The impact of "Eat Out to Help Out" on firm creation*

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Abstract

We investigate the effect of the UK's “Eat Out to Help Out" policy on firm creation. The policy subsidised people to eat-out at participating restaurants for a period over the COVID-19 pandemic. We compare the number of new incorporations in postcodes with participating restaurants against all postcodes. We find a 6.3% increase in business incorporations in areas with participating restaurants due to the policy. The increase is largest in high-street service activities such as ‘hairdressing and other beauty treatment’. We interpret this as evidence of a demand stimulus in one sector, leading to anticipated demand increases in geographically-close sectors, and consequently a supply increase as measured by firm creation.

Key words: COVID-19, Eat Out to Help Out, firm creation
JEL codes: H25, L20

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1 Introduction

In July 2020, the UK Government announced a range of measures to support businesses and protect jobs in the hospitality sector in the wake of the COVID-19 pandemic. Whilst uptake of the Coronavirus Job Retention scheme in the sector was high (87% of eligible businesses as of 31st July 2020), other support schemes[1] did not provide much benefit to the sector (HMRC, 2020a, 2021; British Business Bank, 2021). Therefore, specific measures to support hospitality included a temporary VAT cut to food and non-alcoholic drinks and the Eat Out To Help Out (EOTHO) scheme.

EOTHO was intended to support 130,000 businesses and 1.8 million jobs by encouraging consumers to eat-out in qualifying restaurants and food service establishments. Only UK establishments, licensed to sell food on or before July 7th, were eligible for the scheme. Once registered, these establishments were permitted to offer a 50% discount on food and non-alcoholic drinks up to £10 per diner. Subsequently, the restaurant could claim back this amount from the government. The discount was available on Mondays, Tuesdays and Wednesdays between August 3-31st, 2020. It applied only to meals eaten on the premises (i.e. excluding take-away meals or catering for private functions). The scheme was announced to Parliament on July 8th (Hansard, 2020).[2]

González-Pampillón et al. (2021) and Fetzer (2020) study the effects of the scheme on footfall, job postings and COVID-19 infection rates. Both find a temporary increase in restaurant visits during the scheme. González-Pampillón et al. (2021) show increased recruitment activity in the sector, while Fetzer (2020) proposes a link to increased cases of COVID-19. To the best of our knowledge, our paper is the first to look at the effects of the scheme on firm creation and sector spillovers. It contributes to the literature studying the impact of policies that help with the economic recovery from the COVID-19-related restrictions. We use the Companies House “Basic Company Data” and the HMRC register of restaurants participating in the scheme.

We find evidence of a positive spillover effect on firm creation in non-hospitality sectors. The effect is greatest after the policy announcement (July 8th) but before the

[1] Among the eligible businesses, 2% were supported by the Self-Employment Income Support Scheme (SEISS) (details here) and 3.7% by Bounce Back Loan Scheme (BBLS) and Coronavirus Business Interruption Loan Scheme (CBILS) (breakdown by sector, available here) in the Accommodation and Food Services sector.

[2] The scheme was expected to cost £500 million (HM Treasury, 2020). In comparison with these stated objectives, over 63,000 establishments registered and around 49,000 businesses made claims through the scheme and collectively they claimed £849 million for over 160 million meals (HMRC, 2020b).

scheme implementation (3rd August 2020). This suggests that the spillover to firm creation in other sectors was driven by expectations of the scheme's effects as opposed to the scheme itself. However, the effect continues until the end of the scheme. 26.7% additional new non-hospitality companies were registered in postcodes with at least one participating establishment in EOTHO compared to in all postcodes. The "Other service activities" sector, which includes high-street businesses such as hair and beauty salons, experienced the largest significant effect.[3]

In those postcodes with at least one EOTHO-participating outlet, not all eligible restaurants decided to take part in the scheme. This spatial variation allows us to employ a difference-in-differences approach to compare business registrations based on their postcode of registration, before and after the scheme was live. Our empirical strategy considers the decision of any establishment to participate as exogenous given the timing of the scheme's announcement and its eligibility criteria. First, we report specifications with week, regional and sectoral fixed effects to alleviate concerns about local shocks, time variant and invariant characteristics (e.g. linear pre-treatment trend of company registrations, total population in a certain region). Second, on average, in the absence of EOTHO, 0.12 natural log points fewer companies would have been created. We estimate that the average treatment effect of the scheme on business registrations is 6.3%. Third, our results do not show any effect stemming from the pre-treatment trend.

Finally, as a robustness check, we present Google trends data which reinforces that interest in setting-up beauty salons spiked after the EOTHO policy announcement. This stresses the importance of the announcement and the positive expectations for the scheme.

The rest of the paper is structured as follows. Section 2 describes our data. Section 3 outlines our methodology, including our identification strategy and difference-in-differences approach. Section 4 outlines our results. Section 5 discusses the mechanisms behind the results.

[3]Most of these registrations were in SIC 9602 ("Hairdressing and other beauty treatment") and 9609 ("Other personal service activities n.e.c.") which includes astrological and spiritualists' activities; social activities such as escort services, dating services, services of marriage bureau; pet care services such as boarding, grooming, sitting and training pets; genealogical organisations; activities of tattooing and piercing studios; shoe shiners, porters, valet car parkers etc.; concession operation of coin-operated personal service machines.

