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The Real effects of Brexit on labor demand: Evidence from firm-level data☆

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ABSTRACT

Using the distance to the Irish border of UK firms that have not changed their location since the 2016 Referendum to isolate the effects of Brexit at the firm level, we find that Brexit implementation in 2020 caused exposed firms to cut their workforce by up to 15.7% on average relative to non-exposed firms. These exposed firms are also more likely to have lower growth expectations and more likely to increase their research and development (R&D) expenditure. In addition, having ex-ante trade exposure, either with or outside the EU, can help alleviate such negative effects of Brexit. Such results highlight the role of trade exposure and the expectation channel, and support the hypothesis that firms prioritize innovations in response to Brexit.

1. Introduction

After a much-debated referendum, the United Kingdom voted in favor of leaving the European Union in June 2016, with the actual implementation of such a separation set to take effect in 2020. For the first time in its history, the European Union (EU) has witnessed a sovereign nation abandoning its common economic zone and, with that, many of the economic incentives associated with being in the union.

While the impetus behind such a substantial policy change is built upon, among others, the promise of an improved job market for domestic workers (Becker et al., 2017), the extent to which the *actual* implementation of the policy in 2020 impacts the labor

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market is unclear, with the related literature mainly focusing on the economic effects of the referendum (Sampson, 2017; Faccini and Palombo, 2021). Understandably, one key challenge associated with examining the impact of Brexit implementation lies in its well-anticipated timing: the policy change announced in 2016 was ubiquitously expected to be fully implemented in 2020, potentially confounding the effects of the actual policy due to its anticipation.

Our paper contributes to the literature by examining the effects of Brexit implementation on labor demand. The key source of novelty for our paper is the consideration of a plausibly exogenous proxy for the exposure to Brexit at the firm level to isolate the implementation's effects from confounding effects arising from the policy's anticipation. More importantly, we also provide several potential mechanisms to explain these effects. To the best of our knowledge, our paper is the first to address these issues simultaneously.

Our focus on understanding labor demand is partly motivated by the recent growing interest from the UK's policymakers vis-à-vis improving the prospect of the labor market post-Brexit.[1] Intuitively, firms may reduce their labor demand when they observe an adverse change in the market and anticipate the need to substitute away from a labor-intensive production process. Understanding the presence of a shift in labor demand and, perhaps more importantly, whether firms substitute away from labor is crucial in understanding the effects of Brexit. Specifically, doing so allows us to answer whether the policy change has directly induced businesses to scale down or improve productivity (i.e., via R&D). While the former shows the adverse effect of Brexit, the latter shows its potential innovation-inducing effect, both of which are important for evaluating the effects of the policy.

We design our empirical analysis around a unique feature of the complex legislation arising from the policy change. Despite the expectations leading to Brexit's eventual implementation in January 2020, not all regions in the United Kingdom have been set to be subjected to the same economic burden arising from Brexit.[2] In particular, due to the provisions following the Northern Ireland Protocol, the United Kingdom does not maintain a hard border, effectively allowing free travel and, more importantly, free movements of goods across the Irish border into the European Union for firms located in Northern Ireland. These provisions do not apply to firms located in Great Britain as they are separated from Northern Ireland via the Irish Sea. In other words, these firms are more likely to bear additional burdens doing business than firms located in Northern Ireland as Brexit goes into effect.

Our identification strategy hinges on using the variation in Brexit exposure based on the de facto separation in EU market access across Great Britain and Northern Ireland firms. Using a large-scale longitudinal survey of UK small and medium-sized enterprises (SMEs), we first compute the firms' shortest distance to the port of Newry - strategically located near the Republic of Ireland border with Northern Ireland on the main Belfast-Dublin route. We then use this distance as a plausibly exogenous proxy for Brexit exposure among firms that have not changed location since the Brexit referendum in 2016.

Intuitively, while firms are generally aware of the implementation schedule for Brexit, they may not fully be aware of the intensity of the extent to which leaving the EU may impact their business operations. As a result, by focusing on firms that remain in the same locations throughout the sample period (2015–2022), we exclude the endogeneity arising from firms fully anticipating and, therefore, changing their locations in response to Brexit. Using the distance to the border for this subset of firms allows us to identify the causal effects of Brexit. Specifically, by leveraging the distance to the port of Newry to proxy for Brexit exposure, our empirical strategy revolves around a difference-in-difference approach that examines what would happen to ex-ante otherwise similar firms if they were exposed to Brexit. To help visualize our identification strategy, Fig. 1 illustrates a tale of two distinct regions: Great Britain and Northern Ireland. Newry, a border city located near the Republic of Ireland along the Belfast–Dublin corridor, provided firms with easier access to the EU post-Brexit under the Northern Ireland Protocol. Additionally, an alternative route through Derry also connects to the Republic of Ireland. Our analyses yield consistent results for both Newry and Derry.

To isolate the effects of Brexit on labor demand, we control the supply-side effects on labor by accounting for whether firms report having difficulties hiring skilled and unskilled labor on the market. Doing so allows us to capture the impact of Brexit on labor demand without the confounding feedback from the supply-side effects of Brexit. In addition, the focus on SMEs allows us to avoid the feedback arising from the firms potentially impacting the supply side. Intuitively, due to their relative size, these firms are unlikely to be able to affect labor supply via policy lobbying due to significant barriers associated with the process (Kerr et al., 2014) and relatively low gains (Harstad and Svenson, 2011).

As a preamble to our empirical analysis, we find that before the implementation of Brexit in 2020, firms with low exposure to Brexit were statistically similar to firms with high exposure to Brexit. Upon confirming this parallel trend assumption, we document three main results.

First, the 2020 Brexit implementation has led exposed companies to cut their workforce by up to 15.7% compared to firms located near the Irish border. In addition, we find that the overall effects of Brexit are significant and positive, indicating that the net overall impact of Brexit on labor across all firms, whether exposed or not, is positive. This finding aligns with the recent observation of the overall trajectory of employment of SMEs in the aggregate data.

Second, these exposed firms are also more likely to have lower growth expectations and more likely to increase their R&D expenditure in response. On the one hand, these results highlight the expectation channel's role in reducing labor demand following negative changes in their expectation of growth prospects. On the other hand, these results support the hypothesis that firms prioritize innovations and R&D in response to Brexit.

Third, having trade exposure *ex-ante*, either with or outside the EU, can help alleviate the negative effects of Brexit. Specifically, even though exposed firms generally cut their labor demand, exposed firms that traded *ex-ante* cut their labor demand less than

[1] See, for example, https://publications.parliament.uk/pa/ld201719/ldselect/ldeconaf/11/1106.htm. Last Accessed: November 25, 2024.

[2] Source: https://commonslibrary.parliament.uk/brexit/uk-eu-relationship-%20after-brexit/. Last accessed: Dec. 16, 2024.

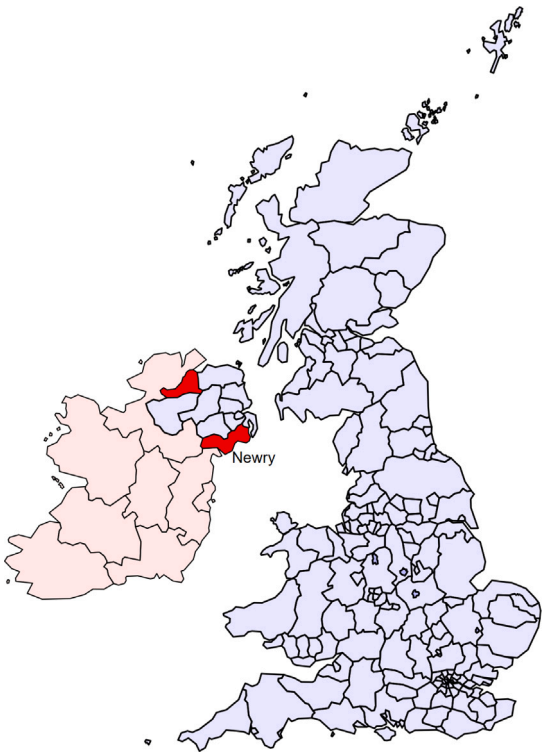

Fig. 1. Map of the United Kingdom.
Notes: The ports of Newry and Derry are in red while the rest of the United Kingdom is in light blue. Republic of Ireland is in light red. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

firms that did not. While the total effects (i.e., *direct* and *indirect*) of Brexit on exposed firms are negative, the *direct* effects are positive, and the *indirect* effects are strongly negative. This contrasting pattern shows that firms without trade exposure bear the largest costs (as evidenced by the statistically and economically significant decrease in their number of employees).

Given that our identification strategy builds around the group of firms that did not change their location after the 2016 Referendum, one potential issue arises in how these firms may inherently self-select into such a group (i.e., selection issue). Our results are consistent regardless of whether a full sample (i.e., including firms that changed and did not change location after 2016) is used or if we restrict the sample to include only firms that did not switch locations. Such consistency between the two sample groups indicates that the selection issue does not systematically bias our results.

Our results are also consistent across a battery of robustness checks. First, we use a dummy variable to determine whether a firm is located in Northern Ireland or Great Britain instead of the distance to the Irish border as a proxy for Brexit exposure. Second, we use the port of Derry - another major transportation hub near the Irish border for products entering the Republic of Ireland - instead of the port of Newry to compute the distance to the Irish border. Third, we conduct a placebo test, randomly assigning firms to different locations and randomizing the timing of Brexit implementation. Fourth, we exclude the period before the 2016 Brexit referendum to check if expectations built up after the referendum may have led to biases in our results. Fifth, we account for the anticipation effects leading to Brexit implementation by interacting our benchmark proxy for Brexit exposure (i.e., distance to the border) with each year dummy. Last but not least, our results are also robust to including a measure of COVID-19 exposure.

One key challenge in studying Brexit's effects is identifying a causal relationship between the event and its economic implications. One source of novelty for this paper is the consideration of a plausibly exogenous proxy for the effects of Brexit at the firm level. Another related contribution is to examine the effects of Brexit implementation on labor demand and to provide several potential mechanisms to explain these effects. After all, the promise of an improved labor market prompted many British to vote to leave the EU (Becker et al., 2017; Fetzer, 2019).

More generally, our paper complements three strands of the literature. First, it extends research on Brexit and firm responses by examining the impacts of its implementation in 2020. While existing papers predominantly study the effects of the 2016 Brexit referendum (Born et al., 2019; Breinlich et al., 2020; Fernandes and Winters, 2021; Bloom et al., 2019), our analysis focuses on the initial year when Brexit's effects became tangible (i.e., January 2020), contributing to the emerging debate on the actual impacts of the policy (Kren and Lawless, 2024).

Second, while the current literature primarily focuses on listed UK firms (Hill et al., 2019; Davies and Studnicka, 2018), our study examines the representative dataset of the UK SME population. Previous research indicates that SMEs, particularly those

with significant levels of irreversible investment, are disproportionately affected by uncertainty due to their limited resources and reduced capacity to withstand sudden shocks (Brown et al., 2019; Chung, 2017). Our study offers empirical evidence on how firms navigate the trade-offs between labor-intensive and technology-intensive business models in response to the Brexit shocks that have taken effect. Our novelty is using distance to the Irish border to proxy for Brexit exposure. Although Zhao and Jones-Evans (2017) use the first-level classification of The Nomenclature of Territorial Units for Statistics (NUTS) regions to define the geographical location of a business, our study identifies the location of SMEs using Local Enterprise Partnerships (LEPs) based on the Department for Business, Energy and Industrial Strategy (BEIS) survey. Subsequently, we match the firms' locations to their respective Local Authority Districts (LADs).

