Business Creation during COVID-19

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Abstract

Using UK data, we present greater empirical detail on the puzzling firm dynamics that emerged during 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.

JEL: E32, L25, L26.

Keywords: Firm Dynamics, COVID-19, Business Dynamism, Firm Entry.

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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 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 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:

(i) 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.

(ii) New firms are disproportionately concentrated in the online retail sector and founded by individuals who started their first business.

(iii) Firm entry is negatively correlated with retail footfall during the pandemic.

(iv) New firms are less likely to post jobs than firms created pre-COVID.

(v) 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, Buckmann, Turrell, and Key 2021; González-Pampillón, Nunez-Chaim, and Ziegler 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, 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 is worse than employment from firm creation during normal times, even though there is more firm creation during COVID.

The limited employment potential of firms created during COVID appears consistent with another finding of ours which is that firms created during COVID are disproportionately setup by individuals with no prior experience in firm ownership, so-called new solo entrepreneurs. Firms set up by these sort of entrepreneurs are both less likely to attempt to hire workers and more likely to dissolve. As a result, the change in ex-ante characteristics of firms created during COVID seems 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 provides 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 is accessible directly or via BvD-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 probability of dissolving for pre- and post-pandemic cohorts of firms, we classify firms by the quarter they are created and study each cohorts' 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, Dunne, Haltiwanger, and Penciakova (2021) present evidence for the US, and OECD (2021) provide evidence for OECD countries. Duncan, Galanakis, León-Ledesma, and Savagar (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). However, the existing literature has not observed the real impact of these new entrants. Benedetti-Fasil, Sedláček, and Sterk (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 startups. Our results focus on the surge in firm entry following this initial decline, and suggest that these new firms are considerably weaker in their ability to buoy employment. More broadly than the COVID-19 pandemic, our findings add to growing research on the importance of firm characteristics at start-up for future employment (Sedláček and Sterk 2017; Sterk, Sedláček, and Pugsley 2021). We stress that the composition of ownership structure responds to the recession. This adds a business cycle perspective to research that shows that ownership structure at start-up affects subsequent firm performance (Felix, Karmakar, and Sedláček 2021).

The remainder of the paper is structured as follows: Section 2 describes our data; Section 3 presents our five facts (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 Bureau van Dijk (FAME) to construct daily entry, dissolution and ownership statistics. We supplement these high frequency statistics with Office for National Statistics (ONS) summary statistics derived from the, confidential, Inter-departmental Business Register (IDBR). We use data from Indeed to measure job postings. Lastly, we use Google mobility data to measure retail footfall.

2.1 Business registrations (Companies House & FAME)

We use the Financial Analysis Made Easy (FAME) dataset provided by Bureau van Dijk (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 entry since 2005 (see Appendix A.1 for a time series of entry).

The Companies House register records all *incorporated companies* in the UK. Incorporated companies are separate legal entities to the business owner.[1] 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 nonprofit organizations. Updates to the Companies House register are automated following a business application. Consequently, there are no administrative lags due to the pandemic.

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 date dissolution date when applicable. This allows us to measure daily number of incorporations and dissolutions by local authority and industrial sector.[2]

[1]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).

[2]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.

2.1.1 Ownership information

The BvD-FAME dataset also reports ownership information, which is acquired from Companies House. Firms registering with Companies 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 five 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 five 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.[3]

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 five years.

- *Solo entrepreneur (serial)*: one owner who has owned at least one other firm in the last five years.

[3]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.

- *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 (IDBR)

Due to accounting exemptions and lags, employment information for new firms in Companies House (BvD-FAME) is sparse, and not timely enough for our analysis.[4] Consequently, we use aggregate data released by the ONS which is derived from the Government's, confidential, business register known as the Inter-departmental Business Register (IDBR).[5] Firms are added to the IDBR if they employ someone (register for payroll tax, PAYE) or register for VAT tax. The data provides 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.[6] 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 Appendix Figure 13). Consequently, to approximate the fraction of Companies House registrations that become employer-firms, we take the following ratio:

$$\frac{\text{Number of IDBR additions with employment in year } a}{\text{Number of company registrations in quarter year } a-1}$$

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); by contrast, this

[4]There were 1.4 million firms registered between March 2020 and September 2021, but only 327,00 (23%) had filed employment information by February 2023.

[5]We use both the detailed annual updates of business demography (ONS "Business demography") and the quarterly updates (ONS "Business demography, quarterly experimental statistics"). Appendix A compares Companies House registrations to ONS firm entry from the IDBR.

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

ratio is only 40% in 2021, i.e. 40% of firm registered in 2020 become employer-firms within a year (in 2021).

This dataset is not used for our five facts, but it allows us to approximate the number of company registrations that become employer firms. This helps us to understand how our facts translate to aggregate employment through a statistical framework.

2.3 Job postings (Indeed)

To study the employment behaviour of firms in more details, we use data from Indeed. The data from Indeed enables us to track job postings at the firm level, and to know the length in days between when the firm was incorporated and when it started posting on Indeed. This way, we can investigate whether the probability to become an employer-firm has changed for cohorts of firms born during the pandemic relative to 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 includes both jobs posted directly on Indeed and on companies' websites scraped by Indeed.[7] 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.[8] We drop a match if the company job posting pre-dates its matched incorporation date in Companies House.

The data covers 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).[9]

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

[8] Similar to Van Dijcke, Buckmann, Turrell, and Key (2021), 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%.

[9] 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.

There were 1.5m 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 UK. Appendix E shows there is a close correlation with official ONS vacancy data from surveys and Van Dijcke, Buckmann, Turrell, and Key 2021 show that the industrial mix in Indeed is similar to the wider UK economy. Moreover, the vacancy data from Indeed is 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 represents 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 shows how visitors to these locations changed compared to a baseline. The baseline is the median value from the 5-week period Jan 3 – Feb 6, 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 UK. The series begins February 15 2020 and ends in October 15 2022.

3 Facts

3.1 Entry cyclicality

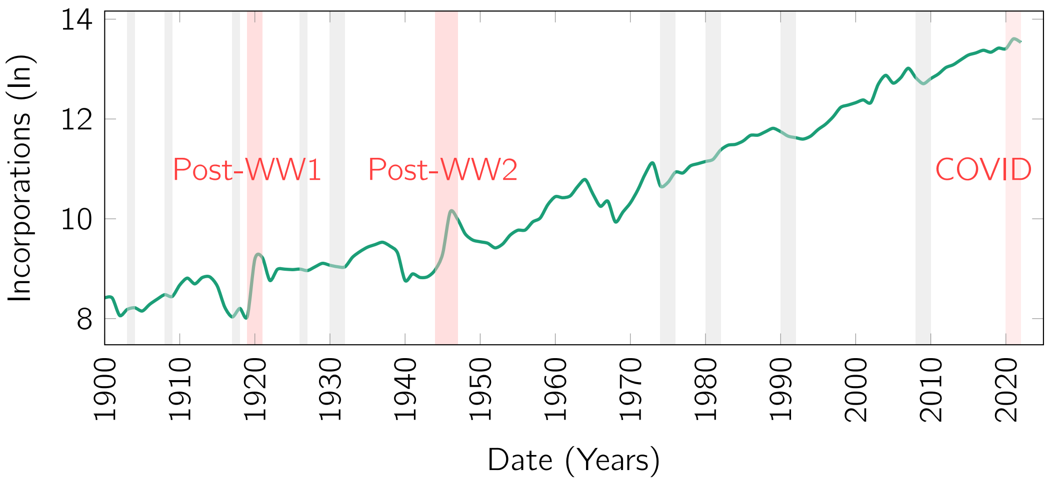
Figure 1 plots firm registrations in the UK over 120 years.[10] Entry of new employers (employer-firms) is typically procylical: it rises in booms and declines in recessions (Lee and Mukoyama 2015; Tian 2018). However, our evidence for firm registrations in the UK (Figure 1) shows that in 'extreme event' recessions, such as wartime and the COVID-19 pandemic, firm entry (registrations) is countercyclical.[11] 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 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.

[10] 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 January 1st the approximation error from doing this is not large.

[11] 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 countercylical (Fossen 2021).

Figure 1: Business creation in the UK, 1900-2022

Source: Historical Companies House Register and BvD-FAME for firm entry, BoE: A Millenium of Macroeconomic Data for GDP.

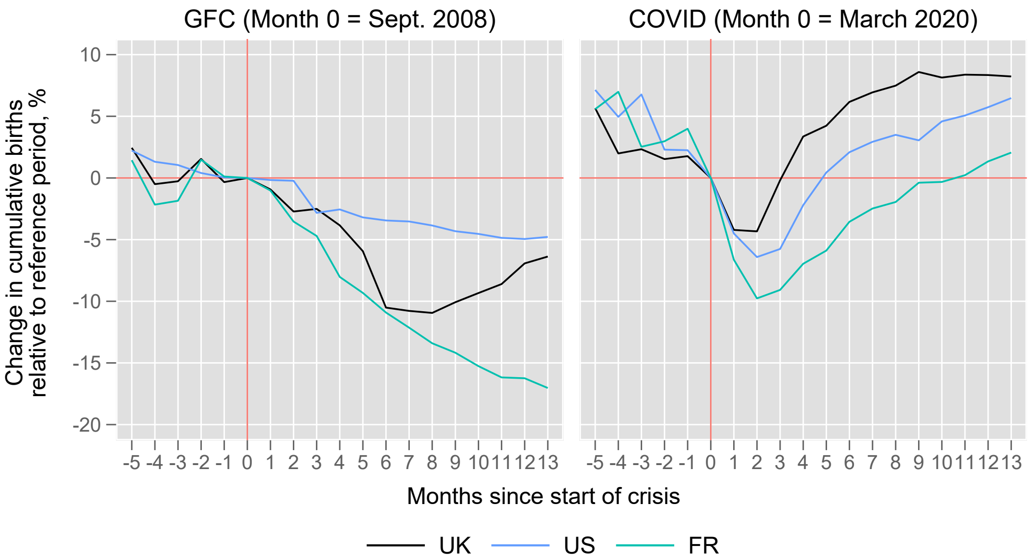


Note: Shaded areas correspond to years when UK GDP growth was negative; the area is flagged in red if firm entry increased over the period.

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, Dunne, Haltiwanger, and Penciakova (2021) for the US (US Census) to France (INSEE) and the UK (Companies House), and we follow their methodology.[12] The interpretation is growth rates of cumulative registrations during the crisis period relative to the reference period. Whilst the COVID period shows a clear decline and sharp recovery across all countries, the GFC exhibited a protracted decline in entry growth rates from the onset of the crisis.

[12]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 2: Cumulative business registrations, Global Financial Crisis (GFC) vs. COVID, for UK, United States, France
Source: Authors' calculations using Companies House, BvD-FAME, US Census and INSEE.



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

3.2 Entry composition

We then focus on the UK experience, and describe in more details 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 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. For reference, firms in online retail accounted for less than 2% of firms in January 2020 (Appendix B). 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 3: Change in number of cumulative incorporations, during COVID (2020q2-2021q3) vs. pre-COVID (2018q2-2019q3) and sector contributions
Source: Authors' calculations using BvD-FAME.

