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Testing the predictive accuracy of COVID-19 forecasts*

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Abstract

We test the predictive accuracy of forecasts of the number of COVID-19 fatalities produced by several forecasting teams and collected by the United States Centers for Disease Control and Prevention for the epidemic in the United States. We find three main results. First, at the short horizon (1-week ahead) no forecasting team outperforms a simple time-series benchmark. Second, at longer horizons (3- and 4-week ahead) forecasters are more successful and sometimes outperform the benchmark. Third, one of the best performing forecasts is the Ensemble forecast, that combines all available predictions using uniform weights. In view of these results, collecting a wide range of forecasts and combining them in an ensemble forecast may be a superior approach for health authorities, rather than relying on a small number of forecasts.

JEL classification codes: C12; C53; I18.

Keywords: Forecast evaluation, Forecasting tests, Epidemic.

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1 Introduction

Forecasting the evolution of an epidemic is of utmost importance for policymakers and health care providers. Timely and reliable forecasts are necessary to help health authorities and the community at large coping with a surge of infections and to inform public health interventions, for example, to enforce (or ease) a lockdown at the local or national level. Accordingly, in recent months there has been a rapidly growing number of research teams developing forecasts for the evolution of the current COVID-19 pandemic caused by the new coronavirus, SARS-CoV-2.

In the United States, the Centers for Disease Control and Prevention (CDC) collects weekly forecasts of the evolution of the COVID-19 pandemic produced by different institutions and research teams. These forecasts are aimed at informing public health decision-making by projecting the probable impact of the COVID-19 pandemic at horizons up to four weeks. The forecasting teams that submit their forecasts to the CDC include data scientists, epidemiologists, and statisticians, and use different models and methods (e.g. SEIR, Bayesian, and Deep Learning models), combining a variety of data sources and assumptions about the impact of non-pharmaceutical interventions on the spread of the epidemic (such as social distancing and the use of face coverings). This wealth of forecasts can be extremely valuable for decision-makers, but it also poses a problem: how to act when confronted with heterogeneous forecasts and, in particular, how to select the most reliable projections. Decision-makers are thus faced with the task of comparing the predictive accuracy of different forecasts. Indeed, selecting models and comparing their predictive accuracy are different tasks, and in this paper we focus on the latter.

As the Diebold and Mariano (DM) test of equal predictive accuracy (see Diebold and Mariano 1995, Giacomini and White 2006) adopts a model-free perspective to compare competing forecasts, imposing assumptions only on the forecast errors loss differential, we use it to compare competing forecasts for the number of COVID-19 fatalities collected by

the CDC. The application of the DM test is particularly challenging when only a few out-of-sample observations are available, as the standard test is unreliable, especially for multi-step forecasts (Clark and McCracken 2013). To overcome this small-sample problem, we apply fixed-smoothing asymptotics, as recently proposed for this test by Coroneo and Iacone (2020).

With fixed-smoothing asymptotics, the limit distribution of the DM statistic is derived under alternative assumptions. In particular, when the long-run variance in the test statistic is estimated as the weighted autocovariances estimate, the asymptotic distribution is derived assuming that the bandwidth-to-sample ratio (denoted as b) is constant, as recommended by Kiefer and Vogelsang (2005). With this alternative asymptotics, usually known as fixed-b, the test of equal predictive accuracy has a nonstandard limit distribution that depends on b and on the kernel used to estimate the long-run variance. The second alternative asymptotics that Coroneo and Iacone (2020) consider is the fixed-m approach, as in Sun (2013) and Hualde and Iacone (2017). In this case, the estimate of the long-run variance is based on a weighted periodogram estimate, the asymptotic distribution is derived assuming that the truncation parameter m is constant, and the test of equal predictive accuracy has a t distribution with degrees of freedom that depend on the truncation parameter m. Both approaches have been shown to deliver correctly sized predictive accuracy tests, even when only a small number of out-of-sample observations is available (see Coroneo and Iacone 2020, Harvey, Leybourne and Whitehouse 2017).

We evaluate forecasts for the cumulative number of COVID-19 fatalities produced at the national level for the United States by the eight forecasting teams that submitted their forecasts to the CDC without interruptions during the period June 20, 2020 to March 20, 2021. Although the evaluation period includes only 40 observations, we document an increase in the volatility of the forecasting errors around the second half of the sample. Accordingly, we perform our forecast evaluation separately on two sub-samples of equal size: the first evaluation sub-sample (from June 20, 2020 to October 31, 2020) and the second

evaluation sub-sample (from November 7, 2020 to March 20, 2021). This implies that for each evaluation sub-sample we can base our inference only on 20 observations, making the use of fixed-smoothing asymptotics crucial for obtaining reliable results.

We compare the predictive accuracy of the forecasts of each team relative to the forecasts of a simple benchmark model, obtained by fitting a second-order polynomial using a rolling window of the last five available observations. We also consider two ensemble forecasts that combine the forecasts from several models using equal weights: one published by the CDC and another one (the core ensemble) computed by us combining only the forecasts included in our evaluation exercise.

A feature that makes forecast evaluation important in its own right, especially when dealing with predicting the spread of COVID-19, is that the cost of under-predicting the spread of the disease can be greater than the cost of over-predicting it. In the midst of a public health crisis, the precautionary principle implies that erring on the side of caution is less costly than predicting the tapering off of the disease too soon. Scale effects may also be important in the evaluation of forecasts of an epidemic outbreak, since the same forecast error may be considered differently when the realized level of fatalities is small, and when there is a large number of fatalities. These effects may be taken into account in the forecast evaluation exercise by a judicious choice of the loss function. Therefore, we evaluate the predictive accuracy of each forecasting team using several loss functions, that include the widely used quadratic and absolute value loss, the absolute percentage loss (that takes into account the scale of the number of fatalities), and a linear exponential loss function (that penalizes under-prediction more than over-prediction).

Our findings can be summarized as follows. First, the simple polynomial benchmark outperforms the forecasters at the short horizon (1-week ahead), often significantly so. Second, at longer horizons (3- to 4-week ahead), the forecasters become more competitive and some statistically outperform the simple benchmark, especially in the first evaluation sub-sample.

This suggests that forecasters can successfully help inform forward-looking policy decisions. Third, the ensemble forecasts are among the best performing forecasts. This is particularly true in the first evaluation sub-sample, but even in the second sub-sample the ensemble forecast combinations outperform the benchmark, although in this sub-sample the DM test statistics are not statistically significant. The reliability of ensemble forecasts underlines the virtues of model averaging when uncertainty prevails, and supports the view in Manski (2020) that data and modelling uncertainties limit our ability to predict the impact of alternative policies using a tight set of models. Overall, our findings hold for all the loss functions considered and caution health authorities not to rely on a single forecasting team (or a small set) to predict the evolution of the pandemic. A better strategy appears to be to collect as many forecasts as possible and to use an ensemble forecast.

The remainder of the paper is organized as follows. Section 2 lays out the methodology to implement the test of equal predictive accuracy. Section 3 describes the data and the models. Results are documented and discussed in Section 4, and Section 5 concludes. Finally, in the Appendix, we perform a Monte Carlo simulation exercise to study the size and power properties of the two tests of equal predictive ability with fixed-smoothing asymptotics for the sample sizes considered in our empirical study, and consider several additional experiments including some alternative benchmark forecasts.

2 Forecast Evaluation

We consider the time series of cumulative weekly deaths $\{y_1, ..., y_T\}$, with T the sample size for which forecasts are available. We want to compare two h-week ahead forecasts $\widehat{y}^{(1)}_{t|t-h}\left(\widehat{\theta}^{(1)}_{w_1}\right)$ and $\widehat{y}^{(2)}_{t|t-h}\left(\widehat{\theta}^{(2)}_{w_2}\right)$, where $\widehat{\theta}^{(i)}_{w_i}$ for $i = 1, 2$ denote the estimates obtained with a rolling window of size w_i used to construct forecast i, if known.

The forecast error for forecast i is $e^{(i)}_{t|t-h} = y_t - \widehat{y}^{(i)}_{t|t-h}\left(\widehat{\theta}^{(i)}_{w_i}\right)$ and the associated loss is $L^{(i)}_{t|t-h} \equiv L\left(e^{(i)}_{t|t-h}\right)$, for example, a quadratic loss would be $L\left(e^{(i)}_{t|t-h}\right) = \left(e^{(i)}_{t|t-h}\right)^2$ and an

absolute value loss would be $L\left(e_{t|t-h}^{(i)}\right) = \left|e_{t|t-h}^{(i)}\right|$. The null hypothesis of equal predictive ability of the two forecasts is

$$H_0 : E\left[L\left(e_{t|t-h}^{(1)}\right) - L\left(e_{t|t-h}^{(2)}\right)\right] = 0. \tag{1}$$

Denote the time-t loss differential between the two forecasts as

$$d_t \equiv L\left(e_{t|t-h}^{(1)}\right) - L\left(e_{t|t-h}^{(2)}\right),$$

and the sample mean of the loss differential as

$$\overline{d} = \frac{1}{T}\sum_{t=w+h}^{w+h+T-1} d_t,$$

where $w \equiv \max(w_1, w_2)$.