Our result on the increase in R&D expenditure following Brexit is consistent with the growing literature that seeks to explain structural changes in the labor market. In particular, the switch to R&D-intensive activities as labor demand declines can be theoretically attributed to overall technological changes (Acemoglu and Restrepo, 2022), the switch to capital-intensive activities (Acemoglu and Restrepo, 2019), or the complementarity between workers in low-skilled and high-skilled occupations (Aghion et al., 2019). Turning to more details, Acemoglu and Restrepo (2022) link technological changes to the displacement of certain worker groups from jobs for which they have a comparative advantage. Acemoglu and Restrepo (2019) attribute changes in US employment over recent decades to the substitution between capital and labor. Specifically, the switch from capital to labor can reduce the labor share in value-added as it raises productivity. In a related contribution, Aghion et al. (2019) study a model where the degree of complementarity between workers in low-skilled and high-skilled occupations reflects how innovative a firm is. As technology advances, demand for high-skilled workers increases, accelerating the switch away from low-skilled activities.

Third, our paper also contributes to the large literature on the role of international trade as a risk-sharing mechanism in response to various unexpected shocks (Cole and Obstfeld, 1991; Devereux and Smith, 1994; Crucini, 1999; Berka et al., 2012; Duong et al., 2024). In particular, we document that having trade exposure *ex-ante* can help significantly alleviate the negative effects of Brexit.

This paper is structured as follows. Section 2 provides an overview of the Brexit literature, highlighting how our study contributes to the existing body of work. Section 3 outlines our research methodology. Section 4 presents the data used in our analysis. Section 5 is dedicated to the main findings and their robustness. Section 6 explores potential mechanisms and additional results. We conclude in Section 7.

2. The United Kingdom, Brexit and related literature

2.1. The United Kingdom, LEPs, and Newry

The United Kingdom comprises four constituent countries: England, Scotland, Wales, and Northern Ireland. These countries are located on the British Isles, including the island of Great Britain (comprising England, Scotland, and Wales) and the northeastern part of Ireland (Northern Ireland). Following centuries of British involvement in Ireland, the Government of Ireland Act 1920, a pivotal piece of legislation, partitioned the island into two separate entities: Northern Ireland, which remained part of the United Kingdom, and Southern Ireland, which eventually became the Republic of Ireland (Welsh, 2003). This act was significant as it marked a major shift in the political and territorial dynamics of the region. While the nations within the United Kingdom share common institutions such as the monarchy and parliament, they also retain varying degrees of autonomy through devolved governments in Scotland, Wales, and Northern Ireland.

In June 2010, the United Kingdom Budget announced the dissolution of regional development agencies and the establishment of LEPs (HM Treasury, 2010). This initiative enables the examination of regional economic growth and the interrelationships among business agents within the same areas. The network comprises 38 LEPs across regions originating from agreements and collaborations between public and private sector partners, enabling coordination of strategies, resources, and knowledge sharing, as well as leveraging funding to support local businesses, attract investment, and address economic challenges.[3] While local economic partnerships originated in England, similar models exist in other parts of the United Kingdom, such as Regional Economic Partnerships in Scotland and Enterprise Zones in Wales. However, terminology and structures may vary to reflect the specific governance arrangements and priorities of each devolved nation.[4]

The role of Newry in Brexit holds significance in trade between the UK and the EU due to its geographical location as a border city between Northern Ireland (part of the UK) and the Republic of Ireland (an EU member state). This border, known as the Irish border, became a focal point during Brexit negotiations.[5] With the UK's withdrawal from the EU, the issue of the Irish border became central to Brexit negotiations. The desire to avoid a hard border between Northern Ireland and the Republic of Ireland led to the creation of the Northern Ireland Protocol and Windsor Protocol, which effectively kept Northern Ireland within the EU's single market for goods and services (House of Commons Library, 2024).

Amid ongoing debates about the hard border and trade activities, SMEs, which account for 99% of businesses in the UK and contribute to half of the private sector's output, hold the potential and responsibility to significantly enhance the nation's growth rate by prioritizing exports (Dhingra and Sampson, 2022). In particular, the British Chambers of Commerce report that SME exporters have been disproportionately affected by COVID-19 lockdowns and the introduction of new trade barriers with the EU (Chambers of Commerce, 2024).

[3] See the regional map of LEPS at https://www.instituteforgovernment.org.uk. Last accessed: May 2024.

[4] See Department for Business and Trade (2023) and Welsh Government (2024) for more details.

[5] "Christmas cross-border trade stays healthy in Newry" – Available at https://www.bbc.co.uk/news/uk-northern-ireland-59727211

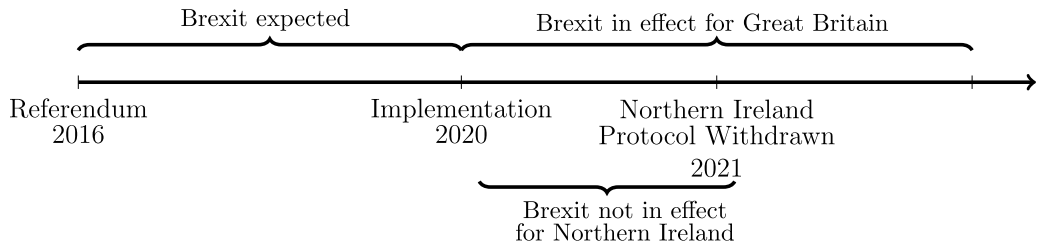

Fig. 2. The Anatomy of Brexit Timing: Northern Ireland vs. Great Britain.

2.2. Brexit and the related literature

Brexit refers to the United Kingdom's (UK) departure from the European Union (EU), representing a process rather than a singular event. Numerous studies have explored the impacts of Brexit on macroeconomic outcomes, including the economic cost of nationalism related to the referendum (Born et al., 2019), heterogeneous firm beliefs and expectations regarding Brexit outcomes (Faccini and Palombo, 2021; Hassan et al., 2024; Davies and Studnicka, 2018), a decline in productivity growth within the tradable sector (Broadbent et al., 2024), and an increase in CPI (Consumer Price Index) inflation (Geiger and Güntner, 2024).

Our paper closely aligns with an emerging branch of literature that examines the regional economic consequences following trade policy shocks, specifically those associated with Brexit. First, Bell (2017) discusses how Great Britain experienced regional disparities, focusing on public expenditure per capita on economic development and economic affairs in Scotland and Northern Ireland from 2014 to 2015. The impacts of Brexit vary significantly across sectors and regions. Utilizing detailed interregional trade data for goods and services within the EU, Thissen et al. (2020) argue that Brexit's effects on regional production costs and the competitive position of firms are considerably more significant for sectors and regions within the UK than for the EU. The disproportionate effects are more pronounced in European countries that are geographically peripheral and economically weaker. These regions have experienced minimal economic exposure to Brexit (Chen et al., 2018). These studies also found that certain UK regions, such as Cheshire, Greater Manchester, and West Yorkshire, have experienced significant improvements in their competitive positions. However, these gains lead to a deterioration in the competitive standings of other nearby regions (Thissen et al., 2020).

In addition to regional analysis, one crucial question is how UK and international firms have responded to Brexit shocks. Breinlich et al. (2020) recently documented an increase in UK outward investment transactions in the remaining European countries following the 2016 Brexit referendum. Similarly, private equity buyout targets are more likely to increase their export value and intensity than non-private equity-backed peers (Lavery et al., 2024). Not only have UK firms been affected, but US firms exposed to Brexit, identified through market and textual-search-based measures, are also more likely to reduce jobs and investment (Campello et al., 2022). In another perspective, Fernandes and Winters (2021) employ the 2016 Brexit referendum as a quasi-natural experiment to evaluate the impact of exchange rate and uncertainty shocks on Portuguese exporters, using transaction-level data to examine changes in different aspects. This study reveals that exporters respond to the shock by reducing export volumes and prices in the UK market, with variations in response based on firm productivity, import intensity, financial constraints, and significant differences observed among goods types and export market entries.

Complementing these empirical findings, McGrattan and Waddle (2020) use structural estimation to explain the optimal policy choices between EU countries and the UK. Accordingly, if UK and EU firms are subject to identical stricter regulations, UK firms, due to their relatively smaller size, are expected to cut back on R&D and other intangible investments and pull back from their EU subsidiaries. Additionally, by analyzing firms listed on the London Stock Exchange, Hill et al. (2019) find that Brexit disproportionately impacts high-growth firms, with the financial sector and consumer goods/services industries experiencing the highest exposure to Brexit-related uncertainty.

The existing literature focuses on several pivotal insights. First, Brexit has caused heterogeneous impacts across various regions and economic sectors within the UK and internationally. Second, most of these studies focus predominantly on the 2016 Brexit referendum, rather than on when Brexit officially took effect in January 2020. Our paper seeks to assess the impacts of Brexit in its effective year (2020), using proximity to Newry, a city bordering Ireland, as a proxy for exposure.

It is important to note that a hard border is avoided on the island of Ireland due to its sensitive nature.[6] Despite considerable efforts, a regulatory border has been implemented in the Irish Sea areas to conduct custom checks on specific products transported from Great Britain to Northern Ireland, especially those intended for the EU single market. This measure stems from the fact that while Northern Ireland is part of the UK customs territory, it must adhere to EU customs and single market regulations to enable the free movement of goods to the Republic of Ireland and thereby into the EU (Murphy, 2022). However, this proposal has not been implemented due to concerns that it could hinder economic growth in Northern Ireland. Additionally, the idea has faced considerable controversy and debate regarding diplomatic and economic integration between the Republic of Ireland and Northern Ireland.

[6] As stated by the European Commission, "a hard border on the island of Ireland is avoided" (E.U. Council, 2024).

3. Empirical strategy

3.1. Identification

Since Northern Ireland does not maintain a hard border with the Republic of Ireland due to the *Northern Ireland Protocol*, firms located in Northern Ireland can transport products into the EU via the Republic of Ireland without having to go through any checkpoints. Indeed, until its withdrawal in January of 2021, the *Northern Ireland Protocol* has protected free travel and, more importantly, free trade of goods across the border between Northern Ireland and the Republic of Ireland (i.e., "the border"). This stipulation puts Northern Ireland's firms in a unique position during the first year that Brexit takes effect (i.e., 31 January 2020) to be involved in *both* the European and the UK markets (Fig. 2). In stark contrast, firms located in Great Britain must pass through the Irish Sea, which is the *de facto* border between Great Britain and Northern Ireland.

This dichotomy in the EU access between firms in Northern Ireland and Great Britain means that the latter fully bear the brunt of the economic burden arising from Brexit while the former do not. In other words, firms closer to the border (e.g., the firms located in Northern Ireland) are less exposed to the effects of Brexit than firms further away from the border (e.g., the firms located in Great Britain).

Conditional on firms knowing that Brexit is coming but not able to change locations or not fully aware of *how significant* its effects are going to be, such a schism between the two groups of firms allows us to use the distance to the border as a plausibly exogenous proxy for the extent to which firms are exposed to the Brexit effects. In our practical application, to identify the groups of firms not fully aware of the veracity of the impact of Brexit, we focus on the groups that kept their locations the same before and after the Brexit announcement. Using the distance to the border for this subset of firms allows us to identify the causal effects of Brexit on small businesses.[7]

Turning to more details, we rely on the shortest distance from the firm's location to Northern Ireland's official border with the Republic of Ireland. Specifically, we use the firms' locations in our survey data, as identified by their LEPs and their LADs, to compute their shortest distance to the port of Newry. We then take the natural log of such a distance and use it as a proxy for firms' exposure to Brexit.[8] It is also helpful to note that since SMEs typically operate regionally, using their reported locations as defined by LEPs in the survey allows us to avoid dealing with firms that might straddle multiple LEPs.

One key challenge in studying the effects of Brexit on labor demand is the need to isolate the impact of demand and supply. To that end, we control for the supply-side effects on labor of Brexit by accounting for whether firms have reported having difficulties hiring skilled and unskilled labor on the market. Doing so allows us to capture the impact of Brexit on labor demand without the confounding feedback from the supply-side effects of Brexit. In addition, our focus on SMEs allows us to avoid input from the firm that could impact the supply side. Intuitively, due to their relative size, these firms are unlikely to be able to affect labor supply via policy lobbying at the local level due to significant barriers associated with the process (Kerr et al., 2014) and relatively low gains (Harstad and Svenson, 2011).

