), Carvalho et al. (2021)). We contribute to this literature by providing a broadly applicable text-based methodology for capturing the transmission of a wide range of large shocks that flexibly accounts for important cross-firm dependencies that include, but are not limited to, customer-supplier or lender-borrower relationships.

A large and growing body of work argues that variation in uncertainty affects asset prices, international capital flows, investment, employment

growth, and the business cycle (Belo, Gala, and Li (2013), Gourio, Siemer, and Verdelhan (2015), Handley and Limão (2015), Kelly, Pástor, and Veronesi (2016), Koijen, Philipson, and Uhlig (2016), Baker, Bloom, and Davis (2016), Besley and Mueller (2017), Mueller, Tahbaz-Salehi, and Vedolin (2017)). This literature has relied on identifying variation in aggregate and sector-level risk using country-level indices, event studies, and textual analysis of newspapers. We add to this literature by proposing a general text-based method for identifying variation in expected costs, benefits, and uncertainty stemming from specific events, policies, and other shocks at the firm level. In doing so, we take an important step toward causal identification of the effects of these shocks.

Our work complements contemporaneous studies that quantify the impact of Brexit on U.K.-based firms (Dhingra et al. (2016), Dhingra et al. (2017), Sampson (2017), Broadbent et al. (2019), Berg et al. (2021), Graziano, Handley, and Limão (2021)) or on EU trade partners (Graziano, Handley, and Limão (2020, 2021)) and financial markets (Belke, Dubova, and Osowski (2018), Berg et al. (2021)). Bloom et al. (2019) conduct a large-scale survey of decision-makers in U.K. firms to measure Brexit exposure and its associated (negative) impact on investment and productivity. While we also find economically meaningful negative consequences for U.K. firms, we highlight the economic consequences of Brexit uncertainty for firms outside of the United Kingdom.[5]

Finally, we add to the growing literature in macroeconomics and related fields that uses text as a source of data (Gentzkow, Kelly, and Taddy (2019)). Our work highlights the versatility of text-based measurement of firm-time–specific variables, adding to recent studies that use transcripts of earnings conference calls and corporate filings of U.S. firms to measure firm-level political and nonpolitical risk (Hassan et al. (2019)), overall risk (Handley and Li (2018)), climate change exposure (Sautner et al. (2023)), cyber risk (Jamilov, Rey, and Tahoun (2021)), country risk (Hassan et al. (2021)), tax policy expectations (Gallemore et al. (2021)), and trade policy risk (Caldara et al. (2019), Kost (2019)). Others use newspapers to capture economic policy uncertainty (Baker, Bloom, and Davis (2016)), disaster concerns (Manela and Moreira (2017)), and the state of the economy (Bybee et al. (2019)). Recent work analyzes news about monetary policy by applying text-based analysis to policymakers' meeting minutes (Hansen, McMahon, and Prat (2017)).

I. Data

Our primary data comprise transcripts of quarterly earnings conference calls held by publicly listed firms. From Refinitiv EIKON, we collected the complete set of 162,380 English-language transcripts from 2011 through 2019, covering 8,177 firms headquartered in 81 countries. Firms host these conference calls together with their earnings announcements, allowing financial analysts and

[5] Campello et al. (2020) document investment and hiring effects of Brexit for a sample of U.S. firms exposed to the U.K. economy. Martin, Martinez, and Mejean (2019) consider the costs related to Brexit for French exporters.

other market participants to ask questions about the firm's financial performance over the past quarter and to discuss current affairs more broadly with senior management (Hollander, Pronk, and Roelofsen (2010)).[6] As shown in Panel A of Table IA.I in the Internet Appendix, of the 8,177 unique firms in our sample, 1,462 are headquartered in EU countries (428 in the United Kingdom), 3,948 in the United States, and 2,767 in the rest of the world. Panel B of Table IA.I shows the extensive global coverage of listed firms in our sample. This coverage is important because Brexit exposure is likely not limited to firms headquartered in the United Kingdom or in adjacent countries—firms may have subsidiaries, suppliers, customers, competitors, or shareholders in the United Kingdom, or they may use U.K. facilities as a hub for hiring or communication. Of the roughly 3,948 U.S.-based firms, 1,634 have disclosed establishments in the United Kingdom.[7]

Financial statement data, which include information on employment, investment, sales, and earnings, are taken from Standard & Poor's Compustat North America (U.S.) and Global (non-U.S.) files. Stock return data come from the Center for Research in Security Prices (CRSP) and Refinitiv Datastream. Data on U.K. subsidiaries are sourced from ORBIS. U.K. district voting results on the Brexit referendum as well as basic demographic data on these districts come from the Office for National Statistics. Details on these data sources and variable construction are in the Internet Appendix.

II. Measuring Firm-Level Brexit Exposure, Risk, and Sentiment

To create a time-varying measure of a firm's Brexit *exposure*, we parse the firm's earnings call transcripts and count the number of times the word "Brexit" is used.[8] We then divide this number by the total number of words in the transcript to account for differences in transcript length,

$$BrexitExposure_{i,t} = \frac{1}{B_{i,t}} \sum_{b=1}^{B_{i,t}} 1[b = Brexit], \tag{1}$$

[6] Alternatively, we could have used firms' annual reports (SEC Form 10-K) as a text source (see, e.g., Campello et al. (2020)). We decided against using annual reports as a text source because doing so would have limited our investigation to the impact of Brexit on U.S.-listed firms only, rather than on the global sample of international firms that we are interested in.

[7] About 3.5% of the transcripts in our sample indicate that they contain content that has been translated into English. These translations could be problematic for our analysis if, for linguistic reasons, translated discussions of Brexit were systematically more positively or negatively toned than the original. We examine this possibility in Table IA.II in the Internet Appendix, where we compare translated and nontranslated earnings calls held by firms headquartered in the same country (column (2)). Subject to our standard vector of controls, we find no significant differences in either Brexit sentiment or Brexit risk. Moreover, removing these translated transcripts from our analysis has no perceptible effects on our main results.

[8] Google Trends shows the first use of the term "Brexit" in October 2012. Its usage increased in January 2016 and peaked in June 2016. "Br*i*xit" was proposed as an alternative term but did not have meaningful volume on Google Trends in the sample period.

where $b = 0, 1, \ldots, B_{i,t}$ are the words contained in firm i's earnings call in quarter t.[9]

A key challenge to isolating the effect of Brexit-related uncertainty is that Brexit's first- and second-moment impacts are likely correlated. For example, a French exporter may worry about future tariffs on her U.K.-bound exports and expect her business to be less profitable (lower conditional mean) in addition to having higher variance (the tariffs may or may not materialize). Thus, teasing out the effects of Brexit-related uncertainty on a firm's actions also requires controlling for Brexit's impact on the conditional mean of the firm's future earnings.

To separate such first- and second-moment impacts, we next construct measures of Brexit risk and sentiment by conditioning our word counts on proximity to synonyms for risk or uncertainty and positive- and negative-tone words, respectively. Following the procedure in Hassan et al. (2019), we define

$$BrexitRisk_{i,t} = \frac{1}{B_{i,t}} \sum_{b=1}^{B_{i,t}} \left\{ 1[b = Brexit] \times 1[|b - r| < 10] \right\},$$

where r is the position of the nearest synonym of risk or uncertainty. To capture risks associated with Brexit, we count mentions of "Brexit" that occur within a 10-word neighborhood of a synonym for "risk" or "uncertainty" from the *Oxford English Dictionary*.[10] To aid interpretation, we standardize *BrexitRisk* by the average *BrexitRisk* for U.K.-headquartered firms as measured in the period after 2015. A value of one thus denotes the average Brexit risk of U.K. firms between 2016 and 2019.

To determine whether Brexit is good or bad news for a firm (i.e., its first-moment impact), we follow the same procedure but now condition on proximity to positive- and negative-tone words, as obtained from the Loughran and McDonald (2011) sentiment dictionary[11]:

$$BrexitSentiment_{i,t} = \frac{1}{B_{i,t}} \sum_{b=1}^{B_{i,t}} \left\{ \{1[b = Brexit] \times \left(\sum_{c=b-10}^{b+10} S(c) \right) \right\},$$

[9] This procedure can be easily modified to obtain counts of variations on Brexit (e.g., "hard" or "soft" Brexit) and of other phrases that have become meaningful in the aftermath of the Brexit referendum (e.g., "no deal" or "WTO terms").

[10] Table IA.III in the Internet Appendix provides a list of these synonyms. We exclude "question" and "questions" from this list as call moderators often ask for the "next question".

[11] Among all of the synonyms for risk or uncertainty used in our sample of earnings conference calls, 13 risk synonyms also have a negative connotation (rather than simply indicating the existence of risk). These synonyms include "hazardous," "erratic," "danger," "dangerous," "risky," "doubt," and "fear." Our measures thus explicitly allow speakers to simultaneously convey the existence of risk and negative sentiment. Empirically, when we include both *BrexitRisk* and *BrexitSentiment* in a regression, any variation that is common to both of these variables (as a result of overlapping words) is not used to estimate parameters of interest. Thus, in principle, overlap does not interfere with our ability to disentangle *BrexitRisk* from *BrexitSentiment*.

where S indicates sentiment, such that $S(c)$ equals $+1$ if $c \in \mathbb{S}^+$, -1 if $c \in \mathbb{S}^-$, and zero otherwise. Positive-tone words include "good," "strong," and "great," while negative-tone words include "slowdown," "decline," and "difficult."[12,13] Tables IA.IV and IA.V in the Internet Appendix list the most frequently used tone words in our corpus.[14] As we do for *BrexitRisk*, we standardize *BrexitSentiment* by the average *BrexitSentiment* for U.K.-headquartered firms after 2015. A value of -1 thus indicates the average Brexit sentiment of U.K. firms between 2016 and 2019.

For use as control variables and in robustness checks, we also construct measures of each firm's non–Brexit-related risk and sentiment following the procedure above, where $\mathbb{R}$ is the set of synonyms for risk and uncertainty taken from the *Oxford English Dictionary*:

$$NonBrexitRisk_{i,t} = \frac{1}{B_{i,t}} \sum_{b}^{B_{i,t}} \{1[b \in \mathbb{R}]\} - BrexitRisk_{i,t},$$

and

$$NonBrexitSentiment_{i,t} = \frac{1}{B_{i,t}} \sum_{b}^{B_{i,t}} S(b) - BrexitSentiment_{i,t}.$$

III. Validation

A. Global Exposure to Brexit

In this section, we explore the properties of our newly created measures to corroborate that they capture firm-level variation in corporate exposure to Brexit. We first show that firms' *BrexitExposure* is significantly correlated with observable business links to the United Kingdom. We then consider the constituent parts of *BrexitExposure* separately, describing (in detail) the patterns of both *BrexitRisk* and *BrexitSentiment* over time and across countries. Finally, to further validate our method, we present the results from an extensive human reading of text fragments ("snippets") in which Brexit is mentioned to determine the content of the associated discussions.

A.1. Brexit Exposure

Table I presents estimates from cross-sectional regressions of the mean *BrexitExposure* of each firm across time, $\overline{BrexitExposure}_i$, on firm-specific characteristics that are ex ante likely to affect a firm's exposure to Brexit. In particular, we consider the geographical location of the firm's operational headquarters and establishments as well as the proportion of total (worldwide) sales

[12] We sum across positive and negative sentiment words rather than simply condition on their presence to allow multiple positive words to outweigh the use of one negative word, and vice versa.

[13] One potential concern with this kind of sentiment analysis is the use of negation, such as "not good" or "not terrible" (Loughran and McDonald (2016)). However, in our human audit of snippets,

Table I
Validation of $\overline{BrexitExposure_i}$

This table reports OLS estimates from cross-sectional regressions that use $\overline{BrexitExposure_i}$ as the dependent variable. We use 84,297 earnings calls between 2016Q1 and 2019Q4 to calculate firm-level mean Brexit exposure. Robust standard errors are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	$\overline{BrexitExposure_i}$			
	(1)	(2)	(3)	(4)
𝟙{U.K. HQ}	0.860***	0.902***	0.110	0.145
	(0.074)	(0.074)	(0.088)	(0.091)
𝟙{U.K. subsidiary}	0.194***	0.207***	0.244***	0.244***
	(0.018)	(0.018)	(0.022)	(0.021)
𝟙{EU non-U.K. HQ}		0.295***	0.085	0.081
		(0.034)	(0.086)	(0.082)
% of sales in U.K. (2010-2015)			1.838***	
			(0.398)	
% of sales in U.K. (2016-present)				1.751***
				(0.394)
R^2	0.074	0.092	0.128	0.128
N	8,177	8,177	3,533	3,742

earned in the United Kingdom.[15] Because of the stickiness of firm location choice, we average each firm's Brexit exposure across our 2016 to 2019 sample period and report robust standard errors. Columns (1) and (2) in Table I only consider geographical location (and have a larger number of observations), while columns (3) and (4) also include the proportion of U.K. sales. Across specifications, we find a positive association between mean *BrexitExposure* and a firm having a U.K. subsidiary.

The estimated coefficient is about 0.2, implying that foreign firms with U.K. subsidiaries mention Brexit about one-fifth as often as firms headquartered in the United Kingdom. (Recall that our measure of Brexit exposure is normalized so that the average exposure of a U.K. firm during the 2016 to 2019 period is one.) We find a similar positive association between a firm headquartered in the United Kingdom and mean *BrexitExposure*, but the estimated coefficient is sensitive to including the proportion of sales earned in the United Kingdom. We consider two different proxies for U.K. revenues with the first based on U.K. sales reported *before* the Brexit vote while the second is based on the period *after* the vote. We also find that firms headquartered in the EU but outside

we found only a few instances in which inferences were affected by negation. Accordingly, we chose not to complicate the construction of our measures by allowing for it.

[14] The most common positive-tone word used near "Brexit" is "despite"; versions of *BrexitSentiment* constructed with and without "despite" have a correlation of 98.73% and do not affect our results.

[15] We determine headquarters location based on the "Country of domicile" field in EIKON. EIKON also offers "Country of legal registration," which we do not use to determine physical presence.