When a large sample T is available, standard asymptotic theory may provide a valid guidance for the statistical evaluation of $\overline{d}$, see Diebold and Mariano (1995) and Giacomini and White (2006). However, the same inference may be severely biased when the sample T has only a moderate size, as it is indeed the case when comparing forecast accuracy of predictions of the number of fatalities of COVID-19. In this case, fixed-b and fixed-m asymptotics can be used to overcome the small-sample size bias, see Coroneo and Iacone (2020), Choi and Kiefer (2010) and Harvey et al. (2017).

As for the fixed-b asymptotics, following Kiefer and Vogelsang (2005), under the null in (1)

$$\sqrt{T}\frac{\overline{d}}{\widehat{\sigma}_{BART,M}} \to_d \Phi_{BART}(b), \text{ for } b = M/T \in (0,1], \tag{2}$$

where $\widehat{\sigma}^2_{BART,M}$ denotes the weighted autocovariance estimate of the long-run variance of d_t using the Bartlett kernel and truncation lag M. Kiefer and Vogelsang (2005) characterize the limit distribution $\Phi_{BART}(b)$ and provide formulas to compute quantiles. For example, for

the Bartlett kernel with $b \leq 1$, these can be obtained using the formula

$$q(b) = \alpha_0 + \alpha_1 b + \alpha_2 b^2 + \alpha_3 b^3,$$

where

$$\alpha_0 = 1.2816,\ \alpha_1 = 1.3040,\ \alpha_2 = 0.5135,\ \alpha_3 = -0.3386 \text{ for } 0.900 \text{ quantile}$$

$$\alpha_0 = 1.6449,\ \alpha_1 = 2.1859,\ \alpha_2 = 0.3142,\ \alpha_3 = -0.3427 \text{ for } 0.950 \text{ quantile}$$

$$\alpha_0 = 1.9600,\ \alpha_1 = 2.9694,\ \alpha_2 = 0.4160,\ \alpha_3 = -0.5324 \text{ for } 0.975 \text{ quantile}$$

When testing assumptions about the sample mean, Kiefer and Vogelsang (2005) show in Monte Carlo simulations that the fixed-b asymptotics yields a remarkable improvement in size. However, while the empirical size improves (it gets closer to the theoretical size) as b is closer to 1, the power of the test worsens, implying that there is a size-power trade-off.

For fixed-m asymptotics, following Hualde and Iacone (2017), under the null in (1) we have

$$\sqrt{T}\frac{\overline{d}}{\widehat{\sigma}_{DAN,m}} \rightarrow_d t_{2m}, \tag{3}$$

where $\widehat{\sigma}^2_{DAN,m}$ is the weighted periodogram estimate of the long-run variance of d_t using the Daniell kernel and truncation m. Similar results, with a slightly different standardisation and therefore a slightly different limit, are in Sun (2013). Monte Carlo simulations in Hualde and Iacone (2017) and Lazarus, Lewis, Stock and Watson (2018) show that fixed-m asymptotics has the same size-power trade-off documented for fixed-b asymptotics: the smaller the value for m, the better the empirical size, but also the weaker the power.

Coroneo and Iacone (2020) analyze the size and power properties of the tests of equal predictive accuracy in (2) and (3) in an environment with asymptotically non-vanishing estimation uncertainty, as in Giacomini and White (2006). Results indicate that the tests in (2) and (3)

deliver correctly sized predictive accuracy tests for correlated loss differentials even in small samples, and that the power of these tests mimics the size-adjusted power. Considering size control and power loss in a Monte Carlo study, they recommend the bandwidth $M = \lfloor T^{1/2} \rfloor$ for the weighted autocovariance estimate of the long-run variance using the Bartlett kernel (where $\lfloor \cdot \rfloor$ denotes the integer part of a number) and $m = \lfloor T^{1/3} \rfloor$ for the weighted periodogram estimate of the long-run variance using the Daniell kernel.

In Appendix A, we perform a Monte Carlo simulation exercise to investigate the empirical size and power of the two tests for sample sizes that match the ones in our empirical study. Our findings indicate that both tests are, in general, correctly sized, even when only 20 observations are available and in presence of autocorrelation of the loss differential, although the test with WCE and fixed-b asymptotics can be slightly oversized in short samples and in presence of strong autocorrelation. On the other hand, the test with WPE and fixed-m asymptotics trails slightly behind the test with WCE in terms of power.

3 Forecasting Teams and Benchmark

3.1 Data and forecasting teams

In our empirical investigation, we use forecasts for the cumulative number of deaths collected by the Centers for Disease Control and Prevention (CDC). The CDC is a federal agency in charge of protecting public health through the control and prevention of diseases. It is also the official source of statistics on the COVID-19 pandemic evolution in the US. In particular, in collaboration with independent teams of forecasters, the CDC has set up a repository of weekly forecasts for the numbers of deaths, hospitalizations, and cases. These forecasts are developed independently by each team and shared publicly.[1] We focus on forecasts of the number of deaths for three main reasons. First, the number of fatalities is more accurate

[1]Background information on each forecasting teams, along with a summary explanation of their methods are available via the link https://www.cdc.gov/coronavirus/2019-ncov/covid-data/forecasting-us.html.

Table 1: Forecasting Teams, Methods, and Assumptions

Code	Team	Model	Method	Change
CO	COVID Analytics - MIT Sloan	DELPHI Model	Deep Learning model	no
UM	University of Massachusetts, Amherst	UMass - MB	Mechanistic Bayesian compartment model	no
UA	University of Arizona	UA - EpiCovDA	Modified SEIR model	yes
GT	Georgia Institute of Technology, Deep Outbreak Project	GT - Deep COVID	Deep Learning model	no
MO	Northeastern University, Laboratory for the Modeling of Biological and Socio-technical Systems	MOBS - GLEAM COVID	Metapopulation, age structured SEIR model	no
PS	Predictive Science, Inc.	PS - DRAFT	SEIR model	yes
LA	Los Alamos National Laboratory	LANL - Growth Rate	Statistical dynamical growth model	no
JH	Johns Hopkins University, Infectious Disease Dynamics Lab	JHU - IDD - CovidSP	Metapopulation SEIR model	yes

Notes: The column code describes the code given in the empirical analysis to each team. A forecasting team is included if it submitted its predictions for all the weeks in our sample. The table reports for each forecasting team the modelling methodology and whether the model considers a change in the assumptions about policy interventions. In the fourth column, "yes" means that the modelling team makes assumptions about how levels of social distancing will change in the future, while "no" means that it is assumed that the existing measures will continue through the projected 4-week time period.

than the number of cases and hospitalizations, since the latter ignores asymptomatic cases and other diseases that are undetected. Second, the number of deaths is reported with less spatial and temporal biases. Third, when faced with a pandemic, the number of fatalities is arguably the primary concern of the health authorities and of the public.

Our sample includes projections for national COVID-19 cumulative deaths made for the period between June 20, 2020 and March 20, 2021 by eight forecasting teams. The deadline for the teams to submit their weekly forecasts is on the Monday of each week, and they are usually published online on Wednesdays. Weekly cumulative data is the cumulative data up to and including Saturday. This means that, for example, the forecasts submitted by June 22 had as targets the cumulative number of deaths as of June 27 (1-week ahead), July 2 (2-week ahead), July 7 (3-week ahead), and July 12 (4-week ahead). Realised values are also taken from the CDC website. Notice that when COVID-19 is reported as a cause of mortality on the death certificate, it is coded and counted as a fatality due to COVID-19.

The eight forecasting teams selected are those that submitted their predictions with no interruptions for all the weeks in our sample. We list the selected teams in Table 1, and report the main features of the selected forecasts. They vary widely with regards to their modelling choice, information input (for example, how the information on infected people

is used), and in their assumptions about the evolution and impact of non-pharmaceutical interventions (for example regarding social distancing).[2]

3.2 Ensemble forecasts

In our forecast evaluation exercise, we also consider two Ensemble forecasts: one published by the CDC (that combines the individual forecasts from several teams and that we label Ensemble - EN) and one computed by us (that combines the individual forecasts from the eight teams listed in Table 1 and that we label Core Ensemble - CE).[3]

Combining forecasts is an effective procedure when there is uncertainty about the model and the data, as it is indeed the case here, where differences also include alternative assumptions on the responses of the public and of the health authorities. In this situation, combining forecasts is useful as it helps to diversify risk and to pool information (see Bates and Granger 1969). In particular, forecast combination is most advantageous when there is pervasive uncertainty, as the ranking of best-performing forecasts may be very unstable and therefore forecast combination provides a robust alternative (see Stock and Watson 1998, Timmermann 2006). Optimal weights that give the best combination, in the sense of minimizing a given loss function, can actually be derived, but in many practical applications estimated optimal weights schemes result in a combined forecast that does not improve simple averaging (see Clemen 1989, Smith and Wallis 2009, Claeskens, Magnus, Vasnev and Wang 2016).

In epidemiology, forecast combination has proved its ability to improve on the performance of individual competing models. For example, Reich et al. (2019) found that ensemble forecasting for influenza performed better on average against the constituting models; similar results have also been obtained by Chowell et al. (2020) in the Ebola Forecasting Challenge.

[2] Additional information about the models used is available on the CDC repository page https://github.com/cdcepi/COVID-19-Forecasts/blob/master/COVID-19_Forecast_Model_Descriptions.md, where links to the modelling teams are also provided.