3.2. Regression specification

Our empirical strategy revolves around a difference-in-difference approach that examines what would happen to *ex-ante* otherwise similar firms if they were exposed to Brexit. We leverage the variation in terms of whether a firm is subject to additional economic barriers due to Brexit taking effect in January 2020 by relying on their distance to Northern Ireland's border with the Republic of Ireland. In particular, we focus on the real effects of Brexit and ask whether Brexit can cause firms to reduce their labor force. Our baseline model writes

$$\text{Employees (Log)}_{i,t} = \alpha + \beta(\text{Brexit}_t \times \text{Distance}_i) + \gamma \text{Distance}_i + \delta \text{Brexit}_t + \zeta \mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t}, \tag{1}$$

where *Employees (Log)*$_{i,t}$ denotes the natural logarithm of one plus the number of employees at firm i in year t.[9] α is the constant term, and $\varepsilon_{i,t}$ is a mean-zero disturbance term. β is the key coefficient, capturing the differential impact of Brexit on employment within UK firms, which is measured using the proximity to Newry — a city situated on the Clanrye River in counties Down and Armagh, Northern Ireland. Newry is also strategically located near the Republic of Ireland border, on the main Belfast-Dublin route. ζ is a vector that contains the coefficients for the set of control variables $\mathbb{X}_{i,t}$, which includes the firm's age (*Firm Age*), whether the firm has the same residence and office premises (*Residential Office*), whether its owner is female (*Female Owned*), whether a firm reported having difficulties hiring skilled and unskilled labor (*Labor Supply*), and its legal status (*Legal Status*). We also explain our motivation for selecting these control variables in Section 4.2. λ_k and φ_t are the industry and year fixed-effects, respectively. In

[7] Our choice of using distance to capture the effects of Brexit, or trade policy in general, is motivated by the extensive literature highlighting the role of distance in trade (Rose, 2004; Brei and von Peter, 2018).

[8] To exclude the possibility that firms may preemptively relocate to avoid the adverse effects of Brexit, we exclude the firms that change addresses during our sample period and find our results consistent across all specifications.

[9] The survey consists of self-employed business owners, which results in respondents reporting zero employee. Since businesses are asked to report the current number of employees on their payroll in the UK, the survey would not capture owners or self-employed individuals as employees. Therefore, even government reports mention businesses with no employees (See more at https://assets.publishing.service.gov.uk/media/64e631c0db1c07000d22b34f/LSBS_2022_non_employers.pdf, accessed on October 21st, 2024). We compute the number of employees by taking the log of one plus the number of employees to account for the fact that self-employed respondents are not included in this count.

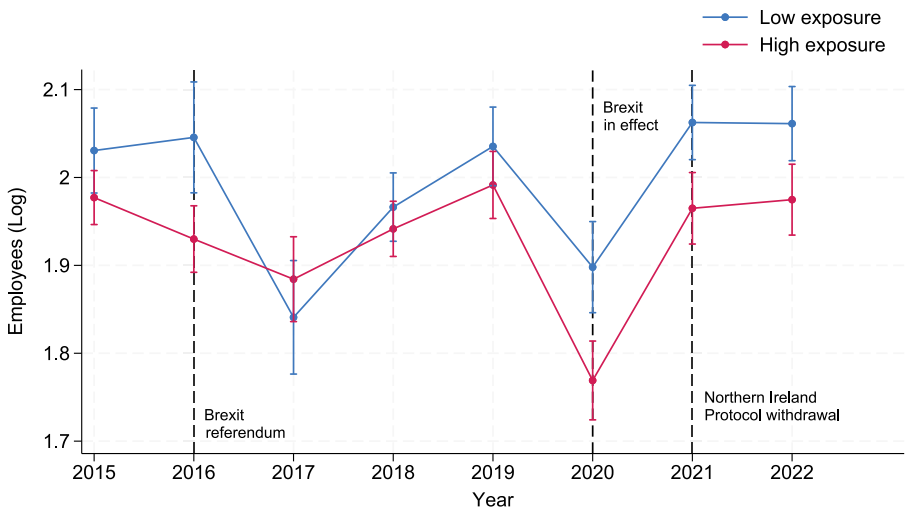

Fig. 3. Employment of High vs. Low-exposure Firms.
Notes: Fig. 3 displays the average number of employees (in logarithmic form) for firms categorized by their exposure to Brexit. *Low-exposure* firms (N = 21,395) are defined as those located at or below the median distance to Northern Ireland's border, while firms beyond this threshold are categorized as *high-exposure* firms. The figure also includes a 95% confidence band for each year represented in the data. It marks the timing of three significant events: the Brexit referendum in 2016, the official implementation of Brexit in January 2020, and the withdrawal of the Northern Ireland Protocol in January 2021. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

specification (1), we do not control for firm fixed-effects since the combination of industry fixed effects and location (i.e., distance to the port of Newry) identifies firms that do not switch locations throughout the sample. This approach is consistent with the gravity-trade literature that studies, among others, the role of distance in trade (Rose, 2004, 2005; Subramanian and Wei, 2007). Standard errors are clustered by the firm to manage the correlation of observations within a firm where Brexit exposure is measured.

As a preamble to our analysis, we investigate whether the firms with low exposure (i.e., close to the border) to Brexit are, on average, *ex-ante* similar to the firms with high exposure (i.e., far from the border). To that end, Fig. 3 plots the average number of employees (in log) of firms with low exposure and high exposure to Brexit. Here, we define *low-exposure* firms as firms with a distance to Northern Ireland's border that is smaller than or equal to the median distance to such a border. The remaining firms are considered *high-exposure*. In Fig. 3, we include the confidence band (at the 95% level) for each year in the sample, along with the timing of three key events: the Brexit referendum in 2016, when Brexit took effect (January 2020), and the withdrawal of the Northern Ireland Protocol (January 2021).

One key insight from Fig. 3 is that before Brexit took effect (on January 2020), low-exposure firms (blue line) and high-exposure firms (red line) largely had statistically similar numbers of employees, as evidenced by their overlapping confidence intervals, with the only exception being 2016 when the Brexit referendum results were announced.[10] In other words, before the treatment (i.e., the Brexit implementation in 2020), low-exposure firms are *statistically indistinguishable* from high-exposure firms. As soon as Brexit took effect in January 2020, the number of employees in low-exposure firms became statistically different (at the 95% level) from those in high-exposure firms.

While weighting, clustering, and stratification within the survey design help obtain more precise standard errors, our dataset comprises 342,320 observations, with 83,870 responses (approximately 24.5%) for our primary variable of interest, *Employees (Log)*$_{i,t}$. Hastie et al. (2009) note that various means of subsetting the data, such as selecting respondents for specific purposes, may cause the original weights to not accurately reflect the representation of this subgroup relative to the overall population. Their concerns are shared by many in the related literature (Winship and Radbill, 1994; Hastie et al., 2009; Solon et al., 2015; Bollen et al., 2016). Consequently, we opt not to use a survey-weighted approach for our main analyses. However, to check the robustness of our findings, we also consider survey-weighted estimations, which are detailed in the online Appendix. Despite the potential drawbacks of using survey weights for subsamples, as noted in the literature, our results are robust to survey-weighing.

4. Data

4.1. Longitudinal small business survey

Our paper leverages a large-scale longitudinal small business survey (LSBS) of UK small business owners and managers between 2015–2022 (UK Data Services, 2023). This survey is one of the most extensive longitudinal data for UK SMEs, comprising eight

[10] We find that our results are robust to excluding the pre-2016 sample.

waves. The impetus of the survey is to investigate the economic condition of the SMEs, the perception of the barriers and enablers of the SMEs' growth, and their behaviors and planning across numerous economic activities, considering their heterogeneous characteristics. Initiated by BEIS, the survey was first conducted by BMG Research Limited Company in 2003 and then continued annually with a similar research design targeting UK SMEs (UK Government BEIS, 2023). LSBS past surveys have been widely used in the literature to explore UK SMEs' economic and innovation behavior and the business barriers they face (Brown et al., 2022, 2019; Harris and Moffat, 2022). For brevity, we leave a more detailed description of the data, the related questionnaires, and the corresponding descriptive statistics in Appendix A.

4.2. Firm-level variables

Our primary dependent variable of interest is the natural logarithm of one plus the number of employees *Employees (Log)*, which is from the following question "Approximately how many employees are currently on your payroll in the UK, excluding owners and partners, across all sites?"(UK Government BEIS, 2023). This question aims to capture the official number of employees working at the business sites. Surveying firms about their number of employees is a common approach in existing literature (Altig et al., 2022). This variable reflects the operational efficiency of business activities. In addition, the data provided categorizes the number of employees into eight groups, offering an alternative measure to validate the robustness of our previous model specification. It is worth mentioning that Boeri et al. (2020) differentiate between self-employed businesses (with no employees) and SMEs with employees. Our survey data includes both groups.

Turning to our independent variables, *Brexit* is a dummy variable where surveyed SMEs from 2020, when Brexit was officially implemented, are coded as one, and those surveyed before 2020 are coded as "zero". This variable captures the period of the Brexit implementation in 2020, while the existing literature primarily focuses on the 2016 Brexit referendum (Bloom et al., 2019; Fernandes and Winters, 2021; Corsetti et al., 2022; Campello et al., 2022). One of our key variables is the distance to the Irish border, a plausibly exogenous proxy to capture Brexit exposure. To compute this distance, we calculate the geographical (straight) distance between the locations where the surveyed SMEs are based and Newry, a city bordering Ireland, excluding those SMEs who have changed or moved their locations during 2015–2022. The distance between two places (x_1, y_1) and (x_2, y_2) is calculated using the following formula (Weber and Péclat, 2017)

$$Distance = \sqrt{(x_2 - x_1)^2 + (y_2 - y_1)^2}, \tag{2}$$

where *Distance* is measured in planar units. Throughout the paper, we take the nature logarithm of *Distance*. Since *Distance* can be sensitive to other locations along the border between the Republic of Ireland and Great Britain, we select the city of Derry as the alternative point on the Irish border from which *Distance* is computed. Since the survey only identifies firm locations within LEPs, we correlate these with the LADs to ensure no variation within firms across years, providing that the firms do not change their locations.

We focus on three mechanism variables: *Firm R&D*, *Expected Growth*, and *Trade Exposure*. First, *Firm R&D* is based on the survey question "Amount invested in R&D in the last 12 months?" This variable is categorical and captures the intensity of R&D activities.[11] As the nature of this survey question is based on firms' R&D expenditures in the last 12 months, we compute our *Firm R&D* for firm i in year t by taking one lead (i.e., one period ahead) of this variable. Although several databases record firm activities related to innovation, such as the UK Community Innovation Survey (Audretsch and Belitski, 2020; Frenz and Ietto-Gillies, 2009), or bespoke surveys (Bloom et al., 2019), our study utilizes the questions available in UK Government BEIS (2023). This approach allows us to effectively match the data with firms' characteristics and locations to estimate our specification models.

Second, *Expected Growth* is derived from the responses to the survey question "Summary of expected growth in the next year". Based on this question, we construct a binary variable to capture the firm expectation with respect to growth. This variable is coded as one if firms anticipate moving from a lower to a higher growth category, reflecting a more optimistic view of their future growth. Conversely, a value of "zero" indicates that firms have lowered their growth expectations, signifying a more pessimistic outlook. We exclude responses from the tenth category where firms indicate uncertainty or refusal to answer. Thus, our expected growth binary variable takes a value of "one" for positive future growth expectations and "zero" otherwise.

