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The economic impacts of the UK's eat out to help out scheme[☆]

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ABSTRACT

We evaluate the economic impacts of the UK's Eat Out to Help Out (EOTHO) scheme on the food service sector. EOTHO was introduced during the COVID pandemic to stimulate demand by subsidizing the cost of eating out, with a 50 % discount Mondays to Wednesdays in August 2020. We exploit the spatial variation in participation using a continuous difference-in-differences approach and an instrumental variables strategy. We measure the effect on footfall using mobility data from Google and on employment using job posts from Indeed. Our estimates indicate that a one standard deviation increase in exposure to the EOTHO scheme increased footfall in retail & recreation by 2–5 %, and job posts in the food preparation & service industry by 6–8 %. These effects are transitory, and we do not find evidence of large spillover benefits to non-recreational activities or other sectors.

1. Introduction

In the last two decades, the global economy has experienced two major shocks that have severely disrupted the economy and led to strong cuts in consumer spending. The first of these, the 2008–2009 financial crisis, mainly affected the net worth of households that had a large marginal propensity to consume (Mian and Sufi, 2015). Aside from the general macroeconomic effects, sector specific shocks were mainly concentrated on goods that those households tended to consume. During the severe economic contraction resulting from the COVID-19 pandemic, sector specific shocks were particularly strong in some sectors, such as retail, as they were directly affected by lockdown measures introduced to stop the spread of COVID-19 (Chronopoulos et al., 2020; Golec, et al., 2020; Carvalho et al., 2020; Coibion et al., 2020; Baker et al., 2020; Bounie et al., 2020; Althoff et al., 2020). In response to both shocks, there have been calls to consider interventions aimed at stimulating demand in affected sectors. These include proposals to cut Value-added tax (VAT) rate (Benzarti and Carloni, 2019) and subsidies to consumers via vouchers and discounts (Wu et al., 2020). Those policies imply a trade-off between the desire to limit the effects of shocks by providing short-term support, while avoiding economic inefficiencies (i.e., deadweight loss) that come with temporary demand stimulus to particular activities. In a context where these non-standard interventions appear to be becoming more common, it is important to quantify their economic impact. We do this for a UK government scheme—Eat Out to Help Out (EOTHO)—targeted at demand in the hospitality sector, aiming to offset some of the negative impacts of the COVID pandemic.

The UK government introduced a variety of policies to mitigate the economic effects of the COVID-19 pandemic (UK Government 2020a).[1]

[☆] An earlier version of this paper is available as: N. González-Pampillón, Gonzalo Nunez-Chaim & Katharina Ziegler. (2021). "Recovering from the first COVID-19 lockdown: Economic impacts of the UK's Eat Out to Help out scheme". CEP, London School of Economics. *COVID-19 Analysis Series: No. 018.*

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[1] The package of measures included a furlough scheme (Job Retention Bonus), a reduction of value added tax (VAT) and the Eat Out to Help Out scheme, among other measures. See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/898421/A_Plan_for_Jobs_Web_pdf.

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Among these, the EOTHO scheme, run during August 2020, aimed to boost demand and protect jobs in the food service sector (UK Government 2020b, 2020c). Participating businesses received government support to offer a discount on food and non-alcoholic drinks consumed on the premises. Over 160 million subsidized meals were served, costing the UK government £849 million (UK Government, 2020d).

We assess some of the economic impacts of the EOTHO scheme on the food service sector. Given the program's objectives, its duration (one month), and data availability, our focus is on footfall and recruitment, for which we have timely data at an appropriate frequency. An increase in the demand for food services is likely to be reflected in higher levels of footfall in recreational activities and more jobs posts as restaurants, pubs and cafes may hire more staff. To capture these effects, we use data on footfall from Google and on job posts from Indeed and compare locations with different levels of participation before and after the introduction of the policy.[2]

The results indicate that a one standard deviation increase in exposure to EOTHO increased footfall in retail & recreation by 2–5 %. Similarly, a one standard deviation increase in exposure to the EOTHO scheme increased job posts in the food preparation & service industry by 6–8 %.[3] The effect on footfall did not persist beyond the duration of the scheme. The impact on job posts lasted a few weeks beyond the end of the program.

Overall, our findings suggest that the policy only induced higher footfall associated with recreational activities on specific days when the discount was available. It did not encourage people to go out for other purposes or to eat out once the scheme ended. The results on footfall are in line with data from OpenTable, pointing to a transitory increase in restaurant bookings concentrated between Mondays and Wednesdays in August (Statista, 2020). We also find increased demand for jobs in the food preparation & service sector. Our indicator measures the flow of job ads; therefore, a transitory effect on job posts could still imply a permanent increase in the number of employees. Unfortunately, we do not know if job posts resulted in individuals being hired, or if any changes in employment were permanent or temporary. Back-of-the-envelope calculations, using different scenarios for the job post to employment conversion rate, suggest that the per job cost of the program was at least three times average earnings in the sector. We do not find evidence of large spillover benefits to other industries in terms of recruitment.

As not all eligible businesses participated in the program, there were spatial differences in participation. We exploit this spatial variation through a continuous difference-in-differences (DiD) approach comparing locations with different participation levels—'the intensity of treatment'—before and after the program. Our empirical strategy relies on the intensity of treatment being exogenous (the conditional independence assumption). We provide evidence to support the validity of our main identifying assumption. We show there is no evidence of diverging pre-trends for each of the outcomes we consider. We also use an instrumental variables (IV) strategy as we may still be concerned that the intensity of treatment could vary across Local Authority Districts (LADs) due to unobservable factors correlated with local labor markets and mobility patterns. Specifically, we instrument the intensity of treatment with the number of restaurant chains that use a corporate business model and have none of their outlets participate in the program. These non-participating chains made centralized decisions on whether to participate in the program that applied to all local outlets regardless of local economic conditions.[4] Nearly all corporate chains that made the same decision for all outlets chose not to participate, so to make our main results easier to interpret we use these non-participating chains to construct the instrument. We find evidence of spillovers in scheme participation—results using our instrument suggest non-participating chains induce non-chain participation. Our results are robust to different specifications and sensitivity checks, including using participating and non-participating chains to construct the instrument, and a bartik-type instrument based on the share of restaurant chains at the local level combined with the number of outlets of those chains in the scheme at the national level.

To the best of our knowledge, this is the first study focused on assessing the economic impact of the EOTHO scheme using a quasi-experimental methodology and timely indicators.[5] It contributes to the literature analyzing the impact of policies that aimed to stimulate consumption and protect jobs in the midst of the pandemic—such as the stimulus checks and the Paycheck Protection Program in the US (Staples and Krumel, 2023; Hubbard and Strain, 2020; Bartik et al., 2020; Autor et al., 2022; Granja et al., 2022)—as well as policies that intended to speed up economic recovery after COVID-19 lockdowns (Chetty at al. 2020).[6] This paper is also closely related to the literature on the labor market effects of fiscal incentives to increase consumption (Kosonen, 2015; Benzarti and Carloni, 2019). Our findings suggest that the program had a limited effect on footfall and vacancies (job posts). Worryingly, Fetzer (2022) concludes that the program was responsible for between 8 and 17 percent of new COVID-19 cases, thus accelerating the second wave of infections in the UK. This is in line with Glaeser et al. (2020), who find that the reopening of States in the US misled consumers to believe that eating out was safe again. Thus, any economic gains from EOTHO may have come at the cost of more infections as supported sectors depend on footfall and social gatherings.