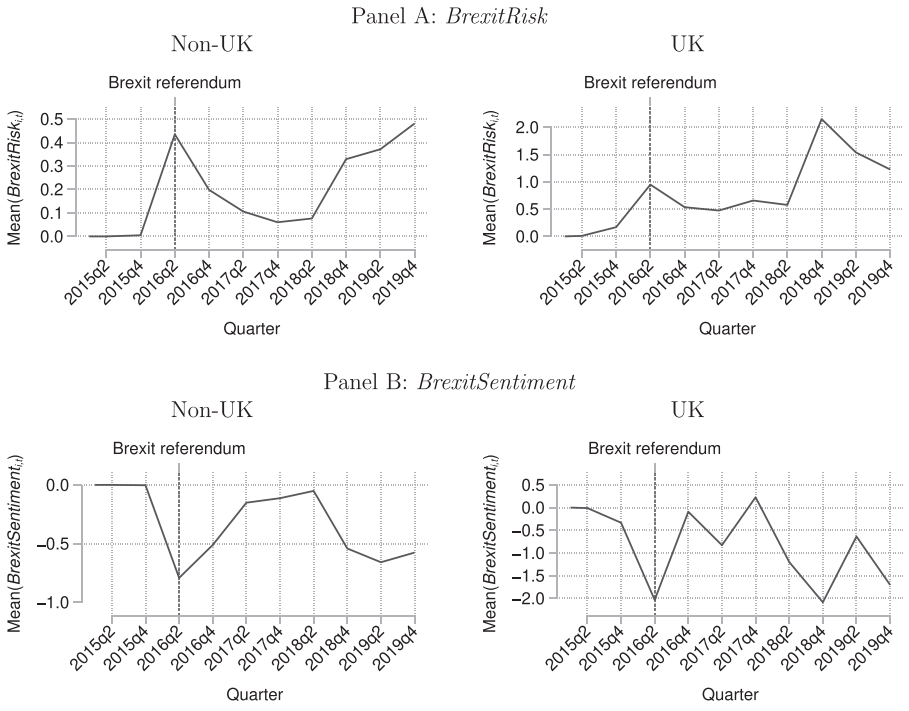

Figure 1. Time-series of *BrexitRisk* and *BrexitSentiment*. This figure plots the semiannual mean of non-U.K.-headquartered (left) and U.K.-headquartered (right) firms' *BrexitRisk* (Panel A) and *BrexitSentiment* (Panel B). BrexitRisk$_{i,t}$ for firm i at time t is normalized using the average *BrexitRisk*$_{i,t}$ of U.K.-headquartered firms over the period 2016 to 2019; *BrexitSentiment*$_{i,t}$ for firm i at time t is normalized using the average $|$*BrexitSentiment*$_{i,t}|$ of U.K.-headquartered firms over the period 2016 to 2019. The Brexit referendum line indicates the quarter in which the referendum was held (2016Q2).

the United Kingdom are more exposed to Brexit than firms with non-EU headquarters. Once more, this effect appears to be subsumed by U.K. sales. Taken together, these findings are consistent with the view that *BrexitExposure* covaries meaningfully with firm characteristics that proxy for the firm being commercially connected to the United Kingdom.

Having offered evidence that supports the validity of our Brexit exposure measure, *BrexitExposure*, we next explore the properties of *BrexitRisk* and *BrexitSentiment*.

A.2. Brexit Risk

Panel A of Figure 1 plots the across-firm average of *BrexitRisk* at each point in time for firms headquartered in the United Kingdom and for firms headquartered in the rest of the world. Consistent with the outcome of the 2016

referendum being a surprise to most parties, we find very low levels of *BrexitRisk* before 2016 in the United Kingdom (right) and in the rest of the world (left).

BrexitRisk increases somewhat in the run-up to the referendum in the first half of 2016. Non-U.K. firms' *BrexitRisk* peaks immediately after the referendum at about 0.4. U.K. firms have a similar peak, with average *BrexitRisk* reaching about one following the referendum.[16] Thus, immediately after the referendum, Brexit risk for international firms reaches almost half the level of the average U.K. firm's Brexit risk in the 2016 to 2019 period. While *BrexitRisk* subsides in 2017, it rises sharply in the second half of 2018, nearly reaching two for U.K. firms and about 0.5 for non-U.K. firms. This time-series pattern closely mimics the negotiation process between the EU and the United Kingdom, particularly at the end of 2018, when the specifics of the deal reached between Theresa May's government and the EU became increasingly clear, as did the difficulties of obtaining parliamentary approval for that deal. In 2019, the prospect of the United Kingdom leaving the EU without a deal (and resorting back to World Trade Organization [WTO] trade terms) became more likely, with uncertainty about Brexit remaining high through the end of our sample period.[17]

Figure 2 shows the average *BrexitRisk* by firm-headquarters country for all countries with nonzero *BrexitRisk* and a minimum of five headquartered firms. (Countries with zero country-level *BrexitRisk* include those far from the United Kingdom, such as Thailand, Nigeria, and Argentina, and some nearer countries for which we have relatively low coverage: Portugal [nine firms] and the Czech Republic [six firms].)

Country-level values are calculated by taking the mean *BrexitRisk* for all firms headquartered in a given country and computing each firm's average *BrexitRisk* using all available post-2015 observations. By construction, the U.K. country-level *BrexitRisk* in this period equals unity. Perhaps the most immediate takeaway from this figure is the position of Ireland with a country-level Brexit risk of 1.74, far greater than the Brexit risk of the average U.K. firm.[18] (This difference is statistically significant; standard errors are reported in Table IA.VI in the Internet Appendix.) Further, geographical proximity to the U.K. matters: other high-scoring countries include nearby Denmark, the Netherlands, France, and Belgium (all EU member states). Non-EU countries

[16] Fisman and Zitzewitz (2019) show a similar (aggregate) pattern for the period between July and December 2016 using their (stock returns-based) Brexit Long-Short Index.

[17] Bloom et al. (2019) report a measure of Brexit uncertainty based on a survey question in the Decision Maker Panel, which asks whether Brexit is a top-three driver of uncertainty. The correlation between *BrexitRisk* and this alternative Brexit uncertainty measure, available for U.K. firms only, is positive and statistically significant.

[18] Interestingly, this finding mirrors the result in Garetto, Oldenski, and Ramondo (2019), who quantify the total welfare effect of Brexit on EU economies. They find that the Brexit shock most reduces purchasing power (i.e., real income) in Ireland. More generally, the literature on geography and trade argues that market and supplier access to neighboring countries is most important for small economies (Redding and Venables (2004)).

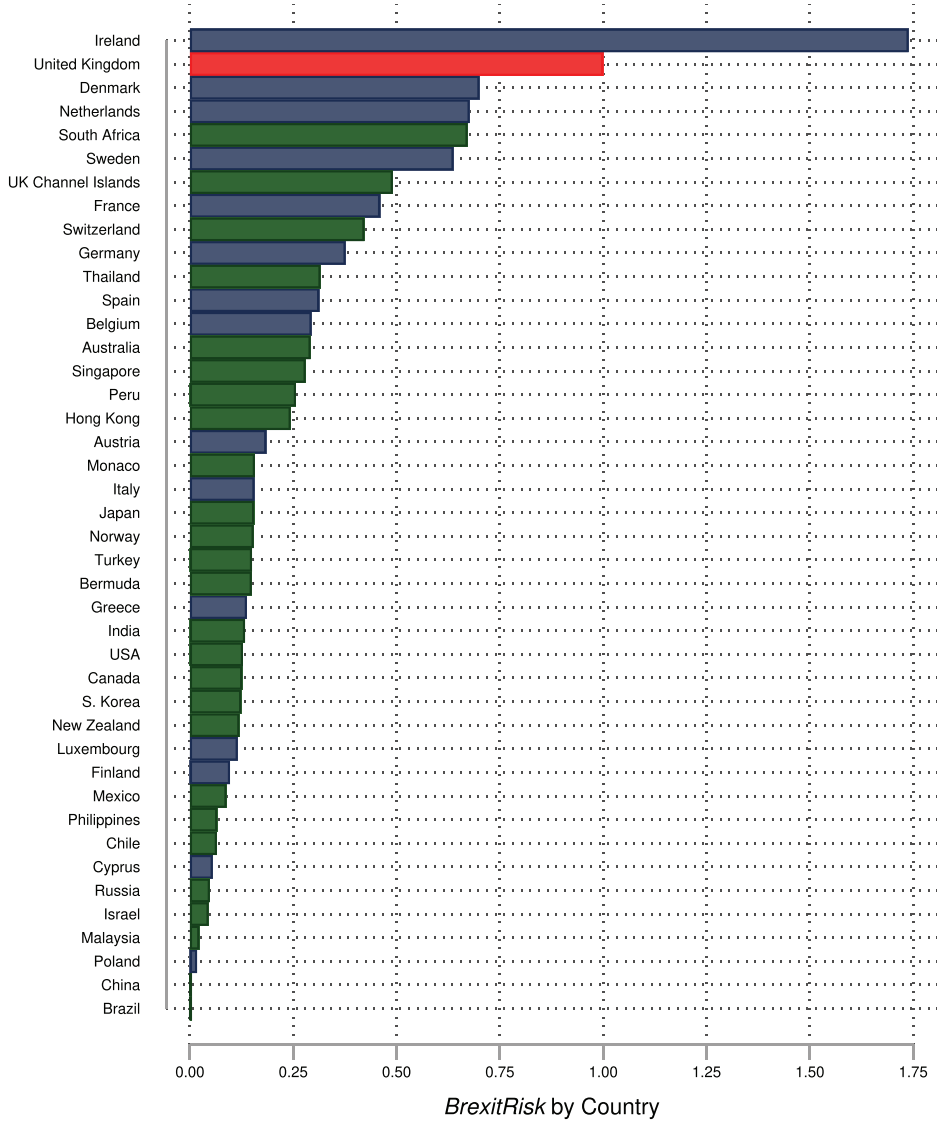

Figure 2. Mean *BrexitRisk* by country. This figure shows the country-level mean of $BrexitRisk_{i,t}$ across all firms headquartered in a specific country. Countries with zero average $BrexitRisk_c$ or countries for which we have fewer than five headquartered firms are excluded. Zero average $BrexitRisk_c$ countries are Puerto Rico, Thailand, Cayman Islands, Portugal, Indonesia, Cyprus, Nigeria, Czech Republic, United Arab Emirates, Argentina, Peru, Phillipines, and Colombia. (Color figure can be viewed at wileyonlinelibrary.com)

with high *BrexitRisk* scores include South Africa, Switzerland, Australia, and Singapore. Many non-EU countries with relatively high Brexit risk scores have longstanding Commonwealth ties to the United Kingdom. The Channel Islands are not part of the Commonwealth, the United Kingdom, or the EU, but

rather are major offshore financial centers and tax havens. Their *BrexitRisk* falls between the scores reported for Sweden and France. In sum, EU-member states appear to have higher country-level Brexit risk than affected countries in other parts of the world. U.S. exposure also appears to be disproportionately high: *BrexitRisk* of the average U.S. firm is 0.13, that is, 13% of the average U.K firm and similar to the average Greek firm.

In Figure IA.1 in the Internet Appendix, we plot the mean *BrexitRisk* by industry for both U.K.- and non–U.K.-headquartered firms. The mean industry *BrexitRisk* is computed by averaging all firms in a particular industry. In almost all industries (Health Services is an exception), the mean *BrexitRisk* is significantly larger in the United Kingdom than in non-U.K. countries. The difference between the United Kingdom and the rest of the world is particularly prominent in the Services and Finance, Insurance, and Real Estate industries.

Finally, we review excerpts of earnings calls discussing Brexit and its associated risks. Table II reports excerpts with the highest *BrexitRisk* among firms with the highest firm-level average *BrexitRisk*. In Panel A, these excerpts are taken from U.K. companies such as Bellway, Millennium & Copthorne Hotels, and Endava. In all cases, the passages confirm that call participants are discussing risks associated with Brexit. For example, in the July 2016 transcript of Berendsen Ltd., management states that "Brexit raises any number of uncertainties for every single business ...." Similarly, in the transcript for the January 2019 call of SThree Plc., firm management states that "there's also a lot of uncertainty around the United Kingdom and Brexit and that will affect most markets." Panel B shows excerpts from companies headquartered outside of the U.K. The top-scoring transcripts come from a range of countries and from across the post–Brexit-referendum sample period. In all cases, we find that the discussion centers on Brexit-related uncertainty faced by the firm in question. For example, in October 2018, management of the Swedish firm Sweco claimed that "there is still an uncertainty when it comes to Brexit and some weakness in the real estate market." Similarly, during their April 2019 earnings call, management of Arjo AB, also headquartered in Sweden, stated that "the entire decline in the quarter came from U.K. where Brexit uncertainty in the last quarter ...."

A.3. Brexit Sentiment

We next repeat the analysis above for *BrexitSentiment*. In Panel B of Figure 1, we start with a plot of the respective time series for U.K. and non-U.K. firms.[19] For both sets of firms, the average *BrexitSentiment* is negative overall. We observe a sharp decrease in sentiment immediately after the Brexit referendum (a phenomenon more pronounced for U.K. firms than international firms), with sentiment scores reverting to slightly below zero for most of 2017. In 2018, average *BrexitSentiment* dropped sharply both in the

[19] In the firm-year panel beginning in 2016, the correlation between *BrexitRisk* and *BrexitSentiment* is −0.3.

Table II

Top *BrexitRisk* Firms' Transcript Excerpts

This table shows transcript excerpts for the top-5 U.K. (Panel A) and the top-10 non-U.K. (Panel B) firms ranked by $\overline{BrexitRisk_i}$, which is calculated as the mean across a firm's available transcripts for earnings calls over the period 2016 to 2019. Synonyms of risk and mentions of "Brexit" are in bold. Country codes: GB = Great Britain (U.K.); U.S. = United States; MU = Mauritius; SE = Sweden; AU = Australia; IE = Ireland; DK = Denmark.

