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Mobility restrictions and alcohol use during lockdown: "A still and dry pandemic for the many"?

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ABSTRACT

Unexpected mobility disruptions during lockdown during the first wave of COVID-19 became 'tipping points' with the potential to alter pre-pandemic routines sensitive to socialisation. This paper investigates the impact of lockdown exposure on alcohol consumption. We document two findings using information from the Google Mobility Report and longitudinal data from the Understanding Society survey (UKHLS) in the United Kingdom. First, we find a sharp reduction in both actual mobility and alcohol use (consistent with a "still and dry pandemic for the many" hypothesis). However, we document an increase in alcohol use among heavy drinkers, implying a split behavioural response to COVID-19 mobility restrictions based on alcohol use prior to the pandemic. Second, using the predictions of the prevalence-response elasticity theory, we find that the pandemic's reduction in social contacts is responsible for a 2.8 percentage point reduction in drinking among men.

1. Introduction

Socialisation and mobility restrictions can exert important consequences on health-related behaviour. However, this is a question that has received limited attention in the literature so far. The COVID-19 pandemic offers an important quasi-natural experiment to test this question, as it encompassed unexpected restrictions to individuals social behaviour. Indeed, a lockdown and a number of mobility restrictions were implemented in the United Kingdom (UK) from 23 March 2020 to 10 May 2020, and people were only allowed to leave their homes for necessities, such as shopping and exercise, thereby reducing opportunities for social engagement leading to alchol use. However, since off-trade premises were still open, it is an empirical question whether individuals who spent more time at home, and were potentially more exposed to a stressful environment, were more inclined to consume alcohol. This essay examines the evidence for this claim.

Previous studies document mixed evidence of an effect of the COVID-19 pandemic on alcohol consumption. According to Jackson et al. (2021) and Stevely et al. (2021) some share of the population stopped drinking completely during the lockdown, even though heavy drinkers were more likely to report an increasing intake of alcohol. Although Kilian et al. (2021) report evidence of an average decrease in alcohol consumption after the COVID-19 pandemic, driven by a reduced frequency of heavy episodic drinking events, White et al. (2022) compared alcohol-related and all-cause deaths among all people 16 years of age and older in 2019 and 2020 in the US and found an increase in alcohol-related deaths by approximately 25% between 2019 and 2020. Hence, the evidence so far is not conclusive.

One way to reconcile such findings is to examine whether the apparent differences result from the different empirical approaches and samples analysed, as some specific mechanisms might dominate over others. Therefore, there is still room for improvement in our understanding of the underlying causes of behavioural change with regards to alcohol consumption during the COVID-19 pandemic, More specifically, this is an important endeavour to identify what policy responses are more appropiate should the need for similar interventions arise again.

We contribute to the literature by documenting causal evidence resulting from a novel difference-in-differences (DD) startegy that complement previous the existing pre-post analysis. Furthemore, our empirical approach suggests a number of relevant mechanisms underlying the overall effects.

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We exploit two sources of evidence. First, we examine data from the first COVID-19 wave of Understanding Society (the UK Household Longitudinal Study) alongside complementary mobility data from Google COVID-19 Community Mobility Reports (GCMR).Second, to define treated and control groups, we combine this information with regional statistics on fresh COVID-19 cases. More precisely, we define our *treated* regions as those exhibiting above the median new daily COVID-19 cases before the announcement, and the introduction of mobility restrictions. Our estimates suggest that men living in treated regions reduced their extensive margin of alcohol use by 2.48 percentage points compared to the control group.

Next, using mobility data, we show that in treated regions, individuals exhibited less mobility to workplaces, retail and recreation and public transportation, and higher mobility to residences and parks than the controls. No significant differences are found when examining other potential channels, such as changes in income. We also report stratified estimates, suggesting limited evidence of changes in risk perceptions, namely fear of the health consequences of COVID-19 among those subgroups of the population who are likely to be more vulnerable due to their fragile health conditions.

Our estimates are robust to a series of checks. These include the following. First, we exclude observations from March 9, 2020, to March 16, 2020, to consider possible anticipatory effects based on the first lockdown in Italy. Second, since DD estimates rely on a regional level data resulting from different level of aggregation, e.g., regional or NUTS1 data, rather than provincial or NUTS3 level data, we have ran our analysis using data at the same level of aggregation, to check whether our baseline estimates were consistent. Furthermore, we have expanded Eq. (3) with NUTS3-specific linear trends as well as a common quadratic component to test that no other NUTS3-specific time-varying factors explain the variations in mobility. Finally, we tested how sensitive our findings are to various thresholds used to define the treatment variable.

We interpret our results as resulting from changes in socialisation – that we capture through changes in mobility behaviour – rather than other competing mechanisms such as income.

The paper is structured as follows. The next section describes the institutional background, the conceptual framework and identification. Section 3 presents the data used in the analysis; Section 4 illustrates the empirical strategy. Section 5 reports the results, and a final section concludes.

2. Background and identification

The literature on the effect of the pandemic restrictions on alcohol consumption in the UK has focused mainly on the pre-post comparison, providing mixed results to date (see Jackson et al., 2021 and Stevely et al., 2021 Roberts et al., 2021 Pollard et al., 2020 White et al., 2022). However, a pre-post comparison of drinking patterns can potentially give rise to misleading conclusions insofar as there may be other observable and unobservable confounders driving such effect. Indeed, individuals may react to an actual or expected reduction in income, by reducing consumption, especially for those goods that are not consumed frequently and that the consumer may feel are unnecessary, e.g. alcohol consumption among occasional drinkers. Accordingly, adopting a pre-post comparison, would fail to identify the effect of an actual (or expected) income loss, and distinguish it from that of mobility restrictions. Another explanation of the heterogenity in study estimates lies in the differences in empirical approaches, alongside samples analysed. This is especially the case when some channels dominate over other competing explanations, such as changes in income, expectations and/or preferences. Hence, to shed light on the mechanisms behind the actual effect of mobility restrictions, we exploit pre-determined conditions at the regional level to define treatment and control, so as to identify the effect in a DD model.

Our identification strategy relies on the predictions of the *prevalence-response elasticity* theory (Fenichel, 2013), which supports the idea that the spread of an outbreak depends on pre-existing conditions or characteristics alongside the endogenous response of other individuals and authorities.[2] That is, although authorities in the UK put forward nationwide mobility restrictions, it is plausible to assume that individuals might have reacted differently depending on their local conditions. In this paper, we test the implications of the prevalence-response elasticity theory using mobility data. That is, we hypothesize that individuals living in regions where we observe an above the median number of new COVID-19 cases before the introduction of restrictions, were more likely to stay at home, hence reducing their social activities, including alcohol use. That is, if individuals perceive to be exposed to a higher risk from the pandemic because they live in an area where COVID-19 is more prevalent, we expect them to react by a stricter compliance with pandemic restrictions compared to those individuals living in regions relatively less exposed to COVID-19.

Our identification strategy relies on the following assumptions. First, we focus on the first wave of the pandemic, as mobility restrictions were often unanticipated 'one size fits all' restrictions that might give rise to heterogeneous effects depending on pre-determined local conditions. Second, both individuals in treated and control groups can be distinguished based on their exposure to daily COVID-19 cases on the 16th of March when the UK Prime Minister stated that unnecessary travel and social contact should be avoided. Assignment into treatment is pre-determined, and hence independent of the restrictions' announcement (the 23rd of March). That is, it is independent of the current level of new daily cases in the region. However, our cut-off date refers to the date of the announcement of mobility restrictions rather than its actual implementation. This is the case insofar as some individuals might have already anticipated such restrictions, and hence adjusted their behaviours accordingly. Heckman and Smith (1995) provides a discussion on similar issues in other contexts, illustrating that only the announcement can be regarded as exogenous to individuals' behaviours.

Fig. 1 reports the timeline of the main events and announcements that can have influenced individuals' mobility and social interactions during the first wave of COVID-19 in the UK. The World Health Organization (WHO) declared the COVID-19 pandemic in March 2020, and four Chief Medical Officers (CMO) in each of the UK countries raised the country risk level from low to moderate. On the 10-th of March 2020, the UK saw the first deaths due to COVID-19, and the number of cases rose higher than 300. Two days later, i.e. the 12-th of March, the UK Prime Minister (PM) stated that "now is the time for everyone to stop non-essential contact and travel", and more than a week later, on the 23-rd of March, the first lockdown was announced. However, the actual implementation took place a couple of days later, on the 26-th of March.

Finally, it is worth mentioning that given our DD set-up, we rely on the standard parallel trend assumption to estimate the parameters of interest. We report drawing on an event-study framework, suggestive evidence suggesting that individuals in treated and control regions exhibited similar behaviour before the announcement of mobility restrictions in terms of mobility and alcohol use. It is worth mentioning that excise duty is charged on each of these categories at a fixed rate – a number of pence per litre. The rate of duty is set in relation to alcoholic strength, and the strength is measured as alcohol by volume (ABV) – the percentage of an alcohol product's volume comprised of pure alcohol.