Third, *Trade Exposure* is a binary variable that indicates whether the surveyed firm traded (export or import) before Brexit implementation in 2020. We consider two measures of trade exposure: within the EU and with all countries, including the EU. For brevity, we leave the details of the surveyed questions used to construct this variable in the online Appendix.

Our control variables include *Firm Age*, *Residential Office*, *Female Owned*, *Legal Status*, and *Labor Supply*. *Firm Age* categorizes firms into age groups from youngest to oldest: (1) 0–5 years, (2) 6–10 years, (3) 11–20 years, and (4) over 20 years, based on the survey question, "In what year did the firm start the business?". Previous research supports the influence of firm age on employment. For example, Brown and Medoff (2003) suggest that newly established firms may not initially set up pension or health insurance schemes, potentially making it challenging to recruit employees. Aubert et al. (2006) argue that adopting new technologies may hinder recruiting new employees; thus, including firm age as a control variable captures these dynamics.

Residential Office, on the other hand, is a dummy variable that takes a value of "one" if the firm has a separate business premise from home. *Female Owned* is a dummy variable that dictates whether the firm has a female owner. *Legal Status* is a categorical

[11] The R&D categories include (1) less than £5000, (2) £5000 to £24,999, (3) £25,000 to £99,999, (4) £100,000 to £499,999, (5) £500,000 to £999,999, (6) £1 million to £9,999,999, and (7) £10 million or more.

variable indicating the firm's legal status.[12] *Labor Supply* is a dummy variable that takes a value of "one" if the firm reports having experienced difficulty recruiting skilled and unskilled labor and "zero" otherwise.

We leverage the existing literature to select control variables (such as *Firm Age*, *Residential Office*, *Female Owned*, *Labor Supply*, and *Legal Status*). Using the number of employees as the dependent variable, Angelini and Generale (2008) accounted for firm-level heterogeneity. The rationale for including *Residential Office* (whether the SME operates as a separate business) follows Kim and Parker (2021), who document that entrepreneurs who work from home without a separate business premise are less likely to hire or recruit employees. Machokoto and Nyantakyi (2023) show that firms with female ownership may benefit from better employee welfare. Similarly, female-owned businesses are associated with better performance, higher sales, and more customers (Pueyo et al., 2020).

Turning to controlling for the supply side of labor, we find the number of employees in a certain period is likely influenced by constraints arising from the labor supply (Blundell et al., 1987). Due to their relative size, these firms are unlikely to be able to affect labor supply through policy lobbying at the local level, as significant barriers are associated with the process (Kerr et al., 2014), and the potential gains are relatively low (Harstad and Svenson, 2011). Therefore, by controlling for labor supply, we account for the extent to which firms could not recruit their employees due to supply-side factors. Finally, changes in legal status could impact the number of employees and post-entry firm growth (Koch et al., 2013), which motivates our choice of controlling for the firms' legal status.

4.3. Identifying firm locations

When conducting the survey, postcodes were used as a sorting criterion to avoid duplication, and businesses were grouped by LEPs. LEPs are not-for-profit organizations formed by BEIS that aim to bring together various stakeholders such as businesses, educators, and local government offices. In our data, 38 LEPs cover the entirety of England. The geographical locations of the UK SMEs are measured by matching the postcode from the UK LADS map (UK Data Government, 2023) and the LEPS' postcodes from the data.

Given the availability of the LEP data from the 2023 survey by the BEIS, we load geographic data from the boundaries of LADS as of December 2023 and merge it with the LEP data. This merger facilitates analyses at a different administrative level. We also refine string data for more transparent labeling and calculate distances from specific locations to each district. We apply a natural logarithm transformation to these distances to prepare them for statistical analysis. We address mismatches between LEPs and LADs by managing cases where multiple districts fall within a single partnership. This meticulous preparation is crucial for enabling comprehensive spatial and statistical analyses.

Our first step is identifying key areas in the survey questions to provide detailed information for creating the location sample. With that in mind, we focus on four nations: England, Wales, Scotland, and Northern Ireland. Specifically, firms in England are associated with their respective LEPs. We then manually match the LEP information with LADs to determine the firms' locations precisely. The detailed list of our matching list can be found in the online Appendix.

It is worth noting that the LEP data only assists in identifying firms located in England. To circumvent this problem, we obtain information about rural and urban areas in Northern Ireland. Specifically, we target the exact locations of firms in Belfast City and Derry City to precisely match their locations in Northern Ireland. Turning to the rest of the UK, the survey does not provide information that matches the locations of firms in Scotland and Wales. We conduct exercises to address this issue, excluding firms from both Scotland and Wales (discussed in the online Appendix) and assigning all Scottish firms to Edinburgh and Welsh firms to Cardiff (detailed in the online Appendix). Our main results remain robust throughout all these exercises.

5. Results

5.1. Baseline results

We begin by exploring the question: How does Brexit implementation affect the labor choices of SMEs in the United Kingdom? To that end, we estimate Eq. (1) using data described in Table 1. The dependent variable in our analysis is *Employees (Log)*.[13] As outlined previously, our identification strategy explores the variation in firm distance to the Irish border among firms that have kept the same location before and after the referendum in 2016. In particular, using the distance to the border serves as a plausibly exogenous proxy for Brexit exposure, enabling us to examine the causal effects of Brexit implementation on firm labor demand.

[12] These legal statuses include: Sole proprietorship/trader, Private limited company, limited by shares (LTD), Public Ltd. Company (PLC), Partnership, Limited liability partnership, Private company limited by guarantee, Community Interest Company, Friendly Society, Co-operative, Industrial & Provident Society, Private Unlimited Company, Foreign Company, a Trust, an Unincorporated Association, and others.

[13] In the online Appendix, we use the category of number of employees and the raw data of number of employees as the dependent variable as alternative measures of employment. In particular, we conduct an analysis using a new dependent variable, categorized into eight distinct groups based on the number of employees. The categories include (1) Zero unregistered, (2) Zero registered, (3) Micro 1–4, (4) Micro 5–9, (5) Small 10–19, (6) Small 20–49, (7) Medium 50–99, (8) Medium 100–249. We present our findings in the online Appendix with two subsections including (i) the raw number of employees and (ii) the number of employees in categories. It is reassuring that our results align with the core findings, using a logarithm of the number of employees plus one as the dependent variable does not drive our results.

Table 1
Baseline results: $Brexit_t$ and $Employees\ (Log)_{i,t}$.

	Dependent variable: $Employees\ (Log)_{i,t}$					
	Full sample			No switching		
	(1)	(2)	(3)	(4)	(5)	(6)
$Brexit_t \times Distance_i$	−0.165***	−0.136***	−0.115***	−0.157***	−0.128***	−0.113***
	(0.046)	(0.044)	(0.038)	(0.046)	(0.044)	(0.039)
$Distance_i$	−0.030	0.050	−0.136***	−0.028	0.050	−0.136***
	(0.040)	(0.038)	(0.032)	(0.040)	(0.039)	(0.032)
$Brexit_t$	2.111***	1.773***	1.425***	2.010***	1.674***	1.394***
	(0.587)	(0.561)	(0.492)	(0.595)	(0.569)	(0.498)
Constant	2.350***	0.792	1.170***	2.327***	0.799	1.170***
	(0.512)	(0.494)	(0.411)	(0.519)	(0.500)	(0.417)
Control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.001	0.090	0.372	0.000	0.088	0.372
Observations	63,558	63,558	50,163	61,318	61,318	48,288

Notes: This table presents all baseline results for the effects of $Brexit_t$ on $Employees\ (Log)_{i,t}$ as outlined in the specification model (1). $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise), and $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)-(3) cover the full sample, while Columns (4)–(6) only include the non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1 presents the estimates from Eq. (1), in which we consider the full sample (specifications 1–3), and a subsample in which we consider firms that do not switch locations throughout the sample (specifications 4–6). All specifications incorporate fixed effects for industry and year to account for the underlying differences across sectors and over time.

The results in Table 1 reveal a negative and statistically significant coefficient for the interaction term $Brexit_t \times Distance_i$ across all regressions. Our baseline estimates indicate that the implementation of Brexit in 2020 has caused *exposed* firms to decrease their workforce on average by between 11.30% and 15.70% if they move their business from the current location to the border relative to non-exposed firms. In other words, Brexit has caused *exposed* firms to reduce their labor demand by up to 15.7% relative to non-exposed firms. Even though the total effects of the policy on *all* firms are positive (i.e., the sum of the coefficients on $Brexit_t$ and $Brexit_t \times Distance_i$, we note that among *exposed* firms, the effects of Brexit (i.e., the coefficient on $Brexit_t \times Distance_i$) are consistently negative.[14] While not denying the importance of interpreting the overall effects of Brexit on labor demand, we shall focus on the Brexit effects among *exposed* firms relative to their non-exposed counterparts going forward.

To provide an alternative approach to assess the magnitude of our effects, we use the actual number of employees–specifically, the raw count of employees on SMEs' payrolls – as the dependent variable.[15] The findings remain consistent with our benchmark results reported in Table 1. On average, following the implementation of Brexit, firms with higher exposure reduced their workforce by approximately two employees for every one percent increase in their distance from the Irish border, relative to less-exposed firms.

We also find it reassuring that in our benchmark results in Table 1, the coefficients for $Distance\ (to\ Newry)_i$ are not statistically different from zero when no controls are included (Specifications 1, 2, 4, and 5).[16] The lack of significance in these estimates is not surprising, given the observation in Fig. 3 that firms further away from the Irish border are ex-ante not significantly different from firms closer to the Irish border in terms of the number of employees. In addition, the aggregated coefficient effects of Brexit are significant and positive, indicating that the net overall impact of Brexit on labor across all firms, whether exposed or not, is positive. This finding aligns with the overall recent trajectory of employment of SMEs.[17] In aggregate, total employment post-2020 remains relatively stable overall and even increase, in spite of the timing of Brexit and COVID-19.[18]

To the extent that our identification strategy builds around firms that did not change their location after the Referendum in 2016, one potential issue arises in how these firms may self-select into keeping their locations (i.e., selection issue). Throughout the paper, we present the regression results for samples that include only non-switching and ones that include both switching and non-switching firms (i.e., full sample). Our results are consistent across all samples: firms exposed to Brexit reduced their labor demand more than firms with low exposure. More importantly, such consistency between the two sample groups indicates that the selection issue does not systematically bias our results. In addition, it is possible for firms that keep locations the same to operate in a way that makes it challenging for them to relocate. Such a possibility motivates us to control for industry fixed effects as these

[14] Our result that the aggregate effects of the timing of Brexit on firm-level employment are positive is consistent with the aggregate evidence. Indeed, as noted in the online Appendix, UK SMEs were substantially involved in international trade between 2015 and 2021. In addition, employment remained relatively stable and even increased slightly around the Brexit implementation period.

[15] For brevity, these results are presented in Section G.1 of the online Appendix.

[16] This coefficient is statistically different from zero only when controls are included, which is expected since some controls are correlated with the distance measure (Table A.4).

[17] Our qualitative illustrations of the number of people employed by SMEs in the UK (2012–2024) for SMEs can be found in the online Appendix.

[18] See, for example, Figure A.3 in the online Appendix.

Table 2
The impact of Brexit on *Employees (Log)*$_{i,t}$ - Robustness test (N.I. vs. Great Britain).

