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Abstract

We assess how balance sheets propagated labour demand shocks during Covid-19 using novel matched data on firms and online job postings. Exploiting regional and firm-level variation in three pandemic policies in the UK, we find that financially healthy firms increased vacancies more in response to positive shocks. Less-leveraged firms and firms with higher credit scores increased postings more in response to the Eat Out to Help Out's local demand subsidies and after receiving a Bounce Back Loan Scheme loan, respectively. These findings complement the link between leverage and employment losses in response to negative shocks.

Key words: Covid-19, recession, vacancies, Indeed, job postings, job ads, heterogeneity, firm, firm-level, balance sheets, industry, big data, alternative data, labour market, natural language processing.

JEL classification: C5, D2, G3, H1, J2, J6.

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The economic shocks induced by the global spread of COVID-19, and the lockdown policies implemented in response to it, affected firms in a highly asymmetric fashion. Some businesses, such as those in hospitality, were forced to close entirely, while others faced reduced demand due to the restrictions imposed on consumers' mobility. In response to these shocks, governments and central banks around the world introduced a plethora of policy interventions that targeted firms at risk of bankruptcy, from large-scale lending programmes to subsidies and tax credits. Together with worker-oriented policies such as furlough schemes and extended unemployment benefits, these interventions often aimed, directly or indirectly, to support employment during the recession.

This paper addresses the question of how the shocks and policies induced by the COVID-19 crisis in the United Kingdom propagated to labour demand via firms' balance sheets. We use novel, large-scale, naturally occurring[2] data on firm-level job vacancies to uncover job posting behaviour during the COVID-19 recession. We obtain this data by matching the universe of daily job vacancies from the online job aggregator Indeed, the largest online job posting board in the UK, to firm-level balance sheets from Bureau van Dijk's FAME database, which covers the universe of registered firms in the UK. This allows us to study, at high frequency, how firm labour demand was affected by a host of economic and policy interventions, and what role firm balance sheets played in the propagation of those shocks. Our study complements existing analysis too; while industry and worker outcomes due to COVID-19 have been extensively studied, there has been less discussion of the impact on firms' job vacancy posting decisions and how these depended on firms' balance-sheet positions, or of how policy interventions have attenuated or exacerbated the propagation of pandemic-related shocks to labour demand through these decisions. Such insights are relevant to future policy interventions.

As well as providing unconditional estimates of how vacancy posting varies by firm characteristic, we exploit the natural variation in three different UK policy interventions via difference-in-differences models: the tiered lockdown restrictions introduced in a staggered manner across the regions of the UK between September and November 2020; the Eat Out to Help Out (EOHO) scheme, which provided direct subsidies worth £850 million to the hospitality sector in August 2020; and, finally, the government-backed loans provided by the Bounce Back Loan Scheme (BBLS), which serviced nearly 1.5 million small and medium-sized enterprises.

We find that the initial pandemic shock and country-wide lockdown in the UK caused an unconditional decline in vacancy stocks of up to 30% compared to their average levels in the year preceding the pandemic, with large heterogeneity across firms. The effect was more pronounced for large, cash-strapped firms with high leverage and lower credit ratings. Using our difference-in-differences identification strategy and various robust estimators, we are also able to plausibly diagnose the effects of several policies that affected firms during the crisis. We find that the second wave of tiered lockdown measures in the UK led to a 7–8% drop in

[2] I.e. data that is generated in the course of economic activity rather than from surveys or administrative sources.

vacancy stocks. This effect was similar across firms regardless of leverage, cash holdings, or size—possibly because policies introduced early in the pandemic that aimed to support labour market demand were effective in reducing the asymmetry of the second lockdown shock. With regards to those policies, we find that the UK government's Eat Out to Help Out scheme (EOHO), which incentivised dining at restaurants, increased vacancy stocks by 3–5% across all sectors, not just hospitality—given the total vacancy stock of 500,000 at the time, this implies the creation of some 15,000 to 25,000 extra job vacancies relative to the counterfactual.[3] The effects of EOHO were also heterogeneous, boosting vacancy posting *less* at more leveraged firms. Notably, this finding complements the well-documented result in the corporate finance literature that more leveraged firms experience larger employment losses in response to *negative* economic shocks (Giroud and Mueller, 2017). Finally, firms with an above-median credit score (lower estimated risk of insolvency) who received a loan under the BBLS increased their vacancies by 0.5–0.8% in the three weeks after, whereas those firms with below-median credit scores that received loans did not.

We build on several strands of the literature. We add to earlier research on the labour market effects of the COVID-19 pandemic (Coibion et al., 2020; Bartik et al., 2020; Montenovo et al., 2022; Cajner et al., 2020; Papanikolaou and Schmidt, 2022; Brinca et al., 2021), particularly in the UK (Adams-Prassl et al., 2022; Blundell and Machin, 2020; Crossley et al., 2021; Hupkau and Petrongolo, 2020-09). Several papers also analyse online job postings during the COVID-19 pandemic. While most of these, like us, study labour demand (Forsythe et al., 2020; Dias et al., 2020; Arthur, 2021; Campello et al., 2023), some study labour supply using job seeker data (Marinescu et al., 2020; Hensvik et al., 2021). Other papers study the impact of policy interventions in the pandemic labour market. Using high-frequency administrative employment data, Autor et al. (2022) and Granja et al. (2022) investigate the US equivalent of the BBLS, the Paycheck Protection Program (PPP). The former of these, which focuses on firms with more than 500 employees, finds strong employment effects; the latter, which considers nearly the entire universe of PPP firms, finds only modest effects.[4] Other studies look at the labour market effects of lockdowns (Bradley et al., 2021; Baek et al., 2021; Palomino et al., 2020; Walker and Hurley, 2021) and unemployment benefits (Gregory et al., 2020; Marinescu et al., 2020) across the globe.

We make four key contributions to the existing economic literature.

First, we construct a unique dataset comprising small and large private and public firms that brings together firm-level balance sheets (from Bureau van Dijk) with high-frequency job vacancy data (from Indeed) and firm-level and regional government data on policy interventions. Combined, these allow us to examine how the financial conditions experienced by firms affected

[3]500,000 total vacancies was the average between June and August 2020 (Evans, 2021).

[4]Additionally, Granja et al. (2022) suggest that firms mostly used the PPP loans to make fixed payments, which resonates with our finding that only firms with healthier pre-pandemic balance sheets increased vacancies in response to the BBLS.

their job vacancy posting decisions. Therefore we also add to a growing number of papers that combine high-frequency, granular data from private companies and public institutions.[5]

Second, we present evidence on the role of firm balance sheets in propagating COVID-19 shocks to labour demand. The closest paper to ours is Campello et al. (2023), which also investigates the relationship between firms' vacancy posting and financial constraints but for publicly listed US firms only, which comprise 1% of all firms and one-third of all employment. By contrast, our dataset covers both private and publicly listed firms.

Third, we make a key contribution to the economic literature on the link between firm balance sheets and firm-level employment and vacancies with our finding that the effects of the BBLS and EOHO schemes boosted vacancy posting *less* at firms with more financial constraints. Interest in the interplay between firm balance sheets and labour decisions goes back to at least Sharpe (1994), who found that firms with more leverage had more pro-cyclical employment. Our finding is consistent with and complementary to several other results examining this link. As important examples: Giroud and Mueller (2017) find that, during the Global Financial Crisis, more highly leveraged firms experienced larger employment losses in response to local consumer demand shocks; Chodorow-Reich (2014) show that firms with pre-crisis banking relationships with less healthy lenders reduced employment by more; Duygan-Bump et al. (2015); Benmelech et al. (2019); Campello et al. (2010); Michaels et al. (2019) all document the role credit constraints played in labour market losses during the Global Financial Crisis. Recent papers have shown that credit constraints can even affect firms' labour market decisions in normal times (Aristizábal-Ramırez and Posso, 2021; Benmelech et al., 2021). Falato and Liang (2016) use a regression discontinuity design to show that when creditors gain rights to accelerate, restructure, or terminate a loan, there are substantial employment cuts at the debtor firms. Our results complement this literature: looking at the positive, as opposed to negative, demand and supply shocks induced by the BBLS and EOHO schemes, we find that more financially healthy and less leveraged firms, respectively, increased hiring more in response to these shocks than their counterparts. This suggests that credit constraints can not only lead firms to cut jobs in the face of a negative shock, but also to forgo job posting in the face of a positive shock. Furthermore, the fact that we find no evidence of firm heterogeneity in response to the second wave of lockdowns in the UK, when both a furlough scheme and emergency loan scheme were readily available to firms, provides indirect evidence that policy interventions can effectively mitigate the link between credit constraints and employment.

Finally, our paper also adds to the literature using data from online job boards to study labour market outcomes (Turrell et al., 2022; Deming and Kahn, 2018; Marinescu, 2017; Hershbein and Kahn, 2018) by matching firm-level job postings data to firm balance sheet and firm-level government data using methods from natural language processing.

The rest of this paper proceeds as follows. In Section 1, we describe our datasets, our approach to matching them, and discuss the context of the UK labour market under COVID-19

[5]See Chetty et al. (2020, p.5) for an overview.

and related policy interventions. In Section 2, we lay out our empirical strategy. In Section 3 we present our results before we conclude in Section 4.

1 Data and Context

1.1 Online Vacancies: Indeed

The online vacancy data we use is provided by Indeed, a worldwide employment website for job listings (Indeed, 2021). We observe several million unique vacancies for jobs in the UK between 2018 and 2021.[6] These are compiled by Indeed from a stable underlying panel of employer and recruiter sources from across the web, including its own job posting board.[7] We limit our attention to the jobs posted on Indeed's UK website.

Each field given for a job posting includes information on the name of the company that posted the vacancy and the region, county, and city where the job will be based.[8] It also contains the date the vacancy was first and last visible on Indeed. From this, we back out daily vacancy stocks (the number of vacancies online on a given day) at the firm-region level. We focus on vacancy stocks throughout the paper, as they are the most direct measure of live labour market demand, capturing both vacancy creation as well as destruction. One potential issue with using online vacancy stocks as a measure of vacancies is that the date a vacancy is taken offline may not accurately reflect the date the position was filled. To allay this concern, we provide results that use new vacancy postings ("flows") as an alternative measure of labour demand in the Online Appendix; these deliver similar estimates for all coefficients, and so it does not appear that there is a substantial bias related to lags in the deactivation of online vacancies.[9] Moreover, since our analyses all involve comparisons to some baseline period, any bias due to a lag in vacancy deactivation may be normalized if this lag stays constant during the sample period. Indeed, Figure 1 suggests the time vacancies are kept online remained stable throughout 2019–2020 at around 30–35 days on average, barring seasonal variation around the holidays.[10]

Since the dataset only covers online job postings, it might not be representative of the wider labour market. We refer to Turrell et al. (2022) for a more in-depth discussion of the potential

[6]We omit precise vacancy counts for confidentiality reasons.

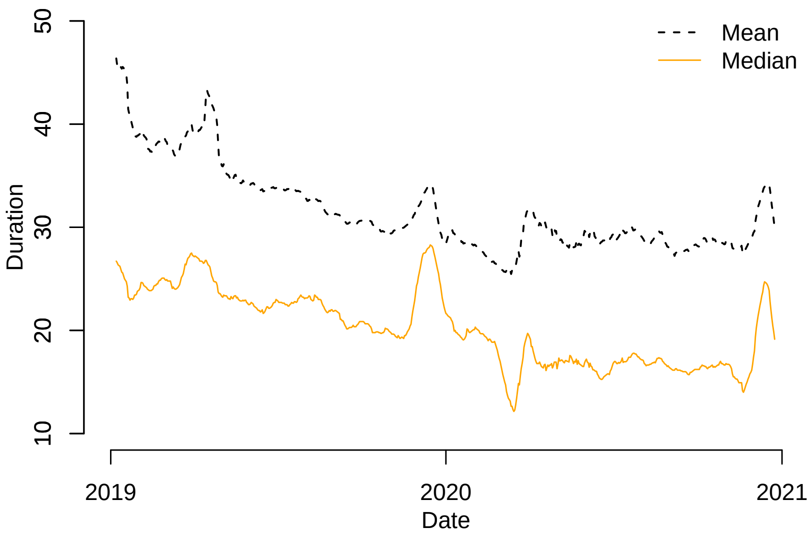
[7]Indeed data has previously been used in Mamertino and Sinclair (2019) to study cross-border job search, by Adrjan and Lydon (2019) to examine labour market tightness through the lens of users' engagement with particular job adverts, by Adrjan and Lydon (2021) to investigate the balance of labour supply and demand as COVID restrictions were eased, and by Bahaj et al. (2022) to study business creation during COVID-19.

[8]The city and county fields are incomplete and do not follow standard classifications but we are able to match them to NUTS-2 classifications for those that do not have missing values on both fields.

[9]The overall effect on flows is lower, which is expected since the change in flows only reflects decreases in *new* vacancy postings and does not capture removals of existing vacancies.

[10]In addition, note that vacancies typically do not remain online indefinitely. Sponsored vacancies incur a cost per day online and/or click received. Free vacancies are typically automatically deactivated after a fixed period, unless the posting firm renews them. Finally, the UK has a law that forbids the posting of "fake" job adverts, though we do not have data on enforcement.

Figure 1: Vacancy Duration Over Time



Note: Black line shows the 14-day moving average duration (total time online) of vacancies over time, while orange line shows the 14-day moving median.

biases of online vacancy data and present evidence for the representativeness of our matched sample below.

1.2 Firm-Level Balance Sheets: FAME

1.2.1 FAME

Information on firm balance sheets is obtained from FAME, a company database provided by Bureau Van Dijk (2021). It contains yearly balance sheet information for all companies registered in the UK and is derived from their filings with Companies House, the UK's company registrar. Since any limited company, both public and private, is required to file with Companies House, we observe a large number of small and medium-sized companies. Filing requirements differ by company size, such that small firms[11] are not required to file profit and loss accounts. To avoid any bias introduced by these differing requirements, we focus on balance-sheet items. In practice, this essentially means that we use assets as a measure of firm size rather than turnover or number of employees.[12]

Although we do not observe sole traders, they account for only a very small share of employment; as an indication of this, 70% have annual costs of less than £10,000 (Cribb et al., 2019). We restrict the sample of firms to those active in the UK at any point between March and December 2020, and only retain balance sheet items for 2019 to capture *a priori* firm

[11]Where "small" means that firms satisfy 2 out of 3 of: an annual turnover less than £6.5 million, a balance sheet less than £3.26 million, or an average number of employees 50 or less.

[12]See Figure A-6 for comparisons of our sample to the ONS vacancy survey by firm size.

heterogeneity—that is, before the COVID-19 crisis. This leaves us with around 5 million unique firms.

1.2.2 Matching FAME to Indeed

Figure 2: Vacancy shares by industry, Indeed vs. ONS

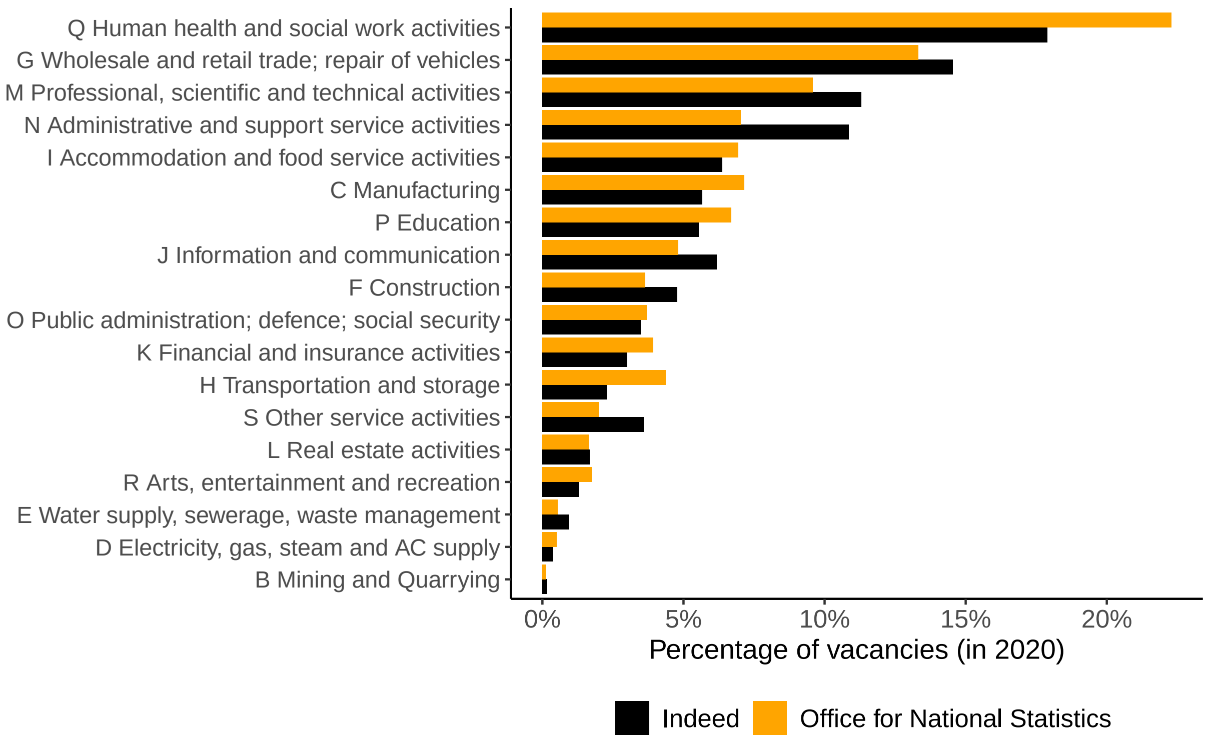
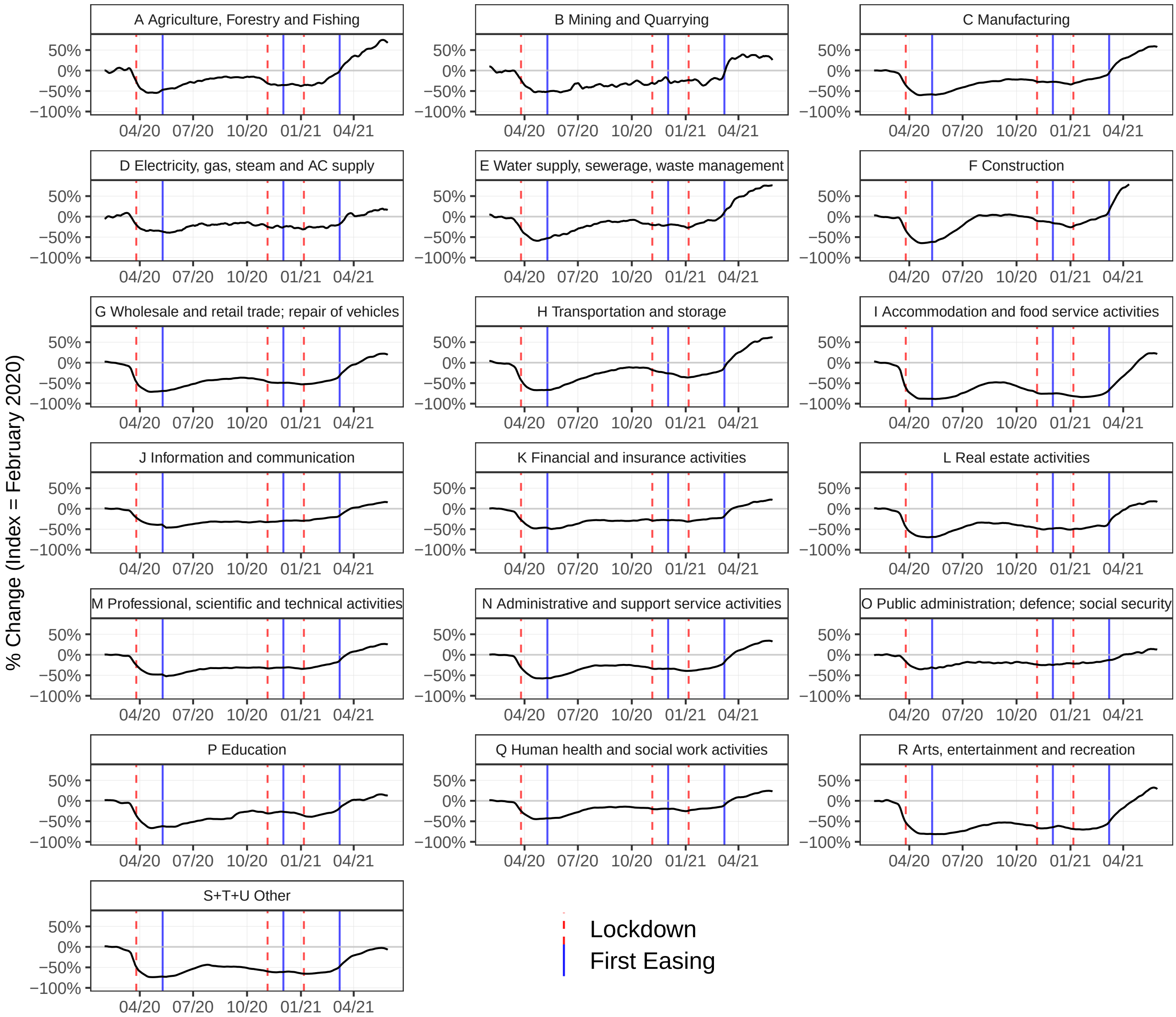


