New Business during COVI-19

SUMMARY

Using UK data, we present greater empirical detail on the puzzling firm dynamics that emerged during coronavirus disease 2019 (COVID-19). We show that firm entry increased during the pandemic across several countries, and this contrasts with typical recessions where firm entry declines. Additionally, the rise in firm entry is driven by individual entrepreneurs creating companies for the first time, particularly in online retail. We find evidence that firm creation responded significantly to declines in retail footfall and that firms created during the pandemic are more likely to exit and less likely to post jobs. Overall, this implies that despite surging firm creation during the pandemic, the overall employment effect is limited. Finally, we find that the primary contributor to limited employment creation is the shift in ownership composition of new entrants during COVID.

JEL codes: E32, L25, L26

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Business creation during COVID-19

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1. INTRODUCTION

Firm entry is a fundamental indicator of economic activity. New firms contribute to aggregate job creation and affect both growth and productivity through competition, innovation and reallocation. Typically, firm entry is procyclical: it rises in booms and declines in recessions. However, during the coronavirus disease 2019 (COVID-19) recession, entry was countercyclical, rising as output fell. Motivated by this puzzling observation, we analyse the dynamics behind firm creation in the UK during the COVID-19

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pandemic. We investigate the drivers of firm creation and ask whether these new firms affect the real economy.

Our analysis leads to five facts on firm creation during the COVID-19 pandemic:

1. Firm entry increased during the COVID-19 pandemic. This contrasts with past recessions in the UK but is similar to other economies during COVID-19.
2. New firms are disproportionately concentrated in the online retail sector and founded by individuals who started their first business.
3. Firm entry is negatively correlated with retail footfall during the pandemic.
4. New firms are less likely to post jobs than firms created pre-COVID.
5. New firms are more likely to exit (dissolve) than firms created pre-COVID.

Our results highlight the rapid self-correcting mechanism of the economy during COVID-19. This complements our growing understanding of how targeted policy interventions affected firms in terms of survival, growth and employment (Van Dijcke *et al.*, 2022; González-Pampillón *et al.*, 2021). There were no *direct* policies targeted at new firm creation, and policies such as furlough (Coronavirus Job Retention Scheme), Eat-Out-to-Help-Out and the Bounce Back Loan Scheme, all required firms to have been in existence prior to the crisis. Despite this, we observe a quick reaction by entrepreneurs in the economy responding by creating firms in lockdown-compliant sectors such as online retail.

However, the promising evidence of burgeoning firm creation during COVID-19 is mitigated by the characteristics of these firms. We find that cohorts of firms created during the pandemic are more likely to dissolve and less likely to post jobs. Additionally, descriptive statistics suggest that, conditional on employing, firms born during the pandemic are smaller in size.

Taking these four margins – registrations, survival, posting rate and firm size – together, we perform approximate calculations for the overall employment effect of the boom in firm creation during the pandemic. Our results show that the positive margin of greater firm creation, which ceteris paribus would raise employment, is more than offset, by weaker survival rates, weaker conversion of surviving firms to employers (posting), and weaker employment growth. The end result is that employment from firm creation during COVID-19 is worse than employment from firm creation over an equivalent duration in normal times, even though there is more firm creation during COVID-19.

We extend the aggregate analysis to understand the effect of changing industry and ownership composition. Our main result is not driven by the industrial composition of the entrants. Instead, we find that an important determinant of the limited employment potential of firms created during COVID-19 is ownership structure. This finding is consistent with our evidence that firms created during COVID-19 are disproportionately setup by individuals with no prior experience in firm ownership, the so-called new solo entrepreneurs. Firms setup by these sorts of entrepreneurs are both less likely to attempt to hire workers and more likely to dissolve. Additionally, we find new firms owned by existing corporations, an important source of employment among entrants, created fewer jobs on average with no offsetting boom in entry. This phenomenon further contributed to the limited employment impact of new entrants. Overall, we show that the change in ex-ante characteristics of firms created during COVID is part of the explanation for why firms had weaker employment potential.

To show our results, we use data from the UK's register of limited firms from Companies House, Bureau Van Dijk (BvD) data on firms' ownership structure, Indeed data on job postings and Google data on footfall. Companies House data provide the registration date, dissolution date and sector of activity of new company registrations. We refer to company registrations as firm entry and business creation throughout the paper. Companies House data are accessible directly or via BvD-Financial Analysis Made Easy (FAME), which also adds ownership information. We can merge all of these data at a granular level, enabling us to estimate the response of entry to footfall and to track whether the newly created companies post jobs or dissolve. We use local projections to show how firm entry responds to a negative footfall shock for several periods after the shock. To study the probability of posting a vacancy and the probability of dissolving for pre-COVID and COVID cohorts of firms, we classify firms by the quarter they are created and study each cohort's probability of posting a job or dissolving as they age. We first present simple cumulative shares of firms posting and dissolving by age for pre-COVID and COVID cohorts, and then we conduct a more detailed fixed effects analysis. The fixed effects analysis purges other aggregate time and sector composition effects that influence job postings and dissolutions during the pandemic. Our final methodological step is to present a statistical framework to quantify the employment effects of these various characteristics of firm dynamics during the crisis. The statistical framework clarifies how some of these channels enhance aggregate employment, whereas others weaken employment.

Related Literature: The resilience of firm entry during COVID-19 has been noted for a number of economies. Dinlersoz *et al.* (2021) present evidence for the United States, and OECD (2021) provide evidence for Organisation for Economic Co-operation and Development (OECD) countries. Duncan *et al.* (2021) present early evidence of the aggregate and sectoral effects of the COVID-19 crisis on UK firm creation. Additionally, our finding that entry is concentrated in the online retail sector is consistent with most US registrations being in non-store retail (Haltiwanger, 2021; Haltiwanger and Decker, 2023). However, existing literature has not studied the real impact of these new entrants on aggregate employment. Benedetti-Fasil *et al.* (2022) show that the initial sharp falls in firm creation in France, Germany, Italy and Spain could have persistent negative effects on employment due to fewer high-growth start-ups. Our results focus on the surge in firm entry following this initial decline and suggest that these new firms are weaker in their ability to buoy employment.

Our findings have implications beyond the context of COVID-19. Recent business cycle theory emphasizes the role of sector-specific shocks which induce structural reallocation (Guerrieri *et al.*, 2022; Baqaee and Rubbo, 2023). Yet, an open question is how much reallocation there is in practice and through which channels reallocation happen. Our paper quantifies the amount of structural reallocation through the firm creation channel. This is one of three prominent channels for structural reallocation. Other channels are incumbent firms expanding production and the re-direction of production to growing sectors. Despite the rapid response of entry, our findings cast doubts on whether large numbers of new firms will always support a reallocation of employment.

Additionally, we add to growing evidence on the importance of firm characteristics at start-up for future employment (Sedláček and Sterk, 2017; Sterk *et al.*, 2021). We stress that the composition of ownership structure responds to the recession and that this is an important dimension for understanding the employment impact of entry. This adds a business cycle perspective to research which shows that ownership structure at start-up affects subsequent firm performance (Felix *et al.*, 2021).

We also contribute to research on entrepreneurship by necessity versus opportunity (Fairlie and Fossen, 2019). While we cannot track the employment situation of new entrepreneurs in our data, our results do suggest that new businesses were created out of opportunity more than by necessity for three reasons. First, our data are on company incorporations, not self-employment[1] and the process of incorporating a company implies a well-defined business entity, rather than more informal self-employment.[2] Second, we show that company incorporations respond to demand shocks in specific COVID-compliant sectors, suggesting a response to opportunities. Third, the UK provided generous employment support during the pandemic to both employed and self-employed individuals preventing a rise in unemployment and a consequent shift into entrepreneurship by necessity. Typically, necessity entrepreneurs enter entrepreneurship from unemployment and create small, low-growth, businesses relative to opportunity entrepreneurs (Poschke, 2013). Our findings suggest that even opportunity entrepreneurs can create small and low-growth businesses, so challenging the idea that the motivation for entrepreneurship, opportunity versus necessity, is a key predictor of future performance.

The remainder of the paper is structured as follows: Section 2 describes our data; Section 3 presents our five facts (Sections 3.1, 3.2, 3.3, 3.4, 3.5) on firm creation during the COVID-19 pandemic. Section 4 discusses our results and presents a statistical framework to analyse the effect of our facts on employment. Section 5 concludes.

2. DATA

We use data from Companies House and BvD (FAME) to construct daily entry, dissolution and ownership statistics. We supplement these high-frequency statistics with Office

1 In the data section (Section 2.1.1), we discuss the differences between these two legal forms.

2 For the same reason, OECD (2021) interpret their analysis of company incorporations as opportunity entrepreneurship.

for National Statistics (ONS) summary statistics derived from the, confidential, Interdepartmental Business Register (IDBR). We use data from Indeed to measure job postings. Last, we use Google mobility data to measure retail footfall.

2.1. Business registrations (Companies House and FAME)

We use the FAME dataset provided by BvD. The dataset keeps track of historical Companies House data in an accessible manner. We use this historical data to build a series of daily firm entries since 2005 (see Supplementary Appendix A1 for a time series of entries). Companies House is the name of the government department which maintains the business register.

The Companies House register records all *incorporated companies* in the United Kingdom. Incorporated companies are separate legal entities to the business owner.[3] We restrict the firms' legal status to private limited companies which represent 91% of all companies on the register. The remaining 9% are public companies and non-profit organizations.

To register a company with Companies House, the business owner completes an online form which costs £12. The registration is processed within 24 h meaning that it appears on the publicly available database within this time.[4] Updates to the Companies House register are automated following a business application. Consequently, there are no administrative lags due to the pandemic. There is a short lag between a company appearing on the public register and BvD recording it in their proprietary dataset (FAME).

The process of dissolving a company is less automated than registration. A company owner can complete a form for removal or Companies House administrators can remove companies that have not returned mandatory forms for a period of time. Consequently, dissolutions often occur at specific points in a firm's lifecycle. For example, in our analysis, we will see dissolutions rising sharply in quarters 6 and 8 after birth. This corresponds to months 18–21 when new firms must submit their first set of accounts.[5] Our analysis does not rely on random firm dissolution over the lifecycle. The main effect of this feature of the data is to weaken the role of dissolutions at shorter time horizons, as few firms dissolve within a year, and strengthen effects at longer time horizons once account submission deadlines are reached.

3 Unincorporated companies are not on the register. They are not legal entities (the owner is fully liable for debts of the business). The most important constituent of unincorporated companies is sole proprietors. In 2021, 56% of all UK businesses were sole proprietors, 37% were companies and 7% were ordinary partnerships (Department for Business, Energy & Industrial Strategy, 2021).

4 Further details on the registration process are here: https://www.gov.uk/limited-company-formation/register-your-company.

5 The relevant documentation from Companies House on account submission deadlines is available at https://www.gov.uk/government/publications/life-of-a-company-annual-requirements/life-of-a-company-part-1-accounts.

When registering, each firm is provided with a unique company number, a registration date, and has to indicate a legal status, a headquarters address and an industry code (4 or 5-digit SIC). BvD reports all of this information, as well as the dissolution date when applicable. This allows us to measure daily number of incorporations and dissolutions by local authorities and industrial sector.[6]

2.1.1. Company incorporation versus self-employment. It is helpful to note the differences between registering an incorporated company versus working as a self-employed individual in the United Kingdom. Although self-employed individuals and incorporated companies represent roughly half of the UK business population each, incorporated companies are much larger when employment or revenues are taken into consideration (Department for Business, Energy & Industrial Strategy, 2021).