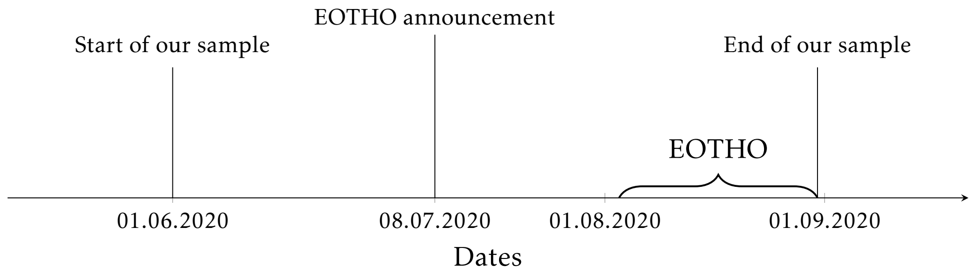

Figure 1: Timeline of EOTHO
Source: Authors' illustration

2 Data

2.1 Data description

Our main data source is the the Companies House register which contains a record of all companies incorporated in the UK.[4] The register lists details of all active companies, including the date of registration, postcode and 5-digit Standard Industrial Classification (SIC) code (e.g. "95250 - Repair of watches, clocks and jewellery"). We focus on a subset of the full register: all companies incorporated between 01/06/20 and 31/08/20. Figure 1 illustrates the timeline of the EOTHO and the key dates we consider in our analysis. In addition to this data from Companies House, we add an indicator variable to each firm registration to indicate whether the postcode is an EOTHO postcode or not. We classify whether a postcode is an EOTHO postcode from an HMRC dataset of participating restaurants.[5] Postcodes that include at least one outlet that participates in the scheme are identified as "EOTHO postcode".[6] By 'postcode', we refer to the 5- to 7-digits (e.g. CT2 7FS) which is associated with no more than 100 unique addresses. Appendix A describes the matching the postcodes between the two data sources.

[4]The name of the dataset is "Basic Company data". We use the dataset from October 2020 to include all active business in the period of interest. This can be found in the project repository or here. Using this particular register release, we eliminate concerns for companies dissolutions that could be in our sample and are out of the register.

[5]The HMRC list of participating restaurants is available here.

[6]We do not consider any measure of intensity. For example, if one food establishment is registered with EOTHO at a CT2 7FS postcode or if ten food establishments are registered at a CT2 7FS postcode makes no difference to our analysis. Both scenarios lead CT2 7FS to be recorded as an EOTHO postcode.

There are 225,749[7] new firm registrations over the full sample (01/06/20 - 31/08/20) and 128,665 (57.0%) of these registrations are at unique postcodes. Once we have assigned an EOTHO indicator variable to each firm in the full dataset of firm creations, we find that 36,972 registrations (16.4%) were created in EOTHO postcodes, whereas 188,777 registrations (83.6%) were created in non-EOTHO postcodes over the full period. Before the government announcement (01/06/20 - 07/07/20), 15,510 registrations (16.1%) were created in EOTHO areas, whereas 80,872 registrations (83.9%) were created in non-EOTHO areas out of 96,382 total registrations. After the government announcement (08/07/20 - 31/08/20), 21,462 registrations (16.6%) were created in EOTHO areas, whereas 107,905 registrations (83.4%) were created in non-EOTHO areas out of 129,367 total registrations.

Finally, for most of our analysis we focus on number of registrations grouped by week. Hence, we have observations of weekly firm registrations over 13 weeks and 128,665 postcodes. We match weekly registrations in 2020 to the same week in 2019 and take the ratio. Analysing registrations in a postcode relative to the same week in the previous year purges our firm creation measure of any postcode or year fixed effect. Hence, we observe changes relative to the baseline (i.e. 2019) for that postcode in that week of the year. Our results should be interpreted as *an increase* in firm creation relative to the norm for that postcode in that week of the year in the presence of EOTHO. For example, an annual boom in firm creations in Glastonbury in the fourth week of June (Glastonbury music festival) will not affect our ratio variable for firm registrations in that postcode on that week.

Table 1 summarises these descriptive statistics.

[7]The original data showed 227,611 new registrations but 1,862 of these had no valid postcode and so were removed from our analysis.

Table 1: Summary statistics

	Pre-announcement (2020-06-01–2020-07-07)			Post-announcement (2020-07-08–2020-08-31)		
	EOTHO	non-EOTHO	Total	EOTHO	non-EOTHO	Total
Mean (s.d.) of weekly hospitality registrations	281.5	860.8	1142.3	266.3	832.9	1099.1
	(45.1)	(96.7)	(133.5)	(28.7)	(61.7)	(82.4)
Mean (s.d.) of weekly ratio of hospitality registrations	1.09308	1.35353	1.27682	1.17153	1.47564	1.38547
	(0.130)	(0.141)	(0.110)	(0.094)	(0.142)	(0.080)
Mean (s.d.) of weekly non-hospitality registrations	2614.8	14131.3	16746.1	2486.0	13016.4	15502.4
	(396.3)	(1718.7)	(2103.7)	(204.8)	(1032.9)	(1213.4)
Mean (s.d.) of weekly ratio of non-hospitality registrations	1.61498	1.39415	1.42454	1.62172	1.36079	1.39661
	(0.140)	(0.107)	(0.110)	(0.147)	(0.101)	(0.103)
Number of (unique) postcodes	47,702	80,963	128,665	47,702	80,963	128,665

Source: Authors' calculation based on Companies House and HMRC

3 Methodology

Our null hypothesis is that EOTHO has *no* effect on firm creation. To test our hypothesis, we compare the number of new registrations in EOTHO postcodes against all postcodes. Our calculations use ratios relative to 2019 which is a pre-COVID-19 baseline. If the number of registrations in EOTHO postcodes, during the EOTHO period, is higher than in all postcodes, we reject the null in favour of a hypothesis that EOTHO raised firm creation. We assume that other policies or external factors that would increase firm creation did not occur concurrently with the EOTHO scheme.