Panel A: U.K. firms

Company	$\overline{BrexitRisk_i}$	Country	Quarter	Transcript excerpts
Bellway PLC	15.34	GB	2018-10	deliver completions in fy we are mindful of the **uncertainty** surrounding **brexit** and we will wait to see whether customer sentiment is affected
Berendsen Ltd	13.06	GB	2016-07	and we have i think a pretty proven resilient business however **brexit** raises any number of **uncertainties** for every single business so were
Endava PLC	11.73	GB	2019-01	plans with us as a result of the **uncertainties** caused by **brexit** mark will talk about how weve mitigated fx **risk** in his
Millennium & Copthorne Hotels PLC	10.9	GB	2018-01	as you know there is still **uncertainty** about british economy and **brexit** for example we are seeing a rise in costs here because
SThree PLC	10.18	GB	2019-01	year theres also a lot of **uncertainty** around the uk and **brexit** and that will affect most markets but i think again the

Panel B: Non-U.K. firms

Company	$\overline{BrexitRisk_i}$	Country	Quarter	Transcript excerpts
Cherokee Inc	43.45	U.S.	2019-04	licensees in europe were hampered by the economic **uncertainty** surrounding **brexit** we think this trend may continue in the future quarters people
Atlantic Leaf Properties Ltd	40.77	MU	2019-10	much has been said about **brexit** and the delays of **brexit** thats on page brexit delays really prolong the **uncertainty** in our

Continued

Table II—*Continued*

Panel B: Non-U.K. firms				
Company	$\overline{BrexitRisk_i}$	Country	Quarter	Transcript excerpts
Northstar Realty Europe Corp	21.15	U.S.	2016-07	rise to greater **uncertainty** this **uncertainty** has been exasperated by **brexit** the **prospect** of **brexit** has resulted in a high degree of
Arjo AB (publ)	20.47	SE	2019-04	the entire decline in the quarter came from uk where **brexit** uncertainty in the last quarter of the nhs financial year has
Apollo Tourism & Leisure Ltd	18.26	AU	2019-07	to what we experienced in north america the **uncertainty** surrounding **brexit** greatly reduced consumer sentiment and suppressed discretionary spending the result of
Sweco AB (publ)	16.6	SE	2018-10	still there is still an **uncertainty** when it comes to **brexit** and some weakness in the real estate market so once again
Ryanair Holdings PLC	14.55	IE	2017-01	the pricing environment has also been affected by the post **brexit** uncertainty which has seen weaker sterling and a switch of charter
Nobia AB	14.03	SE	2019-01	delays in the product deliveries somewhat impacted by increased **uncertainty** for **brexit** but also due to general delays in the construction sites however
Stonegate Mortgage Corp	12.01	U.S.	2016-07	primarily driven by economic concerns abroad in particular **uncertainty** around **brexit** played a major role related to the **instability** of interest rates
Asetek A/S	11.2	DK	2019-10	unresolved china trade issues still an issue for us the **brexit** uncertainty is definitely not helping and its influencing this segment both
Bank of Ireland Group PLC	10.89	IE	2018-07	mortgages weve got the impacts of some **uncertainty** because of **brexit** sentiment in ireland and that has resulted in some of the

United Kingdom and internationally (though, again, the effect is especially pronounced in the United Kingdom). The decline continued well into 2019 for international firms. In the United Kingdom, the figure shows some recovery in the second quarter of 2019, after which average sentiment decreases again as Britain's formal withdrawal from the EU neared.

Figure 3 plots the mean *BrexitSentiment* by country. Overwhelmingly, Brexit-related sentiment in the United Kingdom and elsewhere is negative. Ireland has the strongest negative sentiment scores, even compared to the United Kingdom. Firms from EU member states like Germany, Austria, Italy, Denmark, Sweden, and France also hold strong negative views about the impact of Brexit. The one anomalous area is the Channel Islands, where *BrexitSentiment* is strongly positive with a value of 0.65. Due to the limited number of sample firms headquartered in the Channel Islands (eight), we lack statistical power to distinguish their *BrexitSentiment* score from zero. (Table IA.VII in the Internet Appendix gives standard errors.)

B. Content Analysis of Brexit Discussions in Earnings Calls

The findings above raise the question of what specific risks and news firms associate with Brexit. We address this question by a structured human reading of snippets used to construct *BrexitSentiment* and *BrexitRisk*. To this end, separately for our subsamples of U.K. and non-U.K. firms, we select the 100 firms with the most positive and negative average Brexit sentiment and the 100 firms with the highest average Brexit risk. From these 524 firms (76 firms exhibit high risk and high sentiment) that are most affected by Brexit according to our measures, we extract the three sentences of text surrounding each of the 2,639 mentions of Brexit (in the middle sentence) for our content analysis.

We develop a structured approach to the human reading of these text fragments, modeled on the audit procedure of Baker, Bloom, and Davis (2016). We first conduct a pilot study in which the author team independently reads a sample of snippets to determine relevant topic categories. Reflecting policy discussions on Brexit, we converged on eight specific topics: (i) logistics and transition costs, including any short-run supply disruptions; (ii) asset prices, encompassing all discussions of interest rates, commodities, and property prices; (iii) consumer confidence or customer demand; (iv) foreign exchange (FX); (v) government expenditure; (vi) labor market or employment; (vii) legal or regulatory issues; and (viii) trade deals or market access. We separately track "unspecific" fragments in which no particular topic category is raised, and a residual category containing any other specific discussion of Brexit impacts not reflected in our list of prespecified topics.[20] Table IA.VIII in the

[20] Ex post, the author team read all text fragments classified in this category. This analysis indicated that no frequently mentioned topics other than the eight prespecified topics escaped our attention.

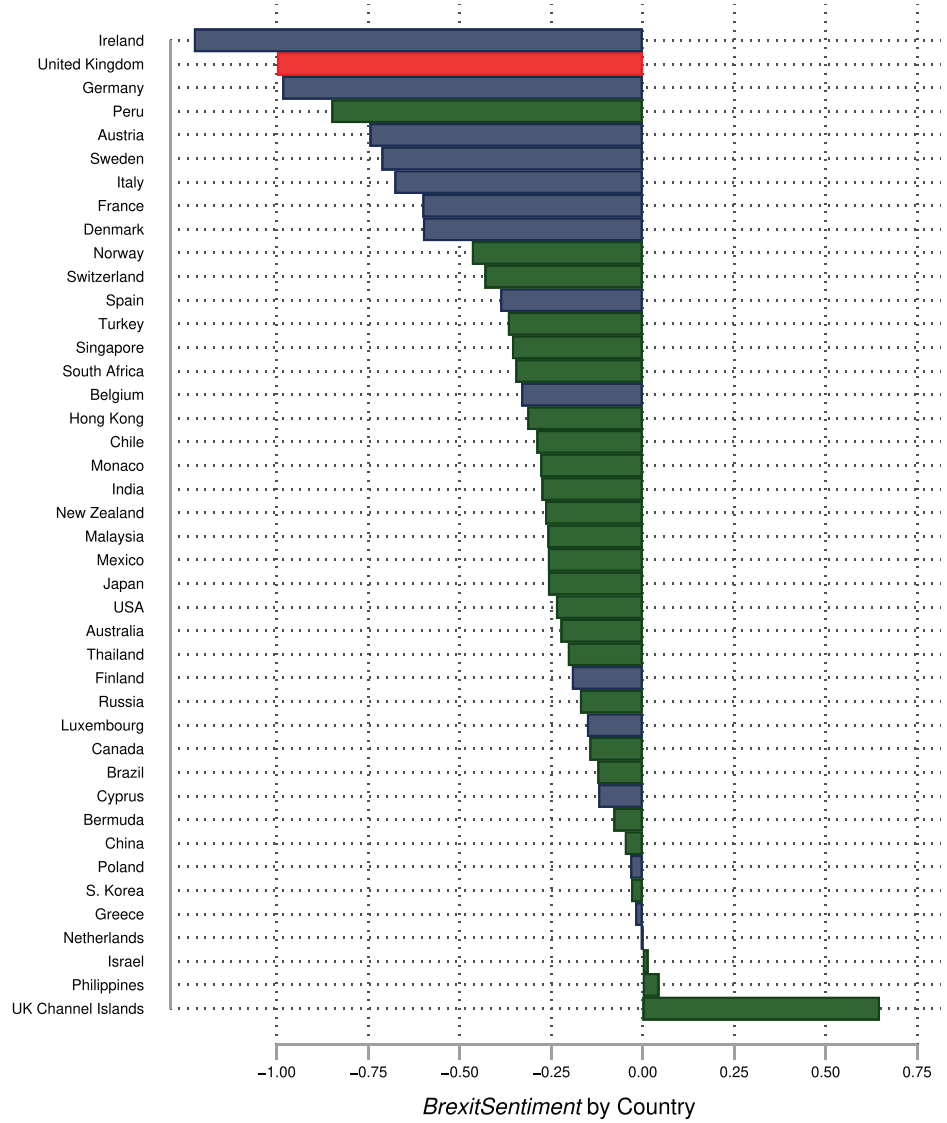

Figure 3. Mean *BrexitSentiment* by country. This figure shows the country-level mean of $BrexitSentiment_{i,t}$ across all firms headquartered in a specific country for the same set of countries as in Figure 2. (Color figure can be viewed at wileyonlinelibrary.com)

Internet Appendix provides illustrative text snippets for each topic for U.K. and non-U.K. firms, our definition of each topic, and a word cloud that shows the frequency of bigrams in classified fragments.

We next wrote an instruction manual for the independent human auditors that we recruited from the Ph.D. program of a U.S. economics department. The manual provides detailed step-by-step instructions on how to classify text

fragments into each of the eight topic categories.[21] The human auditors were then trained in an intensive workshop by the author team before completing their assignments. We also recorded how difficult (easy, medium, hard) the auditors found classifying a given fragment and asked auditors to separately flag fragments in which call participants mention that Brexit has limited or no impact on the firm. Each auditor was assigned 265 fragments, and thus 20% of fragments were assigned to two auditors. Ex post, we find that the auditors agreed on the classifications 77% of the time.[22] For our main content analysis (discussed next), we exclude all fragments that at least one auditor indicated were hard to classify.

The following stylized facts emerge from this human audit. In 59.8% of all text fragments mentioning Brexit risk, auditors found that call participants were discussing at least one of our eight prespecified topics (3.8% of fragments mention two or more). In the remaining 40.2% of fragments, *BrexitRisk* is discussed without specifying a distinct concern, suggesting that in these cases call participants cannot give much color to the specific risk faced by the firm. In the U.K. subsample, this "unspecific" risk category is initially particularly high (51.9% in 2016) and then drops monotonically to 31.9% in 2019, as concerns about Brexit became markedly more specific (see Figure IA.2 in the Internet Appendix).

Panel A of Figure 4 presents the share of each prespecified topic in Brexit-risk discussions. This figure presents topic categories on the vertical axis, while the horizontal axis corresponds to a given topic's share across all specific topics when the firm discusses *BrexitRisk*. To illustrate, *Consumer confidence, demand* is mentioned in about 30% of *BrexitRisk* snippets. Other high-ranking Brexit risk topics include uncertainty related to logistics and transition costs (14.6%), FX (12.1%), other asset prices (10.5%), trade deals (7.4%), and legal or regulatory issues (5.4%).[23]

Panel B of Figure 4 shows the time-variation in these specific concerns driving *BrexitRisk*. We see more pronounced Brexit risks related to asset prices and exchange rates early in the sample. Many of these conversations centered on the value of the British pound, which exhibited substantial volatility following the Brexit vote. Later, when details about the conditions under which the United Kingdom would leave the EU became clearer, the proportion of Brexit-risk discussions related to logistics and transition costs, particularly those related to supply disruptions, rose markedly. We also observe a rise in concerns related to trade deals and negotiations after it became clear that the United Kingdom would not remain in the single market.

In addition to decomposing *BrexitRisk* into these prespecified topics, it is instructive to understand the *degree* to which each topic is associated with risk.

[21] The audit guide is available at our website www.firmlevelrisk.com.

[22] For comparison, in their study on human sentiment classification, Bobicev and Sokolova (2017) find average interauditor agreement of 79.6%.

[23] In Figure IA.3 in the Internet Appendix we find that this pattern is similar when we consider *BrexitExposure* rather than *BrexitRisk*, that is, when we analyze specific topics for text fragments containing the word "Brexit" without conditioning on synonyms for risk or uncertainty.