[3] The CDC Ensemble forecast is produced in collaboration with several research groups who form part of the COVID-19 Forecast Hub consortium (see, https://covid19forecasthub.org/ for a detailed description).

Both these works had access to a sufficiently long history of data, making a data-driven selection of the weights assigned to the contributing models possible. Interestingly, Reich et al. (2019) considered also the equal weighting scheme in their exercise, and found that this naive ensemble performed quite well even against the one with data-driven weights, making it a reasonable choice for the current situation of a new epidemic, in which no previous outbreaks exist and no previous track record of past models is available.

The Ensemble forecast produced by the CDC is also naive, in the sense that it treats equally all the available forecasts. Specifically, it is obtained by combining the forecasts of all the teams that submitted to the CDC, as long as they submitted forecasts up to four weeks ahead and these forecasts were at least equal to the level observed on the day in which the forecast was submitted. The weekly composition of the pool of models contributing to the Ensemble forecast changes, and it includes, in general, a larger number of teams than the one we consider in our evaluation exercise.[4] This loose criterion allows to include as many forecasts as possible, which may be desirable, but there is also the risk of including poorly performing teams. For this reason, we also consider the Core Ensemble constructed by us, which uses only the forecasts (equally weighted) by the eight teams that are included in our forecast evaluation exercise. The conjecture motivating this choice is that, as these are the most long standing forecasting teams, they should also be the most experienced. This experience may give them an edge relative to other teams. In addition, by comparing the performance of the individual forecasts with the Core Ensemble forecast, we can reliably assess the value added by the combination of the forecasts, as the Core Ensemble uses only forecasts that are included in our exercise.

[4]In July 2020, the COVID-19 Forecast Hub changed the way it constructed the CDC Ensemble forecast (we thank an anonymous reviewer for bringing this to our attention). Up until the week ending on July 18, 2020, the CDC Ensemble forecast is obtained from an equally weighted average of forecasts across all eligible models. After that date, the methodology is changed and the Ensemble obtained from the median forecast across all the eligible models (see the COVID-19 hub documentation and, in particular, Ray et al. 2020).

3.3 Benchmark forecasts

The benchmark against which we compare the forecasts collected by the CDC is a polynomial function. That is, benchmark forecasts are obtained as projections from the model:

$$y_t = \beta_0 + \beta_1\, t + \beta_2\, t^2 + u_t, \tag{4}$$

where y_t is the cumulative number of fatalities, t is the time trend, and u_t is an unobserved error term. To accommodate the fact that the forecasted patterns may need changing even over a short span of time, we fit the quadratic polynomial model using Least Squares with a rolling window of the last five observations (using weekly data, this covers approximately a month). To ensure that the benchmark forecasts for the cumulative number of deaths are not decreasing, we compute the benchmark predictions as the maximum between the current value and the prediction from (4).

This very simple statistical model has been chosen because any continuous and differentiable function can be approximated locally by a polynomial, and we take the second degree polynomial as a local approximation. In recent works, the choice of a polynomial benchmark has also been considered by Jiang, Zhao and Shao (2020) and Li and Linton (2021), among others, although with some small differences. In Jiang et al. (2020), the intrinsic instability of the forecasted patterns is accommodated by fitting occasional breaks; Li and Linton (2021) fitted the model to the incidence of deaths, rather than to the cumulative deaths.

3.4 Preliminary Analysis

In this section, we present some preliminary analysis of the forecasts submitted by the forecasting teams in Table 1, the Ensemble (EN) forecast published by the CDC, the Core Ensemble (CE) constructed combining all the forecasts of the teams in Table 1, and the forecast of the polynomial benchmark (PO), described above.

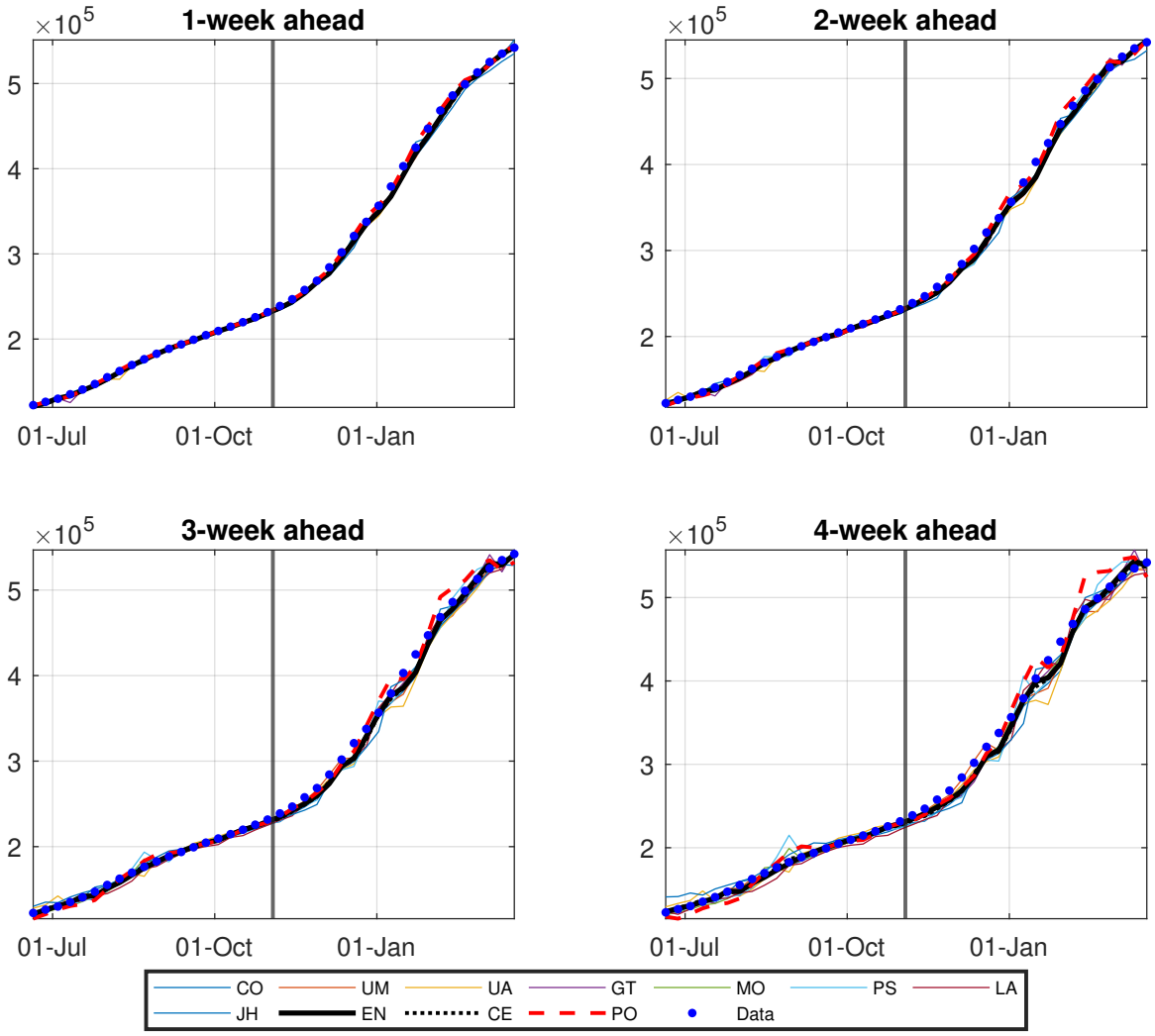
Figure 1 plots all the forecasts considered for the 1 to 4-week ahead forecasting horizons, alongside the realised data. Comparing the graphs in Figure 1 at different horizons, it is apparent that the heterogeneity in forecasts grows with the forecasting horizon and, concurrently, that the forecasts are less precise as the forecast horizon increases. This simple observation may make the case for forecast combination at longer horizons more compelling.

Figure 2 plots the forecast errors for each forecasting team, the ensembles and the benchmark (computed as the difference between the realization and the point forecast). The figure indicates that most forecasting teams seem to have systematically under-predicted the target, in particular in the second part of the sample. This is, of course, relevant for policy makers if the costs of over-prediction and under-prediction are different. Figure 2 also shows that the size of the forecast errors is increasing with the forecasting horizon, suggesting that there is more uncertainty about the evolution of the epidemic in the long-run (4-week ahead), compared to the short-run (1-week ahead).

Table 2 presents some summary statistics for the forecast errors. The table reports for each forecasting horizon and forecasting scheme (team, Ensemble, Core Ensemble or polynomial) the sample mean, median, standard deviation, skewness, and the sample autocorrelations up to order 4 (in the columns AC(1), AC(2), AC(3) and AC(4), respectively). With the exception of the benchmark, the average of the forecast errors are positive for all forecasts, meaning that the forecasters tend to under-predict the fatalities.