We define the *effect* as the difference of ratio of registrations in EOTHO postcodes minus the ratio of registrations in all postcodes. For example, if the ratio of registrations in week 30 is 1.5 in EOTHO postcodes and it is 1.1 in all postcodes, then the effect is 0.3.

$$\text{Effect} = \left(\frac{\text{\# of registrations 2020}}{\text{\# of registrations 2019}}\right)^{\text{EO2HO postcodes}} - \left(\frac{\text{\# of registrations 2020}}{\text{\# of registrations 2019}}\right)^{\text{all postcodes}}$$

3.1 Confidence intervals

In order to understand whether the effect is statistically significant, we use a Monte Carlo-based bootstrapping algorithm. It is not viable to apply a standard parametric statistical test to the effect. This is because it depends on non-linear combinations of (not necessarily independent) random variables. We use the observed data simulations to obtain an approximate probability distribution. We then identify a 95% confidence interval and test if the effect is significant. Full details are given in appendix B.

3.2 Identification

Our main identification strategy classifies as treated those postcode locations that were affected directly by the policy. Hence our results show the effect on firm creation given a region had at least one EOTHO establishment. Our identification strategy fails if other factors affected these postcode locations at the same time. Or, if there are other covariates that are common to EOTHO locations and could have raised firm creations in EOTHO locations after the announcement. For example, consider the following case. EOTHO locations are highly concentrated in metropolitan areas. At the same time as the EOTHO policy is announced, the government announces a relaxation on retail or nightclub operations. In that case, an increase in firm creation in EOTHO locations relative to other areas may be driven by the latter policy and not EOTHO, *per se*. Importantly, it would have to be a factor that would raise firm creation in an EOTHO locations specifically relative to other areas. Therefore, a general blanket policy would not apply as we would expect to see a rise in firm creations across all areas rather than just in EOTHO areas.

3.3 Difference-in-differences approach

We investigate the effect of the EOTHO scheme on the company registrations in any sector apart from Accommodation and Food services sector using a difference-in-differences approach. We rely on (i) the timing of the announcement of the policy and (ii) the complete postcodes of business registrations. Since not all eligible restaurants in a certain postcode participated in the scheme, our strategy depends on the spatial variation to compare business registrations before and after EOTHO takes place. This maintains our assumption that the decision to participate is exogenous.

Our difference-in-differences estimates come from the following regression

$$\begin{aligned}\ln(\text{registrations})_{k,w,j} = \beta_1 \text{EOTHO postcode}_{k,w,j} + \beta_2 \text{EOTHO period}_w \\ +\beta_3 \left(\text{EOTHO postcode}_{k,w,j} \times \text{EOTHO period}_w\right) \\ +\eta_w + \gamma_k + \delta_j + (\eta_w \times \gamma_k) + (\eta_w \times \delta_j) + (\delta_j \times \gamma_k) + (\eta_w \times \delta_j \times \gamma_k) \\ +\varphi T_{t,j} + \vartheta(\chi_i \times \eta_w) + u_{k,w,j}\end{aligned} \tag{1}$$

where $\ln(\text{registrations})_{k,w,j}$ is the natural log of companies registrations in postcode k, week w and 2-digit SIC code j. EOTHO postcode defines the treatment. It receives a value equal to 1 if the company's registered complete postcode is in a postcode where there is at least one outlet that participates in the EOTHO; 0 otherwise. EOTHO period regards the post-treatment period. It receives value equal to 1 if the registration occurs between Aug 3, 2020 - Aug 31, 2020; 0 otherwise.

Our specifications include week fixed effects (η_w) to account for time-varying factors common to all regions; regional fixed effects (γ_k) to consider any time-invariant unobservable factors at NUTS 3 level regions. Finally, sector fixed effects (δ_j) are included to account for time-invariant unobservable factors at 2-digit SIC sectors.[8] Their interaction terms account for any effects taking place in more than one level (e.g. factors affecting a certain region and sector at the same time). Fuller specifications include a linear pre-treatment daily trend ($T_{t,j}$) of company registrations for each NUTS3 region to control for potential time-varying differences in pre-treatment trends across locations. χ_i includes the natural log of total population in district i interacted with week dummies, which accounts for different time-invaring trends across locations in terms of the population size. By this way, we alleviate concerns that more densely-populated areas will notice greater take-up of EOTHO, and hence, will have greater number of registrations.

4 Results

Between June 2020 and September 2020, new incorporations in the hospitality sector were higher than in 2019. However, as eligible businesses had to be established before the announcement date, the policy would not have had a *direct* effect on firm creation

[8] Our analysis takes place at a full postcode level. At this spatial scale, it is very hard to control for more unobservables due to data availability.

in this sector. We interpret the policy as an exogenous demand shock to the hospitality sector designed to support business survival, not creation.[9]

In other sectors (excluding hospitality), incorporations were on average higher than in 2019, as we have seen in earlier research (e.g. Duncan et al. (2021)). In this case, there is an increase in registrations in EOTHO postcodes compared to all, signalling a positive spillover effect. Figure 2 illustrates the ratio of registrations indexed in week 23, by postcodes. On average, the ratio is greater for EOTHO postcodes relative to before the announcement. It peaks earlier than the implementation of the policy.

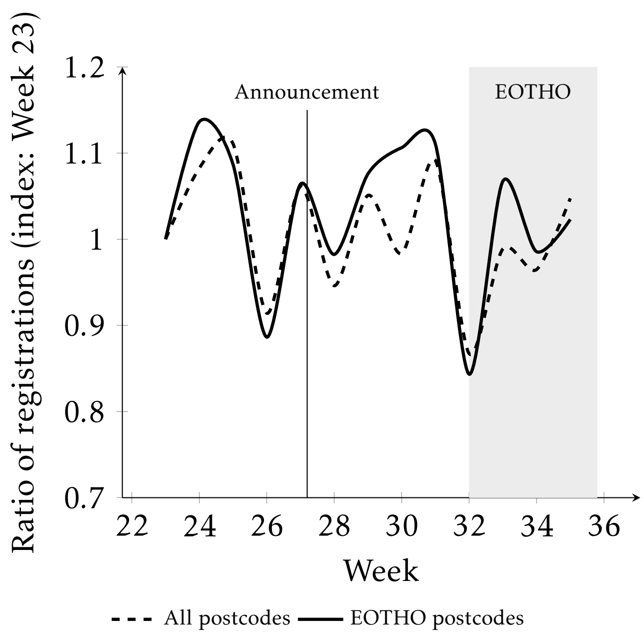

Figure 2: New incorporations: other sectors
Source: Authors' calculation

This spillover effect becomes clearer when plotting the difference between EOTHO postcodes and all postcodes, relative to their 2019 levels, by sector (figure 3). This measure appears to have peaked a week before the scheme implementation. It may mirror positive expectations of business activity. Potential business creators could have seen EOTHO as a stimulus for customers returning to the high street.