Figure plots the average share of monthly sector-specific vacancy stocks in total vacancy stocks for 2020, comparing vacancy stocks from Indeed to those from the ONS Vacancy Survey.

To match the Indeed vacancies to FAME, we use a natural language processing method to calculate the similarity between the official firm names in FAME and the (inaccurately) recorded firm names in the Indeed data. For each firm name present in Indeed, we calculate its cosine similarity to every FAME firm name and retain only the match with the highest similarity score. We then filter these matches so as to achieve an accuracy of 90% based on a manual assessment of a random sub-sample of 1,000 matches. Following this process, we match 75.5% of the vacancies and 73.1% of the Indeed firm names names with the FAME data in the main period of analysis between March and November 2020. We discuss our matching approach and its potential biases in detail in Appendix A1.

A comparison to industry-level vacancy stocks as reported by the Office for National Statistics' Vacancy Survey (Machin, 2003) is provided in Figure 2. For 2020, it shows that the industry shares of our matched vacancy data are close to those found in the ONS' representative data. Figure A-6 in Appendix A2 reports the same comparison by firm size. The shares of groups of firm sizes in our matched data closely track those in the ONS Vacancy survey, confirming that the Indeed data represents both small and large firms.

Figure 3: Vacancy Stocks by Industry (Index = February 2020, Not Seasonally Adjusted)



Note: Figure plots 7-day moving averages of vacancy stocks (number of vacancies online on Indeed on a given day) from February 1, 2020 to May 29, 2021 for all UK SIC (2007) industry sections. Red vertical lines indicate dates of national lockdowns, while green vertical lines indicate dates of first easings of lockdowns. All vacancy stocks are expressed relative to the average in Feb 2020.

To get a first look at the matched vacancy sample, we plot the evolution of vacancy stocks by industry in Figure 3. The red and green vertical lines indicate the dates of England's lockdowns and their respective easings.[13] The plots depict a broad-based decline in vacancy stocks of around 50% in March 2020, which *precedes* the first national lockdown. Subsequently, there is a common recovery that roughly coincides with the first easing, a renewed but less stark decline with the imposition of new lockdowns that lasts throughout the winter, and a

[13]Scottish, Welsh, and Northern Irish lockdowns generally coincided with these dates.

marked recovery after the easing of the third lockdown, which sees vacancy stocks recover above their levels in February 2020 by April 2021. Note the relatively large falls in those industries that were effectively closed by the first lockdown; accommodation and food services; and arts, entertainment and recreation.

1.3 Government Interventions

1.3.1 Tier Restrictions

On October 14, 2020, the British tier restrictions scheme was introduced; we use the timing of the introduction of these as one of our policy interventions. The tiers imposed heterogeneous regional restrictions on movement and commerce across England and were adapted in broadly similar form by the other UK countries. Such restrictions had been imposed in a targeted fashion since July 2020 to combat the second wave of COVID-19. On November 5, 2020, the system was revoked in England and replaced by a country-wide lockdown (coded as Tier 4), which lasted until December 2, 2020.[14]

Table 1: Overview of Tier Restrictions (Source: BBC (2020))

Medium alert (Tier 1)	- Follow the "rule of six"[a] if meeting indoors or outdoors - Pubs and restaurants to shut at 10pm
High alert (Tier 2)	- No household mixing indoors - "Rule of six" will apply outdoors - Pubs and restaurants to shut at 10pm
Very high alert (Tier 3)	- No household mixing indoors or outdoors in hospitality venues or private gardens - "Rule of six" applies in outdoor public spaces like parks - Pubs and restaurants not serving meals will be closed - Guidance against travelling in and out of the area
Lockdown (Tier 4)	- Stay-at-home order with exceptions for essential work and education - Individuals can only meet one person from another household, in a public place - Non-essential retail and other venues ordered to close - International travel and non-work overnight stays away from home banned, guidance against travelling in and out of the area

[a]This rule specified that barring a limited set of exceptions, any gatherings of more than 6 people were against the law.

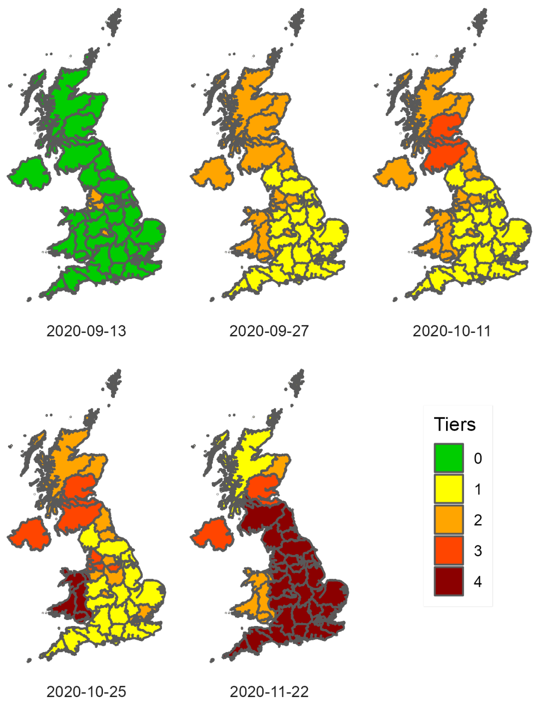
To estimate the effect of the tier restrictions on labour supply across the UK, we mapped the devolved administrations' measures to the English tier equivalents, thus coding the UK-wide

[14]Scotland implemented a similar system on November 2. Northern Ireland introduced a tier system on September 22 and a "circuit-breaker" lockdown on October 16. Wales did the same on September 14 and October 23, respectively.

local restriction levels for each week from September 20, 2020 to November 22, 2020.[15] The tier restrictions were implemented at the Local Authority level, but we only observe vacancies at the less granular NUTS-2 level.[16] Due to clustered regional spread of the virus as well as coordination by regional authorities, nearly all Local Authorities in a given NUTS-2 region had the same level of tier restrictions in place in any given week in our sample. Thus, we code the tier restrictions at the NUTS-2 level as the modal level of tier restrictions weighted by gross value added of the corresponding Local Authorities (rounded to the nearest integer). Figure A-7 shows how often the rounded average tier restriction at the NUTS2 level agrees with the tier level of the Local Authority level. The histogram of the 462 NUTS2-week pairs shows that there is perfect agreement between the aggregated and the lower-level tier restrictions in nearly all cases.

A useful summary of the key differences between the English tiers can be found in Table 1. A full-scale lockdown corresponds to Tier 4 with a stay-at-home order and closure of all non-essential business.[17] Maps of the week-by-week evolution of tier restrictions in NUTS-2 codes are shown in Figure 4.

Figure 4: Tier Restrictions Evolution by NUTS2, Sep-Nov 2020



Note: Maps show the fortnightly evolution of the tier or tier-equivalent restrictions in force in the various NUTS-2 regions of the United Kingdom, for the period from September 20 to November 22, 2020. Tier levels are: 0 – no restrictions; 1 – medium alert level; 2 – high alert level; 3 – very high alert level; 4 – full lockdown. For more detail on the tiers, see Table 1.

[15]We thank Zaar Khan for helping us gain access to this data.

[16]Due to Britain's exit from the European Union, the Office for National Statistics replaced the existing NUTS geographical classification with a UK-only system called International Territorial Levels, or ITLs, in 2021. The first release of ITLs is a direct replication of NUTS codes; we use NUTS throughout.

[17]For an exhaustive list of the restrictions imposed by each tier, see `https://bit.ly/3xg0tCF`.

1.3.2 Eat Out to Help Out

To alleviate the negative economic effects of the COVID-19 pandemic and the lockdowns imposed to combat it, the UK's Treasury introduced a programme that aimed to support recovery and job creation in the hospitality sector, the "Eat Out to Help Out" (EOHO) scheme. The scheme was announced in the context of the UK government's "Plan for Jobs", which was unveiled on July 8, 2020. Under the EOHO scheme, hospitality venues could offer their customers a 50% discount on food and non-alcoholic drinks eaten in from Monday to Wednesday each week between Monday, August 3, and Monday, August 31, 2020, and claim back the discounted amount from Her Majesty's Revenue and Customs (HMRC), the UK's tax authority, with a cap of £10 discount per person per visit. In total, more than 106 million meals were claimed, with an average discount of £5.74, for a total subsidy of £849 million (Fetzer, 2021, pp. 2,5).

We leverage granular regional data on the use of EOHO to estimate its *spill-over effects* on local labour demand as measured by the vacancies posted by local businesses. The data has previously been used in Fetzer (2021) to estimate the effect of the same scheme on COVID-19 infections. Data on the scheme were retrieved from the HMRC's GitHub repository (HMRC, 2020), which saw near-daily updates of a list of participating restaurants by postcode during the month of August. These data were then mapped to the ~7,000 Middle Super Output Layer Areas (MSOA) in England, a detailed geographic hierarchy for England and Wales. The data provide the *number of participating restaurants in an MSOA*, a cross-sectional measure of exposure to the EOHO scheme. An alternative measure of EOHO exposure that is mapped to the MSOA level is the *total number of meals claimed* by participating restaurants at the Parliamentary Constituency level, as reported by HMRC (Customs, 2020). To control for characteristics related to the restaurant supply side, Fetzer additionally collected data on the number of students, the shares of rented and owned accommodation in an MSOA, an MSOA's COVID exposure in the spring of 2020, and commuter flows in and out of the MSOA. For more details on the data, we refer to Fetzer (2021, §1).

In order to estimate the impact of the EOHO-induced local demand shocks on vacancy posting, we use the same baseline measures of EOHO exposure and covariates as Fetzer and additionally include covariates capturing the balance sheet conditions of the firms with Indeed vacancies in the sample period. To mimic the design in Fetzer (2021), we aggregate the firm-level vacancies from the Indeed data to the MSOA-week level. We do that by aggregating the number of vacancies per week for those firms with a single trading address in a given MSOA.[18] We also average the firm balance sheet characteristics at the MSOA level by weighting them by a firm's contribution to the total combined assets of the full subset of firms in a given MSOA, similar to the approach in Giroud and Mueller (2017).

[18] From the Indeed data, we can only map vacancies into the coarser NUTS-2 regions. For firms with only one trading address, however, we know their exact address.

1.3.3 Bounce Back Loan Scheme

The Bounce Back Loan Scheme (BBLS) was a government-backed loan scheme announced by the UK government on April 27, 2020, which guaranteed 100% government backing of loans provided by commercial lenders with no interest payments in the first 12 months and a fixed interest rate thereafter of 2.5%. We obtained information on the names of the firms that received a loan under the BBLS, as well as the date they first received it, from 14 UK banks out of a total of 24 accredited UK lenders.[19] These data have previously been used by Banks et al. (2021) to study what type of firms made use of the BBLS. The data indicate both when a firm's credit facility was first opened by the lender and when the firm actually drew on it. The sample covers the period from the inception of the scheme (May 4, 2020) to the end of 2020 and comprises 780,504 firms that received a BBLS loan.[20] For reference, official statistics from the UK's Treasury indicate that 1,260,940 BBLS facilities had been approved by September 20, 2020 (Treasury, 2021).[21]

We match these data to our Indeed-FAME dataset in order to study the effect of loan provision on labour demand. Matching these datasets is more straightforward than matching the Indeed data: 80% of the firm-level data provided by the banks contains the firm's unique registration number, which we can directly match to the corresponding registration numbers in FAME. For the remaining 20%, we employ the Levenshtein distance, which calculates the dissimilarity between two words as the minimal number of single-character edits needed to transform one word into the other (Levenshtein, 1966).[22] Since the firm names reported to the banks are close to the official firm names — as firms were legally required to provide their official name —, this matching algorithm suffices.[23] With this simple approach, we find that 42,772 firms in our Indeed-FAME sample received a loan under the BBLS. Around one thousand firms initially received a loan under the Coronavirus Business Interruption Loan Scheme, which preceded the BBLS, and were then transferred to the BBLS. We exclude such firms. Since our matched sample contains about 270,000 unique firms, and there are about 6 million firms active in the UK (Hutton and Ward, 2021), we have around 16% of firms receiving a loan under the BBLS in our sample, compared to around 21% of all firms. Thus, we capture a large share of firms in our matched data that actually received a loan under the BBLS.

Appendix Figure A-8 plots the progression of loan take-up in our matched sample, by depicting the share of firms that had taken out a loan on a given date out of all firms using the scheme. The graph makes it clear that the majority of the loan take-up (68%) occurred in the

[19] 23 lenders if Bank of Scotland and Lloyds are counted as one.

[20] We thank Will Banks for helping us access this data.

[21] The lower number in our data is due both to missing values in the data obtained from the lenders, as well as the absence of data from some lenders altogether.

[22] A small number of firm names were redacted for data confidentiality reasons; we are unable to match these.

[23] Note that it would not suffice for the Indeed data since it does not take into account the relative frequency of different strings of letters (e.g. 'and' is extremely frequent, 'qrs' not), and cannot handle word inversions (e.g. "[supermarket chain name] Lombard St" vs. "Lombard St [supermarket chain name]") or partial deletions (e.g. "[supermarket chain name] Lombard St" vs. "[supermarket chain name]").

first month of the scheme, May 2020. In our empirical strategy, we exploit this front-loading of loan applications, as it likely led to a pseudo-random disbursement of loans.

2 Empirical Strategy

To assess how the shocks induced by the COVID-19 recession propagated to firms' job creation through their balance sheets, we adopt a variety of empirical strategies, which we discuss in this section.

First, we paint an overall picture of the pandemic shock by looking at the time trend in online vacancy stocks after March 11, 2020, the day the WHO declared COVID-19 a pandemic, and interact this trend with a set of firm balance sheet variables to assess how the vacancy stocks of different firms fared during the initial stages of the pandemic. Since this exercise suffers from endogeneity, it does not uncover a causal relationship.

For this reason, we also make use of the three policy interventions detailed in Section 1.3 to identify the causal effects of shocks on vacancy posting through firms' balance sheets.

Thus, in the second part of our empirical strategy, we investigate how lockdowns affected firms' vacancy posting decisions. Our identification relies on exploiting natural variation in lockdown intensity across the UK induced by the rapid introduction of the "tier system", which heterogeneously subjected local areas to increasingly severe restrictions on movement and commerce.

In the third part of our empirical strategy, we consider how local demand shocks, induced by subsidies to the hospitality sector under the EOHO scheme, boosted local job creation through spill-over effects, and whether balance sheets played a role in firms' ability to take advantage of these effects.

Finally, we study how targeting firms' balance sheets by providing liquidity through government-backed loans under the Bounce Back Loan Scheme boosted job creation during the recession, and how previous balance sheet conditions affected this potential boost.

Throughout, our dependent variable is *log(1+vacancy stocks)*, and we interpret the effect sizes in percentage terms. Since there are a substantial number of data points with zero vacancy stocks, we also report the baseline results for the vacancy stocks in levels (winsorised at the 99^{th} percentile) and using an inverse hyperbolic transformation in the Online Appendix. The coefficients for the inverse hyperbolic transformation are close to those for the log transformation. Similar results are also found when the outcome is in levels and we scale the coefficients by the average vacancy stock in the sample.

Throughout, we consider the same set of firm balance sheet variables, which we normalize to have a standard deviation of 1: *total assets* (to proxy firm size), *leverage* (current liabilities to assets ratio), *cash to assets ratio*, and *credit score* (a proxy for solvency).[24] Different from the standard definition of leverage as current plus long-term liabilities to assets, we only include

[24]See Table A-2 for a detailed explanation of each variable.

current liabilities due to a high number of missing values in the long-term liabilities variable. Since we only study a sample period within 12 months from the end of the 2019 fiscal year, and long-term liabilities have maturity dates further than 12 months out, we believe this constitutes a reasonable proxy of firm leverage. We only consider firms with unconsolidated balance sheets to avoid double counting and issues with matching to the names of holding companies and headquarters.

