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This paper documents that a large-scale government subsidy aimed at encouraging people to eat out in restaurants in the wake of the first 2020 COVID19 wave in the United Kingdom has had a large causal impact in accelerating the subsequent second COVID19 wave. The scheme subsidized 50% off the cost of food and non-alcoholic drinks for an unlimited number of visits in participating restaurants on Mondays-Wednesdays from August 3 to August 31, 2020. Areas with higher take-up saw both, a notable increase in new COVID19 infection clusters within a week of the scheme starting, and again, a deceleration in infections within two weeks of the program ending. Areas that exhibit notable rainfall during the prime lunch and dinner hours on days the scheme was active record lower infection incidence – a pattern that is also measurable in mobility data – and non-detectable on days during which the discount was not available or for rainfall outside the core lunch and dinner hours. A back of the envelope calculation suggests that the program is accountable for between 8 to 17 percent of all new local infection clusters during that time period.

JEL Classification: N/A

Keywords: health, Externalities, Coronavirus, Subsidies, consumer spending

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Subsidizing the spread of COVID19: Evidence from the UK's Eat-Out-to-Help-Out scheme

Thiemo Fetzer *

October 28, 2020

Abstract

This paper documents that a large-scale government subsidy aimed at encouraging people to eat out in restaurants in the wake of the first 2020 COVID19 wave in the United Kingdom has had a large causal impact in accelerating the subsequent second COVID19 wave. The scheme subsidized 50% off the cost of food and non-alcoholic drinks for an unlimited number of visits in participating restaurants on Mondays-Wednesdays from August 3 to August 31, 2020. Areas with higher take-up saw both, a notable increase in new COVID19 infection clusters within a week of the scheme starting, and again, a deceleration in infections within two weeks of the program ending. Areas that exhibit notable rainfall during the prime lunch and dinner hours on days the scheme was active record lower infection incidence – a pattern that is also measurable in mobility data – and non-detectable on days during which the discount was not available or for rainfall outside the core lunch and dinner hours. A back of the envelope calculation suggests that the program is accountable for between 8 to 17 percent of all new local infection clusters during that time period.

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1 Introduction

The COVID19 pandemic caused by the novel coronavirus (SARS-CoV) has left a significant mark on many economies. The hospitality sector is particularly vulnerable to the economic fallout due to an unprecedented decline in tourism and leisure activities (Brinca et al., 2020; Carvalho et al., 2020; Dingel and Neiman, 2020). Naturally, lockdown measures in spring, which were implemented to slow down the uncontrolled spread of COVID19, directly impacted the hospitality sector's ability to provide its goods and services. Yet, the behavioral changes in consumption patterns due to consumers trying to avoid infections may lead to the hospitality sector to continue to suffer sustained contractions in demand, especially if the disease spread is perceived as being uncontrolled (Baker et al., 2020; Fetzer et al., 2020; Mongey et al., 2020). Since the changed consumption patterns are a direct result of the virus presence in many countries, economists have broadly suggested that aggressive testing- and tracing schemes and the suppression of the virus' spread may be most cost effective strategy to aid the economy (Brotherhood et al., 2020; Kaplan et al., 2020; DELVE, 2020). Nevertheless, some governments have attempted to specifically stimulate demand for the hospitality sector: this paper studies to what extent one such large scale intervention in the UK, the so-called Eat Out to Help Out (henceforth, EOHO) scheme – had the unintended effect of furthering COVID19 infections.

The EOHO scheme was conceived to shore up demand for the hospitality- and restaurant sector in the UK. It effectively saw the cost of meals and non-alcoholic drinks being slashed by up to 50% across tens of thousands participating restaurants across the UK for meals served on all Mondays to Wednesdays from August 3 to August 31, 2020. The discount was capped at a maximum of GBP 10 per person but there was no limit on how often it could be claimed per individual. Early release statistics suggest that during the four weeks in which the program was active, a total of nearly 100 million covers were claimed, at a total cost to the taxpayer of around GBP 500 million. The total value of meals for which the discount was claimed was around GBP 1 billion.[1] Restaurant visits increased dras-

[1]This represents a large share of the the broader non-residential catering sector which is esti-

tically on weekdays Monday to Wednesday, which usually see less traffic, even in a year-on-year comparison. Given a growing body of evidence from epidemiological studies, which suggests that restaurants may be a particular prominent vector of COVID19 transmission (see e.g. Hijnen et al., 2020; Fisher et al., 2020; Lu et al., 2020), this naturally raises the question to what extent the EOHO scheme may have contributed causally to the drastic acceleration of the spread of COVID19 seen in early fall 2020 across the UK.[2]

This paper leverages spatially and temporarily granular data from the UK to make four observations. First, the EOHO scheme appears to have led to a significant increase in restaurant visits over-and-above the levels in the previous year and potentially shifting visits to the weekdays on which the discount was available. Second, areas that have relatively more participating restaurants saw a notable increase in the emergence of new COVID19 infection clusters starting around one week after the scheme launched. Third, the time-patterns of the differential emergence of COVID19 infection clusters across areas with larger uptake of the scheme closely tracks the time-pattern of visits that the scheme appears to have induced when studying Google (2020) mobility data and aggregate data from restaurant booking sites. Fourth, we observe a notable decline in new infection clusters in areas with higher take up of the EOHO scheme around a week after the scheme ended. This again, follows closely patterns in aggregate restaurant visit pattern data, which saw a drastic decline in restaurant visits after the scheme ended, suggesting that the positive economic impact have not been sustained.

The difference-in-difference design findings are already very consistent with the state of epidemiological knowledge and the EOHO program scheme specifics. Nevertheless, there may be concerns about reverse causality. I complement the above findings with further reduced form evidence that, at least, is indicative of the direction of causality. Using very granular high frequency rainfall data, I document that areas that experienced notable amounts of rainfall *during lunch- and dinner hours* on the weekdays during which the discount was available had fewer

mated to have gross-value added of around GBP 37 billion per year.

[2]See also Marcus et al. (2020) which, using US data, suggests that adults with positive SARS-CoV-2 test results were approximately twice as likely to have reported dining at a restaurant than were those with negative SARS-CoV-2 test results.

COVID19 infection clusters emerging relative to areas that saw little or no rain during these hours. These patterns are remarkably robust: rainfall during the same lunch- and dinner time hours on days in the week during which the scheme was not active is uncorrelated with the emergence of new COVID19 infection clusters. Similarly, rainfall during days on which the scheme was active but which fell outside the core lunch- and dinner-time hours is uncorrelated with the subsequent emergence of COVID19 infection clusters. These differential patterns can only be detected during the four calendar weeks when the scheme was active – but not in the immediate four weeks before- and after the scheme was active – which mark periods during which no part of the country was under any form of lockdown.

Naturally, rainfall may affect human mobility in many different ways and may have direct impacts on the spread of COVID19, thereby threatening the implicit exclusion restriction implicit in the above reduced form design. To, at least partially allay these concerns, I again turn to coarser district level Google mobility data. Consistent with the above patterns on COVID19 infections, I find that rainfall during peak lunch and dinner hours is associated with notably less restaurant visits. Further, consistent with the results on infections, these effects are only present during weekdays days on which the scheme was active and for rain falling around the core lunch and dinner hours (but not for rainfall falling outside these hours or weekdays during which the scheme was not active). Further, the patterns do not emerge in the four weeks prior and four weeks after the scheme was officially active. Lastly, the intra-day rainfall measures have no statistically discernible impact on mobility proxies capturing visits to grocery stores, transit places or workplaces, suggesting that the restaurant visit patterns that midday or evening rainfall caused are not confounding more general or other mobility changes. This is further indirect evidence suggesting that the EOHO scheme indeed *caused* a significant increase in COVID19 infections and that the exclusion restriction implicit in the reduced form exercise holds.

The observed empirical results are very robust to a host of further additional checks and exercises. First, it is noteworthy that the timing of the effects on new COVID19 infections is very consistent with the program timings, both in terms of onset- and offset. Second, results are robust to accounting for very demanding time effects that can capture both, various local policy shocks, as well as account for the inherently non-linear and local disease dynamics. Third, results are further robust to controlling non-parametrically for non-linear time trends in a host of other factors that may be confounding the progression of the pandemic into its second wave and also be correlated with program take-up. These are a range of proxies of population density; several measures capturing an areas' exposure to the spring 2020 COVID19; an areas' usual exposure to both in- and out commuting flows; the share of regular university student residents; the structure of the local housing market. Fourth, results are not driven by any one specific major metropolitan area, which we confirm through a host of leave-one-out exercises at different geographic granularities. Fifth, results are not driven by the specific choice of functional form or the precise measurement of the likely area-specific scheme take-up. Especially the latter is relevant as official EOHO statistical release data is yet pending. This paper leverages detailed and granular restaurant-specific data from the government's own public Github repository through which the central "restaurant finder" application was run. This app was the primary go-to website that was used to help interested consumers identify participating restaurants within their neighborhood. We further combine this with a spatially coarser pre-release take-up data measuring the number of meals claimed.[3]

The empirical estimates suggest that the EOHO scheme may be responsible for around 8 to 17% of all new detected COVID19 clusters emerging during August and into early September in the UK. Given the dramatic rise of COVID19 infections across the UK in recent weeks, the likely changes in consumer behavior due to higher infection risks and the ensuing economic damage this generates suggests that the EOHO scheme may have indirect economic- and public health costs that vastly outstrip its short term economic benefits.

[3]Further, more granular EOHO data may be made available in the future. This data will, at best, only help to improve the accuracy of the take-up measures further. We think at present, given we are primarily measuring an intention-to-treat, that we are currently underestimating the true effects. Unfortunately data on COVID19 fatalities at the geographic granularity needed for the analysis is still pending. The UK's official COVID19 tracking website does not provide data on COVID19 fatalities at the same geographic level at which it publishes weekly new local infection data. Monthly mortality data will be produced by the Office of National Statistics and is expected to be forthcoming for the August period the earliest in December 2020.

The paper is related to a rapidly growing literature studying the economic implications of the COVID19 pandemic. The macroeconomic literature has put specific emphasis on understanding how to think of the optimal policy in the context of externalities in individual distancing decisions and socially optimal lockdowns. Notable work is also being conducted to track the economic implications of the pandemic in real-time across a host of margins, such as inequality (Adams-Prassl et al., 2020b,c; Benzeval et al., 2020; Blundell et al., 2020), gender-differentiated effects (Alon et al., 2020), across sectors and social economic groups (Mongey et al., 2020; Coibion et al., 2020b). There is also a growing literature that studied the impacts and implications of various fiscal countermeasures (see Bayer et al., 2020; Coibion et al., 2020a; Kaplan et al., 2020, to name a few). This paper puts an emphasis on a specific fiscal countermeasure that, even at the time of announcement, has been criticized by epidemiologists and economists for its "backfire potential" given the known risks of infection in restaurant settings.

In the broader literature, the paper is related to the strand of work that speaks to the complexity of economic policy making in the wake of a pandemic in a world with both economic externalities and health externalities. Targeted fiscal interventions may be optimal if they reduce both the negative economic impacts of the pandemic, while at the same time, putting in check the underlying health externalities that certain types of economic behaviors may bring about. Most countries opted for a broad set of measures to prevent sectors from making drastic adjustments to its workforce through the expansion of furlough schemes (see Adams-Prassl et al., 2020a for work on the UK scheme), targeted financial and liquidity support to companies, as well as broad demand stabilizing initiatives such as those implemented through the CARES Act in the US (Coibion et al., 2020b) or through measures to temporarily lower the VAT.

The UK's policy response shared many of the broader features of most fiscal interventions. Yet, the EOHO scheme stands out internationally. The intervention, not only reverses a lockdown that ordered in-dining restaurant activity shut (as e.g. studied in Glaeser et al., 2020), but rather was targeted post-lockdown to actively increase demand in the sector. Given a broad set of epidemiological work that suggests that the health externalities associated with hospitality-sector related economic activity may be particularly high (see e.g. Hijnen et al., 2020; Fisher et al., 2020; Lu et al., 2020), the overall soundness of the scheme stands in question. The paper documents that the scheme has broadly accelerated the pandemic, naturally, the question arises to what extent policy makers designing the scheme may not have been aware of the epidemiological risks associated with it or, to what extent misperceptions around trade-offs between "lives versus the economy" may have shaped attitudes towards the scheme.

The paper proceeds as follows: section 2 presents the policy context and the underlying data leveraged in this paper. Section 3 presents the empirical approach, while Section 4 presents and discusses the results. Section 5 concludes.