[9]The ratio of registrations for the Accommodation and Food services sector is available upon request.

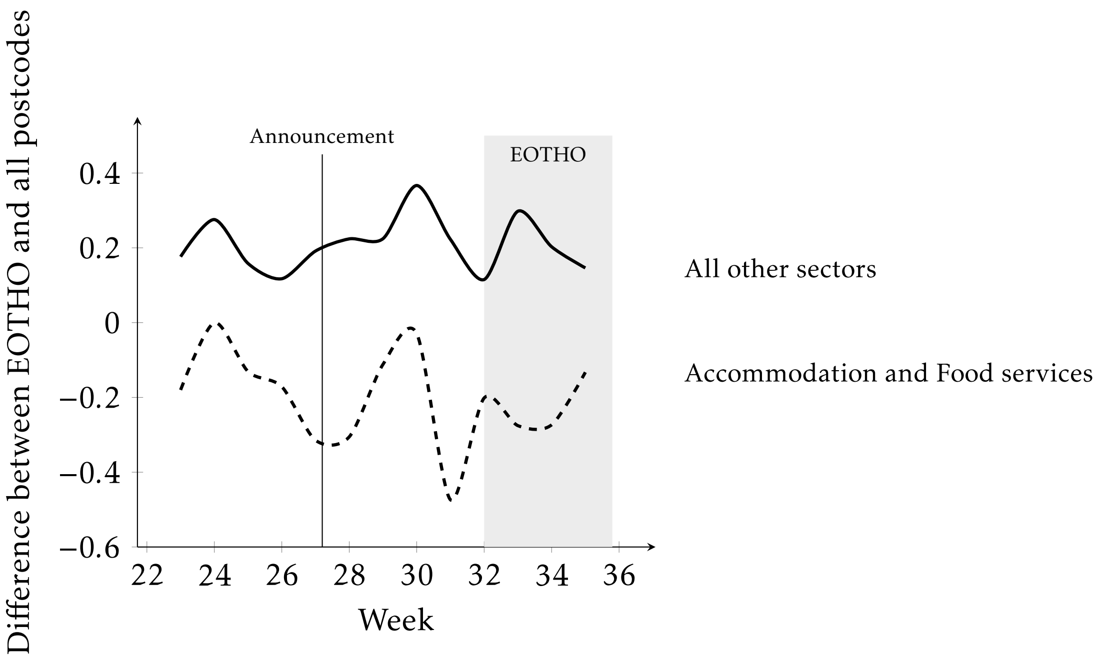

Figure 3: Effect of Eat Out to Help Out scheme, by sector
Source: Authors' calculation

Between the announcement and start of the scheme this difference appears to be greatest: registrations are 22.3% higher in EOTHO postcodes relative to the same week in 2019.[10]

It is possible to calculate the number of additional registrations due to the scheme making two additional assumptions. First, without the scheme the ratio of registrations would have been the same in all postcodes. Second, the difference was a net increase rather than a displacement from other postcodes. In the period between the announcement and start of the scheme, there were 1,430 additional incorporations. Between the start and end of the scheme, there were further 1,362 incorporations.

4.1 Spillover effect by sector

The spillover effect did not affect all sectors equally and not all sectors have links to hospitality.[11] Our results suggest that the effect is significant in only five sectors.

[10] 95% confidence interval: 16.53 - 28.20%.

[11] Some small sectors, including Water supply, Public administration and Electricity and gas supply exhibit large increases in registrations in EOTHO postcodes, but these are not significant.