	Dependent variable: *Employees*$(Log)_{i,t}$					
	Full sample			No switching		
	(1)	(2)	(3)	(4)	(5)	(6)
Brexit$_t$ × Great Britain$_i$	−0.135**	−0.136***	−0.090**	−0.168***	−0.760***	−0.200**
	(0.053)	(0.050)	(0.045)	(0.053)	(0.089)	(0.086)
Great Britain$_i$	0.107**	0.112***	−0.105***	0.129***	0.044	−0.226***
	(0.046)	(0.043)	(0.036)	(0.047)	(0.065)	(0.057)
Brexit$_t$	0.121**	0.156***	0.037	0.121**	0.096	−0.042
	(0.051)	(0.051)	(0.045)	(0.051)	(0.082)	(0.074)
Constant	1.864***	1.271***	−0.500***	1.864***	1.090***	−0.641***
	(0.045)	(0.058)	(0.053)	(0.045)	(0.082)	(0.081)
Control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.000	0.092	0.377	0.001	0.119	0.388
Observations	83,870	83,870	65,838	19,380	19,380	15,839

Notes: This table displays the baseline results for the effects of *Brexit*$_t$ on *Employees (Log)*$_{i,t}$, using a conventional difference-in-difference approach. *Great Britain*$_i$ is a dummy variable assigned a value of one if the firm is located in Great Britain. The *Brexit*$_t$ variable is a dummy indicator ("one" - post-2020; "zero" - otherwise). Columns (1)–(3) cover the full sample, while Columns (4)–(6) include non-switching firms only. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

industry-specific effects might drive the extent to which firms relocate and, subsequently, their labor demand in response to Brexit. Indeed, the continued significance across specifications with industry fixed effects suggests that industry-specific technology does not undermine our main result on the adverse reaction of labor demand among firms exposed to Brexit relative to those who are not.

Our result complements the large and growing literature on Brexit. For example, Bloom et al. (2019) demonstrate that approximately 10% of respondents from a sample of 42,000 active UK businesses with more than ten employees identified labor availability as the largest source of Brexit-related uncertainty, highlighting the significant impact of Brexit on workforce dynamics. Our findings also align with the existing literature on labor reduction post-Brexit (Fuller, 2021; Sampson, 2017), which suggests that the British labor market may become less accessible to foreign workers (Born et al., 2019).

5.2. Robustness

This section presents a series of exercises to test the robustness of the main results of our paper. First, we use a dummy variable to determine whether a firm is located in Northern Ireland or Great Britain instead of the distance to the Irish border as a proxy for Brexit exposure. Second, instead of the port of Newry, we use the port of Derry — another major transportation hub near the Irish border for products entering the Republic of Ireland to compute the distance to the Irish border. Third, we conduct a placebo test, randomly assigning firms to different locations and randomizing the timing of Brexit implementation. Fourth, our analysis excludes the period before the Brexit referendum in 2016. Fifth, we account for the expectation effects leading to Brexit implementation by interacting our benchmark proxy for Brexit exposure (i.e., distance to the border) with individual year dummies. Sixth, we consider whether the timing of COVID-19 may impact our results. Finally, we also include regressions with additional control and subsample analysis. Overall, these robust analyses support the central hypothesis that firms located further from the Irish border experienced more significant impacts due to the implementation of Brexit in 2020.

5.2.1. Alternative measure for Brexit exposure

In the baseline specification in Eq. (1), we use the firms' distance to the port of Newry as a proxy for Brexit exposure. One potential criticism arising from such a distance stems from its continuous nature: the error terms generated from estimating Eq. (1) may correlate with the independent variables. To check whether this is the case, we use a dummy variable that indicates whether a firm is located in Northern Ireland or Great Britain in place of the distance to the Irish border to capture such exposure. Specifically, we consider the following regression specification:

$$\text{Employees (Log)}_{i,t} = \alpha + \beta(\text{Brexit}_t \times \text{Great Britain}_i) + \gamma\text{Great Britain}_i + \delta\text{Brexit}_t + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t}, \quad (3)$$

where *Great Britain*$_i$ is a binary variable that indicates whether the firm is located in Great Britain and the remaining notations follow from Eq. (1). The estimates for Eq. (3) are presented in Table 2, in which the first three columns use the full sample of all firms. The last three columns only use firms that do not switch locations throughout the sample period (2015–2022).

One critical insight from Table 2 is that firms located in Great Britain are more likely to be impacted by Brexit in effect relative to firms located in Northern Ireland. Specifically, on average, firms located in Great Britain reduced their number of employees by 16.8% following the implementation of Brexit, relative to firms in Northern Ireland. The continued significance of these results across all specifications is consistent with our benchmark result that firms located near the Irish border (and therefore are less

Table 3
Robustness tests – An alternative measure for *Distance*.

	Dependent variable: $Employees\ (Log)_{i,t}$					
	Full sample			No switching		
	(1)	(2)	(3)	(4)	(5)	(6)
$Brexit_t \times Distance\ (to\ Derry)_i$	−0.163***	−0.132***	−0.091**	−0.157***	−0.129***	−0.079**
	(0.042)	(0.041)	(0.036)	(0.044)	(0.042)	(0.037)
$Distance\ (to\ Derry)_i$	−0.011	0.056	−0.144***	−0.007	0.061	−0.155***
	(0.037)	(0.036)	(0.031)	(0.039)	(0.038)	(0.032)
$Brexit_t$	2.118***	1.748***	1.123**	2.037***	1.717***	0.967**
	(0.551)	(0.531)	(0.473)	(0.572)	(0.549)	(0.480)
Constant	2.107***	0.702	1.313***	2.059***	0.641	1.437***
	(0.484)	(0.474)	(0.402)	(0.515)	(0.501)	(0.418)
Control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.000	0.090	0.372	0.000	0.088	0.372
Observations	63,558	63,558	50,163	61,318	61,318	48,288

Notes: This table displays the robust results for Brexit in effect on firm employment, using an alternative measurement $Distance\ (to\ Derry)_i$ instead of $Distance_i$. $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise). Columns (1)–(3) cover the full sample, while Columns (4)–(6) include non-switching firms only. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

exposed to Brexit in effect) are less inclined to reduce their labor demand than firms located in Great Britain. More importantly, the results presented in Table 2 suggest that using continuous distance to capture Brexit exposure does not bias the estimated effects.[19]

5.2.2. Alternative location for border crossing

Our previous analysis has primarily utilized the spatial variation from the proximity to the border between Northern Ireland and the Republic of Ireland, commonly called the Irish or British–Irish border. Established in 1923 to facilitate the free movement of people (and in 1993 for goods), the precise timing of this border's creation should not raise concerns regarding its influence on identifying UK firms' responses. We now evaluate the robustness of our results by considering a different border crossing along the border between the United Kingdom and the Republic of Ireland, using the geographical area of "Derry City and Strabane", an alternative to the port of Newry.

Our results for an alternative measurement using the border point of Derry are presented in Table 3. The estimated coefficients for the interaction term (i.e., $Brexit_t \times Distance\ (to\ Derry)_i$) across six specifications in Table 3 are negative and significant, with the point estimates ranging between −0.079 and −0.157. These results suggest that the baseline estimates' signs and statistical significance remain robust despite these variations.

5.2.3. Placebo tests: Randomizing firm location and Brexit timing

We examine whether our main results are driven by a particular draw of distance or the timing of Brexit. First, we randomly assign firms to various locations across the UK instead of using the actual distance from these surveyed firms to the border. Turning to the specifics, we draw the firms' distance from a normal distribution with the same mean and standard deviation as our original variable $Distance_i$. We estimate Eq. (1) using the placebo distance and repeat this exercise 2000 times. Second, we randomly assign the year that Brexit is in effect to firms. We then estimate Eq. (1) using the placebo timing and repeat this exercise 2000 times.

Fig. 4 presents the distribution of the estimates for the interaction term between Brexit and distance over the 2000 replications using placebo distance (Panel A) and placebo timing (Panel B). In each panel, we also overlay the estimate using the actual set of distance and timing using a vertical line. In no instance in Fig. 4 is $Brexit_t \times Distance\ (placebo)_i$ precisely estimated using either placebo distance or timing. Indeed, our estimate using actual data is well below the 1% values for both distributions of placebo estimates. This result indicates that our main results are unlikely to be driven by a random draw of either distance or Brexit timing.[20]

5.2.4. Accounting for Brexit referendum expectation

Building on the observation from Fig. 3 that the 2016 Brexit referendum might have influenced the results, we exclude the pre-2016 sample to check on the robustness of our results. Our findings are reported in Table 4. Overall, after excluding data from the 2016 Brexit referendum, we find the negative impacts of Brexit implementation on labor demand for exposed firms to range from 9.5% to 16.3% on average relative to non-exposed firms if these firms are relocated to the border entirely. These estimates are statistically similar to the baseline results presented in Table 1. The consistency across Tables 1 and 4 suggests that our main findings are robust and unaffected by including the 2016 Brexit referendum data.

[19] The benchmark exposure measure (i.e., distance) captures both the extensive (i.e., being "treated" by Brexit) and intensive (i.e., "how exposed") margin of Brexit. In contrast, the binary variable $Great\ Britain_i$ used in Table 2 only captures the former.

[20] Our results also hold when we draw the firms' distance from a normal distribution with the same mean and standard deviation as our original variable $Distance_i$ for firms located only in Great Britain.

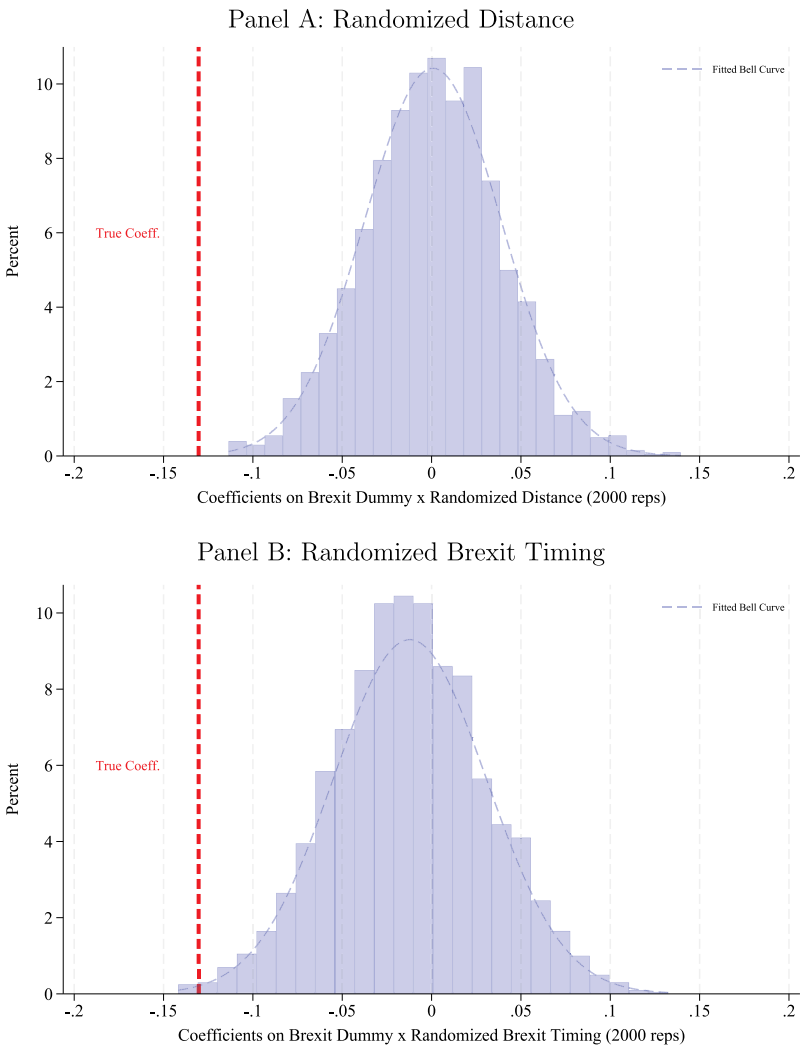

Fig. 4. Estimates using randomized firm distance and Brexit timing.
Note: Panel A displays a placebo test for Brexit in effect on firm employment, using a placebo measurement *Distance (Placebo)*$_i$ instead of *Distance*$_i$, which is a random variable from the same mean and standard deviation distribution. Panel B displays a placebo test for the timing of Brexit, using a randomized year instead of using 2020 as the year Brexit is in effect. Across the two panels, we repeat the exercise 2000 replications and report the distribution of the estimated coefficients on Brexit$_t$ × Distance (Placebo)$_i$ from estimating Eq. (1) (using either placebo distance or timing). The true estimates using actual distance and timing from our baseline results are overlaid as a red vertical line in the figure. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

5.2.5. *Accounting for the expectation of Brexit implementation*

To understand how firms' expectations leading to Brexit implementation may impact our results, we consider a variation of the benchmark regression model in Eq. (1) in which we interact the year dummy with the firm exposure to Brexit. The regression model, specified with robust standard errors, is

$$\text{Employees (Log)}_{i,t} = \alpha + \beta(\text{Year}_t \times \text{Distance}_i) + \gamma\text{Distance}_i + \delta\text{Year}_t + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t}, \tag{4}$$

where *Year*$_t$ is contains a set of year dummies, and the remaining notations follow from Eq. (1). Other denotations are similar to our baseline in Eq. (1). Fig. 5 presents the point estimate of β for each year, along with the corresponding 90% (bold-shaded) and 95% (light-shaded) confidence bands. The figure also marks the timing of three key events: the Brexit referendum in 2016, the official implementation of Brexit in January 2020, and the withdrawal of the Northern Ireland Protocol in January 2021.