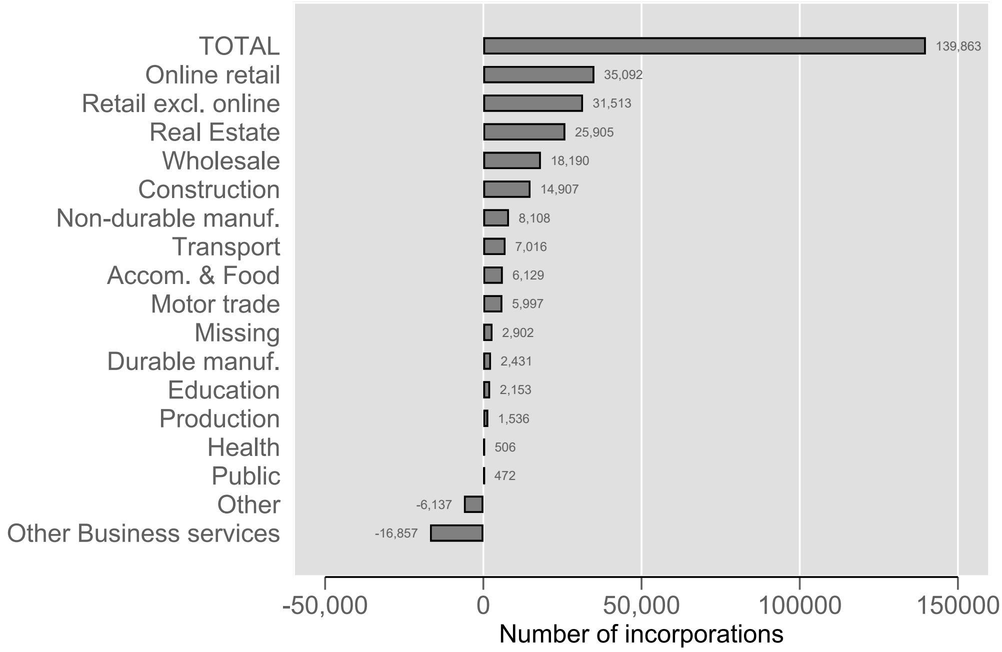


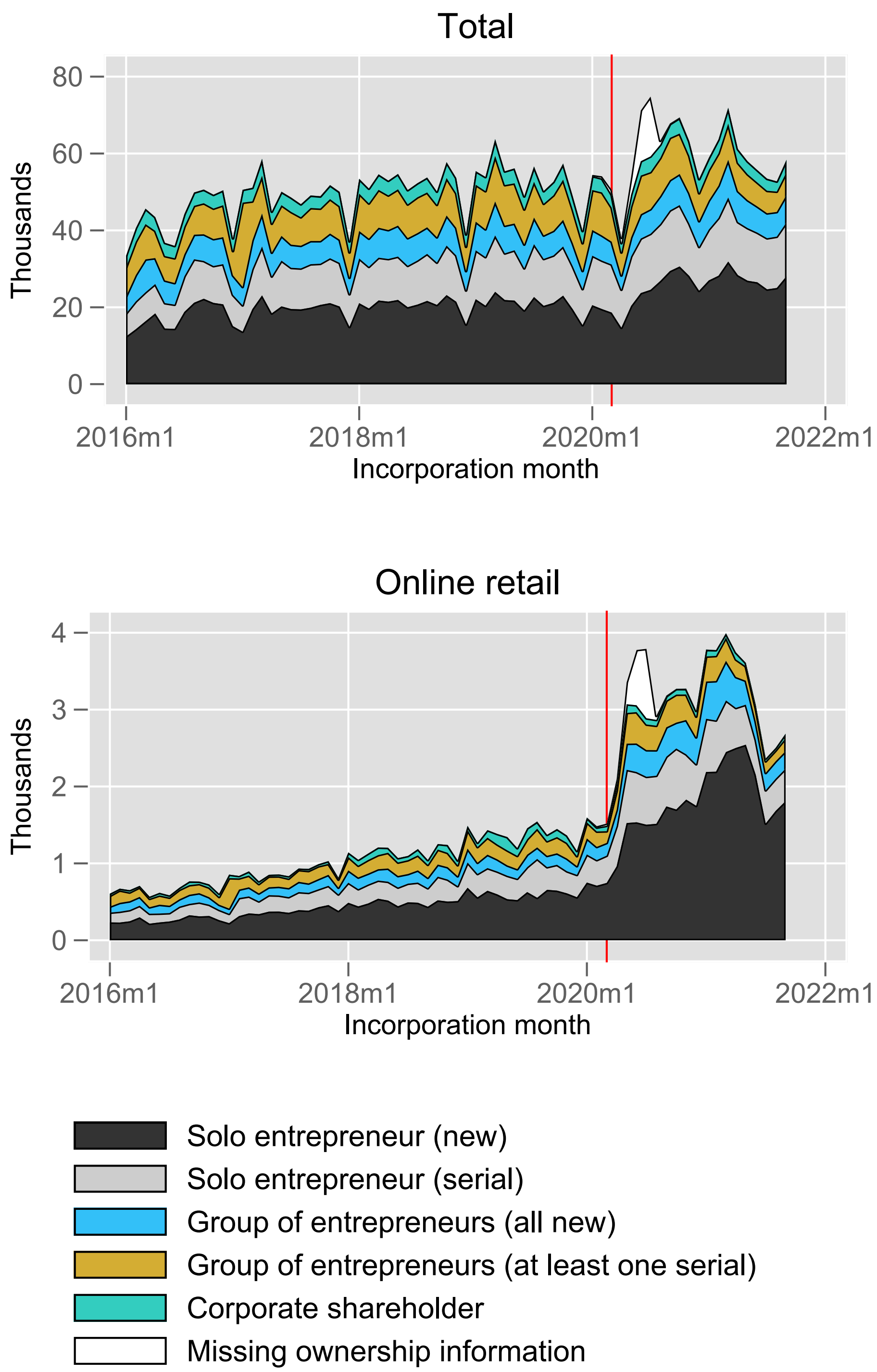
Figure 4 shows monthly firm creation in the aggregate economy and for the online retail sector. Additionally, we decompose firm creation by ownership structure to explore ex-ante sources of heterogeneity, which may affect subsequent behaviour such as survival and job posting. If existing firms quickly 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 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 2020m3. 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.[13]

The online retail sample exhibits similar dynamics to the full sample but more pronounced. Before 2020m3 average monthly entry in online retail is 1,000 compared to total monthly entry of 50,000. After 2020m3 average monthly entry in online retail increases threefold 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).

[13] These numbers correspond to the share of firms by ownership type in total registrations. Note that we observe 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.

Figure 4: Monthly firm creation by type of ownership, total economy and online retail sector, January 2016 to September 2021
Source: authors' calculations using BvD-FAME.



Note: 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 five years prior to the pandemic. See section 2.1 for a detailed description of ownership categories. The red vertical line denotes March 2020, the start of the pandemic.

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 (Appendix C). 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 premises, it is reasonable to assume that footfall changes were not caused by new entrants. Additionally, in Appendix D we show that our results hold when we instrument footfall with a lockdown stringency measure. Although, as we will come 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:

$$\text{Birth rate}_{t+h} = \sum_{j=0}^{4} \gamma_j^h \text{Footfall}_{t-j} + \sum_{j=1}^{4} \eta_j^h \text{Birth Rate}_{t-j} + \varepsilon_t \tag{1}$$

Subscripts represent week (t) and lags (j). 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 Jan 3 – Feb 6, 2020. The dependent variable is a Birth Rate, defined as:

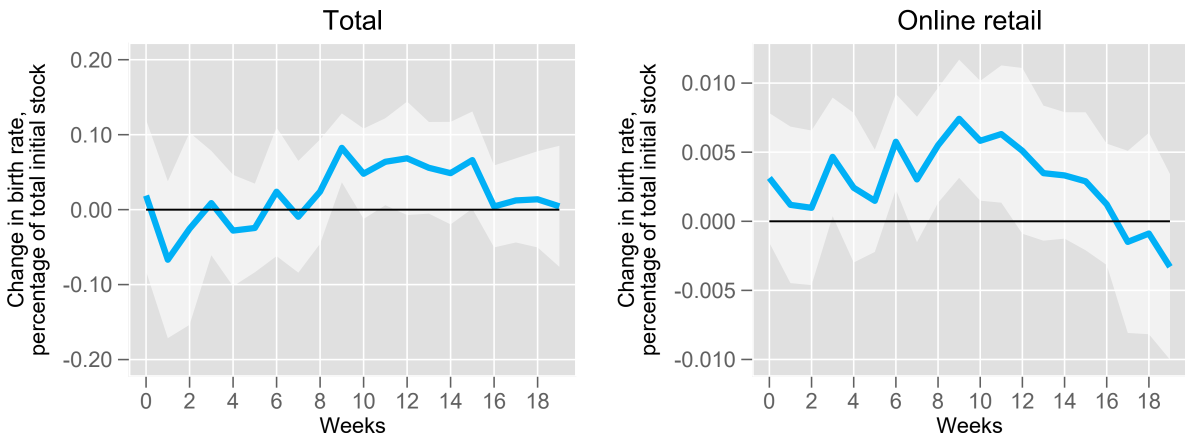
$$\text{Birth Rate}_t \equiv \frac{\text{Entry}_t}{\text{Total Firms in Jan 2020}}$$

The variable Entry_t measures the annualized number of entrants in week t.[14]

Figure 5 presents the impulse response functions following a 1% negative shock to footfall.

[14] 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 Appendix D.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



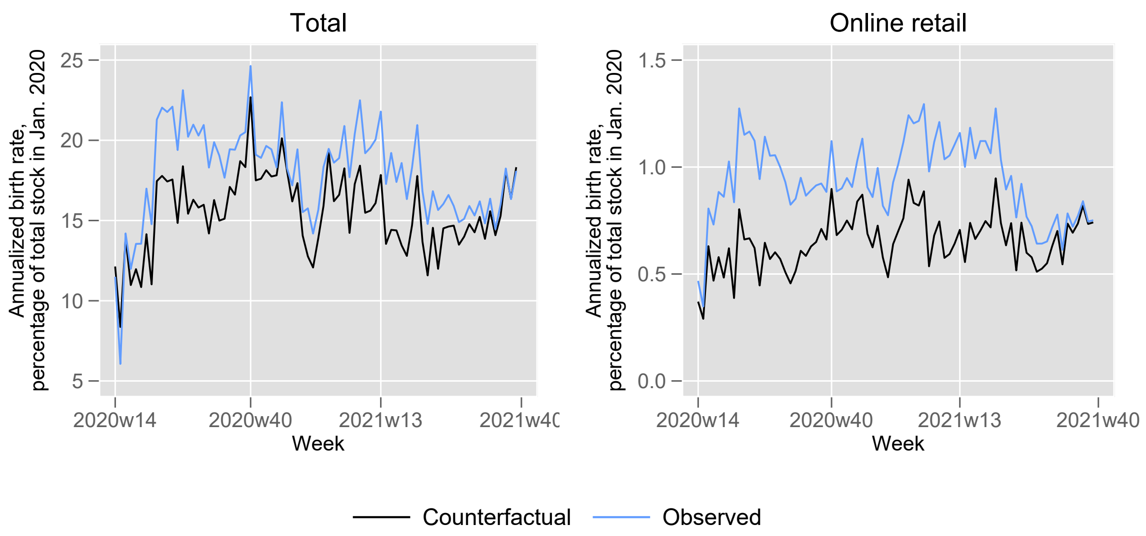
Note: this figure shows the impulse response of the annualized weekly birth rate to a 1% footfall shock using the local projection framework as described in equation (1). The response in online retail is expressed as a contribution to the total birth rate. This annualized birth rate can be converted in weekly firm creation using the total number of firms in Jan. 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.

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.

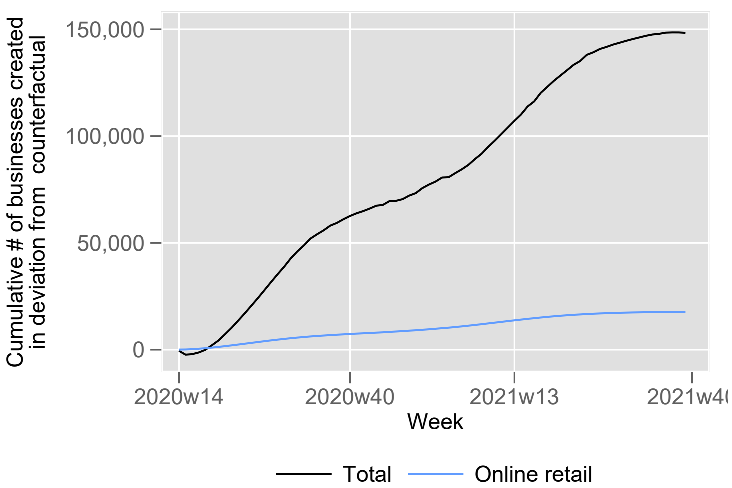
To put these impulse responses into context, Figure 6a shows the observed annualized birth rate (in blue) for the total economy (left-hand side) and the contribution of the online retail sector to this total (right-hand side). It also shows a counterfactual birth rate absent any footfall shock (in black). 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 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 period (2020w14-

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

(a) Annualized birth rate, observed and counterfactual (with no footfall shock), 2020w14-2021w39



(b) Cumulative firm creation in deviation from counterfactual entry with no footfall shock, 2020w14-2021w39



Note: 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, i.e. 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 Jan. 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.

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 relative to pre-COVID that we identified in Figure 3.

In the Appendix D we provide additional robustness checks of our local projections specification including an analysis that utilises the regional component in the data.

3.3.1 Placebo Analysis and Mechanisms

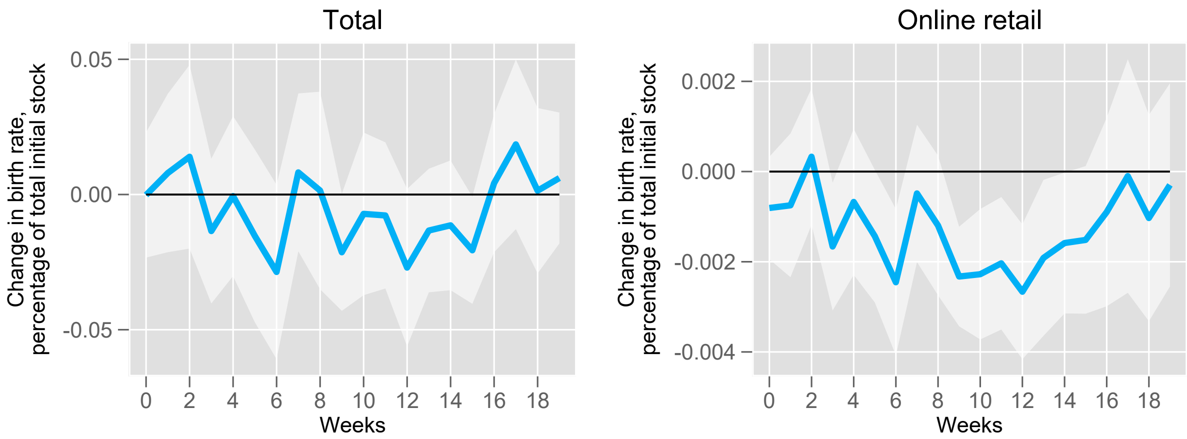
One potential mechanism that explains the relationship between footfall and entry, and, in particular, entry in the online retail sector, is that the pandemic generated a reallocation of demand to lockdown-compliant sectors (e.g. from brick-and-mortar to online retail) and new businesses were created to meet this demand. 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). 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 nine 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.

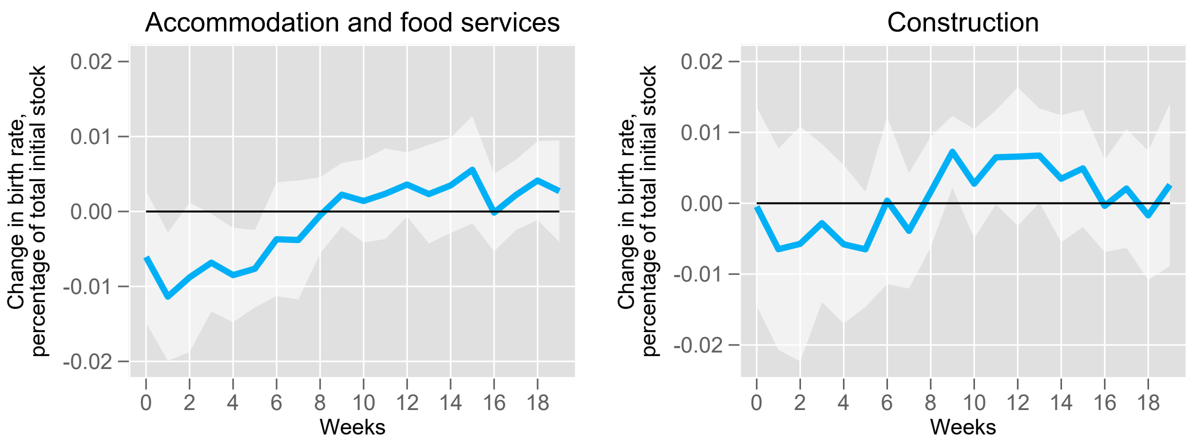
Our second exercise considers industry-level entry responses. If the mechanism runs through a reallocation of demand to lockdown-compliant retail sectors, we would expect: (i) entry in sectors whose activities are unrelated to retail (e.g. construction) to show no response to footfall; and (ii) a decline in entry in sectors that were not lockdown-compliant (e.g. hotels and restaurants). Figure 7b confirms this hypothesis by plotting the impulse

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

(a) Local projection of mobility to parks on the birth rate



(b) Local projection of retail footfall on the birth rate in placebo industries



Note: 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.