2 Context and Data

2.1 COVID19 in the UK

Despite the UK having had advance warning, the government was slow to respond to the COVID19 spread early in the year (see Fetzer et al., 2020). The relatively late decision to lockdown the country resulted in a much longer period during which the lockdown needed to be maintained. This has resulted in a significant drop in GDP, which was quite prominently felt in the UK's hospitality and high-street retail sectors (see e.g. ONS, 2020).

Measuring COVID19 spread We leverage data from the UK's official COVID19 reporting dashboard available on `https://coronavirus.data.gov.uk/`. This provides data on weekly COVID19 case counts at the level of Middle Layer Super Output Area (MSOA) and the district-level.

MSOA's are statistical regions that can be broadly compared to wards. MSOA's have, on average, 8,288 residents but at least 5000 residents as per the 2011 Census. Across England, which is the study area, there are 6,791 MSOAs. For confidentiality protection, the data that is publicly available suppresses counts that are below 2. This implies that not all infection clusters may be detected in this data. An individual case gets recorded and attributed to the MSOA based on his or her residence address. Further, the date of infection is recorded, not based on the date on which the rest result is reported, but rather, based on the date at which a test specimen sample was taken. As it is estimated that at least 50% of symptomatic cases report symptoms within three to five days of infection (see e.g. Qin et al. (2020); Lauer et al. (2020); Chun et al. (2020) for estimates), it is thus not unreasonable for COVID infections occurring on Mondays - Wednesdays to be recorded as early as within the same calendar week in case individuals had a test done later in the week.

COVID19 spread extremely rapidly across the UK. Figure A1 presents the share of MSOA's that had at least two cases per week. All reported cases are linked to the date that the specimen for the test was taken and not the date when the test report came back. While in the week prior to the launch of the EOHO scheme just 7% of English MSOA's reported a local outbreak with more than two new cases, just four weeks later, when the scheme ended, 30.6% of MSOAs reported local outbreaks with more than two new cases of COVID19. We assess to what extent the rapid spread of COVID19 may be attributable to the government operated EOHO scheme.

The resulting dataset at the MSOA level is a balanced panel across 6,791 English MSOAs by calendar week from calendar week 5 to calendar week 40. For this data set, the primary dependent variable is an indicator capturing whether there have been more than three cases reported in any given week. The binary coding is appropriate given the high spatial granularity and the fact that low numbers (less than or equal to two) are not reported for confidentiality protection. We nevertheless also exploit the weekly new case count at that level as well. The dataset has around 250,000 observation, yet, we primarily focus on the time around which the EOHO program was active as quite likely, case counts from early into 2020 are not very reliable due to poor testing performance. Nevertheless, all results are robust to working with the full sample.

2.2 Eat Out to Help Out

The hospitality sector in the UK, as elsewhere, took a significant economic hit as a result of the lockdowns that were implemented in most countries early in 2020. The sector is an important source of employment opportunities for individuals with lower qualifications; it provides a source of income for students and those entering the labor market and is often characterized by a widespread prevalence of small and medium enterprises and often quite deep local economic linkages.

While many countries opted to expand their furlough scheme and provide various forms of direct financial support, the UK government was quick to announce a phase-out of its furlough scheme. Rather, it opted for measures to stabilize demand facing the hospitality sector. Two primary initiatives were taken: a temporary VAT cut for businesses in the hospitality sector, lowering the applicable VAT rate from 20% to 5% as an indirect measure that quite likely will increase margins of restaurants (see Benzarti and Carloni, 2019). At the same time, the government implemented a demand-stabilizing program that was dubbed the "Eat Out to Help Out" (henceforth, EOHO) scheme.

Under the Scheme Government will provide 50% off the cost of food and/or non-alcoholic drinks eaten-in at participating businesses UK-wide. The scheme ran from 3 August (calendar week 32) to 31 August 2020 (calendar week 36), but was only active on Monday-Wednesday. The discount was capped at a maximum of GBP 10 per person. In order to benefit from the scheme, eligible hospitality businesses had to register with the UK's Tax Authority, Her Majesty's Revenue and Customs (HMRC) to participate. Once registered, businesses can offer the discount to customers and claim the money back from HMRC. We primarily leverage this data for this paper using the HMRC Github repository providing the database of registered restaurants. This database was used to develop the governments online restaurant finder showing businesses registered in the EOHO scheme within 5 miles of any postcode entered.

While official statistics on the scheme are still pending, preliminary reports suggests that up to 84,000 premises (either individual restaurants or chains) may have registered for the scheme by late August with a total of nearly 100 million covers (individual meal claims) being sponsored. The average claim was GBP 5.25, just over half the GBP 10 maximum per person. Figure 1 highlights that the program did have a notable, but temporary, impact on restaurant visits when comparing year-on-year changes using data made available as a time-series for the UK by online booking site OpenTable. During the days that the program was active, restaurant visits, year-on-year, increase drastically by up to 100% in the last week during which the program was active. Especially in the first two weeks that the program was active it appears, however, that visits may have simply shifted within the week to earlier weekdays. The effects appear to not persist with visits starting a declining trend year-on-year as soon as the program ended. The significant increase in restaurant visits, within a short period of time and concentrated within a few days in the week, may have had a notable impact on the spread of COVID19. In order to study this using spatially granular data, we leverage two data sources to measure the uptake of the EOHO scheme.

Restaurant registrations While official statistical data is yet to be released (see `https://www.gov.uk/government/statistics/announcements/eat-out-to-help-out-statistics`), some program data was made already available. Naturally, as the data are preliminary, a qualifier about the potential impacts that measurement error could have on the results presented here needs to be made.

For restaurants to participate in the program they had to register with the HMRC. The data team in the HMRC made restaurant registrations with names and addresses publicly available in a GitHub repository (see `https://github.com/hmrc/eat-out-to-help-out-establishments`). Registrations were open during the full program duration. The repository was updated from late July to early September almost daily, providing a time-varying number of participating restaurants. Figure A2 presents the time series of the data available on that repository. At the start of the program, around 53,059 restaurants were registered to participate. Towards the end, that number had increased by nearly 10,000 restaurants to 62,804.

We will exploit the expansion of the program in addition to cross-sectional measures capturing the intensity of the program's use across districts and MSOA's to study whether the program is associated with an acceleration of the spread of the pandemic. For each participating restaurant, at each point in time, we know the exact address including the full post-code. This allows us to map the restaurants to any geographic unit in the UK. Figure 2 displays the distribution of the number of participating restaurants per 10,000 residents at the end of the program. There is ample and notable spatial variation. We will control in all exercises for population density, an area's exposure to the first wave of the pandemic and a host of other factors that shape the local restaurant supply-side structure.

Early Release Statistics A second method of measurement are statistics that were published at the level of the constituency. These have been available at https://www.gov.uk/government/publications/eat-out-to-help-out-scheme-claims-by-parliamentary-constituency. The data provided information on the total number of restaurants by constituency that participated in the program; the number of meals that were claimed under the scheme; the amount of money disbursed to participating restaurants and the average value of the claim. The data were subsequently removed from the website in mid October, likely due to an error in the geographic attribution of some chain restaurants to constituencies. Nevertheless, for the purpose of the empirical exercises presented here, the data is likely to contain sufficient statistical signal as the measurement error, if anything, does not appear to be systematic.

The correlation, for example, between the number of restaurants participating from the micro data on registrations aggregated to the constituency and the number of restaurants per constituency as reported in the early release file is very high (see Appendix Figure A3). Given the tight correlation, the data still contains valid statistical information to be leveraged. Nevertheless, all results obtained here are robust to not using this data at all and working solely with the data on restaurant registrations that come directly from HMRC's Github's repository.

England has around 533 constituencies. Constituencies are spatially much coarser compared to MSOAs. We break down the reported figures on the number of meals claimed to the MSOA level using the number of active restaurants as weight. This gives us a measure of the number of meals claimed as an additional, albeit, inferred measure of take-up at the local level, in addition to the time-invariant and the time-varying number of restaurants at the MSOA.

2.3 Rainfall data and other data

I will show that intra-day rainfall around lunch- and dinner hours measured on days during which the EOHO discount was available is strongly predictive of both, visits to restaurants measured using the Google mobility data, and COVID19 infections.

Rainfall data I use data from the Global Satellite Mapping of Precipitation (GSMaP) project which provides a global hourly rain rate with a 0.1 x 0.1 degree resolution (around 10 x 10 km at the equator). This dataset measures rainfall around the globe in near real time using multi-band passive microwave and infrared radiometers from the GPM Core Observatory satellite with more technical details provided in Okamoto et al. (2005). The rainfall rate measure I leverage here is adjusted for rainfall gauge measures on the ground providing an hourly rainfall rate. To construct the measures I obtain the hourly images and map the grid cell to the centroid for each MSOA.

To construct the rainfall during the lunch- and dinner times I sum up the hourly rainfall rates on each day for lunch hours from 11:00 - 14:00 (inclusive) and dinner time from 17:00-21:00 (inclusive). I also construct the rainfall falling outside these hours. As the infection data at the local level is provided only at the weekly level I aggregate up the rainfall occurring on weekdays during which the EOHO discount was avail is active (Mondays-Wednesdays) as well as for the rest of the week.

For the mobility exercise, I can leverage the daily rainfall data directly.

Google mobility data To understand to what extent the EOHO scheme was changing or affecting local patterns directly, we also use data from the Google Mobility indices – see for example, Besley (2020) for a use of the data with an application to political economy. The data is provided by Google at a level that is slightly coarser than local authority districts, but nevertheless, they can be mapped to the districts. We use the daily-level data to measure the impact of the EOHO scheme on mobility within districts over time to provide corroborating evidence.

I next provide the empirical approach before presenting the main results.

3 Empirical Strategy

The paper primarily studies data at the granular Middle Layer Super Output (MSOA) area. These statistical regions are constructed based on granular census geographies and nest into the original 2011 census geographies and can be broadly considered to be well-defined neighbourhoods with at least 5000 inhabitants. The relative homogeneity in terms of population size make it particularly appealing for statistical purposes. I also conduct some supporting analysis at the much coarser local authority district level, which, due to its coarseness, allow for much less demanding empirical specifications and require stronger identification assumptions.

3.1 Difference-in-difference analysis

I follow a several simple difference-in-difference strategy exploiting cross-sectional variation across MSOAs in the the exposure to the EOHO scheme E_i. That is, I estimate

$$y_{i,t} = \eta_i + \gamma_{l(i),t} + \eta \times Post_t \times E_i + \beta' X_{i,t} + \epsilon_d \tag{1}$$

where $y_{i,t}$ denotes a measure of COVID19 spread. The primary focus of this paper is to measure the emergence of new COVID19 infection clusters. This is primarily motivated by the data granularity measuring new COVID19 infections at the MSOA level by week. For confidentiality reason, instances with less than or two cases are suppressed. As such, we only observe new cases in the data if there are more than two infections. While the results are not sensitive to the choice of the dependent variable or the functional form, it nevertheless seems sensible to focus on measuring the incidence of new COVID19 clusters capturing more than two detected infections in a given week.

The regression controls for district- or MSOA fixed effects, η_i, as well as a set of time fixed effects $\gamma_{l(i),t}$. We explore a range of different time-fixed effects at different spatial resolutions. For example, we can control for district by time fixed effects or Westminster constituency by time fixed effects. The potential for uncontrolled spread of COVID19 makes such flexible time-effects quite relevant as a potential set of controls. They further have the appealing feature of absorbing any time-varying policy shocks. This is particularly relevant for the case of local authority districts as many COVID19 restriction measures and policies are implemented at that level in the UK.

E_i is a measure of an area's exposure to the EOHO scheme. I work with two primary measures capturing either the number of restaurant establishments within an MSOA that participate in the EOHO scheme or, an imputed measure of the number of meals covered under the scheme, exploring various functional form or variable

The above specification naturally also extends to a more flexible econometric specification that allow us to explore to what extent there are common trends in infection outcomes before the scheme started e.g. by estimating.

$$y_{i,t} = \eta_i + \gamma_{l(i),t} + \sum_t \eta_t \times \mathbb{1}(Week = t) \times E_i + \beta' X_{i,t} + \epsilon_d \tag{2}$$

What is important in the above specification is that we can not only explore how the program may have led to an increase in infection *from the onset*, but also can study to what extent infection dynamics slow down as the scheme ends. Throughout the paper, we cluster standard errors at the level of the local authority district.

Control variables In addition to conducting various robustness checks, we can control for a host of potential time-varying factors measured in $X_{i,t}$ that may drive the spread of the disease independently from the EOHO scheme. We will explore a large range of potential confounders, flexibly controlling for them across the empirical exercises, such as an area's exposure to the first pandemic wave; commuting patterns; the prevalence and distribution of different types of commercially used real estate – to measure an areas' specialization in leisure or production; population density and variability in population density; the local age profile or demographics, among many others.