[2] The prevalence-response elasticity has been examined in many contexts (Oster, 2012; Mullahy, 1999; Ahituv et al., 1996; Philipson, 1996) and was recently analysed using COVID-19 data from Lombardy, which is one of the Italian regions mostly affected by the pandemic (Battiston and Gamba, 2020).

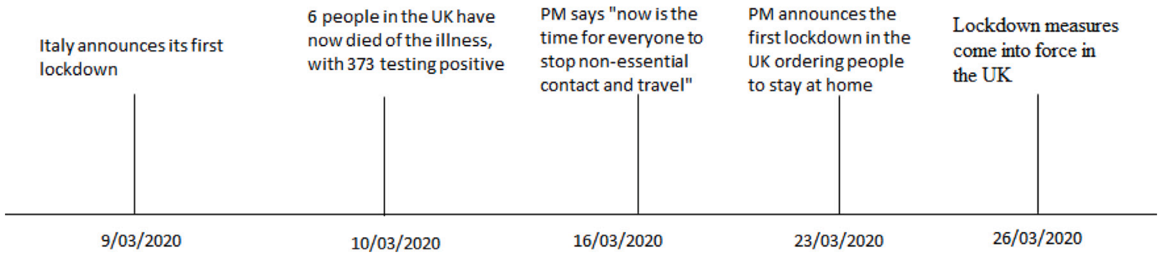

Fig. 1. Timeline of UK government containment measures during March 2020.
Notes: This Figure shows the timeline of the main events and announcements that can have influenced individuals' mobility and social interactions during the first wave of COVID-19 in the UK.

3. Data

We use data from various sources. First, we exploit information from Understanding Society, a longitudinal multidisciplinary survey administered by the Institute for Social and Economic Research (ISER) at the University of Essex (University of Essex & Economic Research, 2020b). The survey, in its regular version, collects information on several aspects of people's lives in the UK, e.g. health, behaviours, sociodemographic characteristics and economic aspects. In April 2020, Understanding Society respondents were invited to participate in a short web survey asking how the pandemic affected their lives. A telephone interview was offered to respondents willing to participate but living in a household where no one was a regular internet user (University of Essex & Economic Research, 2020a). The special COVID-19 survey was repeated each month until July 2020; from September 2020 to September 2021, fieldwork was planned every two months.

Compared to the standard questionnaire, the COVID web survey was shorter and composed of two sets of questions: a core set to track changes in socio-demographic characteristics and economic conditions and a rotating content changing over time. We are especially interested in health behaviours data gathered through the first COVID-19 wave of April 2020. Such data allow us to study how the pandemic affected lifestyles. One of the advantages of this survey is that we can link it to the regular Understanding Society sample. In this paper, we taken advantage of the latter and merged the special COVID-19 survey and previous regular surveys from 2015 to 2019 to provide evidence about the common pre-trend assumption.

Our second source of data refers to mobility indicator retrieved from the freely-available Google COVID-19 Community Mobility Reports (GCMR) dataset provided by Google LLC (2020). The data collects information about mobility changes at the regional level for the following types of visits: (i) workplaces, (ii) own residences, (iii) grocery stores & pharmacies, (iv) retail & recreation, (v) parks, and (vi) public transportation. Mobility indicators are expressed as changes with respect to a baseline value, which is the median value, for the corresponding day of the week, during the 5 weeks Jan 3–Feb 6, 2020. Changes are computed using the same aggregated and anonymised data to identify popular places in Google Maps. Mobility indicators are calculated based on data from users who have opted-in to Location History for their Google Account, representing a sub-sample of Google users that might be selected.[3] Mobility data are collected daily for the regions listed in the Appendix. We aggregate the information at the NUTS3 level,[4] by computing regional daily averages for each mobility indicator, and also at the NUTS1 level to perform some robustness checks.

Finally, we use information from the Office for National Statistics (ONS), which provides the number of total and new COVID-19 cases at the NUTS 3 level in the United Kingdom and deaths at the local authority district level. We aggregated death records at the NUTS3 (regional) level, which allows us to identify weekly deaths attributable to COVID-19 as a share of all deaths within a specific region. According to the ONS, a specific death is attributed to COVID-19 if it takes place 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. The classification of COVID-19 deaths is important but highly contentious, as the WHO and other international institutions have noted, particularly when comparing pandemic statistics between nations. Since COVID-19-related information is reported uniformly across all UK regions, this is less of an issue in our case. More specifically, the COVID-19 death rate is computed as the ratio between COVID-19 deaths and total weekly deaths.

4. Empirical strategy

Our empirical strategy is designed to complement existing evidence on the effect of the COVID-19 pandemic on alcohol consumption. To do so, we exploit different regional pre-determined conditions to define treated and control groups using a DD model.

4.1. Effects on alcohol use

To estimate the effect of the mobility restrictions during DOVID-19 on alcohol consumption, we specify the following model:

$$Drink^k_{i,t} = \delta + \nu post_t \times treated_r + \mu X_{i,t} + \iota_i + \tau_t + \rho_{r,w} \quad (1)$$

where $Drink^k_{i,t}$, with $k = 1, 2$, depicts the extensive margin of alcohol use (1 if respondent drinks and 0 otherwise) and, we consider alcohol use intensity as a binary variable too (1 if respondent drinks more than 4 times per week and 0 otherwise). $treated_r$ regions are NUTS1[5] areas with above the median new daily COVID-19 cases , measured before the announcement of the restrictions . ν measures the differential effect of living in treated regions on drinking behaviours after the announcement. In Eq. (1), ι_i and τ_t refer to individual and time-specific fixed effects respectively. $post_t$ indicates whether the information was collected from the Understanding Society COVID-19 survey, i.e. after 2020, or in a regular round. $X_{i,t}$ is a vector of individual level covariates. For a detailed description of the covariates used in our model, see Table A.1 in the appendix.

[3] The selection of the sample poses an issue in our framework only if it changes differently for treated and control regions after the mobility restriction, which is unlikely to occur.

[4] The Nomenclature of territorial units for statistics, abbreviated NUTS (from the French version Nomenclature des Unités territoriales statistiques) is a geographical nomenclature subdividing the economic territory of the European Union (EU) into regions at three different levels (NUTS 1, 2 and 3 respectively, moving from larger to smaller territorial units). NUTS 1 corresponds to macro-regions, NUTS 2 to regions and NUTS1 to provinces. Above NUTS 1, there is the 'national' level of the Member States. The NUTS is based on Regulation (EC) No 1059/2003 of the European Parliament and of the Council of 26 May 2003 on the establishment of a common classification of territorial units for statistics (NUTS), which is regularly updated.

[5] The NUTS1 level, e.g., the finest territorial level in the Understanding Society survey.

Our DD approach relies on the assumption that individuals in treated and control regions would have had the same trend in health behaviours in the absence of the treatment. This assumption is untestable because we cannot observe counterfactuals for each individual. However, we can investigate whether alchol use before the introduction of mobility restrictions for both treatment and control groups, and provide suggestive evidence supporting the idea that they are indeed comparable. To this end, we use an event study approach as follows:

$$Drink^k_{i,t} = \gamma + \sum_{j=2}^{J} \eta_j (Lag_j)_{i,t} + \sum_{k=1}^{K} \mu_k (Lead_k)_{i,t} + \lambda_i + \psi_t + \xi_{i,t} \tag{2}$$

Lags and Leads are defined as in Clarke and Schythe (2020) and can be interpreted as post-treatment and anticipatory effects, respectively. λ_i and ψ_t represent individual and year-fixed effects. If leads coefficients are not significantly different from zero, this is evidence in favour of the previously discussed parallel trend hypothesis. Based on the available data from the UK Household Longitudinal Study (UKHLS) also known as "Understanding Society", we can estimate two leads (i.e. 2017 and 2015 compared to 2019) and one lag (2020).

4.2. Effects on mobility

Analogously, to analyse mobility behaviour, we estimate the following equation:

$$m^k_{r,d} = \gamma + \eta post_d \times treated_r + \lambda_r + \psi_w + \xi_{r,d} \tag{3}$$

$m^k_{r,d}$ is one of the $k = 1, \dots, 6$ mobility indicators collected from GMRR,e.g., mobility to workplaces, own residences, grocery stores and pharmacies, retail and recreation, parks and public transportation. We include day (ω_d) and province (NUTS3) (δ_r) fixed effects to account for unobservable differences in NUTS3 areas alongside the pandemic diffusion during the first wave. $post_d$ denotes observations collected after the UK's first announcement of mobility restrictions, on the 16th of March, e.g., the day the UK Prime Minister stated that unnecessary travel and social contacts should be avoided. The coefficient of interest here is η, identifying the differential post-announcement effect on actual mobility between treated and control regions.