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.

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 were forced into doing so by 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.[15] However, we do not rule out this mechanism.

3.4 Entrants are less likely to become employers

We have established the unusual increase in firm entry during the COVID 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

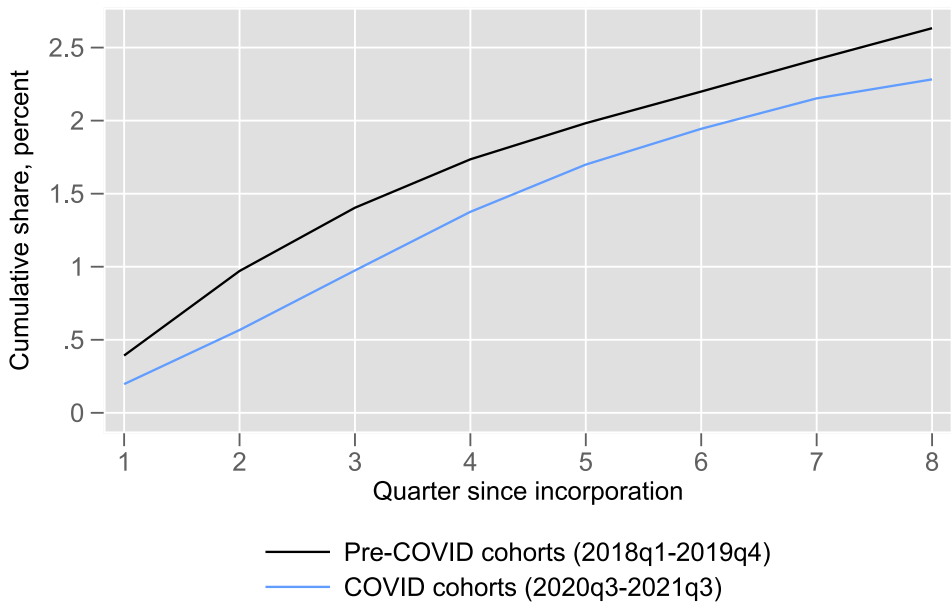
We investigate the probability that COVID cohorts of firms post jobs as they age. To do so, we analyse the cumulative share of all firms incorporated in a quarter (i.e. quarterly cohorts

[15] 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.

of firms) that post a job by quarter since incorporation. We compare these shares for cohorts of firms incorporated during COVID (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 ends in 2022q3, so we observe the final cohort 2021q3 until age 5. 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 two 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.[16]

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.



[16]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 one, i.e. within the first quarter since incorporation.

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 aggregate trends in vacancy postings during the pandemic in Appendix E (see Figure 21). 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 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.

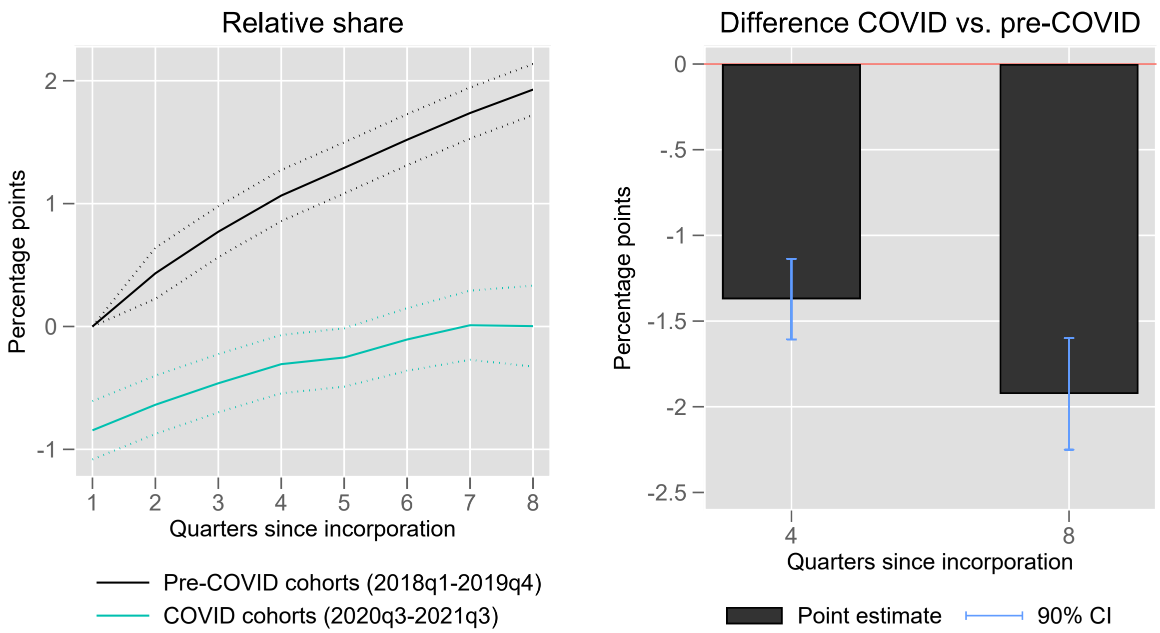
We can remove the average sector time trends from this posting rate by deducting the average posting rates across cohorts (where C is the total number of cohorts in the sample).

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

The age of the firm, a, is simply $q+1-c$.[17] Hence, we can manipulate indices to define

[17] 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, i.e. between their

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 vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

$\tilde{n}_{c,s,a} \equiv \hat{n}_{c,s,a-1+c}$ 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 $i = \text{pre-COVID}, \text{COVID}$).

Figure 9, left panel, plots the age-cohort group fixed effects with 90% confidence interval. We renormalise the coefficient plots such that $\mathrm{FE}_{\text{pre-COVID},1} = 0$. Hence, the values on the

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, i.e. within the first year. This ensures consistency with timings used for our framework in Section 4.

y-axis represents 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 black line suggests that, by quarter 8, there are two percentage points more postings than in quarter 1 for the average cohort pre-COVID.[18]

The green line 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 2p.p. less likely to post a vacancy in Indeed two 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 (i) different levels of posting rates across sectors on average over the full period and (ii) sectors having different dynamics for posting and entry rates both pre and during COVID. We investigate the role of both dimensions in Appendix E.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, i.e. 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

[18]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.

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 25 in Appendix that firms founded by new solo entrepreneurs are significantly less likely than the other groups to a post a vacancy than any other ownership group by 8 quarters after entry. This could contribute to explain the lower probability of firms born during COVID to post on Indeed.

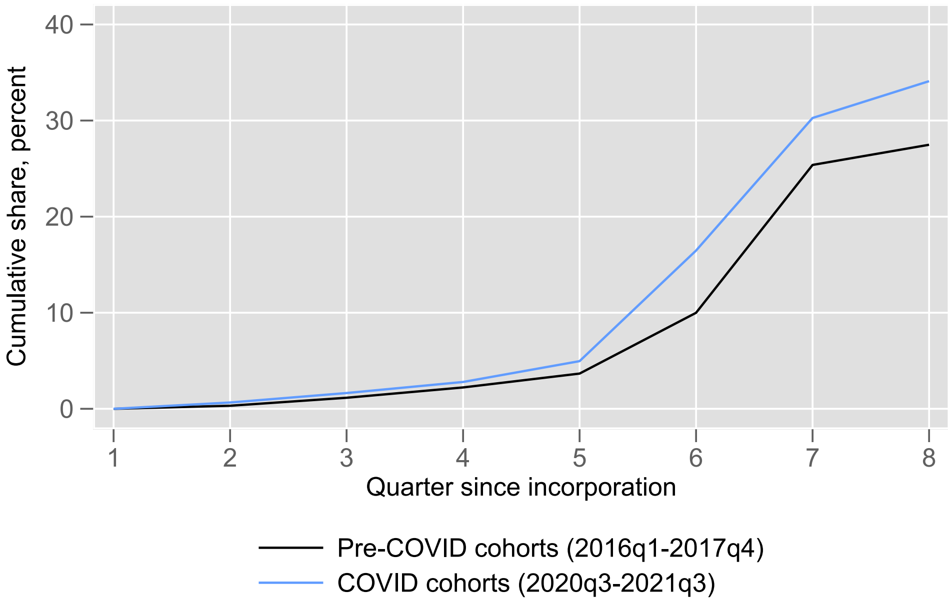
3.5 Entrants are more likely to dissolve

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 vs. COVID). Dissolutions were strongly affected by the 'easement' period in which Companies House stopped registering dissolutions for the first two quarters of 2020 (Appendix F). Consequently we exclude cohorts of firms affected by the easement period. Therefore, we abstract from the 2018q1-2020q2 period and compare dissolutions within the first two years since incorporation for firms born between 2016q1-2017q4 (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 ends in 2022q3, so we observe the final cohort 2021q3 until age 5. 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 two 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.

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.



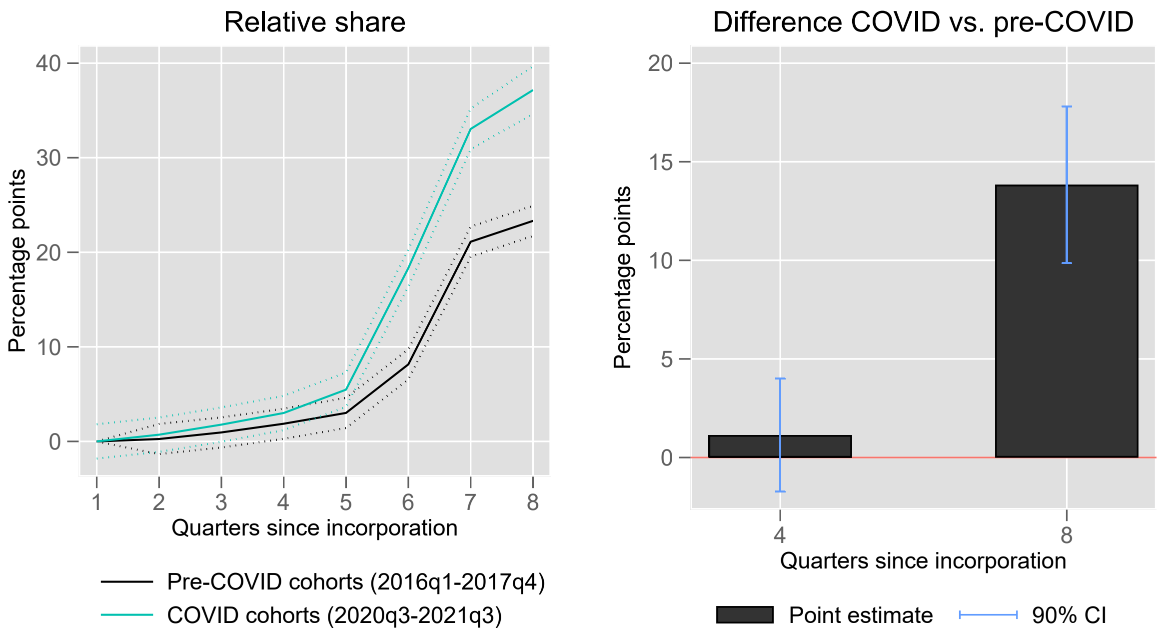
3.5.2 Dissolution probabilities: age-cohort group effects

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.[19]

Figure 11 plots the age-cohort group fixed effects coefficient ($\text{FE}_{i,a}$) from the following regression: $n_{c,s,a} = \text{FE}_{i,a} + \varepsilon_{c,s,a}$, 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 $i = \text{pre-COVID}, \text{COVID}$).

[19] In the Appendix, Figure 26 shows similar life-cycle profiles for dissolutions across all cohorts, except those affected by easement which we exclude, whereas Figure 22 shows clear differences in the shape of the cohort plots across the lifecycle for vacancy postings.

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 vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

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 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

Appendix F; results are unaffected.

As with job postings, the results could reflect changing composition of ownership with the rising share of new solo entrepreneurs. We do find in Figure 29 in Appendix that new solo entrepreneurs dissolve significantly more than most other groups by quarter 8 raising the overall dissolution rate for COVID cohorts.

4 Discussion

Our five facts achieve our first aim of providing 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 Şahin (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_a be the total employment of firms that register to incorporate in period i by the time they reach age a (in quarters). Mechanically, E^i_a can be decomposed into the following product:

$$E^i_a = N^i_0 \times (1 - \delta^i_a) \times p^i_a \times s^i_a. \tag{3}$$

N^i_0 is the number of new firms started in period i and $(1 - \delta^i_a)$ is the number that survive to age a (such that δ^i_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_a is the number of live firms that have hired workers and s^i_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 measures registrations ('incorporations'), rather than the number of new employing firms.

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 $i \in \{\text{pre-COVID, COVID}\}$ denote the two different time periods. We focus on how many jobs new entrants created in aggregate at $a = 4$ and 8 quarters old. To implement our framework, we need to specify each of the four terms in equation (3) for $i \in \{\text{pre-COVID, COVID}\}$ and $a \in \{4, 8\}$. Our data and models allow us to do this for most of the required figures although along some dimensions we only observe proxies. Consequently, our estimates are approximations and uncertainty is greater further into the future when $a = 8$. Nonetheless, the exercise emphasises 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.

Table 1 shows the statistics for each of the four terms in equation (3) for the four cases under consideration. In turn, the table shows the value E_a^i as well as counterfactual job creation by entrants if only one of the four channels changed during the COVID period, with the other three remaining at their pre-COVID levels.