3.2 Exploiting time-variation in (likely) restaurant visits

Unfortunately, at the time this paper was written, time-varying measures of the uptake of the EOHO scheme broken down to different geographic units was yet missing. Nevertheless, I also adopt a reduced form approach exploiting time-variation in weather across areas of the UK around lunch- and dinnertime on weekdays when the scheme was active.

Specifically, I fully focus on the data for the calendar weeks during which the scheme was active from August 3 to August 31, exploiting time-variation within- and between MSOAs in whether an area saw notable amounts of rainfall during the primary lunch- and dinner-time hours during which individuals may have conceivably taken advantage of the substantial price discount to visit restaurants. That is, I estimate

$$y_{i,t} = \eta_i + \gamma_{l(i),t} + \xi \times R_{i,t} + \epsilon_d \tag{3}$$

where again $y_{i,t}$ is a dummy variable that measures whether a new COVID19 infection cluster was identified in an area in a calendar week. The timing is not particularly sharp but epidemiological estimates suggest that 97% of non asymptomatic patients develop symptoms within 8.2 to 15.6 days of infection (Lauer et al., 2020). At least 50% of symptomatic cases report symptoms within two to five days of infection (see e.g. Qin et al. (2020); Chun et al. (2020) for estimates), it is thus not unreasonable for COVID infections occurring on Mondays - Wednesdays to be recorded as early as within the same calendar week in case individuals had a test done later in the week. I will nevertheless show that results are further robust to the precise choice of timing.

The above regression controls for local authority by week fixed effects, again, to account for non-linear growth and potential confounding policy shocks. I construct a whole set of different rainfall measures $R_{i,t}$ which measure the amount of rainfall from 11:00-14:00 proxying potential lunch-time restaurant visitors and from 17:00 to 21:00 proxying potential dinner-time restaurant visits. These measures are constructed for each weekday. I also construct a rainfall measure for the non-peak potential restaurant visitor time-windows. This allows a host of placebo exercises that will be further supported by the mobility analysis described next.

3.3 Supporting Mobility analysis

To corroborate the findings I leverage data from Google Mobility indices providing measures of the percent change relative to the pre-COVID19 baseline in mobility measuring visits and the time spent in Retail, Restaurant, Parks, Workplaces vis-a-vis the home. This data is available broadly speaking at a slightly coarser version of the UK local authorities, but can be matched to them. The data is available by day and allows for a set of analysis that can corroborate the reduced form evidence on infections.

First, I study at the week level that mobility patterns proxying time spent and visits to Restaurants significantly responded to the roll out of the scheme. I estimate variations of specification 1 at the local authority level, documenting how mobility proxies proxying restaurant visits changed on EOHO days comparing the EOHO weekdays both before- and after the scheme started and subsequently after it ended.

Similarly, I study the daily data for just the time-period during which the scheme was active, exploiting the same type of weather variation, now at the district level, as discussed above. Specifically, I estimation variations of specification 3 at the district level, studying to what extent restaurant visit mobility proxies appear to decrease on EOHO weekdays in districts that experienced some notable rainfall. Again, this exercise allows for a host of additional placebo exercises, the results of which will track closely the patterns identified from the infection data.

4 Results and Discussion

4.1 Difference-in-difference analysis

We begin by presenting some evidence that highlights that the scheme was successful in mobilizing more people to Eat Out

4.1.1 EOHO substantiallly increased restaurant visits

We begin by studying Google mobility data to explore to what extent the scheme appears to have attracted more visits to restaurants – and to what extent the patterns change after the program ended. Figure 1 provides a daily time series of restaurant booking service OpenTable across all channels of potential visitors: online reservations, phone reservations, and walk-ins across the UK. The measure captures changes year-on-year relative to the same day in the previous year. The vertical lines indicate the start- and end-dates of the EOHO scheme. The individual dates when the EOHO subsidy was available across participating restaurants in the UK are marked as circles while weekend days of Friday, Saturday and Sunday are indicated as solid black lines. We make three observations: first, while restaurant visits had been recovering from substantially lower bookings relative to the previous year in July, there are notably higher restaurant visits on days during which the scheme was available – the increases suggest that restaurant visits year-on-year are between 10% to more than 200% higher.[4] Second, the patterns suggest that prior to the scheme being active, there is a notable increase in visits during on weekend days within a week – this pattern seems to disappear during the weeks that the EOHO scheme was available, which may suggest that the scheme may have led to shifting of planned visits within the week to days during which the discount was available. Third, there are notable declines in restaurant visits after the scheme ended with visits starting to decline again relative to the previous year.

Figure A5 highlights that the scheme had a signifiant and timely effect, increasing mobility in the category Retail & Recreation, which includes places such as restaurants, cafes, shopping centres, theme parks, museums, libraries and cinemas. On the weekdays Monday to Wednesday, during which the program was active, the mobility score increased drastically by around 6 percentage points. Relative to the mean, this is an increase of around 22%.[5]

The scheme ended on August 31, 2020 as the last Monday during which it was active in calendar week 36. Mobility in restaurants and cafes dropped significantly from week 36 again and does not recover. This suggests that the programs effects were primarily temporary in nature and may have had the adverse effect fo shifting and concentrating restaurant visits to earlier in the week, possibly increasing the

[4]August 31 was the last, so-called Summer Bank Holiday which marks a public holiday. The date of these bank holidays are changing year-on-year with the corresponding holiday the previous year being August 26, 2019. This results in a notable inflation of the restaurant visits in the year-on-year comparison.

[5]Appendix Table A1 highlights that, while there are other changes in mobility, these are relatively marginal. For the empirical exercise at the much more spatially granular MSOA level, we will fully account for mobility and policy changes through flexibly controlling for district by week fixed effects or even constituency by week fixed effects, fully absorbing changes measured at this level.

infection risk even further as restaurants may have been at capacity or possibly even beyond capacity.

4.1.2 Impact of EOHO on infections

We next turn to presenting results from the difference-in-difference analysis. The results are presented in Table 1. As dependent variable in this exercise we use the binary indicator measuring whether there was a new COVID19 infection cluster comprised of more than two new cases that were detected within a given calendar week. The sample period here covers calendar weeks 24 to 36.[6] Across the different panels in the regression table, I explore different ways of measuring the exposure of an area to the EOHO scheme. In Panel A, I measure the exposure as the log number of EOHO covered meals consumed in an area d or the log number of participating restaurant establishments in an area, each normalized by an area's population. For ease of interpretation, the measures are normalized to have unit standard deviation. Across columns, I explore different levels of time-fixed effects moving from coarser NUTS2 region by week fixed effects to much more granular local authority district by week fixed effects.

The results suggest that there is a notable positive and precisely impact: areas that have a higher exposure to the EOHO scheme see notably higher incidences of infections during weeks that the scheme operated. In Panels B and C I show that it is immaterial how we measure the exposure to the scheme in this difference-in-difference exercise. Overall, the estimates across columns and panels suggest that a one standard deviation higher exposure to the EOHO scheme increased the incidence of new infection clusters by, on average, between 0.008 to 0.017 percentage points. Relative to the mean of the dependent variable, this suggests that the EOHO scheme can account for between 8 to 17 percent of all new infections during the period in which the scheme was active.[7]

[6] The respective start time period is insubstantial for the estimation. Week 24 marks the start of the easing of the lockdown restrictions following the first COVID19 wave in the UK.

[7] The results are not an artefact of the choice of functional form studying new COVID19 infections as a binary variable. Appendix Table A2 highlights results are robust to alternative functional forms.

Impact over time A natural question that may arise is whether the results are picking up some trends in infections that may precede the scheme being active. Further, given that the scheme was only available for around four calendar weeks, it raises the question whether the increased infections that are attributed to the scheme continue in the same areas that saw higher exposure to the scheme, after it ended and after which restaurant visits notably declined again. Figure 3 estimates a flexible difference-in-difference design that can provide some answers to these questions. Infections steadily increase around one week after the scheme started in areas that had higher uptake, peaking in the last week the scheme was available, and, from then on declined again. There is no evidence of any diverging pre-trends prior to the program being available. This maps closely to the patterns observed in aggregate restaurant bookings data in Figure 1 as well as the patterns observed in mobility data at the district level in Appendix Figure A5.

We next present some further robustness checks that can allay some plausible concerns.

4.1.3 Robustness

In Table 2 I explores to what extent results are robust to controlling for the potential time-varying impact of other area characteristics that have been muted to being potential correlates or drivers of a subsequent second wave. The specifications across columns include local authority by week fixed effects throughout and successively add more additional control variables that are fully interacted with time fixed effects to allow for non-linear growth patterns in these characteristics. The variables consist of a set of measures capturing population density in column (2), adding measures capturing an area's exposure to the first wave of the pandemic in spring in terms of cases and COVID19 and non COVID19 mortality figures in column (3). Column (4) adds further an areas exposure to both in- and out-commuting flows, which is relevant given that the time marked the reopening of the economy with public calls for people to go back working from their respective offices. Column (5) adds measures capturing the share of the population that lives in an area that is full-time student. This is to account for concerns that the growth in cases may be associated with universities reopening (despite time time period under consideration being outside regular term time as most universities have not started until mid to late September the earliest). Lastly, column (6) further controls for a measure of the house tenure types as rented occupiers, especially in cities, typically may live in housing conditions that facilitate the spread of the disease. Throughout, despite adding a large set of additional control variables, the results remain unchanged.

In Appendix Table A3 we show that the results are robust to controlling for the potential non-linear growth in infections across a wide range of subnational geographies. We explore the NUTS1 - NUTS3 region classification which subdivides England into 9, 30 and 93 spatial units, respectively in columns (1)-(3). We also control for local authority district by week fixed effects allowing for 325 different district-level non-linear time trends in column (4). Column (5) explores parliamentary constituency by week fixed effects controlling for 533 different time trends. Lastly, column (6) explores the cross of the two controlling for 728 unique non-linear time trends. Throughout the results remain unchanged. Especially the latter three are substantially important from a policy perspective: local infection control and local measures are typically implemented at the local authority level, which represents the finest subnational political unit in the UK. Similarly, if there are concerns about some areas benefiting disproportionally e.g. due to political favoritism, the parliamentary constituency by time-effects absorb any such shocks. Since the two geographic units are not nested, the last column highlights that results are robust.

Lastly, Appendix Figure A4 performs a leave-on-one validation exercise dropping all MSOAs that are part of each of Englands 9, 30 and 93 NUTS1- NUTS3 regions. I plot both a kernel density estimate of the distribution of point estimates obtained as well as a box plot. Throughout the exercises, the distribution of point estimates points to the EOHO having a positive impact on disease spread.

We next turn to exploiting arguably exogenous variation in the intensity of restaurant visits due to inclement weather conditions around lunch- and dinnertime during days on which the discount was available.

4.2 Exploiting time-variation in restaurant visits

While the difference-in-difference results are very consistent in terms of timing when compared to data on restaurant visits, there remain some concerns about whether we can interpret results in a causal fashion. To tackle this concern, I leverage high frequency data measuring inclement weather around the typical lunch- and dinner times during which people most likely frequent restaurants. While the subsidy was available during the summer month August – the month was actually quite rainy in comparison to the long term average: in August, England saw rainfall levels at 159% of the usual long term average, ranging from a low 137% of the long term average in the East of the country to 174% in the Central region (Environment Agency, 2020).

I construct a measure capturing whether an area experienced notable rainfall during the prime lunch- and dinner hours across different days over the time window the subsidy was available. This allows me further to exploit intra-day variation in the amount of rainfall that falls outside of regular hours during which one would visit restaurants.

4.2.1 Rainfall on EOHO days and subsequent infections

Table 3 presents the main reduced form estimates linking intra-day and inter-day rainfall measures to subsequent COVID19 infections. Throughout this exercise, I estimate versions of specification 3 with different sets of rainfall measures. For ease of interpretation of the estimates, I discretize the rainfall measure to capture areas and time windows during which rainfall was in the upper decile.[8]

Panel A studies the impact of rainfall on subsequent COVID19 infection clusters emerging. Column (1) suggests that an area that saw notable rainfall during the lunch- and dinner hours measured on days on which the EOHO scheme was active in a week saw, on average, 0.029 percentage points lower COVID19 infections in the week – an affect of around 20% relative to the mean of the dependent during the calendar weeks 32 to 36 during which the scheme was active. Column

[8] Appendix Table A5 presents the same table for alternative rainfall measures, yielding qualitatively very similar results.