All analyses are based on balanced panels of firm-region pairs, where we assign 0 vacancies when a firm did not have any postings on Indeed at a given time in a given region where it had at least one vacancy in the sample period. To avoid attenuation bias, we only include firm-region pairs that had at least one vacancy at some point during the sample period. In the case of the pandemic shock analysis, we relax this constraint to include only firm-region pairs with at least 3 vacancies in the year prior to the start of the pandemic.[25]

2.1 Pandemic Shock: Regression Analysis

The COVID crisis began to severely affect the UK in March 2020. The first COVID cases in the UK were confirmed on January 31, the first death due to the virus was confirmed in early March, and soon after that the first UK wave of the pandemic began. On March 11, 2020, the WHO declared the COVID-19 outbreak a global pandemic. The first national lockdown in the UK came into force on March 26. By early March, firms were anticipating the impending pandemic and were reducing their demand for labour. Figure 3 shows that this period marks the onset of a staggering drop in vacancy stocks that extended to mid-April 2020, and the recovery only cautiously began after the easing of the first national lockdown on May 10, 2020. When we scrutinise the data more closely, we find that the initial impulse for this drop occurred on March 11, the day of the WHO announcement. Hence, to get an initial coarse picture of the way in which the COVID pandemic affected firm-level vacancy posting, we estimate several regressions of vacancy stocks on a dummy that turns on after March 11, 2020, and label this the "pandemic shock".

Specifically, we estimate the following least squares regression,

$$\ln(1+v_{ijt}) = \alpha_{ij} + \gamma_{l(ij),m(t)} + \beta \cdot \text{WHO}_t \cdot X_{ijt} + \varepsilon_{ijt}, \tag{1}$$

where v_{ijt} is firm i's vacancy stock in NUTS2 region j (if it has any in region j during the sample period) in week-year t; α_{ij} denote firm-NUTS2 fixed effects; $\gamma_{l(ij),m(t)}$ denote flexible $l(i,j)$ by $m(t)$ fixed effects, where $l(i,j)$ can denote NUTS2 region or SIC industry, while $m(t)$ denotes either week-year or month-of-the-year; WHO_t is a dummy that is 0 before and 1 after

[25] While for the other designs, we can by construction not study the effects on firms that stopped posting vacancies due to the pandemic, we assessed the impact of selecting on post-pandemic vacancy posting by re-estimating the pandemic shock exercise on a sample that includes only firms-region pairs for which we observed at least one vacancy *after* March 14, 2020. As the magnitudes and directions of the results were very similar, it seems reasonable to assume this is the case for the other designs as well.

March 11, 2020, and X_{ijt} is a vector of firm-level variables that we expect to modify the effect of the pandemic shock on firms' vacancy stocks. Since these regressions only provide correlational evidence, we include an expanded set of firm-level controls to try and account for spurious correlations, including whether the firm is publicly listed and whether it belongs to a corporate group. We estimate these models using a sample that runs from March 1, 2019 to May 10, 2020, the date of the easing of the first national lockdown, to ensure the estimates capture only the effects of the initial pandemic shock to vacancy posting. The long pre-period means that the effects estimated are relative to the average in the 12 months preceding the pandemic.

2.2 Policy Interventions: Difference-in-Differences Analyses

We exploit the shocks induced by three policy interventions in response to the spread of COVID-19: the EOHO scheme, the BBLS, and tiered lockdown restrictions. Below, we discuss our empirical strategy for the three policy interventions in more detail. Common across the three designs is that we rely on difference-in-difference (DiD) designs, by way of two-way fixed effects (TWFE) least squares specifications as well as specifications of the doubly-robust DiD estimator developed by Callaway and Sant'Anna (2020, p.10). The reason for reporting two different estimators is that the canonical TWFE estimator has been shown to be biased in the presence of staggered treatment (Goodman-Bacon, 2021), while the doubly-robust estimator is not. Additionally, the doubly-robust estimator allows for the assumption of parallel counterfactual trends to be conditioned on covariates. One downside of the doubly-robust estimator is that it does not allow for continuous treatments, an issue which we discuss further below. For now, we note that it semi-parametrically estimates sets of group-time average treatment effects on the treated (ATT),

$$ATT_{dr}^{ny}(g,t;\delta) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_{g,t+\delta}(X)(1-D_{t+\delta})(1-G_g)}{1-p_{g,t+\delta}(X)}}{\mathbb{E}\left[\frac{p_{g,t+\delta}(X)(1-D_{t+\delta})(1-G_g)}{1-p_{g,t+\delta}(X)}\right]}\right)\left(Y_t - Y_{g-\delta-1} - m_{g,t,\delta}^{ny}(X)\right)\right],$$

where ny stands for "not yet treated". In the Tier System and EOHO designs, all the units in the sample eventually receive treatment so we naturally use the not-yet-treated units as controls. Not all firms, however, participated in the BBLs, so we restrict the control group to the not-yet-treated units, as we expect firms that have applied to and are about to receive emergency loans to have meaningfully different employment constraints than firms that did not. Further, g indicates the "treatment" cohort, that is, the group of units that become treated on the same date (in the case of staggered treatment, there are multiple such groups); t indicates the time relative to treatment (e.g. one day after treatment); δ indicates the number of periods before treatment that units are assumed to anticipate treatment – unless stated otherwise we assume no anticipation, i.e. $\delta = 0$; G is a dummy that is 1 if a unit is first treated in period g; $p_{g,t+\delta}$ is the probability (propensity score) of becoming treated at time g, conditional on being in treatment group g or on being in the not-yet-treated group by time $t+\delta$, and conditional on a vector of pre-treatment covariates X; $D_{t+\delta}$ is a dummy that is 1 if a unit is treated in period $t+\delta$ (note that treatment is only allowed to be discrete); Y_t is the outcome variable (log of vacancy stocks) in period t; and $m^{ny}_{g,t,\delta}(X) := \mathbb{E}\left[Y_t - Y_{g-\delta-1} \mid X, D_{t+\delta}=0, G_g=0\right]$ are population outcomes regressions for the not-yet-treated by time $t+\delta$ group.[26] Since the staggered DiD designs we estimate contain a large number of treatment cohorts, we summarize these group-time treatment effects by aggregating them along treatment group ("group" effects) and treatment time dimensions ("dynamic effects"). This means that we first calculate average effects by treatment cohort or by treatment time, and then take averages of those averages (see Callaway and Sant'Anna (2020, §4.3) for further reference). Effectively, this makes that average effects aggregated along treatment time ("dynamic" effects) put more weight on the treatment effects of cohorts treated earlier in time, since later-treated cohorts generally will not contribute to estimates of effects long after treatment due to the balanced panels (e.g., we may observe six days after treatment for the first-treated group, but not for the last-treated). The "group" effects, on the other hand, correspond to the interpretation of the DiD estimator for the canonical setup with only two groups and two periods (Callaway and Sant'Anna, 2020, p.18). We discuss the respective TWFE specifications and the precise implementations of the doubly-robust estimator in context below. Additionally, in subsection 2.2.4, we discuss our approach for estimating heterogeneous treatment effects. Finally, in subsection 2.2.5, we briefly discuss how to interpret our estimates obtained from specifications with continuous treatment.

2.2.1 Tier Restrictions

The rapid emergence and implementation of the local restrictions under the tier restrictions we discussed above induced natural variation in lockdown intensity across the UK, which we exploit to estimate the effect of lockdowns on labour demand by estimating the following TWFE DiD specification,

$$\ln(1+v_{ijt}) = \alpha_i + \gamma_{k(i),t} + \delta \cdot \text{Tier}_{jt} + \beta \cdot X_{ijt} + \varepsilon_{ijt} \tag{2}$$

with v_{ijt} being the number of active vacancies firm i had on Indeed in NUTS-2 region j in week t; α_i denotes firm-NUTS-2 fixed effects, $\gamma_{k(i),t}$ denote week fixed effects, which we allow to vary by a vacancy's region or industry (as a function $k(i)$ of firm i). The DiD estimator Tier_{jt} captures the tier the NUTS-2 region j is subject to, X_{it} is a vector of controls which we interact with week fixed effects in our robustness checks, and ε_{it} is the error term. We control for COVID-19 cases and deaths, as well as region-by-week and industry-by-week fixed effects.

We consider two different measures of treatment. First, a categorical measure, which codifies the tier in region j in week t, with the tiers going from 0 (no restrictions) to 4 (full lockdown).[27] Second, a treatment dummy which is one if region j is in Tier 2 or higher in week t. The motivation for discretising the treatment in this manner is that all English regions were moved

[26]Unit subscripts are omitted, which follows the notation in Callaway and Sant'Anna (2020).
[27]See Table 1 for a summary of the restrictions.

into Tier 1 by default on September 20, 2020, the start of our sample. Combined with the fact that meeting with individuals outside one's "support bubble" was only banned from Tier 2 and higher, this suggests that Tier 1 did not impose significant enough restrictions to be considered as a degree of "lockdown". For the doubly-robust estimates, we thus use the discretised Tier 2 dummy.

2.2.2 Eat Out to Help Out

To assess how local demand shocks affect firms' vacancy postings during a recession, we exploit the granular variation in the uptake of the EOHO scheme across England and implement several DiD designs.[28] Similar to Fetzer (2021), we estimate variations of the following TWFE specification,

$$\ln(1+v_{jt}) = \alpha_j + \gamma_{l(j),t} + \delta \cdot \text{Post}_t \cdot \text{EOHO}_j + \beta \cdot X_{jt} + \varepsilon_{j,t} \tag{3}$$

with v_{jt} being the total number of active Indeed vacancies in week t for all firms that have their only trading address in MSOA j; α_j are MSOA fixed effects; $\gamma_{l(j),t}$ are flexible week fixed effects; X_{jt} is a vector of time-varying controls at the MSOA level; Post_t a dummy that is 1 after week 32 (when the scheme was introduced); and EOHO_j a measure of MSOA-level exposure to the scheme. We estimate this specification on a balanced panel of firms running from week 24 (8 weeks before the programme) to week 36 (the conclusion of the programme). The results are robust to changing the length of the pre-period, as well as extending the sample past the end of the programme (see the Online Appendix).

The treatment measures we consider are the same as in Fetzer (2021): the average *number of restaurants* that were listed on the HMRC's EOHO GitHub page throughout August 2020, and the *number of meals claimed* in each MSOA, mapped from official Parliamentary Constituency level data by weighting by number of restaurants in each MSOA. Additionally, as a robustness check, we also employ a doubly-robust estimator using a simple discretized measure of treatment which is 1 after week 32 for any MSOA that had any local restaurants which participated in the scheme. Since this is not a staggered treatment design (the scheme opened for all regions on August 3), all aggregation schemes for the group-time ATTs are identical.

Since we focus on firms with one trading address, these models effectively estimate the effect of the EOHO scheme on vacancy postings by local firms. In Section 3, we discuss the degree to which our findings can be extrapolated to other firms. One motivation for only considering single-establishment firms is that Giroud and Mueller (2019) find that multi-establishment firms reallocate employment across their regions of operation in response to local demand shocks, suggesting that including multi-establishment firms could bias our estimates because they would fail to capture such reallocation effects.

To control for the regional spread of COVID-19, we follow Fetzer (2021) in considering a range of area-by-week fixed effects from the more coarse NUTS-2 region to the highly granular

[28] See Fetzer (2021, Fig.1) for the distribution of EOHO uptake at the MSOA level.

MSOA. Finally, in our robustness checks, we interact a vector of controls with the week fixed effects: following Fetzer (2021), we include the following controls for the restaurant supply side and regional exposure to the COVID crisis (*Population density, spring 2020 COVID-19 exposure, student exposure, tenure types*) and we additionally control for asset-weighted MSOA-level firm characteristics.[29]

2.2.3 Bounce Back Loan Scheme

Finally, we exploit the pseudo-random variation in loan disbursement induced by the overwhelming demand for loans in the first few weeks after the introduction of the BBLS. We estimate the effect of loan provision on firms' vacancy postings and inspect the way in which firm balance sheets mediated this effect. During the first weeks of May 2020, there were widespread reports in the media that UK banks were overwhelmed by the number of applications. The application web pages of several large banks reportedly experienced sporadic outages and banks reported that they had too few staff to process submissions. Further, there was initial confusion about which firms required additional credit checks. The website of the BBLS itself also experienced an outage at the launch of the scheme (Griffiths, 2020; Mustoe and Howard, 2020; Bounds, 2020). Based on these reports, we can expect the cohorts of firms that receive a credit line on any given day in the first weeks of May 2020 to be pseudo-randomly selected. The BBLS was targeted towards small and medium-sized firms, with a cap of £50,000 on the maximum loan amount. Furthermore, only those businesses that were not yet using any of the other government loan schemes were eligible. Hence, we should not expect there to be large differences in businesses' capacity to navigate the BBLS application process (which could have potentially have led to non-random assignment if large businesses were better equipped to make submissions).

We estimate the following TWFE least squares specification,

$$\ln(1+v_{ijt}) = \alpha_i + \gamma_{l(i),t} + \delta \cdot \text{Post}_{ijt} \cdot \text{Loan}\,/\,\text{Turnover}_i + \beta \cdot X_{ijt} + \varepsilon_{jt}, \tag{4}$$

with v_{ijt} being the number of vacancies firm i had active on Indeed in NUTS-2 region j on day t; Post_{ijt} is a dummy that is 1 for firm i on and after the day it first draws on its credit facility; $\text{Loan}\,/\,\text{Turnover}_i$ is the loan amount relative to annual turnover for firm i, where the measure of annual turnover is the one the firm reported to its bank in order to obtain the loan and hence does not suffer from the reporting bias present in the FAME turnover data (see above). The other terms are defined similarly to before, except that, in some regressions, we also allow the week fixed effects $\gamma_{l(i),t}$ to vary by the bank which lent to firm i. The reason we scale the loan amount by firms' turnover is that firms are only allowed to borrow up to 25% of their reported annual turnover with a maximum cap of £50,000, which constitutes a very different treatment for firms with an annual turnover above £200,000 than for firms below this threshold (receiving a loan that likely satisfies the firm's liquidity needs vs. one that only partially does

[29]See Table A-2 for a full description of the variables.

so). For similar reasons, we do not discretise the treatment, since we believe the amount of money received (the treatment dose) is crucial to the treatment in question (receiving a loan).

We estimate this regression on a sample spanning the days between April 4 and May 24, 2020 – from four weeks before the start of the BBLS until three weeks into the scheme – and thus only include firms that drew on their credit facility on one of the days in this period. The reason we exclude firms not treated in the sample period is because we expect the trend in vacancy stocks of firms that do not have an immediate need for a loan to be quite different from the same trend for firms that drew on the scheme in this period. Indeed, when we extend the post-treatment period out several months and estimate an event-study version of the specification above, we find that the pre-trends deviate significantly from zero, suggesting non-parallel trends between treatment and control group. We do not find such deviations for our baseline sample period (see Figure A-12), which we believe supports our choice to focus on the initial weeks of the program and on not-yet-treated firms.

2.2.4 Heterogeneous treatment effects

We investigate whether the effects of these policies are transmitted to labour demand through firms' balance sheets. We interact various firm-level balance sheet variables with the treatment measures described above and the time fixed effects, to estimate TWFE regressions of the form,

$$\ln(1+v_{ijt}) = \alpha_i + \gamma_{l(j),t} + \delta \cdot \text{Treatment}_{it} + \delta_{\text{het}} \cdot \text{Treatment}_{it} \cdot X_{\text{het},i} + \beta \cdot X_{jt} + \beta_{\text{het}} \cdot X_{\text{het},i} + \varepsilon_{jt}, \tag{5}$$

where $X_{\text{het},i} \subseteq (Log(1+\text{Assets}), \text{Leverage/Assets}, \text{Cash/Assets}, \text{Credit Score}, \text{Cases Start})$ is a vector containing the firm- and region-level variables for which we test for heterogeneity in treatment effects. For a detailed explanation of each of these variables, see Table A-2.

To estimate heterogeneous treatment effects for the doubly-robust estimator, we follow the empirical approach of Marcus and Sant'Anna (2021). We obtain two estimates of the simple and time-weighted average of all group-time average treatment effects: for observations with values above and below the median of the dimension of heterogeneity (e.g. firms with above and below-median credit scores). We then calculate the difference between these two effects and bootstrap it to estimate standard errors. We only employ this approach for the tier system study, since that is the only staggered treatment design where we allow for a binary treatment.

2.2.5 Continuous Difference-in-Differences

Most of our DiD designs have treatments that are fundamentally continuous (EOHO exposure, loan-to-assets ratio), or multi-valued (tier restrictions). While we also report estimates for discretised versions of the EOHO and tier restriction DiD designs that are similar in either magnitude or direction to the "continuous" estimates, caution is needed in interpreting the latter. In general, continuous effects may be biased if treated units self-select into receiving certain doses of the treatment (Callaway et al., 2021). This is a concern for the EOHO design, as restaurants signed up voluntarily to the scheme, leading local areas to "select" their overall exposure to the scheme; this is also true for the BBLS design, as firms could choose how much money they wanted to borrow. In the former case, however, we obtain very similar estimates using a discretised measure, for both the TWFE and doubly-robust specifications. In the latter case, the cap on the total loan amount of 25% of turnover or £50,000, whichever is lower, meant that most firms did not have a choice as to the precise loan-to-turnover ratio, assuaging self-selection concerns. Self-selection concerns are also less for the tier system design, as the tier levels were set by central governments.

In the absence of self-selection, Callaway et al. (2021) show that, under a weak conditional parallel trends assumption (treated units at any dose would have trends parallel to the untreated in absence of treatment), the TWFE DiD estimate can be interpreted as a weighted average of average causal responses on the treated ($ACRT(d|d)$) or a weighted average of treatment effects on the treated at a given dose of the treatment, scaled by the intensity of the dose ($ATT(d|d)/d$). The former gives the average effect of a marginal change in the dose for those units that actually received dose d, while the latter gives the average effect per dosage unit (rescaled by the dose) of receiving dose d compared to not being treated, for those units that actually received dose d. Given our discussion of selection effects above, and the fact that the doubly-robust estimator also estimates treatment effects on the treated (ATTs), we focus on these two interpretations.