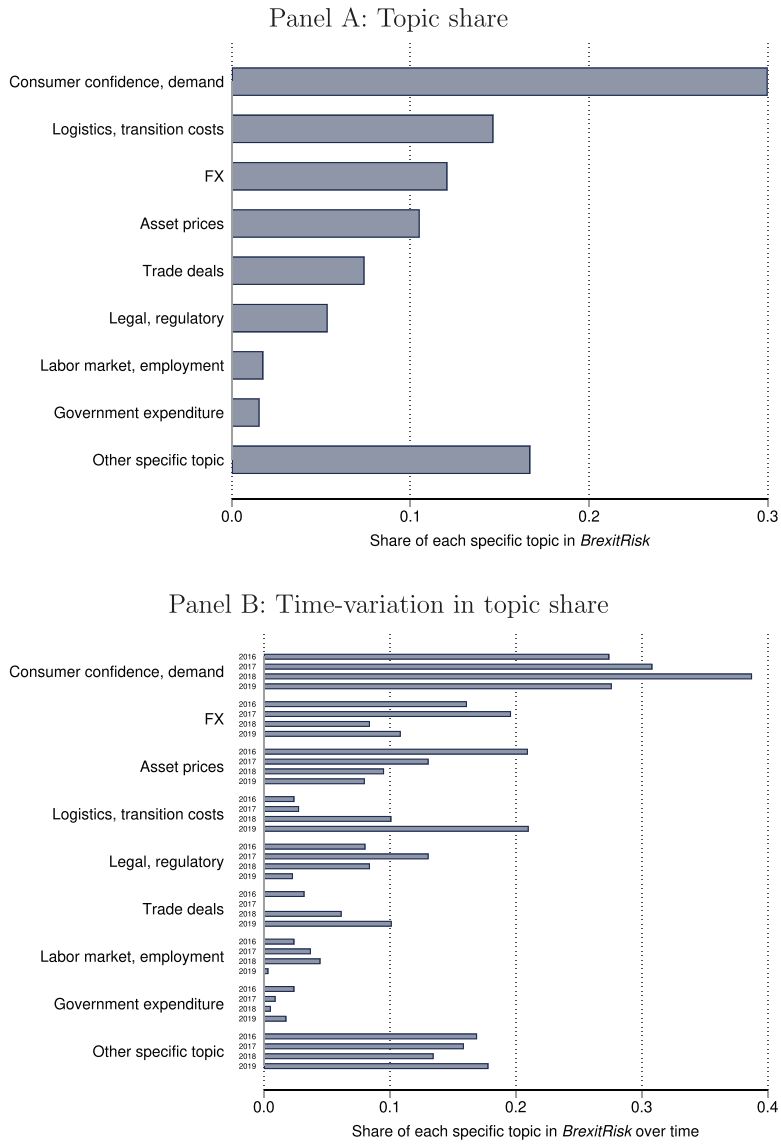

Figure 4. Topic categories in *BrexitRisk* fragments. This figure shows, in Panel A, the share of each pre-specified topic category in *BrexitRisk* fragments (i.e., the proportion of text fragments that mention a specific topic) and, in Panel B, its time variation. For a description of each topic category, see Table IA.VIII in the Internet Appendix and the audit guide available at our website www.firmlevelrisk.com. (Color figure can be viewed at wileyonlinelibrary.com)

Accordingly, we next construct our measure of Brexit risk at the fragment level and project this fragment-level Brexit risk on a vector of indicator variables indicating the discussion of each topic category of interest,

$$y_{i,t,f} = BrexitTopic_{i,t,f}^{'}\zeta + \lambda Limited_{i,t,f} + \delta_{U.K.} + \epsilon_{i,t,f}, \quad (2)$$

where $y_{i,t,f}$ is the count of risk synonyms in Brexit-related text fragment f, $BrexitTopic_{i,t,f}$ is a vector of indicator variables for each topic category of interest (including an indicator for other—i.e., not prespecified—topics or unspecific Brexit concerns), $Limited_{i,t,f}$ is an indicator variable equal to one if the fragment indicates that Brexit has limited or no impact on the firm, and $\delta_{U.K.}$ is a fixed effect for U.K.-headquartered firms.

Panel A of Figure 5 plots each element of ζ and λ, that is, it shows the conditional mean number of risk synonyms per fragment for each of the eight topic areas. The key takeaway from this figure is that referencing each of our eight prespecified topics is associated with significant increase in risk. At the higher end, a reference to Brexit in relation to *Trade deals* and *Consumer confidence, demand* is accompanied, on average, by 0.80 and 0.73 risk synonyms, respectively. At the lower end, references to Brexit in discussions on the labor market and FX concerns increase the frequency of risk synonyms by 0.46 and 0.47, respectively. Brexit references with no specific topic or a topic outside our eight categories are accompanied, on average, by 0.68 ($SE = 0.02$) risk synonyms.[24] In contrast, statements to the effect that Brexit has limited or no impact on the firm reduce, on average, the incidence of risk synonyms by merely 0.13 ($SE = 0.04$). In Figure IA.4 (Panel A) in the Internet Appendix, we partition these estimates into U.K. and non-U.K. firms. We find that, compared with international firms, U.K. firms use significantly more risk synonyms in their discussions of Brexit's relation to consumer confidence and demand, logistics and transition costs, and asset prices. However, these differences are generally relatively small (on average 0.2 to 0.3 additional risk synonyms). Based on this analysis, we conclude that Brexit significantly increases uncertainty at the firm level through a broad range of both specific and unspecific anticipated impacts.

Turning to a firm's sentiment with respect to each Brexit topic, we calculate the Brexit sentiment for each Brexit-related text fragment and project it on the array of indicator variables in equation (2). Panel B of Figure 5 shows a few striking results. First, almost all fragments discussing specific Brexit topics have, on average, a significantly negative tone. We observe the strongest negative sentiment for legal and regulatory issues (-0.78 more negative- than positive-tone words, on average), followed by FX and asset prices (-0.77 and -0.53, respectively), and discussions relating to the labor market and employment (-0.51). Similarly, references to other and unspecific topics related to Brexit have -0.31 ($SE = 0.06$) more negative- than positive-tone words. Notably, the only prespecified topic category with a positive (though not statistically significant) point estimate is government expenditures (0.37, $SE = 0.35$).

[24] Indeed, these differences are not statistically distinguishable from zero.

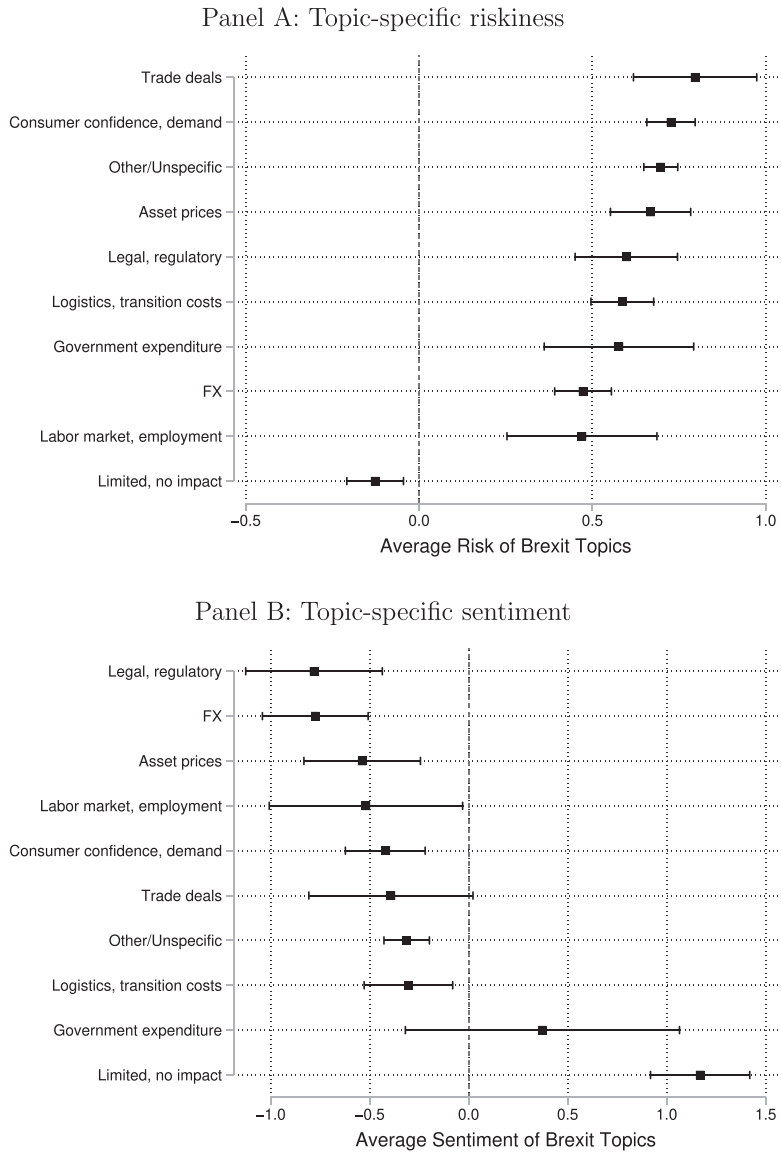

Figure 5. Topic category riskiness and sentiment. This figure shows for each topic category the average risk (Panel A) and average sentiment (Panel B) of Brexit-related discussions. Specifically, Panel A (Panel B) shows, for each pre-specified topic category, the conditional mean number of text fragments in which Brexit is mentioned containing a risk synonym (sentiment words). Here, negative sentiment indicates that topic-specific discussions have, on average, more negative- than positive-tone words. Whiskers indicate 95% confidence intervals. For a description of each topic category, see Table IA.VIII in the Internet Appendix and the audit guide available at our website www.firmlevelrisk.com.

Thus, Brexit-related discussions are only significantly positive when firms indicate that Brexit has little to no impact on their business (1.17, $SE = 0.13$). Interestingly, this negative pattern also prevails in our subsample of U.K.-based firms, where results are broadly similar (see Panel B of Figure IA.4). Specifically, U.K.-based firms have (marginally) less negative discussions related to regulatory issues but significantly more negative discussions about Brexit's relation to government expenditures. There are no other systematic differences in sentiment between these two groups of firms.

It is informative to compare these findings with the pronouncements of the Vote Leave campaign in the debate preceding the Brexit referendum in the United Kingdom. One of the boldest claims was that negotiating with the EU would result in "the easiest trade deal in human history," according to British Secretary of State for International Trade, Dr. Liam Fox. Then Lord Chancellor Michael Gove stated that he foresaw "a free trade area stretching from Iceland to Turkey."[25] Other prominent reasons given for leaving the EU were increased government expenditures and being freed from EU regulation. Remarkably, none of the touted benefits of the Vote Leave campaign are reflected in the sentiment U.K. firms express about these various topics. On the contrary, on average U.K. firms consider leaving the EU bad news for each dimension we consider.

IV. Event Study: Asset Pricing Effects of Brexit

In this section, we consider the implications of the June 23, 2016 referendum to leave the EU on the market valuation of U.K., U.S., and international firms. The outcome of the referendum vote surprised most observers (Fisman and Zitzewitz (2019)), as polling in the preceding months had persistently shown a "Remain" victory (Born et al. (2019)). Famously, Boris Johnson, one of the leading figures of the Vote Leave campaign, went to bed resigned to losing the vote, only to wake up to the sound of demonstrators protesting the vote's outcome at his private residence.[26] The surprise outcome creates favorable conditions for an event study assessing the asset pricing effects of the Brexit referendum. When investors learned about the referendum's outcome, they formed new expectations about publicly listed firms' prospects. Event-period stock price changes should thus reflect changes in investors' expectations about the direct and indirect consequences of Brexit for international firms (Hill, Korczak, and Korczak (2019), Davies and Studnicka (2018)). Correlating the market's assessment with our measures of Brexit exposure, risk, and sentiment also validates our methodology.

[25] Both quotes are reported in https://edition.cnn.com/2019/03/09/uk/brexit-promises-gbr-intl/index.html.

[26] See ITV report on June 24, 2016.

A. Summary Statistics

Table III presents the mean, median, and standard deviation of the variables used in our event study. Columns (4) to (8) provide summary statistics for the subsamples of U.K., international (i.e., non-U.K.), and U.S. firms. The key variables of interest are our Brexit exposure, risk, and sentiment measures. For the purpose of this analysis, we consider both "average Brexit" (denoted by an overline) and "pre-Brexit" exposure, risk, and sentiment variables. The first group of variables is computed by averaging all available Brexit variable scores from 2016 to 2019, while pre-Brexit variables are calculated based on the sample of earnings conference calls held before June 23, 2016, the date of the Brexit referendum.

As can be seen, Brexit exposure, risk, and sentiment are larger in absolute value in the United Kingdom than internationally, regardless of whether they are calculated before or after the Brexit vote. For example, the mean $\overline{BrexitRisk}_i$ for the full sample is 0.232, while for the U.K. sample the corresponding value is equal to one (by construction). Brexit-related sentiment across our sample is, on average, negative. The median values of Brexit-related variables are zero, consistent with analysts and senior management discussing Brexit only when they expect the firm to be impacted. Event-window stock returns are calculated using a window of four trading days starting on June 24, 2016 and ending on June 29, 2016 (since the referendum took place on a Thursday).

B. Regression Results

In Table IV, we present ordinary least squares (OLS) estimates of the specification

$$r_i = \alpha_0 + \delta_s + \beta Brexit_i + X_i^{'}\nu + \epsilon_i, \tag{3}$$

where r_i is the four-trading-day return following the Brexit vote, δ_s are industry fixed effects, $Brexit_i$ represents firm i's $\overline{BrexitExposure}$, $\overline{BrexitRisk}$, $\overline{BrexitSentiment}$, $\overline{PreBrexitRisk}$, or $\overline{PreBrexitSentiment}$, and the vector X_i always includes the log of a firm's assets to control for firm size. In some specifications, we also include a stock's U.S. and U.K. market betas, which we calculated by regressing daily returns in 2015 for firm i on the S&P 500 or on the FTSE 100 index, thus measuring a firm's exposure to the U.S. and the U.K. capital markets, respectively. We exclude firms from the "Non Classifiable" sector. Throughout, we use robust standard errors.

Panel A of Table IV reports estimates for the full sample. In columns (1) and (2), we find a negative coefficient estimate between $\overline{BrexitExposure}$ and event-window stock returns. For a firm with post-Brexit vote exposure equal to that of the average U.K.-headquartered firm (i.e., with a value of one), we find that equity prices drop by 2.8% over the four trading days. The magnitude of the coefficient remains unchanged after controlling for a stock's U.S.- and

Table III
Summary Statistics

This table shows the mean, median, standard deviation, and number of firms for the variables used in the subsequent analysis. Columns (1) to (3) correspond to the sample of all firms, columns (4) and (5) to the sample of U.K. firms, columns (6) and (7) to the sample of non-U.K. firms, and columns (8) and (9) to U.S. firms. *BrexitExposure*, *BrexitRisk*, *BrexitSentiment*, *NonBrexitRisk*, and *NonBrexitSentiment* are calculated, as defined in Section II, for every call transcript by each firm in the sample. In Panel A, $\overline{BrexitExposure_i}$, $\overline{BrexitRisk_i}$, and $\overline{BrexitSentiment_i}$ are averages for each firm in the sample from 2016 to 2019, normalized by U.K.-headquartered firms' 2016 to 2019 mean $BrexitExposure_{i,t}$, mean $BrexitRisk_{i,t}$, and absolute value of mean $BrexitSentiment_{i,t}$, respectively. In Panel B, $\overline{PreBrexitExposure_i}$, $\overline{PreBrexitRisk_i}$, and $\overline{PreBrexitSentiment_i}$ are calculated as in Panel A except using only transcripts of calls held before June 23, 2016 (the day of the Brexit referendum). $Stock\ Returns_i$ are calculated as $\sum_{t=0}^{t=N} log(P_{i,t}/P_{i,t-1})$, where t is at a daily frequency, and [0,N] represents the four trading days following the Brexit referendum starting on June 24, 2016 and ending on June 29, 2016. In Panel C, the sample period for yearly outcomes is 2011 to 2019; $BrexitExposure_{i,t}$, $BrexitRisk_{i,t}$, $BrexitSentiment_{i,t}$, $NonBrexitRisk_{i,t}$, and $NonBrexitSentiment_{i,t}$ are calculated as firm-year averages across all transcripts of calls held by a firm in a year.