(2) studies the impact of notable rainfall during lunch- and dinner hours on days during which the EOHO scheme was available and during days within the same week on which the scheme was not available. Naturally, these rainfall measures are quite correlated. Nevertheless, the primary impact that is statistically precisely estimated is rainfall falling during days on which the EOHO scheme was available. Lastly, column (3) studies rainfall falling *on the same days* during which the EOHO scheme was available – but across different hours. We observe that only rainfall during the peak lunch- and dinner hours is associated with subsequently lower infection incidence – but not rainfall falling outside these hours on the same days.

In Panel B and Panel C I perform a set of placebo exercises. These highlight that the impact of rainfall on days during which the EOHO would have been available (Mondays-Wednesdays) in the four weeks prior- and the four weeks following the scheme is not statistically associated with differential COVID19 incidence. This suggests that rainfall may have had a notable negative impact on restaurant visits during days on which the discount was available relative to areas were the increased restaurant visits that the scheme encouraged were not impeded by poor weather.

It is worth highlighting that this points to the potential detrimental impact that the EOHO scheme had by concentrating restaurant visits *within the week* during a few week days, thereby making it more likely that restaurants had very high turnover on a few days which may have facilitated the spread of the disease due to possible challenges with complying with social distancing rules and other hygiene rules. This is not unlikely given the overall low enforcement of social distancing measures that have been observed elsewhere.[9]

We will document *very consistent patterns* when studying mobility data but flag up a few notable robustness to point to before.

[9] Low enforcement of social distancing rules at the council level is likely to have been substantially hampered by the fact that council budgets have been decreased in value by around 50% in real terms since 2010 due to austerity policies – see Fetzer, 2018, 2020.

4.2.2 Robustness

Timing Naturally, as the EOHO days were early in the week it is not unreasonable to expect to see some impacts on infections later within the same week. We can, however, also explore some alternative timing. In Appendix Table A4 I study whether there are any lead effects – a form of a placebo exercise – or whether there are any lagged effects of rainfall. There appear to be no lead effects, highlighting that the results are not spurious. The impact of rainfall early in the week on EOHO days Mondays-Wednesdays on infections recorded between 7 to 14 days later in Panel B is just marginally insignificant at conventional levels with a p-value of around 15%.

Rainfall measurement As indicated, for ease of interpretation I measure rainfall as a binary variable capturing whether rainfall is in the upper decile relative to its corresponding empirical distribution. In Appendix Table A5 I present alternative rainfall measures, specificially, measuring rainfall simply in overall levels – the results are very similar.

Alternative dependent variables Appendix Table A6 documents that results are robust to using alternative dependent variables to measure COVID19 infections.

4.2.3 Inclement weather and mobility patterns

Lastly, I want to study to what extent the documented link between rainfall and infections – specifically, rainfall on EOHO days and during periods when restaurants typically experience high demand – can also be detected in more general mobility data. This will serve as complementary evidence on the underlying mechanism.

The Google mobility data is only available at a substantially coarser district-level. It is worth flagging up that this is the level at which the time-fixed effects are specified in the previous main exercises, implying that all the patterns presented in what follows are in essence indirectly accounted for in the previous exercises.

Using daily mobility measures from Google (2020) I can directly study to what extent rainfall on days during which the EOHO scheme was active impacted the mobility score that includes visits to restaurants and cafes. Further, I can explore to what extent other types of mobility are impacted. These could be potential confounders or could capture alternative mechanisms through which rainfall or the EOHO scheme more generally could have an impact on mobility.

Results Table 4 presents results studying the impact of intra-day rainfall on the proxy measure for restaurant visits across different weekdays and between calendar weeks when the EOHO scheme was and was not available. Panel A focuses on the calendar weeks 32 to 36 when the EOHO discount was available on Mondays to Thursdays up to the 31st of August. Column (1) documents that rainfall during the core lunch- and dinner hours negatively impacts visits to restaurants. The estimate implies that the restaurant visit mobility score is around 18 percent lower on days in districts that saw notable rainfall during the lunch and dinner hours on the weekdays during which the EOHO discount was available. Column (2) performs the same exercise but focuses on the impact of rainfall on visits occurring later in the same week on the weekdays during which the discount was not available. We observe that rainfall during these days does not reduce restaurant visits. Lastly, column (3) documents the specific importance of rainfall falling during the core lunch and dinner hours in affecting restaurant visits on the Mondays to Wednesdays during which the discount was available.

Panels B and C study the same regressions, yet, focusing on the four calendar weeks immediately before and immediately after the EOHO scheme was active. We do not observe any robust correlation between inclement weather and restaurant visits across these time windows. This is worthy of some interpretation: the EOHO drastically increased restaurant visits – yet, it appears to have done less so in areas that saw poor whether on weekdays on which the discount was available. This observation, coupled with the observations studying infections in Table 3 suggests that *regular restaurant visiting activity* spread out across weekdays before- and after the scheme was active is not associated with infections. Rather, it appears to be the *excess visits* and the concentration of restaurant visits that the EOHO scheme has generated and which are very evident in the aggregate data in Figure 2, that drive the infection dynamics.

It is quite likely that excess crowding and a failing to respect social distancing measures may have contributed to the EOHO schemes impact on infections documented here.

Naturally, there may be some concerns about whether the rainfall during the core lunch and dinner hours have impacts on other mobility measures. That is, rainfall could have reduced infections not through its impacts on lowering visits to restaurants, but rather, due to lowering of other forms of mobility. I show that this appears to not be the case – this is best done visually in Figure 4. This figure represents the impact of notable rainfall on the Mondays to Wednesdays in calendar weeks 32 to 36 during which the EOHO discount was available on Google mobility scores by mobility type. Panel A presents the impact of rainfall on mobility for rainfall occurring during the core lunch- and dinner hours from 11:00 - 14:00 and 17:00 to 21:00. The regression coefficients have been rescaled to represent the mean impact relative to the mean of the dependent variable over the estimating sample. The figure documents that notable rainfall on EOHO days notably reduces the mobility score capturing restaurant visits by up to 20%. Similarly, we see notable declines of around 10% to visits or time spent in parks. We observe marginal increases in time spent at home, but null effects on mobility measures proxying grocery shopping, transit or at workplaces.

Panel B presents the impact of rainfall falling on the same days – but outside the core lunch and dinner hours. Throughout, we see null effects, suggesting that rainfall falling outside the lunch or dinner hours has no notable impacts on mobility during the day, on average.

In Appendix Figure A6 we perform the same exercise exploiting intra-day variation in rainfall during calendar weeks 32 to 36 – but study differential mobility patterns on the days of the week during which the EOHO discount was not available (Thursdays to Sundays). We observe marginally insignificant negative impacts of rainfall during lunch and dinner hours on mobility proxies capturing visits to restaurants – the marginal effects are substantially smaller compared to the days in the week during which the EOHO discount is available. This highlights that rainfall during core lunch and dinner hours on the days during which the EOHO scheme was available had a disproportionate negative impact on restaurant visits vis-a-vis areas that did not see notable rainfall.

Overall the results from the mobility analysis are very consistent in terms of overall patterns, results and significance compared to the patterns detected on infections in the previous section.

5 Conclusion

Policy makers are debating the optimal policy response to the COVID19 pandemic. The economic impact of changed consumer behavior in response to rising and falling COVID19 infections is far from uniformly distributed across sectors (Barrot et al., 2020; Brinca et al., 2020; Carvalho et al., 2020; Dingel and Neiman, 2020). The notion that there may be a trade-offs between health and the economy is broadly refused by most economic experts – disease containment is considered to be the best policy response to reduce both the human cost in terms of lives lost as well as to reduce the economic burden of the pandemic. Naturally, disease containment becomes more costly, if the pandemic is out of control. This suggests that early targeted interventions supported by an effective test and tracing system may be the most effective and least cost interventions (Acemoglu et al., 2020; Kaplan et al., 2020).

In the wake of Europe's first wave of the COVID19 pandemic many countries have mobilized significant fiscal resources to stimulate the economy out of their respective lockdown freezes (Bayer et al., 2020; Coibion et al., 2020a; Kaplan et al., 2020). The UK's policy response shared many similar elements compared to the fiscal measures used in other advanced economies. The most prominent point of divergence between the UK's fiscal response and that of other countries was a large scale demand-inducing measure aimed at the hospitality sector – specifically, restaurants and cafes. A total of GBP 500 million was spent to subsidize the cost of eating out in restaurant by up to 50% in the month of August. At the time, evidence of the likely spread of COVID19 in hospitality settings was already paramount. This paper documents that the Eat-Out-to-Help-out scheme, hailed as an economic cure for the ailing sector, may have substantially worsened the disease. The paper documents that the scheme had a substantial and causal impact leading to new spatially spread out COVID19 infections in the weeks during which the scheme was active. The estimates suggest that the scheme is responsible for around 8-17% of all infections during the summer months and likely, many more non-detected asymptomatic infections, that may have substantially contributed to accelerating the second wave of the pandemic.

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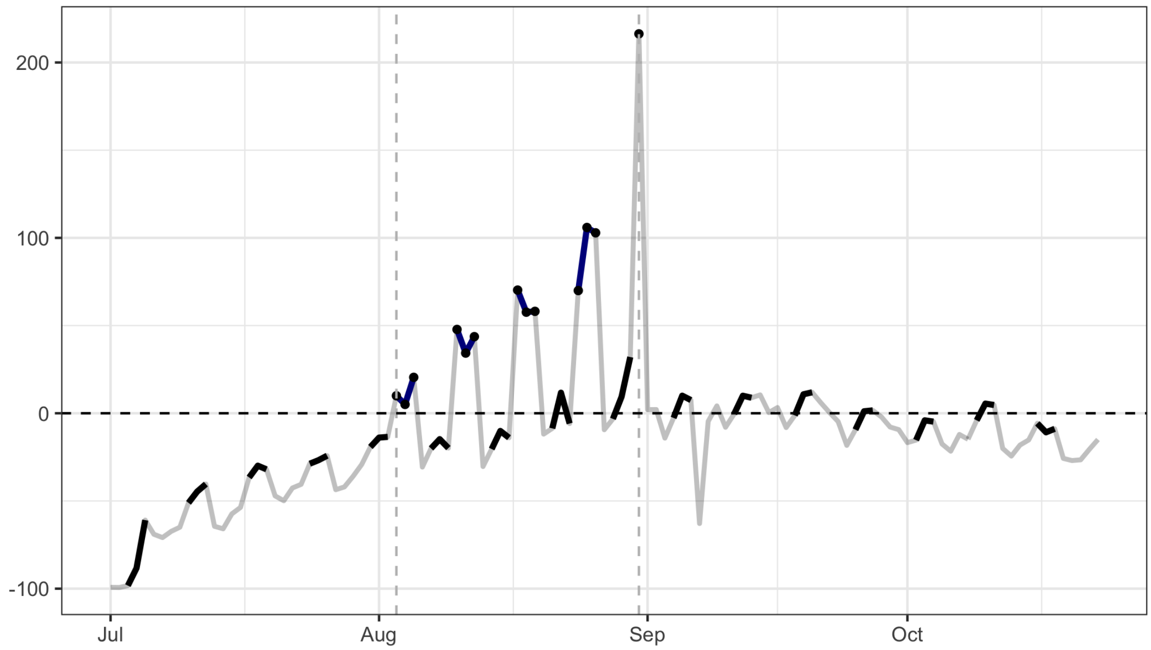
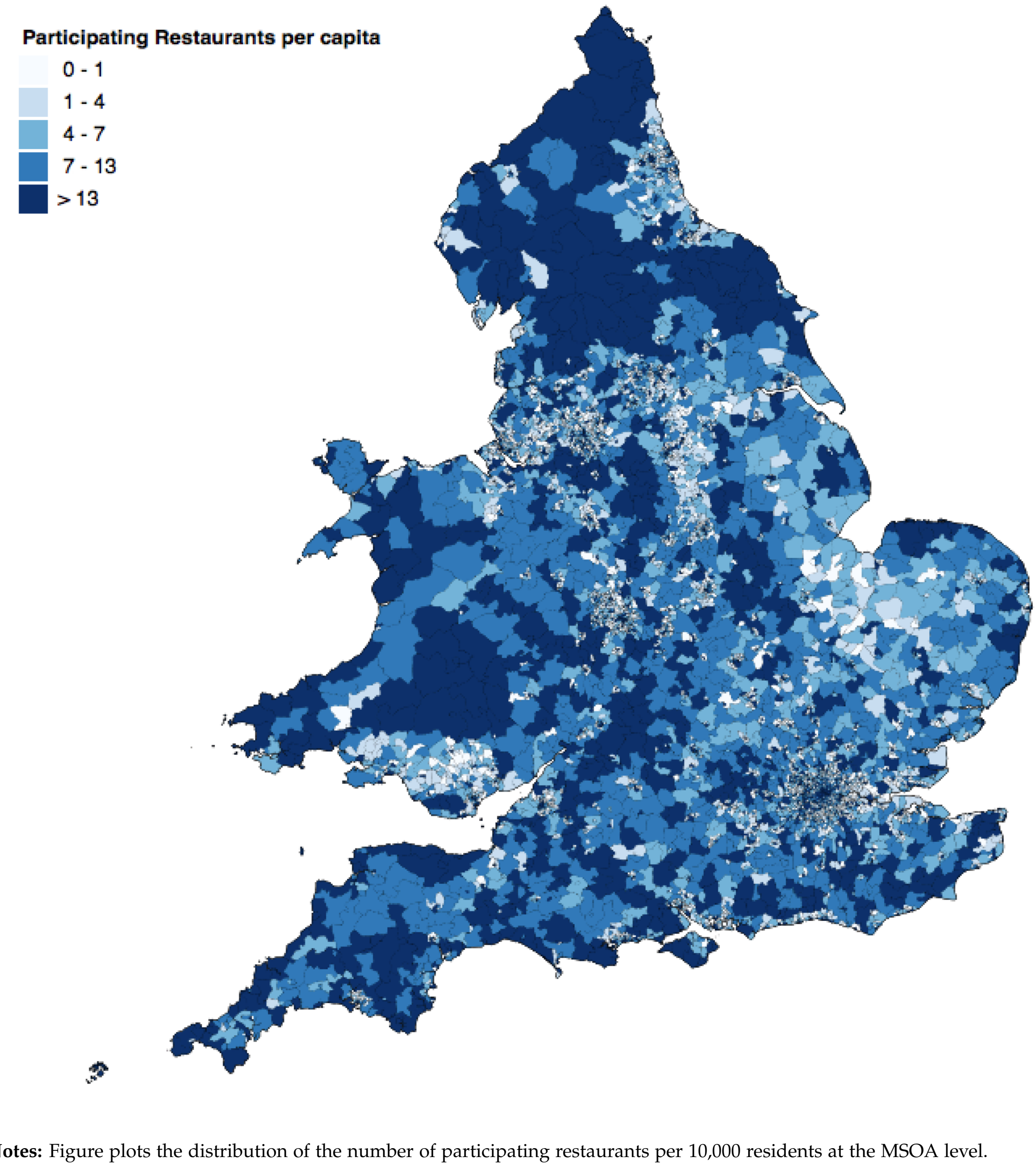
Figure 1: Year-on-Year evolution of restaurant visits as measured by the OpenTable state of the restaurant industry for the UK