The decision to pursue self-employment or incorporation often hinges on personal tax considerations, the need to hire employees, and the potential for borrowing capital. Self-employed individuals face less administrative work compared to incorporated companies. Self-employment requires only an annual personal tax return, while incorporated companies must submit annual company accounts. Additionally, incorporation requires a business bank account as well as various company details, including a set of directors and a business address which are available on the Companies House public register. Incorporated company status is necessary for some external financing.[7]

Incorporated companies represent more formal business entities characterized by well-defined purposes, higher revenue, and the potential for employment. Self-employed businesses are more likely to involve informal work, variable occupations, and a reduced need to employ or to borrow. Informal rules-of-thumb suggest transitioning from self-employment to company status occurs when revenues exceed £50,000.

During the pandemic, the UK offered generous employment support through the Self-Employment Income Support Scheme (SEISS) and the Coronavirus Job Retention Scheme (CJRS). This limited the number of workers who become unemployed but may have increased the number of self-employed individuals switching to *employee* status in order to qualify for this 'furlough scheme', see Blundell and Machin (2020). However, these transitions would not affect our firm creation statistics. Also, government support schemes could only be claimed by firms that existed prior to the pandemic, limiting the likeliness of self-employed to become corporations in order to benefit from these support schemes.

2.1.2. Ownership information. The BvD-FAME dataset reports ownership information, which is acquired from Companies House. Firms registering with Companies

6 We follow the Office of National Statistics and exclude postcodes with more than 500 incorporations in a single day. Multiple incorporations at a single postcode most often reflect registrations by management and personal service companies or are tax motivated, with little economic impact. See Office for National Statistics (ONS) (2020) article for more details.

7 Incorporating a company also incurs a £12 fee.

House usually have to provide information on their ownership, including the name of firm shareholders and the size of their stakes. BvD processes this textual information and provides unique identifiers for shareholders, together with their stake and their type (individual, corporate, unnamed or other). There is a two-month lag for the ownership information to appear in FAME and laws ensure that business ownership information in Companies House is updated regularly. The FAME dataset maintains a historical record of owners. We use this historical data and BvD shareholder identifiers to identify *'new'* and *'serial'* entrepreneurs. We define these types as follows:

- *New entrepreneur:* Has not owned a business in the 5 years prior to the pandemic. Specifically, has not owned a firm registered since January 2016, even one that was subsequently dissolved, or is not owning another live firm in January 2020 created prior to January 2016.
- *Serial entrepreneur:* Has owned at least one business in the 5 years prior to the pandemic. Specifically, the owner founded a firm since January 2016, which may have subsequently dissolved, or, owned a firm that was live in January 2020 but that was born prior to January 2016.[8]

Combining the directly available ownership information (individual, group, corporate) with our constructed new/serial information, we classify new firms as:

- *Solo entrepreneur (new)*: one owner who has not owned another firm in the last 5 years.
- *Solo entrepreneur (serial)*: one owner who has owned at least one other firm in the last 5 years.
- *Group of entrepreneurs (new):* all shareholders are new entrepreneurs.
- *Group of entrepreneurs (serial):* at least one shareholder is a serial entrepreneur.
- *Corporate:* at least one shareholder is a corporation.

2.2. Firm employment (FAME and IDBR)

Due to accounting exemptions and lags, employment information for new firms in Companies House (BvD-FAME) is sparse.[9] We use the data to study the change in firm size for firms born pre-COVID and COVID cohorts but highlight the uncertainty around them provided the sample is small.

8 Of the 7 million individual shareholders in our data, 15% are 'serial' entrepreneurs. Of the 5 million firms founded by individuals between January 2016 and December 2021, 40% of them have at least one serial entrepreneur; and one-third of firms founded by solo entrepreneurs over the same period are founded by serial solo entrepreneurs.

9 There were 1.4 million firms registered between March 2020 and September 2021, but 1.7 thousand have filed employment information by the end of 2021.

We complement this information with aggregate data released by the ONS which is derived from the Government's, confidential, business register known as the Inter-departmental Business Register (IDBR).[10] Firms are added to the IDBR if they employ someone (register for payroll tax, PAYE) or register for VAT tax. The data provide quarterly number of additions to the IDBR in total and the number of employees hired by these new firms, it also provides the annual count of new employer–firms.[11] In 2019, 90% of firms on the IDBR were employer–firms and just over 90% of additions were accounted for by new employers. Additions to the IDBR are closely correlated with Companies House (CH) registrations but with a lag of four quarters (see Supplementary Figure S2).

We use these data in two ways: to approximate the company registration to employee–firm conversion rate and to approximate firm size. To approximate the fraction of Companies House registrations that become employer–firms, we take the following ratio:

$$\frac{\text{ONS IDBR employer-firm births in year } a}{\text{Number of company registrations in year } a{-}1}$$

The ratio represents the company registration to employee–firm conversion rate. It would be one if all company registrations became employers, but in practice, it is less than one. In 2019, this ratio suggests that 54% of firm registrations in Companies House in 2018 become employer–firms in the IDBR in 2019 (assuming they do so within a year); in contrast, this ratio is only 40% in 2021, that is 40% of firm registered in 2020 become employer–firms within a year (in 2021). We use the ratio of the annual number of employees hired by new firms to the annual count of new employer–firms to get annual data on average firm size.

This dataset is not used for our five facts but is used to calibrate our statistical framework in Section 4.

2.3. Job postings (Indeed)

To study the employment behaviour of firms in more detail, we use data from Indeed. The data from Indeed enable us to track job postings at the firm level, and to know the length of days between when the firm was incorporated and when it started posting on Indeed. This way, we can investigate whether the probability of becoming an employer–firm has changed for cohorts of firms born during the pandemic relative to

10 We use both the detailed annual updates of business demography (ONS 'Business demography') and the quarterly updates (ONS 'Business demography, quarterly experimental statistics'). Supplementary Appendix compares Companies House registrations to ONS firm entry from the IDBR.

11 There are short lags from registering with the tax authority to being added to the IDBR. Our data track the date of addition to the IDBR (Office for National Statistics (ONS) 2022b).

cohorts of firms born pre-pandemic, and also look at how this probability changes as firms age. We interpret job postings as vacancies. Technically, job postings differ from job vacancies as a firm can post a single posting for multiple vacancies. However, it proxies a firm's intention to become an employer–firm and the aggregate job posting time series correlates well with official vacancy survey data.

The Indeed data include both jobs posted directly on Indeed and on companies' websites scraped by Indeed.[12] Each record consists of a company name, job title and posting date. We match the Indeed postings with Companies House data using the company name variable.[13] We drop a match if the company job posting pre-dates its matched incorporation date in Companies House.

The data cover 30 million job postings that we match to 450,000 unique firms between January 2018 and September 2022 (57 months). Of the unique firms, 30,000 (7%) were incorporated in the 18-month COVID period (between March 2020 and September 2021).[14] There were 1.5 m incorporations over the COVID period; hence, 2% of firms incorporated over COVID also posted jobs in Indeed.

Indeed is major recruitment platform, but it does not have universal coverage of UK job postings. In January 2020, the number of firms posting in Indeed was 13% of the number of employer–firms in the IDBR. Hence, the proportion of firms posting in Indeed should not be used to extrapolate to the overall level of hiring. However, we consider the Indeed data as a representative sample of job vacancies in the United Kingdom. Supplementary Appendix A5 shows there is a close correlation with official ONS vacancy data from surveys and Van Dijcke, Buckmann, Turrell, and Key 2022 show that the industrial mix in Indeed is similar to the wider UK economy. Moreover, the vacancy data from Indeed are timely, not survey based, and can be matched to firm creation information to track cohorts through time.

2.4. Retail footfall (Google Mobility)

We use retail footfall figures from Google LLC (2021) to assess the impact of changes in mobility on firm entry. Specifically, we use mobility trends for 'retail and recreation' which represent places such as restaurants, cafés, shopping centres, theme parks, museums, libraries and cinemas. We refer to 'visits to retail and recreation' as *footfall*. For a placebo exercise, we also use mobility trends for 'parks' which represents places like local parks, national parks, public beaches, marinas, dog parks, plazas and public gardens. In both cases, the data show how visitors to these locations changed compared to a

12 See https://www.hiringlab.org/indeed-data-faq/ for a description of the data.

13 Similar to Van Dijcke *et al.* (2022), we match unique names based on the cosine similarity of 3 n-grams, using the Python string-grouper package. After an initial clean of names excluding all special characters and common words (such as LLC and LTD), we keep matches with a similarity score over 80%.

14 This figure is less than we would expect given COVID months represent one-third of total months. However, companies incorporated more recently are less likely to have posted.

baseline. The baseline is the median value from the 5-week period from 3 January–6 February 2020 for a specific day of the week. For example, a value of −10% on a Monday in June 2020 would represent 10% fewer visits than the median value for Mondays over the baseline period, on average across all of the United Kingdom. The series begins on 15 February 2020 and ends on 15 October 2022.

3. FACTS

3.1. Entry cyclicality

Figure 1 plots firm registrations in the UK over 120 years.[15] Entry of new employers (employer–firms) is typically procylical: it rises in booms and declines in recessions (Lee and Mukoyama, 2015; Tian, 2018). However, our evidence for firm registrations in the UK (Figure 1) shows that in 'extreme event' recessions, such as wartime and the COVID-19 pandemic, firm entry (registrations) is countercyclical.[16] These 'extreme event' recessions share the feature that the economy restructures to substantial shifts in the patterns of consumer demand and producer supply. The post-war recessions in 1919 and 1946 saw an entry boom as wartime production declined and private enterprise restarted. Similarly, during the COVID-19 pandemic widespread lockdowns reallocated demand to sectors that complied with social distancing.

Figure 2 focuses on the comparison between the Global Financial Crisis (GFC) and COVID-19 recessions but with a cross-country perspective. We extend the evidence of Dinlersoz *et al.* (2021) for the United States (US Census) to France (INSEE) and the United Kingdom (Companies House), and we follow their methodology.[17] The interpretation is growth rates of cumulative registrations during the crisis period relative to the reference period. While the COVID period shows a clear decline and sharp

15 Companies House directly provides the number of registrations back to 1939. To calculate registrations for early periods, we use firms' Companies House numbers. Private limited firms have been numbered sequentially since the first Companies Act in the 1800s. Hence, the difference in the company number between the first firm incorporated in a given year and first firm incorporated in the next year corresponds to new registrations in the year (Scotland has a different set of numbers to England and Wales but they are still sequential). We do not observe the incorporation date of all firms that have ever entered the register. However, we do observe a sample of incorporation dates of old firms in our BvD-FAME data. We can use the company numbers of the first firm founded in a year that we do see to construct an approximate entry rate instead. As the first firm recorded as being incorporated in BvD-FAME in a given year, back to 1900, is still founded on 1 January the approximation error from doing this is not large.

16 Company registrations comove with employer–firm creation, although not all business registrations lead to new employers. Importantly, our measure of business registrations in the UK does not include self-employed individuals. Self-employment is often countercyclical (Fossen 2021).

17 This takes the ratio of cumulative firm creation beginning in 2008m3 for the GFC and 2019m9 for COVID, relative to cumulative firm creation beginning in 2006m3 and 2017m9 for the reference period. It then rescales these ratios as percent deviation from the crisis start, 2008m9 and 2020m3, respectively.