Additional complications arise for staggered treatments, which occur for the tier system and the BBLS. In that case, even in the absence of selection effects, the DiD estimates may not be weighted averages of reasonable treatment effects when there is heterogeneity in treatment effects across treatment groups (different groups would have different effects even when treated at the same time with the same dose) or when there are treatment effect dynamics. For the tier system, we suspect that we have both, and so we should expect the TWFE estimates to be biased. Since we believe there is a reasonable argument for considering the main "treatment" to be binary (see Section 2.2.1), we address this potential bias by reporting both the TWFE and the doubly-robust estimates everywhere. For the BBLS, since we believe that the treatment of receiving a loan is inherently continuous, we cannot take this approach. However, we address concerns about treatment effect dynamics by estimating several event-study designs (Goodman-Bacon, 2021). Moreover, based on our discussion above, we expect that the pseudo-random disbursement of loans in the first few weeks of the BBLS guarantees the absence of treatment heterogeneity across groups of treated firms.

3 Results and Discussion

3.1 Initial pandemic shock: WHO Announcement

Our estimates of the effect of the initial pandemic shock on vacancies are reported in Table 2. The first column estimates the effect of the pandemic shock only, with no time fixed effects apart from month-of-the-year by SIC effects to account for seasonality. The second and third columns estimate the heterogeneous effect of the pandemic shock on firms with different initial balance sheet conditions (as filed in 2019). To ensure these heterogeneous effects are not driven by industry or regional heterogeneity, we progressively introduce week-year by SIC and week-year by NUTS2 fixed effects, which absorb the non-interacted WHO dummy.

In line with the time series in Figure 3, the coefficient on the WHO dummy indicates that firms drastically cut their vacancies in response to the COVID shock, with a reduction of about 30% in vacancy stocks for the average firm. Breaking this effect down by firms' pre-pandemic balance sheet conditions, we find that large, listed firms see a significantly higher reduction in their vacancy stocks. This aligns with findings in earlier work that smaller firms performed better during the pandemic in the UK (Hurley et al., 2021; Aquilante et al., 2020). We also see that firms with high leverage (current liabilities over assets), low cash holdings, and lower credit scores cut their vacancies by more than those with more healthy balance sheets. This is in line with previous economic literature, which has shown that firm leverage affects employment decisions when credit constraints become binding or when labour is a semi-fixed factor of production (see introduction), and additionally that firms' cash holdings play an important role in their performance in recessions (Joseph et al., 2020; Duchin et al., 2010).

3.2 Effect of Lockdown: Tier Restrictions

The above estimates give an unconditional estimate of how firm balance sheets have propagated the economic shocks induced by the pandemic to labour demand. They cannot, however, be interpreted as causal effects as they lump together the entire period after the WHO announcement as the "pandemic shock". In reality, that period was marked by a plethora of supply and demand shocks, including non-pharmaceutical interventions aimed at mitigating the spread of the virus and policy interventions aimed at softening the economic blow for firms and workers. Each of these affected firm labour demand in unique ways that we cannot disentangle with the above approach. Therefore, to shed more light on the mechanisms at play, we look at one of the most characteristic policy interventions associated with the COVID-19 pandemic: lockdowns.

Table 3 presents the results of the TWFE specification in Equation 2.[30] Panel A reports the results for the discretised treatment measure (being in a tier of 2 or greater), while panel B reports the results for the categorical treatment measure (moving to tiers 1 to 4). Introducing area-by-week and sector-by-week fixed effects increases the estimated negative effect, possibly

[30] We estimate it on a sample of only those NUTS-2 regions that saw a weakly increasing progression of tier restrictions, to avoid imposing an assumption that moving into a lower or higher tier has symmetric effects. In practice, this means that the two Welsh NUTS-2 regions as well as the Scottish Highlands drop out, though the results remain very similar when we cut the sample before Wales moved into a lower tier (November 15), so that it does not drop out.

Table 2: Impact of Initial COVID-19 Shock on Firm Vacancy Stocks

Dependent Variable:	Log(1+vacancy stock)		
Model:	(1)	(2)	(3)
Variables			
Post WHO	-0.2980***		
	(0.0020)		
Post WHO × Log(1+assets)		-0.0891***	-0.0908***
		(0.0040)	(0.0040)
Post WHO × Leverage / assets		-0.0145**	-0.0143**
		(0.0061)	(0.0060)
Post WHO × Cash / assets		0.0036**	0.0036**
		(0.0015)	(0.0015)
Post WHO × Credit score		0.0238***	0.0239***
		(0.0030)	(0.0030)
Post WHO × Age		-0.0172***	-0.0173***
		(0.0028)	(0.0028)
Post WHO × Listed company (=1)		-1.312***	-1.320***
		(0.1193)	(0.1185)
Post WHO × Corporate group (=1)		-0.0273***	-0.0268***
		(0.0046)	(0.0046)
Fixed-effects			
Firm-NUTS2	Yes	Yes	Yes
Month of year x SIC	Yes	Yes	Yes
Week x SIC		Yes	Yes
Week x NUTS2			Yes
Fit statistics			
Mean(exp(DV)-1)	1.8465	1.7042	1.7042
Observations	6,533,793	2,525,040	2,525,040
Clusters	103,711	40,080	40,080
Adjusted R^2	0.59030	0.57906	0.57947

Clustered (Firm-NUTS2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table present results from estimating variations of Eq. (1), controlling for increasingly stringent fixed effects, where SIC indicates the firm's 5-digit SIC industry. The sample period goes from March 1, 2019 to May 10, 2020, the date of the first easing of the first national lockdown. All variables are normalized to have a standard deviation of 1. The outcome variable is the logarithm of a firm's vacancy stocks on a given week within a given NUTS-2 region. WE only include firms-NUTS2 pairs for which we observed at least 3 vacancies in the year prior to the start of the pandemic. Model (2) and (3) are estimated only for firms with unconsolidated balance sheets. Covariates are as follows, where firm variables are common across regional branches of the same firm, and are measured from 2019 filings: *Post WHO* dummy that is 1 after March 11, the date the WHO declared COVID-19 a global pandemic; *Log(1+assets)*: log of total assets + 1; *Leverage / assets*: current liabilities / total assets; *Cash / assets*: bank and deposits / total assets of firm; *Age*: years since firm's incorporation; *Corporate group (=1)*: dummy for whether the firm is part of a corporate group. *Listed (=1)*: dummy for whether the firm is listed.

Table 3: Effect of Lockdowns on Firm Vacancy Stocks

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)
Panel A				
Post (Tier >= 2)	-0.0089**	-0.0113**	-0.0098**	-0.0090**
	(0.0039)	(0.0050)	(0.0046)	(0.0042)
Mean(exp(DV)-1)	1.7988	1.7988	1.7988	1.7988
Observations	747,692	747,692	747,692	747,692
Additional controls	2	122	231	957
Panel B				
Tier (0-4)	-0.0076***	-0.0091**	-0.0076**	-0.0064**
	(0.0025)	(0.0038)	(0.0034)	(0.0031)
Mean(exp(DV)-1)	1.7988	1.7988	1.7988	1.7988
Observations	747,692	747,692	747,692	747,692
Additional controls	2	122	231	957
Area by Week FE		NUTS1	NUTS1	NUTS1
Sector by Week FE			SIC1	SIC2

Clustered (NUTS2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference two-way fixed effect estimates based on Eq. (2) studying the impact of the regional lockdown measures put in place across the UK between September 20 and November 22, 2020 on firm-level online vacancy stocks. **Panel A** discretizes the treatment to enter into effect above tier level 2, while **Panel B** estimates the treatment effect on a categorical measure of the tier levels. Tier levels are: 0 – no restrictions; 1 – medium alert level; 2 – high alert level; 3 – very high alert level; 4 – full lockdown. For more detail on the tiers, see Table 1. All regressions control for new COVID cases and deaths in the NUTS-2 area, as well as firm and week fixed effects, with more granular fixed effects introduced stepwise. Mean DV gives the average number of vacancies a firm had on a given week across the sample.

due to anticipation effects in areas with high pre-treatment regional spread of COVID-19.[31] Mean $(\exp(\mathrm{DV})-1)$ is the mean vacancy stock across firms.

The estimated average treatment effect of moving into tier 2 (Panel A) or higher on a firm's vacancy stock is about -0.85%, while the estimated average treatment effect of moving into a higher tier (Panel B) is of roughly the same size.

However, the TWFE estimates may be biased due to the staggered design or additional heterogeneity in the treatment effects. To scrutinise this more closely, we report the estimates obtained from a doubly-robust estimator in Table 4, introducing an increasingly large set of controls. Panel A reports the equivalent TWFE specification, where the controls are interacted with week fixed effects while panels B and C report the doubly-robust estimator for group and dynamic effects.[32] Since the doubly-robust estimates do not allow for time-varying controls, we control for viral spread by controlling for COVID-19 cases and deaths in the NUTS-2 area in the first week of the sample, as well as for average weekly growth in deaths and cases in the weeks before treatment. Additionally, we allow for 1 week of treatment anticipation in the doubly-robust estimator, since regions' moves into higher tiers were sometimes anticipated in the media and by the public.

In light of the discussion in Section 2, the similarity of the "group" and "dynamic" doubly-robust estimates reflects the fact that the various treatment cohorts have similar treatment effects. Looking at the results from the estimation in Table 4, the OLS point estimates (panel A) decrease slightly when introducing firm-level controls, and become only marginally significant. The doubly-robust estimates, on the other hand, only become significant once we introduce firm-level controls, with an estimated drop of 7-8% in firm-level vacancy stocks from moving into Tier 2 or higher. Additionally, the Wald tests for parallel pre-trends for the doubly-robust estimator (P-val par. trends) indicate that we can only *not* reject the null of no pre-trends after *allowing* for firm-level controls. Since the doubly-robust estimator only imposes parallel trends conditional on controls (not unconditionally), this suggests that different types of firms had different pre-trends. Since for the doubly-robust estimates, the magnitudes increase by an order of magnitude – suggesting substantial bias in the TWFE estimates from the staggered design; and since the unconditional parallel trends assumption appears to be violated based on the Wald test, our preferred estimates are the doubly-robust ones that control for firm-level characteristics. This gives an estimated average effect of the tier restrictions on firm vacancy stocks of -7 to -8%, meaning that nearly 1 out of every 10 job vacancies were removed or not posted at an average firm in response to the tier restrictions.

For additional robustness, we report event-study estimates that include the full set of controls for both the TWFE and the doubly-robust estimator in Figure A-9. Both the TWFE and

[31]Note that the most granular area-by-week fixed effects we can introduce here are at the NUTS-1 level, since we code the treatment at the NUTS-2 level.

[32]For comparability with the doubly-robust estimates, and because they likely absorb a large amount of the variation in lockdown intensity across NUTS-2 regions, we do not control for NUTS-1-by-week, but only for SIC-2-by-week fixed effects in these regressions.

Table 4: Effect of Lockdown on Firm Vacancy Stocks: Doubly-Robust

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)
Panel A: OLS					
Post (Tier >= 2)	-0.0095***	-0.0093**	-0.0097**	-0.0077	-0.0071
	(0.0033)	(0.0035)	(0.0039)	(0.0055)	(0.0056)
Mean(exp(DV)-1)	1.7988	1.7988	1.7988	1.6443	1.6221
Observations	747,692	747,692	747,692	448,624	434,170
Additional controls	837	857	867	866	876
Panel B: doubly-robust (group)					
Post (Tier >= 2)	0.0059	-0.0223	-0.0272	-0.079***	-0.0794***
	(0.0073)	(0.0193)	(0.0299)	(0.0231)	(0.0277)
Mean(exp(DV)-1)	1.7988	1.7988	1.7988	1.6443	1.6221
Observations	747,692	747,692	747,692	448,624	434,170
Additional controls	837	857	867	866	876
P-val par. trends	0.0088	0.0043	2e-04	0.2291	0.1446
Panel C: doubly-robust (dynamic)					
Post (Tier >= 2)	0.0069	-0.025	-0.0386	-0.0714***	-0.0761***
	(0.0087)	(0.025)	(0.0383)	(0.02)	(0.022)
Mean(exp(DV)-1)	1.7988	1.7988	1.7988	1.6443	1.6221
Observations	747,692	747,692	747,692	448,624	434,170
Additional controls	837	857	867	866	876
P-val par. trends	0.0088	0.0043	2e-04	0.2291	0.1446
Sector by Week FE:	SIC-2	SIC-2	SIC-2	SIC-2	SIC-2
Week *times* additional control:					
COVID-19		X	X	X	X
Pop. Density			X	X	X
Log(1+assets)				X	X
Leverage / assets					X

Clustered (NUTS2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference estimates (OLS in Panel A and B, doubly-robust in Panel C) based on Eq. (2) studying the impact of the regional tier restrictions and lockdown measures put in place across the UK between September 20 and November 22, 2020 on firm-level online vacancy stocks. Doubly-robust SEs are bootstrapped 300 times. P-val par. trends reports the p-value of a Wald test for parallel trends (joint deviations of the pre-trends from zero). **Panel A** and **C** discretize the treatment to enter into effect above tier level 2, while **Panel B** estimates the treatment effect on a categorical measure of the tier levels. Tier levels are: 0 – no restrictions; 1 – medium alert level; 2 – high alert level; 3 – very high alert level; 4 – full lockdown. DR estimates allow for 1 week of treatment anticipation. For more detail on the tiers, see Table 1. All OLS regressions control for firm, week and week-by-SIC-2 fixed effects. Additional controls are introduced stepwise, and, in the OLS regressions, are interacted with week fixed effects. Additional controls are, by order of introduction: *COVID-19*: per capita weekly new COVID cases and deaths in NUTS-2 area – for DR estimator: average and weekly cases and deaths; *Pop. Density*: number of inhabitants per 1,000 km^2; *Log(1+assets)*: log of total assets (th. GBP) of firm; *Leverage / assets*: ratio of current liabilities to total assets of firm.

the doubly-robust dynamic treatment estimates are of similar magnitude as the corresponding pooled DiD estimates in Table 4, while the conservative 90% confidence bands include 0 for all the pre-trends of the doubly-robust estimator, providing additional support for the conditional parallel trends assumption. The doubly-robust estimates suggest that the negative effect on vacancy stocks deteriorated further over time, possibly due to the fact that most regions only gradually moved to the highest tier level after entering Tier 2.

We conclude that the second wave of lockdown measures in the UK led, on average, to a 7–8% drop in firms' vacancy stocks. Together with the -8 p.p. decrease in turnover growth estimated for the tier system by Hurley et al. (2021), this suggests that UK firms cut vacancy stocks by 1% for an additional percentage point decrease in turnover growth.

Next, we consider heterogeneity in the effect of lockdowns across firms and regions. Table 5 reports the difference in estimated overall ATTs between observations above and below the median along several dimensions of heterogeneity. Panel A reports the difference between simple averages of the ATTs, while Panel B reports the difference between the time averages. Table A-3 reports the TWFE results. We find little consistent evidence for firm-level heterogeneity, with scarcely any of the coefficients for the firm-level variables being significantly different from zero. This is remarkable, given the stark heterogeneity we documented in the context of the initial pandemic shock and the findings reported in the literature on firm-level employment and firm balance sheets, discussed above. One potential interpretation, though speculative, is that at the time the tier system was introduced, most government support programmes had already been put in place, including the BBLS and the Coronavirus Job Retention (furlough) scheme, which covered up to 80% of furloughed workers' wages. Hence, the absence of firm heterogeneity suggests, indirectly, that these policy interventions were successful at relaxing the employment-related credit constraints of firms and attenuating the propagation of negative shocks to firm-level employment through firm balance sheets. Even though the tier system's overall effect on vacancy stocks was still negative, these findings suggest that policy interventions that help relax firms' credit constraints can be effective in curtailing balance-sheet driven employment losses in the face of a negative shock. These averted employment losses are potentially large, given our findings in Table 2, which suggest an additional 1% decline in vacancy stocks for each standard deviation increase in a firm's leverage to assets ratio, all else equal.

3.3 Local Demand Shock: Eat Out to Help Out

Now, we look at how the Eat Out to Help Out scheme affected firms' labour demand. This programme functioned as a temporary, local positive demand shock by injecting up to £850 million into hospitality venues and consumers' wallets across the country, subsidising meals in restaurants and cafes. In Table 6, we replicate Table 1 of Fetzer (2021) but with local vacancy stocks as the outcome variable (instead of local COVID-19 clusters). Like that paper, we consider various alternative measures of treatment. Each panel tests a different transformation

Table 5: Effect of Lockdown on Firm Vacancy Stocks: Heterogeneity (Doubly-Robust)

DV: Log(1+Vacancy Stock):	Cases Growth	Density	Log(1+assets)	Leverage	Cash	Credit score
Panel A: doubly-robust (simple)						
Post (Tier >= 2)	0.0241	0.013	0.0083	-0.0163*	0.0031	-4e-04
	(0.0171)	(0.0116)	(0.0059)	(0.0086)	(0.0091)	(0.0101)
Panel B: doubly-robust (dynamic)						
Post (Tier >= 2)	0.0258	0.0158	0.0139*	-0.0139	-0.0023	-0.0086
	(0.0173)	(0.013)	(0.0084)	(0.0122)	(0.0124)	(0.0159)
Mean(exp(DV)-1)	1.7988	1.7988	1.6443	1.6221	1.7097	1.6077
Observations	747,692	747,692	448,624	434,170	340,296	453,376

Clustered (NUTS2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents doubly-robust difference-in-differences estimates studying the heterogeneous impact of the regional tier restrictions and lockdown measures put in place across the UK between September 20 and November 22, 2020 on firm-level online vacancy stocks. Estimates allow for 1 week of treament anticipation. Reported estimates are difference in ATTs between firms with values above and below median of heterogeneity variables reported in column. Treatment is discretized to enter into effect above tier level 2. For more detail on the tiers, see Table 1. Standard errors are bootstrapped 300 times. All regressions control for Cases start, Deaths start, and Density. Heterogeneity variables are, *Cases start*: per capita weekly new COVID cases in NUTS-2 area in first week of sample (37); *Cases mean*: average weekly new COVID cases in NUTS-2 area in weeks before treatment; *Density*: number of inhabitants per 1,000 km^2; *Log(1+assets)*: log of total assets (th. GBP) of firm; *Leverage / assets*: ratio of current liabilities to total assets of firm; *Cash / assets*: ratio of bank and deposits to total assets of firm; *Credit score*: annual probability of firm failure, based on credit rating.

of the treatment variable from EOHO exposure in levels over logged exposure to logged exposure per capita.