Our estimates are based on the parallel trend assumption between treated and controls. To provide evidence in this regard, we estimate the following:

$$m^k_{r,d} = \gamma + \sum_{j=2}^{J} \eta_j (Lag_j)_{r,d} + \sum_{k=1}^{K} \mu_k (Lead_k)_{r,d} + \lambda_r + \psi_d + \xi_{r,d} \tag{4}$$

Lags and Leads are defined as in Clarke and Schythe (2020), in terms of days from and to the lockdown announcement date. λ_r and ψ_d represent NUTS3 and day fixed effects.

5. Results

5.1. Preliminary evidence

Fig. 4 shows the time variation of the mobility indicators described above. In particular, we display estimates of changes in mobility with respect to the baseline (pre-COVID-19) value.[6] In each graph, we report two dashed lines. The first one corresponds to the 16th of March, e.g., the day on which the UK Prime Minister stated that unnecessary travel and social contact should be avoided. The second one to the 23-rd of March, e.g., the date when the prime minister announced the first lockdown[7] in the UK. We decided to use the former to define the treatment because it is much closer to the tipping point for almost all mobility patterns across the UK. Notice that even the 16th of March does not correspond exactly to the observed decline in mobility. In other words, the evidence indicates that individuals seemed to have anticipated the UK prime minister's decisions. This may be due to the simultaneous announcement of mobility restrictions in nearby nations like Italy and Spain, which implemented lockdown measures on March 9 and March 15, 2020, and were heavily publicised in the UK media. Individuals in the UK may have thus partially reacted to such measures. For this reason, as a robustness check, we re-run our DD models on mobility variations excluding observations between the Italian lockdown and the UK's first announcement.

Turning to examine workplace mobility, Fig. 4, panel a, displays evidence of a significant variation in workplace mobility at the time of the COVID-19 restrictions announcement date. Indeed, we find that workplace mobility declined by 60%. Other mobility indicators also point to behavioural changes after the UK-COVID-19 restrictions announcement date. Fig. 4, panel b, shows a significant increase in mobility to own residences after the announcement, with an average increase of more than 20% with respect to the pre-announcement period. Regarding mobility to grocery stores and pharmacies and to retail and recreation 4, panels c and d, we can notice that the announcement of restrictions has generated a sharp drop comparable to workplace mobility.

Next, we document an upward trend in mobility to grocery stores right before the lockdown announcement (23rd of March) in the UK, which might be compatible with the stockpiling phenomenon documented in the news. Indeed, we document a drop in mobility is close to 40%. Mobility to retail and recreation places shows a decrease even larger than that observed for workplaces. In fact, in this case, the recorded variation is 80% with respect to the pre-announcement period. At the bottom of Fig. 4, we show graphically the estimated variations in mobility patterns related to public transportation (right-hand side) and parks (left-hand side). Again, we document a significant drop on March 16th for the former and a less precise variation for the latter. It must be emphasised, though, that visiting parks was still permitted as long as people respected the social distance.

Next, we graphically document a correlation between mobility reductions and COVID-19 cases and deaths. Fig. 2 shows the average variation in mobility to specific destinations, estimated during the entire analysis period, by NUTS3 regions. The darker the colour in the map, the more negative the variation in mobility. For instance, the top-left map shows that London and neighbouring areas exhibit the highest mobility reductions to workplaces. The top-centre map shows that, in such areas, we find the highest positive increase in mobility to residential destinations. Similar conclusions can be reached by looking at other mobility indicators, except for mobility parks, which do not exhibit a similar pattern. This is reasonable as visiting a park does not necessarily entail a risk to an individual's health. Fig. 3 depicts the geographical variability in the average value of COVID-19 total cases, new cases and death ratio between COVID-19 deaths and total deaths by NUTS3 areas. The darker the colour, the higher the value for the number of totals, new cases and the death ratio. These two Figures reveal a correlation between mobility and COVID-19 cases and deaths. In particular, we find that the higher the death ratio or the presence of total and new cases, the higher the contraction in mobility to workplaces.

In Table A.2, we display the descriptive statistics for our regional variables on mobility and COVID-19 cases and deaths before and after the COVID-19 restrictions announcement of the 16th of March for treated and control regions. COVID-19 total and new daily cases increased after the announcement.[8] The ratio between COVID-19 and

[6] The baseline value is defined as the median value, for the corresponding day of the week, during the 5 weeks Jan 3–Feb 6, 2020

[7] Lockdown measures came into force the 26-th of March in the UK. However, we decided to focus on the dates relative to the most relevant announcements because we believe that most people reacted to such announcements rather than the official introduction of mobility restrictions.

[8] More specifically, the former increased from 0.34 to 145.51 and from 2.09 to 432.35 in *control* and *treated* regions, respectively, whereas the latter increased from 0.05 to 5.22 and from 0.44 to 12.9 in *control* and *treated* regions, respectively.

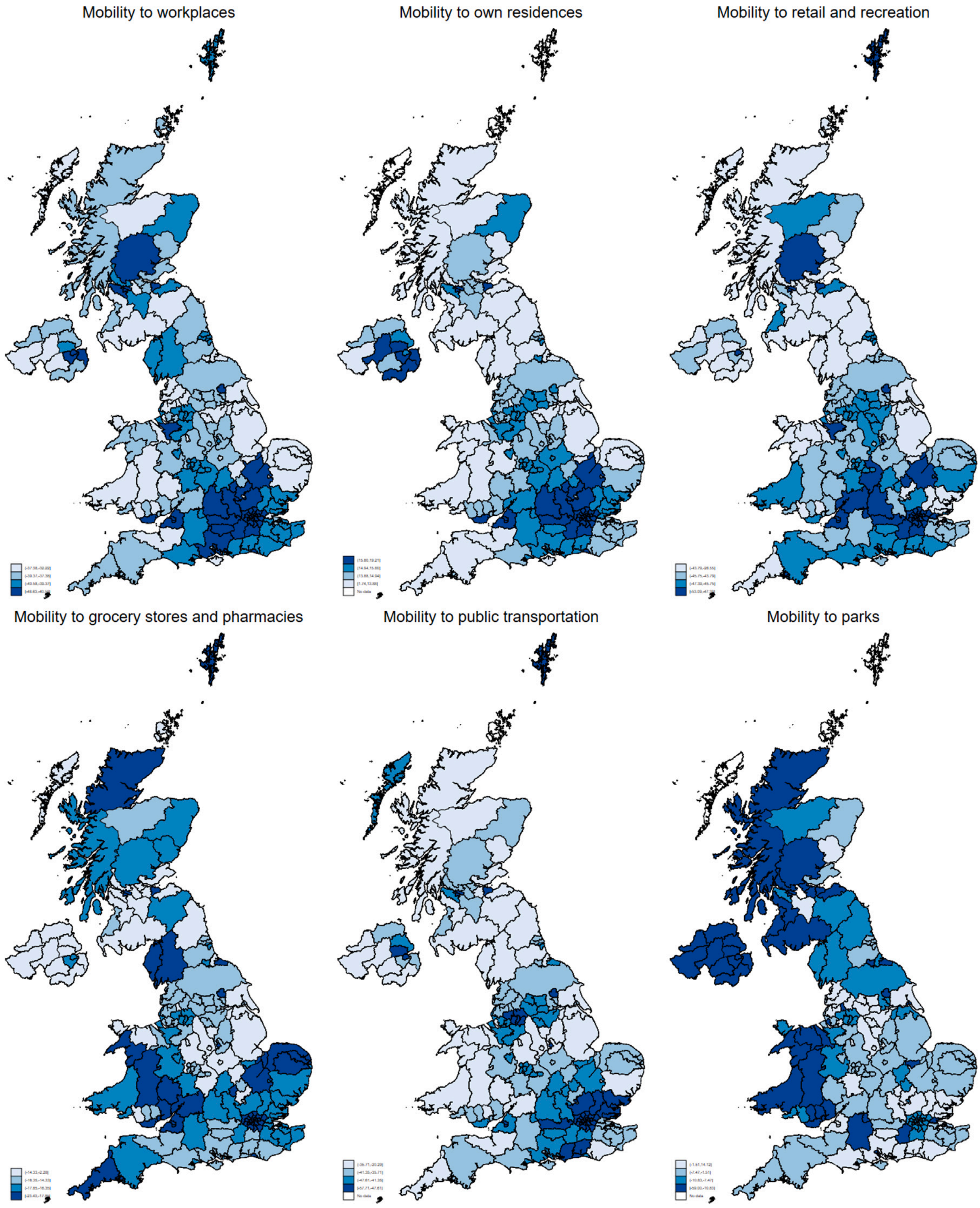

Fig. 2. Territorial distribution of changes in mobility. Average values for the period 15/2 - 19/5 of 2020.
Notes: This Figure shows the average variation in mobility to specific destinations, estimated during the entire analysis period, by NUTS3 regions. The darker the colour in the map, the more negative the variation in mobility. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

total deaths increased from 0 to 0.19 and from 0 to 0.25 in *control* and *treated* regions, respectively. In *treated* regions, mobility to workplaces decreased by 54.97 (−57.89+2.92) percentage points, whereas in *control* regions, it decreased by 51.98 (−54.08+2.1) percentage points. The average pre-post decrease in *treated* regions is larger by almost 3 percentage points than in *control* regions. The same is true also for mobility to grocery stores and pharmacies (27.11–26.36 = 0.75 percentage points), retail and recreation (69.13–67.42 = 1.71 percentage points) and public transportation (61.38–53.98 = 7.4 percentage points). Instead, according to mobility to own residences and parks, the average pre-post increase in *treated* regions is larger by almost 1.88 (21.56–19.68) and 11.22 (−9.36 + 20.58) percentage points than in *control* regions.