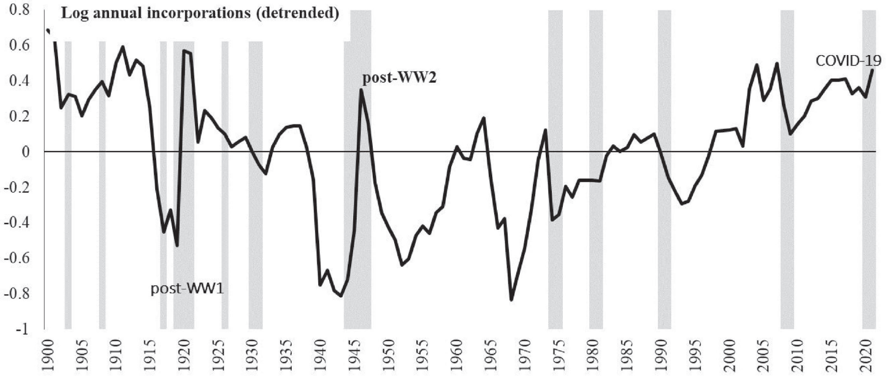

Figure 1. Business creation in the UK, 1900–2020

Notes: Logarithm of annual new business registrations, linearly detrended. Shaded areas correspond to years when UK GDP growth was negative.

Source: Historical companies house register and BvD-FAME for firm entry, BoE: A millennium of macroeconomic data for GDP.

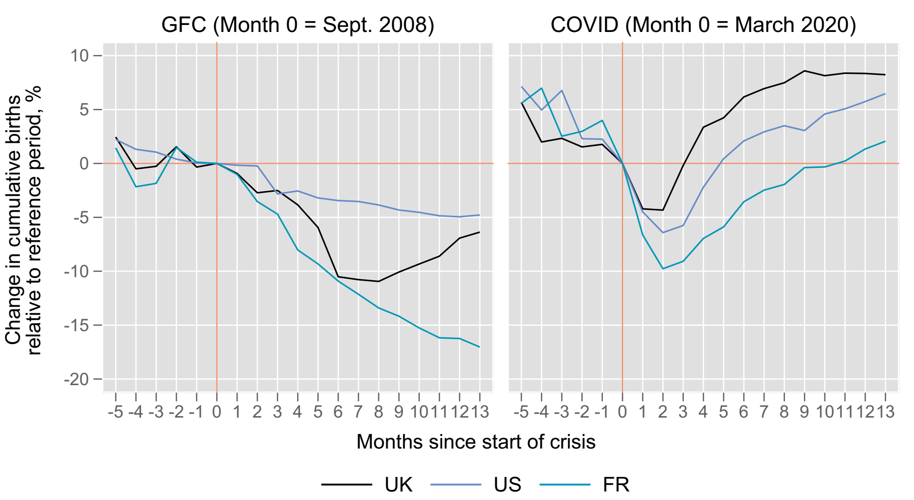

Figure 2. Cumulative business registrations, GFC versus COVID, for United Kingdom, United States and France

Notes: Registrations of corporations or equivalent. Reference period: similar month of 2018 for COVID, 2006 for the GFC.

Source: Authors' calculations using Companies House, BvD-FAME, US Census and INSEE.

recovery across all countries, the GFC exhibited a protracted decline in entry growth rates from the onset of the crisis.

Since the pandemic, several papers have documented other cross-country evidence on firm creation. Different economic policies and different data measurements of firm creation across countries (e.g. whether it includes self-employment or informal work) are likely to explain differences across countries. A detailed analysis is beyond our scope, but it is helpful to reflect on several studies compared to our UK evidence. Importantly our evidence is for incorporated companies, and the UK Government offers among the most generous employment support worldwide.

OECD (2021) documents that countries such as Australia, Belgium, France, Norway, Singapore, the United Kingdom and the United States experienced V-shaped recoveries in firm creation, noting strong firm creation throughout 2021. Other countries, such as Hungary, Italy, Portugal and Spain, experienced U-shaped recoveries with milder firm creation into 2021. Consistent with the analysis in this paper, the OECD evidence is for company incorporations. As described in Section 1, that the literature typically considers entrepreneurship by necessity to lead to rises in self-employment rather than incorporations. This suggests that the patterns seen reflect changes in opportunity entrepreneurship over necessity entrepreneurship.

Recent work on the United States by Haltiwanger and Decker (2023) finds resilience in firm creation since COVID-19 that has persisted into 2023. They verify their results against other well-established US data sources on firm creation, and they find evidence for a rise in employer businesses and a break in trend. A difference with our work is that the core data – Business Formation Statistics (BFS) – may include self-employed individuals since there are reasons for them to seek Employer Identification Numbers (EINs). In a discussion of this work, Pugsley (2023) analyses self-employment data from the Current Population Survey (CPS), also covered in Fairlie (2020), which surges during the pandemic. He hypothesises that the surge is due to replacement (exit of firms) and incumbents shrinking, such that aggregate labour goes towards start-ups in the form of self-employment or small businesses, the latter of which is more consistent with the UK experience. For Spain, Albert *et al.* (2023) show that there was an overall increase in necessity entrepreneurship, but opportunity entrepreneurship accounts for relatively higher firm creation among wealthier groups since they faced fewer financial constraints. Given generous employment support in the United Kingdom, our evidence is consistent with the limited financial constraints hypothesis and the stark difference we show between the pandemic recession and the Global Financial Crisis (GFC) in Figure 2. In early work, Galanakis *et al.* (2023) find positive correlations between furlough intensity and business creation. Furlough gave workers a guaranteed income, limiting their financial constraints.

3.2. Entry composition

We then focus on the UK experience and describe in more detail the rise in firm entry during COVID. Figure 3 shows the total cumulative increase in firm creation over the COVID-19 pandemic (2020Q2–2021Q3) relative to pre-pandemic (2018Q2–2019Q3) and its decomposition by sector. We can see that 139,863 more firms were created

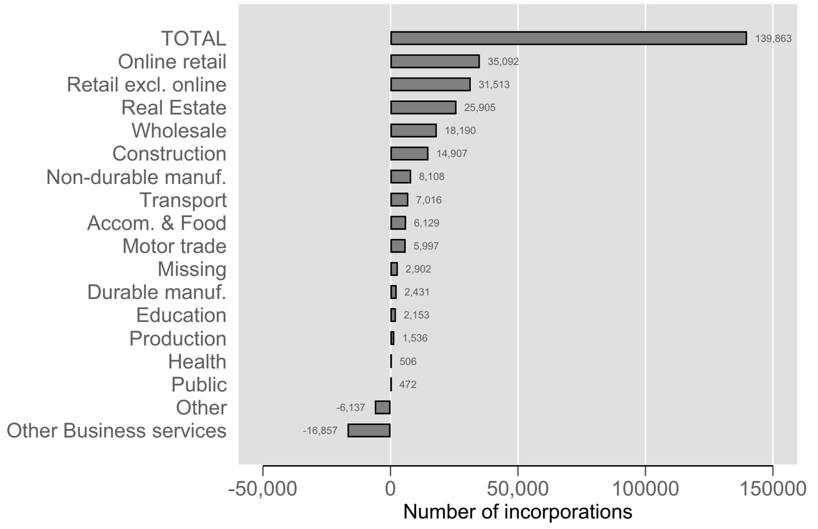

Figure 3. Change in number of cumulative incorporations, during COVID (2020Q2–2021Q3) versus pre-COVID (2018Q2–2019Q3) and sector contributions

Source: Authors' calculations using BvD-FAME.

during the pandemic relative to pre-pandemic, with the online retail sector (SIC 4-digit sector 'Retail sale via mail order houses or via internet') being the largest contributor to this increase with 35,092 more firms.[18] For reference, firms in online retail accounted for less than 2% of firms in January 2020 (Supplementary Appendix A2). Despite the sector's modest size in aggregate figures, the contribution of 35,092 extra firms in online retail represents one-fourth of the increase of 139,863 in total entry. In other words, online retail makes up 25% of excess entry.

Figure 4 shows monthly firm creation in the aggregate economy and for the online retail sector. Additionally, we decompose firm creation by ownership structure to explore ex-ante sources of heterogeneity, which may affect subsequent behaviour such as survival and job posting. If existing firms quickly adjusted to setup online retail subsidiaries or benefit indirectly from business support packages we would expect to see an increase in entry from firms that are part of existing groups and therefore have a corporate shareholder. We do not find evidence for this. In fact, our evidence shows that new solo entrepreneurs play a disproportionate role, particularly in the online retail sector. Notably, there is no increase in the share of corporate-owned firms which might occur if existing firms responded quickly to new opportunities created by the pandemic. This may be because the shifts in demand patterns generated by the pandemic created

18 One caveat with our results for the UK is that the pandemic is contemporaneous with the United Kingdom's exit from the European Union in January 2020, which entered fully into force in January 2021. This adds some uncertainty to our results but the fact that the changes in entrepreneurship align so closely with events during the pandemic lends some confidence to our analysis.

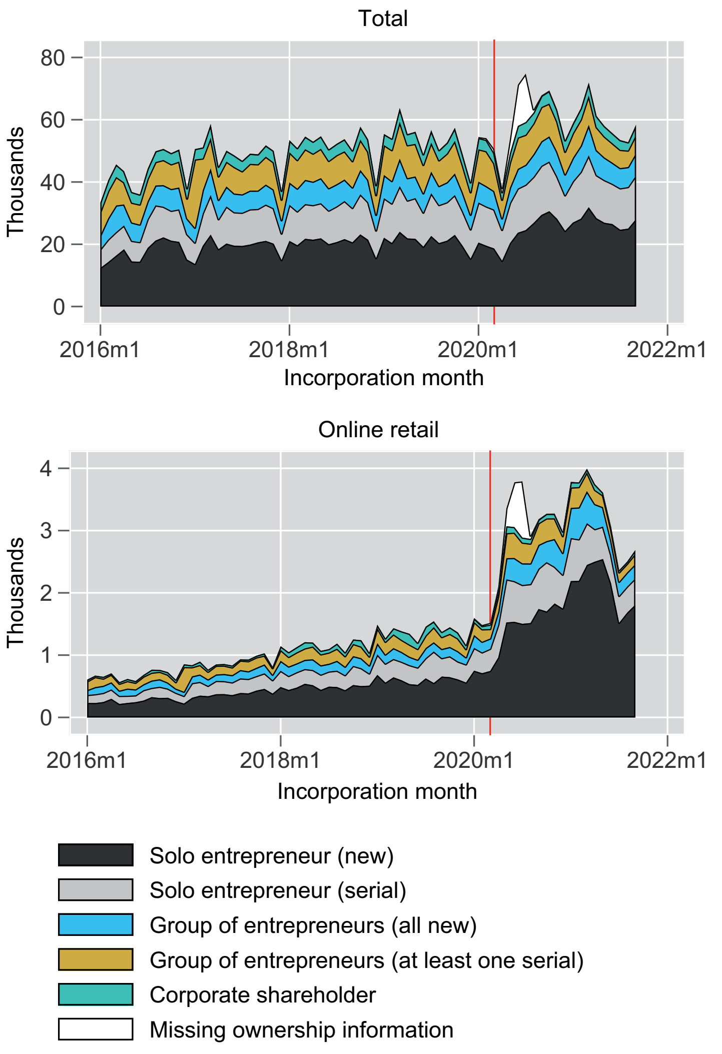

Figure 4. Monthly firm creation by type of ownership, total economy and online retail sector, January 2016 to September 2021

Notes: New solo entrepreneurs are individuals who started their first business during the pandemic. Serial entrepreneurs are individuals who owned at least one live business in January 2020 or had started another business in the 5 years prior to the pandemic. See Section 2.1 for a detailed description of ownership categories. The vertical line denotes March 2020, the start of the pandemic.

Source: Authors' calculations using BvD-FAME.

opportunities for these businesses founded by new entrepreneurs. Alternatively, this finding may represent a shift in the supply of nascent entrepreneurs due to a reduction in formal employment or an increase in available time due to the government furlough scheme and shorter travel times.