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

		$a=4$	$a=8$
	Pre-COVID		
(1)	N_0^{pre} (annualised)	639,483	
(2)	δ_a^{pre}	0.98	0.73
(3)	p_a^{pre}	0.55	0.84
(4)	s_a^{pre}	3.06	3.45
(5)	$\boldsymbol{E_a^{\text{pre}}}$	**1,060,645**	**1,339,650**
	COVID		
(6)	N_0^{covid} (annualised)	732,725	
(7)	δ_a^{covid}	0.97	0.66
(8)	p_a^{covid}	0.44	0.73
(9)	s_a^{covid}	2.79	3.24
(10)	$\boldsymbol{E_a^{\text{covid}}}$	**872,338**	**1,137,656**
(11)	Difference	-188,307	-201,994
	Contribution of channels to the difference		
(12)	Only N_0^i Changes	154,161	195,332
(13)	Only δ_a^i Changes	-6,075	-122,290
(14)	Only p_a^i Changes	-219,444	-178,280
(15)	Only s_a^i Changes	-95,177	-79,297

To understand our calibration first consider N_0^{pre} and N_0^{covid}. The COVID period corresponds to the 6 quarters from 2020q2-2021q3 where footfall shocks generated excessive entry (per Section 3.2). Therefore a reasonable pre-COVID comparison would be the equivalent 6 quarters just prior to the pandemic – 2018q2-2019q3. During the pre-COVID period 959,225 (639,483 annualised) firms were registered versus 1,099,088 (732,725 annualised) registered in the COVID period. This difference of 139,863 closely matches 148,345 counterfactual difference in entry that would have occurred absent any footfall shocks (Figure 6b).

Second, consider the dissolution rates, $\delta_a^{\text{pre}}$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 immediately pre-COVID are not reliable. Instead, we compute average, realised, dissolution rates for the same pre-COVID and COVID cohorts as in Figure 10.[20]

[20]Note that we take the realised aggregate dissolution rates (as shown in Figure 10) as opposed to using the

Third, consider aggregate hiring rates of live firms, $p_a^{\text{pre}}$and p_a^{covid}. These require more work to ascertain directly. Our Indeed job posting data is advantageous in allowing us to track posting rates of cohorts over time. However, as only around 13% of employers use Indeed, the data severely underestimates aggregate posting rates. From the IDBR we know the number of firms that become employers in a given year. This number tracks new firm registrations with a one year lag (see Appendix Figure 13). Therefore, to approximate p_4^{pre} we use the ratio between new company registrations in 2018 and the number of new employer-firms in 2019 as described in Section 2.2, that we further divide by $(1-\delta_4^{\text{pre}})$ to account for dissolutions. This gives a figure of 55% for p_4^{pre}. This is an overestimate as firms may seek to become employers long after they have incorporated. Now, the evolution of job posting rates in Indeed is broadly representative, even if the level is not (see Appendix E.1). Hence, we then compute the remaining three hiring rates (p_4^{pre},p_4^{covid}, p_8^{covid}) assuming that the ratio between those rates and p_4^{pre}, is the same as the equivalent ratios of the posting rates seen in Indeed for the two different cohorts at different horizons (as in Figure 8).

Lastly, consider firm size given by the number of workers per employer-firm (s_a^i). Our facts offer less direct evidence on this channel, but we can still obtain estimates from the data. From the quarterly IDBR we know that new employer-firms started with 3.06 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 2.79.[21] Our BvD data enables us to calculate mean employer-firm growth in its labour force between quarters 4 and 8, by industry, on average over 2015-2019. We use these industry growth rates, and weight them by the industry composition of entry over 2018q2-2019q3 and 2020q2-2021q3 to get the change in potential firm growth pre and post COVID. We get that firms pre-COVID grow by 13% between its first and second year (since incorporation) suggesting s_8^{pre} is 3.45; similarly we get that COVID firms grow by 16% between its first and second year (since incorporation) suggesting s_8^{post} is 3.24.[22]

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 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 127,404 jobs.

[21] 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.

[22] Interestingly, the sectoral composition of entry suggests that firms born during COVID have a higher growth

Putting these numbers together, row (5) of Table 1 suggests that prior to the pandemic a year's worth of new entrants would create approximately 1.06 million jobs in their first year and an additional 300,000 jobs in their second. By way of comparison, the IDBR data shows that new employers created 1.09 million jobs in 2019. Since a new employer could be a firm that registered more than a year prior, the number of jobs created by new employers in a given year should always exceed those created by those registered in a given year but one would expect a similar order of magnitude. Hence, our framework passes the simple check of being consistent with aggregate statistics.

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 this figures is that cohort of firms born during the pandemic, despite the cohort's size, generated (on annualised basis) 188,307 fewer jobs in their first year than the pre pandemic cohort and 201,994 fewer in their second year; a resp. 18% 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 154,161 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 of a similar order of magnitude. Compounding this is a fall in firms' survival probabilities reducing job creation by about 122,290 in the two years following firm birth.

As a final check we can compare these figures to official statistics on job creation from new employers from the ONS IDBR. See Appendix G for details of the analysis on ONS data (Office for National Statistics (ONS) 2022a). This data has drawbacks compared to our analysis as one cannot track firms through time nor is it clear when the firms that enter the

potential than firms born pre-COVID. However, this growth channel is very small and shutting this channel off suggests that firms created during the pandemic created 36,934 more jobs (3% increase) than firms created pre-pandemic in their second year since incorporation.

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.

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

5 Conclusion

We study firm creation in the UK during the COVID-19 pandemic. We show that firm creation has been countercyclical during the COVID crisis, and this is at odds with nearly all recessions over the last century in the UK. Furthermore, we investigate the mechanisms through which this puzzling fact arises. The emerging picture is 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. Using footfall data, we show that as footfall in brick-and-mortar retailers declines, firm entry in all sectors, especially online retail, expands. Finally, we show that this boom in firm creation does not have the usual employment effects that we would expect in normal times. Using matched data from online job postings, we show that firms created during the pandemic are less likely to post jobs, and are more likely to dissolve. We combine these facts in an aggregation framework, and show that despite the surge in business creation, new firms generated fewer jobs in their first two years than firms born pre-pandemic.

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Appendix for online publication

The Appendix is divided into the following sections:

- **A:** Comparing data on firm entry in the UK.
- **B:** More details on sector composition of entry during the pandemic.
- **C:** Google mobility data for retail footfall.
- **D:** Local projections robustness.
- **E:** Additional results using Indeed data.
- **F:** Additional results using dissolution data.
- **G:** ONS data on firm births and employment.

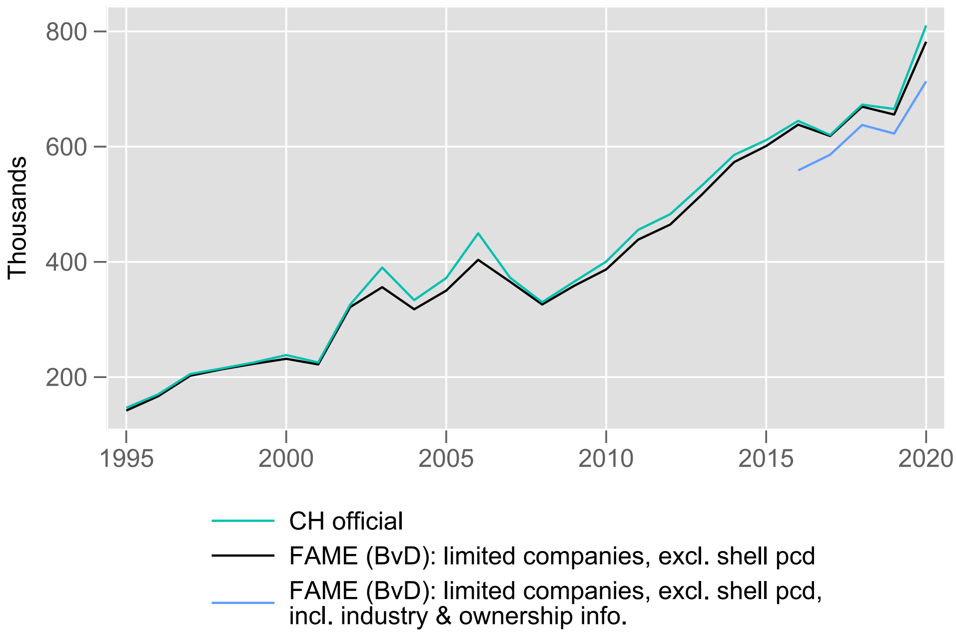
A Comparing data on firm entry in the UK

There are two main sources of firm entry data in the UK. First, the Companies House Register which is available directly from Companies House or indirectly through FAME by BvD. FAME allows construction of a time series of the Companies House register. Second, the inter-departmental business register (IDBR) which is accessible in aggregated form via the ONS's official Business Demography statistics.

A.1 FAME entry data

Figure 12 compare our FAME dataset series (black line) with Companies House (CH) official aggregate numbers (green line). Our final series displays less entrants than CH official numbers because we focus on limited companies and exclude shell postcodes (postcodes with more than 500 registrations in a single day). For some of the analysis, we focus on the sample of firms with industry of activity and ownership (blue line) information. The blue line excludes firms indicating "not elsewhere classified (nec)" for either the industry or ownership information.

Figure 12: Annual number of incorporations by data source



Source: Authors' calculations using Companies House and BvD-FAME.
Note: Shell postcodes (pcd) are postcodes with more than 500 registrations in a day.

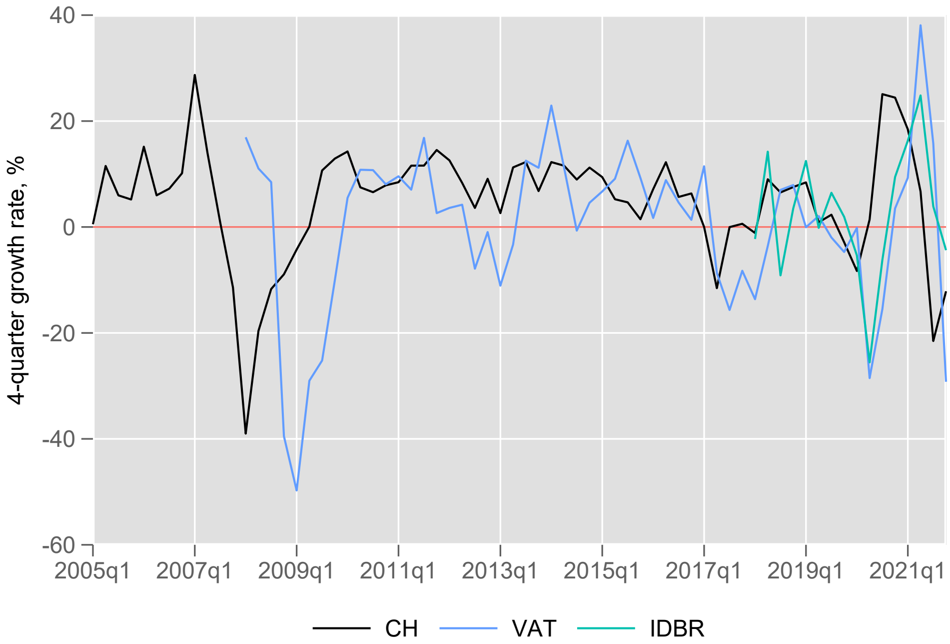
A.2 Inter-departmental Business Register (IDBR) entry data

The Companies House register differs from the Government's internal register of firms called the inter-departmental business register (IBDR). The IDBR only includes firms that are registered for PAYE tax or VAT tax. That is, a firm employs someone (uses PAYE, i.e. registering payroll with UK HMRC) or if it pays VAT (in 2020 the annual turnover threshold for paying VAT was $85,000). Therefore, entries to the IDBR lag entries to the Companies House register and there are fewer entries as not all companies that register with Companies House grow to meet the IDBR thresholds.[23]

Figure 13 compares IDBR quarterly firm entry from the ONS (green line) to Companies House official numbers (black line). Additionally, we include entry on the VAT register from the ONS (blue line) as the IDBR data only starts in 2018q1. We can see that entry in the IDBR usually lags entry in Companies House by roughly four quarters.

[23] Entry to the IDBR is also lagged relative to registering with HMRC for PAYE or VAT, see ONS.

Figure 13: Entry by data source, 4-quarter growth rate 2005q1-2021q3, %

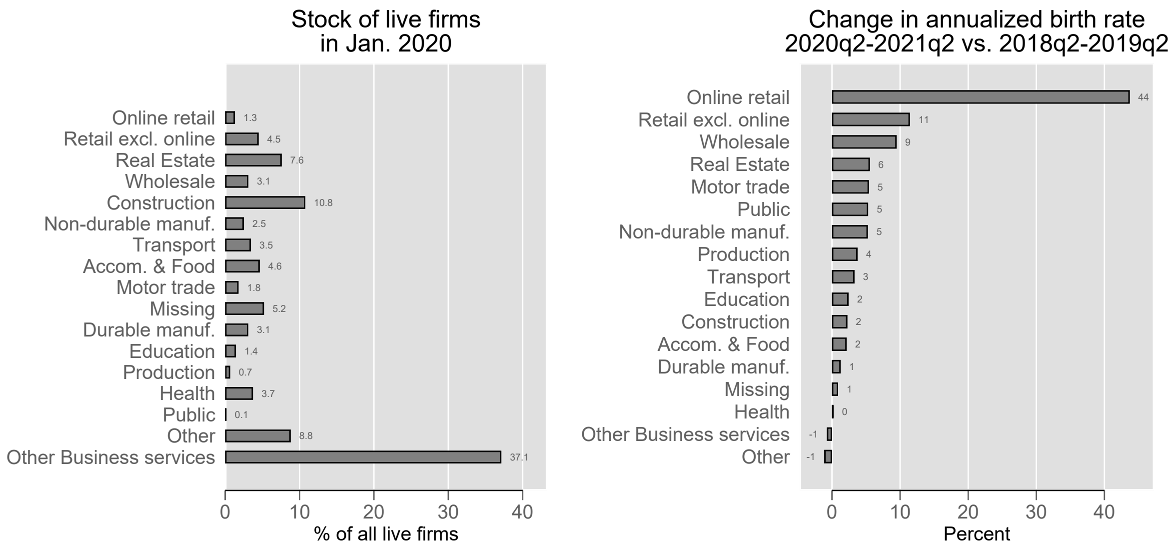


Source: Authors' calculations using Companies House and ONS.

B More details on sector composition of entry during the pandemic

Figure 14 shows that online retail represents less than 2% of total live firms in January 2020. It also shows that online retail is by far the sector with the largest increase in birth rate over the pandemic, relative to pre-pandemic.