The paper is structured as follows: The next section describes our data sources. Section 3 describes the variation in participation in the EOTHO scheme and the empirical strategy. Section 4 presents the results. The last section discusses the findings and concludes.

2. Background and data sources

Businesses participating in EOTHO received government support to offer a 50 % discount, up to £10 per person, on food and non-alcoholic drinks consumed on the premises. The program was announced in July 2020 as part of the Plan for Jobs strategy.[7] Support was available for discounts offered from Monday to Wednesday from the 3rd of August to the 31st of August 2020, with no limits on how many times an individual could use the discount (UK Government, 2020b). The EOTHO subsidy

[2] Mobility data from Google can be found at https://www.google.com/covid19/mobility/. Job posts correspond to ads published by businesses on Indeed's website, see https://www.hiringlab.org/uk/.

[3] Official figures from HMRC show a reduction in the number of businesses that furloughed employees in August—through the Coronavirus Job Retention Scheme (CJRS)—among businesses participating in EOTHO (UK Government, 2020d). The interplay between EOTHO & CJRS may have attenuated the effect on hiring.

[4] Firm decisions to participate in the program should be based on expected costs and benefits. Firms with a corporate business model should consider the net expected benefits of all their outlets, in contrast to firms with a franchise structure where outlet decisions should be based on the expected net benefits for the franchise. This means the corporate business model chains that we use to construct our instrument should be responsive to economic conditions across all locations served, rather than to local economic conditions in a specific franchise area. See Section 3.2 for further discussion.

[5] Estimates the impact of EOTHO on new COVID-19 cases and, as a part of that analysis, documents that credit card users sharply increased transactions on Food and Beverage establishments during days in which the subsidy was available.

[6] Chetty et al. (2020) exploit real time data to track economic activity in the US. They find that State-ordered reopening only had a small effect on employment and spending. In contrast, cash transfers to low-income households increased spending, although this did not benefit the most affected businesses.

[7] Other support to the hospitality industry included a temporary VAT cut, business rates relief, small business grants, and a Christmas grant for pubs, some of which ran contemporaneously with EOTHO, but none of which operated only during August 2020.

aimed to increase demand for the food preparation & service industry.

This paper focuses on two relevant indicators—footfall and job posts—that should capture the increase in demand for food services and provide a proxy for the economic impact of the scheme. These two daily indicators are available at the Local Authority District (LAD) level and represent the best data available for considering the economic impacts. In principle, it may be possible to directly assess effects on employment, turnover and survival when the relevant data becomes available in the Inter Departmental Business Register. This would require the government to be willing to identify subsidized firms in that data. Even then, the IDBR only has quarterly data which may be too long a period to properly capture the effects of EOTHO as we will show below.

2.1. Data on the EOTHO scheme

We use publicly available data on businesses that participated in the program to construct a measure the intensity of treatment of the program by LAD. Applications to the scheme opened at the end of July and closed at the end of August. Establishments in the UK could sign up if they were registered as a food business with the relevant local authorities on or before the 7th of July 2020 and had eat-in space within the premises. During this period, HM Revenue and Customs' (HMRC) used a GitHub repository to collect details of participants in the program when establishments registered for the scheme. The repository remains publicly available on the GitHub website.[8] This source includes information on the date of registration for the scheme, name of the business, and full address including the postcode. The source provides the same information for each participating outlet, regardless of whether they are an independent business or part of a restaurant chain. There were around 52,000 establishments registered by 3 August, when the discount was first available, increasing to over 62,000 by the end of the scheme on 31 August (Fig. A.1 in the Appendix). We merge the repository with ONS Postcode Directory data (ONSPD) and aggregate at the LAD level.

Official statistics for the scheme were published in the first quarter of 2021. This release includes information on the total number of outlets that made a claim to the EOTHO scheme by LAD. These figures exclude restaurant chains with more than 25 participating outlets which are included in the GitHub data. There is a strong correlation in the number of participant outlets by LAD in data from HMRC's GitHub repository and these official statistics (Fig. A.2). In any case, we also report estimates using the official numbers as the intensity of treatment and show that the results are the same as when using the GitHub data (see Table O.1 in the Online Appendix).

2.2. Data on outcomes

2.2.1. Footfall

We measure footfall, using daily data for LADs, which is available online from Google on mobility.[9] Data is reported as a percentage change relative to a pre-pandemic reference date (the median of the period between 3 January and 6 February 2020). We create an index using the reference period as the base.[10] Google published data on categories that are useful for measuring social distancing efforts, as well as access to essential services. The data is split into six categories based on the destination of trips—retail & recreation, supermarket & pharmacy, parks, public transport, workplaces and residential. Our analysis focuses on footfall in the retail & recreation category which includes visits to restaurants, cafes, shopping centers, theme parks, museums, libraries, and cinemas. We also test whether the program affected trips to other types of outlets by looking at footfall in the supermarket & pharmacy category. The footfall data is unlikely to be representative of the UK since only a subset of the population uses Google and consents to share their location history. We will discuss the implications of this for our results in Section 5.

2.2.2. Job posts

We use daily data on job posts from Indeed to measure the number of job ads across time in each LAD.[11] All job ads posted on Indeed's website include a location field. Employers are not required to use a standardized format—the information provided can be the full address, a partial postcode or a broader geographical area like the name of the town or city. Using the information provided by employers, Indeed identifies the LAD of each job ad. Job posts can usually be allocated to a LAD, except in cases when the ad only includes the name of a city which contains more than one LAD. In these cases, Indeed allocates job ads to a LAD, which is usually that with the highest proportion of employment in the relevant city. We exclude these eight LADs, given that number of ads in these LADs is inaccurate by construction, although results are very similar when including them.[12]

The data available to us corresponds to the rate of growth relative to a pre-pandemic reference date—the 1st of February in each year (2019 and 2020). As with the mobility data we create an index using the reference period as the base for each year. We focus on the impact on job posts in the food preparation & service category as it comprises ads that are more likely to reflect restaurants responding to any increased demand generated by the scheme. We also extend the analysis to measure the effect on job posts in all sectors except food preparation & service, and hospitality & tourism.[13] One limitation of using data on job posts is that some of these may not translate into actual jobs. In addition, we may observe additional persistence in job posts if ads remain on the website after recruitment is completed. Finally, the data is also only representative of a subset of food establishments that advertise positions through online channels, and potentially larger businesses that are more likely to have the capacity to hire more staff. Again, we will discuss potential implications for our results in Section 5.