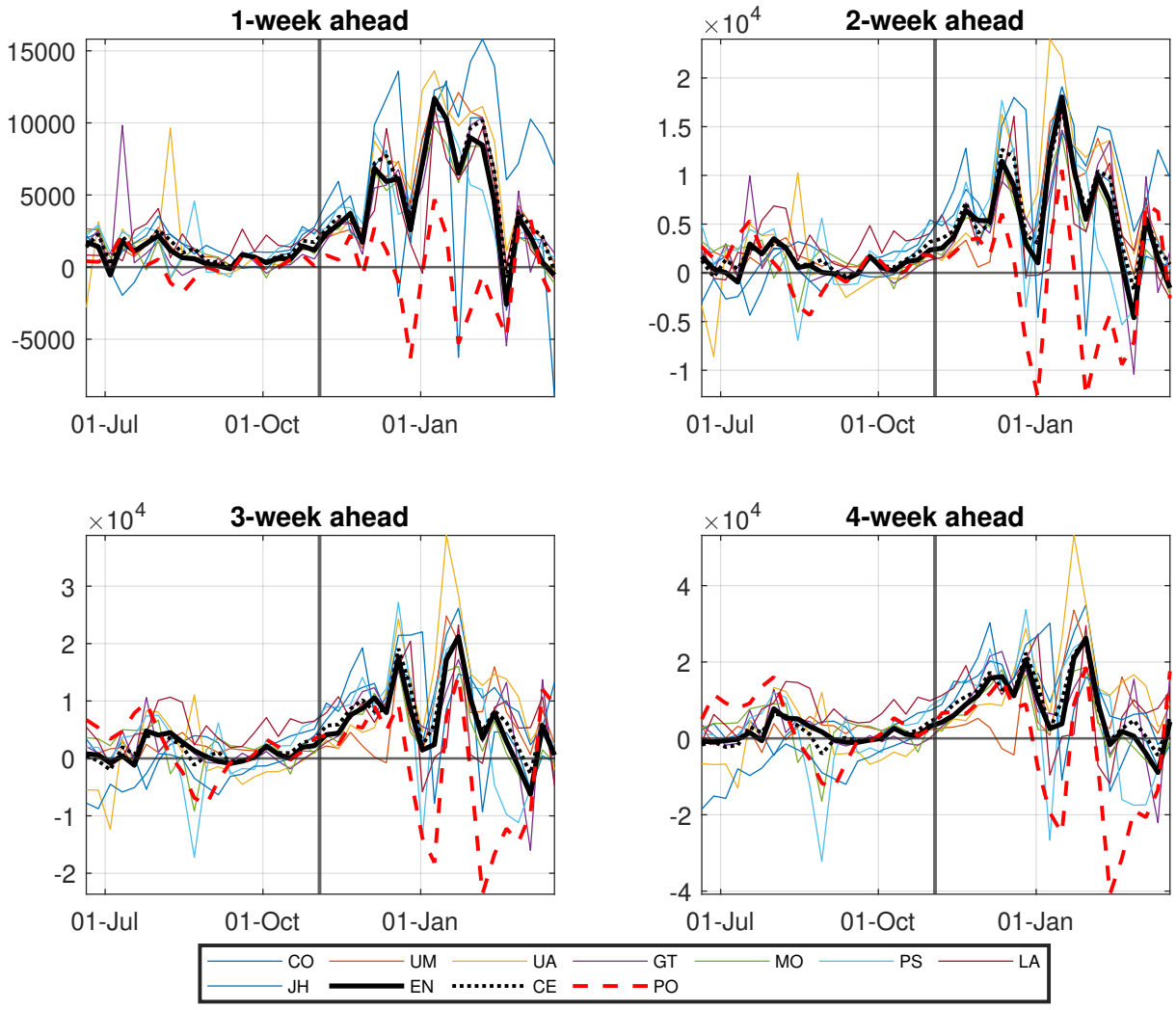
At the 1-week horizon, the benchmark polynomial model appears to outperform all the teams, with a much smaller average error and smaller dispersion. However, its performance deteriorates at longer horizons, with the volatility of the forecast errors increasing substantially, and becoming larger than those of most forecasting teams. This is not surprising, as epidemiological models are designed to predict the evolution of a pandemic in the medium and the long-run, and we observe here that even a very simple forecast does better when the horizon is very short. At longer horizons, however, epidemiological models should be

Figure 1: Cumulative deaths in US, observed vs. forecasts



Note: Forecasts at forecasting horizons from 1 to 4 weeks, along with realised cumulative fatalities. Weekly observations from June 20, 2020 to March 20, 2021. The vertical line indicates November 3, 2020 and delimits the two sub-samples. The names of the forecasting teams are as in Table 1; EN denotes the Ensemble published by the CDC, CE denotes the Core Ensemble constructed combining all the forecasts of the teams in Table 1, and PO denotes the polynomial benchmark.

Figure 2: Forecast errors



Note: Forecast errors at forecasting horizons from 1 to 4 weeks. Weekly observations from June 20, 2020 to March 20, 2021. The vertical line indicates November 3, 2020 and delimits the two sub-samples. The names of the forecasting teams are as in Table 1; EN denotes the ensemble forecast, CE denotes the core ensemble, and PO the polynomial benchmark. Forecast errors are defined as the realised value minus the forecast.

Table 2: Summary Statistics of Forecast Errors

1-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	2475.85	1920.00	4429.68	0.22	0.19	0.06	0.15	0.43
UM	3363.88	1705.50	3693.80	1.19	0.83	0.72	0.62	0.52
UA	3663.35	2062.00	4310.21	0.87	0.73	0.65	0.55	0.51
GT	2695.21	1334.09	3651.89	0.71	0.43	0.40	0.38	0.39
MO	2520.66	1549.87	2965.75	0.90	0.70	0.56	0.52	0.54
PS	3057.91	2454.00	3028.03	0.92	0.70	0.46	0.43	0.38
LA	2860.78	2108.79	2883.87	1.31	0.47	0.22	0.23	0.40
JH	5096.85	2333.36	4921.48	0.75	0.78	0.61	0.60	0.71
EN	2754.32	1678.50	3246.07	1.11	0.71	0.54	0.52	0.55
CE	3216.81	2015.61	3256.36	1.10	0.74	0.56	0.56	0.62
PO	−97.66	91.00	2210.91	−0.74	0.22	−0.44	−0.16	0.35
2-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	3737.88	2200.00	5428.87	0.87	0.42	0.11	0.25	0.44
UM	3830.68	1697.00	4541.00	1.52	0.70	0.43	0.52	0.59
UA	4586.73	2705.50	6911.65	0.98	0.71	0.52	0.51	0.52
GT	3330.98	2166.46	4945.81	0.25	0.31	0.20	0.36	0.35
MO	3004.34	2365.75	3780.38	0.81	0.56	0.26	0.33	0.44
PS	3759.05	3333.50	5381.69	0.60	0.56	0.15	0.14	0.19
LA	4128.15	3400.92	4235.16	1.37	0.16	-0.14	-0.06	0.35
JH	4986.19	2812.73	6491.23	0.49	0.68	0.44	0.47	0.61
EN	3322.70	1808.50	4459.98	1.27	0.61	0.25	0.36	0.47
CE	3920.50	2627.34	4299.01	1.28	0.66	0.33	0.47	0.60
PO	−89.38	1053.40	5021.96	−0.75	0.42	−0.26	−0.14	0.13
3-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	4971.90	3164.00	7114.77	0.88	0.60	0.27	0.25	0.36
UM	4410.38	2082.00	5849.59	2.00	0.52	0.22	0.35	0.58
UA	5644.75	3893.50	9930.94	1.20	0.70	0.48	0.43	0.51
GT	3988.10	2844.71	6407.18	−0.20	0.25	0.26	0.37	0.32
MO	3496.54	2592.72	5166.81	0.47	0.48	0.27	0.05	0.20
PS	4129.98	3890.75	9066.45	−0.03	0.52	0.09	0.06	0.06
LA	5992.51	5576.28	5790.07	0.72	0.05	−0.20	−0.18	0.28
JH	4343.70	2386.35	8874.59	0.43	0.71	0.55	0.53	0.60
EN	3951.13	2309.00	5562.79	1.32	0.60	0.28	0.21	0.35
CE	4622.23	3005.15	5569.31	1.32	0.66	0.35	0.39	0.54
PO	27.63	2232.54	8841.85	−0.87	0.58	0.01	−0.13	0.01
4-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	6594.93	4090.00	9744.80	0.70	0.73	0.36	0.26	0.26
UM	4963.63	3037.50	7521.18	2.13	0.43	0.01	0.18	0.44
UA	7021.95	4854.00	12912.43	1.37	0.72	0.44	0.41	0.48
GT	4875.40	3314.53	8662.06	-0.14	0.36	0.18	0.37	0.30
MO	3882.06	3566.27	7309.12	-0.11	0.54	0.30	-0.01	0.01
PS	4058.40	5026.75	13878.24	-0.50	0.50	0.11	0.05	0.01
LA	8273.18	7802.85	8444.13	0.06	0.16	-0.06	-0.08	0.37
JH	2769.82	16.31	12755.55	0.54	0.75	0.63	0.60	0.63
EN	4641.08	2610.50	7286.16	1.16	0.66	0.31	0.22	0.29
CE	5304.92	2759.80	7359.80	1.11	0.70	0.37	0.37	0.45
PO	-301.20	4249.03	13860.09	-1.03	0.64	0.18	0.02	0.07

Notes: The table reports summary sample statistics of forecast errors for the teams, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The table reports mean, median, standard deviation (std), skewness (skew), and autocorrelation coefficients up to order 4 (AC(1), AC(2), AC(3), AC(4)). Weekly observations from June 20, 2020 to March 20, 2021.

expected to produce forecasts that are superior to simple statistical benchmarks.