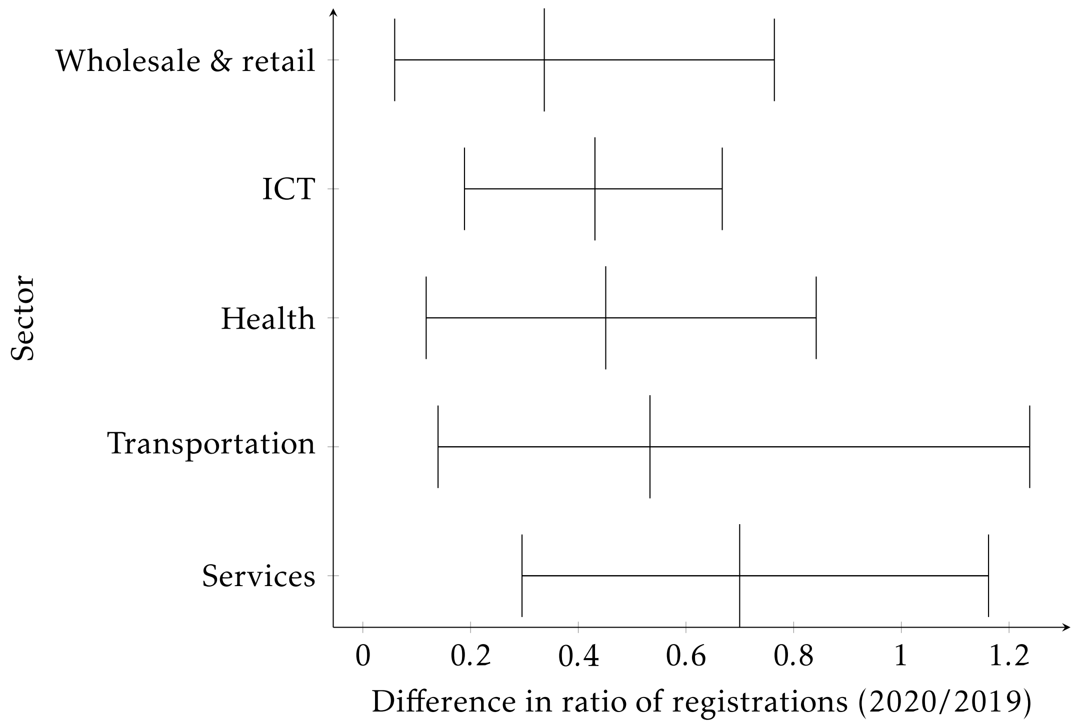

Figure 4: Spillover effect by sector; significant effects only

Note: Median shows the difference between the ratio of registrations in EOTHO postcodes and all. Lines show the 95% confidence intervals.

Source: Authors' calculation

Looking at the timeseries for these sectors (figure 5), it is possible to further refine our assessment of which sectors benefited from the scheme. However, there was a fairly large difference in "Transportation and storage". This started before the announcement; hence, it is unlikely to be a direct result from EOTHO. The difference is also difficult to discern in "Information and communication" (ICT) and Wholesale and retail trade sectors. This leaves a large effect on "Other service activities"[12] and a moderate effect on Human health and social work activities. The latter effect is driven by 'Medical and dental practices'. The following mechanism can explain this effect. Practices are located in close proximity to high-street areas, where it is more likely to find a hospitality outlet that participates in the EOTHO scheme. For example, let us take a less urban area. High-street visitors could combine their visit to the dentist, as well as, other activities in nearby shops before or after their meal. Therefore, the effect seen in "Other service activities" and the Health sector is not surprising.

[12]It includes high street businesses such as Hairdressing and other beauty treatment (SIC 9602).

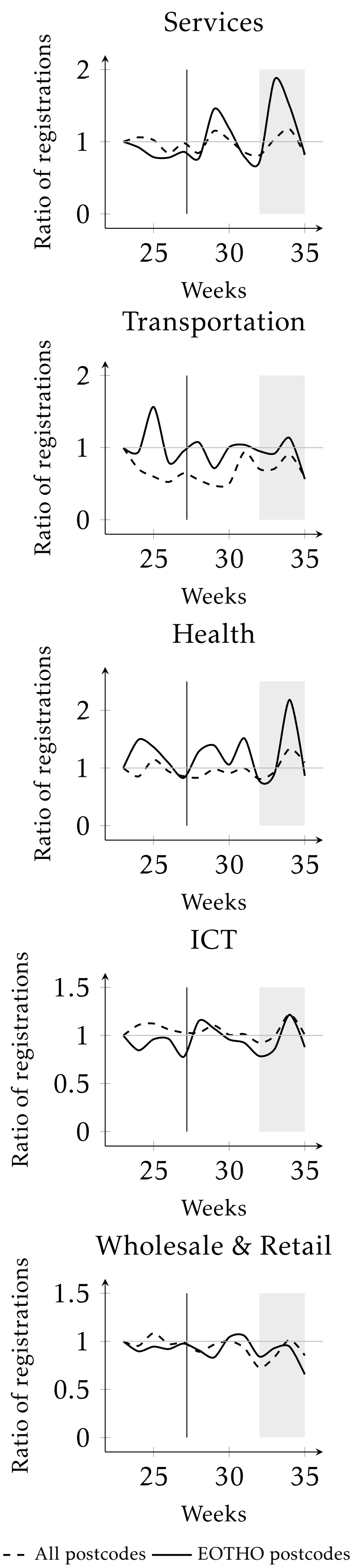

Figure 5: Ratio of new incorporations for sectors with a significant effect

Note: In each panel, vertical line shows the date of announcement, while grey shaded area the period EOTHO took place.

Source: Authors' calculation

4.2 Difference-in-Differences

To establish a causal relationship between the scheme and the firm creation we report the estimates from the difference-in-differences approach we followed. Appendix C shows the parallel trends before the implementation of EOTHO. In the absence of EOTHO scheme, we calculate that 0.12 natural log points (or 1.3 per day) fewer companies would have been created. Table 2 reports estimates of difference-in-differences coefficient of interest (β_3), i.e. treated (in EOTHO postcode) × EOTHO period. It captures the effect of the EOTHO scheme on firm creation in the period when the programme was taking place in August 2020.

Table 2: Estimates of EOTHO on companies registrations; excluding Accommodation and food services

	Dependent variable: Natural log of company registrations							
	(1)		(2)		(3)		(4)	
Treated (in EOTHO postcode)	0.0563	***	0.0563	***	0.0561	***	0.0561	***
	(0.0018)		(0.0018)		(0.0018)		(0.0018)	
Treated × Post-EOTHO	0.0070	**	0.0069	**	0.0068	**	0.0068	**
	(0.0032)		(0.0033)		(0.0030)		(0.0031)	
Baseline FE	Yes		Yes		Yes		Yes	
Pre-treatment trend	No		Yes		No		Yes	
$\chi_i \times \eta_w$	No		No		Yes		Yes	
Observations	192,257		192,257		192,257		192,257	
R squared	0.0652		0.0652		0.0654		0.0654	

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Note: Estimates for the natural logarithm of the companies registrations. Baseline fixed effects refer to fixed effect for the week, NUTS3, 2-digit SIC code and their interaction. Robust standard errors in parentheses.