Notes: Figure plots year-over-year proportional changes in seated diners at a sample of restaurants on the OpenTable network across all channels: online reservations, phone reservations, and walk-ins across the UK in %. The vertical lines indicate the start- and end-dates of the EOHO scheme. The individual dates when the EOHO subsidy was available across participating restaurants in the UK are marked. August 31 was the last, so-called Summer Bank Holiday which marks a public holiday. The date of these bank holidays are changing year-on-year with the corresponding holiday the previous year being August 26, 2019. This results in a notable inflation of the restaurant visits in the year-on-year comparison.

Figure 2: Participating Restaurants Per 10,000 Residents



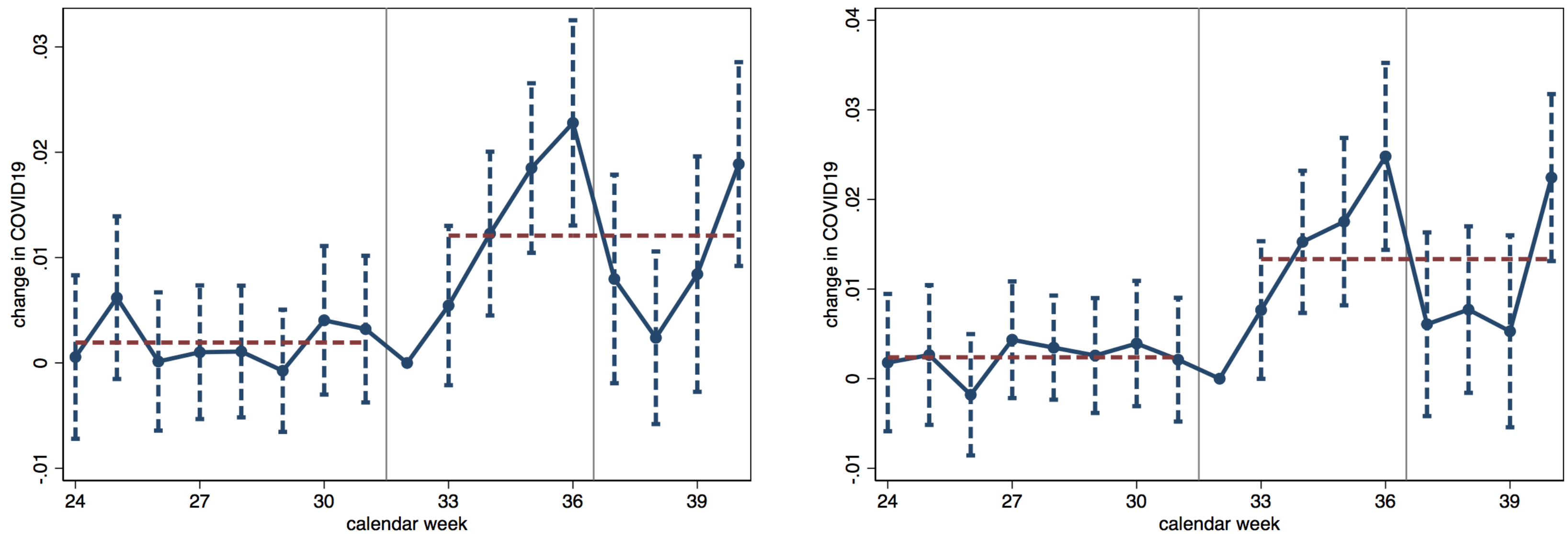
Notes: Figure plots the distribution of the number of participating restaurants per 10,000 residents at the MSOA level.

Figure 3: Difference-in-difference and parallel trends assumption: impact of EOHO scheme on new COVID19 clusters

Emergence of new COVID19 infection clusters across MSOAs associated with EOHO exposure measured as...

Panel A: Imputed Number of Meals per capita

Panel B: Number of participating restaurants

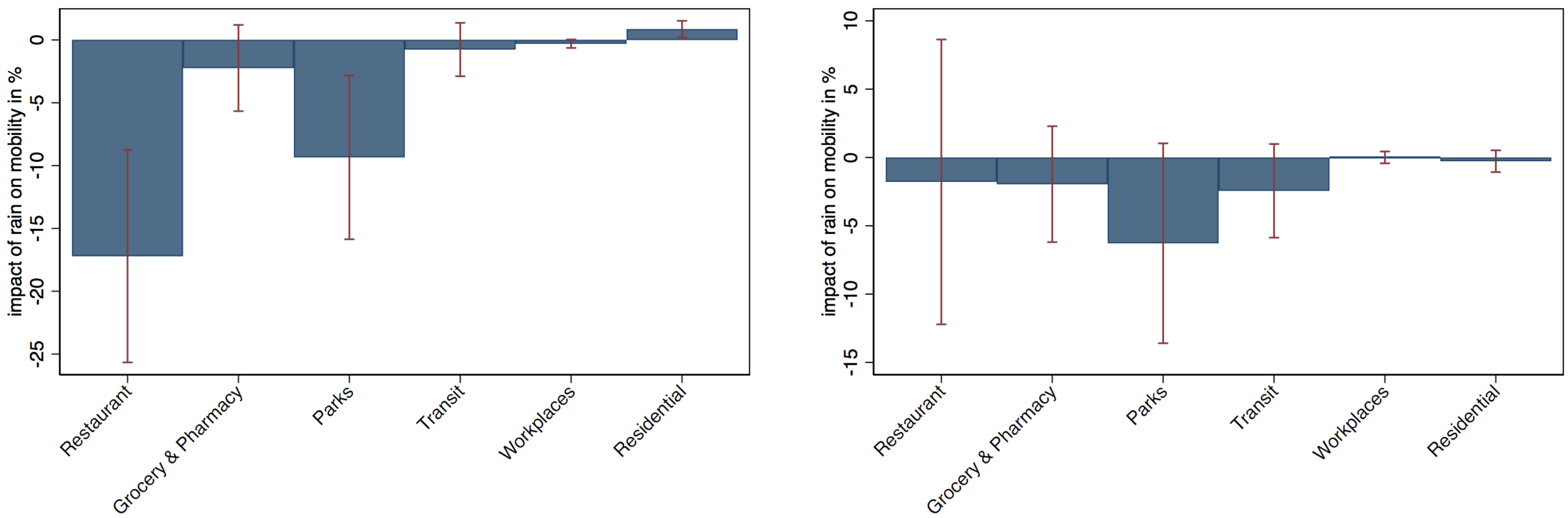


Notes: Figure presents regression estimates capturing the impact of EOHO exposure of an MSOA on the probability of a new COVID19 cluster being detected over time. The regressions control for MSOA fixed effects and local authority district by week fixed effects. Standard errors are clustered at the district level with 90% confidence intervals shown. The dependent variable is a dummy that is equal to 1 in case a new COVID19 infection cluster was detected. A cluster is defined as at least two newly detected infections. Week refers to the week in which the specimen for the COVID19 test was taken. New infection clusters increase sharply within a week of the introduction of the EOHO scheme and decline once again with the end of the scheme in MSOAs with more exposure to the scheme.

Figure 4: Impact of intra-day rainfall on EOHO days on daily Google mobility scores

Relative impact of notable rain on Google mobility scores category for rain falling during

Panel A: Core lunch & dinner hours *Panel B*: Outside lunch & dinner hours



Notes: Figure presents results from regression estimates presented in Appendix Table A7. The figure represents the impact of notable rainfall on days during which the EOHO discount was available in calendar weeks 32 to 36 on Google mobility scores by mobility type. Panel A presents the impact of rainfall on mobility for rainfall occurring during the core lunch- and dinner hours from 11:00 - 14:00 and 17:00 to 21:00. Panel B presents the impact of rainfall falling on the same day but outside the core lunch and dinner hours. The regressions control for district fixed effects and NUTS2 region by date fixed effects. 90% confidence intervals obtained from clustering standard errors at the district level are indicated.

Table 1: Impact of EOHO on Emergence of Local Infection Clusters

DV: Any new COVID19 cluster	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: EOHO exposure measured in log normalized by population						
Post × log(EOHO covered meals per capita)	0.008***	0.010***	0.010***			
	(0.003)	(0.003)	(0.003)			
Post × log(EOHO restaurants per capita)				0.008***	0.010***	0.011***
				(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Panel B: EOHO exposure measured in log +1						
Post × log(EOHO meals)	0.017***	0.016***	0.014***			
	(0.004)	(0.003)	(0.003)			
Post × log(EOHO restaurants)				0.014***	0.016***	0.017***
				(0.003)	(0.003)	(0.003)
Mean DV	0.100	0.100	0.100	0.099	0.099	0.099
Observations	79547	79547	79547	88283	88283	88283
MSOA	6119	6119	6119	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Panel C: EOHO exposure measured in levels						
Post × EOHO covered meals	0.018***	0.017***	0.015***			
	(0.004)	(0.003)	(0.004)			
Post × EOHO restaurants				0.012**	0.013**	0.013**
				(0.005)	(0.005)	(0.005)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Area by Week FE:	NUTS2	NUTS3	LAD	NUTS2	NUTS3	LAD

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters across the 13 calendar weeks from 24 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Robustness of Impact of EOHO on Emergence of Local Infection Clusters: Additional non-parametric control variables

DV: Any new COVID19 cluster	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Measuring EOHO by imputed meals per capita						
Post Week 32 $\times$ log(EOHO covered meals per capita)	0.010***	0.010***	0.008***	0.007**	0.006**	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88270	88270	88270	88270	88270
Clusters	326	325	325	325	325	325
Panel B: Measuring EOHO by number of restaurants per capita						
Post Week 32 $\times$ log(EOHO restaurants per capita)	0.011***	0.010***	0.010***	0.008***	0.006**	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88270	88270	88270	88270	88270
Clusters	326	325	325	325	325	325
Week x Additional control:						
Population density measures		X	X	X	X	X
Spring 2020 COVID19 exposure			X	X	X	X
Commuting exposure				X	X	X
Student exposure					X	X
Tenure types						X