From Table 2 we can also observe that the forecast errors are autocorrelated, as documented in the columns AC(1), AC(2), AC(3) and AC(4). This happens even for one-step-ahead forecasts, where the first order sample autocorrelation may be as high as 0.83. This is interesting because, under mean squared error loss, optimal h-step ahead forecast errors should be at most MA($h-1$): so 1-step ahead forecast errors should be Martingale Differences, 2-step ahead errors should be at most MA(1), and so on. Indeed, this is the very argument given in Diebold and Mariano (1995) to justify the choice of the rectangular kernel to estimate the long-run variance. However, this condition is clearly violated by all forecasts.

One explanation of this higher order autocorrelation in the forecast errors and the fact that the forecasting teams systematically underpredict the number of fatalities could be that the forecasting teams use alternative loss functions to produce their forecasts. Indeed, Patton and Timmermann (2007) show that, under asymmetric loss and nonlinear data generating processes, forecast errors can be biased and serially correlated of arbitrarily high order.

Finally, Figure 2 shows a break in the volatility of the forecast errors across the first and the second halves of the sample (as illustrated by the vertical line in each diagram of Figures 1-2). This is also shown in Tables 3-4, where we report summary statistics for the forecast errors in the two sub-samples. In particular, we note that the volatility of the forecast errors is considerably higher in the second sub-sample. Such decline in the quality of the forecasts in the most recent sub-sample may at first be puzzling: one would expect the forecasting teams to improve their performance as more information becomes progressively available. However, this structural break in the forecasting ability of all models could in part be related to the emergence of a new strain of the virus in the end of 2020, with specific mutations in the spike protein of SARS-CoV-2 resulting in increased transmissibility. Consistent with this explanation, research from the CDC reports that the B.1.1.7 virus strain (often referred to as the "Kent" variant) is estimated to have emerged in September 2020. This variant exhibited

Table 3: Summary Statistics of Forecast Errors - First evaluation sub-sample

1-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	965.10	1060.50	1238.58	-0.59	0.61	-0.13	-0.57	-0.64
UM	1059.70	740.00	799.46	0.99	0.33	-0.09	-0.02	-0.12
UA	1149.25	718.50	2377.89	2.20	0.01	0.20	-0.03	0.01
GT	961.88	340.84	2235.86	3.32	-0.14	0.20	0.18	-0.09
MO	860.03	804.30	914.07	0.10	-0.01	0.18	0.19	-0.15
PS	1461.63	1469.50	1388.68	0.12	0.12	-0.17	0.12	0.20
LA	1583.01	1261.77	1034.43	0.68	0.21	0.13	-0.26	-0.26
JH	1314.88	1135.98	1028.01	0.21	0.60	0.16	-0.13	-0.35
EN	878.69	781.00	702.74	-0.08	0.22	0.10	0.15	-0.23
CE	1169.43	1072.08	739.99	0.24	0.24	0.33	0.20	-0.11
PO	184.05	240.70	863.85	-0.01	0.52	0.10	0.11	-0.18
2-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	1248.00	1622.00	2072.00	-0.76	0.60	0.05	-0.24	-0.39
UM	1286.20	1245.50	933.21	0.70	0.33	0.05	-0.22	-0.32
UA	535.85	-223.00	3737.17	0.23	0.23	0.32	0.01	-0.13
GT	1099.22	436.94	2391.34	2.65	-0.14	0.34	-0.01	0.08
MO	1334.24	1707.04	1769.66	-1.58	0.06	0.18	-0.16	-0.09
PS	1766.03	2284.75	2986.82	-1.13	0.25	-0.27	-0.08	0.17
LA	2873.01	2637.79	2117.38	0.18	0.41	0.11	-0.20	-0.45
JH	-24.23	-308.80	1874.64	0.38	0.68	0.32	-0.18	-0.40
EN	988.15	653.50	1227.02	0.41	0.37	0.14	-0.23	-0.33
CE	1264.79	1296.37	1069.34	0.25	0.40	0.24	-0.11	-0.15
PO	665.81	1050.20	2286.05	-0.30	0.72	0.31	0.13	-0.11
3-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	1733.15	2588.00	2487.69	-1.11	0.49	-0.04	-0.10	-0.31
UM	1543.35	1401.00	1212.55	0.03	0.15	0.16	-0.07	-0.26
UA	-307.65	-1466.00	5456.38	0.26	0.36	0.40	0.01	-0.20
GT	1179.99	869.56	2787.10	2.08	-0.07	0.45	-0.04	-0.01
MO	1761.96	2198.23	3470.14	-1.61	0.17	0.19	-0.22	-0.25
PS	1303.08	2203.00	5607.86	-1.79	0.40	-0.19	-0.18	-0.08
LA	4844.44	4477.22	3177.37	0.13	0.51	0.15	-0.25	-0.50
JH	-2536.57	-2703.05	3367.77	-0.01	0.80	0.49	0.16	-0.08
EN	1151.16	648.45	1792.83	0.71	0.50	0.25	-0.20	-0.38
CE	1190.22	948.76	1733.00	0.16	0.48	0.16	-0.15	-0.38
PO	1546.59	1622.03	4607.05	-0.36	0.81	0.46	0.20	-0.05
4-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	2495.20	3046.00	2746.48	-0.53	0.46	-0.14	-0.11	-0.24
UM	1985.65	2532.00	2120.85	-0.99	-0.09	-0.01	0.18	-0.05
UA	-862.95	-3495.00	7232.63	0.73	0.47	0.43	0.14	-0.22
GT	1346.80	293.71	3724.08	1.70	0.04	0.44	-0.02	-0.02
MO	2238.44	3443.73	5868.38	-1.70	0.29	0.22	-0.25	-0.33
PS	200.98	1934.75	9607.37	-1.97	0.48	-0.08	-0.23	-0.22
LA	6953.06	6431.77	4444.55	0.16	0.56	0.32	0.01	-0.32
JH	-6589.80	-5408.15	5803.66	-0.38	0.85	0.66	0.28	0.07
EN	1309.30	368.50	2490.15	1.14	0.52	0.37	-0.01	-0.34
CE	970.92	597.7	2736.02	0.36	0.56	0.25	-0.13	-0.56
PO	2672.81	2938.39	7805.47	-0.35	0.83	0.53	0.25	-0.04

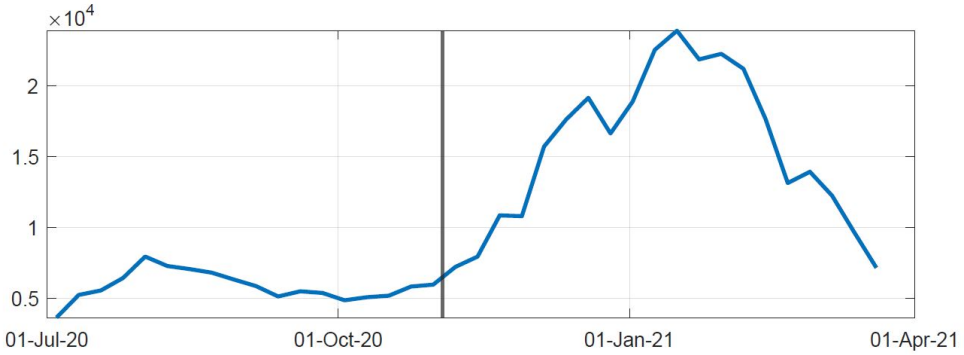
Notes: The table reports summary statistics of forecast errors for the teams, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The table reports mean, median, standard deviation (std), skewness (skew), and autocorrelation coefficients up to order 4 (AC(1), AC(2), AC(3), AC(4)). Weekly observations from June 20, 2020 to October 31, 2021.