Source: Authors' calculation

We report 4 specifications, in which we add gradually controls and the linear pre-treatment trend. The common feature of all reported specifications is the baseline fixed effects. They refer to the fixed effects for the week, NUTS3 regions and 2-digit SIC codes, and their interaction. Treated are all registrations occurred in postcodes with at least one participating outlet in the scheme. We see the effect is statistically significant. In the treated postcodes 5.6% more registrations take place than in non-treated postcodes over the entire period. Looking at the coefficient of our interest, we report the difference between before and after EOTHO, given the treatment. Alternatively, 0.7% is the effect of increased generosity affecting postcodes with at least one participating outlet during the period when the scheme was live. The significance drops at 5%. This may come from the fact that in some sectors most of the registrations occurred before the start (August 3rd), but after the announcement (July 8th), of the scheme. The average marginal (treatment) effect of the scheme is 6.3%. This means that EOTHO caused a 6.3% increase in firm creation during the period it took place.

Robustness check: Google Trends

We find that High Street businesses, like hairdressers and beauty salons, benefit the most due to EOTHO. If this is correct, the willingness to create a High-Street business should be higher after the announcement of the scheme. As a robustness check, we use data from Google Trends to proxy willingness-to-create a firm. We focus on the trends of "how to set up a beauty salon" between July and August 2020. Taking beauty salons as an example, there is a large spike in search activity on the next day of the announcement of EOTHO (July 9th). We are not able to know if these Google searches did translate into creations. However, there is a positive correlation between the willingness-to-create and realised creation.

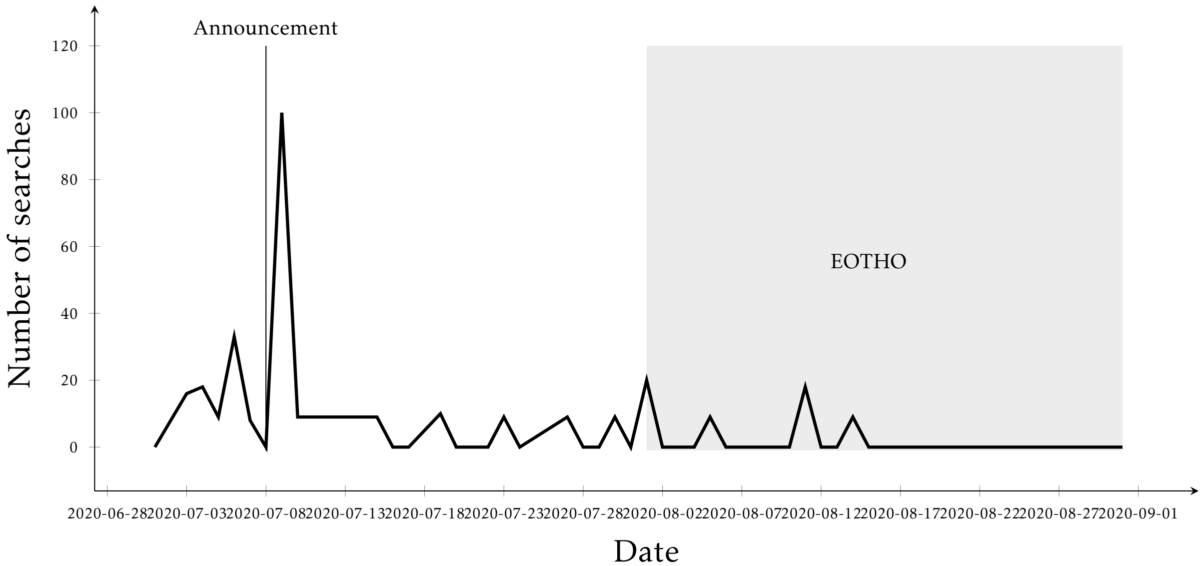

Figure 6: Google searches: "How to set up a beauty salon"
Note: Solid vertical line indicates the announcement of the EOTHO scheme. The Google Trends for the phrase "How to set up a beauty salon" peaks the next day of the announcement.
Source: Google Trends

5 Discussion of mechanisms

We identify an answer to the question: *What* was the effect of EOTHO on firm creation? We find that the announcement of EOTHO caused an increase in registrations that would not have happened in the absence of EOTHO. A further question is: *How* did EOTHO affect firm creation? This focuses on the channels through which firm creation increased and would be much harder to identify. Although we do not attempt to provide a strict identification of the channels increasing firm creation, we provide some possible hypotheses and suggestive evidence.

1. **Geographic spillover:** People visit areas where establishments participate to EOTHO and subsequently use nearby amenities. Our evidence of Google trends in people searching how to setup beauty salons is supportive of this mechanism.

2. **Fiscal multiplier:** People employed in the hospitality sector have an increase in income due to EOTHO. This could cause a spillover effect to other industries through the geographic argument in point 1. That is, furloughed hospitality employees return from the suburbs to metropolitan areas. In terms of a wider fiscal

multiplier effect, González-Pampillón et al. (2021) observe a 7-14% increase in job posts in the hospitality sector which suggests some stimulus which could lead to a general multiplier. However, in our analysis, if the multiplier effect was correct, greater demand in the restaurant industry would have increased wages and jobs and raised firm creation in general. We observe statistically significant greater firm creation in EOTHO postcodes rather than a blanket increase.

3. **Signalling:** We could interpret EOTHO to have a positive effect on expectations. The EOTHO announcement may signal to people that lockdown restrictions will be easing in the future. Consequently, EOTHO announcement causes people to create businesses. Specifically, entrepreneurs create businesses in EOTHO locations because they believe those will see a sharper increase in demand given the increased footfall. González-Pampillón et al. (2021) estimate that footfall increased by 5%- 6% on those days the discount was available.

4. **Other policies:** Other policies at the same time as EOTHO that encouraged firm creation could include, for example, local authority discretionary grants. Because we study EOTHO locations specifically, this should rule out blanket policies that could have affected both EOTHO and non-EOTHO locations at the same time. Finally, to alleviate any concern, in our difference-in-differences analysis, we use controls for time-variant trends across locations to account for local shocks or alternative policies.