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters across the 13 calendar weeks from 24 to 36. The dependent variable is 1 in case an MSOA reported more than two new detected infections per calendar week. The specifications across panels explore the robustness to adding additional successively more MSOA-level control variables interacted with week fixed effects to account for non-linear trends in these measures. Population density measures include: population density, the standard deviation of population density across lower-level super output areas (LSOAs) that make up the MSOA, and the area size of the MSOA in km2. Spring 2020 COVID measures an MSOA's exposure to COVID from March to July 2020 as the number of COVID19 deaths per capita ; the number of COVID19 cases per capita; the number of non-COVID19 deaths per capita and the share of COVID19 deaths among all deaths. Commuting exposure measures based on 2011 census the number of people usually commuting for work into an MSOA divided by the MOSA's population; the number of commuters usually resident but commuting elsewhere divided by the MSOA's population. Student exposure measures based on the 2011 census the share of full time students resident in an MSOA. Tenure types measures the share of households living in rented or owned accommodation. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Reduced Form Impact of Rainfall on EOHO days on emergence of local COVID19 infection clusters later in the week

DV: Any new COVID19 cluster	(1)	(2)	(3)
Panel A: Data window covering exactly the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-0.029**	-0.030***	-0.030***
	(0.011)	(0.011)	(0.011)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.024*	
		(0.014)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.023
			(0.020)
Mean DV	0.151	0.151	0.151
Observations	33955	33955	33955
Clusters	317	317	317
Panel B: Data window four week window prior to EOHO scheme (placebo)			
Significant Rainfall on EOHO days during lunch and dinner time	-0.018	-0.018	-0.018
	(0.020)	(0.020)	(0.020)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.030*	
		(0.016)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.002
			(0.014)
Mean DV	0.059	0.059	0.059
Observations	1268	1268	1268
Clusters	317	317	317
Panel C: Data window four weeks after the EOHO scheme (placebo)			
Significant Rainfall on EOHO days during lunch and dinner time	0.014	0.014	0.013
	(0.020)	(0.020)	(0.020)
Significant Rainfall on Non-EOHO days during lunch and dinner time		0.001	
		(0.023)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.036
			(0.026)
Mean DV	0.534	0.534	0.534
Observations	27164	27164	27164
Clusters	317	317	317

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters during calendar weeks 32 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table 4: Reduced Form Impact of Rainfall on EOHO days on Google mobility scores proxying visits to restaurants and cafe's across districts over time

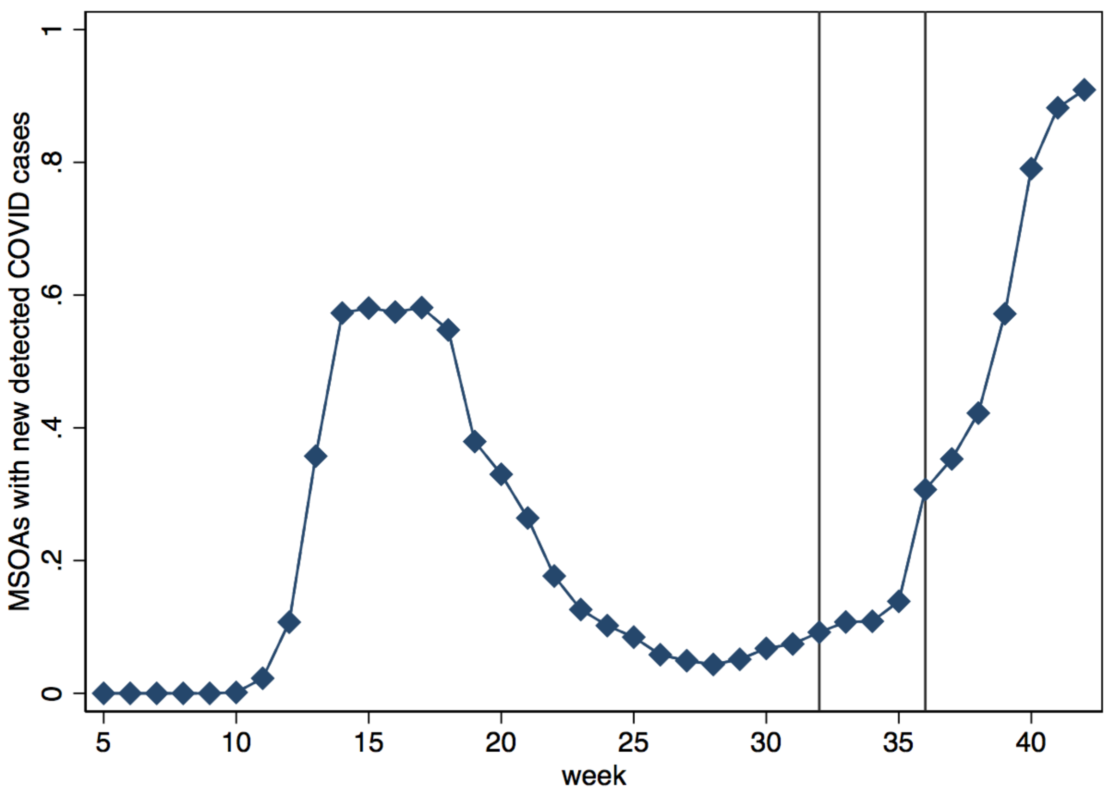
DV: Google mobility visits to restaurants	(1)	(2)	(3)
Panel A: Data window covering exactly the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-1.459***		-1.419***
	(0.441)		(0.422)
Significant Rainfall on non-EOHO days during lunch and dinner time		-0.743	
		(0.464)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.148
			(0.521)
Mean DV	-8.244	-21.968	-8.244
Observations	2401	4233	2401
Clusters	311	312	311
Panel B: Data window four week window prior to EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	0.021		-0.167
	(1.112)		(1.117)
Significant Rainfall on non-EOHO days during lunch and dinner time		1.273	
		(1.252)	
Significant Rainfall on EOHO days outside lunch and dinner hours			1.903**
			(0.966)
Mean DV	-21.407	-28.633	-21.407
Observations	3732	4979	3732
Clusters	311	312	311
Panel C: Data window four weeks after the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-0.155		-0.154
	(0.606)		(0.605)
Significant Rainfall on non-EOHO days during lunch and dinner time		-1.397*	
		(0.712)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.185
			(0.865)
Mean DV	-13.965	-18.172	-13.965
Observations	2918	4478	2918
Clusters	311	312	311

Notes: Table presents regression estimates studying the impact of inclement weather on Google mobility proxies capturing visits to Restaurants and Cafes within local authority districts over time. Column (1) and (3) exploit intra-day variation in rainfall falling during core lunch and dinner hours and outside these hours to study its impact on mobility to restaurants on Mondays to Wednesdays during which the EOHO scheme would have been available during calendar weeks 32 to 36. Column (2) explores the impact of rainfall falling during core lunch and dinner hours on restaurant visits occurring from Thursdays to Sundays – days during which the EOHO discount would not have been available. Panel A focuses on the calendar weeks 32 to 36 when the EOHO was available, while Panel B and Panel C can be thought of as placebo exercises studying the rainfall and mobility relationships during times when the EOHO scheme was not available. All regressions control for district fixed effects and NUTS2 area by date fixed effects. Standard errors are clustered at the district level with starts indicating *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Appendix to "Subsidizing the spread of COVID19: Evidence from the UK"

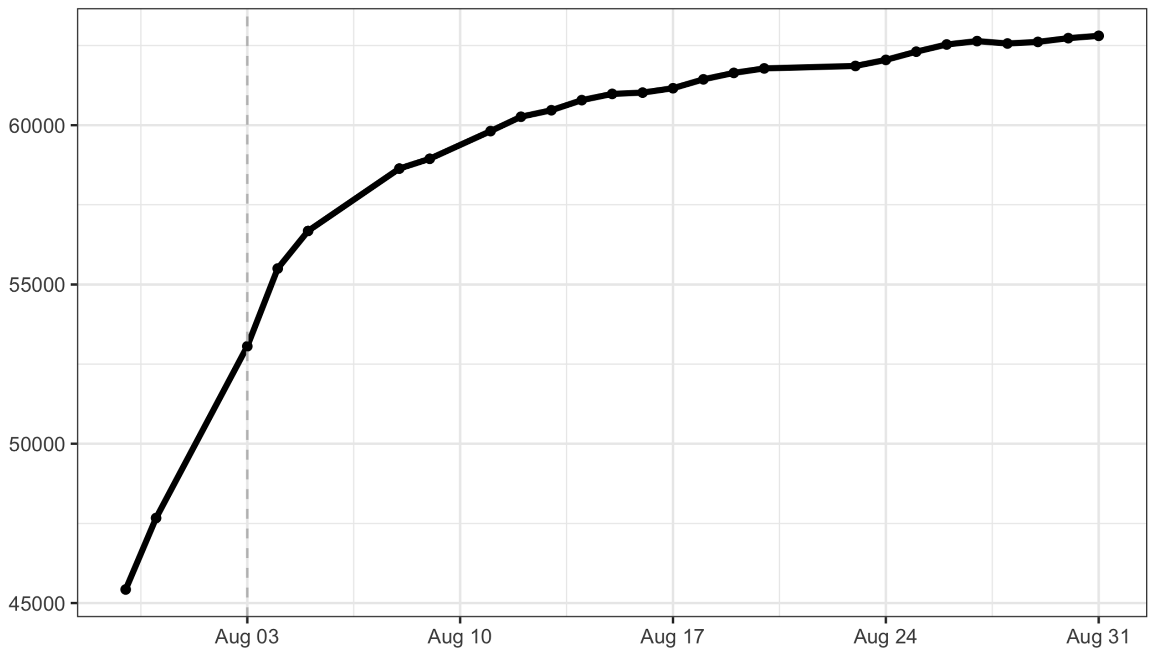
For Online Publication

Figure A1: COVID19 spread across MSOA's in England



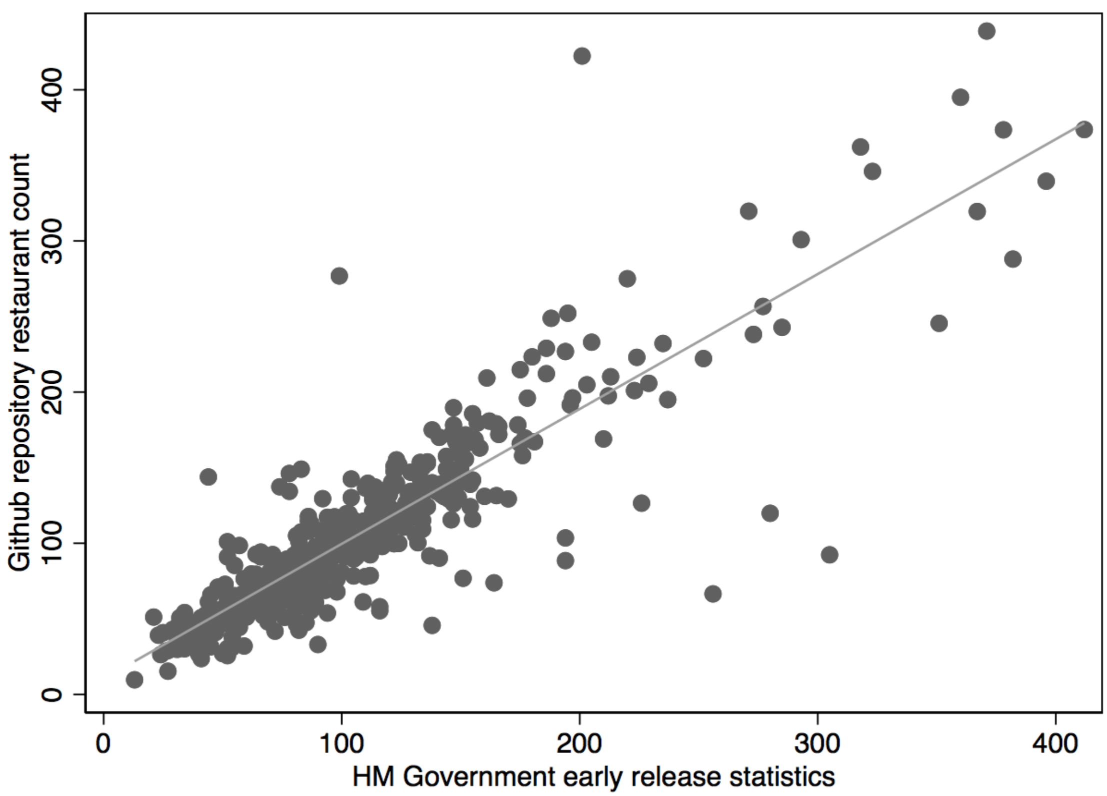
Notes: Figure plots the share of English MSOA's that report at least three new cases of COVID19 per calendar week. The vertical lines indicate the time that the Eat Out to Help Out scheme was open.