All variables are standardized for ease of interpretation, and all regressions are clustered by Local Authority District. The columns introduce increasingly stringent area-by-week fixed effects to control for regional spread in a non-linear manner, as in Fetzer (2021). That way, we estimate a fairly tight range of treatment effects, with a one standard deviation increase in EOHO exposure leading to a 3–5.4% counterfactual increase in regional vacancy stocks, on average. Per our discussion above, this effect is a weighted average of average causal responses by treatment dose and may be affected by selection bias. For that reason, we report doubly-robust estimates in Panel C of Table A-5.[33] We also introduce an increasingly stringent set of controls, which combines our firm-level controls with the regional controls of Fetzer (2021), and report TWFE estimates in panels A and B of Table A-5 for comparison.[34] That way, we continue to find a significant positive effect of the EOHO scheme on local vacancy stocks of 1.1–3.4% in the TWFE specification and 4.7–6.2% in the doubly-robust specification. One worry with these estimates is that, given the total size of the subsidy, they might be driven by temporary vacancies in the hospitality sector that were created to meet the additional demand. To address

[33] The doubly-robust estimator is based on the discretised treatment. In this case the EOHO exposure is equivalent for the two measures, restaurants and meals.

[34] We do not report estimates for the doubly-robust estimator when controlling for student exposure and tenure types as including these variable leads to multicollinearity issues.

this concern, we first re-estimate the models in Table 6 but exclude firms in the hospitality sector; the results are presented in Table A-6. This results in a slightly lower range of treatment effects of around 2.3–4.5%, suggesting that the local demand shocks induced by this scheme led to a general increase in labour demand across sectors. For context, if the scheme increased vacancy stocks by 3–5% relative to the counterfactual at an average of 500,000 total vacancies in the UK between June and August 2020 (Evans, 2021), this implies a total increase of 15,000–25,000 vacancies. This estimate, of course, is only a rough back-of-the-envelope calculation and depends on the implicit assumption that all firms would change their vacancies in a similar way in response to additional exposure to the EOHO scheme as the firms with only one trading address in our sample did. Given that large employers tend to account for a larger share of both positive and negative fluctuations in vacancies, however, the estimated impact on vacancies is more likely to be an underestimate, since firms with a single trading address employ fewer people on average (Moscarini and Postel-Vinay, 2012).

To test whether the increase in vacancies was only temporary, we estimate event studies in Figure A-11. All three specifications show that the increase in vacancies does not revert after the end of the programme in week 36, suggesting that the programme had persistently positive effects on job postings.

In Table 7, we look more closely at how these local hospitality demand shocks affected firms' labour demand differently depending on their balance sheets.[35] Since the doubly-robust estimates of the shock to labour demand are generally in line with the TWFE estimates – with most being around 5% – and since the EOHO design is not staggered (there is only one treatment cohort), we focus on the TWFE specifications. We separately interact the treatment dummy with each of the firm-level variables, and combine them all in the last column (columns 5 and 10). Panel A reports the coefficients from the interactions of the treatment dummy with dummies for whether the region is above the median of the relevant variable or not, while Panel B reports the coefficients from the interactions with the continuous variables. All regressions also interact the week fixed effects with the included firm-level variables. That way, we estimate a significant negative coefficient on the interaction between the treatment dummy and a firm's leverage ratio (columns 1 and 6), which persists even when including all interactions together (columns 5 and 10). In all regressions where leverage is the only interacted variable, the marginal effect of the interaction with leverage is small enough that the total effect of the policy remains positive even for highly leveraged firms. In other words: the EOHO scheme boosted job creation at all firms, but to a lesser extent at more leveraged firms. This finding complements the well-documented result in the corporate finance literature that highly leveraged firms reduce employment by more than their less-leveraged counterparts during recessions (see Introduction). Not only do negative shocks disproportionately *hurt* job postings at leveraged firms (see Table 2), but positive local demand shocks also *boost* job postings less at leveraged firms. One can

[35]Here, we use the participating restaurants as the EOHO exposure measure. The results are similar when using the number of claimed meals instead (see Table A-4)

Table 6: Impact of EOHO Local Demand Shocks on Firm Vacancy Stocks

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: EOHO exposure in levels						
Post ×meals	0.0305***	0.0307***	0.0316***			
	(0.0061)	(0.0063)	(0.0066)			
Post × restaurants				0.0328***	0.0323***	0.0337***
				(0.0077)	(0.0079)	(0.0086)
Panel B: EOHO exposure in log						
Post × Log(1+ meals)	0.0396***	0.0393***	0.0395***			
	(0.0050)	(0.0053)	(0.0054)			
Post × Log(1+ restaurants)				0.0529***	0.0533***	0.0543***
				(0.0054)	(0.0057)	(0.0060)
Panel C: EOHO exposure per capita in log						
Post × Log(1+meals per capita)	0.0400***	0.0399***	0.0404***			
	(0.0052)	(0.0054)	(0.0056)			
Post × Log(1+ restaurants per capita)				0.0481***	0.0482***	0.0501***
				(0.0055)	(0.0058)	(0.0062)
Mean(exp(DV)-1)	4.8426	4.8426	4.8426	4.8426	4.8426	4.8426
Observations	88,283	88,283	88,283	88,283	88,283	88,283
MSOA	6,791	6,791	6,791	6,791	6,791	6,791
Additional controls	388	1,207	4,119	388	1,207	4,119
Clusters	317	317	317	317	317	317
Area by Week FE	NUTS2	NUTS3	LAD	NUTS2	NUTS3	LAD

Clustered (LAD) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference estimates based on Eq. (3) studying the impact of the EOHO scheme on the MSOA-level online vacancy stocks of local firms with one single trading address on Indeed, across the 13 calendar weeks from 24 to 36. All regressions also control for area by week fixed effects. Regressors are normalized to have standard deviation equal to 1 for ease of interpretation.

interpret this finding as complementing the results of Benmelech et al. (2021), who showed that a banking deregulation shock decreased unemployment by relaxing financing constraints on firms' employment decisions. If leveraged firms are more constrained in their employment decisions because they cannot or are loathe to access the external finance needed to expand or optimize operations in response to a local demand shock (Whited, 1992), they will post fewer additional jobs than their counterparts in response to such a shock. Alternatively, firms with debt overhang may have distorted incentives, leading to under-performance and strategic default in extreme cases (Giroud et al., 2012; Myers, 1977). Such under-performance could manifest in suboptimal responses to local demand shocks. An important policy implication is that sector-specific subsidies aimed at spurring consumer demand, such as the EOHO scheme, may not be particularly well-suited to boosting employment at credit-constrained firms. While our earlier findings indirectly suggest that support policies aimed at alleviating credit constraints and subsidizing labour hoarding can help attenuate the difference in employment losses between credit-constrained firms and their counterparts in the face of a negative shock, the evidence we find from the EOHO scheme suggests that positive local demand shocks particularly boost job creation at less-leveraged firms.

An important caveat to our results is that we only observe one element of labour demand – vacancy posting. The differential impact of EOHO on vacancy posting may therefore be partially due to leveraged firms having more furloughed employees at the onset of the EOHO scheme that they were able to call upon to meet additional demand induced by the scheme. Unfortunately, we do not have high-frequency firm-level employment data, and data on participation in the Coronavirus Job Retention Scheme (CJRS) is only available from December 2020 onward, so we cannot directly assess this. However, the previous finding in the literature, discussed in the Introduction, is that firms in a more precarious financial position tend to cut employment by more in response to negative shocks. Since the CJRS only subsidized up to 80% of wage costs, one can expect these findings to equally apply to furloughed employment. Hence, it appears unlikely that leveraged firms had more workers on furlough at the start of the EOHO scheme. Moreover, given that we find that these firms cut their job postings (and likely, employment) by more in the first few months of the pandemic, one could expect that they had more pressing employment needs in response to the positive demand shock in August and so would increase their vacancies by more, not less. In addition, the number of jobs on furlough in August 2020 was close to its local minimum, with its decline decelerating, as can be seen in Figure A-10. Thus, it does not appear that the EOHO scheme induced a marked increase in the number of workers being brought back from furlough.

To assess pre-trends and treatment dynamics, we show TWFE and doubly-robust event studies for our three treatment measures in Figure A-11. We estimate the dynamic effects for an extended sample that goes out until week 40, four weeks after the end of the programme, to

Table 7: Impact of EOHO Local Demand Shocks on Firm Vacancy Stocks: Heterogeneity

DV: Log(1+Vacancy Stock):	(1)	(2)	38)	(4)	(5)
Panel A: interactions: dummy variables					
Post × EOHO restaurants	0.0542***	0.0271	0.0450***	0.0166*	0.0599***
	(0.0154)	(0.0166)	(0.0153)	(0.0085)	(0.0225)
× Leverage / assets (=1)	-0.0431**				-0.0370**
	(0.0168)				(0.0175)
× Log(1+assets) (=1)		-0.0134			-0.0047
		(0.0172)			(0.0191)
× Cash / assets (=1)			-0.0300*		-0.0214
			(0.0180)		(0.0177)
× Credit score (=1)				0.0051	0.0120
				(0.0125)	(0.0106)
Panel B: interactions: continuous variables					
Post × EOHO restaurants	0.0382***	0.0238**	0.0321**	-0.0179	0.0157
	(0.0092)	(0.0100)	(0.0128)	(0.0364)	(0.0398)
× Leverage / assets	-0.0208***				-0.0186***
	(0.0050)				(0.0051)
× Log(1+assets)		-0.0266			-0.0439
		(0.0320)			(0.0276)
× Cash / assets			-0.0100		-0.0119
			(0.0104)		(0.0097)
× Credit score				0.0122	0.0138
				(0.0120)	(0.0127)
Mean(exp(DV)-1)	6.2746	6.2746	6.2746	6.2031	6.3059
Observations	67,015	67,015	67,015	67,951	66,573
Additional controls	4,119	4,119	4,119	4,119	4,158
Clusters	316	316	316	316	316
Area by Week FE:	LAD	LAD	LAD	LAD	LAD

Clustered (LAD) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference regression estimates studying the heterogeneous impact of the EOHO scheme on the MSOA-level online vacancy stocks of local firms with one single trading address on Indeed, across the 13 calendar weeks from 24 to 36. *Log(1+assets)*: log of average total assets (th. GBP) of all firms with open vacancies in MSOA on given week; *Leverage / assets*: employment-weighted average of ratio of current liabilities to total assets of all firms with open vacancies in MSOA on given week; *Cash / assets*: employment-weighted average of ratio of bank and deposits to total assets of all firms with open vacancies in MSOA on given week. Dummy variables are equal to 1 (=1) if an observation is above the median for the corresponding variable.

assess the persistence of the effects.[36] The estimated pre-trends are not significantly different from zero at the conservative 10% level for 17 out of 18 estimated pre-trends, providing additional support for the parallel trends assumption. We find some evidence of treatment dynamics for the TWFE estimates, with the effect on vacancy stocks increasing as the programme progresses. Importantly, the effects of the policy do not revert to zero but persist for at least 4 weeks after the end of the programme in week 36. This stands in contrast to the estimates of the impact on COVID-19 cases in Fetzer (2021), which revert after the end of the programme. We also find no marked differences in the other EOHO estimates when extending the time window in this way. This suggests that the EOHO had a persistent impact on local vacancy posting and that the gap in response between leveraged and non-leveraged firms did not close after the programme ended.

3.4 Loans and Labour Demand: Bounce Back Loan Scheme

Next, we look in more detail at how positive liquidity supply shocks induced by the pseudo-random provision of loans to firms in the first weeks after the introduction of the BBLS affected firms' job posting decisions. Table 8 presents the estimates from the TWFE specification in Eq. (4). As before, we gradually introduce more stringent area-by-week and sector-by-week fixed effects. We fail to find any evidence that the sudden availability of liquidity when firms draw on their BBLS credit facility had any immediate effect on vacancy stocks that is common across firms, even though our sample consists of 1.4 million firm-NUTS2-by-day observations, which should provide ample statistical power. It is important to note, nonetheless, that, even though our control group consists only of the not-yet-treated firms – firms that eventually receive a loan within the three-week sample period – it might not capture the counterfactual trend adequately if firms only cut vacancies when their liquidity constraints become binding, that is, when they have to start cutting into employment to meet other short-term costs. If that were the case, our current control group would have deceptively stable vacancy stocks, even if the loan disbursement did help avert eventual vacancy losses. Moreover, due to the relatively short sample period (three weeks after the implementation date), our estimates can only draw a short-term picture of the effect of the BBLS on vacancies.[37]

Breaking the estimated effects of the BBLS on vacancy posting down according to firms' balance sheets in Table 9, however, reveals that a subset of firms did see a counterfactual uptick in their vacancy postings after receiving a loan. Specifically, we find that firms with a higher credit score had vacancy stocks that were about 0.5-0.9% higher if they received a loan that was larger by one standard deviation (relative to their annual turnover). This suggests that less financially healthy firms used the loan to service other, more urgent costs instead of

[36] The baseline estimates in Table 6 are similar when we use this extended window, see the Online Appendix.

[37] Extending this window further than a month out leads to significant violations in the pre-trends, so we do not report the corresponding estimates as they are likely invalid. Though this limits our ability to assess long-term effects, it does support our baseline design by indicating that later-treated firms have significantly different pre-trends from earlier-treated ones.

Table 8: Effect of Bounce Back Loan Scheme on Firm Vacancy Stocks

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)
Post × Loan / turnover	0.0006	0.0007	0.0010	0.0009
	(0.0006)	(0.0006)	(0.0007)	(0.0007)
Mean(exp(DV)-1)	0.07661	0.07661	0.07661	0.07661
Observations	1,218,747	1,218,747	1,218,747	1,218,747
Firm-NUTS2	23,897	23,897	23,897	23,897
Additional controls	864	2,139	2,648	5,861
Clusters	23,948	23,948	23,948	23,948
Area by Week FE	NUTS1	NUTS2	NUTS2	NUTS2
Sector by Week FE			SIC1	SIC2

Clustered (Firm-NUTS2 & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table shows pre- and post-treatment effects of the Bounce Back Loan Scheme on daily firm-level online vacancy stocks in the UK, obtained from a difference-in-difference design as in Eq. (4) where treatment occurs on the first day a firm draws money from the loan facility at the lending bank, and is equal to *loan amount / annual turnover*. Sample goes from April 4, 2020 to May 24, 2020. Only the not-yet-treated firms are used as controls. All models include firm-NUTS2, day, and bank by day fixed effects, where bank indicates the bank which provided the loan. Regressors are normalized to have standard deviation equal to 1 for ease of interpretation.

increasing their labour demand. The small effect for financially healthier firms relative to the effects estimated for the other shocks we study – which ranged from 5–30% – suggests that these firms also allocated a share of the loan to non-payroll costs. This aligns with evidence for the United States' equivalent of the BBLS, the Paycheck Protection Program, which found small employment effects that were driven by firms building up savings buffers and using the loans to make non-payroll fixed payments (Granja et al., 2022).[38] It also aligns with recent evidence on firm-level employment response to positive credit supply shocks (Aristizábal-Ramırez and Posso, 2021). The absence of heterogeneous effects for different firms of the tier system that we documented earlier suggests that boosted savings buffers may, nonetheless, have averted additional vacancy cuts down the line.

To assess pre-trends, document treatment dynamics, and test for robustness to expanding the time window around the introduction of the BBLS, we estimate several event studies in Figure A-12. The figure shows coefficients for observations with a credit score above (blue line) and below (black line) the median. Only one of the 54 estimated pre-trends is significantly different from 0 at the conservative 90% confidence interval, providing support for the parallel trends assumption. Additionally, we find that the vacancy effects for financially healthy firms only materialize after around 4 to 5 days, most likely due to a lag between the receipt of funds

[38] Though note that the PPP included a requirement that firms do not decrease their wages or number of full-time employees.

Table 9: Effect of Bounce Back Loan Scheme on Firm Vacancy: Heterogeneity

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)
Panel A: interactions: dummy variables					
Post × Loan / turnover	-0.0008	0.0005	-0.0009	0.0010	-0.0034
	(0.0009)	(0.0008)	(0.0018)	(0.0010)	(0.0029)
× Credit score (=1)	0.0049***				0.0087***
	(0.0014)				(0.0030)
× Log(1+assets) (=1)		-5.94×10^{-5}			-0.0012
		(0.0017)			(0.0026)
× Cash / assets (=1)			0.0006		-0.0007
			(0.0025)		(0.0024)
× Leverage / assets (=1)				-0.0014	0.0009
				(0.0014)	(0.0023)
Mean(exp(DV)-1)	0.08659	0.08659	0.08659	0.08659	0.08659
Observations	1,109,709	1,080,945	611,082	1,054,374	594,711
Firm-NUTS2	21,759	21,195	11,982	20,674	11,661
Additional controls	5,913	5,913	5,912	5,913	6,017
Clusters	21,810	21,246	12,033	20,725	11,712
Panel B: interactions: continuous variables					
Post × Loan / turnover	-0.0050**	0.0005	-0.0007	-0.0004	-0.0058
	(0.0019)	(0.0019)	(0.0015)	(0.0008)	(0.0042)
× Credit score	0.0028***				0.0039***
	(0.0009)				(0.0014)
× Log(1+assets)		-0.0007			-0.0015
		(0.0009)			(0.0018)
× Cash / assets			8.89×10^{-5}		-0.0002
			(0.0010)		(0.0011)
× Leverage / assets				0.0013	-0.0006
				(0.0013)	(0.0008)
Mean(exp(DV)-1)	0.07873	0.07873	0.08775	0.07908	0.08896
Observations	1,109,709	1,080,945	611,082	1,048,050	594,711
Firm-NUTS2	21,759	21,195	11,982	20,550	11,661
Additional controls	5,912	5,912	5,913	5,912	6,017
Clusters	21,810	21,246	12,033	20,601	11,712
Area by Week FE:	NUTS2	NUTS2	NUTS2	NUTS2	NUTS2

Clustered (Firm in NUTS2 & Day) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table shows pre- and post-treatment effects of the Bounce Back Loan Scheme on daily firm-level online vacancy stocks in the UK, obtained from a difference-in-difference design where treatment occurs on the first day a firm draws money from the loan facility at the lending bank, and is equal to *loan amount / annual turnover*. Sample goes from April 4, 2020 to May 24, 2020. Only the not-yet-treated firms are used as controls. All models include firm-NUTS2, day, and bank by day fixed effects, where bank indicates the bank which provided the loan. Regressors are normalized to have standard deviation equal to 1 for ease of interpretation. All regressions include time by covariate fixed effects for those covariates interacted with the DID estimator, which are: *Log(1+assets)*: log of total assets (th. GBP) of firm; *Leverage / assets*: ratio of current liabilities to total assets of firm; *Cash / assets*: ratio of bank and deposits to total assets of firm; *Credit score*: annual probability of firm failure, based on credit rating.

and subsequent decisions relating to workforce expansion. Finally, our findings are robust to extending the sample period by one week in either direction.