The full sample shows a sharp decline and rapid rise in firm entry after the introduction of the first national lockdown in March 2020. Before the crisis, there are roughly 50,000 monthly registrations in total and this increases to 60,000 after March 2020. Solo entrepreneurs increase from 60% to 65% of total firm registrations, with the increase driven mostly by new solo entrepreneurs. On average, new solo entrepreneurs share rises from 40% pre-COVID to 43% of all new monthly registrations from March 2020.[19]

The online retail sample exhibits similar dynamics to the full sample but is more pronounced. Before 2020m3, average monthly entry in online retail was 1,000 compared to total monthly entry of 50,000. After 2020m3, average monthly entry in online retail increased three-fold to 3,100, while total entry rises to 60,000. Hence, the sector's importance more than doubles from 2% of entry to 5% of entry. Furthermore, the right panel also shows that the surge in firm entry in online retail is driven to even greater extent by companies setup by new solo entrepreneurs. Pre-pandemic 65% of monthly firm entry in the online retail sector is attributable to solo entrepreneurs and 42% to new solo entrepreneurs; these numbers increase to 76% and 57%, respectively, during the pandemic. The rise from 65% to 76% share for solo entrepreneurs during the pandemic is entirely driven by entrepreneurs opening a business for the first time (new solo entrepreneurs).

3.3. Entry responds to footfall changes

To understand the dynamic response of entry to the pandemic at a high frequency, we investigate the relationship between firm creation and footfall in retail locations using weekly data. There was substantial variation in footfall during the pandemic in response to lockdown policies (Supplementary Appendix A3). If the increase in entry seen over 2020 and 2021 was due to the pandemic we would expect there to be a relationship between footfall and entry rates.

At a weekly frequency, given the lags in starting a business and finding a premise, it is reasonable to assume that footfall changes were not caused by new entrants. Additionally, in Supplementary Appendix A4, we show that our results hold when we instrument footfall with a lockdown stringency measure. Although, as we will come

19 These numbers correspond to the share of firms by ownership type in total registrations. Note that we observed an unusual number of registrations with missing ownership information in the first few months of the pandemic (between March and July 2020) that we cannot allocate to ownership types. The share of registrations with missing ownership information increased from an average of 0.1% pre-pandemic to 3% during the pandemic and peaked at 20% in July 2020.

back to below, fluctuations in footfall could capture several channels via which the pandemic can affect entry.

We use local projections (Jordà, 2005) to estimate the dynamic effect on firm entry of a shock to footfall. We estimate the following equation:

$$\text{Birth rate}_{t+h} = \sum_{l=0}^{4} \gamma_l^h \text{Footfall}_{t-l} + \sum_{l=1}^{4} \eta_l^h \text{Birth Rate}_{t-l} + \varepsilon_t \quad (1)$$

Subscripts represent week (t) and lags (l). The superscript is time horizon (h). We include a contemporaneous measure of footfall with four lags and four lags of the dependent variable birth rate. We study a 20-week time horizon. The explanatory variable of interest is Footfall_t. As explained in Section 2.4, it is defined as the percentage deviation of visits to retail and recreation locations versus the baseline calculated over 3 January–6 February 2020. The dependent variable is a Birth Rate, defined as:

$$\text{Birth Rate}_t \equiv \frac{\text{Entry}_t}{\text{Total Firms in Jan 2020}}$$

The variable Entry_t measures the annualized number of entrants in week t.[20]

Figure 5 presents the impulse response functions following a 1% negative shock to footfall. The left panel shows that following the decrease in footfall, the firm birth rate takes 9 weeks to have a significant positive effect, peaking at 0.082%. The right panel shows that the reaction of online retail is faster, and at the peak new entrants in online retail explain about a tenth (0.007 out of 0.08) of the increase in the total birth rate, despite accounting for 2% of the pre-pandemic stock.

To put these impulse responses into context, Figure 6a shows the observed annualized birth rate (in blue) for the total economy (left-hand side) and the contribution of the online retail sector to this total (right-hand side). It also shows a counterfactual birth rate absent any footfall shock (in black). The counterfactual is constructed by using the estimates of Equation (1) at horizon zero to generate the alternative time series for Birth Rate_t absent any shock to footfall, that is $\text{Footfall}_t = 0$. Figure 6b translates this rate in number of firms and shows the difference between the observed cumulative business creation and the counterfactual over 2020w14–2021w39. The results show that the footfall shock alone can account for 148,345 more businesses created over the full COVID-19 period (2020w14–2021w39, corresponding to 2020Q2–2021Q3), with 17,686 of them in the online retail sector (12%). This corresponds roughly to the 139,863 extra firms created during COVID-19 relative to pre-COVID that we identified in Figure 3.

20 We hold the definition of the denominator fixed as the regional or sectoral total across all firm types. We do this for comparability and, as we run an alternative specification exploiting regional variation in Supplementary Appendix A.4.2, to avoid over-weighting regions with small initial levels of firms in online retail.

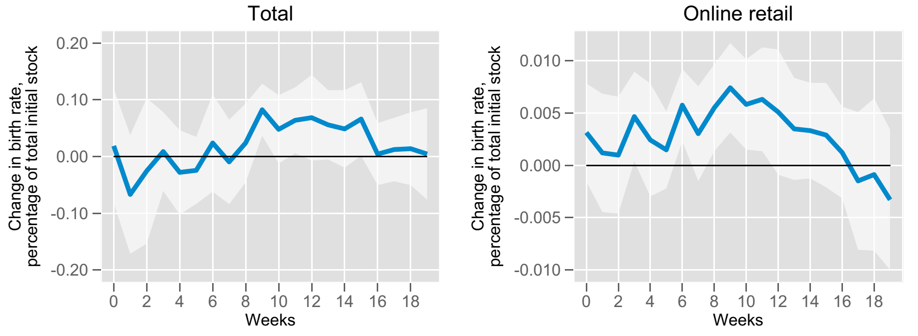

Figure 5. Local projection of retail footfall on the birth rate: estimated coefficient following a 1% decline in footfall

Notes: This figure shows the impulse response of the annualized weekly birth rate to a 1% footfall shock using the local projection framework as described in Equation (1). The response in online retail is expressed as a contribution to the total birth rate. This annualized birth rate can be converted in weekly firm creation using the total number of firms in January 20 (about 4 million of firms); a 0.082% increase in the birth rate corresponds to about 0.082%*4m/52 = 63 more firms. Standard errors are clustered at the week level. The light shaded area shows the 90% confidence interval.

In the Supplementary Appendix A4, we provide additional robustness checks of our local projections specification including an analysis that utilizes the regional component in the data.

3.3.1. Placebo analysis and mechanisms. One potential mechanism that explains the relationship between footfall and entry, and, in particular, entry in the online retail sector, is that the pandemic generated a reallocation of demand to lockdown-compliant sectors (e.g. from brick-and-mortar to online retail) and new businesses were created to meet this demand, i.e. entrepreneurship by opportunity. To provide evidence for this mechanism we consider two placebo exercises.

Our first placebo exercise illustrates that it is footfall in the retail sector specifically that explains entry. During the pandemic, consumers faced various restrictions on leaving their homes. Footfall dropped across many locations where people usually spend time, but our evidence shows that footfall declines in other locations have no impact on entry. To illustrate this, Figure 7a shows the impulse responses to a shock to mobility to 'parks' (see definition in Section 2.4, and further discussion in Supplementary Appendix A3). This serves as a placebo since access to such places was also restricted during the pandemic.

Our results show that a decline in mobility to parks has no significant effect on firm creation in total, and in online retail there is a modest negative effect after 9 weeks. This shows that the type of footfall shock matters to firm creation responses. Specifically, declines in footfall in retail can explain a rise in firm creation, whereas declines in mobility to parks cannot lead to a rise in firm creation. This favours the hypothesis that lockdowns led to firm creation through a demand channel.

Our second exercise considers industry-level entry responses. If the mechanism runs through a reallocation of demand to lockdown-compliant retail sectors, we would

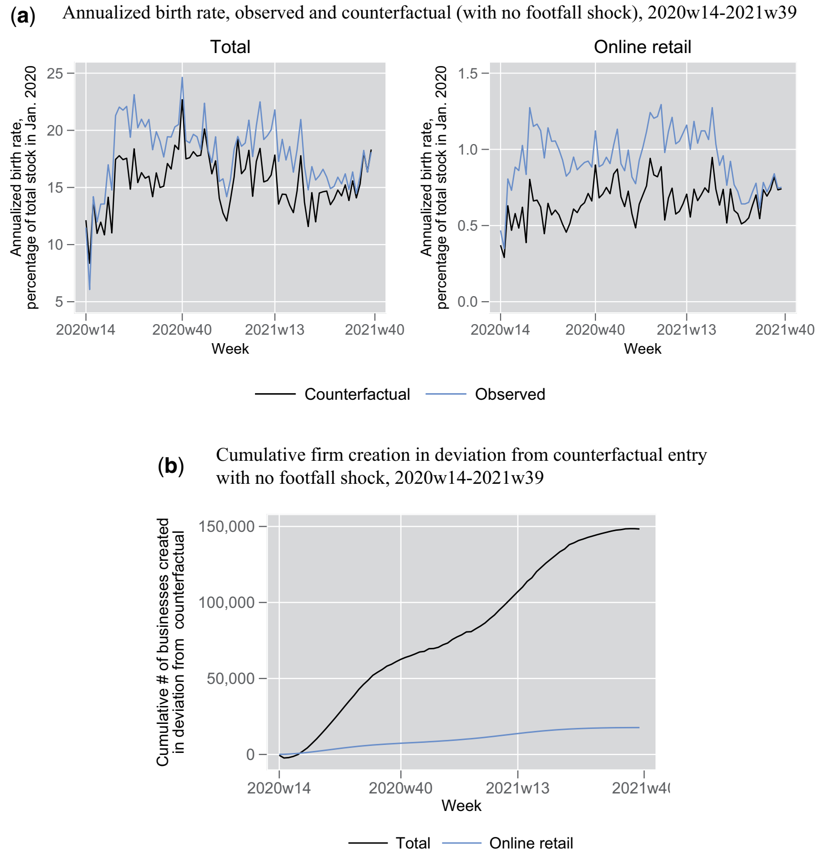

Figure 6. Local projection of retail footfall on the birth rate: observed versus counterfactual

Notes: The counterfactual is estimated using Equation (1) at horizon 0: $\text{Birth rate}_t = \sum_{j=0}^{4} \gamma_j^0 \text{Footfall}_{t-j} + \sum_{j=1}^{4} \eta_j^0 \text{Birth Rate}_{t-j} + \varepsilon_t$. We start by predicting the birth rate in 2020w10 assuming the footfall shock is null, that is using four past observations of the birth rates and their estimated coefficients (η_j^0) as well as the residuals (ε_t). We then process by iteration and use the lagged predicted values to predict following weeks. This way, we are able to compute an annualized counterfactual birth rate over 2020w14–2021w39, shown in Figure 6a. We then convert the annualized birth rate in weekly firm creation using the total number of firms in January 20 (about 4 million of firms). Figure 6b shows the difference between observed cumulative business creation over 2020w14–2021w39 and counterfactual cumulative business creation over 2020w14–2021w39 absent any footfall shock.

expect: (1) entry in sectors whose activities are unrelated to retail (e.g. construction) to show no response to footfall and (2) a decline in entry in sectors that were not lockdown-compliant (e.g. hotels and restaurants). Figure 7b confirms this hypothesis by plotting the impulse responses to a retail footfall shock in two industries, namely *Accommodation and Food Services* and *Construction*. Neither sector shows a significant firm creation response to a decline in retail footfall with the former showing a decline in entry. This suggests that sector footfall captures sector demand. This is in contrast to our main results for the *Online Retail* sector, where we would expect a decline in retail footfall to stimulate firm creation because of a demand shift.