Figure A2: Number of restaurants registered to participate in the Eat Out to Help Out scheme across England



Notes: Time series plots the number of restaurants that are registered in the scheme at different points in time in England. The program started on Aug 3, 2020 and lasted until Aug 31, 2020. Dots indicate points where a flat file with the restaurants was downloadable from the HMRC Github repository track changes. The data in between is interpolated.

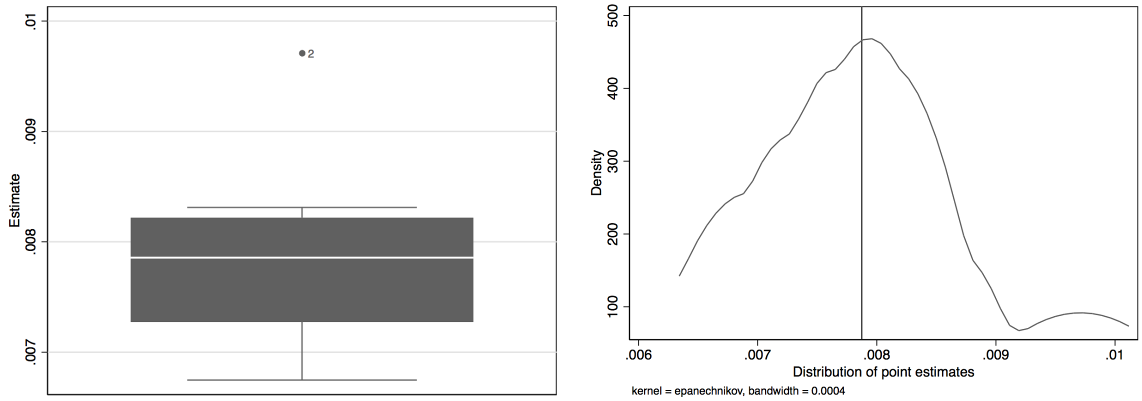
Figure A3: Correlation between number of participating restaurant as measured from two sources



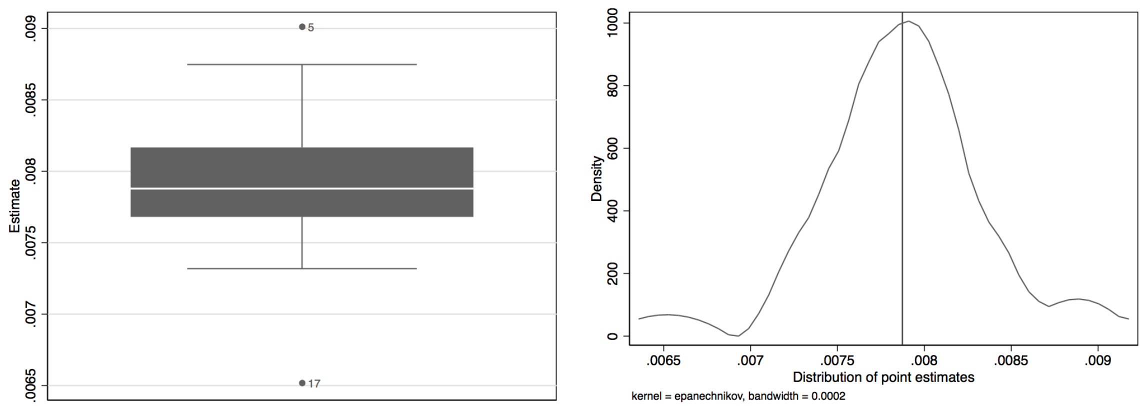
Notes: Time series plots the number of restaurants that are registered in the scheme at different points in time in England. The program started on Aug 3, 2020 and lasted until Aug 31, 2020. Dots indicate points where a flat file with the restaurants was downloadable from the HMRC Github repository track changes. The data in between is interpolated.

Figure A4: Distribution of point estimates obtained when dropping one region a time

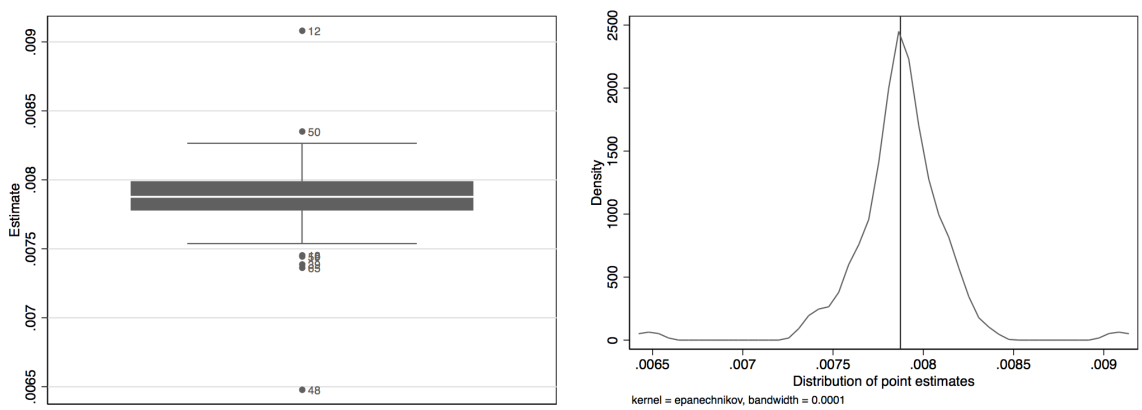
Panel A: Dropping each of the 9 NUTS1 regions in turn



Panel B: Dropping each of the 30 NUTS2 regions in turn

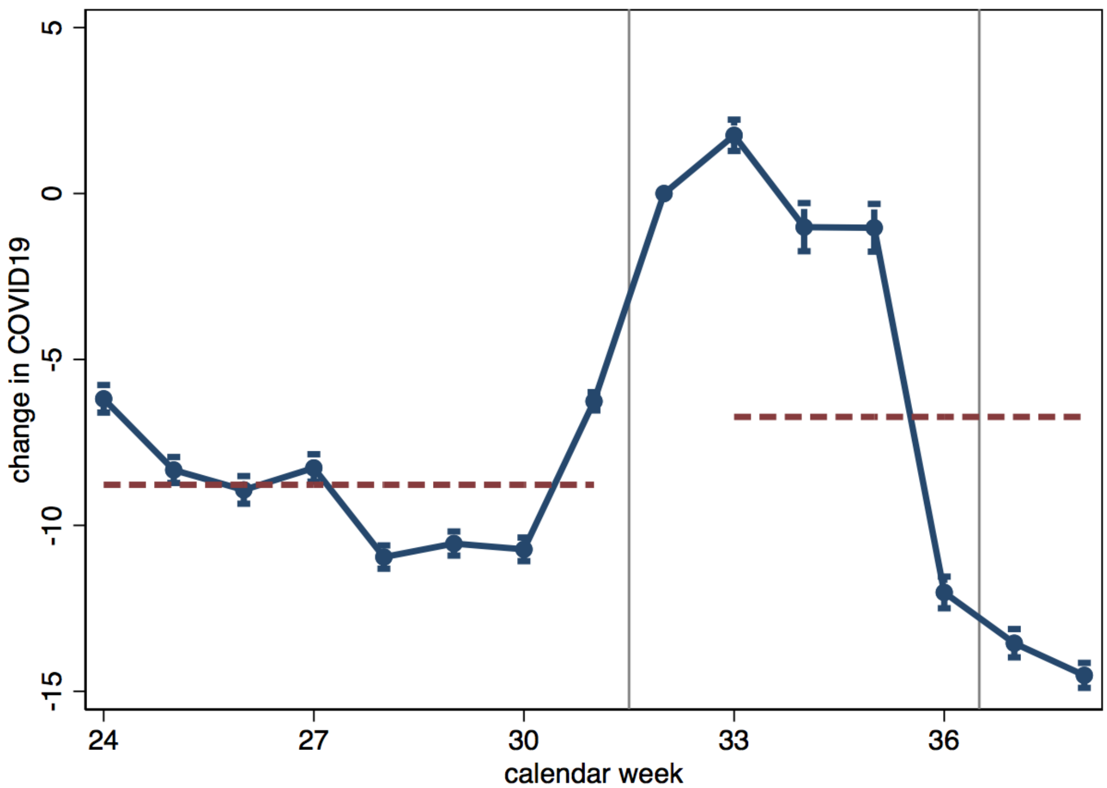


Panel C: Dropping each of the 93 NUTS3 regions in turn



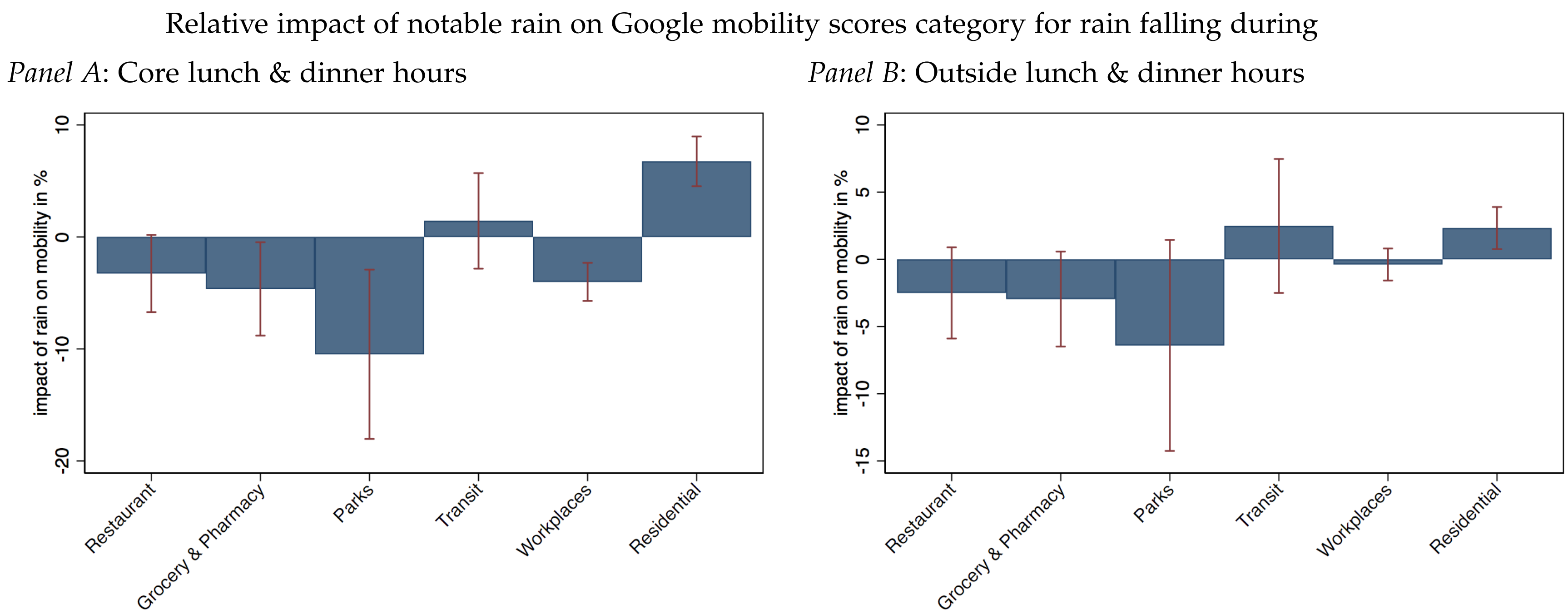
Notes: Figures present the distribution of the point estimates obtained when dropping one region a time. The estimating regression has as dependent variable an indicator that is equal to 1 in case a new COVID19 cluster of more than two cases was detected in an MSOA. The regressions include MSOA fixed effects and district by time fixed effects. The coefficient estimate is the interaction between the post indicator marking the start of the EOHO scheme and the log of the number of number of restaurants +1 divided by the MSOA population. Standard errors are clustered at the district level.