Table 4: Summary Statistics of Forecast Errors - Second evaluation sub-sample

1-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	3986.60	4375.00	5825.61	-0.56	0.01	-0.13	0.01	0.38
UM	5668.05	5257.00	4023.67	0.22	0.71	0.50	0.28	0.04
UA	5668.05	5257.00	4023.67	0.22	0.71	0.50	0.28	0.04
GT	4428.55	4503.65	4006.28	-0.38	0.38	0.20	0.16	0.14
MO	4181.28	4291.58	3378.33	-0.13	0.58	0.30	0.24	0.27
PS	4654.20	4090.50	3395.22	0.15	0.65	0.27	0.17	0.05
LA	4138.55	3610.50	3544.55	0.43	0.33	-0.05	0.00	0.24
JH	8878.81	9467.99	4306.70	-0.21	0.43	-0.01	-0.05	0.21
EN	4629.95	4108.50	3705.17	0.05	0.56	0.25	0.22	0.27
CE	5264.19	4893.07	3520.45	0.10	0.56	0.18	0.18	0.28
PO	-379.36	-446.50	3019.97	-0.34	0.19	-0.51	-0.22	0.36
2-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	6227.75	6081.00	6569.00	-0.01	0.21	-0.19	0.05	0.34
UM	6375.15	4424.50	5275.00	0.57	0.54	0.09	0.26	0.31
UA	8637.60	7281.00	7038.68	0.63	0.63	0.15	0.20	0.21
GT	5562.74	5781.50	5831.40	-0.84	0.15	-0.12	0.22	0.15
MO	4674.44	4429.20	4508.98	0.13	0.48	-0.01	0.16	0.36
PS	5752.08	5342.25	6493.53	0.08	0.52	0.04	-0.01	0.00
LA	5383.28	4634.00	5386.77	0.77	0.01	-0.35	-0.17	0.39
JH	9996.60	10425.31	5488.58	-0.81	0.12	-0.48	-0.28	0.13
EN	5657.25	5428.00	5277.33	0.31	0.44	-0.12	0.11	0.26
CE	6576.20	5324.99	4684.47	0.40	0.42	-0.19	0.12	0.35
PO	-844.56	1196.90	6733.56	-0.33	0.37	-0.37	-0.25	0.11
3-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	8210.65	8926.50	8696.68	-0.02	0.49	0.06	0.03	0.24
UM	7277.40	5187.00	7173.65	1.06	0.36	-0.07	0.10	0.43
UA	11597.15	11219.50	9903.23	1.18	0.56	0.04	0.03	0.25
GT	6796.22	8185.65	7739.23	-1.30	0.04	0.00	0.19	0.17
MO	5231.13	4332.77	6034.96	0.33	0.50	0.15	-0.05	0.22
PS	6956.88	9382.50	10975.21	-0.38	0.49	0.03	-0.02	-0.05
LA	7140.59	7402.15	7479.55	0.30	-0.10	-0.34	-0.23	0.39
JH	11223.98	10597.45	7117.63	-0.59	0.13	-0.29	-0.27	0.17
EN	6751.10	6560.00	6618.05	0.41	0.45	-0.01	-0.12	0.15
CE	8054.25	7246.77	5988.86	0.58	0.43	-0.11	-0.03	0.34
PO	-1491.33	3231.80	11592.63	-0.44	0.52	-0.12	-0.26	-0.04
4-week ahead	Mean	Median	Std	Skew	AC(1)	AC(2)	AC(3)	AC(4)
CO	10694.65	14709.50	12328.52	-0.25	0.67	0.21	0.07	0.12
UM	7941.60	4073.50	9640.82	1.21	0.34	-0.20	0.00	0.34
UA	14906.85	12236.50	12611.04	1.62	0.56	-0.08	-0.06	0.24
GT	8404.01	9258.78	10673.80	-1.12	0.26	-0.16	0.24	0.21
MO	5525.68	4118.23	8338.64	0.14	0.63	0.26	-0.01	0.11
PS	7915.83	12412.50	16483.96	-0.70	0.45	0.06	0.01	-0.05
LA	9593.30	10939.26	11087.67	-0.26	0.09	-0.15	-0.12	0.45
JH	12129.44	11167.02	10763.97	0.02	0.34	0.01	0.05	0.30
EN	7972.85	7476.50	8910.65	0.23	0.57	0.06	-0.04	0.13
CE	9638.92	9422.56	8009.63	0.31	0.53	-0.04	0.01	0.28
PO	-3275.21	6527.39	17741.72	-0.58	0.58	0.05	-0.09	0.03

Notes: The table reports summary statistics of forecast errors for the teams, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The table reports mean, median, standard deviation (std), skewness (skew), and autocorrelation coefficients up to order 4 (AC(1), AC(2), AC(3), AC(4)). Weekly observations from November 7, 2020 to March 20, 2021.

Figure 3: Incident deaths in the US over the evaluation period



Note: Weekly observations from June 20, 2020 to March 20, 2021. The vertical line indicates November 3, 2020 and delimits our two evaluation sub-samples.

rapid growth in the US in early 2021, and was predicted to be the predominant variant by March 2021 (Galloway, Paul, MacCannell, Johansson, Brooks, MacNeil, Slayton, Tong, Silk and Armstrong 2021). This is illustrated in Figure 3 showing the incident deaths across the overall evaluation period. Thus the increased forecasting errors may be driven by the sudden heightened incident deaths and associated increased slope of the cumulative deaths curve.

At any rate, the volatility of the forecasting errors increases markedly starting from the beginning of November 2020. We thus perform our forecast evaluation separately on two equally-sized sub-samples: the first evaluation sub-sample (from June 20, 2020 to October 31, 2020), and the second evaluation sub-sample (from November 7, 2020 to March 20, 2021). This means that for each evaluation sub-sample we base our inference on just 20 observations. With such small sample sizes, fixed-smoothing asymptotics is crucial to obtain correctly sized tests.

4 Forecast Evaluation Results

Our main results for the test of equal predictive ability of each forecasting team vis-à-vis the benchmark model (4) are reported in Table 5.

We conduct the analysis separately for the two evaluation sub-samples identified in Figure 2.

This yields 20 observations for each sub-sample, underlying the importance of alternative asymptotics in evaluating predictive ability.[5] In the baseline analysis, we evaluate forecast errors relying on the quadratic loss function. We present the test statistics using both the weighted covariance estimator with Bartlett kernel (WCE) and the weighted periodogram estimator with Daniell kernel (WPE) of the long-run variance. A positive value for the test statistic indicates that the forecast in question is more accurate than the benchmark.

We report two-sided significance at the 5% and 10% levels, using fixed smoothing asymptotics (fixed-b for WCE and fixed-m for WPE) to establish the critical values. In particular, for $T = 20$ and the bandwidths recommendations in Coroneo and Iacone (2020), the critical values are ± 2.57 and ± 2.09 with fixed-b asymptotics and ± 2.78 and ± 2.13 with fixed-m asymptotics. For comparison, we also report significance based on bootstrap critical values, constructed using the overlapping stationary block-bootstrap of Politis and Romano (1994) using an average block length of $T^{1/4} \approx 2$ and a circular scheme, as described in Coroneo and Iacone (2020).

We consider first the upper panel of Table 5, which reports results for the first sub-sample from June 20, 2020 to October 31, 2020. Results indicate that no forecasting scheme predicts better than the benchmark at the 1-week forecasting horizon. In fact, we find that the benchmark often significantly outperforms the forecasting teams. On the other hand, at the 2-week horizon the sign of some test statistics turns from negative to positive, reflecting a smaller relative loss by the forecasting teams. The relative performance of the forecasters improves further at longer horizons (3 and 4 weeks ahead), and we observe statistically significant relative gains in performance for some forecasting teams and the ensemble forecasts.

The MAEs for the first sub-sample reported in Table 6 indicate that, at 3- and 4-weeks ahead, both ensemble forecasts (the EN ensemble provided by the CDC and the CE core ensemble constructed combining all the forecasts of the teams in Table 1) performed better than the

[5] In Appendix C, we conduct the test of equal predictive ability on the full sample.

Table 5: Tests for Equal Predictive Ability

First evaluation sub-sample: Jun 20, 2020 – Oct 31, 2020

	1-week ahead		2-week ahead		3-week ahead		4-week ahead	
	WCE	WPE	WCE	WPE	WCE	WPE	WCE	WPE
CO	-2.829**	-2.063	-0.340	-0.294	1.452	1.254	2.147*	1.812
UM	-2.231*	-2.362*	1.663	2.237*	2.939**	2.782**	2.953**	2.636*
UA	-1.993	-1.867	-1.378	-2.157*	-0.510	-1.636	0.689	0.791
GT	-1.149	-0.954	1.204	1.040	3.619**	3.147**	4.057**	3.579**
MO	-2.248*	-1.860	-0.645	-0.879	1.412	1.317	1.587	1.479
PS	-4.815**	-3.758**	-3.018**	-2.801**	-0.523	-0.458	0.086	0.073
LA	-3.527**	-3.292**	-1.862	-1.509	-0.925	-0.746	-0.241	-0.199
JH	-2.646**	-2.280*	1.194	1.119	0.714	0.544	-0.307	-0.243
EN	-1.840	-1.701	1.881	1.911	3.074**	2.798**	3.445**	3.014**
CE	-2.992**	-2.521*	1.381	1.381	2.937**	2.789**	3.472**	3.044**

Second evaluation sub-sample: Nov 7, 2020 – Mar 20, 2021

	1-week ahead		2-week ahead		3-week ahead		4-week ahead	
	WCE	WPE	WCE	WPE	WCE	WPE	WCE	WPE
CO	-3.985**	-3.235**	-1.163	-0.972	0.055	0.048	0.322	0.269
UM	-2.704**	-2.186*	-0.747	-0.625	1.732	1.493	2.450*	2.097
UA	-2.845**	-2.279*	-1.875	-1.563	-0.765	-0.664	0.061	0.053
GT	-3.084**	-2.501*	-1.833	-1.797	0.546	0.572	0.995	0.930
MO	-2.504*	-2.016	0.183	0.157	1.674	1.532	2.073	1.890
PS	-2.641**	-2.207*	-0.900	-0.813	-0.643	-0.583	-0.413	-0.366
LA	-2.327*	-1.845	-0.211	-0.199	0.754	0.694	0.730	0.656
JH	-6.675**	-6.429**	-5.431**	-6.656**	-0.884	-0.775	0.357	0.306
EN	-2.756**	-2.212*	-0.639	-0.562	1.102	1.023	1.486	1.342
CE	-3.249**	-2.616*	-1.036*	-0.902	0.709	0.641	1.221	1.066

Note: test statistics for the test of equal predictive accuracy using the weighted covariance estimator (WCE) and the weighted periodogram estimator (WPE) of the long-run variance. The benchmark is a second degree polynomial fitted on a rolling window of 5 observations. The forecast errors are evaluated using the absolute value loss function, and a positive value of the test statistic indicates lower loss for the forecaster (i.e. better performance of the forecaster relative to the polynomial model). ** and * indicate, respectively, two-sided significance at the 5% and 10% level using fixed-b asymptotics for WCE and fixed-m asymptotics for WPE. ▓ and ▒ indicate, respectively, two-sided significance at the 5% and 10% level using the bootstrap. Bootstrap critical values are constructed using the overlapping stationary block-bootstrap of Politis and Romano (1994) using an average block length of $T^{1/4} \approx 2$ and a circular scheme, as described in Coroneo and Iacone (2020).