[8] Businesses needed to register online and had to wait seven days from registration date to make a first claim (UK Government, 2020b). Here is the repository: https://github.com/hmrc/eat-out-to-help-out-establishments.

[9] COVID-19 Community Mobility Reports: https://www.google.com/covid19/mobility/. The following nine LADs were excluded from the analysis as the mobility data contained missing values for more than 25 days: Ceredigion, Clackmannanshire, Isle of Anglesey, Isles of Scilly, Merthyr Tydfil, Na h-Eileanan Siar, the Orkney Islands, Rutland, and the Shetland Islands.

[10] Low sample sizes mean that Google data can be missing for some areas in some days. To address this issue, we imputed around 7.5 % of our sample using the average value of the previous two days and the subsequent two days for each location.

[11] To post a job ad on Indeed's website, businesses need to login into their employer account and create a new post including at least job title, location, and type of employment (full-time, part-time). Details on salary, skills, experience, and education are optional. At the time of creating the ad, employers can set an end date, which automatically removes the ad from the website. Employers can also manually pause or cancel the job ad at any time. If the employer does not set an end date and does not manually cancel the ad, it could remain on the website for an extended period.

[12] These correspond to Birmingham, Camden, Glasgow City, Lambeth, Manchester, Nottingham, Portsmouth, and Westminster. The following five LADs were also excluded from the analysis as job ads were not common and only posted on a few dates: Isles of Scilly, Nah-Eileanan Siar, Orkney Islands, Shetland Islands, and East Renfrewshire. As with the mobility data, low sample sizes mean this data can be missing for some areas in some days. We imputed 0.3 % of our sample using the average value of the previous two days and the subsequent two days for each location.

[13] We also exclude hospitality & tourism given that official figures indicate around 8.2 % of businesses that participated in the program belong to this sector. See UK Government (2020d).

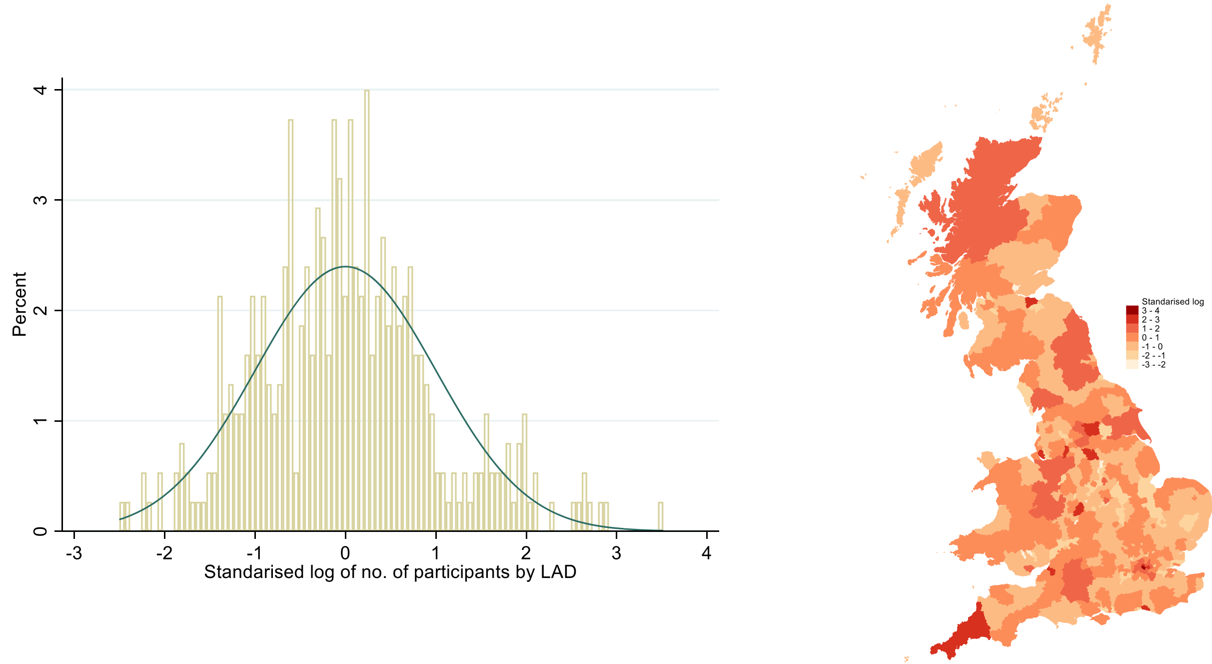

Fig. 1. Variation in the number of participating outlets.
Note: The left-hand panels plot a histogram and smoothed density plot for our measures of the intensity of use of the scheme for all LADs across the UK. The right hand panels map the same data—the darker the color on the map, the higher the intensity. Source: Author calculations using data from ONS, HMRC and HMRC's GitHub repository.

3. Empirical strategy

The EOTHO scheme was implemented at the same time across the UK. All establishments registered as a food business before the 7th of July 2020 and that had eat-in space within their premises were eligible to apply. Given this, we focus on estimating the effect of intensity of treatment rather than considering treatment and control groups. To do this, we employ a continuous difference-in-differences strategy that exploits spatial variation in the number of participating outlets in the scheme across locations in the UK. This involves a before-and-after comparison across LADs with different intensity of the treatment measured by the number of participating outlets. Identification relies on the exogeneity of the spatial variation in the intensity of treatment after controlling for confounding factors such as local shocks. Clearly this is quite a strong identifying assumption as firms opt into the scheme, so the intensity of treatment may vary across LADs due to unobservable factors correlated with local labor markets and mobility patterns—e.g., the ability of firms to survive after lockdown measures were introduced. To deal with these concerns, we construct an instrument for the intensity of treatment by exploiting the fact that some restaurant chains made centralized decisions on whether to participate in the EOTHO scheme. These decisions will affect the intensity of treatment but should be independent of local labor market conditions and mobility patterns.

3.1. Intensity of treatment of the scheme

Our empirical strategy exploits the fact that participation in the scheme varies widely across LADs, so the intensity of treatment differs. The level of participation in a location may depend on factors that are directly associated with footfall, economic activity, an idiosyncratic component of the location or with aspects which are uncorrelated with our outcomes (e.g., lack of program awareness). We discuss how we address the resulting identification challenges below.