Figure 14: Sector contributions to firm entry during COVID, relative to pre-COVID

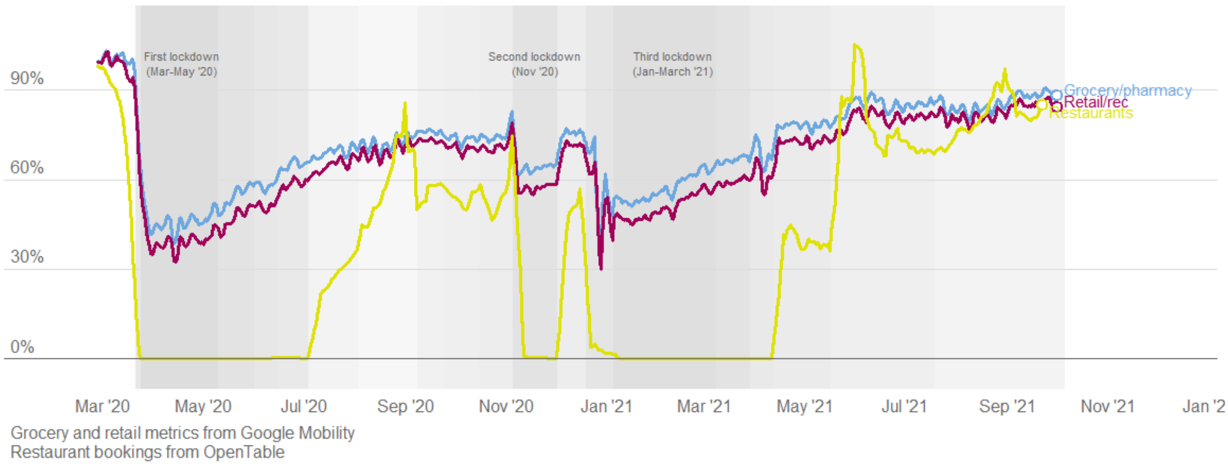


Source: Authors' calculations using BvD-FAME.

C Google mobility data for retail footfall

Figure 15 plots Google mobility data for an example region (London). It plots two of Google's defined place categories (Google Place Category definitions): Grocery & Pharmacy and Retail & Recreation. The remaining categories are Parks, Transit Stations, Residential and Workplaces. The restaurant data is from OpenTable. We measure lockdown intensity using Retail & Recreation footfall data which corresponds to the purple line in the figure. The response of retail footfall is closely correlated with lockdown periods that are indicated by shaded regions.

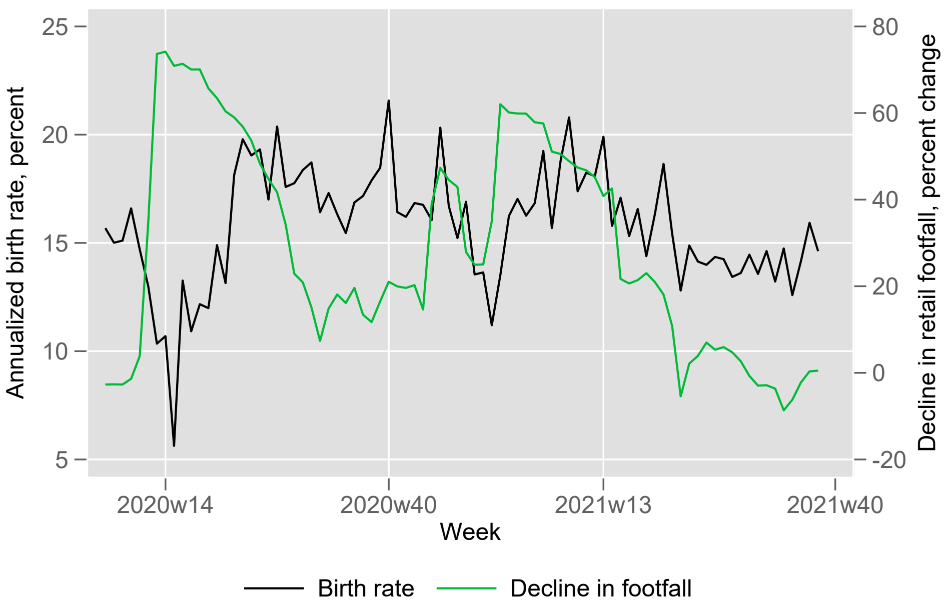
Figure 15: Retail footfall as an indicator of lockdown intensity: London example



Source: Coronavirus (COVID-19) Mobility Report, Greater London Authority (GLA).

Figure 16 shows the relationship between retail footfall and the annualised birth rate since the onset of the crisis in late March 2020. The footfall indicator is expressed as changes relative to the first five weeks of 2020, and the birth rate is measured as entry relative to the stock of firms in January 2020. Initially, there is a sharp decline in the birth rate but this recovers by June. The birth rate stabilizes around 15% after June 2021 (2021w23). The footfall indicator co-moves with birth rates. When there is a decline in footfall, birth rates decrease with a slight lag.

Figure 16: Annualised birth rate and decline in footfall



Source: Authors' calculations using BvD-FAME, Companies House and Google mobility data.
Note: The footfall indicator is expressed in deviation to the median corresponding day of the week during the five week period Jan 3-Feb 6, 2020; we then take the weekly average of these growth rates. Decline in footfall is the negative of the of the mobility trends for places like cafes, restaurants, shopping centers, theme parks, museums, libraries, and movie theaters.

D Local projections robustness

We present robustness results for our local projections. First, we provide additional checks to our main aggregate model. We present IV estimates to address possible endogeneity, and we also present a sensitivity check on our inclusion of lagged dependent variables. Second, we present an alternative model to our aggregate model which exploits the regional panel dimension of our data, such that the source of variation is footfall across regions rather than aggregate variation due to national lockdowns.

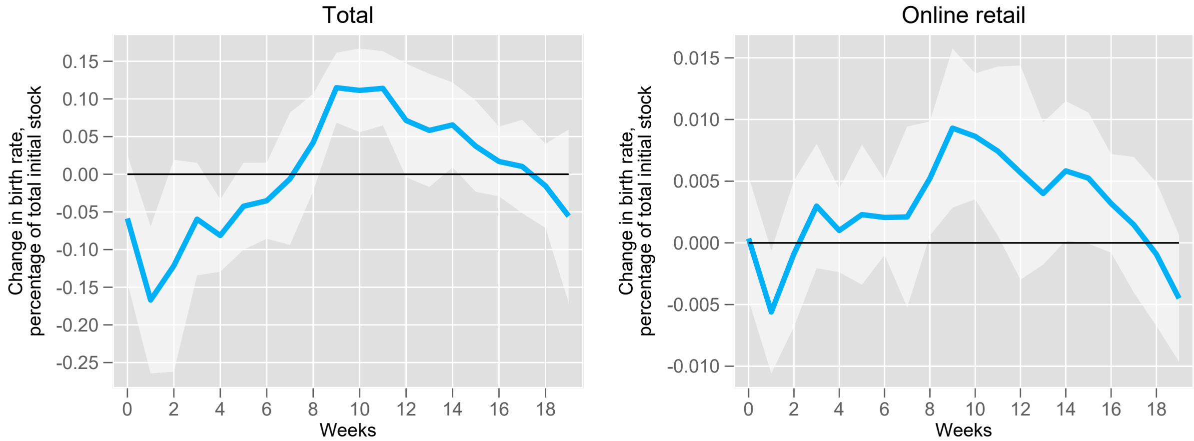
D.1 Robustness results using aggregate model

D.1.1 IV estimation with lockdown stringency

Figure 17 presents results for our main local projections regression where we use instrumental variable estimation. Footfall and entry are often considered endogenous because of reverse causality. Entry can cause new footfall to an area, e.g. in the case of a new retail park, or footfall can cause entry as supply responds to demand indicated by people visiting an area. Our main justification for overcoming this endogeneity is that the high-frequency of our data limits the problem. There are time lags in securing a premises, acquiring stock and advertising to customers which suggest it is unlikely for entry to cause footfall within a week. This would not be the case if we had annual data.

An alternative strategy to address this problem is to use lockdown stringency measures as an instrument. Then the entry-causing-footfall channel becomes: new registration generate a change in footfall, which in turn generates a change in disease transmission, and finally a change in lockdown policy. Given the infection lags with the disease this seems implausible to occur within a week. Additionally, it is possible that some of the variation in footfall that we see is due to other shocks unrelated to the pandemic. The IV estimator would also address this concern. We test for the weak instrument problem and find an F-stat exceeding 10.

Figure 17: Instrumenting footfall with a lockdown stringency index

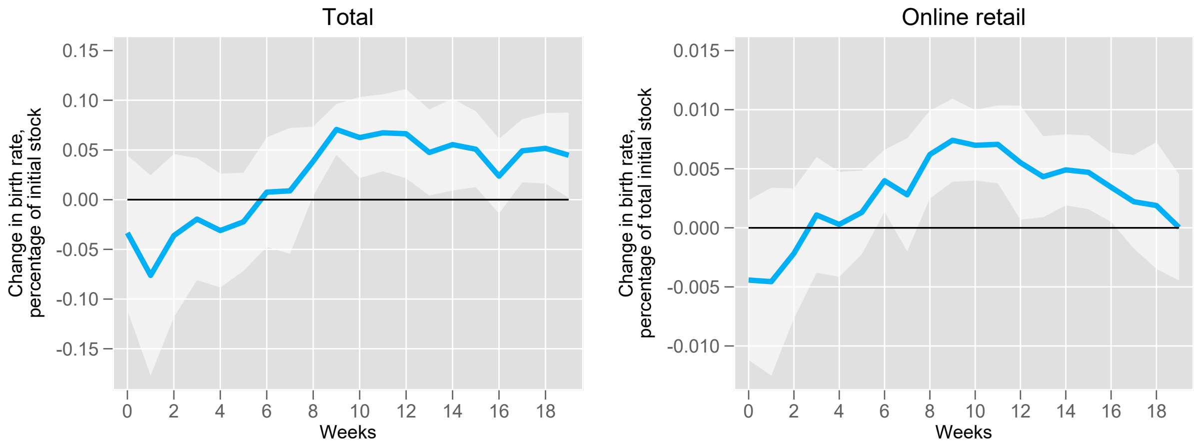


Note: this figure shows the impulse response of the annualized weekly birth rate to a 1% footfall shock using the local projection framework as described in equation (1), and instrumenting footfall with a lockdown stringency index. The response in online retail is expressed as a contribution to the total birth rate. Standard errors are clustered at the week level. The light shaded area shows the 90% confidence interval.

D.1.2 Lagged Dependent Variables Sensitivity Check

Figure 18 reports the impulse responses to a negative retail footfall shock for our main model without lagged dependent variables included. They do not affect our conclusions.

Figure 18: Omitting the lagged dependent variables



Note: 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), but excluding the lags of the dependent variable. The response in online retail is expressed as a contribution to the total birth rate. Standard errors are clustered at the week level. The light shaded area shows the 90% confidence interval.

D.2 Alternative local projections using regional variation

In our main analysis we focus on aggregate data. There is also a regional dimension to our data. However, adding this panel dimension to our model raises econometric challenges. Dynamic panel models with lagged dependent variables and fixed effects are subject to bias. For completeness, we present the results exploiting a regional dimension below. Our results do not vary greatly across specifications.[24]

The regional specification of the model is as follows, where subscript k denotes region:

$$\text{Birth rate}_{k,t+h} = \sum_{j=0}^{4} \gamma_j^h \text{Footfall}_{k,t-j} + \sum_{j=1}^{4} \eta_j^h \text{Birth Rate}_{k,t-j} + FE_k + \varepsilon_{k,t}.$$

The explanatory variable birth rate is:

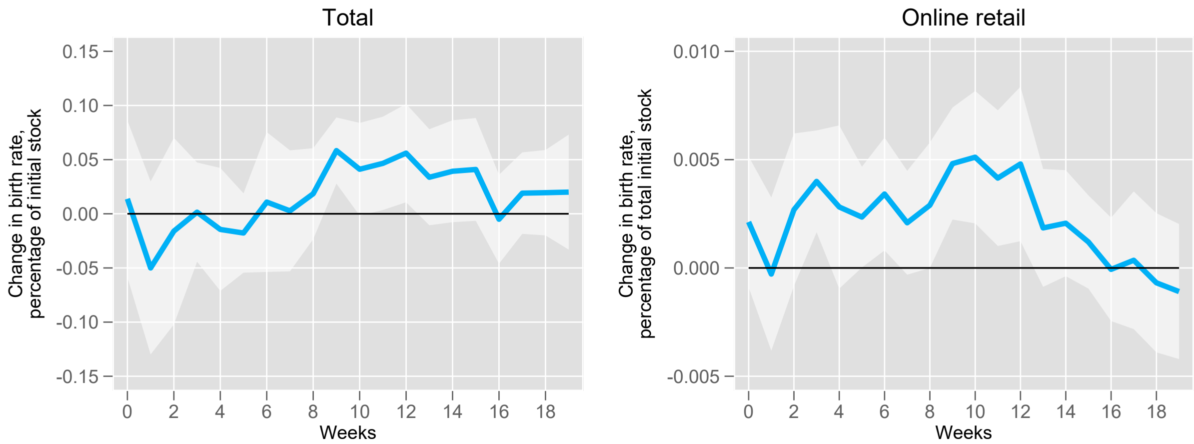
$$\text{Birth Rate}_{k,t} \equiv \frac{\text{Entry}_{k,t}}{\text{Total Firms in Jan 2020}_k}.$$

[24] In order to conduct the regional analysis, we match Companies House incorporations to the local authority where the firm is located using an ONS tool (ONS region lookup tool). Similarly, the Google mobility data is divided into 381 specific regions which we match using the same tool. This procedure follows Fetzer (2021).

The variable $\text{Entry}_{k,t}$ measures the number of entrants in location k in week t. We hold the definition of the denominator fixed as the regional total across all firm types. We do this for comparability and to avoid over-weighting regions with small initial levels of firms in online retail.

Figure 19 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 with a peak of 0.075% after 12 weeks. 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 (0.005 out of 0.05) of the increase in the total birth rate, despite accounting for 2% of the pre-pandemic stock.