	All firms			U.K. firms		Non-U.K. firms		U.S. firms		Total
	Mean	Median	*SD*	Mean	*SD*	Mean	SD	Mean	*SD*	*N*
					Panel A: Firm-level risk and sentiment					
$\overline{BrexitExposure_i}$	0.223	0.000	0.728	1.000	1.517	0.180	0.631	0.129	0.407	8,177
$\overline{BrexitRisk_i}$	0.232	0.000	1.107	1.000	2.058	0.189	1.013	0.128	0.651	8,177
$\overline{BrexitSentiment_i}$	−0.317	0.000	2.541	−1.000	4.057	−0.279	2.425	−0.237	1.480	8,177
					Panel B: Event-study variables					
$\overline{PreBrexitExposure_i}$	0.042	0.000	0.366	0.251	0.716	0.034	0.343	0.022	0.250	4,399
$\overline{PreBrexitRisk_i}$	0.038	0.000	0.478	0.230	1.209	0.030	0.422	0.025	0.377	4,399
$\overline{PreBrexitSentiment_i}$	−0.084	0.000	2.130	−0.335	3.067	−0.074	2.084	−0.033	0.983	4,399
$Stock\ Returns_i$: *June 24–29, 2016*	−0.033	−0.027	0.065	−0.085	0.100	−0.030	0.062	−0.031	0.061	6,077
$NonBrexitRisk_i$	1.078	0.933	0.705	0.896	0.569	1.086	0.710	1.175	0.697	6,077
$NonBrexitSentiment_i$	0.843	0.844	0.444	1.047	0.438	0.834	0.442	0.886	0.425	6,077
					Panel C: Firm-year outcomes					
$BrexitExposure_{i,t}$	0.117	0.000	0.655	0.558	1.484	0.095	0.574	0.067	0.424	52,363
$BrexitSentiment_{i,t}$	0.111	0.000	0.953	0.495	2.042	0.092	0.858	0.063	0.663	52,363
$BrexitSentiment_{i,t}$	−0.162	0.000	2.446	−0.544	5.379	−0.142	2.194	−0.122	1.687	52,363
$NonBrexitRisk_{i,t}$	1.389	1.186	1.000	1.240	1.017	1.396	0.999	1.467	0.945	52,363
$NonBrexitSentiment_{i,t}$	1.609	1.597	1.000	1.951	1.002	1.592	0.997	1.745	0.927	52,363
$I_{i,t+1}/K_{i,t} \cdot 100$	24.467	14.236	41.103	19.800	31.194	24.713	41.544	26.280	43.946	51,387
$\Delta emp_{i,t}/emp_{i,t-1} \cdot 100$	8.123	2.863	29.567	7.227	28.191	8.173	29.642	8.714	30.607	54,860
$\Delta sales_{i,t}/sales_{i,t-1} \cdot 100$	17.666	6.489	70.807	12.204	52.278	17.941	71.602	18.769	73.930	64,024

Table IV
Event Study

This table reports OLS estimation results from cross-sectional regressions of $Stock\ Returns_i$: *June 24–29, 2016* on $\overline{BrexitRisk_i}$ and $\overline{BrexitSentiment_i}$, separately for all firms (Panel A) and for U.S.-headquartered firms (Panel B). Stock returns are calculated as $\sum_{t=0}^{t=N} log(P_{i,t}/P_{i,t-1})$, where t is at a daily frequency, and [0,N] represents the four trading days (including weekend days) following the Brexit referendum starting on June 24, 2016 and ending on June 29, 2016. All other variables are as defined in Table III. All specifications include industry (one-digit SIC) fixed effects. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. These regressions exclude non-U.K. firms with less than seven transcripts in the sample and firms in the "Non Classifiable" sector.

	$Stock\ Returns_i$: *June 24–29, 2016*					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All firms						
$\overline{BrexitExposure_i}$	−0.028***	−0.028***				
	(0.003)	(0.003)				
$\overline{BrexitRisk_i}$			−0.013***	−0.013***	−0.013***	
			(0.002)	(0.002)	(0.002)	
$\overline{BrexitSentiment_i}$			0.002***	0.002***	0.002***	
			(0.001)	(0.001)	(0.001)	
$\overline{PreBrexitRisk_i}$						−0.007***
						(0.002)
$\overline{PreBrexitSentiment_i}$						0.001***
						(0.000)
$\overline{NonBrexitRisk_i}$					−0.003	
					(0.002)	
$\overline{NonBrexitSentiment_i}$					0.001	
					(0.002)	
Constant	−0.015***	0.002	−0.014***	0.002	0.006	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
R^2	0.106	0.151	0.078	0.122	0.123	0.095
N	4,588	4,544	4,588	4,544	4,544	3,823
Panel B: U.S. firms						
$\overline{BrexitExposure_i}$	−0.024***	−0.024***				
	(0.003)	(0.002)				
$\overline{BrexitRisk_i}$			−0.009***	−0.008***	−0.008***	
			(0.002)	(0.002)	(0.002)	
$\overline{BrexitSentiment_i}$			0.003***	0.003***	0.003***	
			(0.001)	(0.001)	(0.001)	
$\overline{PreBrexitRisk_i}$						−0.005**
						(0.002)
$\overline{PreBrexitSentiment_i}$						0.002**
						(0.001)
$\overline{NonBrexitRisk_i}$					−0.004	
					(0.003)	
$\overline{NonBrexitSentiment_i}$					0.004	
					(0.003)	

Continued

Table IV—*Continued*

Panel B: U.S. firms						
Constant	−0.011**	0.008	−0.009*	0.009*	0.013**	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
R^2	0.077	0.132	0.067	0.123	0.126	0.115
N	2,816	2,785	2,816	2,785	2,785	2,534
Beta controls	N	Y	N	Y	Y	Y

U.K.-market beta, implying that the effect is not explained by differences in systematic exposure to U.S. or U.K. market risk.

We then "decompose" Brexit exposure into mean and variance components, that is, we consider how markets priced differential exposure to $\overline{BrexitRisk}$ and $\overline{BrexitSentiment}$ in the time window surrounding the announcement of the referendum result (columns (3) and (4)). As expected, we find that higher Brexit risk leads to lower stock returns (coef. $= -0.013$, $SE = 0.002$). In addition to this second-moment effect, we find that an increase in Brexit sentiment leads to higher stock returns (coef. $= 0.002$, $SE = 0.001$), consistent with the view that firms expecting negative consequences of Brexit lose significant market valuation immediately after the referendum results became known. The results therefore suggest that our text-based measures successfully disentangle valuation effects attributable to first- versus second-moment exposure to Brexit. Column (4) shows that these estimates are unaffected when we control for a stock's U.S. and U.K. market beta. The same is true in column (5) when we add $\overline{NonBrexitRisk}$ and $\overline{NonBrexitSentiment}$ to the specification, that is, when we additionally control for the firm's average 2016 to 2019 uncertainty and sentiment unrelated to Brexit. Finally, in an attempt to estimate the market's response using only the information available at the time of the referendum, in column (6) we use the *PreBrexitRisk* and *PreBrexitSentiment* variables to explain the event-window stock price response. We again find a negative effect of $\overline{PreBrexitRisk}$ (-0.007, $SE = 0.002$) and a positive effect of $\overline{PreBrexitSentiment}$ (0.001, $SE = 0.000$).[27]

In Panel B, we repeat this analysis after restricting the sample to firms headquartered in the United States, which reduces the sample size from 4,588 to 2,816 firms. Our estimates for the U.S.-headquartered sample do not deviate meaningfully from those for the full sample. Indeed, in columns (1) and (2) the coefficient estimates on $\overline{BrexitExposure}$ for the U.S.-headquartered sample are

[27] To corroborate our choice of standard errors, Panel A of Figure IA.5 in the Internet Appendix shows the results of a falsification exercise where we repeatedly regress stock returns from four consecutive trading-day windows between January 1, 2012 and December 31, 2015 on $\overline{PreBrexitRisk}$. The figure provides a histogram of t-statistics on the estimated coefficient on $\overline{PreBrexitRisk}$. The t-statistics are centered around zero, with no noticeable tendency for positive or negative estimates. Reassuringly, the rate of false rejection in the left tail is 3.6%. In Panel B, where we condition on trading days with large market movements more comparable to the Brexit referendum event (trading days for which the average firm gains or loses at least 1%), the false rejection rate drops to 1.2%.

almost identical to those in the corresponding columns in Panel A. When teasing out the two components of exposure to Brexit in columns (3) to (5), we find a slightly stronger stock price response to *BrexitSentiment* and a somewhat weaker response to *BrexitRisk*. Both remain statistically significant at the 1% level. Again, controlling for *NonBrexitRisk* and *NonBrexitSentiment*, as we do in column (5), does not materially affect our inferences.

We further examine the event-study results in Figure IA.6 in the Internet Appendix, which depicts OLS regression estimates of *PreBrexitRisk* (corresponding to column (6) of Panel B in Table IV) onto a sequence of four-day return windows before and after the Brexit referendum vote on June 23, 2016. Specifically, each return window consists of four consecutive trading days, where the actual "treatment" window stretches from June 24, 2016 to June 29, 2016, and the remaining four-day return windows are in the periods before and after the treatment. Reassuringly, we find a statistically significant coefficient estimate on *PreBrexitRisk* only during but not before or after the treatment window, indicating that the referendum outcome was largely unexpected and pivotal, but nevertheless was quickly impounded into prices.[28] These results bolster our confidence that the event-study estimates for Brexit risk are not inadvertently picking up some other omitted factor or confounding event.[29]

Finally, in Figure 6, we estimate the event-study results separately for U.K. and non-U.K. firms. The figure shows binned scatter plots depicting the relation between four-day return windows and *BrexitRisk*. Panel A (Panel B) shows the relation for the sample of U.K. (non-U.K.) firms. The plots are again based on panel regressions that control for *BrexitSentiment*, *log*(*assets*), and sector and time fixed effects. We find a negative relation in both panels (although the slope coefficient is more negative for the U.K. sample), implying that the

[28] Interestingly, we also see a slight uptick (though not statistically distinguishable from zero) in the valuation of high–Brexit-risk firms in the week before the referendum, when the "Remain" campaign was making large gains in the polls, consistent with the analysis of polling averages in Hill, Korczak, and Korczak (2019).

[29] The event-study framework also lends itself to examining the sensitivity of our method to the choice of measurement window when constructing conditional measures such as *BrexitRisk* and *BrexitSentiment*. Recall that a reference to Brexit only counts toward the *BrexitRisk* (*BrexitSentiment*) score if, in a 10-word neighborhood, a risk (sentiment) synonym is present. We repeat the event-study analysis reported in Panel B of Table IV (i.e., using the U.S. sample) and vary the width of the text window (i.e., number of words) around "Brexit." We start with a window of five words and report increments of five words until the window is extended to 25 words. We also consider a text window of three sentences (or "triples"), where the middle sentence contains the word "Brexit." We report our findings in Table IA.IX in the Internet Appendix. In summary, the estimated coefficient on *BrexitRisk* does not vary much across different text windows. At the same time, however, the coefficient on *BrexitSentiment* attenuates as the text window widens, to the point that we lose significance when measured using a sentence triple. We conclude that Brexit risk can be measured by a longer or shorter text window without affecting inferences. For Brexit sentiment, in contrast, extending the text window beyond 10 words introduces more noise. This pattern likely emerges because tone words are used about three times more frequently than synonyms for risk, so longer text windows are more likely to pick up tone words related to non-Brexit issues discussed in the surrounding text.

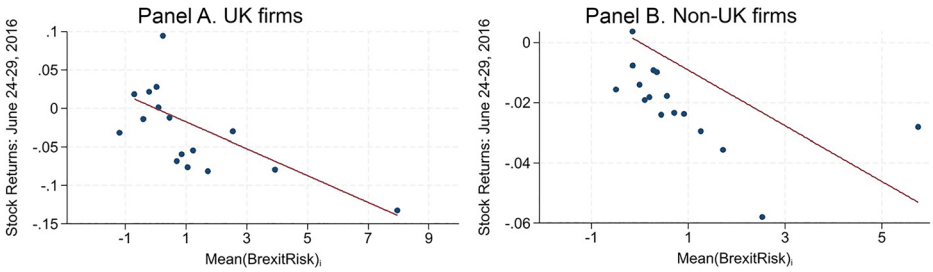

Figure 6. Effect of $\overline{BrexitRisk_i}$ on *Stock Returns*$_i$: *June 24–29, 2016*. The figure shows binned scatter plots and a linear regression for the relation between *Stock Returns*$_i$: *June 24–29, 2016* and $\overline{BrexitRisk_i}$ for firms headquartered in the United Kingdom (Panel A) and outside of the United Kingdom (Panel B). The relation is plotted after controlling for $\overline{BrexitSentiment_i}$, *log(assets)*, and industry (one-digit SIC) and country fixed effects. Standard errors are clustered by firm. Each scatter plot has 16 bins: the first bin has all firm-year observations with zero $\overline{BrexitRisk_i}$; the other 15 bins are equally populated by firm-year observations with nonzero $\overline{BrexitRisk_i}$. (Color figure can be viewed at wileyonlinelibrary.com)

asset price response to Brexit uncertainty is negative for both U.K. and non-U.K. firms.

To summarize, our results show that equity prices quickly impounded firms' first- and second-moment exposures to Brexit. One implication of this finding is that conventional event studies cannot sharply distinguish winners and losers (i.e., first-moment exposure) from those affected by risks (i.e., second-moment exposure) related to the Brexit shock because both sentiment and risk potentially affect stock prices. In contrast, our two text-based measures, $\overline{BrexitSentiment}$ and $\overline{BrexitRisk}$, enable researchers to disentangle the two exposures. Moreover, in the next section, we show that our text-based classification approach also persuasively separates Brexit risk and sentiment from risk and sentiment deriving from other, non–Brexit-related sources. This feature is particularly helpful when studying firm-specific responses to Brexit, as we discuss more in the next section.