4 Conclusion

The COVID-19 pandemic and the policy interventions that aimed to reduce the spread of the disease led to a sharp contraction in economic activity in the UK. In this paper, we study the evolution of labour demand during this period using novel, comprehensive data on firms' vacancy posting behaviour. We pay particular attention to the role that the financial health of firms entering the pandemic played in the propagation of shocks.

We report a substantial decline in vacancy posting at the onset of the pandemic of about 30% for the average firm. We find significant heterogeneity in the magnitude of the response, however, with larger, cash-strapped firms with high leverage and lower credit ratings cutting their demand for labour by more.

In order to isolate some of the different influences on labour demand during this period, we also exploit natural variation introduced by three different policy interventions. First, we find that regional variation in lockdown restrictions in the UK during the second wave of the pandemic led to a significant decline of around 7–8% in firms' vacancy stocks with little variation across firms (in contrast to the first wave). The difference in response relatively to the first wave is likely the result of the ready availability of government support during the second wave of the pandemic in the UK in Autumn 2020, providing indirect evidence for the ability of government support to help firms withstand temporary negative shocks.

Second, we estimate that the positive local demand shocks induced by the Eat Out to Help Out (EOHO) scheme led to an increase in job posting of 3–5% — around 15,000–25,000 extra vacancies (alongside the extra COVID cases that it was found to have caused according to analysis in Fetzer (2021)). Notably, the scheme led to positive spillover effects to local firms in all sectors, not just the hospitality firms that were directly targeted by the policy, and the response was more pronounced for firms with lower leverage.

Finally, we find that firms with a higher credit score increased their posted vacancies by around 0.5–0.9% in the 10 days after receiving a loan, but low-credit-score firms did not. These findings complement the link between firm leverage and employment losses in response to negative shocks documented in the corporate finance literature, by establishing how firm balance sheets propagate positive demand and supply shocks.

Our results suggest that while policy interventions may be able to mitigate asymmetries in vacancy posting between firms in better and worse financial health in the face of negative shocks, job creation in the face of positive shocks is largely driven by firms in better financial health. That finding may be important for policymakers to consider as they construct interventions in response to future economic crises.

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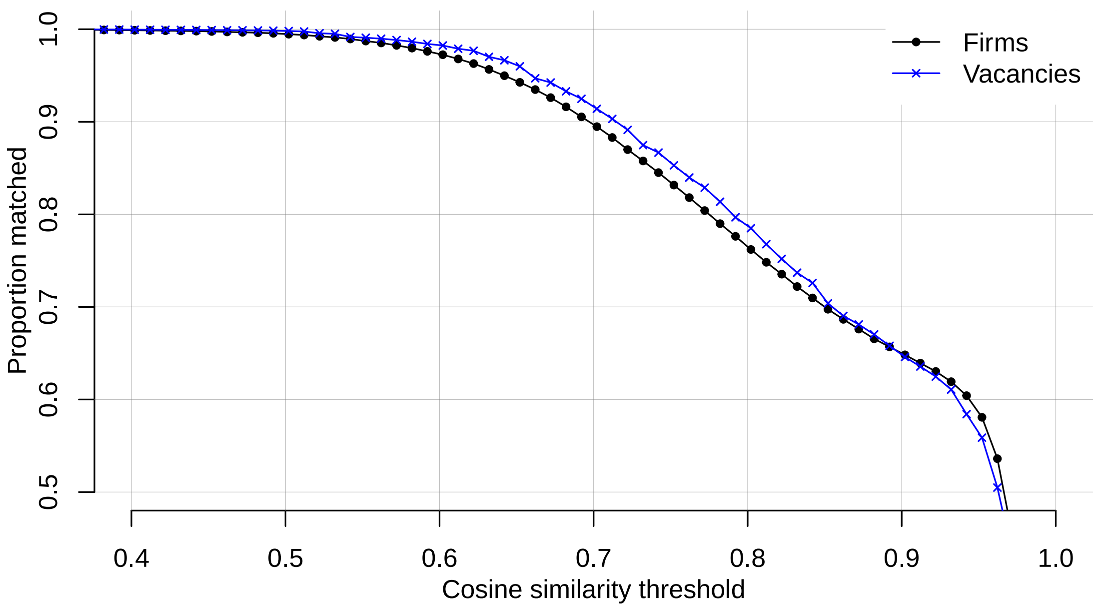
A1 Matching Approach Details

Firm Name Matching

Since the vacancies aggregated by Indeed are often posted manually by the advertising companies, the company name field contains many inconsistencies and typos. We observe ~500,000 "unique" (with possible duplicates due to typos) companies that posted vacancies between January 2019 and June 2021 and ~4 million unique registered firms that were active at any point in the same period. To match the former to the latter, we rely on a large-scale fuzzy matching approach.

Figure A-1: Proportion of Companies Matched by Cosine Similarity Threshold



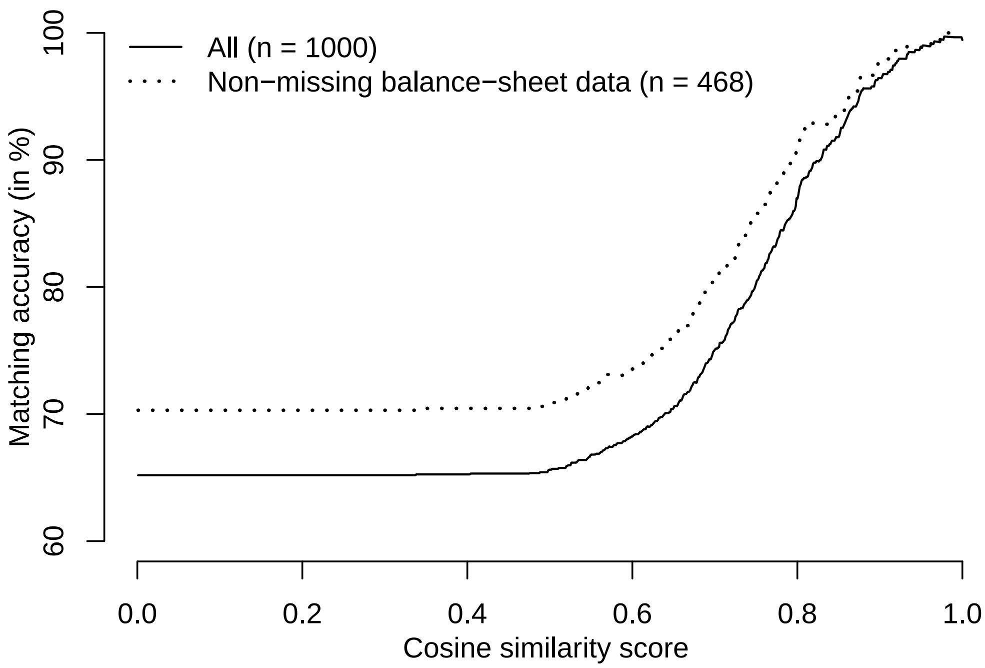
Note: Vertical axis: Proportion of Indeed company names (black line) and Indeed vacancies (blue line) that were matched with a registered company name in the FAME data as a function of the minimum cosine similarity required to establish a match. The period of the analysis is March to November 2020.

To start, we construct a vector containing all 3-grams from the combined set of Indeed firms and registered firms. We then calculate the Term Frequency-Inverse Document Frequency (TF-IDF) of each of the $i = 1, \ldots, I$ 3-grams for each of the $D := N \times M$ companies $j_1 \in 1, \ldots, N$, $j_2 \in 1, \ldots, M$ in the set of N Indeed and M FAME companies, as:

$$\text{TF-IDF}_{ij_C}^{C} := TF_{ij_C} \times DF_i := \text{Frequency of 3-gram } i \text{ in company name } j_C \times \left(\ln\left(\frac{1+D}{1+df_i}\right) + 1 \right) \tag{A-1}$$

for $C \in 1, 2$; where TF-IDF is a $D \times K$ matrix that represents each firm name as a vector of I 3-grams and df_i is the frequency of the 3-gram i in the entire corpus of firm names. We calculate the cosine similarity between each of the firm names in the Indeed and registered firm name sets as the scaled dot product between the two matrices, keeping only the most similar result for each name.[39] Figure A-1 shows the proportion of Indeed firms and vacancies in our main period of analysis (March–November 2020) that are successfully matched for each cosine similarity cutoff (including exact matches).

Figure A-2: Matching Accuracy by Cutoff



Note: Figure shows matching accuracy by cosine similarity cutoff, for all firms and those with non-missing FAME balance sheet data.

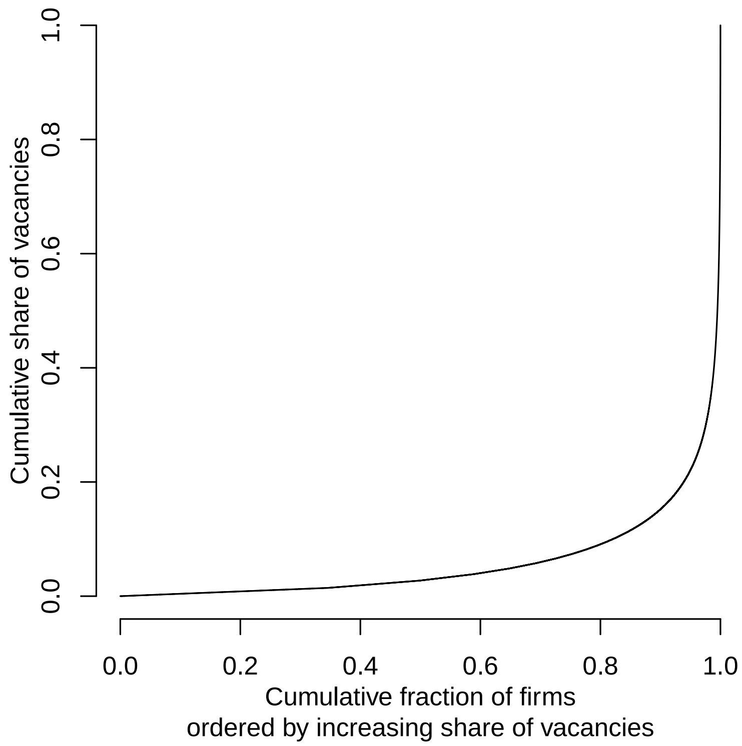
Based on these initial matches, a research assistant manually classified a random sub-sample of 1,000 matches into good and bad matches – based on a subjective assessment of whether the name of the Indeed firm and the FAME firm referred to the same entity – without viewing the calculated similarity score. Figure A-2 shows the accuracy of the matching based on a manual assessment of the 1,000 matches and a subset of 468 including those firms with balance sheet information as a function of the cutoff of the similarity score. Relying on this analysis, we target an accuracy of 90%, which sets the similarity cutoff to 0.828.

Most vacancies are posted by a minority of the firms as evidenced by Figure A-3 which depicts a Lorenz curve for the distribution of vacancies across firms in our sample between January 2019 and June 2021. Just 6.1% (17.9%) of the firms account for 80% (90%) of the vacancies; the distribution of posting is highly unequal.

Table A-1 reports summary statistics that compare the full FAME sample of firms active in 2020 to the sub-sample of firms matched to Indeed with a cosine similarity cutoff of 0.828. The matched sample consists of larger (by total assets), older firms with higher levels of liabilities and more cash (Bank and Deposits). Moreover, the matched firms appear to more often have

[39] This procedure is fully implemented in Python and C++ in the https://pypi.org/project/string-grouper/ and https://pypi.org/project/sparse-dot-topn/ packages.

Figure A-3: Lorenz Curve for Share of Vacancies in Data



subsidiaries or be part of a corporate group, and tend to employ more people than the firms in the full sample. Part of these differences are mechanical, insofar as the smallest firms have only one employee and so should only rarely post vacancies. Indeed, such single-employee firms span the bottom quartile of employment in FAME, while in the matched sample they only make up the first percentile. It is also possible that part of this difference is driven by a bias in the firms posting on Indeed, for example if large firms are more likely to post vacancies online. Finally, it is also possible that large firms entered their own names more accurately and consistently on Indeed as compared to smaller firms. Figure A-1 speaks to this effect. It plots the share of unique firms and vacancies matched for increasingly stringent similarity cutoffs. At similarity scores below 0.85, the slope of the curve for the share of vacancies matched is slightly flatter that the slope of the curve for share of firms matched. This suggests that firms that account for fewer vacancies tend to be matched with lower accuracy. Nonetheless, both curves are very similar, suggesting this should not introduce severe biases.

The matching, however, is biased by chain stores tending to be matched to their headquarters' firm name with relatively low accuracy. This is because chain stores' names often contain extra words indicating the location of the store (e.g. "[Supermarket chain name] Lombard Street" would be matched to "[Supermarket chain name] Limited", but with lower accuracy than "[Supermarket chain name]" alone). To account for this bias, our research assistant inspected all FAME firms with more than 10 Indeed firms matched to them. Around 65% of these firms are chain stores, whose matches tend to have a low cosine similarity, though there are very few cases of incorrect assignment (as determined by manual inspection) for these types of firms. The remaining 35% of these firms are "sinkhole" firms, which we define to be firms with a general name that attracts matches, for example "The Barber Shop". For the chain stores, we drop the requirement that the match occur with a cosine similarity above our threshold, while we remove all "sinkhole" firms from our sample, even if they meet the threshold.

Table A-1: Summary Statistics: FAME vs. Matched Sample.

	FAME (N=4,938,785)			**Matched** (N=266,994)		
Variable	Mean	Median	Pct. NA	Mean	Median	Pct. NA
Total Assets th GBP 2019	10048.11	40.00	0.47	12666.42	261.00	0.45
No of companies in corporate group	10.81	0.00	0.00	16.12	0.00	0.01
Current Liabilities th GBP 2019	-4344.70	-25.00	0.52	-6347.17	-127.00	0.46
Bank & Deposits th GBP 2019	665.86	16.00	0.74	1376.69	74.00	0.62
No of subsidiaries	0.08	0.00	0.00	0.16	0.00	0.01
Number of employees 2019	14.61	2.00	0.70	51.40	10.00	0.57
Credit score	39.55	37.00	0.46	51.34	45.00	0.44
Age 2020	7.98	5.00	0.19	11.96	8.00	0.34
Listed	0.00	0.00	0.00	0.00	0.00	0.01

Note: Table shows summary statistics for full FAME Sample compared to subsample of firms that are matched to Indeed with a cosine similarity above the cutoff of 0.83.

Another important potential bias in our matching approach comes from the fact that many recruitment firms, who are contracted to hire new staff, post vacancies under their own firm's name rather than the firm they are recruiting for. To account for this, we drop all firms in SIC industries that relate to employment and recruitment agencies.[40] Furthermore, we drop all firms that have any term specific to employment and recruitment agencies in their firm name.[41]

A last issue that our matching approach might face is that FAME contains many subsidiary and parent companies, which often have similar names. This could lead us to attribute subsidiaries to their parents or vice versa, leading to double counting or the incorrect attribution of balance sheets to firms. We address this issue in two ways. First, following the literature (e.g. Cravino and Levchenko (2017)), we retain only firms with unconsolidated balance sheets. To account for the fact that unconsolidated balance sheets do not reflect any access that firms in corporate groups might have to internal capital markets, we include a dummy for whether a firm is part of a corporate group in our main analysis. Second, we closely scrutinise the ownership structure of the matched firms. Figure A-4 (left panel) plots the number of matches from Indeed assigned to each company in FAME against the number of companies in the corporate group of each company in FAME.

The concentration of points near the vertical axis indicates that the Indeed firms that have similar names (and thus are matched to the same FAME firm) do not tend to be part of a

[40]These are SIC (2007) codes 78100, 78101, 78109, 78200, and 78300.

[41]Specifically, we filter for: recruitment, resourcing, headhunter, headhunters, recruiter, recruiters, recruit, hiring, outsource, outsourcing, employment, career, careers, personnel, workforce, placement.