Fig. 5 shows that the effects of Brexit, as measured by the point estimates of β over time, are mainly insignificant (except for 2017) before the Brexit implementation in 2020. Once Brexit is implemented, we document this policy change's negative and statistically significant effects: firms with higher exposure to Brexit are more likely to cut their labor demand in response to the Brexit effect than non-exposed firms.

Table 4
Robustness check: Excluding pre-referendum.

	Dependent variable: *Employees (Log)*$_{i,t}$					
	Full sample			No switching		
	(1)	(2)	(3)	(4)	(5)	(6)
Brexit$_i$ × Distance$_i$	−0.172***	−0.142***	−0.099**	−0.163***	−0.136***	−0.095**
	(0.045)	(0.043)	(0.039)	(0.046)	(0.044)	(0.040)
Distance$_i$	−0.023	0.063	−0.149***	−0.022	0.063	−0.151***
	(0.042)	(0.041)	(0.035)	(0.043)	(0.041)	(0.035)
Brexit$_t$	2.213***	1.970***	1.282**	2.106***	1.883***	1.230**
	(0.576)	(0.554)	(0.499)	(0.585)	(0.562)	(0.506)
Constant	2.248***	0.534	1.290***	2.230***	0.532	1.309***
	(0.540)	(0.519)	(0.443)	(0.549)	(0.527)	(0.451)
Control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.001	0.084	0.374	0.001	0.082	0.374
Observations	46,637	46,637	40,302	44,840	44,840	38,662

Notes: This table displays the robustness for Brexit in effect on firm employment, excluding the pre-referendum (2016). It means that all regressions cover the period from 2017–2022. *Brexit*$_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise) while *Distance*$_i$ measures the firm's proximity to the Irish border. Columns (1)–(3) cover the full sample, while Columns (4)–(6) include non-switching firms only. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

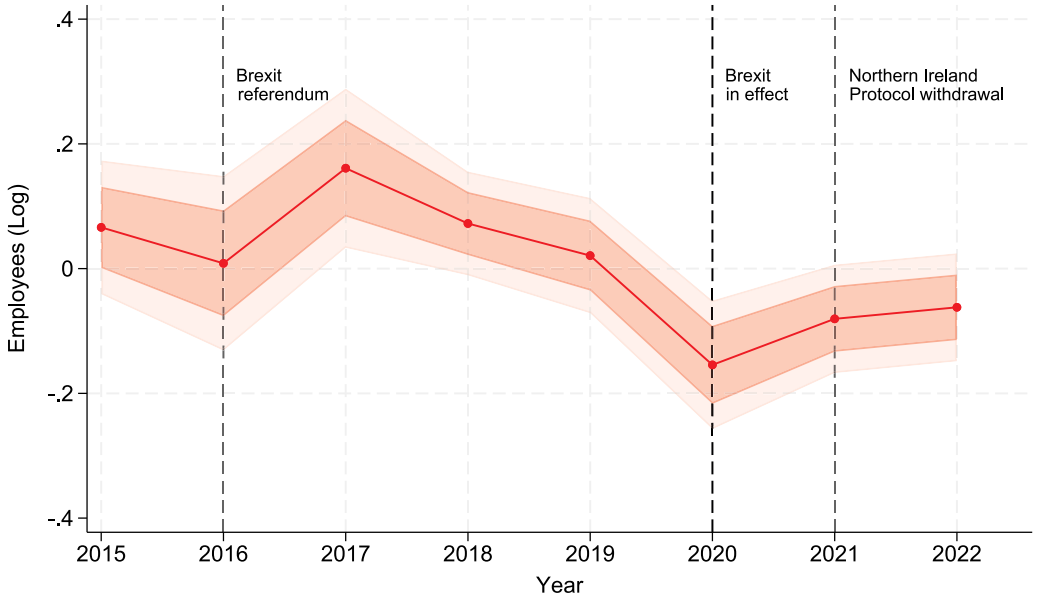

Fig. 5. Regression coefficients of Employees (Log)$_{i,t}$ on Year$_t$ × Distance$_i$.
Notes: Fig. 5 illustrates the coefficients of Distance$_i$ × Year$_t$ from each regression analysis. The regression model, specified with robust standard errors, is defined as Employees (Log)$_{i,t} = \alpha + \beta(\text{Year}_t \times \text{Distance}_i) + \gamma\text{Distance}_i + \delta\text{Year}_t + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t}$, where Employees (Log)$_{i,t}$ represents the natural logarithm of one plus the number of employees as the dependent variable. The fixed effects λ_k and φ_t correspond to industry and year, respectively. The bold shaded area denotes the 95% confidence interval for the estimated coefficients, while the lighter shaded area corresponds to the 90% interval. The figure also marks the timing of three key events: the Brexit referendum in 2016, the official implementation of Brexit in January 2020, and the withdrawal of the Northern Ireland Protocol in January 2021. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

5.2.6. Accounting for COVID-19 effects

The timing of Brexit implementation coincides with the onset of the COVID-19 pandemic. To check on the possibility that COVID-19 exposure might have exacerbated the extent to which labor demand responds to Brexit implementation, we estimate our baseline results with the existence of COVID-19. Our results are robust to the inclusion of a measure of firm-level exposure to COVID-19. We leave additional details in the online Appendix for brevity.

5.2.7. Additional sensitivity analyses

This section briefly explains how our results remain robust after including additional control variables. Specifically, we account for the nature of trade in our baseline model, and the results continue to yield precisely estimated coefficients, as detailed in the

Table 5
Mechanism test - $Brexit_t$ and $Firm\ R\&D_{i,t}$.

	Dependent variable: $FirmR\&D_{i,t}$			
	Full sample		No switching	
	(1) OLS	(2) Ordinal logit	(3) OLS	(4) Ordinal logit
$Brexit_t \times Distance_i$	1.015**	0.882**	1.100**	1.026***
	(0.450)	(0.349)	(0.457)	(0.356)
$Distance_i$	−0.352	−0.299	−0.388	−0.380
	(0.347)	(0.256)	(0.353)	(0.263)
$Brexit_t$	−13.620**	−11.342**	−14.726**	−13.219***
	(5.816)	(4.493)	(5.908)	(4.570)
Control variables	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adj R-squared	0.052		0.050	
Pseudo R-squared		0.053		0.055
Observations	1,168	1,168	1,084	1,084

Notes: This table presents our mechanism tests, which examine the effects of Brexit on SMEs' R&D spending expenses. It specifically analyzes the categories variable $FirmR\&D_{i,t}$, which represents the R&D expenditure from 2018 to 2022. $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise) while $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(2) cover the full sample, while Columns (3)–(4) include non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

online Appendix. Furthermore, we take a closer look at firms engaged in trading activities. While the estimated coefficients slightly decrease, they remain significant, with detailed regressions provided in the online Appendix. One potential concern is that our baseline results might not be robust if firms relocate. To address this, we identify firms with location changes across the full sample and control for this factor, confirming that our results remain unchanged, as shown in the online Appendix.

6. Mechanism

This section considers potential channels that explain Brexit's negative impact on labor demand, as documented in the previous section. In particular, we find that firms exposed to Brexit are more likely to have lower growth expectations and to increase their R&D expenditure than non-exposed firms. We also find that having *ex-ante* trade exposure can help alleviate the negative effects on labor demand.

6.1. Technological substitution

The extant literature explains the channel for employment to technological substitution under wage shocks (Aaronson and Phelan, 2019; Van Reenen, 1997). The history of technology is not only about automation displacing human labor, but also includes the development of new technologies that respond to potential shocks. Therefore, Acemoglu and Restrepo (2019) argue that this effect could be called "reinstatement effect", which might counter the job reduction from technological development by expanding the roles and increasing the demand for human labor, thereby boosting productivity. Given the findings of well-established studies on such substitution (Aaronson and Phelan, 2022, 2019; Acemoglu and Restrepo, 2019), we hypothesize that UK firms that reduce their number of employees, a process known as labor reduction, are more likely to increase their R&D activities to acquire frontier technology. The following specification is used for our estimation:

$$\begin{aligned}\text{Firm R\&D}_{i,t} = \alpha + \beta(\text{Brexit}_t \times \text{Distance}_i) + \gamma\text{Distance}_i \\ + \delta\text{Brexit}_t + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t},\end{aligned} \tag{5}$$

where $Firm\ R\&D_{i,t}$ denotes the categories of the amount of money spent for R&D activities at firm i in year t. α is the constant term, and $\varepsilon_{i,t}$ is a mean-zero disturbance term. β is the key coefficient, capturing the differential impact of Brexit shocks on $Firm\ R\&D_{i,t}$ within UK firms. Table 5 presents the results of a study examining the impact of Brexit on firms' R&D activities, based on their varying levels of exposure to Brexit from Eq. (5). Such exposure is measured by the firms' proximity to the Irish or British-Irish border.

The coefficients for the interaction term $Brexit_t \times Distance_i$ in Table 5 are significantly positive across our four specifications. Specifically, a one-percent increase in the distance to Newry induces an increase (i.e., on average, roughly one category) in R&D expenditures for business activities among exposed firms relative to non-exposed firms. Taking into account all estimated coefficients, we note that while the aggregate effects of Brexit's timing on R&D are negative, firms with higher exposure to Brexit are more likely to increase their R&D spending.[21] This finding suggests a substitution effect between employment reduction and technological

[21] Our results on the negative effects on *aggregate* R&D spending align with a recent report on the overall trend of R&D expenditure post-Brexit. Indeed, such declines are particularly pronounced among SMEs (Michael and Ospina, 2024).

Table 6
$Brexit_t$ and $Employees\ (Log)_{i,t}$ – $R\&D\ Dummy_{i,t}$ (An additional control variable).