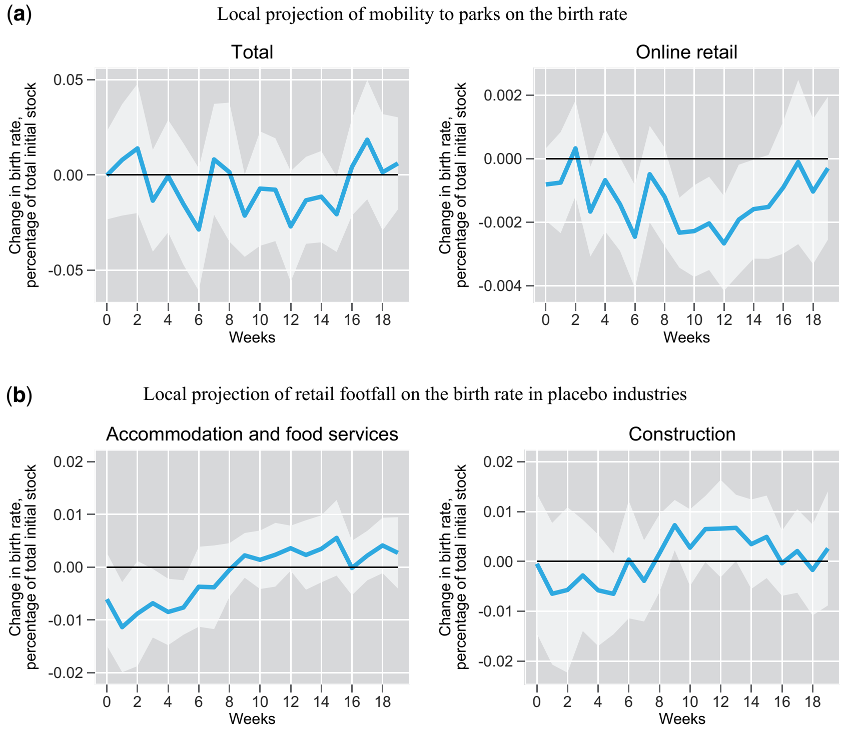

Figure 7. Estimated coefficient following a 1% decline in footfall: placebo analysis

Notes: This figure shows the impulse response of the annualized weekly birth rate to a 1% change in mobility to parks (panel a) and the impulse response of the annualized weekly birth rate for specific industries to a 1% change in the retail footfall indicator (panel b) using the local projection framework as described in Equation (1). Industry-level birth rates are expressed as contributions to the total birth rate. Standard errors are clustered at the week level. The light shaded areas show the 90% confidence interval.

An alternative mechanism that could explain our results is that the supply of entrepreneurs responds positively to footfall as there are now more people with either the additional time to create firms or became entrepreneurs by necessity due to disrupted labour markets (despite the furlough scheme). Such a mechanism would be consistent with the rise of new solo entrepreneurs documented above. On the other hand, it is harder to reconcile with our two findings that it is retail footfall that matters for entry not footfall in general and that the entry is concentrated in industries that seem best placed to benefit from the demand shift brought on by the pandemic.[21] However, new entrants may choose to congregate in sectors where demand is strongest so we do not rule out this mechanism.

21 Entry of solo entrepreneurs specifically is not sensitive to footfall empirically. However, this result may be due to data quality issues in the early stages of the pandemic. As can be seen in Figure 4, there is a sharp increase in missing ownership information among firms entering in 2020m3–m7.

3.4. Entrants are less likely to become employers

We have established the unusual increase in firm entry during the COVID-19 recession and the close relationship to declines in footfall and entry, particularly for online firms. Next, we consider whether this increase in entry is having a real economic impact. Are the new firms seeking to hire workers? Are they dissolving at a faster rate? We start by matching our Companies House firm entry data with job posting data from Indeed (see Section 2.3 for more details) in order to understand whether newly created firms are likely to have an effect on employment.

3.4.1. Job posting probabilities. We investigate the probability that COVID cohorts of firms post jobs as they age. To do so, we analyse the cumulative share of all firms incorporated in a quarter (i.e. quarterly cohorts of firms) that post a job by quarter since incorporation. We compare these shares for cohorts of firms incorporated during COVID-19 (from 2020Q3) and pre-COVID (since 2018).

Figure 8 presents the average share for cohorts born pre- and during COVID. There are eight pre-COVID cohorts (2018Q1–2019Q4) and five COVID cohorts (2020Q3–2021Q3). Our data end in 2022Q3, so we observe the final cohort 2021Q3 until age 5.[22] The figure reports the percentage of firms within a cohort posting a job by age, which can be interpreted as the probability of posting a job by age. The statistics show that firms born during COVID are less likely to post jobs at every age. There are 2.63% of firms posting within 2 years (8 quarters) for the average pre-COVID cohort, but only 2.28% for the average cohort of firms born during COVID, so a 13% decline in the posting probability.[23]

3.4.2. Job posting probabilities controlling for sector–time trends. However, the results in Figure 8 require further investigation to conclude that there was something special about the pandemic that depressed the posting rates of new entrants in particular. There are a couple of reasons for this.

First, the differential posting rate of COVID cohorts could be explained by aggregate labour market conditions that affected all firms trying to hire at the time rather than something specific about firms that were born in the pandemic. We discuss the

22 Our Indeed data begin in 2018Q1; hence, our first cohort in the pre-COVID control group is 2018Q1. A limitation of not having earlier data is that some pre-COVID cohorts will include COVID-19 periods as they age. For example, cohort 2018Q3 will be ages 7 and 8 in 2020Q1 and 2020Q2 when COVID began. Overall, this effect will decrease the pre-COVID posting rate at higher ages, so we would understate the difference in posting activity. We discuss this further in the Supplementary Appendix.

23 Because job posting and incorporation data are on a daily frequency, we observe some firms posting a job within the same quarter they incorporate. This case would correspond to firms posting at age 1, that is within the first quarter since incorporation.

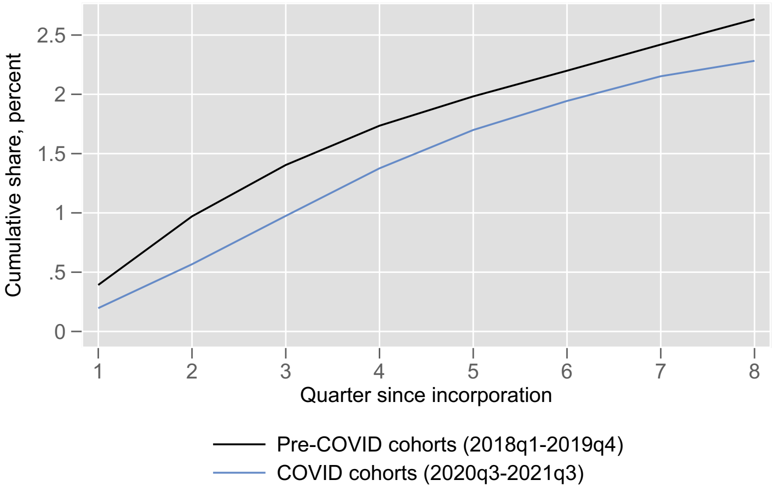

Figure 8. Cumulative share of firms posting a vacancy by quarter since incorporation: average over cohorts born pre-COVID and during COVID

Source: Authors' calculations using matched Indeed and BvD-FAME data.

aggregate trends in vacancy postings during the pandemic in Supplementary Appendix A5 (see Figure S11). Importantly, there was a strong decline in the first quarters of the pandemic followed by a strong rebound in aggregate vacancy postings from Spring 2021 as the economy recovered. Therefore, entrants born during COVID-19 operated in an aggregate environment of strong labour demand soon after their birth.

Second, entry in only a few sectors was particularly affected by the pandemic (see Figure 3). If new entrants were in sectors that typically see a lower hiring rate, then Figure 8 could simply be explained by a shift in industry composition.

To deal with these two forces, we control for sector–time trends. First, we transform cohort shares of firms posting at each age by subtracting the average time–sector trend across all cohorts, and we then look at the average posting per period to compare pre-COVID and COVID trends.

More specifically, let the variable $n_{c,s,q}$ be the share of companies posting on Indeed in quarter q, that were within the cohort of firms that incorporated in quarter c and operate in sector s.

We can remove the average sector–time trends from this posting rate by deducting the average posting rates across cohorts (where C is the total number of cohorts in the sample).

$$\hat{n}_{c,s,q} = n_{c,s,a} - \frac{1}{C}\sum_{c} n_{c,s,q}$$

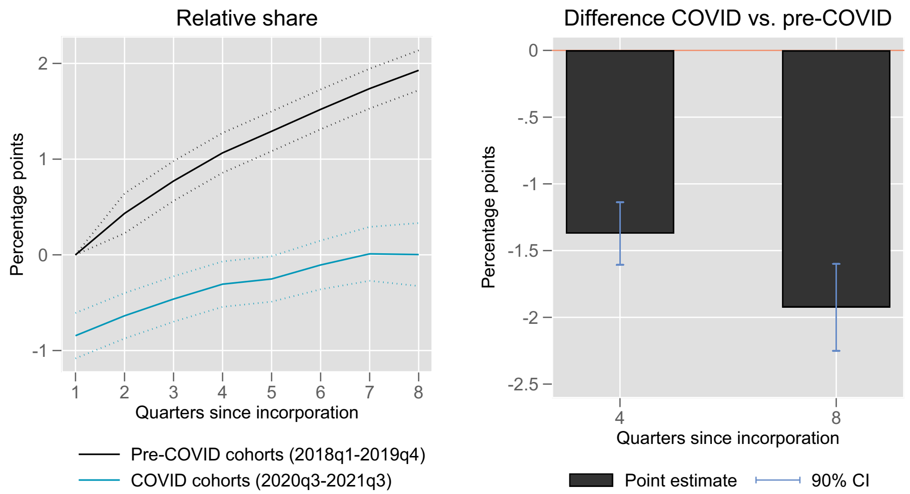

Figure 9. Cumulative share of firms posting a vacancy by quarter since incorporation: age–cohort group effects for cohorts born pre-COVID and during COVID, controlling for sector–time trends in vacancy postings

Note: The figure on the left-hand side plots the age–cohort group fixed effects from a regression using the demeaned cumulative share of posting in Indeed in each quarter by 2-digit sector. It shows age effects by cohort group for an average cohort (absent sector–time trends). We normalize the results so that the pre-COVID group share at age one is zero. Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID versus pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference. The left-hand side focuses on the significance of the coefficients taken independently, while the right-hand side shows the significance of their difference.

The age of the firm, a, is simply $q+1-c$.[24] Hence, we can manipulate indices to define $\tilde{n}_{c,s,a} \equiv \hat{n}_{c,s,a-1+c}$ as the detrended job posting rate for firms of a particular age rather than at a particular quarter. Given this, we consider the age–cohort group fixed effect coefficient ($\mathrm{FE}_{i,a}$) from the following regression:

$$\tilde{n}_{c,s,a} = \mathrm{FE}_{i,a} + \varepsilon_{c,s,a} \tag{2}$$

where i is a dummy denoting cohort groups ($c \in i$, with $i = \text{pre} - \text{COVID}, \text{COVID}$).