Figure A-4: Matching: Identical Names

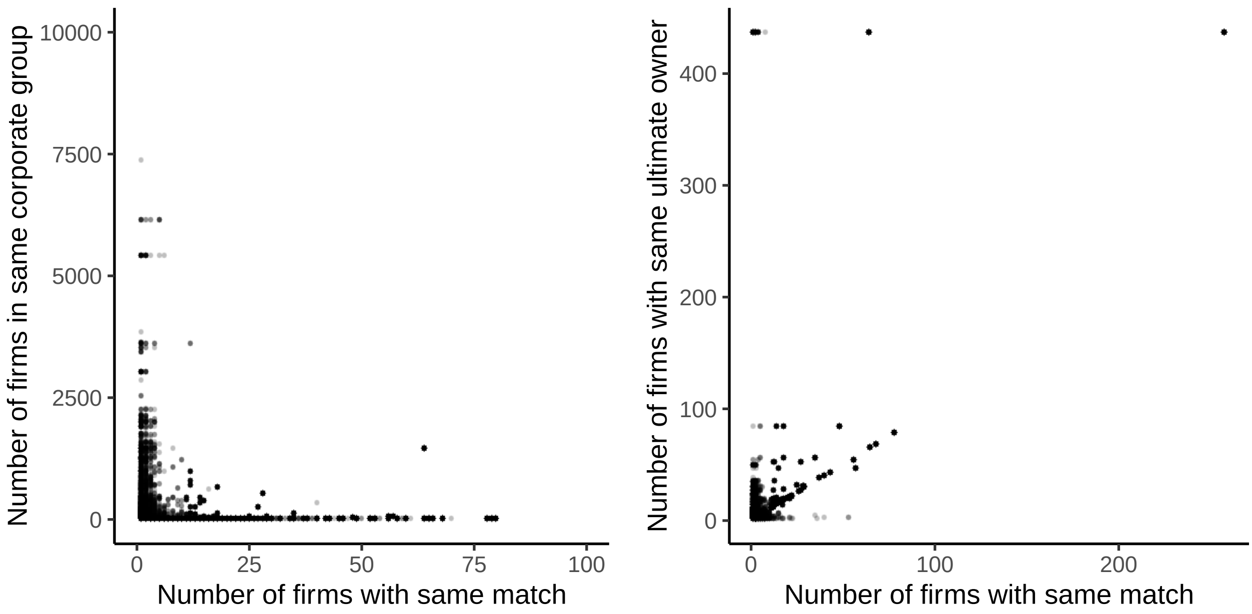
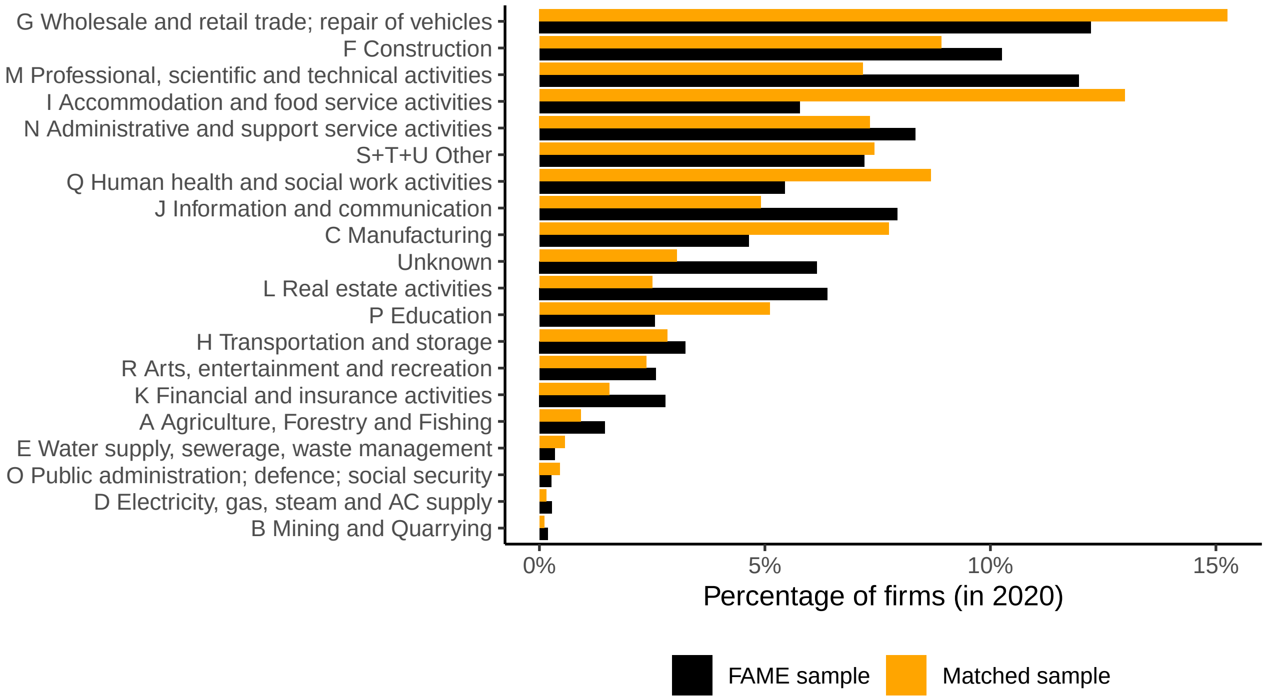


Figure A-5: Firm shares by industry, Full FAME vs. Matched Sample



Note: This figure plots the percentage of total unique firms observed in each industry in the full FAME sample (blue) and the matched FAME-Indeed sample (orange).

(large) corporate group. This suggests that firms in corporate groups in the UK tend to have sufficiently different names. This conjecture is reinforced by comparing the number of Indeed firms which matched to the same FAME firm against the number of other FAME firms that have the same ultimate owner in Figure A-4 (right panel). We observe two distinct cases in the data: a trivial case, where all Indeed firms that share the same owner are matched to the same FAME firm, depicted by the points on the 45 degree line; and a non-trivial concentration of points near the vertical axis, suggesting that outside the trivial case, most FAME firms that share ultimate owners tend to have distinct names. Finally, our manual inspection of firms with more than 10 Indeed matches turns up only very few cases of wrong assignment of similarly named firms within company groups, which further suggests that this should not pose a big threat to our matching approach.

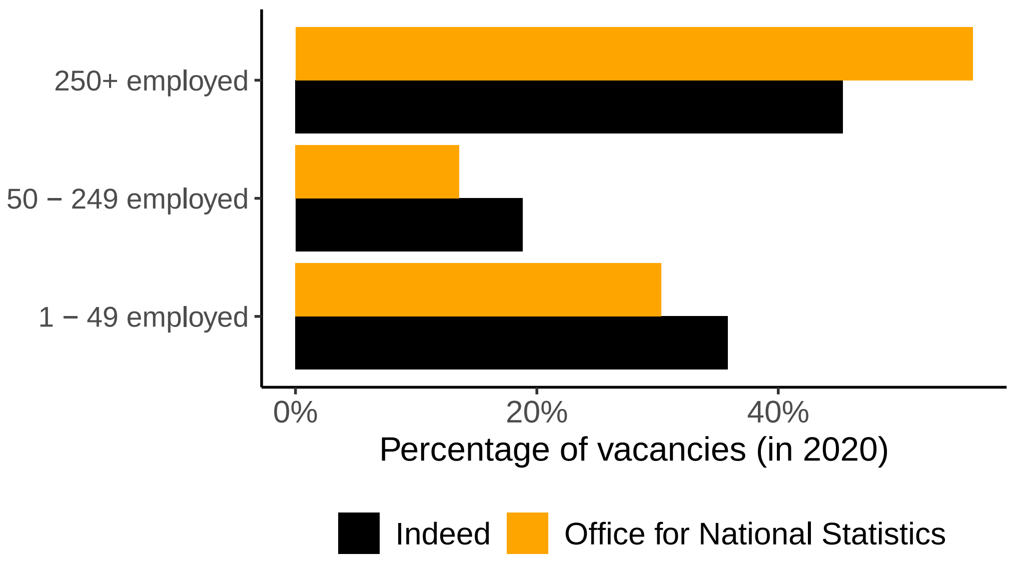
After having accounted for these various biases, we can inspect the representativeness of our matched firms by looking at industry representation in the matched sample. Figure A-5 reports the proportion of vacancies in each SIC division for both the full FAME sample and the matched sample. Most industry shares in the matched sample are in the same ballpark range as in the FAME sample, suggesting a fair representation of the various industries in the Indeed vacancies. Industries whose shares do see fairly large differences are Real Estate Activities (L), Accommodation and food services (I) and Education (P). The number of firms that did not report industry codes decreases by about three percentage points in the matched sample.

Region Code Matching

The vacancies are assigned region (UK country and county ISO) and city codes by Indeed but these fields do not have full coverage and do not match standard statistical classifications. We assign each vacancy a NUTS 2016 code at the most granular level possible by mapping the county ISO codes to NUTS codes. Wherever a vacancy's county code is missing, we retrieve its city's NUTS code using Postcodes.io, a geolocation API for the UK. This way, we are able to match 100% of the vacancies to a NUTS-2 code, which is the main regional unit we use in our analyses.

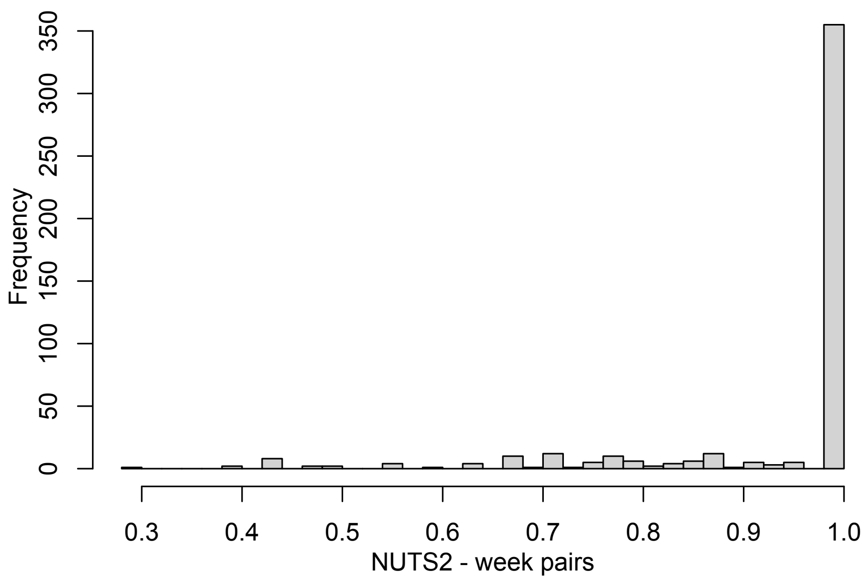
A2 Figures

Figure A-6: Vacancy shares by Employment Size, Indeed vs. ONS



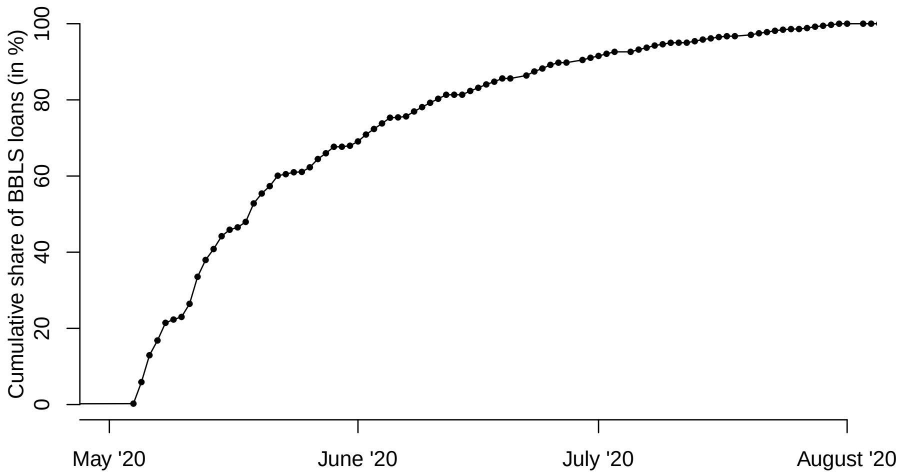
Note: This figure plots the average share of monthly firm-size-specific vacancy stocks in total vacancy stocks for 2020, comparing vacancy stocks from Indeed to those from the ONS Vacancy Survey. Firm with missing employment information in the Indeed data are assigned an estimated number of employees by multiplying the firm's total assets by the industry-wide employee to assets ratio, following the recommendations in the manual of Bureau Van Dijk (2021).

Figure A-7: Agreement of Rounded Average Tier Restriction with the Tier Level on the Local Authority Level



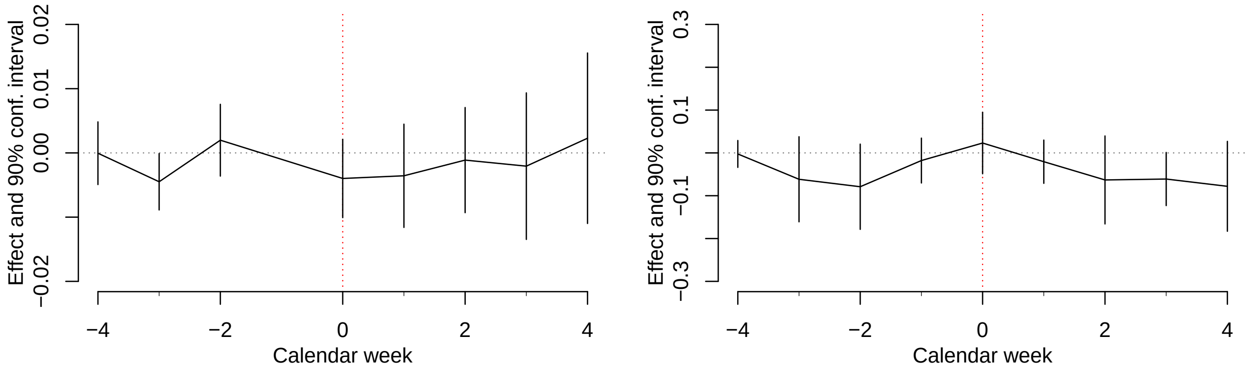
Note: This histogram shows the agreement between aggregated NUTS-2 tiers and the Local Authority level tiers for the 462 NUTS-2-week pairs, with 1.0 indicating perfect agreement.

Figure A-8: Share of Bounce Back Loan Scheme Firms That Took Out First Loan, May–August 2020



Note: This figure plots the cumulative share of all firms in the matched sample that took out a BBLS loan on a given date.

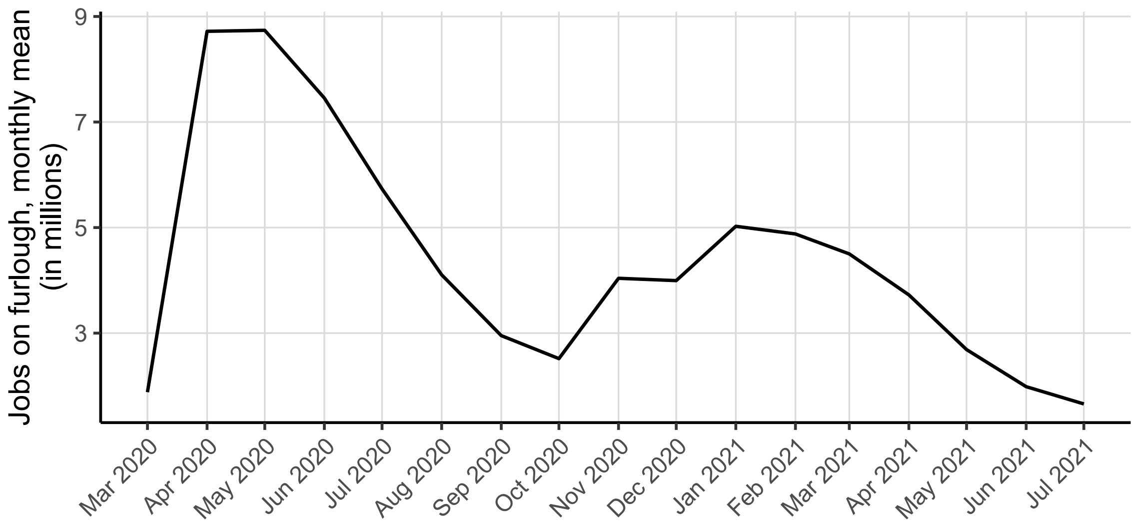
Figure A-9: Tier System: Dynamic Effects



(a) TWFE (Tier ≥ 2) **(b)** Doubly-robust (Tier ≥ 2)

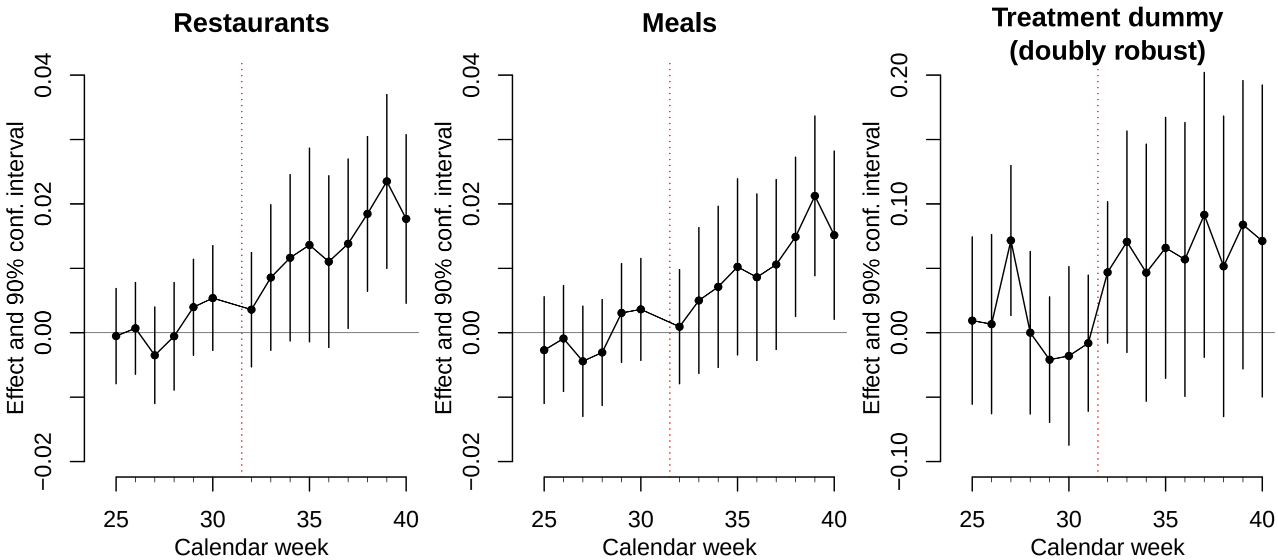
Note: This figure shows event-study equivalents of difference-in-difference design for effect of tier system on vacancy postings, along with 90% confidence intervals. **Panel a)** estimates TWFE event-study specification with discretized treatment and balanced panel, using periods -5 and -1 as reference periods and absorbing periods more than 5 weeks before and 4 weeks after treatment with dummies, following Borusyak et al. (2021). Controls include week fixed effects interacted with: COVID-19 cases and deaths, and population density. **Panel b)** shows doubly-robust estimator, aggregated by treatment period. Controls include COVID-19 cases and deaths, and population density. Standard errors are bootstrapped 300 times.