	Dependent variable: $Employees(Log)_{i,t}$					
	Full Sample			No Switching		
	(1)	(2)	(3)	(4)	(5)	(6)
$Brexit_t \times Distance_i$	−0.160***	−0.136***	−0.114***	−0.151***	−0.128***	−0.112***
	(0.046)	(0.044)	(0.038)	(0.046)	(0.044)	(0.039)
$Distance_i$	−0.037	0.049	−0.136***	−0.036	0.049	−0.137***
	(0.040)	(0.038)	(0.032)	(0.040)	(0.039)	(0.032)
$Brexit_t$	2.041***	1.810***	1.429***	1.937***	1.709***	1.398***
	(0.586)	(0.560)	(0.492)	(0.594)	(0.568)	(0.498)
R&D $Dummy_{i,t}$	0.374***	0.389***	0.212***	0.377***	0.391***	0.210***
	(0.031)	(0.030)	(0.027)	(0.031)	(0.031)	(0.028)
Constant	2.425***	0.747	1.155***	2.403***	0.753	1.154***
	(0.511)	(0.493)	(0.411)	(0.518)	(0.499)	(0.417)
Baseline control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.003	0.093	0.373	0.003	0.091	0.373
Observations	63,558	63,558	50,163	61,318	61,318	48,288

Notes: This table presents results for the effects of $Brexit_t$ on $Employees\ (Log)_{i,t}$ as outlined in the baseline specification model, but we add $R\&D\ Dummy_{i,t}$, which is a dummy indicator ("one" - if firms' R&D investments are non-missing; "zero" - otherwise). From the UK Data Archive Data Dictionary, the data code item for $R\&D\ Dummy_{i,t}$ is J5A ("How much have you invested in R&D in the last three years?"). $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise), and $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(3) cover the full sample, while Columns (4)–(6) only include the non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

development in UK SMEs, indicating that exposed firms may compensate for reduced employment with increased investment in technology (Autor et al., 2015) more relative to non-exposed firms. While (Bloom et al., 2019) find that Brexit has reduced spending on intangibles such as R&D in their surveyed firms, the effects might differ in SMEs. These smaller firms may reduce the number of employees to increase their research and development activities.

One potential explanation for the reduction in labor demand following Brexit is the financial constraints arising from such a policy change. We consider this channel in the online Appendix and find no evidence of financial constraints impacting firm labor demand among the SMEs surveyed, which is a significant finding.[22] For brevity, we discuss these results in the online Appendix.

Given our result that Brexit exposure prompts firms to increase their R&D activity, we next ask whether firms' R&D choice might have affected how firms adjusted their labor demand following Brexit. Indeed, as the sample used in the analysis includes both R&D and non-R&D firms, we reconsider our baseline regression described in Eq. (1) where the set of control $\mathbb{X}_{i,t}$ now consists of a dummy variable that indicates whether the firm engages in R&D. It is reassuring, as Table 6 shows, that our main result on the effects of Brexit on labor demand remains robust. This result suggests that R&D status does not impact the extent to which firms exposed to Brexit cut their labor force in the face of such a policy change.

We also use an alternative measure of R&D, which captures the R&D investment amount in absolute values (from the UK Data Archive Data Dictionary, the data code item for R&D investment amounts is J5A: "How much have you invested in R&D in the last three years?"). We estimate the following regression specification

$$\begin{aligned}\text{R\&D (Log)}_{i,t} = \alpha &+ \beta_1(\text{Brexit}_t \times \text{Distance}_i \times \text{Employees (Log)}_{i,t}) \\ &+ \beta_2(\text{Distance}_i \times \text{Brexit}_t) + \beta_3(\text{Distance}_i \times \text{Employees (Log)}_{i,t}) \\ &+ \beta_4(\text{Brexit}_t \times \text{Employees (Log)}_{i,t}) + \gamma\text{Distance}_i + \delta\text{Brexit}_t \\ &+ \theta\text{Employees (Log)}_{i,t} + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t}, \end{aligned} \tag{6}$$

where we interact the log of the number of employees plus one $Employees(Log)_{i,t}$ with the Brexit exposure measure ($Distance_i$) and Brexit dummy. Our coefficient of interest is β_1, which captures the elasticity of substitution between labor demand and R&D for firms exposed to Brexit relative to firms not exposed to Brexit after Brexit implementation in 2020.

We present the results for Eq. (6) in Table 7, where the dependent variable is the R&D investment amount in logarithm $R\&D\ (Log)_{i,t}$. Table 7 shows that our results are robust even if we use these alternative measures of R&D (i.e., $R\&D\ (Log)_{i,t}$). We replicate our baseline regressions in Columns (2) and (4) in Table 7 with this $R\&D\ (Log)_{i,t}$ dependent variable and find the results in Columns (2) and (4) to be consistent with the benchmark R&D results in Eq. (5). Our main coefficient of interest (i.e., β_1) captures the elasticity of substitution between labor demand and R&D for firms exposed to Brexit relative to firms not exposed to Brexit after Brexit implementation in 2020. The negative and significant estimate of $Employees\ (Log)_{i,t} \times Brexit_t \times Distance_i$ reported in Columns (1) and (3) in Table 7 directly shows that firms exposed to Brexit did indeed substitute away from labor into additional R&D.

[22] In addition, we find it reassuring that our results remain robust even after controlling for the number of employees and trade exposure.

Table 7
$Brexit_t$ and $R\&D\ (Log)_{i,t}$ – Triple interaction: $Employees\ (Log)_{i,t}$, $Brexit_t$, and $Distance_i$.

	Dependent variable: $R\&D\ (Log)_{i,t}$			
	Full sample		No switching	
	(1)	(2)	(3)	(4)
Employees $(Log)_{i,t}$ × $Brexit_t$ × $Distance_i$	−0.243*		−0.246*	
	(0.146)		(0.147)	
Employees $(Log)_{i,t}$ × $Distance_i$	−0.119**		−0.123***	
	(0.047)		(0.048)	
Employees $(Log)_{i,t}$ × $Brexit_t$	3.699**		3.706**	
	(1.871)		(1.887)	
$Brexit_t$ × $Distance_i$	1.415***	0.527**	1.432***	0.571**
	(0.424)	(0.241)	(0.428)	(0.243)
$Distance_i$	0.225**	−0.002	0.230**	−0.009
	(0.104)	(0.070)	(0.107)	(0.071)
$Brexit_t$	−9.291*	3.376	−9.459*	2.810
	(5.498)	(3.113)	(5.551)	(3.131)
Employees $(Log)_{i,t}$	1.632***	0.167***	1.684***	0.165***
	(0.610)	(0.013)	(0.620)	(0.013)
Constant	−3.032**	−0.187	−3.096**	−0.091
	(1.336)	(0.916)	(1.364)	(0.923)
Baseline control variables	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adj R-squared	0.929	0.924	0.930	0.926
Observations	6,112	6,112	5,907	5,907

Notes: This table presents results for the effects of $Brexit_t$ on $Employees\ (Log)_{i,t}$ as outlined in the baseline specification model, where we also include the triple interaction ($Employees\ (Log)_{i,t}$ × $Distance_i$ × $Brexit_t$). The dependent variable is R&D ($R\&D\ (Log)_{i,t}$), which is the R&D investment amounts in logarithm (from the UK Data Archive Data Dictionary, the data code items for R&D investment amounts is J5A: "How much have you invested in R&D in the last three years?"). $Employees\ (Log)_{i,t}$ is the natural logarithm of one plus number of employees. $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise), and $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(2) cover the full sample, while Columns (3)–(4) only include the non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.2. Expectation

The current literature explores the relationship between labor reduction and technological development and how UK firms have formed their expectations regarding Brexit events. Born et al. (2019) document a downward adjustment in growth expectations following the Brexit referendum in 2016. Similarly, Bloom et al. (2019) report that firms anticipated reducing their investments, with pessimistic expectations observed among international firms (Hassan et al., 2024). In this study, we extend the existing literature by explaining why the UK firms choose to reduce their number of employees based on expectations.

Using the survey question "Summary of expected growth in next year" from (UK Government BEIS, 2023), we create a dummy variable to determine whether firms expect to achieve economic growth in the coming year. We consider the regression specification in Eq. (7) for this mechanism as follows:

$$\begin{aligned}\text{Expected Growth}_{i,t} = \alpha &+ \beta(\text{Brexit}_t \times \text{Distance}_i) \\ &+ \gamma\text{Distance}_i + \delta\text{Brexit}_t + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \varepsilon_{i,t},\end{aligned} \tag{7}$$

in which, $Expected\ Growth_{i,t}$ represents a binary variable that takes a value of "one" if firm i in period t anticipates growth in the upcoming year and "zero" otherwise. The coefficient β is crucial, as it measures the differential impact of Brexit shocks on UK firm expectations.

As shown in all columns of Table 8, the average marginal effect of the sample at the median indicates that a 100% increase in the distance to the border (i.e., moving to the Irish border) is estimated to reduce the probability that exposed firms maintain their optimistic outlook on future growth by up to 3.8% relative to non-exposed firms in response to Brexit. We build upon and add to the existing literature by reflecting on this generally negative outlook and the economic benefits promised by the Vote Leave campaign (Hassan et al., 2024). Our findings demonstrate the tangible impacts, showing that UK firms will likely become more pessimistic about growth when Brexit takes effect. Our study also extends Bloom et al. (2019) by suggesting that firms that perceive Brexit as a source of uncertainty in 2016 would lower their expectations after activating the referendum. Turning to the non-interaction term on the effects of Brexit_t on $\text{Expected Growth}_{i,t}$, we find that despite statistical distinction between exposed and non-exposed firms (i.e., the interaction terms in Table 8), the effects of Brexit on expectation on *aggregate* are mixed, as evidenced

Table 8
Mechanism test - $Brexit_t$ and $Expected\ Growth_{i,t}$.

	Dependent variable: Expected growth$_{i,t}$			
	Full sample		No switching	
	(1)	(2)	(3)	(4)
Brexit$_t$ × Distance$_i$	−0.020**	−0.034***	−0.024**	−0.038***
	(0.010)	(0.011)	(0.010)	(0.011)
Distance$_i$	0.053***	0.057***	0.054***	0.058***
	(0.007)	(0.008)	(0.007)	(0.008)
Brexit$_t$	0.192	0.380***	0.238*	0.423***
	(0.125)	(0.137)	(0.127)	(0.139)
Control variables	No	Yes	No	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Pseudo R-squared	0.036	0.046	0.036	0.046
Observations	49,741	38,028	47,826	36,464

Notes: This table displays our mechanism based on firms expectations by using the Probit estimations. The number presented as the marginal effects at the median for the dependent variable ($Expected\ Growth_{i,t}$) ("one" - firms with a more optimistic outlook on their future growth and "zero" otherwise). $Brexit_t$ is a dummy indicator ("one" - post-2020; "zero" - otherwise) while $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(2) include analyses using full sample, while Columns (3)–(4) analyze using only non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 9
$Brexit_t$ and $Employees\ (Log)_{i,t}$ – $Expected\ \ Growth_{i,t}$ (An additional control variable).