Table 6: MAE across sub-samples

MAE

	1st sub-sample				**2nd sub-sample**			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	1304	1979	2486	3045	6013	7347	9740	13744
UM	1063	1330	1700	2559	5681	6399	7354	8640
UA	1544	2482	4118	5870	6201	8821	11790	14907
GT	1144	1430	1847	2512	5009	6846	9013	11601
MO	1034	1957	3221	5026	4516	5300	6182	7797
PS	1671	2827	4357	6367	4879	7036	11414	16466
LA	1583	3003	4983	7048	4260	5739	8275	12443
JH	1372	1431	3318	7016	8879	10453	12004	13382
EN	945	1168	1535	1899	4943	6274	7518	9422
CE	1169	1340	1636	2204	5365	6752	8318	10137
PO	654	1830	3848	6541	2415	5515	9896	15132

Relative MAE

	1st sub-sample				**2nd sub-sample**			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	1.99	1.08	0.65	0.47	2.49	1.33	0.98	0.91
UM	1.63	0.73	0.44	0.39	2.35	1.16	0.74	0.57
UA	2.36	1.36	1.07	0.90	2.57	1.60	1.19	0.99
GT	1.75	0.78	0.48	0.38	2.07	1.24	0.91	0.77
MO	1.58	1.07	0.84	0.77	1.87	0.96	0.62	0.52
PS	2.56	1.54	1.13	0.97	2.02	1.28	1.15	1.09
LA	2.42	1.64	1.29	1.08	1.76	1.04	0.84	0.82
JH	2.10	0.78	0.86	1.07	3.68	1.90	1.21	0.88
EN	1.44	0.64	0.40	0.29	2.05	1.14	0.76	0.62
CE	1.79	0.73	0.43	0.34	2.22	1.22	0.84	0.67
PO	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: The table reports the MAE of forecast errors for each team, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The top panel shows the MAE level, and the bottom panel shows the MAE relative to the MAE of the benchmark model. The first evaluation sub-sample is from June 20, 2020 to October 31, 2020, and the second evaluation sub-sample from November 7, 2020 to March 20, 2021.

individual forecasting teams. This finding is consistent with the consensus in the literature about the advantages of forecast combination (see Stock and Watson 1998, Timmermann 2006).

Turning to the second sub-sample from November 7, 2020 to March 20, 2021, we can see from Table 5 that the benchmark still significantly outperforms some teams and the ensemble forecasts at the shortest horizon. However, in this sub-sample the forecasting teams and the ensembles fail to significantly outperform the benchmark even at the longer horizons. The

MAEs for the second sub-sample reported in Table 6 indicate that, also in this sub-sample, at 3- and 4-week horizons, the two ensembles performed better than most of the individual teams.

Finally, notice that the performance of the Ensemble forecast published by the CDC is similar to the one of the Core Ensemble constructed combining all the forecasts of the teams in Table 1. However, the MAEs for the Ensemble are in all cases smaller than the ones for the Core Ensemble, indicating that combining a larger set of forecasts than the ones considered in Table 1 can provide some benefits in terms of predictive ability, albeit small.[6]

Results are overall similar regardless of the type of estimator of the long-run variance. We also notice that findings from the bootstrap are largely the same, and confirm that fixed-smoothing asymptotics is a suitable and computationally much less time-consuming alternative to bootstrapping, as also found in Coroneo and Iacone (2020) and Gonçalves and Vogelsang (2011). Moreover, by using fixed-smoothing asymptotics, we have known critical values for each test, given the sample size and choice of bandwidth.

4.1 Alternative Loss Functions

The absolute value loss function that we use in the baseline forecast evaluation reported in Table 5 is a common choice in forecast evaluation. In particular, the null hypothesis is the equality of the mean absolute prediction error. However, in relation to predicting the spread of COVID-19 (and, more generally, predicting the spread of an epidemic), the cost of under-predicting the spread of the disease can be greater than the cost of over-predicting it. Similarly, scale effects are important, since the same forecast error may be more costly for public health policy interventions when the number of fatalities is small compared to when it is large. For these reasons, in this section we consider alternative loss functions.[7]

[6]The equal predictive ability of the Ensemble forecast relative to the forecasting teams and the Core Ensemble is formally tested in Appendix E.

[7]The teams submitting forecasts to the CDC were advised that their point forecasts would be evaluated with the mean absolute error loss function. The predictive median minimizes the mean absolute error and

The DM test can be applied directly to alternative loss functions. Thus, we consider three alternative loss functions that provide alternative criteria for forecast comparison. Denoting e_t as the forecast error (thus abbreviating in this way $e^{(i)}_{t|t-h}$), the alternative loss functions considered are the following:

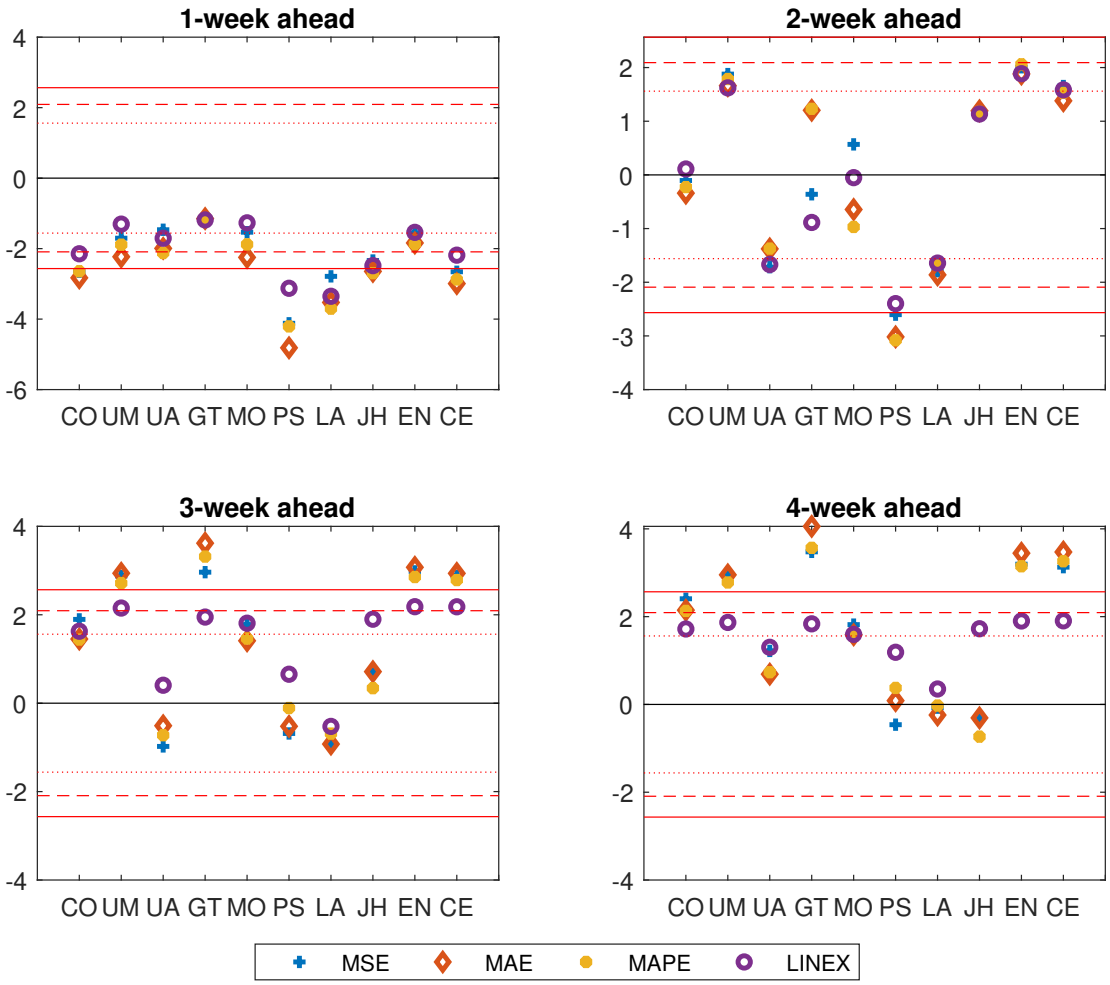
- Quadratic: $L(e_t) = (e_t)^2$;
- Absolute Percentage: $L(e_t) = |e_t|/(y_t - y_{t-1})$;
- Linex: $L(e_t) = \exp\left(e_t/(y_t - y_{t-1})\right) - e_t/(y_t - y_{t-1}) - 1$.