Figure 9, left panel, plots the age–cohort group fixed effects with 90% confidence interval. We renormalize the coefficient plots such that $\mathrm{FE}_{\text{pre}-\text{COVID},1} = 0$. Hence, the values on the y-axis represent the cumulative posting rates for the two groups at different ages in deviation from the rate of the average pre-COVID cohort at age 1. The

24 Note that $a > 0$ and $a = 1$ corresponds to the same quarter the firms incorporate. $n_{2018q1,1}$ thus corresponds to the share of firms born in 2018Q1 and posting within the first quarter, that is between their incorporation day and the last day of 2018Q1. This number is non-zero as we observe both daily incorporations and postings. We define age this way so that $a = 4$ captures firms within their fourth quarter since incorporation, that is within the first year. This ensures consistency with the timings used for our framework in Section 4.

black line suggests that, by quarter 8, there are two percentage points more postings than in quarter 1 for the average cohort pre-COVID.[25]

The figure shows that a significantly lower share of firms from cohorts of firms incorporated during the COVID pandemic (green line) relative to pre-COVID (black line) tend to post vacancies in the eight quarters following their incorporation. Indeed, the fact that the green line is significantly below the black line means that at each quarter after creation a firm created during COVID is less likely to post a job than a firm in the same sector, subject to the same aggregate shocks, at the same point in its lifecycle, than pre-COVID. The figure on the right-hand side compares the two coefficients (COVID minus pre-COVID) and shows that firms born during the pandemic are about 2 percentage points (p.p.) less likely to post a vacancy in Indeed 2 years or eight quarters after they incorporate than firms born pre-COVID.

By including sector–time fixed effects and using an unweighted regression (i.e. giving an equal weight to each cohort), we control for sectoral composition effects arising from (1) different levels of posting rates across sectors on average over the full period and (2) sectors having different dynamics for posting and entry rates both pre- and during COVID. We investigate the role of both dimensions in Supplementary Appendix A.5.2. We investigate the role of different dynamics for posting rates across sectors using a weighted regression (using cohort size). We then allow also for different posting rate levels across sectors running the regression on aggregate data, that is at the quarterly cohort level and collapsing cohorts across sectors. In both cases, the results are unaffected.

The limited role of industry composition in driving our results suggests that the greater difference between posting rates observed in Figure 9 as opposed to the simple share shown in Figure 8 is due to aggregate time trends. Specifically, despite the more favourable labour market conditions in 2021 experienced by firms born during COVID, they did not post jobs at a faster rate than their predecessors who had a more adverse environment. Our methodology enables us to control for these aggregate trends and suggests that even a lower share of firms posting for firms born during COVID relative to firms born pre-COVID.

In contrast, our specification does not control for the changing composition of ownership of new entrants. One explanation for a declining job posting rate among firms born during COVID is that the entrants are more likely to be founded by new entrepreneurs rather than serial entrepreneurs or existing corporations. We show in Figure S15 in the Supplementary Appendix that firms founded by new solo entrepreneurs are significantly less likely to post a vacancy than any other ownership group 8 quarters after entry. This is an explanation for the lower probability of firms born during COVID-19 to post on Indeed.

25 Note that this share is an average share absent sector–time trends in postings. As a reference, 2.49% of firms post in Indeed by quarter 8 since incorporation for the average cohort.

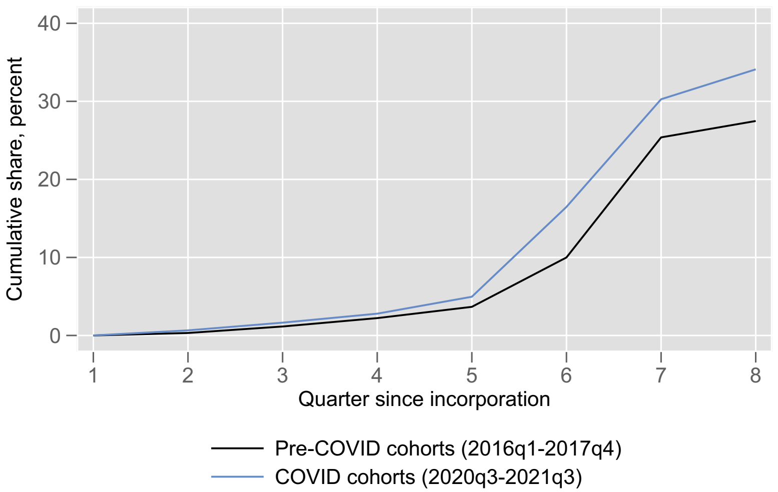

Figure 10. Cumulative share of firms dissolving by quarter since incorporation: average over cohorts born pre-COVID and during COVID

Source: Authors' calculations using BvD-FAME data.

3.5. Entrants are more likely to dissolve

We next turn to the dissolution analysis and want to analyse the share of firms dissolving by quarter since incorporation and by cohort (born pre-COVID versus COVID). Dissolutions were strongly affected by the 'easement' period in which Companies House stopped registering dissolutions for the first two quarters of 2020 (Supplementary Appendix A.6). Consequently we exclude cohorts of firms affected by the easement period. Therefore, we abstract from the 2018Q1 to 2020Q2 period and compare dissolutions within the first 2 years since incorporation for firms born between 2016Q1 and 2017Q4 (and dissolving before the easement period) to firms born after 2020Q2.

3.5.1. Dissolution probabilities. Figure 10 presents the average share for cohorts born pre- and during COVID. There are eight pre-COVID cohorts (2016Q1–2017Q4) and five COVID cohorts (2020Q3–2021Q3). Our data end in 2022Q3, so we observe the final cohort 2021Q3 until the age of 5 years. The figure reports the percentage of firms within a cohort dissolving by age, which can be interpreted as the probability of dissolving by age. The statistics show that firms born during COVID are more likely to dissolve at every age. There are 27.5% of firms dissolving within 2 years (8 quarters) for the average pre-COVID cohort, and 34.1% for the average cohort of firms born during COVID, so a 24% increase in the dissolution probability.

In Supplementary Appendix A.6.1.1, we present the data points underlying Figure 10 and list the cohorts used in obtaining the averages. The sharp increase in dissolutions at ages 6 and 7 is because new firms must submit their accounts from 18 to 21 months old, hence many dissolve at this stage.

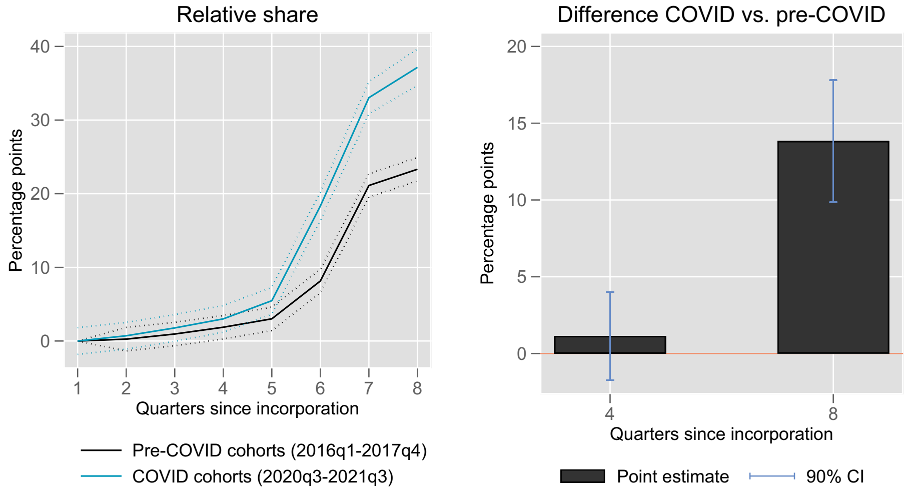

Figure 11. Cumulative share of firms dissolving by quarter since incorporation: age–cohort group effects for cohorts born pre-COVID and during COVID

Note: The figure on the left-hand side plots the age–cohort group fixed effects from a regression using the demeaned cumulative share of firms dissolving in each quarter by 2-digit sector. It shows age effects by cohort group for an average cohort. We normalize the results so that the pre-COVID group share at age one is zero. Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID versus pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference. The left-hand side focuses on the significance of the coefficients taken independently, while the right-hand side shows the significance of their difference.

3.5.2. Dissolution probabilities: age–cohort group effects. Because we already exclude cohorts affected by the easement period, and because there is no aggregate trend in dissolution rates, we do not need to control for time-sector trends as we do in the job postings analysis.[26]

Figure 11 plots the age–cohort group fixed effects coefficient ($\mathrm{FE}_{i,a}$) from the following regression: $n_{c,s,a} = \mathrm{FE}_{i,a} + \varepsilon_{c,s,a}$, with $n_{c,s,a}$ denoting this time the share of companies dissolving, with s the sector, a the quarterly age of the cohort and c the quarter of incorporation. We have i a dummy denoting cohort groups ($c \in i$, with $i = \mathrm{pre-COVID, COVID}$).

Figure 11, left panel, shows the age–cohort group fixed effects as well as its 90% confidence interval. It shows that a significantly higher share of firms from cohorts of firms incorporated during the COVID pandemic (green line) relative to pre-COVID (black line) tend to dissolve within the first eight quarters following their incorporation, and this relative higher probability increases as firms age. The figure on the right-hand side compares the two coefficients (COVID minus pre-COVID) and shows that firms born

26 In the Supplementary Appendix, the Figure shows similar lifecycle profiles for dissolutions across all cohorts, except those affected by easement which we exclude, whereas the Figure shows clear differences in the shape of the cohort plots across the lifecycle for vacancy postings.

during the pandemic are about 14p.p. more likely to dissolve in the eighth quarter after they incorporate than firms born pre-COVID.

The regressions presented here are again unweighted, so do not account for a change in cohort size and do not capture composition effects driven by a change in sector composition. We present a weighted regression (using cohort size) and a regression on aggregate data in Supplementary Appendix A6; results are unaffected.

As with job postings, the results could reflect changing composition of ownership with the rising share of new solo entrepreneurs. We find in Figure S19 in the Supplementary appendix that new solo entrepreneurs dissolve significantly more than most other groups by quarter 8 raising the overall dissolution rate for COVID cohorts.

4. STATISTICAL FRAMEWORK

Our five facts achieve our first aim of providing a deeper understanding to the firm dynamics observed during COVID-19. However, the findings still present a puzzle when it comes to the aggregate impact of firm creation during the pandemic: many more firms were born but they were also more likely to fail and, conditional on survival, less likely to hire workers. Therefore, did the surge in firm creation generate an increase in aggregate employment? To answer this question, we map our observed facts into a statistical framework, based on a simplified version of work by Pugsley and Sahin (2018). This allows us to estimate the aggregate effect of entry on employment and to decompose the employment effect into different channels as cohorts of firms age.

Let $E^i_{j,a}$ be the total employment of firms of type j that register in period i by the time they reach age a (in quarters). To fix ideas, in line with the analysis above, we will focus on j's that index firms industry or ownership status. Mechanically, $E^i_{j,a}$ can be decomposed into the following product:

$$E^i_{j,a} = N^i_{j,0} \times (1-\delta^i_{j,a}) \times p^i_{j,a} \times s^i_{j,a}. \tag{3}$$

$N^i_{j,0}$ is the number of new firms started in period i and $(1-\delta^i_{j,a})$ is the number that survive to age a (such that $\delta^i_{j,a}$ is a cumulative dissolution rate as in the previous section). Thus the product of the first two terms is the number of live firms remaining as potential employers. Next, $p^i_{j,a}$ is the share of live firms that hire workers and $s^i_{j,a}$ is the average size of an employer–firm at age a. Together, the last two terms give us the average employment per live firm. Recall that, as with other rapidly available data on firm creation during the pandemic such as US BFS data (Haltiwanger, 2021), our Companies House data measure registrations ('incorporations'), rather than the number of new employing firms. Aggregate job creation is simply $E^i_a = \sum_j E^i_{j,a}$.