	Dependent variable: $Employees\ (Log)_{i,t}$					
	Full sample			No Switching		
	(1)	(2)	(3)	(4)	(5)	(6)
Brexit$_t$ × Distance$_i$	−0.158***	−0.126***	−0.113***	−0.150***	−0.118***	−0.110***
	(0.047)	(0.045)	(0.040)	(0.048)	(0.046)	(0.040)
Distance$_i$	−0.030	0.052	−0.131***	−0.028	0.051	−0.132***
	(0.041)	(0.040)	(0.033)	(0.042)	(0.040)	(0.034)
Brexit$_t$	2.028***	1.651***	1.400***	1.933***	1.550***	1.369***
	(0.604)	(0.578)	(0.508)	(0.614)	(0.587)	(0.514)
Expected Growth$_{i,t}$	0.020	0.027	−0.052***	0.023	0.030	−0.048***
	(0.018)	(0.018)	(0.017)	(0.019)	(0.018)	(0.017)
Constant	2.350***	0.777	1.112***	2.327***	0.784	1.111**
	(0.531)	(0.511)	(0.426)	(0.538)	(0.519)	(0.432)
Baseline control variables	No	No	Yes	No	No	Yes
Industry fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	No	Yes	Yes	No	Yes	Yes
Adj R-squared	0.000	0.090	0.371	0.000	0.088	0.372
Observations	60,123	60,123	47,422	57,980	57,980	45,629

Notes: This table presents results for the effects of $Brexit_t$ on $Employees\ (Log)_{i,t}$ as outlined in the baseline specification model, but we add an additional control variable capturing firms' $Expected\ \ Growth_{i,t}$. $Expected\ \ Growth_{i,t}$ is a dummy indicator ("one" - firms anticipate moving from a lower to a higher growth category, reflecting a more optimistic view of their future growth; "zero" - otherwise) (from the UK Data Archive Data Dictionary, the data code item is EXPGROW). The $Brexit_t$ variable is a dummy indicator ("one" - post-2020; "zero" - otherwise), and $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(3) cover the full sample, while Columns (4)–(6) only include the non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * p < 0.10, ** p < 0.05, *** p < 0.01.

by the mixed statistical significance of the coefficients on Brexit$_t$ across columns (1)-(4) of Table 8. This result aligns with recent evidence in the literature that documents mixed effects of Brexit on *aggregate* expectation (Dhingra and Sampson, 2022).[23]

Given the results in Table 8 that firms exposed to Brexit have a more pessimistic view of the future, one natural question arises as to whether the impact of Brexit will persist even after controlling for the firms' future expectations. To answer this question, we reconsider our baseline regression described in Eq. (1) where the set of control $\mathbb{X}_{i,t}$ now includes the firm's growth expectation as a control variable. We present the results in Table 9. While not denying the importance of future growth expectations, we find that having exposure to Brexit causes firms to reduce their labor demand in a way that is beyond how they typically react following having formed a pessimistic expectation. More importantly, the continued significance of the interaction term in Table 9 underscores the robustness of our result. While expectation *on average* does not meaningfully alter the magnitude or significance in the benchmark regression, this channel is vital in their *differentiated* effects on exposed firms relative to non-exposed firms. Indeed,

[23] Specifically, Dhingra and Sampson (2022) document that many of the *anticipated* long-run effects of Brexit did not materialize until 2021.

these complementary effects not only align with the importance of the expectation channel that the literature (Bloom et al., 2019) has carefully documented but also expand on *how* such a channel is important. Specifically, our results in Table 9 show that the expectation channel does not necessarily manifest via the *aggregate* effects as documented by Dhingra and Sampson (2022), but rather via the heterogeneity in Brexit exposure that we have reported throughout the paper.

6.3. Trade exposure

Firms with different trade exposures may respond differently to Brexit in terms of employment. Indeed, SMEs significantly engage in international trade in the UK.[24] From 2015 to 2021, 80% of SMEs conducted business with foreign markets, including EU member states and other global economies.

To understand the role of trade in the context of Brexit, we define $Trade\ Exposure_i$ a dummy variable that indicates whether the firm trades (export or import) *ex-ante*.[25] In particular, we estimate the following specification:

$$\begin{aligned}\text{Employees (Log)}_{i,t} = \alpha &+ \beta_1(\text{Brexit}_t \times \text{Distance}_i \times \text{Trade Exposure}_i) \\ &+ \beta_2(\text{Distance}_i \times \text{Brexit}_t) + \beta_3(\text{Distance}_i \times \text{Trade Exposure}_i) \\ &+ \beta_4(\text{Brexit}_t \times \text{Trade Exposure}_i) + \gamma\text{Distance}_i + \delta\text{Brexit}_t \\ &+ \theta\text{Trade Exposure}_i + \zeta\mathbb{X}_{i,t} + \lambda_k + \varphi_t + \epsilon_{i,t}, \end{aligned} \tag{8}$$

where we interact $Trade\ Exposure_i$ with the distance measure used to proxy for Brexit exposure and the Brexit dummy (i.e., triple-difference-in-differences) to disentangle the direct and indirect effects of Brexit via the trade exposure channel. We consider two measures of $Trade\ Exposure_i$: whether the firm trades with the world (including the EU) and trades exclusively with the EU. The results are reported in Table 10, in which columns (1) and (2) use the former trade exposure measure and columns (3) and (4) use the latter measure.

Our results in Table 10 are consistent with our main results in the paper that firms with high exposure to Brexit significantly reduce their labor force relative to firms with low exposure to Brexit after Brexit is in effect. This result is evidenced by the significant and negative coefficient on $Distance_i \times Brexit_t$. It is also reassuring that such a result is consistent across the two trade exposure measures and with all controls present.

More importantly, having trade exposure prior to the implementation of Brexit alleviated the negative effects of Brexit. In particular, the triple-difference-in-difference coefficient (i.e., $Brexit_t \times Distance_i \times Trade\ Exposure_i$) is significant and *positive* across all specifications considered. Among firms with high exposure to Brexit, those with ex-ante trade exposure generally increase their labor demand *relative* to firms without ex-ante exposure. We note that this result *does not* necessarily imply that trade exposure leads to an increase in the number of employees (as evidenced by the sum of $\beta_1 + \beta_3 + \beta_4$ being significant and negative). In other words, even though exposed firms generally cut their labor demand, exposed firms that traded *ex-ante* cut their labor demand less than similarly exposed firms that did not. While the total effects (i.e., *direct* and *indirect*) of Brexit on firms are negative, the *direct* effects are positive, and the *indirect* effects are strongly negative. This contrasting pattern shows that firms without trade exposure bear the largest costs (as evidenced by the statistically and economically significant decrease in their number of employees).

In terms of the magnitude of the triple-difference-in-difference coefficients in Table 10, we find that having *ex-ante* trade exposure (with any countries) can help *exposed* firms increase their number of employees by around 17.4% relative to *similarly exposed* firms without such ex-ante exposure. In other words, even though being exposed to Brexit can negatively impact firms' labor demand (i.e., a decrease of around 14.8% relative to non-exposed firms), having trade exposure can help alleviate such detrimental effects by roughly 17.4%. That is to say that, to a first-order approximation, having *ex-ante* trade exposure (with any countries) can significantly offset (i.e., 17.4%) the overall negative effects of Brexit exposure (i.e., −14.8%). Turning to columns (3) and (4), we find that having *ex-ante* trade exposure (with the EU) can help *exposed* firms increase their number of employees by around 19.5% relative to *similarly exposed* firms without such ex-ante exposure. This result implies that having *ex-ante* trade exposure (with the EU) can also offset a significant portion of the total negative effects of Brexit exposure (i.e., −14.9%). These results presented in Table 10 highlight the importance of ex-ante trade exposure, particularly with the EU.

All in all, our results with trade exposure (either as a control, in the online Appendix, or an interaction term) show that firms with high exposure to Brexit significantly reduce their labor demand, and they do so in a way that is consistent with our benchmark results in the main text. We find exposed firms with ex-ante trade exposure to expand their labor force relative to firms without such exposure. Consistently, we also find that firms without ex-ante trade exposure exhibit consistent results with our baseline estimations, as shown in the online Appendix. Regardless of their ex-ante trade exposure statuses, all firms exposed to Brexit in effect significantly cut their labor force following the implementation of Brexit in 2020.

[24] The number of SMEs engaged in trade activities and the percentage of total firms, based on UK trade in goods statistics by business characteristics. Source: https://www.ons.gov.uk. Last access: November 25, 2024.

[25] We also obtain estimates for our interaction term ($Brexit_t \times Distance_i$) when controlling for exposure measures that are qualitatively consistent with those obtained in Table 1. The details are presented in the online Appendix.

Table 10
$Brexit_t$ and *Employees (Log)*$_{i,t}$ – Triple Interaction: *Trade Exposure*$_{i,t}$, $Distance_i$ and $Brexit_t$.

	Dependent variable: *Employees (Log)*$_{i,t}$			
	Full sample	No switching	Full sample	No switching
	(1)	(2)	(3)	(4)
Trade Exposure$_{i,t}$ × Distance$_i$ × Brexit$_t$	0.174**	0.174**		
	(0.085)	(0.087)		
Trade Exposure$_{i,t}$ × Distance$_i$	−0.212***	−0.213***		
	(0.064)	(0.065)		
Trade Exposure$_{i,t}$ × Brexit$_t$	−2.378**	−2.385**		
	(1.093)	(1.111)		
EU Trade Exposure$_{i,t}$ × Distance$_i$ × Brexit$_t$			0.195**	0.203**
			(0.091)	(0.092)
EU Trade Exposure$_{i,t}$ × Distance$_i$			−0.217***	−0.223***
			(0.069)	(0.070)
EU Trade Exposure$_{i,t}$ × Brexit$_t$			−2.622**	−2.723**
			(1.161)	(1.182)
Brexit$_t$ × Distance$_i$	−0.148***	−0.147***	−0.149***	−0.148***
	(0.045)	(0.046)	(0.043)	(0.044)
Distance$_i$	−0.089**	−0.088**	−0.094***	−0.093***
	(0.036)	(0.036)	(0.034)	(0.035)
Brexit$_t$	1.875***	1.862***	1.806***	1.804***
	(0.577)	(0.584)	(0.557)	(0.563)
Trade Exposure$_{i,t}$	3.061***	3.073***		
	(0.827)	(0.841)		
EU Trade Exposure$_{i,t}$			3.172***	3.243***
			(0.888)	(0.905)
Constant	0.550	0.523	0.681	0.649
	(0.462)	(0.469)	(0.442)	(0.449)
Baseline control variables	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adj R-squared	0.379	0.379	0.379	0.379
Observations	50,163	48,288	50,163	48,288

Notes: This table presents results for the effects of $Brexit_t$ on *Employees (Log)*$_{i,t}$ as outlined in the baseline specification model, but we add two additional variables capturing trade exposure. The *Trade Exposure*$_{i,t}$ variable is a dummy indicator ("one" - if firms have imported or exported in the past 12 months; "zero" - otherwise). The *EU Trade Exposure*$_{i,t}$ variable is a dummy indicator ("one" - if firms have imported or exported with the EU countries in the past 12 months; "zero" - otherwise). From the UK Data Archive Data Dictionary, the data code items for *Trade Exposure*$_{i,t}$ and *EU Trade Exposure*$_{i,t}$ include C2GA ("In the past 12 months, have you directly imported goods or services from the European Union?"), C2CA ("In the past 12 months have you exported to the European Union?"), C2GB ("In the past 12 months, have you directly imported goods or services from non-European countries?"), C1 ("Whether export services"), and C2 ("Whether export goods"). The $Brexit_t$ variable is a dummy indicator ("one" - post-2020; "zero" - otherwise), and $Distance_i$ measures the firm's proximity to the Irish border. Columns (1)–(3) cover the full sample, while Columns (2)–(4) only include the non-switching firms. Standard errors are clustered at the firm level and presented in parentheses. Significance levels are indicated by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7. Conclusion

Drawing on the longitudinal survey of SMEs in the UK, our findings show that Brexit significantly affects labor demand. Our empirical strategy leverages the distance to the Irish border as a plausibly exogenous proxy for firms' exposure to Brexit implementation in 2020, thereby isolating the confounding effects arising from anticipation of such a policy since the referendum in 2016. Using the variation in firms' exposure to Brexit, we find that Brexit in effect in 2020 causes exposed firms to cut their workforce by up to 15.7% on average relative to non-exposed firms. In addition, exposed firms also experience the expectation of low growth and are more likely to increase R&D spending relative to non-exposed firms in response to Brexit.

Furthermore, our findings demonstrate that ex-ante trade exposure significantly ameliorated the negative consequences of Brexit. While exposed firms universally experienced labor demand reductions, those with pre-existing trade relationships exhibited less pronounced job cuts than non-exposed firms. Although the aggregate impact of Brexit on firms was negative, the direct effects were positive, while the indirect effects were notably negative. This contrasting pattern underscores the disproportionate burden of firms without trade exposure, as evidenced by the statistically and economically significant decrease in their workforce size.

Our paper contributes to the literature on UK SMEs' responses to Brexit, particularly regarding regional economic and policy implications for immigration and innovation. We extend previous research on SME owners' perceptions of Brexit, market access, and R&D expenditure. Our findings highlight the complex and regionally varied impact of Brexit, emphasizing the different channels that can ameliorate the negative effects of such a policy implementation.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jinteco.2025.104129.

Data availability

Replication package for "The Real Effects of Brexit on Labor Demand: Evidence from Firm-level Data" (Original data) (Mendeley Data)

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