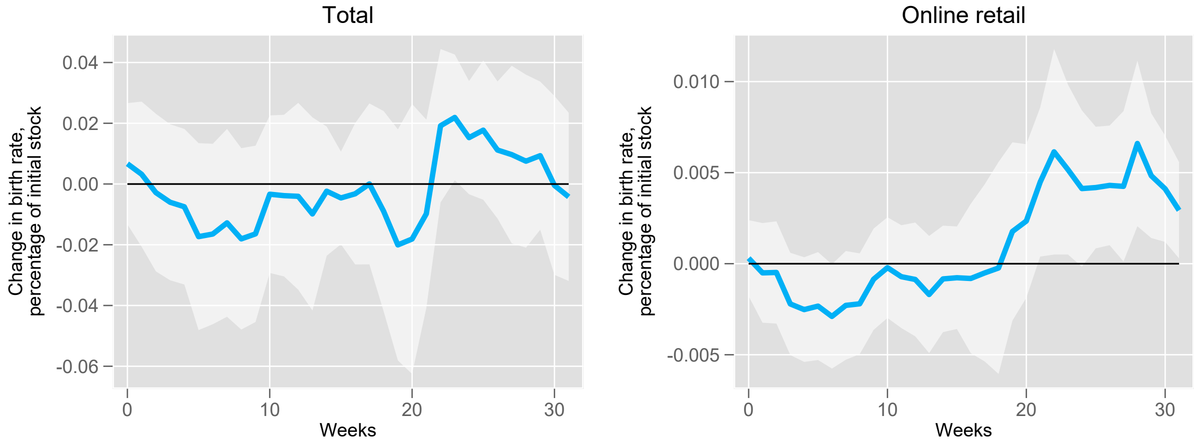
Figure 19: Estimated coefficient following a 1% decline in footfall from regional data



Note: these figures show the impulse response of the annualized weekly birth rate to a 1% footfall shock using the following extended version of the local projection framework described in equation 1 in the paper, where k denotes region: $\text{Birth rate}_{k,t+h} = \sum_{j=0}^{4} \gamma_j^h \text{Footfall}_{k,t-j} + \sum_{j=1}^{4} \eta_j^h \text{Birth Rate}_{k,t-j} + FE_k + \varepsilon_{k,t}$. The response in online retail is expressed as a contribution to the total birth rate. Standard errors are two-way clustered at the county and week level. The light shaded area shows the 90% confidence interval.

Figure 20 shows the impulse responses from our local projection analysis when we include time fixed effects. Relying solely on variation between regions, we find a delayed effect on entry, and the impact of regional footfall shocks on entry is weaker. Therefore, the spillovers across regions that arise from a national lockdown (in other words, the results absent time FE) strengthen the entry response. We do not want to purge these spillover effects since they are an important part of the national lockdown.

Figure 20: Estimated coefficient following a 1% decline in footfall using regional variation (including time fixed effects)



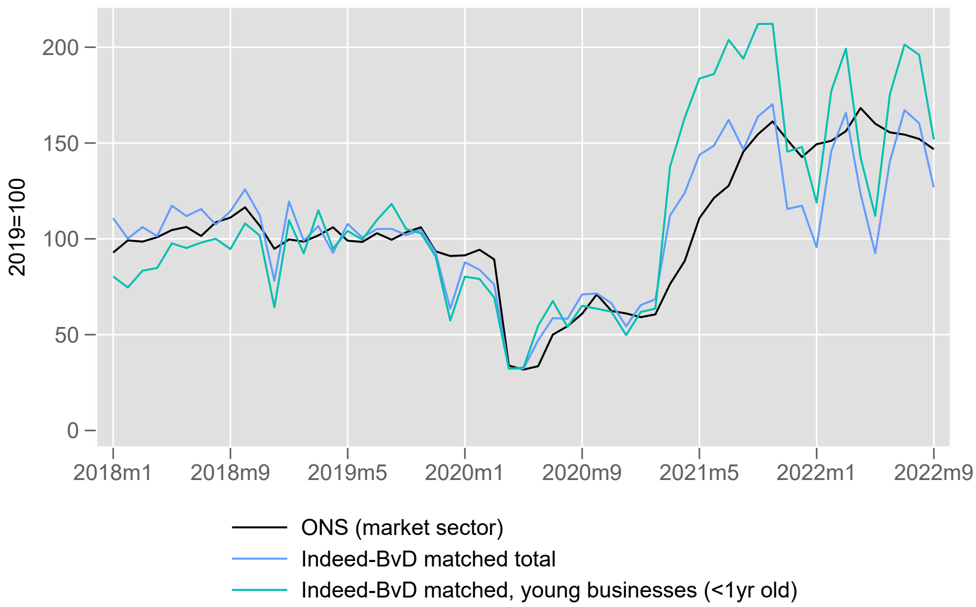
Note: these figures show the impulse response of the annualized weekly birth rate to a 1% footfall shock using the following extended version of the local projection framework described in equation 1 in the paper, where k denotes region: Birth rate$_{k,t+h} = \sum_{j=0}^{4} \gamma_j^h$Footfall$_{k,t-j} + \sum_{j=1}^{4} \eta_j^h$Birth Rate$_{k,t-j} + FE_k + FE_t + \varepsilon_{k,t}$. The response in online retail is expressed as a contribution to the total birth rate. Standard errors are two-way clustered at the county and week level. The light shaded area shows the 90% confidence interval.

E Additional results using Indeed data

E.1 Indeed postings versus ONS vacancy survey

Figure 21 shows that our job postings data from Indeed is closely related to vacancies in the ONS Vacancy Survey (Office for National Statistics (ONS) 2023). This figure shows ONS vacancies and Indeed postings relative to the 2019 average. The Indeed vacancies are for the sub-sample of Indeed postings that match with new firm creation in Companies House. Overall the data shows a sharp decline in job postings from the onset of the pandemic in the first quarter of 2020, and a recovery from Spring 2021. Note that the recovery is stronger in Indeed data relative to the ONS Vacancy Survey. This is because the ONS Vacancy Survey does not include newly incorporated firms, and, as we can see on the figure, the recovery in job postings was strongest for young businesses in Indeed (incorporated within the year of posting the position).

Figure 21: ONS vacancies vs. Indeed job postings, by posting date, 2019=100

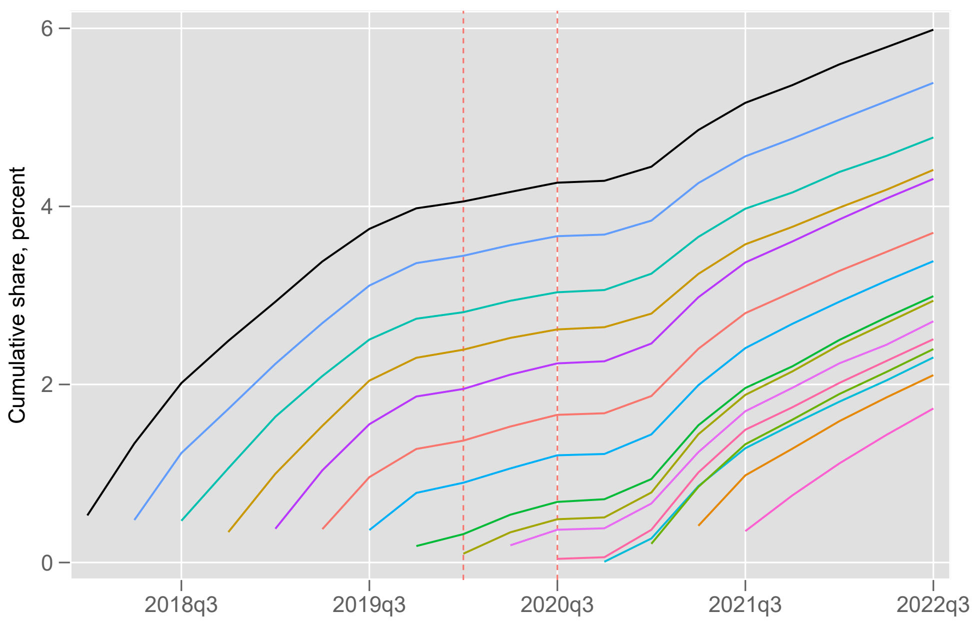


Source: Authors' calculations using BvD-FAME, Indeed and ONS Vacancy Survey data monthly experimental data.

E.2 Job postings cohort analysis

Figure 22 plots the cumulative share of firms posting a vacancy for 15 cohorts of firms. That is each quarterly cohort between 2018Q1 and 2021Q3. Consider the first cohort, indicated by the black line that begins in 2018Q1 at 0.5% cumulative share. This shows that of firms born in 2018Q1 roughly 0.5% of them post a vacancy in their first quarter, rising to 1% in their second quarter, and 2% by three quarters in 2018Q3. By the time that cohort of firms is 15 quarters old, in 2022Q3, 6% of the cohort have posted a job. The figure shows that there are quarterly fixed effects in vacancy postings. For example, in 2021Q3 the curves flatten across all cohorts. And, all cohorts flatten around the middle period when COVID began.

Figure 22: Cumulative share of firms posting a vacancy by quarterly cohorts of incorporation



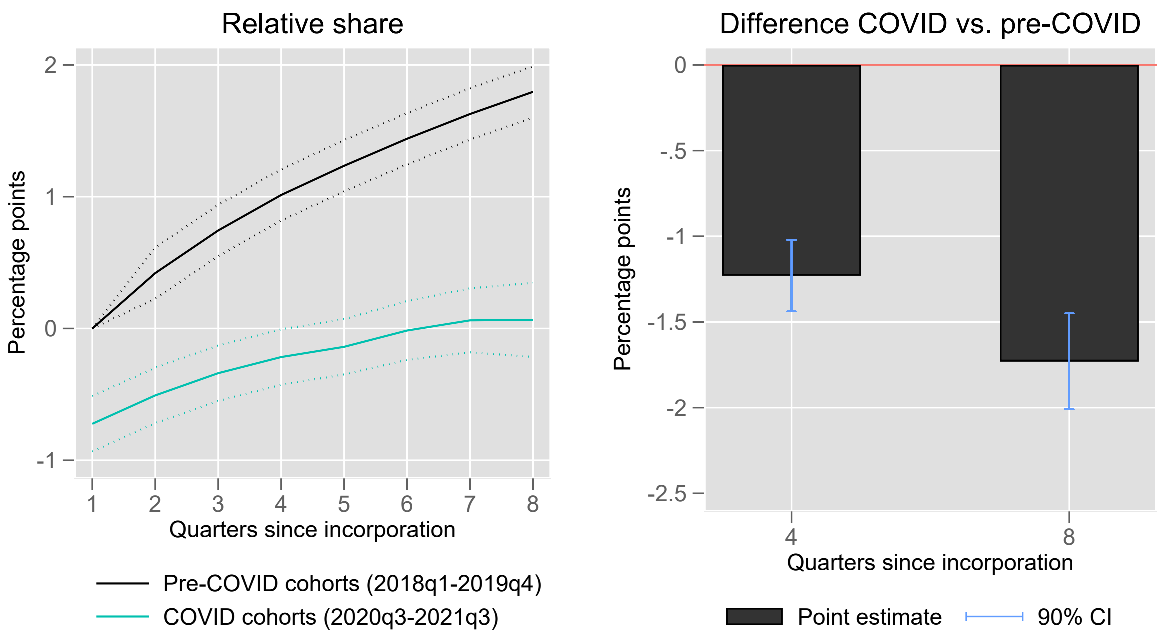
Source: Authors' calculations using matched Indeed and BvD-FAME data. Note: cohorts born before the first red vertical line (2020q1) are firms born pre-COVID, firms born after the second red vertical line (2020q3) are born during COVID-19 (post march 2020).

To control for aggregate trends in vacancy postings, in the main text we do not present this average but a transformed version controlling for sector-time average effects. We plot the age-cohort group fixed effects in Figure 9.

E.2.1 Job postings regressions weighted by cohort size

Note that the regression presented in the main paper is unweighted, meaning each cohort of firms by sector has the same weight not taking into account the cohort size and changing sector composition. We plot in Figure 23 results from a weighted regression using cohort size. This captures compositional effects stemming from different posting dynamics across sectors. The results are very similar to the unweighted ones.

Figure 23: Cumulative share of firms posting a vacancy by quarter since incorporation: average over cohorts born pre-COVID and during COVID, weighted regression

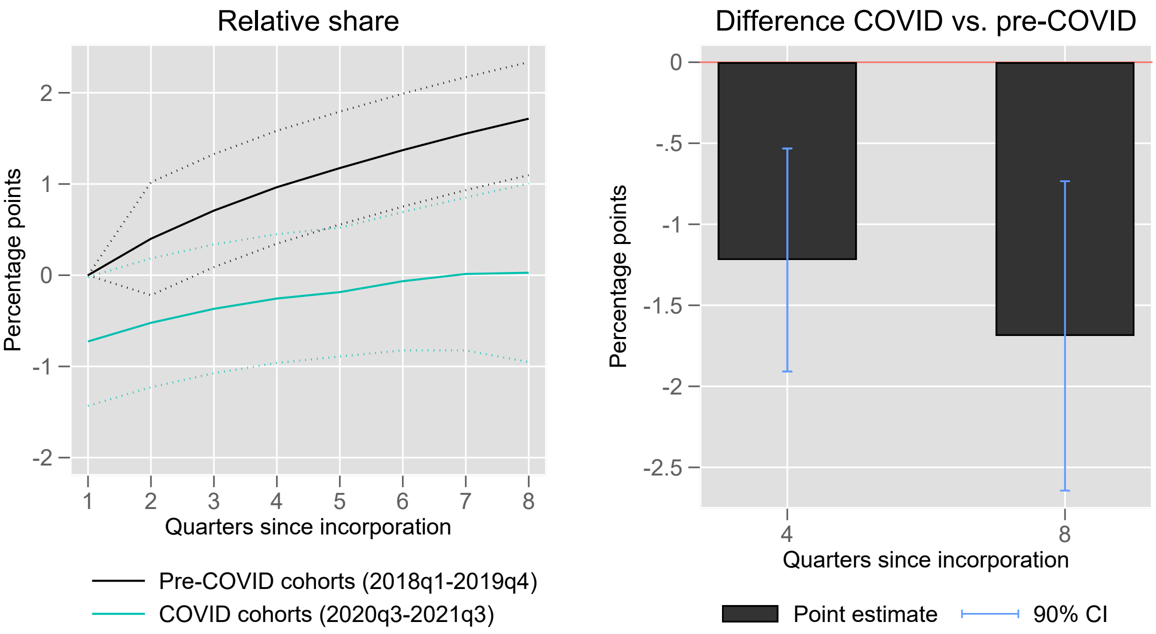


Note: The figure on the left-hand side plots the age-cohort group fixed effects from a weighted regression using the demeaned cumulative share of posting in Indeed in each quarter by 2-digit sector. We use the cohort size as weight. The figure shows age effects by cohort group, relative to the pre-COVID cohort in their first quarter, and for an average cohort (absent sector-time trends). Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

E.2.2 Job postings regressions excluding sectoral dimension

Figure 24 shows results using aggregate data. That is, pooling regressions at the quarterly cohort and not sectoral level. This captures compositional effects stemming from different posting dynamics and different average levels of postings across sectors. Results remain broadly unchanged.