Next, we report in Fig. 5 graphical evidence of the trends in total and new daily cases and the ratio between COVID-19 and total deaths for treated and control regions separately. Fig. 5 displays evidence that treated regions, after week 9 (e.g., two weeks before the COVID-19 restrictions announcement) reveal a positive number of COVID-19

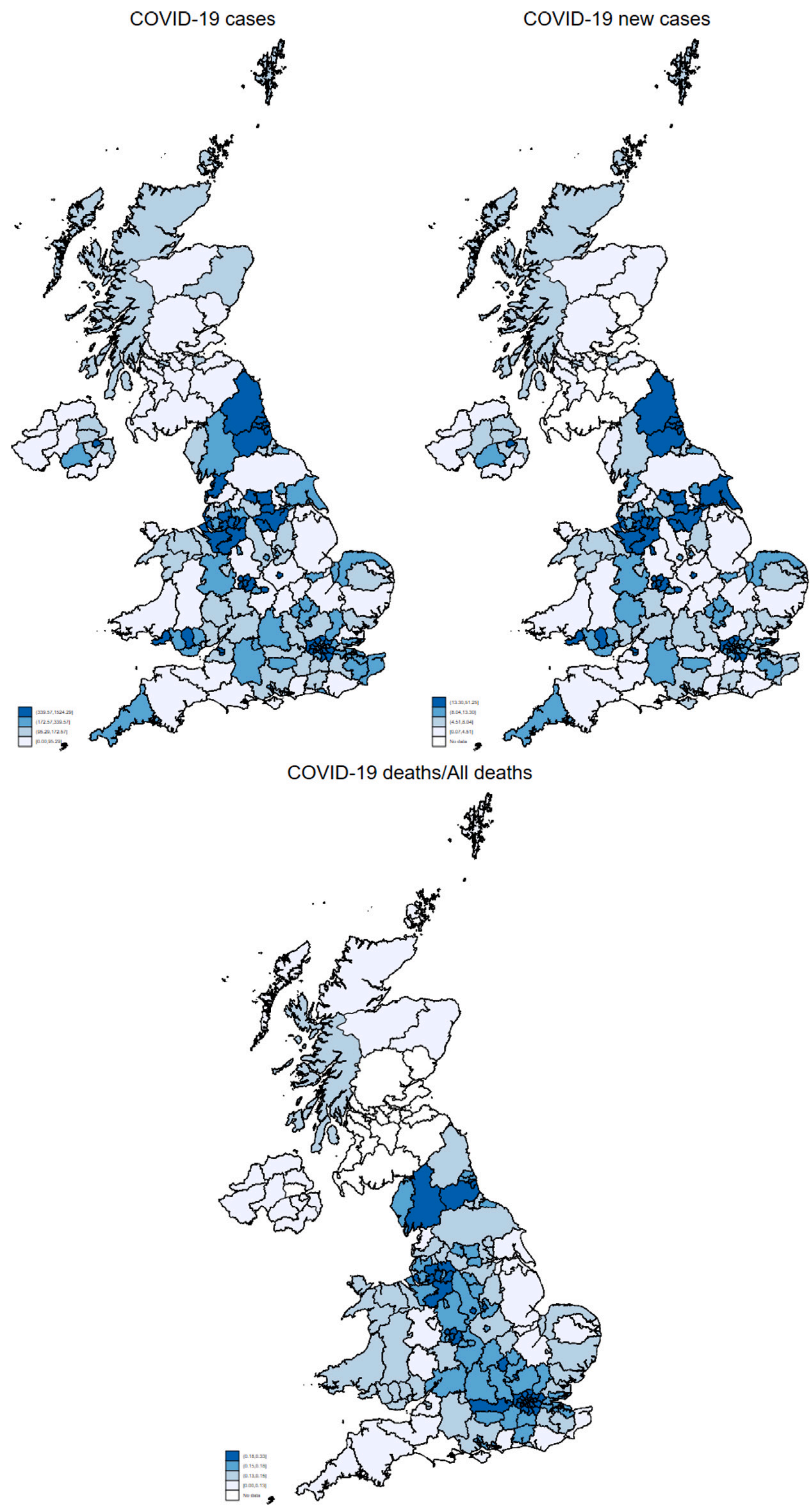

Fig. 3. Geographical variability for the ratio between COVID-19 deaths and deaths for other causes.
Notes: This Figure shows geographical dispersion for the average value of the ratio between COVID-19 deaths and deaths for other causes during the period 15/2 - 19/5 of 2020. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

total and new daily cases, whereas control regions exhibit negligible totals and new daily cases. Right after the announcement of COVID-19 restrictions, control regions started to reveal a positive number of total and new daily COVID-19 cases, but always lower than treated regions. Finally, looking at the ratio between COVID-19 deaths and all deaths from week 11 (i.e., the week corresponding to the COVID-19 restrictions announcement) treated regions exhibit a positive number of COVID-19-related deaths. Such graphical trends provide the empirical basis for the definition of our treatment and control groups. That is, regions exposed earlier to the COVID-19 pandemic, are also those more likely to display COVID-19 restrictions. The latter will be extensively tested in the following sections.

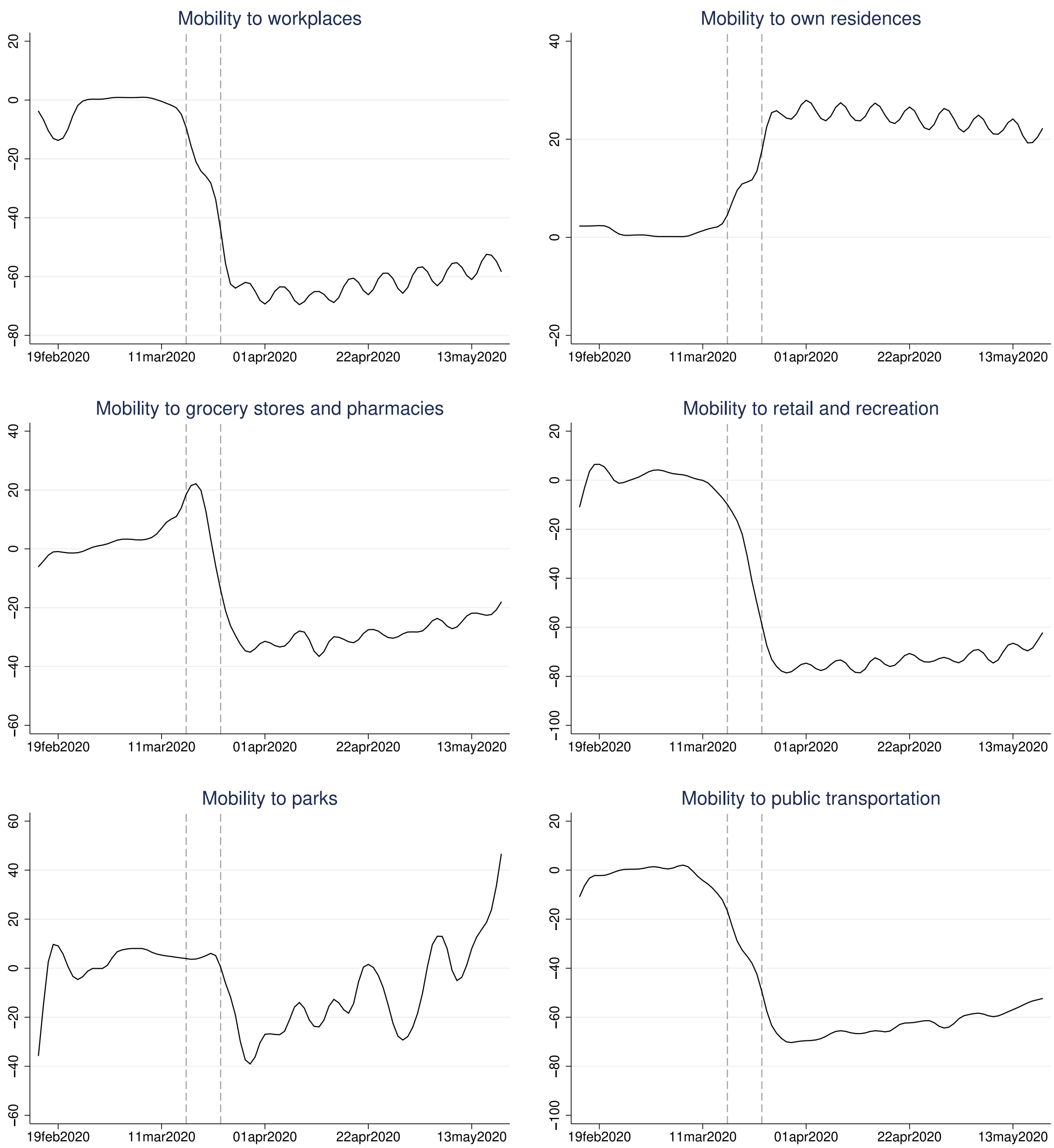

Fig. 4. Mobility trends during the period 15/2 - 19/5 of 2020.