The most natural measure of the intensity of treatment would be the share of establishments that participated in the scheme. We do not use this as we cannot get a precise measure for the denominator and any take-up rate would introduce large measurement error (see Online Appendix A for more details). Therefore, our main measure for the intensity of treatment is based on the number of participating outlets.[14] We construct a measure by LAD using the number of establishments registered as participating in HMRC's GitHub repository on the last day of August 2020. We exclude 19 businesses that participated in the scheme (0.03 % of the total number of participants) given that the reported postcode is incorrect, and thus we could not allocate them geographically to a LAD. The left panel of Fig. 1 shows substantial variation in the logarithm of the number of participating outlets, with the distribution across LADs approximating a normal distribution. The right panel presents the spatial distribution of the intensity of treatment.[15] Across the UK, many eligible businesses did not use the scheme. Participation in EOTHO ended up being less than half of what the UK government had anticipated.[16] This is surprising given that the food sector had been struggling after lockdown measures were introduced. However, the low demand for the EOTHO scheme is in line with low uptake of other types of interventions such as business support programs.[17] Anecdotal evidence suggests low participation may have reflected capacity constraints, exacerbated by the COVID-19 pandemic. For some establishments participation meant opening early in the week, for

[14] We also consider several alternative measures for the intensity of treatment as robustness checks (see Table O.1 to Table O.3 in the Online Appendix B).

[15] These measures have been standardised to ease interpretation, so the distributions are centred around zero. LADs located in the North & South–West of the UK had higher participation rates in EOTHO.

[16] The UK government aimed to support around 130,000 businesses with the EOTHO scheme. See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/898421/A_Plan_for_Jobs_Web_.pdf.

[17] See https://www.gov.uk/government/publications/research-to-understand-the-barriers-to-take-up-and-use-of-business-support.

Table 1
Impact of EOTHO on footfall and job posts.

	DiD estimates			IV estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Footfall [(a)]			
Intensity: Log of outlets in EOTHO	0.038***	0.035***	0.013***	0.049***	0.050***	0.020***
	(0.005)	(0.005)	(0.003)	(0.006)	(0.006)	(0.004)
F-statistic	N/A	N/A	N/A	179	174	161
			Panel B: Job posts [(b)]			
Intensity: Log of outlets in EOTHO	0.027	0.048**	0.032*	0.062**	0.078**	0.071***
	(0.019)	(0.021)	(0.017)	(0.031)	(0.032)	(0.027)
F-statistic	N/A	N/A	N/A	111	98	94
Day, week & LAD fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Area by week fixed effects	Region	County	County	Region	County	County
LAD-specific linear trends	No	No	Yes	No	No	Yes

Note: The table presents DiD and IV results estimated using data at the LAD level for ten calendar weeks from 29 June 2020 to 6 September 2020. The dependent variables are the natural logarithm of the footfall index and job post index. The reported coefficients correspond to the interaction between the respective intensity and a dummy equal to 1 for dates in which the scheme was live.; N/A: Not applicable. Standard errors in parentheses clustered at the LAD level.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$. Results in panel (a) use 10,850 observations and 155 clusters; in panel (b) 10,220 observations and 146 clusters.

others, it required large operational adjustments, such as implementing a new system for tracking orders and calculating discounts.[18] In short, low take-up likely reflects the fact that for many establishments the expected costs outweigh the expected benefits.

3.2. The empirical model

We first use a continuous-treatment difference-in-differences (DiD) model that compares outcomes for LADs before-and-after the introduction of EOTHO as a function of the intensity of treatment. To deal with the potential non-randomness of scheme participation, we instrument for the intensity of treatment using the number of non-participating restaurant chains (see Online Appendix A for details on the data we use), as discussed below. Our DiD estimating equation is the following:

$$\begin{aligned} \ln(y_{it}) &= \alpha_i + \lambda_d + \eta_w + \gamma_{rw} + \beta T_{it} + \varepsilon_{it} \\ T_{it} &= I_i \times post_t \end{aligned} \tag{1}$$

where y_{it} is one of several outcome variables in LAD i on date t. We include additive LAD fixed effects (α_i), day fixed effects (λ_d), week fixed effects (η_w), and week-by-region or counties (NUTS1 or NUTS2) dummies (γ_{rw}). I_i represents the continuous and time-invariant measure for the intensity of treatment in each LAD. $post_t$ is a dummy taking the value of one during the dates which the scheme was live (from 3 August to 31 August), and zero otherwise.

Our coefficient of interest is β which captures the impact of the program on outcomes conditional on the fixed effects. The LAD fixed effects account for time-invariant unobservable factors at the LAD level, while week and day fixed effects account for time-varying factors common to all LADs. The week-by-region or counties fixed effects capture local economic shocks, shocks related to the spread of the disease across UK areas and effects of local measures implemented to mitigate the spread of COVID-19. In our most complete specification, we further include LAD-specific linear trends by calendar week to account for potential differences in trends across LADs. This approach relies on the assumption that any pre-existing trends across differently exposed LADs are linear and would have evolved at the same rate in the absence of the scheme.

[18] See https://www.theguardian.com/business/2020/jul/22/eat-out-to-help-out-thousands-of-retailers-yet-to-sign-up-for-scheme. https://confidentials.com/manchester/eat-out-to-help-out-which-manchester-restaurants-are-taking-part.

The main identification concern is that OLS will give biased estimates if participation levels are not randomly distributed across LADs. This will be the case if there are underlying unobservable factors (e.g., different perceptions of the risk of being infected) correlated with outcomes and explaining differences in the intensity of treatment across LADs. We employ an instrumental variables approach to deal with these concerns. For this, we use data on restaurant chains, a group of establishments with presence in multiple locations that share the same name and concept (see Fig. A.3 and Table A.1 in the Appendix). For reasons discussed below, we focus on chains with a corporate structure that decide none of their outlets will participate (for convenience we will refer to these as 'non-participating restaurant chains'). We instrument T_{it} from Eq. (1) with Z_{it} the number of non-participating restaurant chains in each LAD—a time-invariant variable—interacted with $post_t$ (see Online Appendix A for a discussion of alternative instruments). Then, we estimate β_{IV} the effect of EOTHO via two-stage least squares (2SLS) where the two stages are given by:

$$T_{it} = \theta_i + \delta_d + \vartheta_w + \omega_{rw} + \rho Z_{it} + u_{it} \tag{2}$$

$$\ln(y_{it}) = \alpha_i + \lambda_d + \eta_w + \gamma_{rw} + \beta_{IV} T_{it} + \varepsilon_{it} \tag{3}$$

with Eq. (2) corresponding to the first-stage and Eq. (3) to the second-stage. Like Eq. (1), the first-stage includes additive LAD fixed effects (θ_i), day and week fixed effects (δ_d and ϑ_w respectively), and week-by-region or counties (NUTS1 or NUTS2) dummies (ω_{rw}). For the second-stage, we also include the same set of fixed effects as in Eq. (1) (α_i, λ_d, η_w and γ_{rw}). We report estimates from the first-stage (Eq. (2)) in Table A.2 and report the associated F-statistics in our main tables.