Figure 24: Cumulative share of firms posting a vacancy by quarter since incorporation: average over cohorts born pre-COVID and during COVID, aggregate regression



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. The figure shows age effects by cohort group, relative to the pre-COVID cohort in their first quarter, and for an average cohort (absent time trends). Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

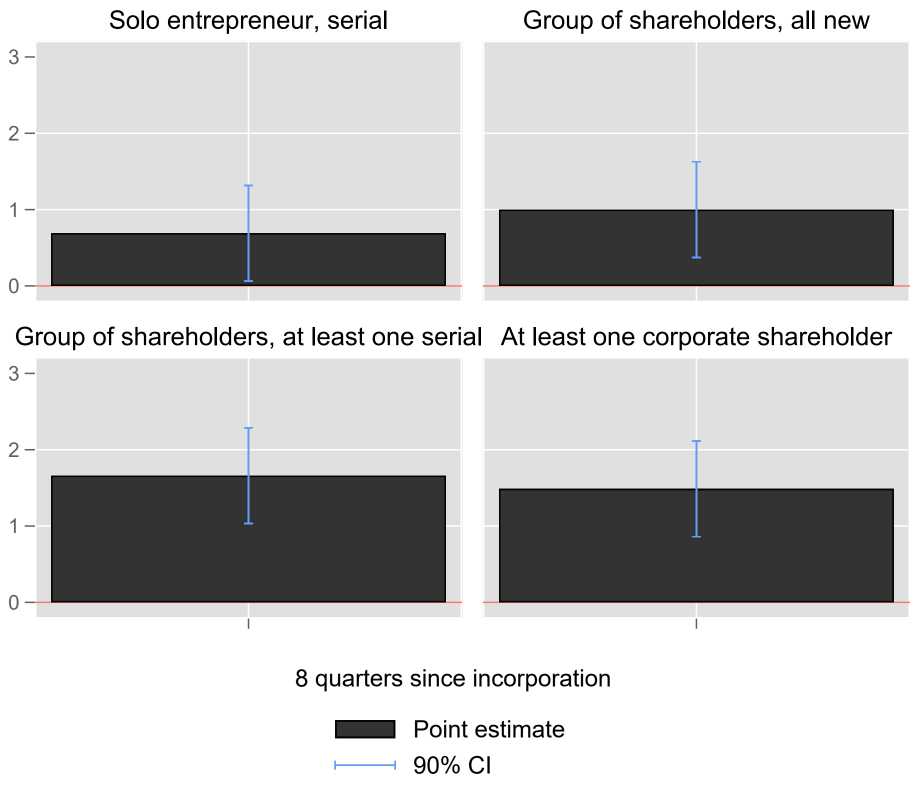
E.2.3 Job postings probabilities by ownership type

Implicit in our results is the changing composition of cohorts by type of ownership over time. Figure 25 plots the age fixed effects coefficient by type of ownership ($\text{FE}_{o,a}$). The results are from the following regression:

$$\tilde{n}_{c,o,a} = \text{FE}_{o,a} + \varepsilon_{c,o,a} \quad \text{with} \quad \tilde{n}_{c,o,a} \equiv \tilde{n}_{c,o,a} = n_{c,o,a-1+c} - \frac{1}{C}\frac{1}{O}\sum_{c} n_{c,o,q} \tag{4}$$

with o the ownership type as defined in Section 2.1. We then get the age-ownership fixed effect for age eight, and compare each groups to the new solo entrepreneur group for the same age. The results show that, controlling for aggregate time trends, all ownership types post significantly more than new solo entrepreneurs at quarter 8. This suggests that the rising share of new solo entrepreneurs could contribute to a lower probability to post on Indeed for firms born during COVID as opposed to pre-COVID.

Figure 25: Share of firms of posting a vacancy 8 quarters after incorporation, by ownership type, in deviation from new solo entrepreneurs



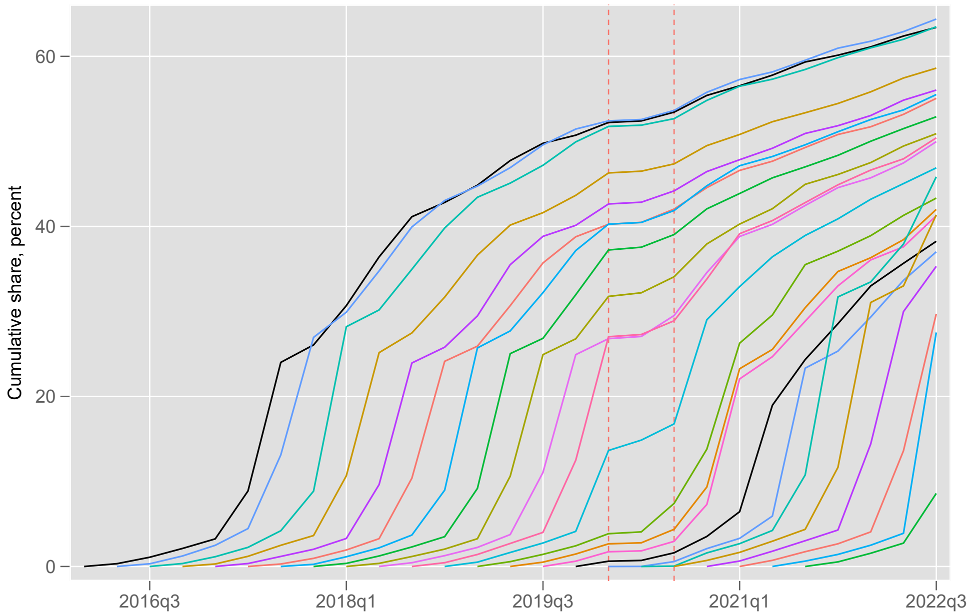
Note: The figure plots the age-ownership fixed effects from a regression using the demeaned cumulative share of posting in Indeed in each quarter by cohort and type of ownership. It plots the ownership effect relative to the new solo entrepreneur cohort group at quarter 8, and shows the 90% confidence interval around the difference.

F Additional results using dissolution data

F.1 Dissolutions cohort analysis

Figure 26 plots the cumulative share of firms in a cohort dissolving for 23 cohorts of firms. This is each quarterly cohort from 2016Q1 to 2021Q3. This share is strongly affected by the easement period in which Companies House suspended dissolving companies from March to September 2020. For this reason, we compare cumulative shares for cohorts of firms excluding the easement period, that is 2016q1-2017q4 vs. 2020q3-2021q3.

Figure 26: Cumulative share of firms dissolving by quarterly cohorts of incorporation

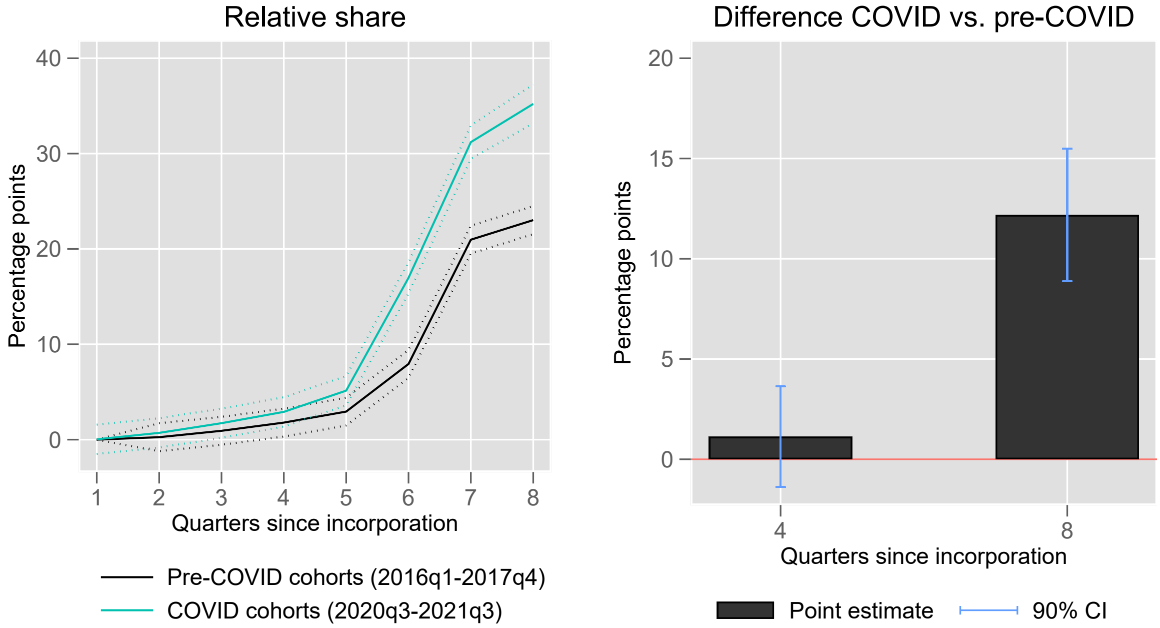


Source: Authors' calculations using BvD-FAME. Note: The red vertical lines denote the easement period 2020Q1 and 2020Q3.

F.1.1 Dissolution regressions weighted by cohort size

Note that the regression presented in the main paper is unweighted, meaning each cohort of firms by sector has the same weight not taking into account the cohort size and changing sector composition. In Figure 27 we plot results from a weighted regression using cohort size (number of firms). This captures compositional effects stemming from different posting dynamics across sectors. The results are similar to our unweighted regressions.

Figure 27: Cumulative share of firms dissolving by quarter since incorporation: average over cohorts born pre-COVID and during COVID, weighted regression

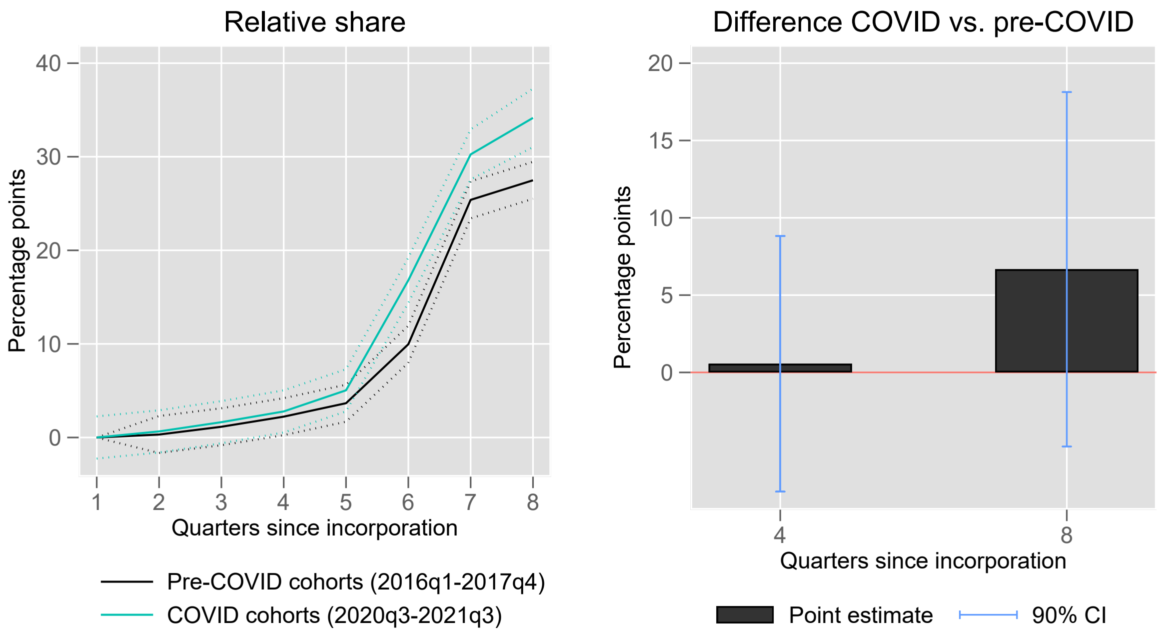


Note: The figure on the left-hand side plots the age-cohort group fixed effects of a weighted regression of the cumulative share of dissolving in each quarter by 2-digit sector on age-cohort group fixed effects, using the cohort size (number of firms) as weight. The figure shows age effects by cohort group, relative to the pre-COVID cohort in their first quarter, and for an average cohort (absent sector effects). Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

F.1.2 Dissolution regressions excluding sectoral dimension

Figure 28 shows results using aggregate data where we pool sectors together. That is, regressions at the quarterly cohort and not sectoral level. This captures compositional effects stemming from different posting dynamics and different average levels of postings across sectors. Our results remain broadly unchanged.