Notes: This Figure shows time series of changes in mobility towards (i) workplaces, (ii) own residences, (iii) grocery stores and pharmacies, (iv) retail and recreation, (v) parks and (vi) public transportation. All series are smoothed using a median smoother of odd span using 5 observations. The data shows the visitor variation in a given day compared to a reference, defined as the average level of mobility calculated immediately before the COVID-19 outbreak, i.e. from January 3 to February 6, 2020.

5.2. Effects on alcohol use

In this section, we report estimates of the effect of mobility restrictions on health behaviours. Our outcome of interest is depicted by drinking frequency and consumption. The parameters of interest are identified by a dummy variable referring the 2020 year, labelled *post* in the table. Such varianle measures the overall post-restrictions variation in drinking behaviours and *Treatment* × *post*, capturing the differential post-restriction effect for individuals living in *treated* regions. We show the results from these estimates in Table 1, where the first two columns report estimates for alcohol use among men (col. 1) and women (col. 2) separately, whereas columns 3 and 4 refer to drinking frequency (having more than 4 drinks per week), again for men (col. 3) and women (col. 4) separately. Interestingly, after the restrictions were introduced, alcohol use decreased by 11.45 among for women. In contrast, drinking intensity increased by 13.93 percentage points among men and 16.08 percentage points among women. As expected, drinking behaviours in 2019 are very similar to that of the base year (e.g., 2017). However, we find a significant negative effect on alcohol use among men in treated regions: point estimates suggest a decrease in the probability of drinking by 2.48 percentage points.

Table 3 shows that treated and control individuals have the same pre-restriction drinking behaviour, supporting the parallel trend assumption.

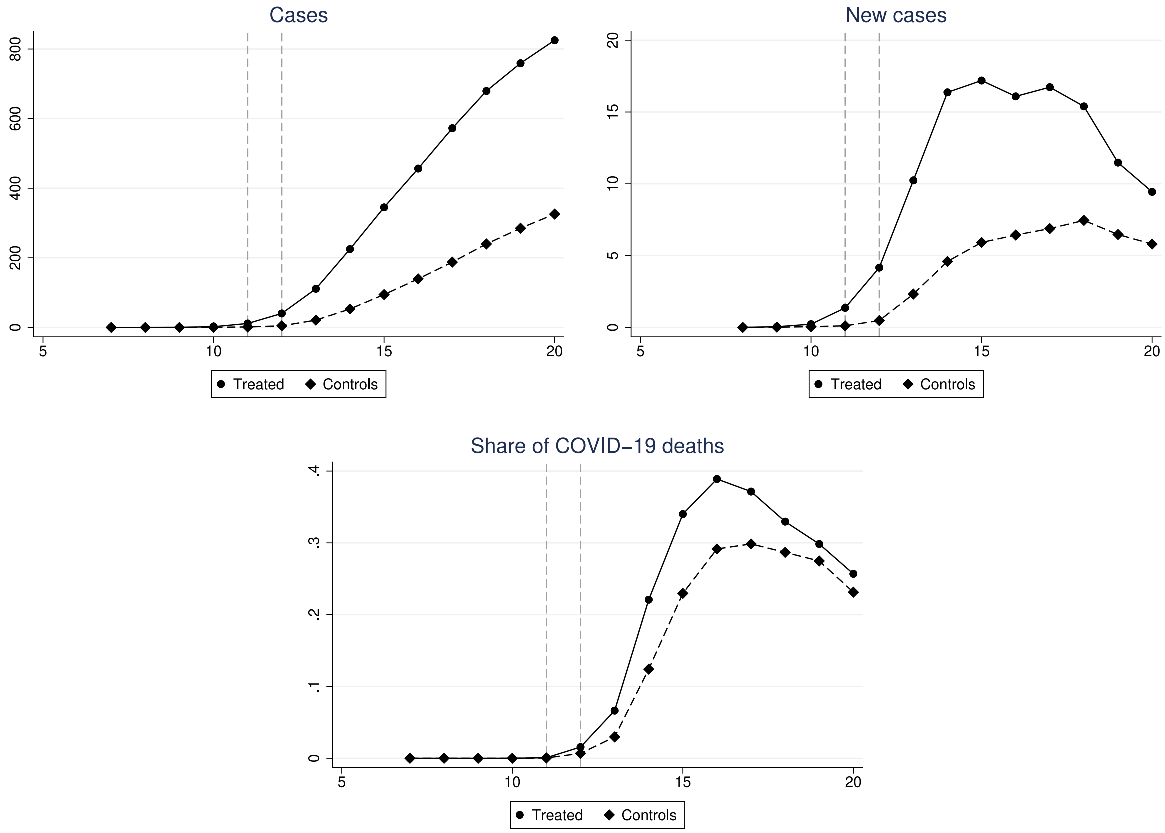

Fig. 5. Weekly values of COVID-19 cases, COVID-19 new cases, COVID-19 deaths, all deaths, and share of COVID-19 deaths on all deaths.
Notes: This Figure shows weekly values of COVID-19 cases, COVID-19 new cases, COVID-19 deaths, all deaths, and share of COVID-19 deaths on all deaths during the period 15/2 - 19/5 of 2020. Information about cases is available from the ONS daily. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate.

Table 1
Effect of COVID-19 restrictions on alcohol use in the UK.

	Drinking			
	Participation		Intensity	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
post	−0.1145***	−0.1514***	0.1393***	0.1608***
	(0.007)	(0.007)	(0.011)	(0.009)
Treated × post	−0.0248***	−0.0081	−0.0194	−0.0034
	(0.009)	(0.008)	(0.013)	(0.011)
Constant	0.8145***	0.8028***	0.2205***	0.1340***
	(0.017)	(0.014)	(0.021)	(0.015)
Mean of Y	0.800	0.734	0.231	0.151
SD of Y	0.400	0.442	0.422	0.358
Number of individuals	17,036	21,292	13,937	16,169
Observations	28,707	37,331	23,286	28,012

Notes: This Table shows DD estimates of COVID-19 restrictions on drinking habits of individuals living in treated regions, compared to controls using Understanding Society data. All specification control for individual and year fixed effects and individual level covariates. For a detailed description of the covariates used in our model, see Table A.1 in the appendix. We defined as *treated*, NUTS1 regions with a pre-COVID-19 restrictions death ratio above average, whereas *control* regions have a ratio below average. The death ratio is calculated as the ratio between deaths attributable to COVID-19 and deaths for other causes in NUTS1 regions before the imposition of COVID-19 restrictions, i.e. before the 16-th of March — the day in which the UK Prime Minister stated that unnecessary travel and social contacts should be avoided. The date of the official COVID-19 restrictions in the UK is the 23rd of March. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. Standard errors clustered at the individual level. The post takes the value 1 for observations collected during the first COVID-19 wave of Understanding Society released in April 2020 and 0 for observations collected in the previous waves, i.e. 2019, 2017 and 2015. Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.3. Effects on mobility

Table 4 shows estimates of Eq. (3). We find that treated regions significantly decreased mobility towards workplaces, retail and recreation and public transportation and significantly increased mobility towards own residences and parks compared to control regions. The estimated effects are −2.97, −1.77, and −5.39 percentage points for workplaces, retail and recreation, and public transportation and 2.19 and 10.98 percentage points respectively when looking at mobility to own residences and parks. Such estimates are non-negligible since they entail a change in 110.41%, 211.72%, 322.75%, 203.72% and 492.37% compared to the pre-lockdown mobility to workplaces, retail and recreation, public transportation, own residences, and parks, respectively but are indeed much smaller than variations shown in Fig. 4. These estimates are suggestive of the importance of DD analysis. The same analysis is also shown graphically in Fig. 6, where we show the results from the event study analysis where lags and leads are included to estimate post-treatment and anticipatory effects. Here we focus first on the latter to verify the common trend assumption. When we turn to examine mobility to all places, with the exception of own residences, we find evidence of positive leads values decreasing right before the announcement of COVID-19 restrictions, e.g., the 16-th of March. In contrast, mobility to own residences shows the opposite behaviour. We find evidence of negative leads increasing right before the 16-th of March. The presence of these trends right before the COVID-19 restrictions' announcement date can be interpreted as evidence of the possibility of an additional anticipation effect, which may depend on the fact that some people in treated regions modified their mobility behaviours already before that date.[9] Fig. 7 provides additional event study evidence moving the date identifying the post-treatment period to the 9th of March, i.e. the date of the Italian lockdown. In this case, almost all the leads are not statistically different from 0, meaning that treated and control regions have the same behaviour in terms of mobility.