V. Firm-Level Effects of Brexit

Two substantive facts emerge from the empirical findings in the previous sections. First, firms are exposed to the shock caused by the Brexit referendum, not just in the United Kingdom but globally. Even though the shock is perhaps strongest in the (nearby) EU countries, it extends as far as the United States, Singapore, and South Africa. Second, equity markets quickly impound the first- and second-moment implications into stock prices. In a four-day return window around the 2016 referendum, firms with high average Brexit risk lose value, while firms with more positive Brexit sentiment increase in stock value. While these findings are consistent with the forward-looking nature of equity markets, they also leave open the question of the precise way in which individual firms respond to the Brexit shock. In this section,

we estimate the effect of firm-level Brexit risk and sentiment on investment, hiring, and sales, using the specification

$$y_{i,t+1} = \delta_s + \delta_t + \delta_c + \beta BrexitRisk_{i,t} + \theta BrexitSentiment_{i,t} + X_{i,t}^{'}\zeta + \epsilon_{i,t}, \quad (4)$$

where $y_{i,t+1}$ is one of the three firm-level outcomes of interest, and δ_s, δ_t, and δ_c are industry, year, and headquarters-country fixed effects, respectively. The vector $X_{i,t}$ includes the log of the firm's assets, to control for firm size, and *NonBrexitRisk* and *NonBrexitSentiment*, to control for non–Brexit-related sources of risk and overall (again, non–Brexit-related) sentiment expressed in the earnings call, respectively. Some specifications also include *Realized volatility* as an alternative proxy for (generic) market-assessed risk. We compute *BrexitRisk*, *BrexitSentiment*, *NonBrexitRisk*, and *NonBrexitSentiment* annually by averaging across all available earnings call transcripts in a given year. Firm-level outcome variables are measured yearly from 2011 to 2019. Descriptive statistics for all firm-level variables are presented in Table III. Inferences are based on standard errors clustered at the firm level.

Before discussing the results of this analysis, we note that it is well recognized, in both theoretical and empirical work, that uncertainty can directly influence firm-level investment and employment (Pindyck (1988), Bernanke (1983), Dixit and Pindyck (1994), Bloom, Bond, and Van Reenen (2007), Gilchrist, Sim, and Zakrajšek (2014)).[30] Recent developments in the literature show that first- and second-moment shocks can appear together, either amplifying or confounding each other (Bloom et al. (2018), Berger, Dew-Becker, and Giglio (2020)). We examine these predictions in the context of Brexit, leveraging the fact that our text-based approach allows us to separate the two.[31]

Figure 7 shows a binned scatter plot of the relation between firm-level capital investment ($I_{i,t+1}/K_{i,t}$) and $BrexitRisk_{i,t}$, controlling for $BrexitSentiment_{i,t}$, $log(assets)$, and sector and time fixed effects. The red (blue) line represents the estimated slope for the sample of U.K. (non-U.K.) firms. In both panels, $BrexitRisk_{i,t}$ is negatively and significantly associated with the capital investment rate. In fact, the estimated coefficients are very similar in magnitude: -0.583 ($SE = 0.249$) for the U.K. and -0.614 ($SE = 0.150$) for the non-U.K. sample. The latter coefficient implies that for each year after 2016, an international firm with a *BrexitRisk* equal to that of the average U.K. firm experienced a 0.614 percentage point decrease in its investment rate relative to an unexposed firm. This effect corresponds to a 2.5% drop relative to the mean (24.5)

[30] In macroeconomic models, an increase in aggregate risk may increase or decrease aggregate investment due to general equilibrium effects on the interest rate (see, e.g., Fernández-Villaverde et al. (2015), Hassan and Mertens (2017)). However, this ambiguity does not usually exist in the cross-section of firms. In models with adjustment costs, a firm facing a relative increase in firm-level risk should always decrease its investment compared to other firms.

[31] Bloom et al. (2019) points out that Brexit presents a persistent uncertainty shock that should have a heterogeneous impact on U.K. firms; the impact depends on firms' prior exposure to the EU. Moving beyond the impact on U.K. firms, we can also estimate the effects of this uncertainty shock on non-U.K. firms or on U.S. firms specifically. We further show that Brexit is not merely an uncertainty shock but also has first-moment implications.

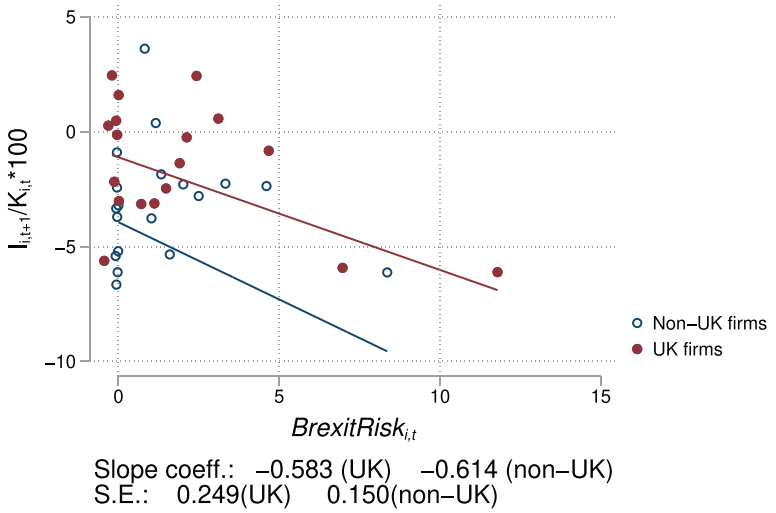

Figure 7. ***BrexitRisk*$_{i,t}$ and firm investment**. This figure shows a binned scatter plot and linear regression for the relation between $I_{i,t+1}/K_{i,t} \cdot 100$ and $BrexitRisk_{i,t}$, separately for U.K. firms (closed circle) and non-U.K. firms (open circle) over the 2011 to 2018 period. The scatter controls for *log(assets)* and industry (one-digit SIC) and year fixed effects. Standard errors are clustered by firm. The scatter plot has 29 bins for U.K. and non-U.K. firms. The first nine bins correspond to all firm-year observations with zero $\text{BrexitRisk}_{i,t}$ grouped by nine one-digit SIC codes; the other 20 bins are equally populated by firm-year observations with nonzero $BrexitRisk_{i,t}$. (Color figure can be viewed at wileyonlinelibrary.com)

or a 4.3% drop relative to the median investment rate of firms in our sample (14.24).

In Table V, we conduct a more systematic analysis of the relation between a firm's capital investment rate and Brexit risk and sentiment. In Panel A, we consider the full sample of U.K. and international firms. Column (1) provides a base specification of our two variables of interest, $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$, controlling for $log(assets)$, without any fixed effects. As expected, we find a significant negative association between $BrexitRisk_{i,t}$ and the capital investment rate (-0.741, $SE = 0.126$). Firms most affected by Brexit-related risks thus lower their investment rates, consistent with the effects of an uncertainty shock that raises the option value of delaying investment. Interestingly, we find no significant association between $BrexitSentiment_{i,t}$ and $I_{i,t+1}/K_{i,t}$, which suggests that firms for which Brexit is purely good or bad news do not appear to be reacting systematically to this news before the U.K.'s exit from the single market.

The next four columns work toward our preferred specification. Column (2) adds time and sector fixed effects. Column (3) adds the interaction between headquarters-country and time fixed effects, thus controlling for any systematic movement in exchange rates between the United Kingdom and the firm's headquarters country. (Such adjustments in the exchange rate appear to have

Table V

$BrexitRisk_{i,t}$, $BrexitSentiment_{i,t}$, and Firm Investment

This table reports results from regressions of $I_{i,t+1}/K_{i,t} \cdot 100$ on $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$ using yearly data, separately for the full sample (Panel A) and for sample firms headquartered in the United States (Panel B). $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$ are calculated by taking the yearly average across a firm's quarterly earnings call transcripts. The dependent variable is winsorized at the 1st and 99th percentiles. All specifications control for *log(assets)*. Industry fixed effects are based on two-digit SIC codes. *Realized volatility*$_{i,t}$ is the standard deviation of the daily stock return for firm i during quarter t. The regressions exclude non-U.K. firms with fewer than 10 transcripts of calls in the 2015 to 2018 period and firms in the "Non Classifiable" sector. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	$I_{i,t+1}/K_{i,t} \cdot 100$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: All firms						
$BrexitRisk_{i,t}$	−0.741***	−0.528***	−0.396***	−0.464***	−0.430***	−0.434***	−0.477***
	(0.126)	(0.134)	(0.136)	(0.138)	(0.135)	(0.138)	(0.147)
$BrexitSentiment_{i,t}$	−0.058	−0.083	−0.086	−0.084	−0.080	−0.089	−0.101
	(0.070)	(0.069)	(0.070)	(0.073)	(0.072)	(0.073)	(0.081)
$NonBrexitRisk_{i,t}$					−0.818***	−0.694**	−0.576*
					(0.285)	(0.286)	(0.294)
$NonBrexitSentiment_{i,t}$						0.833***	0.827***
						(0.232)	(0.234)
Realized volatility$_{i,t}$							0.074**
							(0.035)
R^2	0.006	0.033	0.058	0.079	0.080	0.080	0.082
N	25,851	25,836	25,759	25,744	25,744	25,744	22,866
Year FE	N	Y	Y	Y	Y	Y	Y
Industry FE	N	Y	Y	Y	Y	Y	Y
Country x Year FE	N	N	Y	Y	Y	Y	Y
Industry x Year FE	N	N	N	Y	Y	Y	Y
	Panel B: U.S. firms						
$BrexitRisk_{i,t}$	−1.148***	−0.871***	−0.871***	−0.821***	−0.764***	−0.793***	−0.910***
	(0.256)	(0.263)	(0.263)	(0.255)	(0.256)	(0.258)	(0.282)
R^2	0.005	0.040	0.040	0.068	0.068	0.069	0.068
N	16,385	16,385	16,385	16,367	16,367	16,367	14,530
Year FE	N	Y	Y	Y	Y	Y	Y
Industry FE	N	Y	Y	Y	Y	Y	Y
Industry x Year FE	N	N	N	Y	Y	Y	Y

been important for the initially resilient response of U.K.-based firms to the Brexit referendum (Broadbent et al. (2019)).) Column (4) adds the interaction of sector and time fixed effects, thus absorbing any differential trends in the investment rates of firms in different sectors. The coefficient on Brexit risk remains stable at −0.464 ($SE = 0.138$), while the coefficient on Brexit sentiment remains indistinguishable from zero. Columns (5) and (6) add controls for

the firm's overall (i.e., non–Brexit-related) risk and sentiment—specifically, mentions of risk and positive- and negative-tone words, respectively, that do not appear together with the word "Brexit." Reassuringly, we find that firms exposed to higher overall uncertainty (i.e., higher *NonBrexitRisk*) have *lower* investment rates, with a one-standard-deviation increase in a firm's non–Brexit-related risk associated with a 0.818 ($SE = 0.285$) percentage point decrease in its investment rate. Similarly, firms for which overall sentiment (i.e., *NonBrexitSentiment*) is positive have *higher* investment rates.

Turning to our variables of interest, we document that the negative association between $BrexitRisk_{i,t}$ and investment remains effectively unchanged at 0.434 ($SE = 0.138$). This stability reflects the fact that firm-level exposures to Brexit and non-Brexit risk are uncorrelated (the correlation between the two is 1.31%). Only 0.5% of the references to risk in our sample occur together with the word "Brexit." For the average global firm in our sample, Brexit is thus but one of many sources of risk affecting their business. Nevertheless, our text-based measures retain sufficient statistical power to distinguish the marginal effects of Brexit and non-Brexit risk. In fact, the estimated effect in our preferred specification (column (6)) suggests that for firms exposed to Brexit risk equal to that of an average postreferendum U.K. firm (1), the decrease in investment is 0.434 percentage points (or 1.8% relative to the mean): a decrease comparable in magnitude to that associated with a persistent one-standard-deviation increase in the firm's non–Brexit-related risk.[32] Extrapolating from the country-specific mean Brexit risk in Figure 2, this implies a $0.43 \times 1.74 \times 100 = 0.74$ percentage point decrease in investment rate for the average Irish firm (average investment rate 29.84%), and a $0.43 \times 0.67 \times 100 = 0.28$ percentage point decrease for the average South African firm (average investment rate 17.98%) in our sample. Table IA.VI in the Internet Appendix repeats this calculation to give the estimated impact of Brexit risk relative to the average investment rate in each country shown in Figure 2.

The final column, column (7), of Table V adds *Realized volatility* (i.e., the standard deviation of the firm's daily stock return, measured over the quarter, adjusted for dividends and stock splits) to the specification in the previous column, as an additional control. Consistent with our text-based measure of non-Brexit risk overlapping to some degree with this market-based measure of generic risk, including *Realized volatility* attenuates the coefficient estimate on *NonBrexitRisk* somewhat (from −0.694 to −0.576). That said, consistent with our results above, we find that including this additional control has no perceptible effect on our coefficients of interest.[33]

[32] Table IA.X in the Internet Appendix shows the robustness of these inferences to a range of alternative choices of standard errors.

[33] In Table IA.XI in the Internet Appendix, we report the same specification as in column (7), but replace *Realized volatility* with *Implied volatility*, derived from 90-day at-the-money options. Data for implied volatility are sparser, reducing the sample size. We find that using implied volatility rather than realized volatility does not materially affect the coefficient on *BrexitRisk* for the full

Panel B of Table V repeats this analysis for the sample of U.S.-headquartered firms. The results point in the same direction as those for the full sample. Specifically, in Panel B, we repeat the same sequence of specifications as in Panel A, but report only the coefficient estimates on $BrexitRisk_{i,t}$ to save space. (Consistent with our results above, the effect of $BrexitSentiment_{i,t}$ remains statistically indistinguishable from zero in all specifications.) Our estimates for U.S. firms are larger than for the full sample, potentially because firm-level variables are measured with less error in this more homogeneous subsample. Given an average $BrexitRisk_{i,t}$ among U.S.-headquartered firms of 0.128, our preferred estimate in column (6) (-0.793, $SE = 0.258$) suggests that Brexit risk accounts for a $0.128 \times 0.793 = 0.10$ percentage point decrease in the investment rate of the average U.S.-based firm in each year after 2016.