Our instrument (Z_{it}) exploits the fact that many restaurant chains appear to have made centralized decisions on whether to participate in the program. This may not be the case for chains operating a franchise business model with decentralized decision-making processes—even if it is common for a group of franchises to have the same owner—so we restrict attention to chains using corporate models. For these chains, centralized decisions could still be made outlet by outlet to reflect local economic conditions. To rule out this possibility, our instrument uses information only on chains that make the same decision on program participation for all their outlets rather than decisions by establishment. That is, for each chain, we need either all outlets registered to participate in the scheme or none of them. These chains provide a source of variation that is highly likely to be exogenous to local economic conditions.[19]

Table A.1 suggests that many restaurant chains did make centralized decisions regarding the participation of all their establishments in the scheme. To construct the table, we obtain the share of outlets participating in EOTHO from their total number of establishments. This share is likely to be underestimated since we have no data on whether outlets were eligible in August 2020 (i.e., were open and had a dine-in option). This means that totals may include outlets that were ineligible for EOTHO. From the list of 80 most popular restaurants chains, 5 have a take up rate above 85 % and 56 of them below 15 %, which means that three quarters of these restaurant chains appear to have taken a mainly centralized decision.[20] From these chains, as just discussed, we further limit the possibility that local conditions influence chain participation by focusing on chains with corporate models that have either all outlets participate, or no outlets participate. As the table makes clear, from these chains, there are 19 non-participating and only 3 participating. Although it makes no substantive difference to our results (see Table O.5 in the Online Appendix), we construct our instrument based on the 19 non-participating chains. This strengthens the exclusion restriction given that non-participating chains are unlikely to attract more customers as they did not offer the EOTHO discount nor to hire more staff since they did not face increased demand. In addition, an instrument based on non-participating chains makes our results slightly easier to interpret.[21]

To see why this restriction helps with interpretation, note that our empirical strategy most obviously provides an estimate of the local average treatment effect caused by variation in the number of chain restaurants that do not participate in EOTHO. However, it will provide an estimate of the average treatment effect (ATE) (rather than the local average treatment effect) if our instrument is correlated with participation of non-chain restaurants due to spillovers. That is if non-chain outlets are more likely to apply for the scheme if they observe surrounding chain restaurants (their competitors) not participating. A similar spillover could operate if chain participation induces non-chain participation but combining the two effects by constructing the instrument using all-participating and non-participating chains—makes any spillover results harder to interpret. This decision makes no difference to our results (see Table O.5 in the Online Appendix), which is unsurprising given the small number of chains that choose all-participation.

We observe a high correlation between the intensity of treatment and our instrument (see first-stage estimates in Table A.2 and Fig. A.4). To look for evidence of spillovers, we split the sample of participants between outlets from restaurant chains and establishments from non-chains. From the total number of around 62,000 outlets participating in EOTHO, over 4000 are part of one of the 80 most popular chains in the UK. Table A.3 and Table A.4 present the first-stage estimates using an intensity of treatment measured by only the number of chain and non-chain participating outlets, respectively. In both cases, we obtain positive and statistically significant estimates at the 1 % level. The *F*-statistics are larger for participation of outlets from restaurant chains (direct competitors) compared to our main sample, which includes all establishments (Table A.3). More importantly, the estimates from Table A.4 suggest that the presence of non-participating restaurant chains induce non-chain outlets to participate in the program, thus, providing evidence on spillovers in scheme participation.[22] This suggests our empirical strategy should provide an estimate of the average treatment effect.

4. Results

Our analysis concentrates on the first post-lockdown period—that is, from the last week of June 2020—to exclude weeks with mobility restrictions. We also focus on LADs from Primary Urban Areas (PUAs).[23] To explain why, we start by analyzing longer-term trends of footfall and job posts for LADs from PUAs and non-PUAs (Figs. A.5 and A.6). For footfall, PUAs and non-PUAs present a similar trend from the beginning of 2020 until mid-July 2020, when the latter group begins diverging and having a faster return to pre-pandemic levels, besides a higher level of footfall for the rest of 2020. We see a similar pattern for job posts. That is, a faster return to pre-pandemic levels and a higher index in non-PUAs compared to PUAs after mid-July 2020. The different trend between these groups could be associated to an heterogenous impact of the pandemic on the economy, given that is easier to follow social distancing and other safety measures in (more) rural areas. Further, Google does not recommend comparing their mobility data between urban and rural regions. Thus, our analysis focuses on LADs located in PUAs.

Figure A.7 presents the footfall trend for PUAs in the UK with footfall split by category. We see a sharp drop in footfall after lockdown measures were introduced in mid-March, followed by a slow recovery which started to accelerate after lockdown restrictions were relaxed –on the 4th of July– until early November. Fig. A.8 shows the trend of job posts in 2020 by category for LADs from PUAs. As with job posts, we observe a large drop in the number of job posts, with the lowest point around mid-May. The index suggests that the food preparation & service sector was severely affected by the crisis, and that the recovery only began after lockdown restrictions were relaxed.

4.1. Baseline estimates for footfall and job posts

We focus on two outcomes: the natural logarithm of the footfall index and of the job post index. We consider a standardized intensity measure, which corresponds to the natural logarithm of total number of participating outlets. Table 1 presents the average treatment effect of the EOTHO program on footfall in the retail & recreation category (Panel A) for the weeks in which the discount was available, and on job posts in

[19] Empirically, the focus on chains with all-participation or no-participation renders the distinction between franchise and corporate models less relevant as highly correlated local economic conditions in the relevant markets should drive the same zero-one decision in either a corporate or franchised model. This might also raise concerns over corporate chains operating in a small number of markets—an issue we address in a robustness check below.

[20] One remaining concern might be for regional restaurant chains if their decision to participate is correlated with local market conditions at the LAD level. This is unlikely to be a major issue given the UK has 12 regions and each one has between 12 and 64 LADs. Consistent with this, excluding two chains with presence in only one region, gives similar point estimates with the same significance level (see Table O.4 in the Online Appendix).

[21] To address concerns over the exclusion restriction, note that our econometric specifications include LAD fixed effects. This means that the time varying information used for identification comes from chain participation in EOTHO (rather than time variation in the number of chains). Any direct effect of number of chains should therefore be captured by the LAD fixed effect. As a further check, we can use a version of our instrument constructed using the two restaurant chains that did not have post on Indeed in Spring 2023 (assuming they also were not using Indeed in 2020), the results are less precise but still similar. Finally, note that, the cross-sectional correlation between number of chains and job ads, or footfall is negative, partially explained by the fact that chains tend to be found in LADs with less employment (pre-COVID). So, if the presence of chains directly drives higher footfall and job posting during August, this would imply correlation in the time series that was opposite to that in the cross-section.

[22] Participation in EOTHO entailed costs and benefits for firms. Restaurant chains that made a centralised decision to not participate potentially increased the benefits to competing establishments which appears to have induced these competing establishments to enrol in the program, hence the spillover from non-participating chains to participation in non-chain outlets.