Fig. 7 depicts the 9th of March effects on the mobility indicators used in the analysis. We show that mobility to workplaces (panel a) starts to decrease gradually in treated regions, dropping to −5 percentage points around the 18th of March, i.e. a couple of days after the announcement of restrictions in the UK. After this date, the effect decreases for a week and then stabilises around −3 percentage points after the 25th of March, a couple of days after the announcement of the first COVID-19 lockdown in the UK. Mobility to own residences mirrors mobility to workplaces but with variations of opposite signs. (Panel b) of Fig. 7 suggests that mobility to own residences slightly increases during the first lags but jumps to almost +2.5 percentage points from the 16th of March, lag(9), and then remains stable, except for Saturdays and Sundays, when mobility to own residences in treated regions is close to that of control regions.

Next, mobility to retail and recreation places (panel c) in treated regions is shown to decrease significantly until the 23rd of March and then converges to pre-restriction values. Consistently, mobility to grocery stores and pharmacies (panel d) increased significantly in treated regions during the 17th and 18th of March, suggesting a possible stockpiling effect in treated regions a couple of days after the announcement of mobility restrictions and then shows a mobility pattern similar to control regions. Mobility to public transportation (panel e) starts to decrease after the 9th of March. It continues to drop until the 22nd of March, settling on a negative variation of about 5 percentage points with respect to the pre-restrictions period. Mobility to parks in treated regions (panel f) shows an almost stable behaviour until lag(13), e.g., the 22nd of March, settling on a positive variation of about 10 percentage points with respect to the pre-restrictions period. The empirical evidence on mobility data can be interpreted as individuals in treated regions being more likely to decrease social contracts and social drinking.

5.4. Other potential explanations

One caveat of our analysis is that individuals in treated regions may be more likely to reduce drinking because of the fear of the health consequences of COVID-19 rather than the decrease in social contact. We try to shed light on this concern by estimating the model presented in Eq. (1) on various subsamples of individuals more at risk if exposed to COVID-19. Table 2 presents heterogeneous effects for individuals over the age of 65 (columns 1 and 2), who had COVID-19 symptoms (columns 3 and 4), with high blood pressure (columns 5 and 6), and with previous health conditions (columns 7 and 8). The upper panel of Table 2 shows estimates when the outcome is alcohol use, whereas the lower panel considers drinking intensity. If we find statistically different coefficients from those estimated in the overall population, results may be driven by fear of COVID-19 consequences. Otherwise, it should suggest evidence of the socialisation explanation.

Turning to alchol use (drinking participation), Table 2 suggests evidence of a significant reduction in the overall population for both men and women with comparable coefficients with respect to those presented in Table 1. The additional effect of living in a treated region is confirmed in almost all male subsamples, except for men who experienced COVID-19 symptoms, with estimated coefficients ranging from 4.13 to 5.90 percentage points. These values are very close to the overall effect estimated for men in Table 1. In addition, Table 2 highlights that women with high blood pressure and a previous health condition decreased their alcohol use by 4.01 and 2.03 percentage points, respectively, meaning that the health channel might be a plausible explanation for changes in alcohol use among women. Looking at the lower panel of Table 2, we find again that drinking intensity increased in 2020, i.e., the COVID-19 pandemic. Still, we do not find evidence of additional effects for men or women living in treated regions. Estimates without including observable covariates are shown in the appendix. See Tables A.3, A.4.

Another potential explanation refers to the fact that changes in drinking behaviour resulting from a reduction in household income are more pronounced in treated regions compared to control regions. We test this competing effect using the probability of being employed or furloughed as well as net income as an outcome and verify that they do not change differently between treated and control regions after the introduction of mobility restrictions. As shown in Table A.5, all coefficients associated with DD estimates (Treated × post) are not statistically different from zero for both men and women.

5.5. Robustness

In this section, we perform several robustness checks to test the validity and stability of our estimates.

First, we re-run our estimates after excluding observations from March 9, 2020, to March 16, 2020. Looking at Fig. 4, it is apparent that mobility decreases after March 9, e.g.,. the date of the Italian lockdown. Italy was the first European country to implement a strict lockdown which was heavily reported. This could have led people in other countries to decreate their mobility, possibly leading to a downward bias when estimating the lockdown effect on mobility. Consistently, we show these estimates in Table A.7. After excluding these observations, the estimated effects from the DD model are generally in line with those presented in Table 4. Still, we find larger differences when we examine mobility to retail and recreation and public transportation, which now are −2.18 and −5.83 percentage points rather than −1.77 and −5.39 percentage points before excluding observations, respectively.

[9] A plausible explanation could be connected to the fact that other European countries already implemented restrictions on mobility before the UK, and people in our treated regions may have partly reacted to these measures.

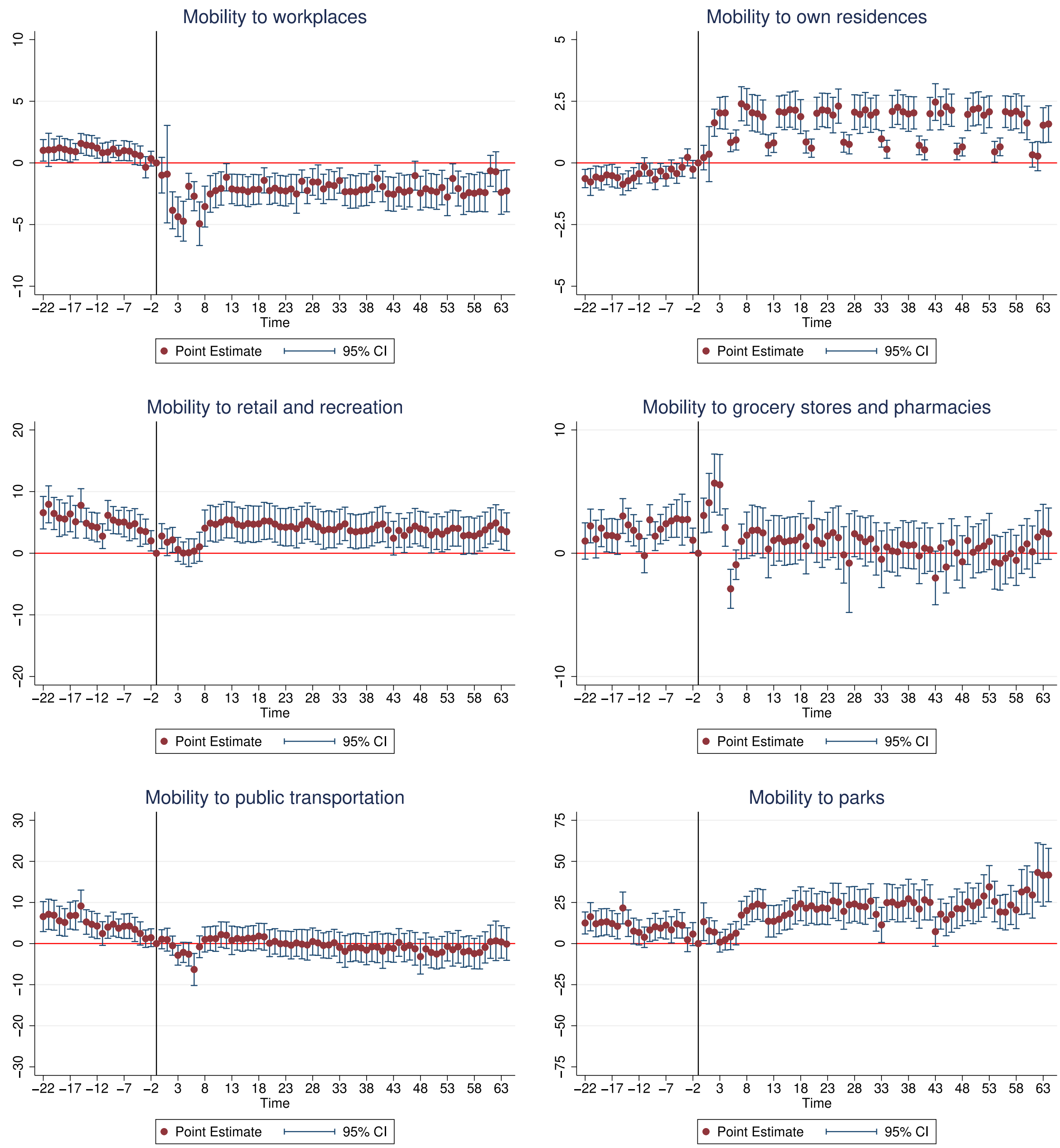

Fig. 6. Event estimates of the effect of living, during the UK COVID-19 restrictions, in regions with the ratio between COVID-19 deaths and deaths for other causes higher than the country average on mobility.