Our discussion suggests that a plausible mechanism is that EOTHO worked as a signalling mechanism to businesses and entrepreneurs, leading them to create firms in EOTHO areas in anticipation of geographic spillover effects from the scheme.

6 Conclusions

In this paper, we investigate the effect of the “Eat Out to Help Out" scheme on firm creation in the UK. We observe that more companies in non-hospitality sectors were established in postcodes of participating EOTHO outlets. We find evidence that the policy announcement had a stronger effect than its subsequent implementation. We estimate an additional 2,792 companies were created in non-hospitality sectors as a result of EOTHO. That is, 22.3% more firm creation than in the absence of EOTHO. Or, on average, 1.3 more companies daily. In other words, the average treatment effect of

EOTHO on firm creation is 6.3%. It seems unreasonable to expect that close proximity to participating establishments would affect firm creation in all sectors equally. For example, we would not expect an effect of EOTHO restaurants on nearby construction companies. However, the sector with the most significant positive effect is 'Other service activities'. This sector includes many high-street businesses. This suggests a geographic spillover in EOTHO locations.

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Online Supplement

A Matching EOTHO participants with Companies House

The `restaurants.csv` dataset from HMRC gives the names and addresses of establishments registered for EOTHO. To obtain richer information, such as incorporation date and sector, it is desirable to match this list with the Companies House (CH) dataset of incorporated companies. However, there is no unique identifier for an obvious way to match records, so several different approaches were attempted.

In some cases, a company name may include a *Limited* or *Ltd* suffix not present in the trading name. A simple cleaning function deletes any instance of this suffix, converts letters to uppercase and deletes any punctuation. Having applied this function to both datasets and looking for exact matches, 21,642 of the 63,176 (34.26%) participating restaurants are matched.

This approach has two main issues. First, this excludes any companies whose trading name is different from the company name, even after the basic cleaning for example, many pubs have the same name across the country but each company name must be unique. Second, the scheme was open to companies with multiple premises. Indeed, according to administrative statistics from HMRC (2020b), 7.3% of claims were made by businesses with more than 1 registered outlet.

To address this second issue and to validate the matches, matched records are cross-checked by postcode so that only restaurants located in the same postcode as the company registration are kept. Prior to this, the postcodes are homogenised, by removing any spaces and converting letters to uppercase, to avoid issues with inconsistency. This gives 6,677 results, so this clearly excludes more than just the secondary premises. Cross-checking by postcode area only (just the first one or two letters in the postcode) gives 10,490 results.

The remaining unmatched restaurants are merged with the CH data by postcode, then refined by address number (the first number present in the first or second lines of address). Any remaining restaurants where neither the name nor postcode are the same as the registered companys would be difficult to match with confidence.

A Fuzzy Match approach is then used on the names and addresses using the Jaro-Winkler method and matches are further refined by requiring an edit distance for each field of less than 0.5. Finally, from the HMRC statistics, the majority of companies were from one of 8 2-digit SIC code categories (see below) so only these are kept. After this, there remain some restaurants which are matched to multiple companies, so only the match with the lowest edit distance is kept. This results in 9,759 further matches in addition to the 10,490.

The HMRC (2020b) statistics suggest a total of 36,993 businesses made claims through the scheme but it is unclear whether there would be an injective relation with the companies listed in the CH data. The restaurants dataset also includes premises which registered for the scheme but did not make a claim. However, assuming that this is the appropriate total, the resulting sample of 20,249 has a sampling rate of 54.7%.

Several checks are used to test the validity and representativeness of the sample. First, the SIC codes of the matched companies are compared to the SIC codes from the HMRC statistics. This is shown in Table A.1. In particular, Pubs and licensed bars are significantly under-represented, which could be due to them trading under very different names or operating other than as incorporated companies. Retail is also over-represented and indeed there are more matched companies in this sector than actually made claims.

Second, the number of matched companies is compared to the number of participating restaurants in each region. The sampling rate in each region is between 27.9% in the East and 37.1% in the North East. The regional distribution of matching rate is shown in Fig A.1.

SIC Code	Description	Matched companies sample		HMRC Statistics	
		n	% of sample	n	% of businesses
1	**Agriculture**	**182**	**0.90%**	**246**	**0.70%**
10	**Food manufacturing**	**317**	**1.57%**	**161**	**0.50%**
11	**Drink manufacturing**	**80**	**0.40%**	**68**	**0.20%**
46	**Wholesale**	**441**	**2.18%**	**177**	**0.50%**
47	**Retail**	**1714**	**8.46%**	**1498**	**4.40%**
55	**Accommodation**	**1325**	**6.54%**	**2943**	**8.70%**
56	**Food and beverage services**	**14670**	**72.45%**	**26562**	**78.60%**
561	*Restaurants*	11823	58.39%	18328	54.30%
562	*Event catering*	810	4.00%	491	1.50%
563	*Pubs and licensed clubs*	2037	10.06%	7743	22.90%
93	**Sport, amusements & recreation**	**894**	**4.42%**	**931**	**2.80%**
	(No match)	40	0.20%		
	Other	**988**	**4.88%**	**1187**	**3.50%**

Table A.1: SIC Codes of matched sample and HMRC statistics
Source: Authors' elaboration and HMRC (2020b)

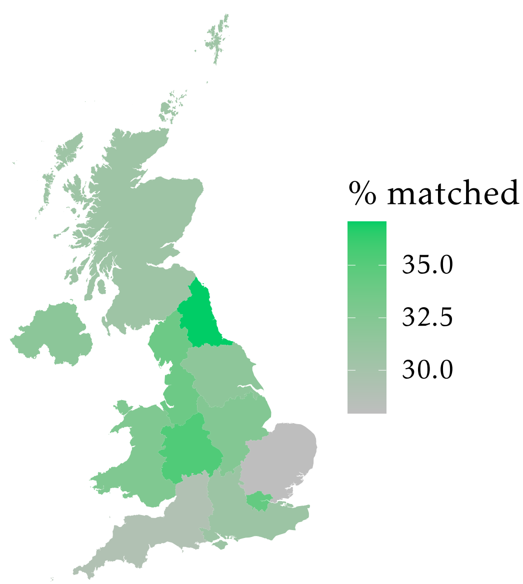

Figure A.1: Regional matching
Source: Authors' elaboration

B Confidence intervals

This section outlines the algorithm to calculate the confidence intervals for the size of the effect.