[23] PUAs are defined as the built-up area (i.e., the physical footprint) of cities, which aims to capture the concentration of economic activity. There are 63 primary urban areas in the UK. For further technical details see the following: https://www.centreforcities.org/the-changing-geography-of-the-uk-economy/.

the food preparation & service category (Panel B). Our estimates for both footfall and job posts are robust to the use of alternative measures for the intensity of treatment and several sensitivity checks (see Online Appendix B).[24]

The DiD estimates from estimating Eq. (1) are reported in the first three columns (1 to 3). The IV estimates from Eq. (3) are reported in the next three columns (4 to 6).[25] We consider three specifications in each case, including different area by time fixed effects to account for i) local policy measures related to the pandemic; and ii) area-specific shocks associated to the evolution of the pandemic. We first include region-by-week fixed effects (columns 1 and 4), then we replace these by county-by-week fixed effects (columns 2 and 5).[26] Finally, we also add district-specific linear trends (columns 3 and 6), which is our preferred specification as it better accounts for local pandemic-related shocks.

Panel A of Table 1 reports results looking at the impact of the program on footfall in retail & recreation to quantify to what extent EOTHO increased the number of people visiting establishments in this category. All estimates are positive and statistically significant at the 1 % level. The estimate from our most complete DiD specification (column 3) corresponds to a 1.3 % increase.[27] A complementary approach exploiting within-location and within-week variation by comparing footfall from Monday-Wednesday against Thursday-Sunday within the same LAD and conditional on scheme participation produces similar results (see Table O.10 in the Online Appendix B). IV estimates suggest larger effects (column 6), such that a one standard deviation increase in the number of participants in the scheme led to an increase in footfall of 2.0 % across PUAs in the UK. The larger IV point estimates, albeit not significantly different, suggest a downward bias in the OLS that seems reasonable as unobservable confounders (e.g., fears of infection) are likely to be negatively correlated with both footfall and the likelihood that restaurants felt it was worthwhile participating in the scheme. Breaking down by days (Table A.6) this effect mainly comes from increased footfall on Tuesdays (1.4 %) and Wednesdays (2.3 %) in August, which is when the discount was available. We find evidence of displacement from Mondays (−2.1 %) to Tuesdays–Wednesdays. The scheme had no significant impact between Thursdays and Sundays.

Next, we examine the effects on job posts. Panel B of Table 1 captures how firms reacted to the increase in the demand for food and restaurant services. The estimate from our preferred DiD specification (column 3) corresponds to a 3.2 % increase. Our IV estimates show larger impacts, with estimates being statistically different. From our preferred IV specification (column 6), the results indicate that a one standard deviation increase in the exposure to the EOTHO scheme increased job posts by 7.1 %.[28] As expected, IV estimates seem larger than OLS, for similar reasons as discussed for footfall above. The EOTHO scheme led to higher activity in the labor market in the form of job ads and recruitment efforts across PUAs in the UK.

We also measure the effect of EOTHO on footfall in the supermarket & pharmacy category to understand if the program affected trips to other types of outlets. Table A.7 presents the results, which indicate a negative effect of between −3 % and −1 % on this category. This suggests there was a small displacement effect from supermarket & pharmacy to retail & recreation activities. We extend the analysis by measuring the effect of the policy on the number of job posts in all sectors except food preparation & service, and hospitality & tourism. This allows us to understand whether there were spillover effects to other industries. Table A.8 presents the results of this exercise. We conclude the program had a small effect (a 1.6 % increase) on job posts in other sectors during the period of analysis, but demand mainly increased in the food preparation & service sector.

4.2. Dynamic treatment effects

We also consider dynamic effects of EOTHO on footfall and job posts. To do this, we replace the interaction of our treatment intensity with the dummy for the period when the scheme was active ($I_i \times post_t$ in Eq. (1)) by the interaction of the same intensity (I_i) with week dummies to obtain weekly estimates for the impact of the program.

We use the same measure of the intensity of the program and our preferred specification, which includes county-by-week fixed effects and district-specific time trend. Since we include district fixed effects, we need to omit one week—we choose week 31 (the week before the scheme went live), so all estimates are relative to that week. As well as allowing us to consider the timing of effects, this exercise also provides evidence on the common trend assumption as we report estimates for four weeks before the discount was available.

Panel A of Fig. 2 presents an event study graph with the weekly estimates for footfall in the retail & recreation category. This allows us to assess the impact of the EOTHO program on footfall over time—i.e., before, during and after the scheme was live. Grey lines highlight the weeks in which the EOTHO scheme was live, while the vertical black lines depict 95 % confidence intervals. The estimates in the weeks prior to the start of the program suggest no obvious pre-trend prior to the start of the scheme. Consistent with this, the p-value obtained from a joint test for the equality of coefficients for the pre-scheme weeks commencing 6, 13, and 20 July 2020 is 0.61. Although such pre-trend tests may suffer from low power, calculations following Roth (2022) suggest our test is not under-powered under a plausible violation of the common trend

[24] One concern we address is that our estimates could result from different pre-treatment trends in the outcome variable across LADs with different exposure to the scheme. To allow for this, we obtain estimates considering changes in our outcome variables (instead of levels) as well as controlling for changes in the same outcome variable but from a four-week pre-treatment period. We still observe a positive and significant effect on both footfall and job posts (see Table O.6 in the Online Appendix).

[25] In Table A.5, we also consider reduced-form estimates derived by replacing the intensity of treatment measure with our baseline instrument (number of non-participating restaurant chains), with results showing positive and significant effects on both footfall and job posts. For IV results, we consider two variations of our instrument by i) using the total number of establishments from non-participating restaurant chains (see Table O.7 in the Online Appendix); and ii) constructing a bartik-type instrument (see Table O.8 in the Online Appendix). In these alternatives, the coefficients are similar to our main results for both footfall and job posts.

[26] Overall, we observe larger coefficients among specifications that include county-by-week fixed effects compared to those with region-by-week fixed effects. This could be due to local policy shocks having a larger role within a smaller geography (i.e., counties) relative to broader areas (i.e., regions), and the spatial correlation in the spread of COVID-19—i.e., LADs in the same county may have observed a similar evolution of the pandemic and, as a result, similar containment measures.

[27] To cross check our comparison of footfall and job post estimates we obtain estimates using the same sample of LADs as used for job posts. The IV results indicate a similar effect (see Table O.9 in the Online Appendix).

[28] We include a robustness check considering seasonality of job posts by using data from 2020 relative to 2019 (see Table O.11 in the Online Appendix). The IV coefficient from our preferred specification is similar to our main result, suggesting there is not a seasonal pattern that could affect our estimates. We also conduct a placebo test measuring the effect EOTHO on job posts but in 2019. All the coefficients oscillate around zero and are statistically insignificant, which reinforces the validity of our empirical strategy and results (see Table O.12 in the Online Appendix). Mobility data from Google is not publicly available for 2019, so an equivalent robustness check and placebo test are not possible for footfall.

assumption.[29]

The point estimates for footfall are positive, but marginally insignificant in the first two weeks of the scheme. Given that there were no other policies influencing footfall, nor constraints preventing customers from going out, the lack of effect in the first weeks could be because customers were less aware of the scheme in the early days (it was announced less than a month in advance), or because concerns from going out to potentially crowded venues (given the discount offered) eased as the month passed. The estimates jump sharply and become significant in the second half of the month. These coefficients capture the net effect of the scheme as the discount was only available from Monday to Wednesday. The effect on footfall starts decreasing towards the end of the scheme, with estimates becoming statistically insignificant in the last week of September and in subsequent weeks.