Notes: This Figure shows event estimates of the effect of living during the UK COVID-19 restrictions in regions with the ratio between COVID-19 deaths and deaths for other causes higher than the country average on mobility towards (i) workplaces, (ii) own residences, (iii) grocery stores and pharmacies, (iv) retail and recreation, (v) parks, (vi) public transportation. The vertical line is set on the day before the 16-th of March, i.e. the day the UK Prime Minister stated that unnecessary travel and social contact should be avoided. The date of the official COVID-19 restrictions in the UK is the 23-rd of March.

Second, since DD estimates of health behaviours rely on a different level of aggregation, e.g., NUTS1 rather than NUTS3, we run the analysis at the same level of aggregation to check whether our baseline estimates were consistent. We list these estimates in Table A.6. Estimates align with those already presented in the analysis, ensuring that our identification strategy can also be applied at the more aggregated NUTS1 territorial level.

Furthermore, Tables A.8 report evidence that no other NUTS3-specific time-varying factors explain the variations in mobility. We include in Eq. (3) NUTS3-specific linear trends plus a common quadratic component. Such a demanding specification accounts for the effect of other unobservable variables at the NUTS3 level that may be responsible for the observed decrease in mobility. Results from this analysis, reported in Table A.8, reveal that even when accounting for NUTS3-specific linear trends, we find coefficients are very close to those estimated in Table 4. The only exception refers to mobility to parks, which becomes non significantly different from zero when using this specification.

Finally, we check the sensitivity of our results to the choice of different thresholds to define the treatment variable. Table A.9 displays the results from this analysis. The effect of the treatment on mobility is stronger when we consider higher thresholds to define our treatment group. Looking at workplaces, the estimated effect ranges

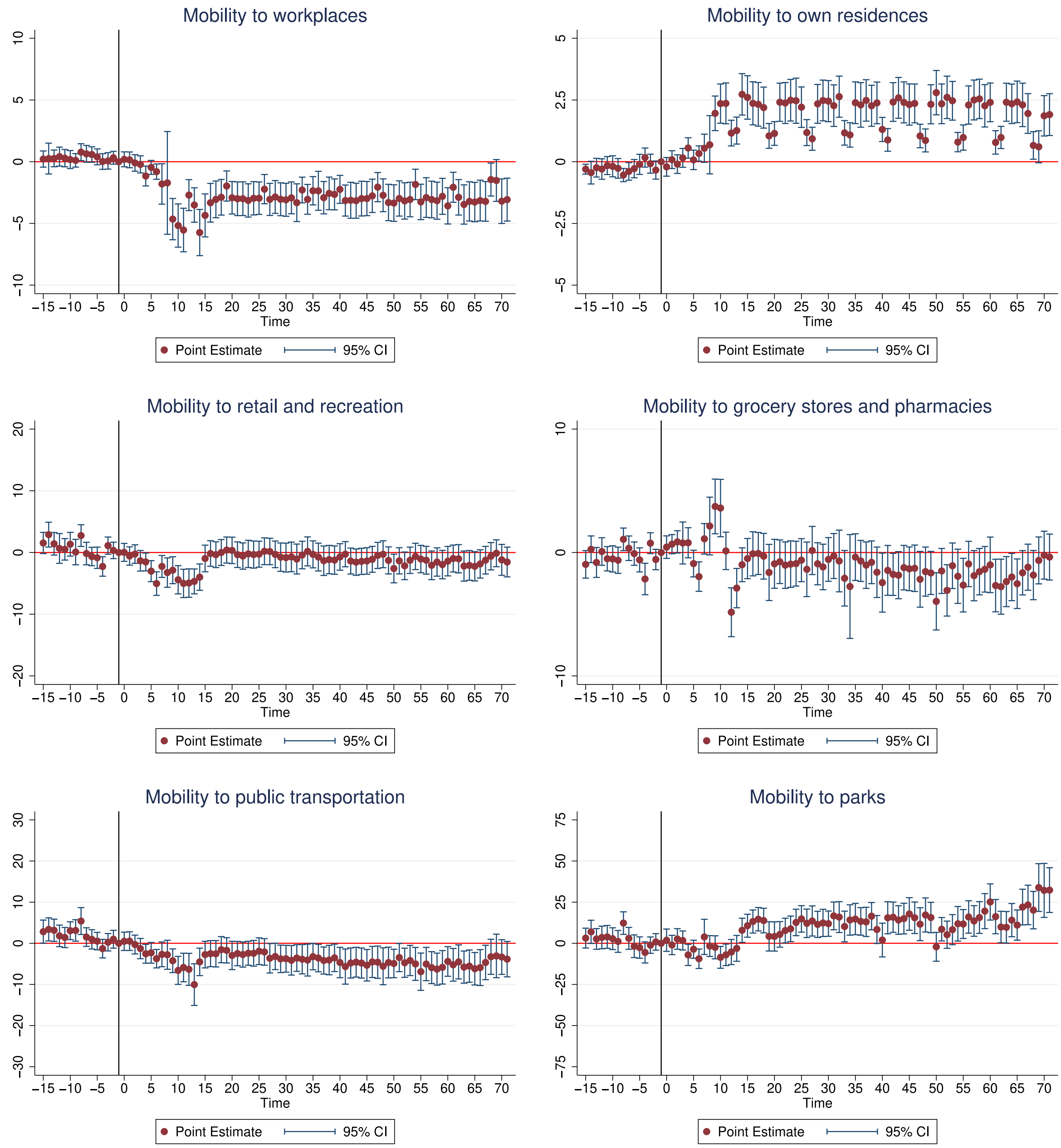

Fig. 7. Event estimates of the effect of living, during the UK COVID-19 restrictions, in regions with the ratio between COVID-19 deaths and deaths for other causes higher than the country average on mobility.

Notes: This Figure shows event estimates of the effect of living during the UK COVID-19 restrictions in regions with the ratio between COVID-19 deaths and deaths for other causes higher than the country average on mobility towards (i) workplaces, (ii) own residences, (iii) grocery stores and pharmacies, (iv) retail and recreation, (v) parks, (vi) public transportation. The vertical line is set on the day before the 9-th of March, i.e. the day Italy implemented a national lockdown.

from −2.16 to −5.96 percentage points, using the 10-th and the 90-th percentiles, respectively. The reduction in mobility to workplaces reaches −8.39 percentage points in regions above the 90-th percentiles of pre-announcement new COVID-19 daily cases compared to regions below the 10-th percentile of pre-announcement new COVID-19 daily cases. We draw attention to very comparable patterns for the other mobility indicators used in the analysis. Interestingly, when using the 75th percentile of pre-announcement new COVID-19 daily cases, the effect on pharmacies and grocery stores also turns negative and significant.

6. Conclusion

We study the effects of mobility restrictions during the first wave of the COVID-19 pandemic on alcohol use in the United Kingdom (UK). We document a polarised post-restriction effect, that is, alcohol use increased among heavy drinkers and reduced among low to moderate drinkers. This result is in line with what was found by some other studies in the literature (Jackson et al., 2021; Stevely et al., 2021). Two factors can explain the reduction in alchol use: (i) the variation in actual or expected earnings implied by the pandemic or (ii) a decrease in social gatherings. In contrast, the increase in the number of heavy drinking is explained by the higher stress levels during the pandemic.

Next, we exploit the predictions from the *prevalence-response elasticity* theory to isolate the effect of socialisation from that of other unobservable confounders. We document that socialisation matters more for men than for women. That is, consistently with a 'still and dry hypothesis' drinking participation decreased by 2.48 percentage points among men confined in their homes for longer hours alongside

Table 2
Effect of COVID-19 restrictions on drinking habits in the UK — heterogeneous effects by age and health status.