The findings reported in Table V are based on regressions that constrain the association between $BrexitRisk_{i,t}$ and the investment rate to be time-invariant. To analyze whether the effect of Brexit-related uncertainty is indeed constant, versus time-varying, we estimate a regression of $I_{i,t+1}/K_{i,t}$ on interactions of postreferendum year indicator variables and $BrexitRisk$, our standard set of controls, and country-time and industry-time fixed effects. Figure 8 presents the results. (Table IA.XII in the Internet Appendix provides details and shows similar results for employment as an alternative outcome variable.) The figure shows the strongest marginal effect for a given level of $BrexitRisk$ in the year immediately after the referendum vote (2017). The effect is somewhat weaker in the following year and then dissipates. This pattern is consistent with reactions to uncertainty tied to a single future event: international, non-U.K. firms likely have a limited number of investment projects vulnerable to Brexit. After the referendum, firms respond by postponing these investments, resulting in a level difference in the stock of investments that persists through the end of our sample period.

Despite the comprehensive set of controls included in Table V, a causal interpretation of the results is subject to three concerns. First, corporate executives might use Brexit risk as an excuse to justify bad performance, even if their firm is not really exposed to the shock. The correlation between our $BrexitRisk$ measure and the decline in firm investment might then be spurious, picking up "cheap talk" about Brexit. However, we have already seen that introducing controls for the firm's Brexit and overall (i.e., nonBrexit related) sentiment has no perceptible effects on our coefficient of interest (compare columns (5) and (6) of Table V). Of course, our proxies for sentiment may not fully capture pertinent first-moment effects. We therefore add additional controls for the firm's recent financial performance in columns (1) to (3) of Table VI. These three columns and all remaining specifications in this table include all controls from our most demanding specification in column (7) of Table V. For brevity, we report only the coefficients on Brexit risk and the newly added controls. Column (1) adds a measure for the firm's earnings surprise (Ball and Bartov

sample but reduces the estimated coefficient for U.S. firms by about 50%, although it remains statistically significant at the 5% level.

Table VI

Robustness: $BrexitRisk_{i,t}$, $BrexitSentiment_{i,t}$, and Firm Investment

This table reports estimation results from regressions of $I_{i,t+1}/K_{i,t} \cdot 100$ on $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$ using yearly data for the full sample. $BrexitRisk_{i,t}$ is defined as in Table V. *Earnings surprise*$_{i,t}$ is defined as $(EPS_{i,t} - EPS_{i,t-1})$*/end-of-year stock price*$_{i,t}$, where $EPS_{i,t}$ is the earnings per share of firm i during year t (Compustat item epspx). *Stock returns*$_{i,t}$: *Quarterly* is the firm's average stock return in the quarter in which the earnings call is held, and *Stock returns*$_{i,t}$: *Week before EC* is the average stock return in the week before the earnings call is held. *PRiskTrade*$_{i,t}$ (std.) is the Political Risk: Trade Policy Index variable from Hassan et al. (2019), standardized by its own standard deviation. *Brexit window return*$_i$ is firm i's stock return between June 24 to 29, 2016 (i.e., during the four days following the Brexit referendum). All specifications control for *log(assets)*. Industry fixed effects are based on two-digit SIC codes. The dependent variable is winsorized at the 1st and 99th percentiles. The regressions exclude non-U.K. firms with fewer than 10 transcripts in the 2015 to 2018 period and firms in the "Non Classifiable" sector. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	$I_{i,t+1}/K_{i,t} \cdot 100$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All firms							
$BrexitRisk_{i,t}$	−0.539**	−0.388***	−0.463***	−0.498***	−0.489**	−0.567***	−0.401***
	(0.239)	(0.146)	(0.147)	(0.153)	(0.230)	(0.182)	(0.151)
Earnings surprise$_{i,t}$	−0.078						
	(0.049)						
Stock returns $_{i,t}$: *Quarterly*		0.270***					
		(0.028)					
Stock returns$_{i,t}$: *Week before EC*			0.117*				
			(0.060)				
PRiskTrade$_{i,t}$ (std.)				−0.480**			
				(0.225)			
Average U.K. sales$_i$ *(pre-Brexit)*					3.516		
					(4.573)		
$\overline{BrexitExposure_i}$						0.599	
						(0.668)	
Brexit window return$_{i,t}$							2.465
							(4.808)
R^2	0.090	0.098	0.082	0.087	0.102	0.082	0.087
N	16,485	22,122	22,409	21,910	15,535	22,863	23,956
Panel B: U.S. firms							
$BrexitRisk_{i,t}$	−0.866***	−0.731**	−0.877***	−0.884***	−0.669***	−1.104***	−0.749***
	(0.281)	(0.285)	(0.283)	(0.284)	(0.212)	(0.360)	(0.260)
R^2	0.068	0.084	0.069	0.070	0.082	0.069	0.070
N	12,780	14,336	14,331	14,290	12,511	14,530	16,019
Controls	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Industry x Year FE	Y	Y	Y	Y	Y	Y	Y
Country x Year FE	Y	Y	Y	Y	Y	Y	Y

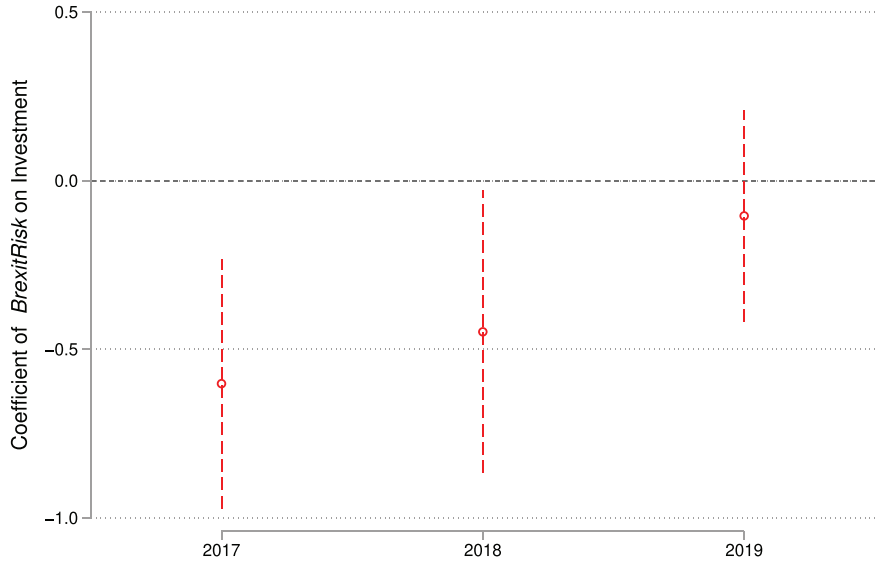

Figure 8. Investment and $BrexitRisk_{i,t}$: Timing of the effect of $BrexitRisk_{i,t}$. This figure plots the coefficients estimates along with 95% confidence intervals from a regression of $I_{i,t+1}/K_{i,t} \cdot 100$ on interactions of post-referendum year (2016, 2017, 2018) indicator variables and $BrexitRisk_{i,t}$. The dependent variable is the one-year-ahead investment rate, and hence the coefficient estimate on the interaction between the 2016 indicator variable and $BrexitRisk_{i,t}$ is reported as 2017 on the horizontal axis, and so on. The regression controls for *log(assets)*, $BrexitSentiment_{i,t}$, $NonBrexitRisk_{i,t}$, $NonBrexitSentiment_{i,t}$, and country-year and industry-year fixed effects. Industry fixed effects are based on two-digit SIC codes. The dependent variables are winsorized at the 1st and 99th percentiles. Standard errors are clustered at the firm level. (Color figure can be viewed at wileyonlinelibrary.com)

(1996)). Columns (2) and (3) add the firm's contemporaneous stock return—measured as the firm's average return in the quarter of each earnings call or as the average return in the week before the date of the earnings conference call. Poor performance should be reflected in lower unexpected earnings and lower returns. We find that none of these additional controls significantly attenuate the coefficient of interest, bolstering confidence that our estimates are not driven by cheap talk or by inadequate controls for first-moment effects.

Importantly, to the extent that these additional controls capture first-moment effects not well reflected by *BrexitSentiment*, the findings can be interpreted as sensitivity checks on our inference that *BrexitRisk* captures second-moment rather than first-moment effects. Despite our efforts, we acknowledge that both *BrexitRisk* and *BrexitSentiment* likely suffer from measurement error, so our firm-level estimates may be attenuated (i.e., biased toward zero). At the same time, there is no reason to believe that measurement error differs systematically between the risk and sentiment variables. We consistently document a statistically significant (negative) correlation between *BrexitRisk*

and investment, whereas *BrexitSentiment* and investment are never significantly correlated.

The second concern is that firms affected by Brexit risk may also be disproportionately affected by other types of risk. Again, controlling for non–Brexit-related risk and realized volatility has no perceptible effect on our estimates (compare Table V), demonstrating that the reduction in investment that we document is specific to Brexit-related risk. Furthermore, column (4) of Table VI also controls for the firm's exposure to trade policy risk ($PRiskTrade_{i,t}$). This variable, developed in Hassan et al. (2019), is constructed in the same way as *BrexitRisk* but differs in that it counts synonyms of risk or uncertainty near words that indicate discussion of political interference in trade policy.[34] As expected, we find that exposure to trade-policy risk lowers the firm's investment rate: a one-standard-deviation increase in $PRiskTrade_{i,t}$ is associated with a 0.480 ($SE = 0.225$) percentage point decrease in the firm's investment rate. However, including this control has little effect on our coefficient of interest, which remains stable at -0.498 ($SE = 0.153$).

The third potential concern is that U.K.-exposed international firms may be systematically different and may generally invest less than other firms. To address this concern, column (5) adds a firm's average sales in the United Kingdom *before* the Brexit referendum as a control variable. Column (6) adds the firm-specific average of our measure of Brexit exposure, $\overline{BrexitExposure_i}$, calculated using all observations of a given firm in the sample. Similarly, column (7) adds the firm's market response to the Brexit vote (i.e., the "event-study returns" from Table IV) as an alternative cross-sectional measure of Brexit exposure. Note that three of these variables are "bad controls" (Angrist and Pischke (2008)) since they are potential proxies for Brexit-related risk and/or sentiment and hence may inappropriately reduce the explanatory power of our variables of interest. That said, they could also be correlated with unobserved differences across firms implicated in Brexit, which may, in turn, impact investment. Adding these variables to our specification is tantamount to controlling for this heterogeneity.[35] Mindful of the econometric concerns, we find little evidence that adding these additional controls changes the tenor of our main findings. Neither the pre-Brexit U.K. sales variable nor $\overline{BrexitExposure_i}$ or the event-study returns are significantly associated with firms' investment rates when $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$ are included as controls. Furthermore, the magnitude of the estimated coefficient on $BrexitRisk_{i,t}$ remains stable and highly statistically significant despite the inclusion of these controls.

[34] As one might expect, this measure shows sharp increases coinciding with various trade disputes between the United States and other countries from 2016 to 2019. See www.firmlevelrisk.comfor details.

[35] Including firm fixed effects offers an alternative approach to removing (time-invariant) unobserved heterogeneity. We do not have sufficient power in most tests to implement this design. For the U.S. sample, however, when adding firm fixed effects to the specification of column (5) of Table V (Panel B), we find a coefficient estimate on $BrexitRisk_{i,t}$ equal to -0.448 ($SE = 0.307$), reassuringly comparable to our corresponding estimate for the full sample in Panel A of the same table.

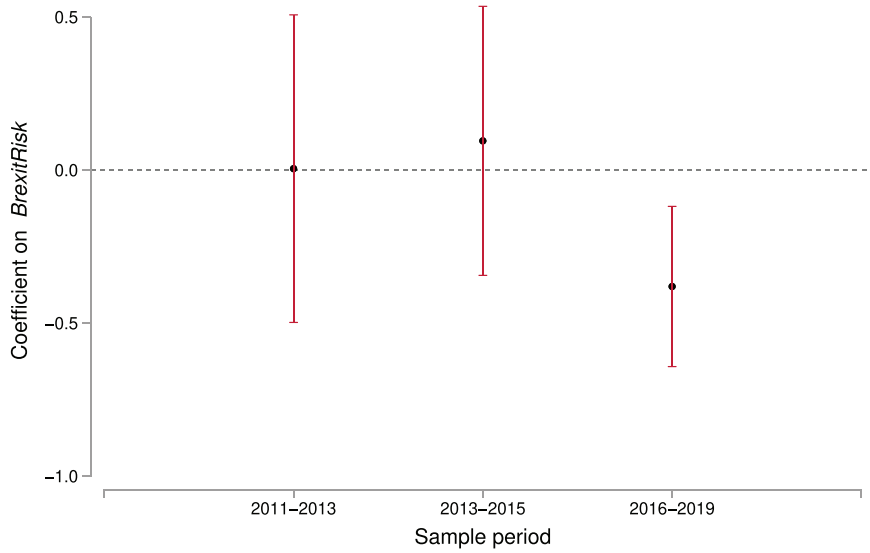

Figure 9. Placebo test: Counterfactual Brexit. This figure plots coefficient estimates and 95% confidence intervals for $BrexitRisk_{i,t}$ from three separate panel regressions of $I_{i,t+1}/K_{i,t} \cdot 100$ on $BrexitRisk_{i,t}$ and the same control variables as in column (5) of Table V, Panel A. For the 2011 to 2013 and 2013 to 2015 sample periods, we reassigned each firm's time series of 2016 to 2019 $BrexitRisk_{i,t}$ to the sample period indicated; for the 2016 to 2019 sample period, $BrexitRisk_{i,t}$ is firm i's actual $BrexitRisk_{i,t}$ in that sample period. (Color figure can be viewed at wileyonlinelibrary.com)

Figure 9 shows the results of a placebo exercise where we reestimate our preferred specification of column (6) in Table V but erroneously assign each firm's *BrexitRisk* to three years before 2016. The first coefficient shows the results when we set each firm's *BrexitRisk* to the years from 2011 to 2013. The second coefficient repeats the exercise for the years 2013 to 2015. Comfortingly, the point estimates are close to zero, and we find no statistically significant effect of Brexit risk before 2016. For comparison, the third coefficient shows our preferred specification's actual Brexit risk estimate. Taken together, the results consistently suggest that our estimates capture the causal effect of Brexit risk on firm-level investment.