Panel B of Fig. 2 presents the weekly estimates for the effect of EOTHO on job posts in the food preparation & service category. In line with what we observed for footfall, the job posts estimates are not statistically different from zero before August, again suggesting no obvious pre-trend prior to the start of the scheme. The *p*-value from a joint test for the equality of coefficients using the first four lags is 0.93. The point estimates are positive and significant from the beginning of August when the EOTHO scheme went live (3 August 2020). The coefficient is stable while the scheme is active and for three weeks after the scheme ended, although the statistical significance fluctuates over the eight-week period for which these effects are seen.

One interpretation of these patterns is that there was little hiring early on in anticipation of the increase in footfall. The lack of pre-trend could be partially explained because the gap between the program announcement (July 8th) and start date (August 3rd), meant little time for businesses to plan and prepare for operational changes and recruitment needed. Applications opened at the end of July, with the first businesses registering on July 30th, just 4 days before the program went live. In addition, the uncertainty around the response to the scheme could have delayed recruitment decisions. The results also indicate some late hiring in line with the overall increase in demand because of EOTHO. This late hiring could also reflect businesses' expectations of a more permanent change in demand induced by the scheme, even after it had concluded.[30]

Overall, our results suggest a positive, but transitory, effect on both footfall and job posts due to the EOTHO scheme. However, a transitory increase in job posts could still imply a permanent increase in the number of employees.

5. Discussion and conclusions

The UK's EOTHO scheme was a policy response to a major economic shock, which aimed to stimulate demand in a specific sector, and by doing so protect jobs and partly restore consumer confidence for visiting places. The scheme subsidized the cost of eating out Mondays to Wednesdays in August 2020.

The program had a relatively low take-up rate, averaging 33 % across LADs (Fig. A.9). We find evidence of spillovers in scheme participation as the presence of non-participating restaurant chains induced non-chain outlets to participate. Our results indicate that EOTHO increased footfall in the recreation & retail category. This effect is concentrated on days when the discount was available (Mondays to Wednesdays in August). The policy failed to encourage people to go out for other purposes and to eat out after the discount ended. The scheme also increased the number of jobs posts on the Indeed website in the food preparation & service category. This effect was also temporary, only lasting until the end of September. We do not find evidence of large spillover benefits to other industries. Our results may overestimate the effect of the program given that our data is unlikely to be representative of the population and business in the UK (Table O.13 in the Online Appendix B explores this).[31]

What can our estimates tell us about value for money? It is difficult to translate results for footfall into monetized benefits, but we can use our results for job posts to provide rough estimates of the costs per job.[32] The Indeed data does not allow direct assessment of whether job posts translated into new jobs, and if they did, whether these jobs persisted after the program ended. We also do not know the total number of job posts, or what proportion of jobs are posted on Indeed. Converting the percentage change in job posts to a percentage change in employment requires several assumptions. First, it requires the Indeed data to be representative of what is happening to overall job posts (see the discussion above). Second, given we do not know the total number of job posts we need to assume that the ratio of total job posts to total employment is constant between the pre and policy period. Using these two assumptions, and a figure for the conversion rate between job posts and employment (i.e., the number of jobs created for each job ad), we can convert the coefficient on job posts to a coefficient for employment by multiplying it by the conversion rate.[33] This requires a further assumption, because we do not know the conversion rate—we use several scenarios to give us some bounds. Multiplying this figure by the average number of participating outlets gives us an average percentage increase in employment by LAD. Getting to a figure for additional employment at the LAD level requires an estimate of total food sector employment at the LAD level, which is only available for March 2020 (essentially, pre-COVID-19). Finally, multiplying by the number of LADs gives us a figure for EOTHO as a whole (Table A.9).

For each scenario, we provide two estimates of cost per job—an upper bound derived by dividing the cost of the scheme (£849 million) by the number of jobs created when the scheme was live, and a lower bound by including the month after the program ended (when we continue to capture a positive effect). The upper bound is £2306.

[29] We are worried about positive pre-trends where higher mobility rates pre-EOTHO may drive more exposure to EOTHO (e.g., because restaurants in areas which get lots of tourists respond to seasonal increases in footfall by signing up to EOTHO). Given this, we construct the slope and the constant of a hypothesised pre-trend using the point estimate for one-week pre-EOTHO (first lag) and the lower bound of the confidence interval for two weeks pre-EOTHO (second lag). Following Staples and Krumel (2023), we compute the power of the pre-trend test under this plausible linear violation of the common trend assumption and get a power of .93 (for footfall) and 0.99 (for job postings) well-above 0.8 widely used as a recommended benchmark. We perform the same exercise when estimating the dynamic treatment effects for job posts (Panel B of Fig. 2), and we find that the power of the pre-trend test is 0.99 under a plausible linear violation of the common trend assumption.

[30] As described above, businesses could set an end date for the job post when creating the ad. They could also manually pause or cancel it at any time. If many did not set an end date and did not manually cancel the ad, they could remain on Indeed's website, and this could help explaining a positive effect after the scheme ended.

[31] The footfall data (from Google) is biased towards younger people, who may also be more inclined to go out. In the same way, job posts (from Indeed) may be biased towards larger businesses, which are also more likely to have capacity to hire more staff. Hence, our coefficients of both footfall and job posts could be upward biased and may correspond to upper bound estimates.

[32] It is important to note that these calculations are only indicative of the potential costs per job, and do not represent a comprehensive cost-benefit assessment of the program.

[33] Our main intensity in all tables is the number of outlets converted to logs and standardised to allow easy interpretation as the effect of a one standard deviation increase in participation. For these calculations, we drop the standardisation to get estimates using the (log of) the number of establishments in EOTHO as the intensity (see Table O.14 in the Online Appendix; we use results from column 6).