Let $EOTHO_{2020}$, $EOTHO_{2019}$, All_{2020}, All_{2019}, be the number of registrations in the obvious postcode subsets and years. As before,

$$\begin{aligned}\text{Effect} &:= \text{Ratio of registrations}_{\text{EOTHO postcodes}} - \text{Ratio of registrations}_{\text{all postcodes}} \\ &= \frac{EOTHO_{2020}}{EOTHO_{2019}} - \frac{All_{2020}}{All_{2019}}.\end{aligned}$$

The effect is a non-linear function of four random variables with unknown distributions. A simple bootstrapping algorithm is used to estimate confidence intervals for the effect size. Following a straightforward Monte Carlo approach to case sampling, we build a function to generate 10,000 resamples for the effect and use this to approximate a distribution and identify a 95% confidence interval. The function takes the four observations ($EOTHO_{2020}$, $EOTHO_{2019}$, All_{2020}, All_{2019}) as parameters.

First, it creates a resample for All_{2020}, generating pseudorandom numbers from a binomial distribution with size, $n_1 = All_{2020} + All_{2019}$, and probability, $p_1 = \frac{All_{2020}}{n_1}$. Second, it converts these random numbers to ratios. Since the registrations in EOTHO postcodes is a subset of those in all postcodes, another step must be added to generate the sample for $EOTHO_{2019}$. This time, the pseudorandom numbers come from a binomial distribution which in turn has a pseudorandom size

- from a binomial distribution with size, n_1, and probability, $p_{2a} = \frac{EOTHO_{2020}+EOTHO_{2019}}{n_1}$;
- and probability, $p_{2b} = \frac{EOTHO_{2020}}{EOTHO_{2020}+EOTHO_{2019}}$.

Again it converts these random numbers to ratios. For completeness, the ratio should have been calculated based on the pseudorandom size but this would be computationally expensive, so the observed ($EOTHO_{2020} + EOTHO_{2019}$) was used.

The function then calculates the differences between the ratios for 100,000 samples for the Effect, and then draws the values at the 2.5th and 97.5th percentiles for a 95% confidence interval.

The R code for the function is displayed below:

```
boot_ci <- function(All_2020, All_2019, EOTHO_2020, EOTHO_2019) {
```

```
  #Convert observations to parameters for resampling.
  N_1 <- All_2020 + All_2019
  P_1 <- All_2020 / N_1
  N_2b <- EOTHO_2020 + EOTHO_2019
  P_2a <- N_2b / N_1
  P_2b <- EOTHO_2020 / N_2b
  #Random resample for All_2020 from Bin(N,P).
  db_all <- rbinom(10000, N_1, P_1)
  #Convert All_2020 to ratio.
  db_all <- db_all / (N_1 - db_all)
  #Random number for EOTHO_2020 from binomial distribution with
  #size ~Bin(N,(M/N)) and probability P_2b.
  db_eotho <- rbinom(100, rbinom(100,N_1,P_2a), P_2b)
  #Convert to ratio using M for size.
  db_eotho <- db_eotho / (M - db_eotho)
  #Calculate difference of ratios.
  db_diff <- db_eotho - db_all
  #Output confidence interval.
  list(Lo=quantile(db_diff,0.025)[[1]],Hi=quantile(db_diff,0.975)[[1]])
  #Optionally display histogram of the distribution.
  #hist(db_diff)
}
```

C Parallel trends

To apply the difference-in-differences approach, we show the average number of registrations in our sample by week and treatment, indexed in week 22. We normalise in week 22, when our sample starts (June 1st). However, we note that this is not a full week.

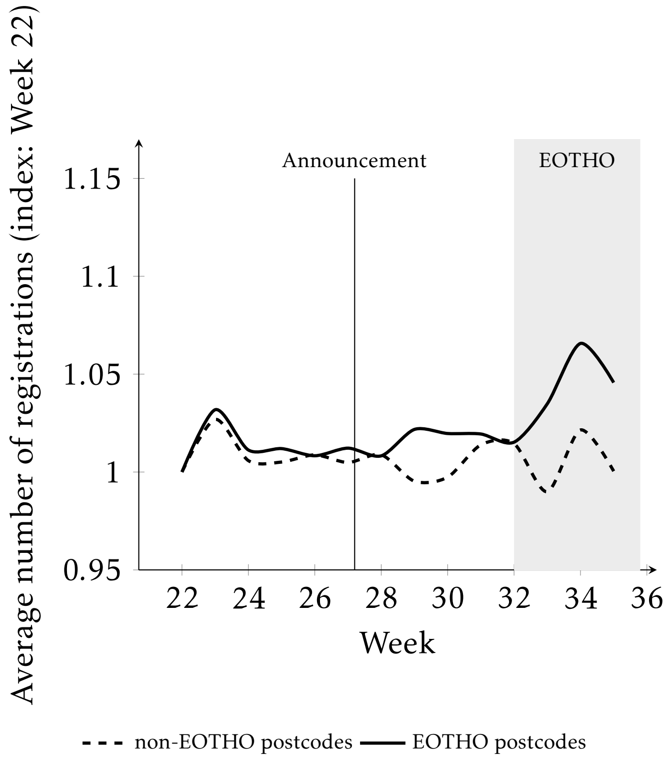

Figure C.1: Average number of registrations by treatment