Having established a consistent negative relation between Brexit risk (though not sentiment) and firms' capital investment rate, we now turn to firms' employment and sales growth. In Table VII, we report panel regressions that correspond to Table V, both with and without the full set of interacted fixed effects. Here too, we provide estimates based on the full sample and our sample of U.S.-based firms separately.

Prior work on the economic consequences of uncertainty suggests that hiring and investment should respond similarly to changes in uncertainty since both activities exhibit adjustment costs. In line with these predictions, Panel A in Table VII shows (across both samples) a significantly negative relation

Table VII

$BrexitRisk_{i,t}$, $BrexitSentiment_{i,t}$, and Other Firm Outcomes

This table reports estimation results from panel regressions of $\Delta emp_{i,t}/emp_{i,t-1} \cdot 100$ (Panel A) and $\Delta sales_{i,t}/sales_{i,t-1} \cdot 100$ (Panel B) on $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$. $BrexitRisk_{i,t}$ and $BrexitSentiment_{i,t}$ are calculated as in Table V. All specifications control for *log(assets)*, $NonBrexitRisk_{i,t}$, $NonBrexitSentiment_{i,t}$, and *Realized volatility*$_{i,t}$. Industry fixed effects are based on two-digit SIC codes. The regressions exclude non-U.K. firms with fewer than 10 transcripts in the 2015 to 2018 period and firms in the "Non Classifiable" sector. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: $\Delta emp_{i,t}/emp_{i,t-1} \cdot 100$

	All firms		U.S. firms	
$BrexitRisk_{i,t}$	−0.406***	−0.364***	−0.735***	−0.764***
	(0.119)	(0.132)	(0.236)	(0.254)
$BrexitSentiment_{i,t}$	−0.026	−0.032	−0.087	−0.073
	(0.068)	(0.069)	(0.126)	(0.131)
$NonBrexitRisk_{i,t}$	−0.725***	−0.773***	−0.721***	−0.666**
	(0.214)	(0.222)	(0.268)	(0.273)
$NonBrexitSentiment_{i,t}$	1.370***	1.398***	1.568***	1.558***
	(0.176)	(0.196)	(0.251)	(0.257)
R^2	0.027	0.065	0.028	0.058
N	27,619	27,524	18,283	18,263

Panel B: $\Delta sales_{i,t}/sales_{i,t-1} \cdot 100$

	All firms		U.S. firms	
$BrexitRisk_{i,t}$	−0.245	−0.092	−0.231	−0.209
	(0.198)	(0.209)	(0.329)	(0.329)
$BrexitSentiment_{i,t}$	0.114	0.120	0.151	0.198
	(0.092)	(0.106)	(0.207)	(0.223)
$NonBrexitRisk_{i,t}$	−0.172	−0.205	−0.049	−0.043
	(0.507)	(0.528)	(0.686)	(0.696)
$NonBrexitSentiment_{i,t}$	1.962***	2.019***	2.082***	1.807***
	(0.320)	(0.350)	(0.469)	(0.478)
R^2	0.028	0.067	0.039	0.060
N	29,584	29,481	18,977	18,957
Controls	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Industry × Year FE	N	Y	N	Y
Country x Year FE	N	Y	n/a	n/a

between $BrexitRisk_{i,t}$ and firms' net hiring, $\Delta emp_{i,t}/emp_{i,t-1}$. Our preferred coefficient estimates are −0.364 ($SE = 0.132$) and −0.764 ($SE = 0.254$) for the full sample and for the United States, respectively, where the point estimate for U.S.-based firms is again considerably larger than that for the full sample. The former estimate implies that an international firm with a level of Brexit risk equal to that of the average U.K.-based firm (1) experiences a decrease in employment growth of 0.36 percentage points (relative to an average net

hiring rate of 8.12%). (Table IA.VI in the Internet Appendix breaks these numbers down by individual country.) The latter implies a $0.128 \times 0.764 = 0.10$ percentage point reduction for the average U.S. firm (the average net hiring rate for U.S. firms is 11.36%). As for the capital investment rate, we find no significant association between $BrexitSentiment_{i,t}$ and employment growth, again likely reflecting the fact that, before the U.K.'s actual exit from the single market, Brexit affected firm decisions largely by raising uncertainty. As before, the coefficients on *NonBrexitRisk* and *NonBrexitSentiment* are statistically significant and have the predicted sign. (Table IA.XIII in the Internet Appendix reports the same battery of robustness checks as in Table VI.)[36]

We consider sales growth, our third firm-level outcome variable, in Panel B. While we find a negative relation between $BrexitRisk_{i,t}$ and sales growth in all sample partitions, the association is no longer statistically significant. This finding is consistent with predictions from the real options literature that postulate a larger short-run effect of risk on hard-to-reverse investment in physical and human capital than on short-run sales growth (e.g., Baker, Bloom, and Davis (2016)). We also find no significant evidence of a positive association between $BrexitSentiment_{i,t}$ and sales growth. Instead, sales growth shows persistent positive correlations with *NonBrexitSentiment*, consistent with the idea that sales respond more directly to positive and negative shocks only after they are realized. (See Table IA.XIV in the Internet Appendix for additional variations and robustness checks.)[37]

VI. Regional Support for Brexit

Before we conclude, we present a final application for our Brexit exposure measures, which builds on the simple intuition that voters who live in a region where the operational headquarters of a firm has elevated Brexit exposure may be more likely to vote "Remain" in the referendum. Previous studies generally focus on voter characteristics (such as age, ethnicity, and educational achievements) to explain geographical variation in voting (e.g., Alabrese et al. (2019), Fetzer (2019)). We argue that a voter's referendum choice will also be affected by their assessment of how Brexit will affect local economic and employment conditions. In particular, if local companies find Brexit risky, the regional share in support of "Leave" is expected to decrease. We test this prediction in Table VIII.

[36] Simulations reported in Broadbent et al. (2019) interpret the Brexit referendum as news about a future slowdown in productivity growth in the U.K.'s tradable sector and predict a reduction in investment growth, while employment remains relatively stable.

[37] In Table IA.XV in the Internet Appendix, we examine the timing of the effect of Brexit risk on investment and employment outcome variables. Specifically, we regress both the capital investment rate and the employment growth rate onto contemporaneous $BrexitRisk_{i,t}$ and onto one-period-lagged $BrexitRisk_{i,t-1}$. We find that employment responds more quickly than investment to changes in Brexit risk. Indeed, firm hiring responds more to concurrent than to lagged Brexit risk, while the opposite is true for the investment rate.

Table VIII
Voting in Brexit Referendum

This table reports OLS estimates from cross-sectional regressions of *Pct Vote for Leave*$_d$ on *BrexitRisk*$_d$ and *BrexitSentiment*$_d$, as defined in Table III. *Share U.K. born*$_d$ (the share of U.K.-born individuals residing in district d) and *Income per capita*$_d$ are controls in the regression measured for district d as reported in the 2011 census. We use 2,945 transcripts of the earnings calls of 407 unique sample firms held between 2015Q1 and 2019Q1 to calculate firm-level means. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Pct Vote for Leave$_d$		
	(1)	(2)	(3)
BrexitRisk$_d$	−0.838*		−0.929**
	(0.456)		(0.378)
BrexitSentiment$_d$		0.358***	0.386***
		(0.133)	(0.114)
Share U.K. born$_d$	50.481***	51.592***	52.395***
	(7.296)	(7.484)	(7.380)
Income per capita$_d$	−0.024***	−0.022***	−0.023***
	(0.004)	(0.003)	(0.004)
R^2	0.580	0.586	0.604
N	110	110	110

To do so, we first determine each U.K. firm's location using the area code of its *operational* headquarters and then map these locations to the U.K.'s 110 electoral districts, d. Next, we compute district-level Brexit risk and sentiment by averaging $\overline{BrexitRisk_i}$ and $\overline{BrexitSentiment_i}$ across firms in the district. We then estimate cross-sectional regressions of the district-level vote in support of "Leave" (*Pct Vote for Leave*$_d$) on our two variables of interest, $\overline{BrexitRisk_d}$ and $\overline{BrexitSentiment_d}$, and on two demographic controls, namely, share U.K. born (i.e., the proportion of the district's population born in the United Kingdom) and income per capita.[38] Specifically,

$$PctVote\,for\,Leave_d = \alpha + \beta\overline{BrexitRisk_d} + \gamma\overline{BrexitSentiment_d} + X_d^{'}\zeta + \epsilon_d. \quad (5)$$

In column (1) of Table VIII, where we only consider district-level $\overline{BrexitRisk_d}$, we find a negative association with the "Leave" vote share. Turning to $\overline{BrexitSentiment_d}$ in column (2), we show that when firms in the district view Brexit negatively, the association with the "Leave" vote share is strongly negative. In column (3), we include both variables of interest and find results similar to the separate estimates. The estimated coefficients imply that a one-standard-deviation increase in $\overline{BrexitRisk_d}$ (1.59) is associated with a 1.48 percentage point decrease in the share of the vote for leaving the EU.

[38] Note that the distribution of our 407 U.K. firms is geographically clustered. As reported in Table IA.XVI in the Internet Appendix, many districts have only one sample firm and many sample firms are headquartered in a handful of districts (e.g., the City of London, Greater London).

Similarly, a one-standard-deviation decrease in $\overline{BrexitSentiment_d}$ (4.44) is related to a 1.71 percentage point drop in support for Brexit.[39] Figure IA.7 in the Internet Appendix depicts this association. For completeness, note that wealthier districts and districts with a larger immigrant population have lower support for "Leave." Comparing these findings to Alabrese et al. (2019) and Fetzer (2019), who find substantial *geographical* heterogeneity in the extent to which demographic variables can explain the Brexit vote, our results suggest that spillovers from local companies might be a source of this geographical heterogeneity.

VII. Conclusion

Assessing the economic impact of specific policy measures, reforms, and other market-wide shocks requires quantifying how these events affect the calculations and expectations of decision-makers. In this paper, we develop a simple and versatile text-based method to measure the costs, benefits, and risks that thousands of decision-makers all around the globe associate with specific shocks. Our method offers several helpful features that address some of the challenges identified in recent research. First, it measures perceptions directly and in real time without having to conduct expensive large-scale surveys (see, e.g., Arteaga-Garavito et al. (2022) and Hassan et al. (2022) for applications in the context of the COVID-19 shock). Second, it meaningfully distinguishes between the perceived risks, costs, and opportunities associated with a given event, thus separating variation in first- and second-moment effects of shocks. This is particularly useful in the context of Brexit, where policymakers have long pointed to the potentially detrimental effects of Brexit-related uncertainty, which we quantify directly. Third, many shocks do not (fully) play out in a short period but rather present persistent challenges to economic actors. A method allowing researchers to measure over-time variation in a firm's exposure to a persistent shock is particularly valuable in light of recent evidence that the response to a persistent shock might be very different from the response to a shock that fades away quickly (Bloom et al. (2019)).

The 2016 Brexit referendum is an ideal test of our method to assess the extent to which the vote's outcome affected international (i.e., non-U.K.) firms. Our measures of Brexit exposure, risk, and sentiment behave in economically meaningful ways, strengthening our validity claims. We also document that firms inside and outside the United Kingdom overwhelmingly view Brexit as "bad news." Further, we report significant cross-country differences in Brexit risk: Ireland's Brexit risk is larger even than the U.K.'s, nearby EU countries experience the strongest increase in Brexit-related risk, and Brexit risk also has a material (though weaker) impact in the United States and other non-EU countries.

Using transcripts of earnings conference calls as the source text provides rich context, enabling us to identify firms' concerns about Brexit in detail. From analyzing the narrative text underlying our measures, we find that even

[39] The partial R^2 of these two variables in column (3) is about 5%.

"Brexit winners" often simply note that they are not presently negatively affected by the prospect of Brexit. Those that see Brexit as bad news, however, expect difficulties for their businesses due to regulatory divergence, reduced labor mobility, decreased trade access, and post-Brexit operational adjustments. In fact, we find that U.S. and international firms most exposed to Brexit risk have significantly reduced investment and employment growth. We also find that equity markets quickly impounded both first- and second-moment exposures to Brexit: Our Brexit sentiment and risk measures partially explain returns in equity markets in the days following the referendum.

Taken together, the evidence suggests that up to the end of our sample period in 2019, the Brexit vote mostly acted as an uncertainty shock, leading to significant precautionary reductions in investment and employment growth in the firms and countries most exposed. In addition to this activity-depressing effect of sustained uncertainty, equity markets also anticipated large negative first-moment effects from the implementation of Brexit on firms around the world, effects that have not yet been materialized in corporate actions. Our reading of the evidence suggests that the greater the rupture between the United Kingdom and the EU, the larger these direct effects (including post-Brexit adjustment costs) will be. As time passes, now that Britain has formally left the EU, the consequences for investment and employment may be larger than those associated with Brexit uncertainty alone.

Beyond this application to Brexit, Section III of the Internet Appendix briefly shows that our method is sufficiently versatile to be more generally useful for characterizing and quantifying firm-level exposures to the costs, benefits, and risks associated with other shocks, using the 2011 Fukushima nuclear disaster in Japan as an example. Future applications may also estimate the firm-level impacts of natural disasters, political events (e.g., revolutions, the U.S. government shutdown), or specific regulatory reforms in response to the climate emergency.

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Supporting Information

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Appendix S1: Internet Appendix.
Replication Code.