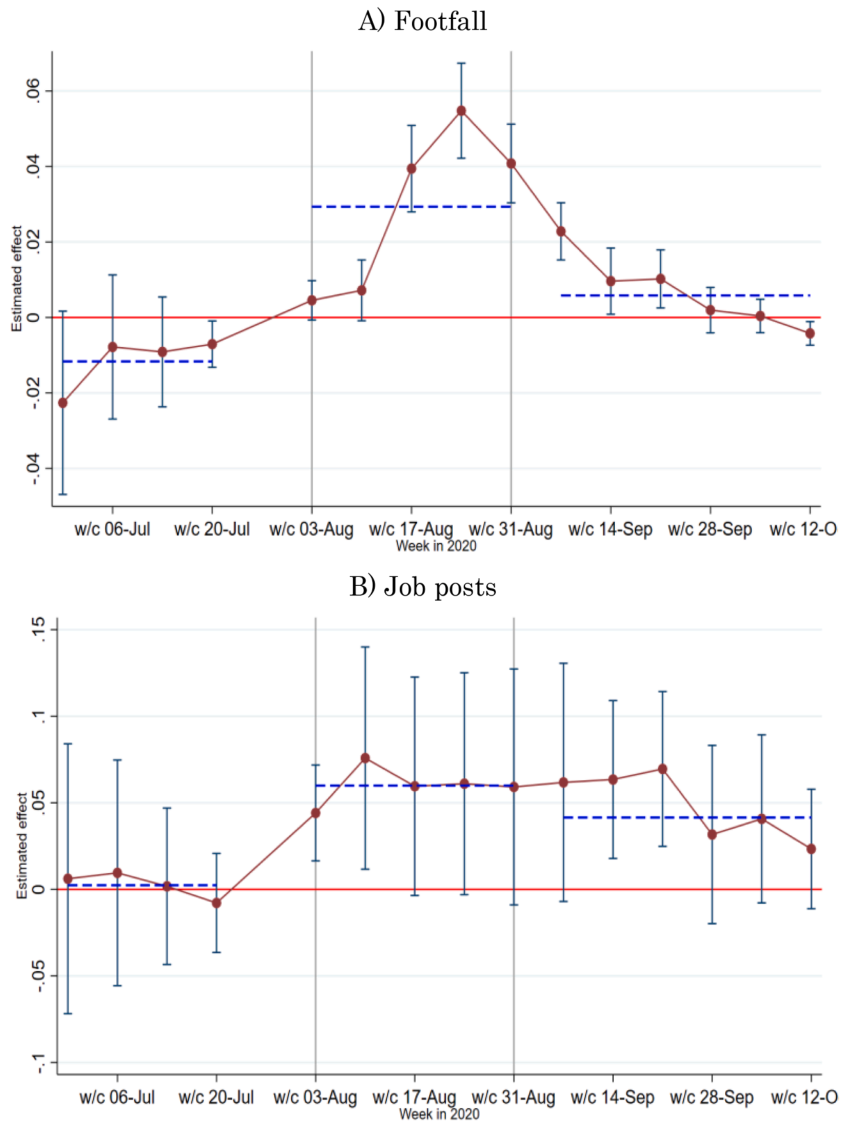

Fig. 2. Dynamic treatment effects.
Note: Weekly estimates for the effect of EOTHO on job posts using data at the LAD level. The estimates were obtained from our preferred DiD specification with the intensity of use of the scheme measured using the log number of outlets in EOTHO (column 3 of Table 1). The vertical black lines depict 95 % confidence intervals. The vertical grey lines highlight the weeks in which the EOTHO scheme was live.

Allowing for effects to persist for a month post scheme roughly halves the costs per job for a lower bound of £1153.[34] To put these figures in perspective, we compare the weekly cost against the average weekly earnings of employees from the wholesaling, retailing, hotels & restaurants sector in August 2020 (columns 4 and 6 of Table A.9). Our cost per job estimates are at least 3 times the average earnings of employees in this sector, even when considering a high ad to employment conversion rate and a longer duration for the effect.

How does this compare to other schemes? Aside from furlough, the main direct business support scheme in the UK during this period, for which no cots per job figures are yet available, was Coronavirus Business Interruption Loans Scheme (CBILS) which provided loans, rather than directly stimulating spending. EOTHO has some similarities with the US stimulus checks program—which provided direct cash payments to individuals—as both incentivized consumption as a policy response to the COVID-19 pandemic. The emphasis of studies looking at the effects of that scheme has been on the impacts on households, finding that stimulus checks had the largest effects on consumption among low-income households (Chetty et al., 2020) and those who lost their job or were on a temporary layoff, (Carroll et al., 2020). Given EOTHO targeted food establishments, there was no information collected on consumers making it hard to consider differences across households. Data at the LAD level suggests expenditure on food establishments due to the program was higher in locations with higher incomes than in the most deprived areas (Fig. A.10), the opposite of the US finding on low-income households.

EOTHO also shared some similarities with another US scheme, the Paycheck Protection Program (PPP), as both aimed to protect jobs and support small business.[35] EOTHO did this via a consumption subsidy (targeted to the food industry), while the US provided funds directly to businesses (with 500 or fewer employees but not targeted by sector or

[34] This assumes that all the induced jobs persist for either the duration of the program or two months. We do not differentiate between part-time and full-time employment.

[35] PPP provided support in the form of uncollateralised, low-interest loans (most of them forgivable).

type of firm). Several studies find positive effects of PPP on firm survival and employment (Staples and Krumel, 2023; Hubbard and Strain 2020; Bartik et al., 2020). Autor et al. (2022) and Granja et al. (2022) suggest PPP had annual costs per job of at least £133,000 (US $ 175,000). Granja et al. (2022) concluded that many firms used the financial support received for non-payroll purposes, which can help explain the small employment effects. Annualizing the weekly cost per job of EOTHO—assuming the same cost per week and duration of employment—gives a comparable estimate for EOTHO of at least £120,000. Given the multiple assumptions needed to calculate the cost per job for EOTHO we take this as providing no more than a rough benchmark.[36]

Despite some progress, which of these alternatives (subsidizing consumers in one industry or providing untargeted financial support directly to businesses) could be more effective in terms of job creation remains an open question. Compared to sector earnings, the costs per job from both programs are relatively large. For EOTHO our figures suggest costs at least equal to three times what employees in the sector earned in August 2020 (in our most conservative scenario). The issues previously described, as well as the interaction across different schemes, complicates any full cost-benefit calculation of EOTHO. Further research is needed to assess the overall cost-effectiveness of this and similar programs for boosting demand in specific sectors and supporting the economic recovery after severe disruption to the economy. Although, the high cost per job of EOTHO support would urge considerable caution in using similar programs in the future.

CRediT authorship contribution statement

Nicolas Gonzalez-Pampillon: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Writing – original draft, Writing – review & editing. **Gonzalo Nunez-Chaim:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing. **Henry G. Overman:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

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[36] As described above, we do not know if the jobs posts effectively materialised into new jobs, or if any changes in employment were permanent or temporary. We also do not know whether the boost in demand effectively led to higher turnover or if it increased the probability of firm survival. Finally, there is no publicly available data that allows assessment of the price effects and spending behaviour of EOTHO, which would be helpful to provide insights on deadweight and the distortions introduced by the subsidy. A descriptive analysis from the ONS suggests that consumer inflation would have been around 0.9 % in August 2020 without EOTHO and the VAT reductions scheme, compared to the actual rate of 0.5 % (Office for National Statistics 2020).

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2024.103682.

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