Figure A-10: Total Jobs on Furlough in UK, 2020



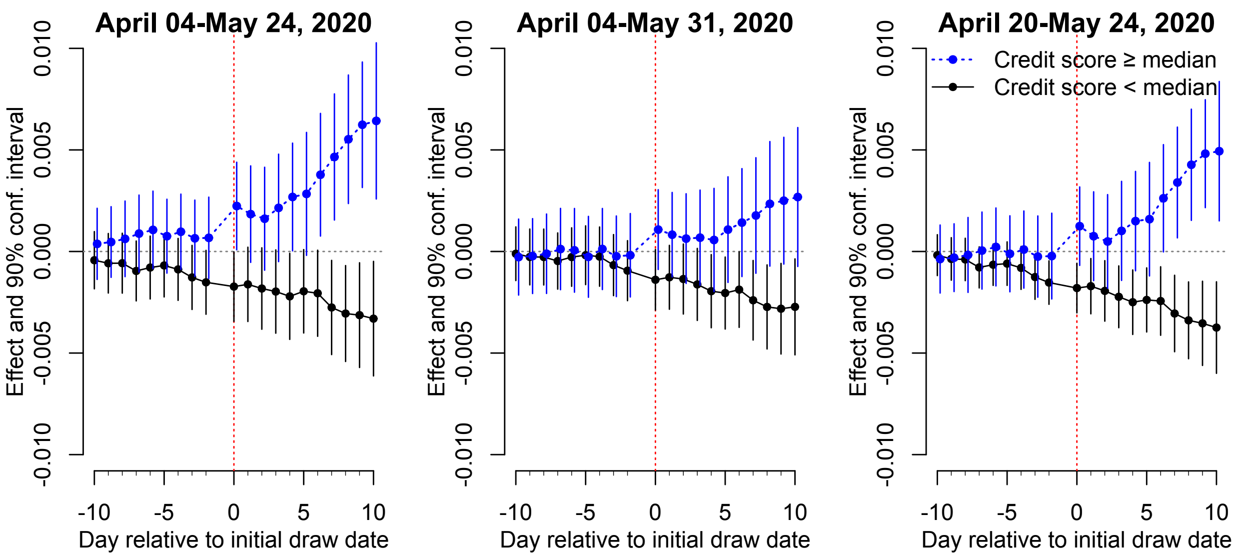
Note: This figure shows time series of monthly averages of total jobs claimed for under Coronavirus Job Retention Scheme across the UK, May-Dec 2020 (UK Government, 2021a). Employers claiming less than 100 jobs in a claims period (one calendar month) were not required to enter start and end dates of claim, so the entire month is considered claim period.

Figure A-11: Eat Out to Help Out: Dynamic Effects



Note: This figure shows event-study equivalents of difference-in-difference design for effect of EOHO scheme on local vacancy postings, along with 90% confidence intervals. Controls include area- and sector-by-week fixed effects for the TWFE specifications, and the full set of firm and region controls for both specifcations. The **left and middle panel** estimate TWFE event-study specification with continuous treatment and balanced panel, using weeks 30 and 24 as reference periods, following Borusyak et al. (2021). The **right panel** shows doubly-robust estimator for discretised treatment, aggregated by treatment period. Standard errors are bootstrapped 1,000 times.

Figure A-12: Bounce Back Loan Scheme: Dynamic Effects



Note: This figure shows pre- and post-treatment effects of the Bounce Back Loan Scheme on daily firm-level online vacancy stocks in the UK, obtained from a difference-in-difference design where treatment occurs on the first day a firm draws money from the loan facility at the lending bank, and is equal to *loan amount / annual turnover*. DV is log(1+vacancy stocks). All panels show the estimates from a two-way fixed effects DiD design estimated using demeaned OLS, with varying sample sizes.

A3 Tables

Table A-2: Description and Sources of Key Variables

Context	Variable	Description	Source
All	Vacancy stock	The number of unique active vacancies a firm (or a firm's regional branches) had on Indeed in a given time period. In EOHO context: number of active vacancies in MSOA.	Indeed (2021)
	Assets	Total assets (GDP, in thousands) a firm possessed in 2019, consisting of current assets = stock + w.i.p. + trade debtors + bank and deposits + other current assets + investments; and fixed assets = tangible assets + intangible assets + long-term investments.	Bureau Van Dijk (2021)
	Leverage	Current liabilities (GDP, in thousands) a firm possessed in 2019, consisting of trade creditors, short-term loans and overdrafts, and total other current liabilities (= tax + accruals + dividends + social securities + V.A.T. + other).	Bureau Van Dijk (2021)
	Cash	Bank and deposits (GDP, in thousands) a firm possessed in 2019.	Bureau Van Dijk (2021)
	Credit score	Predicted likelihood of a company going insolvent in the next 12 months, as of December 2019, as estimated by Bureau van Dijck, based on companies' accounts, SIC data, directors' history, shareholders' data, court judgements, and holding/subsidiary structure.	Bureau Van Dijk (2021)
EOHO	Restaurants	Average number of restaurants listed on HMRC EOHO app throughout August 2020 in a given MSOA.	Fetzer (2021)
	Meals	Total number of meals claimed in a given parliamentary constituency (PC), weighted by the number of enrolled restaurants in a given MSOA as a share of total enrolled restaurants in all MSOAs in PC.	Fetzer (2021)
	Population density	Number of inhabitants per 1,000 square km in a given MSOA.	Fetzer (2021)
	Spring 2020 COVID-19 exposure	Cumulative number of COVID-19 deaths in MSOA between March and July, 2020.	Fetzer (2021)
	Student exposure	Total number of full-time and part-time students living in MSOA.	Fetzer (2021)
	Tenure types	Share of accommodation rented and owned in MSOA.	Fetzer (2021)
Tier system	Tiers (0-4)	Level of tier restriction in place in NUTS-2 region in given week, calculated as the average of the restrictions in all local authority districts in the NUTS-2 region, rounded to the nearest integer.	Bank of England
	Deaths	Number of weekly reported COVID-19 deaths, excluding deaths classified as COVID-19 deaths that occurred more than 28 days after positive COVID-19 test.	UK Government (2021b)
	Cases	Number of weekly reported COVID-19 cases, by diagnosis date.	UK Government (2021b)
	Deaths start	Number of weekly reported COVID-19 deaths, excluding deaths classified as COVID-19 deaths that occurred more than 28 days after positive COVID-19 test, in calendar week 38, at the start of the sample period.	UK Government (2021b)
	Cases start	Number of weekly reported COVID-19 cases in calendar week 38, at the start of the sample period, by diagnosis date.	UK Government (2021b)
BBLS	Loan amount	Total loan amount approved for each firm that participated in the BBLS.	Bank of England
	Turnover	Annual turnover of firm in 2019, as reported to lending bank to determine maximum allowed loan amount under BBLS.	Bank of England

Note: Table provides description of key variables used in analyses. *Context* column describes analytic context in which the variables appear.

Table A-3: Effect of Lockdown on Firm Vacancy Stocks: Heterogeneity (TWFE)

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dummy interactions							
Post (Tier >= 2)	-0.0168***	-0.0113***	-0.0048	-0.0012	-0.0041	-0.0053	-0.0156
	(0.0034)	(0.0033)	(0.0035)	(0.0054)	(0.0068)	(0.0036)	(0.0162)
× Cases growth (=1)	0.0097*						0.0075
	(0.0051)						(0.0123)
× Density (=1)		0.0041					0.0020
		(0.0061)					(0.0116)
× Log(1+assets) (=1)			0.0033				0.0040
			(0.0077)				(0.0091)
× Leverage / assets (=1)				-0.0029			-1.9×10^{-5}
				(0.0052)			(0.0061)
× Cash / assets (=1)					0.0022		0.0006
					(0.0059)		(0.0062)
× Credit score (=1)						0.0041	0.0018
						(0.0077)	(0.0088)
Panel B: Continuous interactions							
Post (Tier >= 2)	-0.0066	-0.0064	-0.0089	-0.0029	-0.0048	-0.0105	-0.0028
	(0.0040)	(0.0042)	(0.0066)	(0.0047)	(0.0074)	(0.0086)	(0.0130)
× Cases growth	0.0092***						0.0152**
	(0.0032)						(0.0066)
× Pop. Density		-0.0059*					-0.0222***
		(0.0031)					(0.0050)
× Log(1+assets)			0.0025				0.0014
			(0.0034)				(0.0039)
× Leverage / assets				0.0029*			0.0002
				(0.0014)			(0.0141)
× Cash / assets					0.0022		0.0014
					(0.0030)		(0.0031)
× Credit score						0.0032	0.0035
						(0.0044)	(0.0061)
Panel C: Dummy interactions							
Tier (0-4)	-0.0095***	-0.0083***	-0.0048**	-0.0019	-0.0045	-0.0049**	-0.0053
	(0.0017)	(0.0014)	(0.0023)	(0.0034)	(0.0048)	(0.0022)	(0.0100)
× Cases growth (=1)	0.0029						-0.0003
	(0.0035)						(0.0069)
× Density (=1)		0.0017					0.0009
		(0.0041)					(0.0084)
× Log(1+assets) (=1)			0.0017				0.0014
			(0.0051)				(0.0059)
× Leverage / assets (=1)				-0.0028			-0.0007
				(0.0028)			(0.0045)
× Cash / assets (=1)					0.0023		0.0008
					(0.0040)		(0.0041)
× Credit score (=1)						0.0022	-0.0004
						(0.0045)	(0.0053)
Panel D: Continuous interactions							
Tier (0-4)	-0.0064*	-0.0058**	-0.0106**	-0.0036	-0.0064	-0.0071	-0.0086
	(0.0033)	(0.0026)	(0.0041)	(0.0030)	(0.0050)	(0.0057)	(0.0098)
× Cases growth	0.0031*						0.0053*
	(0.0018)						(0.0027)
× Pop. Density		-0.0039**					-0.0162***
		(0.0018)					(0.0028)
× Log(1+assets)			0.0029				0.0050**
			(0.0022)				(0.0023)
× Leverage / assets				0.0027*			0.0116
				(0.0015)			(0.0099)
× Cash / assets					0.0037*		0.0036*
					(0.0022)		(0.0019)
× Credit score						0.0015	-0.0002
						(0.0028)	(0.0043)
Mean(exp(DV)-1)	1.7988	1.7988	1.6443	1.6221	1.7097	1.6077	1.6797
Observations	747,692	747,692	448,624	434,170	340,296	453,376	332,772
Additional controls	122	122	122	122	122	122	177

Clustered (NUTS2) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents TWFE difference-in-difference estimates studying the heterogeneous impact of the regional tier restrictions and lockdown measures put in place across the UK between September 20 and November 22, 2020 on firm-level online vacancy stocks. **Panel A** and **B** discretise the treatment to enter into effect above tier level 2, while **Panel C** and **D** estimates the treatment effect on a categorical measure of the tier levels. Each treatment indicator is interacted with continuous variables and dummies which are 1 if the continuous variable is above its median. Tier levels are: 0 – no restrictions; 1 – medium alert level; 2 – high alert level; 3 – very high alert level; 4 – full lockdown. For more detail on the tiers, see Table 1. All OLS regressions control for firm, week and week-by-SIC-2 fixed effects. Dimensions of heterogeneity, which are interacted with treatment indicator and week fixed effects, and are standardized, are: *Cases growth*: average per capita weekly growth in new COVID cases in NUTS2 area in weeks before treatment; *Density*: number of inhabitants per 1,000 km^2; *Log(1+assets)*: log of total assets (th. GBP) of firm; *Leverage / assets*: ratio of current liabilities to total assets of firm; *Cash / assets*: ratio of bank and deposits to total assets of firm; *Credit score*: annual probability of firm failure, based on credit rating.

Table A-4: Impact of EOHO Local Demand Shocks on Firm Vacancy Stocks: Heterogeneity when using claimed meals as EOHO exposure measure.

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)
Panel A: interactions: dummy variables					
Post × EOHO meals	0.0338***	0.0114	0.0230	0.0091	0.0225
	(0.0118)	(0.0127)	(0.0147)	(0.0071)	(0.0184)
× Leverage / assets (=1)	-0.0241*				-0.0220
	(0.0136)				(0.0149)
× Log(1+assets) (=1)		0.0021			0.0025
		(0.0147)			(0.0158)
× Cash / assets (=1)			-0.0078		-0.0061
			(0.0157)		(0.0166)
× Credit score (=1)				0.0151	0.0165
				(0.0116)	(0.0117)
Mean(exp(DV)-1)	6.2746	6.2746	6.2746	6.2031	6.3059
Panel B: interactions: continuous variables					
Post × EOHO meals	0.0312***	0.0190**	0.0170	-0.0481*	-0.0326
	(0.0086)	(0.0077)	(0.0122)	(0.0270)	(0.0319)
× Leverage / assets	-0.0174***				-0.0127**
	(0.0056)				(0.0055)
× Log(1+assets)		-0.0239			-0.0298
		(0.0358)			(0.0355)
× Cash / assets			-7.02×10^{-5}		-0.0035
			(0.0083)		(0.0079)
× Credit score				0.0222**	0.0229**
				(0.0090)	(0.0094)
Mean(exp(DV)-1)	6.2746	6.2746	6.2746	6.2031	6.3059
Observations	67,015	67,015	67,015	67,951	66,573
Additional controls	4,119	4,119	4,119	4,119	4,158
Clusters	316	316	316	316	316
Area by Week FE:	LAD	LAD	LAD	LAD	LAD

Clustered (LAD) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference regression estimates studying the heterogeneous impact of the EOHO scheme on the MSOA-level online vacancy stocks of local firms with one single trading address on Indeed, across the 13 calendar weeks from 24 to 36. *Log(1+assets)*: log of average total assets (th. GBP) of all firms with open vacancies in MSOA on given week; *Leverage / assets*: employment-weighted average of ratio of current liabilities to total assets of all firms with open vacancies in MSOA on given week; *Cash / assets*: employment-weighted average of ratio of bank and deposits to total assets of all firms with open vacancies in MSOA on given week. Dummy variables are equal to 1 (=1) if an observation is above the median for the corresponding variable.

Table A-5: Impact of EOHO Local Demand Shocks on Online Vacancies: Robustness

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: EOHO exposure: meals									
Post × EOHO covered meals	0.0316***	0.0327***	0.0327***	0.0177***	0.0174***	0.0178***	0.0178***	0.0130*	0.0112
	(0.0066)	(0.0071)	(0.0070)	(0.0064)	(0.0065)	(0.0065)	(0.0066)	(0.0067)	(0.0070)
Panel B: EOHO exposure: restaurants									
Post × EOHO restaurants	0.0337***	0.0337***	0.0337***	0.0196***	0.0193***	0.0198***	0.0197***	0.0151**	0.0135*
	(0.0086)	(0.0095)	(0.0094)	(0.0072)	(0.0073)	(0.0073)	(0.0074)	(0.0075)	(0.0076)
Panel C: EOHO exposure: doubly-robust									
Post × EOHO restaurants	0.054***	0.0527***	0.053***	0.0556*	0.0553*	0.0624**	0.06*	0.0473	
	(0.0138)	(0.0146)	(0.0147)	(0.0307)	(0.0325)	(0.0295)	(0.0336)	(0.033)	
Mean(exp(DV)-1)	4.8426	4.8426	4.8426	6.3127	6.3127	6.3127	6.3456	6.3456	
P-val par. trends	0.0000	0.0000	0.0000	0.0642	0.0639	0.0881	0.1241	0.1427	
	6.3127	6.3456	6.3456	6.3456					
Observations	88,283	88,283	88,283	66,599	66,599	66,599	66,144	66,144	66,144
Additional controls	4,119	4,131	4,143	4,142	4,154	4,166	4,178	4,190	4,214
Clusters	317	317	317	316	316	316	316	316	316
Area by Week FE:	LAD	LAD	LAD	LAD	LAD	LAD	LAD	LAD	LAD
Week *times* **additional control:**									
Population density		X	X	X	X	X	X	X	X
Spring 2020 COVID-19 exposure			X	X	X	X	X	X	X
Log(1+assets)				X	X	X	X	X	X
Leverage / assets					X	X	X	X	X
Cash / assets						X	X	X	X
Credit score							X	X	X
Student exposure								X	X
Tenure types									X

*Clustered (LAD) standard-errors in parentheses; Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Table presents difference-in-difference estimates studying the impact of the EOHO scheme on the MSOA-level online vacancy stocks of local firms with one single trading address on Indeed, across the 13 calendar weeks from 24 to 36. **Panel A and B** present estimates from two-way fixed effects regressions for estimated number of meals claimed and estimated number of participating restaurants, controlling for area-by-week fixed effects. **Panel C** presents estimates obtained using the estimator from (Callaway and Sant'Anna, 2020). For both estimators, we gradually introduce additional covariate-specific time trends. *Population density*: number of people per sq. km in MSOA; *Spring 2020 COVID-19 exposure*: total number of COVID-19 deaths in MSOA between March and July 2020; *Log(1+assets)*: log of average total assets (th. GBP) of all firms with open vacancies in MSOA on given week; *Leverage / assets*: employment-weighted average of ratio of current liabilities to total assets of all firms with open vacancies in MSOA on given week; *Cash / assets*: employment-weighted average of ratio of bank and deposits to total assets of all firms with open vacancies in MSOA on given week; *Student exposure*: number of full-time students resident in MSOA (2011 Census); *Tenure types*: share of households living in rented or owned accommodation. Regressors are normalized to have standard deviation equal to 1 for ease of interpretation.

Table A-6: Impact of EOHO Local Demand Shocks on Online Vacancies: No Hospitality Sector

DV: Log(1+Vacancy Stock):	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: EOHO exposure in levels						
Post × meals	0.0232***	0.0240***	0.0247***			
	(0.0055)	(0.0058)	(0.0060)			
Post × restaurants				0.0262***	0.0264***	0.0272***
				(0.0071)	(0.0074)	(0.0081)
Panel B: EOHO exposure in log						
Post × Log(1+meals)	0.0322***	0.0323***	0.0327***			
	(0.0049)	(0.0052)	(0.0053)			
Post × Log(1+restaurants)				0.0434***	0.0443***	0.0453***
				(0.0054)	(0.0057)	(0.0060)
Panel C: EOHO exposure per capita in log						
Post × Log(1+meals per capita)	0.0321***	0.0324***	0.0330***			
	(0.0050)	(0.0053)	(0.0054)			
Post × Log(1+ restaurants per capita)				0.0376***	0.0384***	0.0400***
				(0.0056)	(0.0059)	(0.0062)
Mean(exp(DV)-1)	4.5809	4.5809	4.5809	4.5809	4.5809	4.5809
Observations	88,283	88,283	88,283	88,283	88,283	88,283
MSOA	6,791	6,791	6,791	6,791	6,791	6,791
Additional controls	388	1,207	4,119	388	1,207	4,119
Clusters	317	317	317	317	317	317
Area by Week FE	NUTS2	NUTS3	LAD	NUTS2	NUTS3	LAD

Clustered (LAD) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Table presents difference-in-difference estimates studying the impact of the EOHO scheme on the MSOA-level online vacancy stocks of local firms with one single trading address on Indeed, excluding firms in the food services and accommodation industry (I), across the 13 calendar weeks from 24 to 36. All regressions also control for area by week fixed effects. Regressors are normalized to have standard deviation equal to 1 for ease of interpretation.