Figure 28: Cumulative share of firms dissolving by quarter since incorporation: cohort analysis pre/post COVID, regression on aggregate data

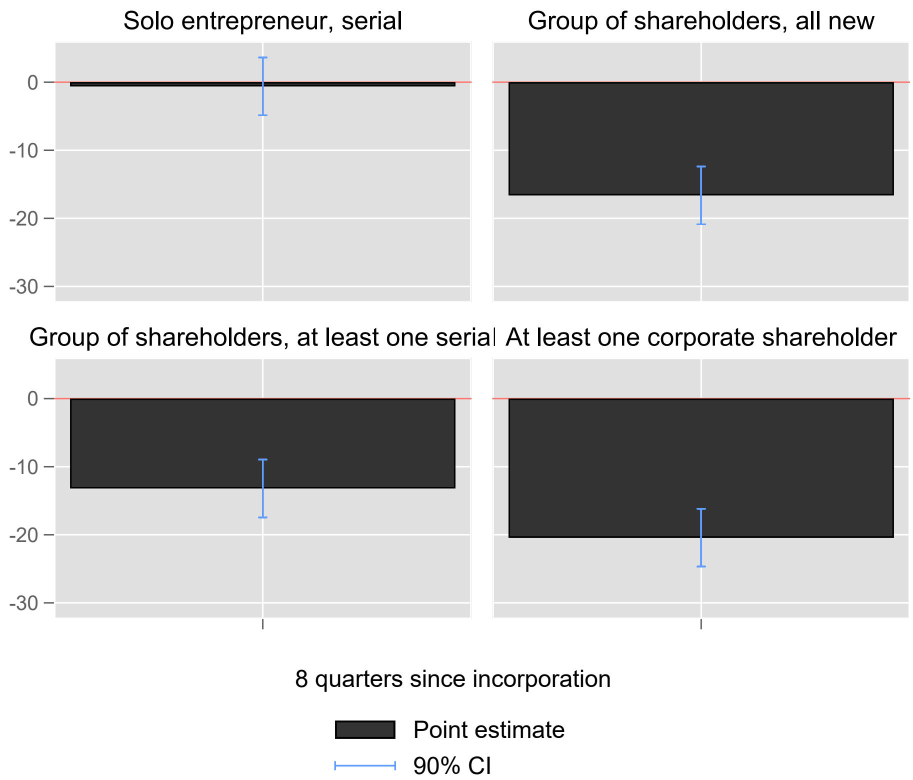


Note: The figure on the left-hand side plots the age-cohort group fixed effects of a regression of the cumulative share of dissolving in each quarter on age-cohort group fixed effects. The figure shows age effects by cohort group, relative to the pre-COVID cohort in their first quarter. Dotted lines plot the 90% confidence intervals. The figure on the right-hand side compares the coefficients for COVID vs. pre-COVID cohorts at quarters 4 and 8, and shows the 90% confidence interval around the difference.

F.1.3 Dissolution probabilities by ownership type

All these results implicitly capture the changing composition of cohorts by ownership type. Figure 29 plots the average share of firms that dissolve by quarter 8 since incorporation by type of ownership, in comparison to the new solo entrepreneur group for the same age. The plots show that all ownership types, except serial solo entrepreneurs, dissolve significantly less than new solo entrepreneurs by quarter 8. This supports the hypothesis that the rising share of new solo entrepreneurs during COVID could contribute to a higher dissolution probabilities after the pandemic began.

Figure 29: Cumulative share of firms dissolving 8 quarters since incorporation, by ownership type, in deviation from new solo entrepreneurs



G ONS data on firm births and employment

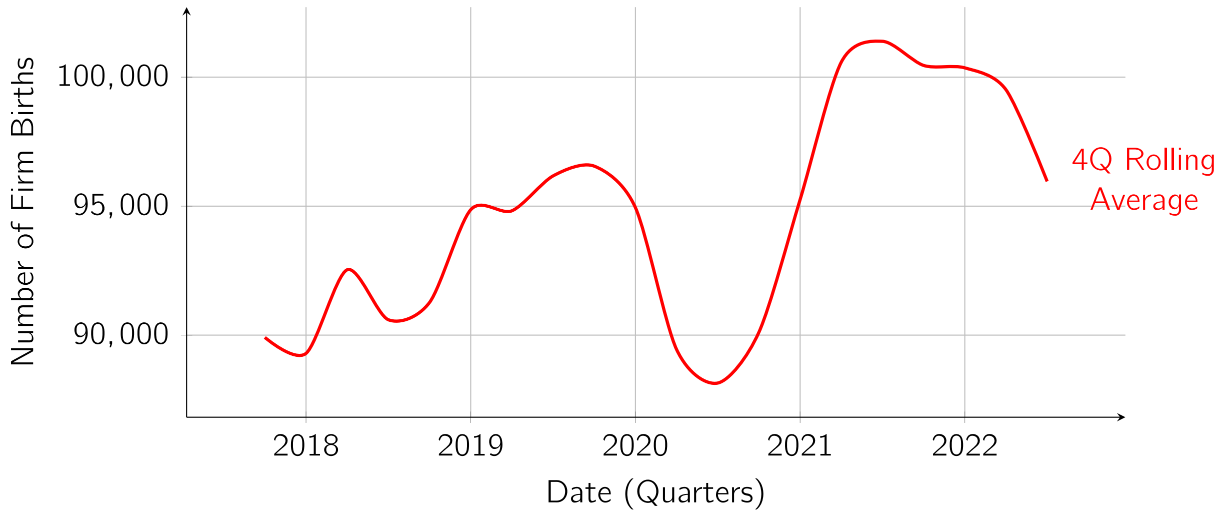
Office for National Statistics (ONS) (2022a) presents recent data on employment created by firm births in the UK, where a firm birth is recorded as the day a firm is added to the Inter-Departmental Business Register (IDBR).

There are two notable differences to our analysis of firm creation from Companies House registrations. First, firm entry in the IDBR denotes entry of firms at a later stage of their life cycle as opposed to firm registration in Companies House, as explained in Appendix A. Second, the ONS data from the IDBR only records employments from firm births, at birth. This is what we refer to as the extensive margin, which consists of the number of births multiplied by size at birth. It does not account for the growth of firms once they are born (intensive margin), which we measure in our accumulation exercises.

Despite these differences, the broad message from this data is consistent with our findings.

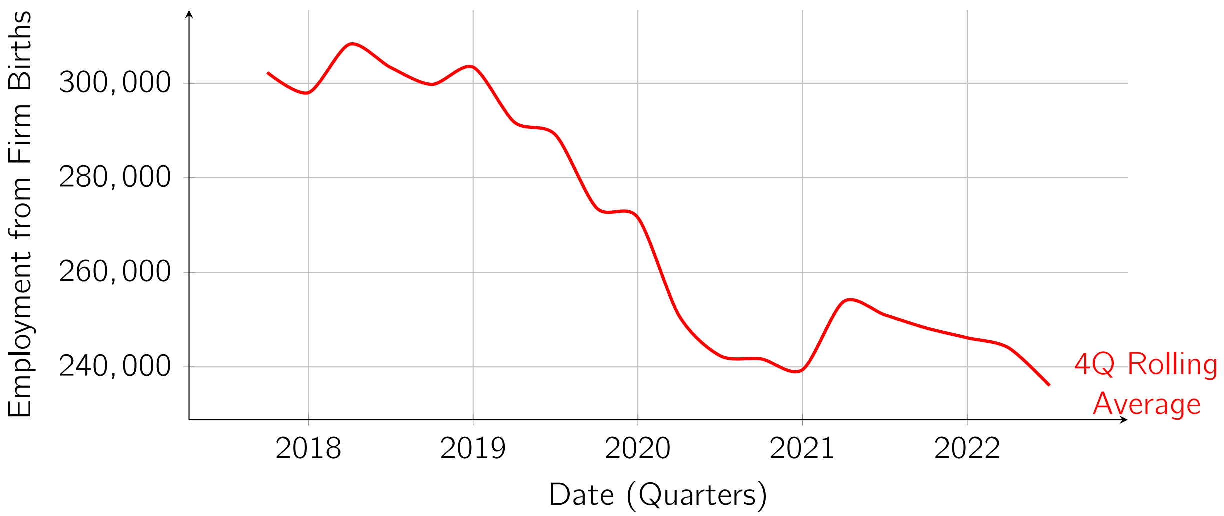
There is booming firm creation following a sharp decline after the COVID-19 outbreak (Figure 30 and Figure 32), however employment from firm creation is worse after the pandemic begins than pre-pandemic (Figure 31 and Figure 33).

Figure 30: Number of Firm Births
Source: Authors' calculations from ONS 'Business Demography Quarterly Experimental Statistics'



Plot shows number of firms added to the the Inter Departmental Business Register (IDBR) ("firm births"). We plot a four quarter rolling average because seasonality in the raw data masks the trend.

Figure 31: Employment from Firm Births
Source: Authors' calculations from ONS 'Business Demography Quarterly Experimental Statistics'

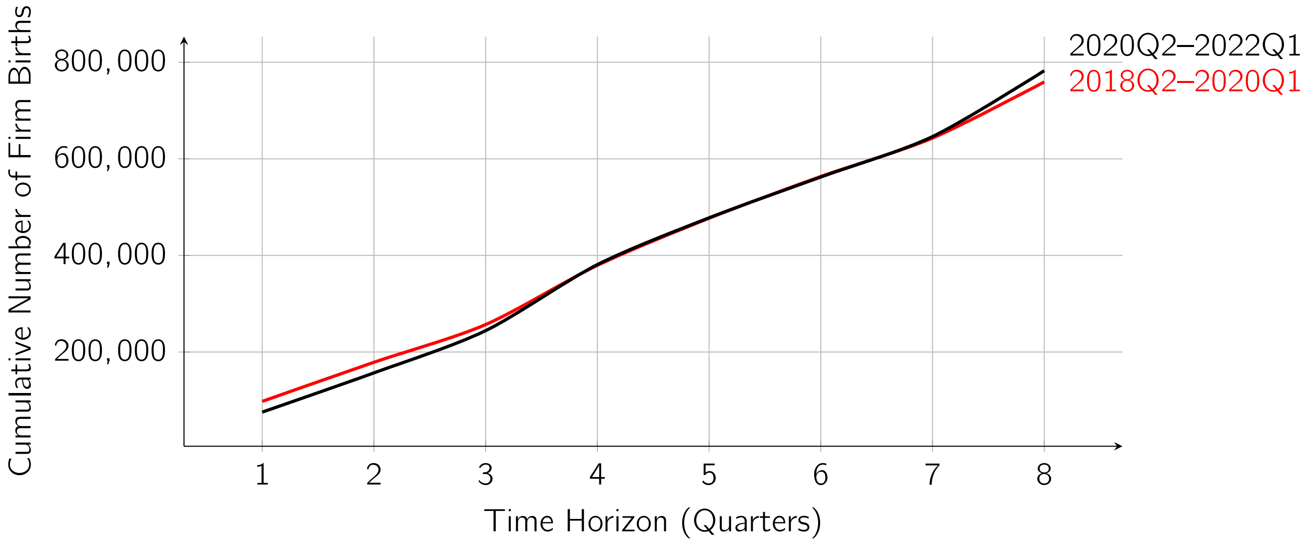


Plot shows total employment by new firms added to the the Inter Departmental Business Register (IDBR). We plot a four quarter rolling average because seasonality in the raw data masks the trend.

G.1 Cumulative Analysis

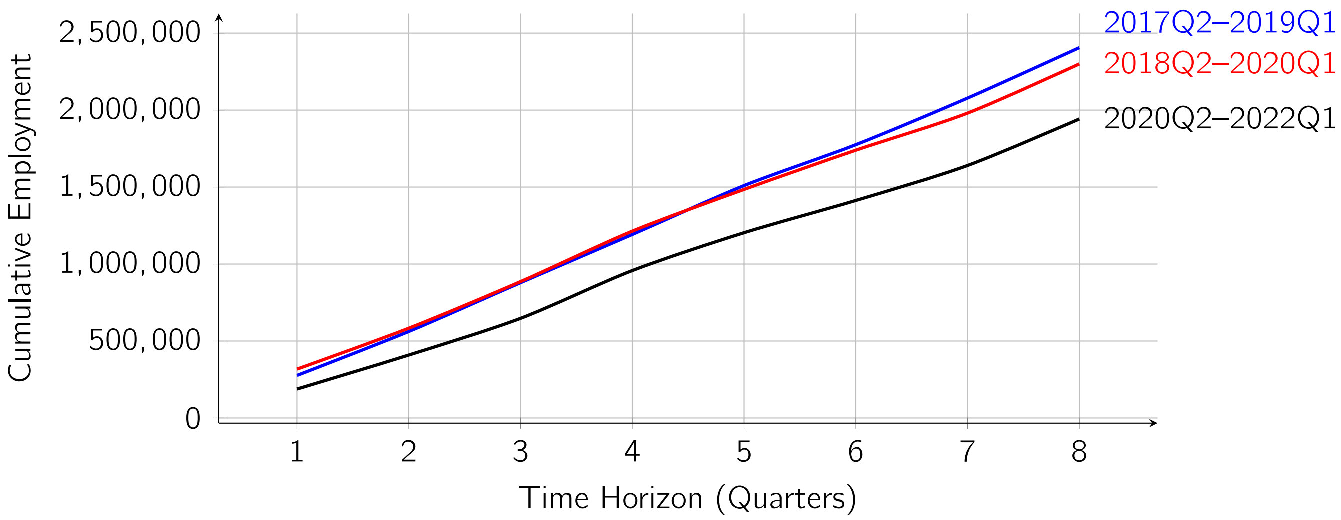
Figure 32 shows an initial shortfall in firm creation over the first three quarters of the pandemic, but then a strong catch-up such that after four quarters cumulative firm creation is approximately the same as pre-pandemic. Figure 33 shows that cumulative employment from firm creation after COVID-19 (2020Q2) is significantly lower than the two pre-COVID reference periods. The data shows cumulative employment from firm creation only (extensive margin), rather than including subsequent within-firm expansion (intensive margin) in the quarters following birth. The underlying data shows that from 2018Q2-2020Q1 there are 2,299,879 employments from firm births, whereas from 2020Q2-2022Q1, there are 1,942,236 employments from firm births. Therefore, over the eight quarters following the pandemic outbreak there are 357,643 fewer employments than the eight quarters preceding the pandemic outbreak, a decline of 18%. This occurs despite there being cumulatively more firm births over the 2020Q2-2022Q1 compared to 2018Q2-2020Q1 (Figure 32). Therefore, since cumulative firm creation is similar over the two periods but cumulative employment from firm creation is much lower, this implies that firms added to the IDBR have fewer employees registered than in the past.

Figure 32: Cumulative Number of Firm Births
Source: Authors' calculations from ONS 'Business Demography Quarterly Experimental Statistics'



Plot shows cumulative number of firms added to the the Inter Departmental Business Register (IDBR). Each line represents cumulative births over an eight quarter time period. The two time periods are seasonally-equivalent, each beginning in Q2. The 2019Q2-2021Q2 line is omitted because it includes COVID and non-COVID periods. The 2017Q2-2019Q1 line is omitted because it is very similar to 2018Q2-2020Q1 which worsens clarity.

Figure 33: Cumulative Employment from Firm Births
Source: Authors' calculations from ONS 'Business Demography Quarterly Experimental Statistics'



Plot shows cumulative employment by new firms added to the the Inter Departmental Business Register (IDBR). Each line represents cumulative employment over an eight quarter time period. The three time periods are seasonally-equivalent, each beginning in Q2. The eight quarter time horizon following 2019Q2 is omitted because it includes COVID and non-COVID periods.