Figure A5: Impact of the EOHO scheme on Google Mobility for Retail and Recreation (which includes restaurant visits)



Notes: Figure plots the change in Google mobility measure on days during which the EOHO scheme was active (Monday, Tuesday, Wednesday) before and after the scheme was introduced from calendar week 32 inclusive onwards up until Monday 31, August inclusive in calendar week 36. The regression controls for district fixed effects, district-specific linear trends by calendar week and day of week fixed effects. 90% standard errors obtained from clustering standard errors at the district level are indicated.

Figure A6: Placebo exercise studying impact of intra-day rainfall during calendar weeks 32 to 36 *outside the EOHO days* on daily Google mobility scores



Notes: Figure presents results from regression estimates. The figure represents the impact of notable rainfall during weeks 32 to 36 when the EOHO was available – *but on the weekdays when the discount was not offered (Thursday-Sunday)* - on Google mobility scores by mobility type. Panel A presents the impact of rainfall on mobility for rainfall occurring during the core lunch- and dinner hours from 11:00 - 14:00 and 17:00 to 21:00. Panel B presents the impact of rainfall falling on the same day but outside the core lunch and dinner hours. The regressions control for district fixed effects and NUTS2 region by date fixed effects. 90% confidence intervals obtained from clustering standard errors at the district level are indicated.

Table A1: EOHO and Google Mobility

DV: Google mobility in	Retail & Recreation	Grocery	Parks	Transit	Workplace	Residential
	(1)	(2)	(3)	(4)	(5)	(6)
Post Week 32 $\times$ EOHO Weekday	6.780***	-0.615***	6.489***	-1.295***	-2.625***	0.059**
	(0.253)	(0.100)	(1.178)	(0.258)	(0.077)	(0.023)
Mean DV	-31.568	-10.997	69.819	-31.403	-38.000	11.908
Observations	24061	24597	17023	24224	26925	26331
Clusters	312	311	300	311	312	311

Notes: Table presents difference-in-difference regression estimates studying the evoltution of Google mobility measures at the district level over time between calendar weeks 24 and 36. The EOHO scheme was active on Mondays, Tuesdays and Wednesdays from August 3, 2020 to August 31, 2020 (calendar weeks 32 to 36). The dependent variable is the mobility measure relative to pre COVID19 levels per day across the categories provided by Google indicated in the column head. The regressions control for district FE, week fixed effects, and weekday fixed effects. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Robustness of Impact of EOHO on Emergence of Local Infection Clusters: Alternative functional forms

DV: indicated in panel label	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable: Any COVID19 cluster						
Post × log(EOHO covered meals per capita)	0.008***	0.010***	0.010***			
	(0.003)	(0.003)	(0.003)			
Post × log(EOHO restaurants per capita)				0.008***	0.010***	0.011***
				(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Panel B: Dependent variable: log(of COVID19 cases in cluster)						
Post × log(EOHO covered meals per capita)	0.014***	0.016***	0.016***			
	(0.005)	(0.005)	(0.006)			
Post × log(EOHO restaurants per capita)				0.011**	0.014***	0.016***
				(0.005)	(0.005)	(0.005)
Mean DV	0.172	0.172	0.172	0.172	0.172	0.172
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Panel C: Dependent variable: Inverse hyperbolic sine (asinh) of of COVID19 cases in cluster						
Post × log(EOHO covered meals per capita)	0.018***	0.021***	0.021***			
	(0.007)	(0.007)	(0.007)			
Post × log(EOHO restaurants per capita)				0.015**	0.018***	0.021***
				(0.006)	(0.007)	(0.007)
Mean DV	0.222	0.222	0.222	0.222	0.222	0.222
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	390	1209	4121	390	1209	4121
Clusters	326	326	326	326	326	326
Area by Week FE:	NUTS2	NUTS3	LAD	NUTS2	NUTS3	LAD

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters across the 13 calendar weeks from 24 to 36. The dependent variable is 1 in case an MSOA reported more than two new detected infections per calendar week. The independent variable in panel A measures the EOHO scheme as the log number of meals served in restaurants in an MSOA that participate in the EOHO scheme plus 1 divided by the population in the area. The independent variable in panel B measures the EOHO scheme as the log number of restaurants that participate in the EOHO scheme in an MSOA plus 1 divided by the population in the area. The specifications across panels explore the robustness to controlling for more granular non-linear time fixed effects. NUTS refers to the nomenclature unitÃ©s territoriales statistiques which subdivides the England into 11, 30 and 93 regions. LAD refers too local authority districts. PCON refers to Westminster parliamentary constituencies. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Robustness of Impact of EOHO on Emergence of Local Infection Clusters: Alternative fixed effects

DV: Any new COVID19 cluster	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Measuring EOHO by imputed meals per capita						
Post Week 32 $\times$ log(EOHO covered meals per capita)	0.008***	0.008***	0.010***	0.010***	0.008***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	117	390	1209	4121	6929	9373
Clusters	326	326	326	326	326	326
Panel B: Measuring EOHO by number of restaurants per capita						
Post Week 32 $\times$ log(EOHO restaurants per capita)	0.006**	0.008***	0.010***	0.011***	0.009***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mean DV	0.099	0.099	0.099	0.099	0.099	0.099
Observations	88283	88283	88283	88283	88283	88283
MSOA	6791	6791	6791	6791	6791	6791
Additional controls	117	390	1209	4121	6929	9373
Clusters	326	326	326	326	326	326
Area by Week FE:	NUTS1	NUTS2	NUTS3	LAD	PCON	PCON x LAD

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters across the 13 calendar weeks from 24 to 36. The dependent variable is 1 in case an MSOA reported more than two new detected infections per calendar week. The independent variable in panel A measures the EOHO scheme as the log number of meals served in restaurants in an MSOA that participate in the EOHO scheme plus 1 divided by the population in the area. The independent variable in panel B measures the EOHO scheme as the log number of restaurants that participate in the EOHO scheme in an MSOA plus 1 divided by the population in the area. The specifications across panels explore the robustness to controlling for more granular non-linear time fixed effects. NUTS refers to the nomenclature unitÃ©s territoriales statistiques which subdivides the England into 11, 30 and 93 regions. LAD refers too local authority districts. PCON refers to Westminster parliamentary constituencies. Standard errors are clustered at the district level with starts indicating *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A4: Reduced Form Impact of Rainfall on EOHO days on emergence of local COVID19 infection clusters later in the week

DV: Any new COVID19 cluster	(1)	(2)	(3)
Panel A: Rainfall and new COVID19 cases later in same week			
Significant Rainfall on EOHO days during lunch and dinner time	-0.023**	-0.023**	-0.023***
	(0.009)	(0.009)	(0.009)
Significant Rainfall on Non-EOHO days during lunch and dinner time		0.001	
		(0.010)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.002
			(0.014)
Mean DV	0.151	0.151	0.151
Observations	33955	33955	33955
Clusters	317	317	317
Panel B: Rainfall and new COVID19 cases later in next week			
Significant Rainfall on EOHO days during lunch and dinner time	-0.015	-0.015	-0.014
	(0.010)	(0.010)	(0.010)
Significant Rainfall on Non-EOHO days during lunch and dinner time		0.005	
		(0.011)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.009
			(0.014)
Mean DV	0.153	0.153	0.153
Observations	33955	33955	33955
Clusters	317	317	317
Panel C: Rainfall and new COVID19 cases later in previous week (placebo)			
Significant Rainfall on EOHO days during lunch and dinner time	-0.009	-0.009	-0.008
	(0.009)	(0.009)	(0.009)
Significant Rainfall on Non-EOHO days during lunch and dinner time		0.006	
		(0.010)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.019
			(0.014)
Mean DV	0.149	0.149	0.149
Observations	33955	33955	33955
Clusters	317	317	317

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters during calendar weeks 32 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Reduced Form Impact of Rainfall on EOHO days on emergence of local COVID19 infection clusters later in the week: alternative rainfall measures

	(1)	(2)	(3)
Panel A: Any significant rainfall			
Significant Rainfall on EOHO days during lunch and dinner time	-0.029**	-0.030***	-0.030***
	(0.011)	(0.011)	(0.011)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.024*	
		(0.014)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.023
			(0.020)
Mean DV	0.151	0.151	0.151
Observations	33955	33955	33955
Clusters	317	317	317
Panel B: Rainfall in levels			
Rainfall on EOHO days during lunch and dinner time	-0.005**	-0.005**	-0.005**
	(0.002)	(0.002)	(0.002)
Rainfall on Non-EOHO days during lunch and dinner time		0.001	
		(0.002)	
Rainfall on EOHO days outside lunch and dinner hours			-0.001
			(0.002)
Mean DV	0.151	0.151	0.151
Observations	33955	33955	33955
Clusters	317	317	317

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters during calendar weeks 32 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Reduced Form Impact of Rainfall on EOHO days on emergence of local COVID19 infection clusters later in the week: alternative dependent variables

	(1)	(2)	(3)
Panel A: Data window covering exactly the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-0.029**	-0.030***	-0.030***
	(0.011)	(0.011)	(0.011)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.024*	
		(0.014)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.023
			(0.020)
Mean DV	0.151	0.151	0.151
Observations	33955	33955	33955
Clusters	317	317	317
Panel B: Data window covering exactly the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-0.053***	-0.053***	-0.054***
	(0.020)	(0.020)	(0.020)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.029	
		(0.023)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.027
			(0.037)
Mean DV	0.262	0.262	0.262
Observations	33955	33955	33955
Clusters	317	317	317
Panel C: Data window covering exactly the EOHO scheme			
Significant Rainfall on EOHO days during lunch and dinner time	-0.156**	-0.157**	-0.157**
	(0.075)	(0.076)	(0.073)
Significant Rainfall on Non-EOHO days during lunch and dinner time		-0.071	
		(0.073)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.036
			(0.169)
Mean DV	0.804	0.804	0.804
Observations	33955	33955	33955
Clusters	317	317	317

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters during calendar weeks 32 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A7: Reduced Form Impact of Rainfall on EOHO days on Google mobility across districts over time

	(1)	(2)	(3)
Panel A: Recreation and Retail			
Significant Rainfall on EOHO days during lunch and dinner time	-1.937***		-1.952***
	(0.356)		(0.364)
Significant Rainfall on non-EOHO days during lunch and dinner time		-0.807**	
		(0.355)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.039
			(0.349)
Mean DV	-8.244	-21.968	-8.244
Observations	2401	4233	2401
Clusters	311	312	311
Panel B: Grocery			
Significant Rainfall on EOHO days during lunch and dinner time	-0.777***		-0.581**
	(0.234)		(0.228)
Significant Rainfall on non-EOHO days during lunch and dinner time		-0.769***	
		(0.257)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.542**
			(0.235)
Mean DV	-12.288	-12.136	-12.288
Observations	2893	4336	2893
Clusters	311	311	311
Panel C: Parks			
Significant Rainfall on EOHO days during lunch and dinner time	-22.904***		-19.904***
	(3.569)		(3.376)
Significant Rainfall on non-EOHO days during lunch and dinner time		-9.048***	
		(3.003)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-5.920
			(4.059)
Mean DV	92.791	75.969	92.791
Observations	937	2179	937
Clusters	239	295	239
Panel D: Transit			
Significant Rainfall on EOHO days during lunch and dinner time	-1.255***		-0.937**
	(0.469)		(0.473)
Significant Rainfall on non-EOHO days during lunch and dinner time		0.052	
		(0.505)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.850
			(0.548)
Mean DV	-35.748	-27.783	-35.748
Observations	2934	3947	2934
Clusters	311	311	311
Panel D: Workplace			
Significant Rainfall on EOHO days during lunch and dinner time	-0.189**		-0.131
	(0.086)		(0.087)
Significant Rainfall on non-EOHO days during lunch and dinner time		-1.279***	
		(0.271)	
Significant Rainfall on EOHO days outside lunch and dinner hours			-0.172*
			(0.104)
Mean DV	-46.681	-32.225	-46.681
Observations	4624	4837	4624
Clusters	312	312	312
Panel D: Residential			
Significant Rainfall on EOHO days during lunch and dinner time	0.309***		0.258***
	(0.042)		(0.043)
Significant Rainfall on non-EOHO days during lunch and dinner time		0.700***	
		(0.080)	
Significant Rainfall on EOHO days outside lunch and dinner hours			0.153***
			(0.047)
Mean DV	11.417	7.504	11.417
Observations	4647	5533	4647
Clusters	311	311	311

Notes: Table presents difference-in-difference regression estimates studying the impact of the EOHO at the MSOA level on the emergence of new COVID19 infection clusters during calendar weeks 32 to 36. All regressions also control for local authority by week fixed effects. Standard errors are clustered at the district level with starts indicating *** p< 0.01, ** p< 0.05, * p< 0.1.