Our objective is to compare typical pre-COVID job creation by new entrants to job creation from new entrants born during the COVID period. Let $i \in$ {pre-COVID, COVID} denote the two different time periods. To implement our framework, we need to specify each of the four terms in Equation (3) for $i \in$ {pre-COVID, COVID} for different age horizons a and for different firm types j. Our data and models allow us to do this for most of the required parameters although along some dimensions we only observe proxies. Consequently, our estimates are approximations, and uncertainty is greater further into the future (e.g. when $a = 8$) and for certain firm types where the sample size is small. Nonetheless, the exercise emphasizes the relative importance of the different channels for aggregate employment and distils how the facts we have documented push in opposite directions in terms of their effect on aggregate employment.[27]

4.1. Aggregate employment projections

To start, we abstract from ex-ante heterogeneity in firm types, dropping j subscripts accordingly, and treat all firms born at a particular time i as having equivalent prospects. This has the advantage of improving the precision of our estimates for the average firm but at the cost of not taking into account that the composition of entry also changed during the pandemic. We focus on how many jobs new entrants created in aggregate at $a = 4$ and 8 quarters old.

4.1.1. Calibration. To calibrate the required parameters, first, consider the firm creation levels, N_0^{pre} and N_0^{COVID}. The COVID period corresponds to the six quarters from 2020Q2 to 2021Q3 where footfall shocks generated excessive entry (Section 3.2). Therefore as a pre-COVID comparison, we take the equivalent six quarters prior to the pandemic – 2018Q2–2019Q3. During the pre-COVID period, 959,225 (639,483 annualized) firms were registered versus 1,099,088 (732,725 annualized) registered in the COVID period. This difference of 139,863 closely matches the 148,345 counterfactual difference in entry that occurred due to footfall shocks (Figure 6b).

Second, consider the dissolution rates, δ_a^{pre} and δ_a^{COVID} for $a \in \{4, 8\}$. These are directly observable from our data. The only complexity is dealing with the previously discussed easement period which means dissolution rates at firms created immediately pre-COVID are not reliable. Instead, we compute average, realized, dissolution rates for the same pre-COVID and COVID cohorts as in Figure 10.[28] That is, pre-COVID cohorts are 2016Q1–2017Q4 and COVID cohorts are 2020Q3–2021Q3.

Third, consider aggregate hiring rates of live firms, p_a^{pre} and p_a^{COVID}. These require more work to ascertain directly. Our Indeed job posting data are advantageous in

27 Brexit uncertainty affects all cohorts over the time period. Pre-COVID cohorts are affected by the impact of the UK vote to exit the EU in June 2016 and COVID cohorts by the UK leaving the EU's single market and customs union in January 2021.

28 Note that we take the realized aggregate dissolution rates (as shown in Figure 10) as opposed to using the estimates from Figure 11 which control for industry-specific factors to distinguish between periods. This enables us to capture any effect on dissolutions that comes through changes in the industrial composition of entrants. This decision makes little difference for $a = 4$. However, if δ_8^{COVID} was 14pp higher than δ_8^{pre} as Figure 11 suggests, versus the 7pp difference in the aggregate data, then job creation would fall by an additional 151,787 jobs.

allowing us to track posting rates of cohorts over time at high frequency. However, since only about 13% of employers use Indeed, the data underestimate aggregate posting rates. From the IDBR, we know the number of employer–firm births in a given year. This number tracks new firm registrations with a 1-year lag (Supplementary Figure S2). Therefore, to approximate p_4^{pre}, we use the ratio between employer–firm births in 2019 (described in Section 2.2) and company registrations in 2018 adjusted for dissolutions:

$$p_{4,IDBR}^{\mathrm{pre}} = \frac{\text{ONS IDBR employer} - \text{firm births}_{t=2019}}{\text{Companies House registrations}_{t=2018} \times (1-\delta_4^{\mathrm{pre}})} = 0.55.$$

Hence, our approximation for p_4^{pre} is 55% implying that 55% of registrations become employers within 1 year. This is likely to be an underestimate as firms may become employers long after they have incorporated. Although levels in Indeed may not represent the aggregate, the time-series evolution of postings is reliable (see Supplementary Appendix A.5.1). Therefore, to compute the remaining three hiring rates (p_8^{pre}, p_4^{COVID}, p_8^{COVID}), we assume that the ratio between each rate and $p_{4,IDBR}^{\mathrm{pre}}$ equals the equivalent ratio in our Indeed posting data. Hence, we scale the Indeed data ratio by our approximations of p_4^{pre}:

$$p_8^{\mathrm{pre}} = p_{4,IDBR}^{\mathrm{pre}} \times \frac{p_{8,Indeed}^{\mathrm{pre}}}{p_{4,Indeed}^{\mathrm{pre}}}, \quad p_4^{\mathrm{COVID}} = p_{4,IDBR}^{\mathrm{pre}} \times \frac{p_{4,Indeed}^{\mathrm{COVID}}}{p_{4,Indeed}^{\mathrm{pre}}}, \quad p_8^{\mathrm{COVID}} = p_{4,IDBR}^{\mathrm{pre}} \times \frac{p_{8,Indeed}^{\mathrm{COVID}}}{p_{4,Indeed}^{\mathrm{pre}}}.$$

The parameter values with subscript ‘Indeed’ correspond to the data in Figure 8.

Lastly, consider firm size given by the number of workers per employer-firm (s_a^i). Our facts do not offer direct evidence on this channel, but we can obtain estimates from ONS-IDBR data. From the quarterly IDBR, we know that new employer–firms started with 3.56 employees on average over 2019 which we use to approximate s_4^{pre}. A similar calculation from the IDBR over 2021 gives s_4^{COVID} as 3.42.[29] Our BvD data enable us to calculate mean employer–firm growth in its labour force between quarters 4 and 8, on average, over 2017–2019 and for the 1.7k firms that already filed employment data and were born from 2020Q1. We get that the average firm pre-COVID grows by 10% between its first and second year (since incorporation), suggesting s_8^{pre} is 3.92; similarly, we get that the average COVID firm grows by 8% between its first and second year (since incorporation) suggesting s_8^{post} is 3.68.

4.1.2. Results. The first two columns in Table 1 present our calibration for each of the four parameters in Equation (3) for horizons $a = 4$ and $a = 8$. In turn, the table shows

29 The quarterly IDBR only enables us to measure employment creation over total firm births, which we rescale using the share of employer–firms in total births using IDBR annual files. This number is thus firm size conditional on being an employer.

Table 1. Estimated job creation by new firms before and during the pandemic

	Weighting: Horizon:	- a = 4	- a = 8	Industry a = 4	Ownership a = 4
	Pre-COVID				
(1)	N_0^{pre} (annualized)	639,483	639,483	639,483	639,483
(2)	$1 - \delta_a^{\text{pre}}$	0.98	0.73	see	See
(3)	p_a^{pre}	0.57	0.86	Table	Table
(4)	s_a^{pre}	3.56	3.92	A.3	2
(5)	$E_a^{\mathbf{pre}}$	**1,268,322**	**1,565,397**	**1,281,340**	**1,225,954**
	COVID				
(6)	N_0^{COVID} (annualized)	732,725	732,725	732,725	732,725
(7)	$1 - \delta_a^{\text{COVID}}$	0.97	0.66	See	See
(8)	p_a^{COVID}	0.45	0.75	Table	Table
(9)	s_a^{COVID}	3.42	3.68	A.3	2
(10)	$E_a^{\mathbf{COVID}}$	**1,105,597**	**1,335,024**	**1,124,738**	**763,267**
(11)	E_a Difference	−162,725	−230,373	−156,602	−462,688
	Counterfactuals				
(12)	Only $N_{j,0}^i$ Changes	184,932	228,248	71,934	66,948
(13)	Only $\delta_{j,a}^i$ Changes	−7,265	−142,898	−13,630	−3,940
(14)	Only $p_{j,a}^i$ Changes	−256,617	−200,505	−93,427	−202,749
(15)	Only $s_{j,a}^i$ Changes	−51,702	−94,861	−80,656	−353,493
(16)	No change in $s_{j,a}^i$	−115,741	−144,254	−69,750	−154,259

the value of employment creation (E_a^i) given these values, and the difference in employment creation from pre-COVID to COVID periods.

Given our calibration, row (5) of Table 1 suggests that prior to the pandemic a year's worth of new entrants would create approximately 1.27 million jobs in their first year and an additional 300 thousand jobs in their second. By way of comparison, the IDBR data show that new employers created 1.07 million jobs in 2019.

Row (10) of Table 1 provides the equivalent figure for the COVID period and row (11) the difference between E_a^{COVID} and E_a^{pre}. The message from these figures is that cohort of firms born during the pandemic, despite the cohort's size, generated (on an annualized basis) 162,725 fewer jobs in their first year than the pre-pandemic cohort and 230,373 fewer in their second year; respectively, 13% and 15% reduction in job creation over the first and second year.

To understand what drives this finding, rows (12)–(15) consider the difference between E_a^{pre} and four counterfactual values of E_a^{COVID} where individually only one of N_0^i, δ_a^i, p_a^i and s_a^i are allowed to vary from their pre-COVID levels. The message from row (12) is that if the firms created during the pandemic survived and hired workers at the same rate as their pre-COVID counterparts, an extra 184,932 jobs would have been created in their first year relative to the pre-COVID baseline. This amounts to a 15% rise in job creation. However, the fact that fewer firms created during the pandemic sought to hire workers and when they did, hired fewer of them, pushes in the opposite direction leading to an overall reduction in job creation.

Row (16) considers the robustness of our calibration as an alternative where we hold fixed average employer–firm size at s_a^{pre} in the COVID period. Firm size is the most uncertain part of calibration as it requires extrapolation from the limited number of firms that have reported employment data in BVD for the pandemic period. Holding size fixed diminishes the estimated decline in aggregate job creation by around a third and serves as a useful lower bound on our estimates.

As an overall robustness check, in Supplementary Appendix A7, we compare these figures to official statistics on job creation from new employers from the ONS IDBR (Office for National Statistics (ONS) 2022a). These data cannot be directly used for our analysis because we cannot track firms through time and it is unclear when the firms that entered the IDBR were first founded. Nonetheless, the number of jobs created by new firms appearing on the IDBR has fallen from just over 300,000 per quarter prior to the pandemic to around 240,000 during and after the pandemic. This 20% decline matches our own estimates from Table 1.

Last, note that our data provide sufficient information to study employment effects in our statistical framework for up to 2 years. The longer run impact beyond this 2-year horizon is uncertain and our data are insufficient to shed light on what the consequences will be several years down the line.

4.2. Employment projections by ownership and industry

The statistical framework in Equation (3) is clearly non-linear. The initial approach of taking aggregate averages misses composition effects. For example, the analysis in columns (1) and (2) of Table 1 will underestimate the effect of the pandemic on employment creation if there has been a shift in entrants away from industries that hire more workers or grow more quickly than the economy-wide average.

Our facts above highlighted two key changes in entry composition during the pandemic. So we now turn to considering how the change in industry and ownership composition among new entrants has affected employment creation. Specifically, we now apply the statistical framework in Equation (3) when j indexes firms by (1) each of the seventeen different industry types in Figure 3 and (2) the 5 different ownership types articulated in Section 1. We focus solely on $a = 4$, data constraints that prevent a calibration based on $a = 8$.