	Over 65		Had symptoms		Blood pressure		Previous health condition	
	Men	Women	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Drinking participation							
post	−0.1171***	−0.1776***	−0.1060***	−0.1481***	−0.0865***	−0.1598***	−0.1219***	−0.1719***
	(0.013)	(0.016)	(0.023)	(0.022)	(0.014)	(0.017)	(0.010)	(0.010)
Treated × post	−0.0567***	−0.0169	−0.0171	0.0070	−0.0590***	−0.0401*	−0.0413***	−0.0203*
	(0.018)	(0.020)	(0.025)	(0.024)	−0.018	(0.022)	(0.013)	(0.012)
Constant	0.9412***	0.8670***	0.8698***	0.8984***	0.8541***	0.8342***	0.8790***	0.8706***
	(0.069)	(0.106)	(0.044)	(0.046)	(0.068)	(0.064)	(0.034)	(0.027)
Mean of Y	0.807	0.699	0.839	0.796	0.842	0.750	0.827	0.760
SD of Y	0.395	0.459	0.368	0.403	0.365	0.433	0.378	0.427
Number of individuals	4,106	4,814	792	1,167	1,369	1,319	3,328	4,466
Observations	6,818	7,596	2,155	3,165	3,839	3,737	9,264	12,395
	Drinking intensity							
post	0.0949***	0.1225***	0.1875***	0.1490***	0.1211***	0.1789***	0.1293***	0.1509***
	(0.019)	(0.021)	(0.034)	(0.029)	(0.023)	(0.022)	(0.015)	(0.013)
Treated × post	−0.0270	−0.0058	−0.0447	−0.0086	−0.0429	−0.0715	−0.0284	−0.0056
	(0.025)	(0.025)	(0.039)	(0.032)	(0.028)	(0.077)	(0.018)	(0.016)
Constant	0.4689***	0.1587**	0.2067***	0.0977**	0.3771***	0.2794***	0.3207***	0.1890***
	(0.084)	(0.065)	(0.052)	(0.043)	(0.063)	(0.074)	(0.037)	(0.028)
Mean of Y	0.337	0.223	0.264	0.169	0.345	0.225	0.320	0.198
SD of Y	0.473	0.416	0.441	0.375	0.475	0.418	0.467	0.398
Number of individuals	3,393	3,456	708	1,038	1,239	1,115	2,984	3,830
Observations	5,578	5,465	1,817	2,536	3,245	2,820	7,695	9,472

Notes: This Table shows DD estimates by age and health status recorded before the pandemic, of COVID-19 restrictions on drinking habits of individuals living in treated regions, compared to controls, using Understanding Society data. All specification control for individual and year fixed effects and individual level covariates. For a detailed description of the covariates used in our model, see Table A.1 in the appendix. We defined as *treated*, NUTS1 regions with a pre-COVID-19 restrictions death ratio above average, whereas *control* regions have a ratio below average. The death ratio is calculated as the ratio between deaths attributable to COVID-19 and deaths for other causes in NUTS1 regions before the imposition of COVID-19 restrictions, i.e. before the 16-th of March — the day in which the UK Prime Minister stated that unnecessary travel and social contacts should be avoided. The date of the official COVID-19 restrictions in the UK is the 23-rd of March. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. Standard errors clustered at the individual level. The post takes the value 1 for observations collected during the first COVID-19 wave of Understanding Society released in April 2020 and 0 for observations collected in the previous waves, i.e. 2019, 2017 and 2015. Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3
Pre-trend for drinking habits before the imposition of COVID-19 restrictions in UK.

	Drinking			
	Participation		Intensity	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
Lead(−5)	0.0104	0.0010	0.0370	0.0227
	(0.010)	(0.011)	(0.023)	(0.032)
Lead(−3)	0.0005	0.0036	0.0107	−0.0006
	(0.006)	(0.006)	(0.010)	(0.008)
Constant	0.9013***	0.8486***	0.2950***	0.1647***
	(0.022)	(0.002)	(0.030)	(0.002)
Number of pidp	6,611	8,979	5,903	7,693
Observations	22,565	22,273	16,077	14,411

Notes: This Table shows event estimates of COVID-19 restrictions on drinking habits for individuals living in treated regions, compared to controls to test the common pre-trend assumption for the DD model using Understanding Society data. All specification control for individual and year fixed effects and individual level covariates. For a detailed description of the covariates used in our model, see Table A.1 in the appendix. We defined as *treated*, NUTS1 regions with a pre-COVID-19 restrictions death ratio above average, whereas *control* regions have a ratio below average. The death ratio is calculated as the ratio between deaths attributable to COVID-19 and deaths for other causes in NUTS1 regions before the imposition of COVID-19 restrictions, i.e. before the 16-th of March — the day in which the UK Prime Minister stated that unnecessary travel and social contacts should be avoided. The date of the official COVID-19 restrictions in the UK is the 23-rd of March. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. Standard errors clustered at the individual level. The post takes the value 1 for observations collected during the first COVID-19 wave of Understanding Society released in April 2020 and 0 for observations collected in the previous waves, i.e. 2019, 2017 and 2015. Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4
Effect of COVID-19 restrictions on mobility in UK.

	Workplace (1)	Residential (2)	Grocery (3)	Retail (4)	Parks (5)	Public transports (6)
Treated × post	−2.9683***	2.1906***	−0.7858	−1.7680**	10.9818***	−5.3901***
	(0.683)	(0.357)	(0.736)	(0.682)	(2.849)	(1.536)
Constant	−3.7280***	2.3008***	−6.0386***	−10.8058***	−35.4700***	−11.0692***
	(0.265)	(0.159)	(0.214)	(0.406)	(0.920)	(0.763)
Mean of Y before 16/3	−2.692	1.075	2.438	0.836	2.239	−1.676
SD of Y before 16/3	5.723	1.204	5.263	6.782	18.85	7.224
Number of NUTS3	179	176	179	179	175	178
Observations	16,927	15,126	16,828	16,739	15,176	16,753

Notes: This Table shows DD estimates of COVID-19 restrictions on changes in mobility towards (i) workplaces, (ii) own residences, (iii) grocery stores and pharmacies, (iv) retail and recreation, (v) parks, (vi) public transportation during the period 15/2 – 19/5 of 2020 for individuals living in treated regions, compared to controls (Eq. (3)) using Google COVID-19 Community Mobility Reports data. All specification control for day and NUTS3 fixed effects. We defined as *treated*, NUTS3 regions with a pre-COVID-19 restrictions death ratio above average, whereas *control* regions have a ratio below average. The death ratio is calculated as the average ratio between deaths attributable to COVID-19 and deaths for other causes in the period before the imposition of COVID-19 restrictions, i.e. before the 16-th of March, i.e. the day in which the UK Prime Minister stated that unnecessary travel and social contacts should be avoided. The date of the official COVID-19 restrictions in the UK is the 23-rd of March. Information about deaths is available from the ONS weekly. A specific death case is attributed to COVID-19 if it corresponds to a death that occurred 28 days after a positive COVID-19 test, and hence COVID-19 is mentioned in the death certificate. Standard errors clustered at the NUTS3 level. Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

no opportunities for social drinking. However, we find no significant effects for women.

Our results are robust to a series of robustness checks. First, our results are not driven by the fear of the health consequences of COVID-19 but rather by the reduction in social contact. When we re-estimate our baseline model at different subsamples of individuals depending on their risk exposure to COVID-19 we find that, for men, coefficients are systematically not statistically different from those of the overall population, whilst we find some differences among women. Drinking decreases by a respective additional 4.01 and 2.03 percentage points when considering women with high blood pressure or a previous health condition.

In examining different mechanisms we find that, after the lockdown, mobility decreased sharply in all the indicators considered, including mobility to workplaces. When we compare mobility among those in treated and control regions, we find an extra-reduction in mobility in the former areas. People living in areas with more cases before the introduction of mobility restrictions are more likely to respect national guidelines regarding social distancing after lockdown reducing their mobility.

These results are consistent with a ’still and dry pandemic for the many’ hypothesis, namely a reduction in alcohol use among social drinkers. However, we identify a rise in alcohol use among heavy drinkers (“the few”), suggesting evidence of ’risky drinking’ in which higher risk exposure, namely higher risk exposure drives some people to drink as a coping mechanism.

Our findings suggest that mobility restrictions can exert several potential effects beyond influencing mobility, such as restricting alcohol use for some, which is more common among individuals for whom alcohol use is a means to socialise (Rosenquist et al., 2010). These estimates suggest policy implications, that is, that availability and social effects have an important influence on alcohol use. Hence, restricting opportunities to drink socially can help individuals reduce their alcohol consumption.

CRediT authorship contribution statement

Martina Celidoni: Conceptualization, Methodology, Data curation, Writing – original draft, Visualization, Investigation, Writing – review & editing. **Joan Costa-Font:** Conceptualization, Methodology, Data curation, Writing – original draft, Visualization, Investigation, Writing – review & editing. **Luca Salmasi:** Conceptualization, Methodology, Data curation, Writing – original draft, Visualization, Investigation, Writing – review & editing.

Data availability

The authors do not have permission to share data.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.ehb.2023.101268.

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