4.2.1. Calibration. We calibrate parameters using broadly the same methodology as in our aggregate analysis, except that we now estimate $\{N^i_{j,a}, p^i_{j,a}, \delta^i_{j,a} s^i_{j,a}\}$ for groups of companies by their industry or ownership structure.

From the Companies House data, it is straightforward to directly measure $N^i_{j,a}$ and $\delta^i_{j,a}$ in both the pre- and during *COVID* periods by industry and firm ownership type.

To calibrate hiring rates of live firms, $p^{\text{pre}}_{j,a}$ and $p^{\text{COVID}}_{j,a}$, the ONS data required are only available at the industry level and not at the ownership level. For industries, we proceed as before and approximate $p^{\text{pre}}_{4,j}$ using the ratio between employer–firm births by industry in 2019 and company registrations by industry in 2018 adjusted for industry-level dissolutions (described in Section 2.2 and Section 4.1.1). Specifically, this rate for industry j is:

$$p^{\text{pre}}_{4,IDBR,j} = \frac{\text{ONS IDBR employer-firm births}_{t=2019,j}}{\text{Companies House registrations}_{t=2018,j} \times (1-\delta^{\text{pre}}_{4,j})}$$

To get a rate by type of ownership, we use Indeed relative posting rates by ownership to rescale the aggregate posting rate from the IDBR. Specifically, this rate for ownership type o is:

$$p^{i}_{4,o} = \frac{p^{i}_{4,o,Indeed}}{(1-\delta^{i}_{4,o})} \times \frac{p^{i}_{4,IDBR}}{p^{i}_{4,Indeed}},$$

where $p^{i}_{4,o,Indeed}$ is the job posting rate of ownership type o of cohort i at age $a = 4$.

For the number of workers per employer–firm ($s^{i}_{j,a}$), IDBR quarterly data are only available for the aggregate. However, our BvD data enable us to calculate employer–firm size in quarter 4, by industry and ownership relative to the aggregate, on average, over 2017–2019 and for the 1,700 firms that already filed employment data and were born from 2020Q1. We apply these relative sizes to ONS aggregate firm size pre-COVID and during COVID. Specifically, letting $s^{i}_{a,BVD}$ and $s^{i}_{a,IDBR}$ be the average number of workers recorded in BVD and IDBR, respectively, then our calibration is:

$$s^{i}_{j,a} = \frac{s^{i}_{a,\text{IDBR}}}{s^{i}_{a,\text{BVD}}} \times s^{i}_{j,a,\text{BVD}}.$$

Since there are only 1,700 firms incorporated during COVID-19 that already declare employment data in BVD, the sample size becomes small as we exploit more granular characteristics to compute $s^{\text{COVID}}_{j,a,\text{BVD}}$. For example, there are only 31 firms born during COVID-19 and in online retail with employment data. This is the reason we focus only on calibrating to $a = 4$. As above, employer–firm size is the most uncertain element of our calibration. Below, we will also conduct a sensitivity check assuming that the firm size margin does not change, and these do not affect our broad conclusions.

4.2.2 Results. Column (3) of Table 1 presents the aggregate employment creation in the *pre-* and during *COVID* periods when we account for the industrial composition of start-ups. Supplementary Appendix Table S3 present the underlying calibration and predictions for each industry. The headline result from accounting for the industrial composition of start-ups is nearly identical to the aggregate calibration in column (1): a 12% decline in employment creation (from 1,281,340 to 1,124,738).

However, this total result masks offsetting forces present when one accounts for industrial heterogeneity. The counterfactual where we only allow the number of entrants by industry to change yields 71,934 new jobs compared to the 184,932 new jobs created in the equivalent exercise in column (1) which just considers aggregate entry. This is consistent with a shift in entry composition towards an industry that typically created fewer jobs even to prior to the pandemic. Offsetting this is a reduced role for the decline in the job posting rate. *Ceteris paribus*, the decline in job postings accounts for 93,437 fewer jobs when one accounts for the industrial composition compared to 256,617 fewer jobs from the exercise in column (1). The message is that decline in job postings is concentrated in industries where dissolution rates are relatively high and ex post firm size is low so the effect is mitigated.

Column (4) of Table 1 considers the total effects where we account for start-ups ownership composition. The key result is that change in employment creation from pre- to COVID periods is significantly worse when we consider the change in ownership composition, rather than the change in industry composition or using aggregate data. There is a fall of 1,225,954 to 763,267, a 38% decline.

Table 2 presents employment projections by ownership structure. Corporate owners (i.e. subsidiaries) are the most important component of total employment in both periods, but they are the least important in terms of registrations. The decline in their size after four quarters is the key factor reducing employment by entrants during COVID-19. Additionally, there is no offsetting positive effect on employment from increased firm creation which we observe in other categories; in fact, entry declines for corporate owners which reinforces the fall in employment creation by 12,851.

In stylized fact (2) (Section 3.2), we stress the disproportionate role of 'Solo entrepreneurs (new)' – owners setting-up their first business – in driving the boom in firm creation. Table 2 reiterates the importance of this category in terms of firms created rising from 253,395 pre-COVID to 314,915 during COVID-19. However, given those companies are usually very small (1.60 employees on average pre-COVID, versus 3.56 for the average firm), this surge translates into few jobs – in isolation, this channel boosts employment by 43,206 only.[30] Moreover, we see that the total employment contribution from this category falls. The decline in employment creation of 102,919 mainly occurs because firm size decreases drastically. In isolation the firm size fall causes a 105,396 decline, and this is also reinforced by a decline in registrations converting to live firms, contributing another 28,560. There is a modest reinforcing effect from greater dissolutions adding 1,568 to the decline, but at the four quarter horizon dissolution rates play a

30 Had solo entrepreneurs employed an average of 3.56 employees, the surge in business creation by solo entrepreneurs would have added 231,250 jobs relative to pre-COVID.

Table 2. Employment projections by ownership, $a = 4$

		Solo new	Solo serial	Group new	Group serial	Corp.	Other	Total
	Pre-COVID							
(1)	$N^{pre}_{j,0}$ (annualized)	253,395	144,675	83,765	111,669	45,639	340	639,483
(2)	$1-\delta^{pre}_{j,a}$	0.98	0.98	0.99	0.98	0.98	0.98	
(3)	$p^{pre}_{j,a}$	0.45	0.61	0.62	0.80	0.69	0.57	
(4)	$s^{pre}_{j,a}$	1.60	1.51	2.07	2.83	18.14	3.56	
(5)	$\boldsymbol{E^{pre}_{j,a}}$	**177,962**	**129,833**	**106,695**	**246,491**	**564,298**	**674**	**1,225,954**
	COVID							
(6)	$N^{COVID}_{j,0}$ (annualized)	314,915	167,315	84,413	96,757	44,600	24,725	732,725
(7)	$1-\delta^{COVID}_{j,a}$	0.97	0.97	0.98	0.98	0.98	0.97	
(8)	$p^{COVID}_{j,a}$	0.38	0.45	0.59	0.67	0.58	0.45	
(9)	$s^{COVID}_{j,a}$	0.65	1.51	1.81	2.11	12.61	3.42	
(10)	$\boldsymbol{E^{COVID}_{j,a}}$	**75,043**	**110,572**	**88,442**	**133,300**	**318,603**	**37,308**	**763,267**
(11)	$E_{j,a}$ Difference	–102,919	–19,261	–18,253	–113,191	–245,696	36,633	–462,688
	Counterfactuals							
(12)	Only $N^{i}_{j,0}$ Changes	43,206	20,317	825	–32,914	–12,851	48,365	66,948
(13)	Only $\delta^{i}_{j,a}$ Changes	–1,568	–599	–1,091	126	–804	–4	–3,940
(14)	Only $p^{i}_{j,a}$ Changes	–28,560	–33,760	–5,073	–40,514	–94,705	–136	–202,749
(15)	Only $s^{i}_{j,a}$ Changes	–105,396	–27	–13,599	–62,483	–171,960	–27	–353,493
(16)	No change in $s^{i}_{j,a}$	6,075	–19,238	–5,335	–67,927	–106,053	38,219	–154,259

The column headings are abbreviations of: Solo entrepreneur, new; Solo entrepreneur, serial; Group of individual shareholders, all new; Group of individual shareholders, at least one serial; Corporate shareholder, at least one; Other/Missing.

limited role across all categories (row 13). As discussed earlier, dissolutions are more important from 6 to 8 quarters when firms should submit their first set of accounts.

As described, the most uncertain parameter in our model is firm size. Repeating the robustness check where we hold firm size fixed alters the magnitude of results, but the qualitative messages remain the same. Employment creation by new firms fell during COVID and accounting for the change in ownership type of new firms leads to worse employment outcomes than the change in industry structure. In fact, holding firm size fixed substantially weakens the impact of the pandemic on job creation when we control for industry composition as entry grew in a couple of industries where we estimate the size fell substantially (particularly online retail). In contrast, controlling for the ownership composition and holding firm size fixed leads to an employment decline of 154,259 compared to a figure of 115,741 when using aggregate data. This is mainly driven by the corporate sector which experiences a fall in registrations and a decline in conversion of registrations to live firms. Solo entrepreneurs now play less of a role. As before, the corporate ownership type plays an important role in total effects because it is large in terms of employment. Therefore, even small changes in underlying parameters –

namely registrations or conversions to employer firms – lead to large effects on aggregate outcomes.

5. CONCLUSION

We study firm creation in the UK during the COVID-19 pandemic. We establish five stylized facts from new data. First, we show that firm creation has been countercyclical during the COVID crisis, and this is at odds with nearly all recessions over the past century in the United Kingdom. Then, we investigate the background to this puzzling fact. Our second fact shows that firm creation has been concentrated in specific sectors like online retail, and of that most registrations come through solo entrepreneurs rather than groups of individuals or corporations. Our third fact uses footfall data to identify a mechanism for rising firm creation. We show that as footfall in brick-and-mortar retailers declines, firm entry in all sectors, especially online retail, expands. Our last two facts show that this boom in firm creation does not have the usual employment effects that we would expect in normal times over a short-run horizon. Using matched data from online job postings, in fact four we show that firms created during the pandemic are less likely to post jobs, and in fact five we show that firms created during COVID-19 are more likely to dissolve.

Given our five facts, we then bring them together into a statistical framework in order to understand what the facts imply for aggregate employment. Our framework divides employment changes into four channels which relate to our stylized facts: registrations of new firms, conversion of registrations to employer firms (i.e. job postings), dissolutions and firm employment size changes. The framework shows that despite the surge in business registrations, new firms generated fewer jobs in their first 2 years than firms born pre-pandemic. This framework highlights the opposing effects of different channels, for example booming firm creation – ceteris paribus – would increase employment, but it is more than offset by lower conversion of registrations to employer firms, higher dissolution rates and declines in firm size.

As a final exercise, we apply our statistical framework to individual ownership types and industry structures. The aim of this exercise is to understand the implication of changing industry and ownership composition during COVID-19, which we stress in stylized fact (2). The application shows that the change in ownership composition contributes more negatively to employment creation than the change in industry composition, though both led to declines in employment creation relative to pre-COVID. The ownership type that experienced a significant boom in firm creation – new solo entrepreneurs – did little to offset the overall effect, and in fact reinforced it, as the new solo entrepreneurs hired fewer employees than other groups, and those registered during COVID-19 were even less likely to become employer firms, more likely to dissolve, and less likely to grow.

In summary, the main takeaways of our paper are (1) despite surging firm creation during COVID, the short-run employment effects are limited and (2) the change in ownership composition of new firms during COVID-19 played a significant role in lowering employment creation by new firms.

SUPPLEMENTARY DATA

Supplementary data are available at *Economic Policy* online.

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