The quadratic loss function is a popular measure that penalises more large forecast errors: in this case, it seems natural to interpret it as giving more weight to fatalities that happen when the epidemic is less predictable. The absolute percentage loss considers the scale of the number of fatalities occurring in the period, thus allowing to evaluate differently the same forecast error when only a few fatalities occur, as opposed to when there is a large number of fatalities. Finally, with the linear exponential (linex) loss function we impose asymmetric weights, with more penalty given to under-prediction than to over-prediction. This reflects the fact that the social cost of the two errors, under- and over-prediction, are different, as the cost of not responding to the pandemic and incurring in a large loss of lives in the future is often regarded to be much higher than the economic and social cost of responding too quickly, imposing a lockdown when it is not necessary (on the precautionary principle in public health see Goldstein 2001).

The findings are summarized in Figures 4 and 5 for the first evaluation sub-sample, and in Figures 6 and 7 for the second evaluation sub-sample (the results for the absolute value loss function are also included, to facilitate the comparison). The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels, which are,

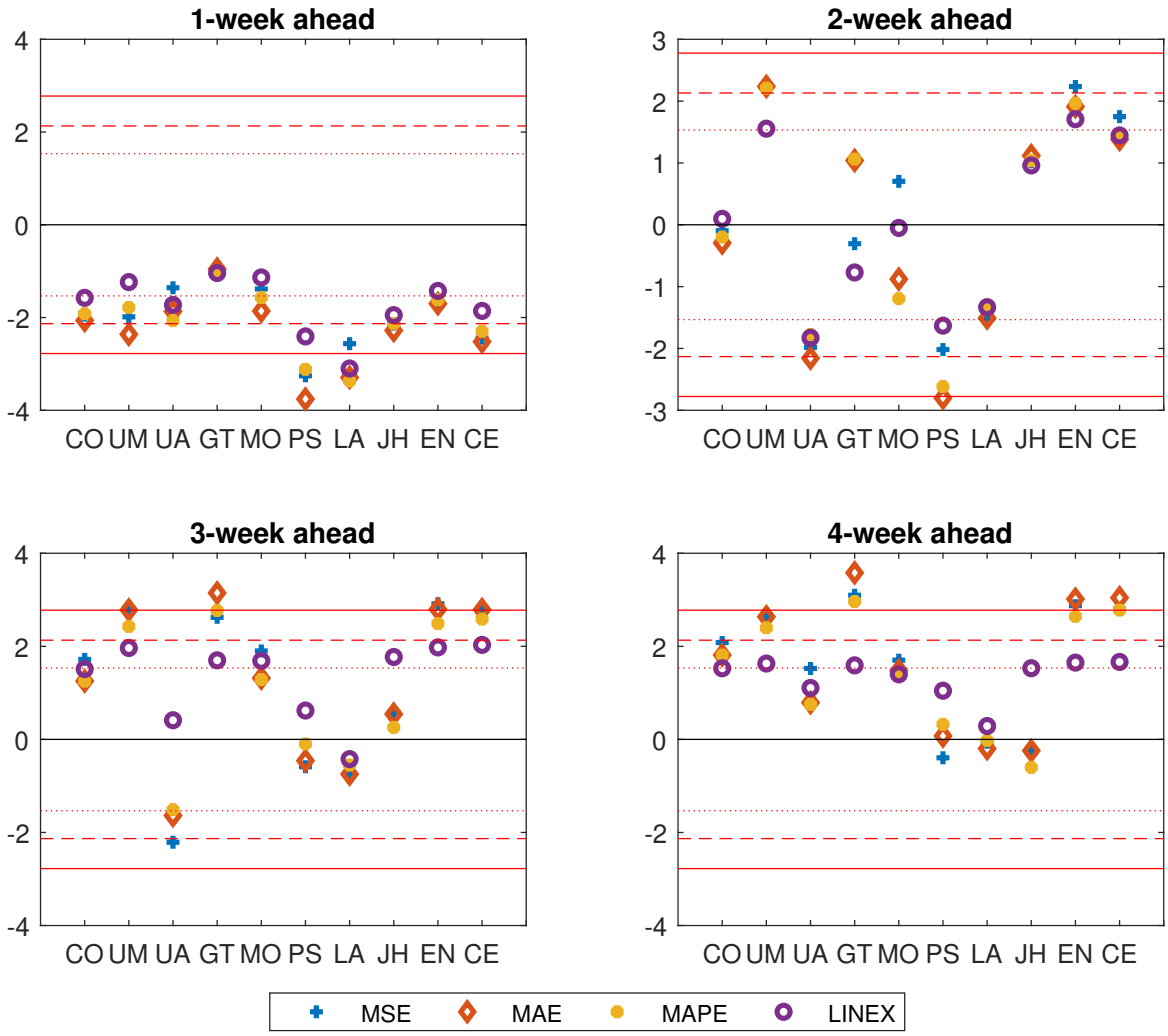
should, therefore, correspond to the optimal point forecast. However, if forecasters put greater weight to underprediction and, thus, seek to minimize a Linex type loss function, Christoffersen and Diebold (1997) show that such loss function implies an optimal point forecast that is a weighted sum of the mean and variance.

Figure 4: Forecast evaluation with WCE - First evaluation sub-sample



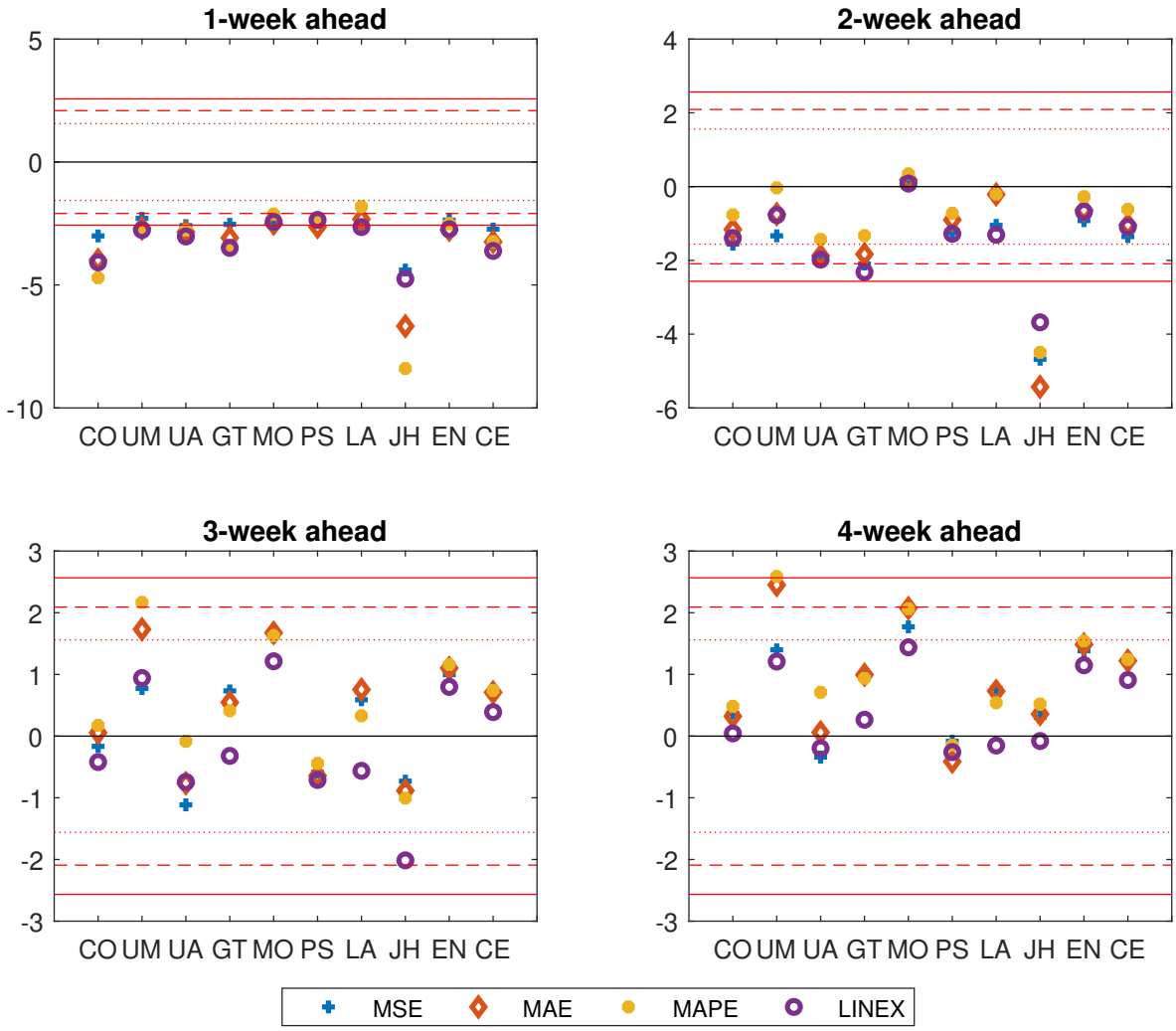
This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to October 31, 2020.

Figure 5: Forecast evaluation with WPE - First evaluation sub-sample



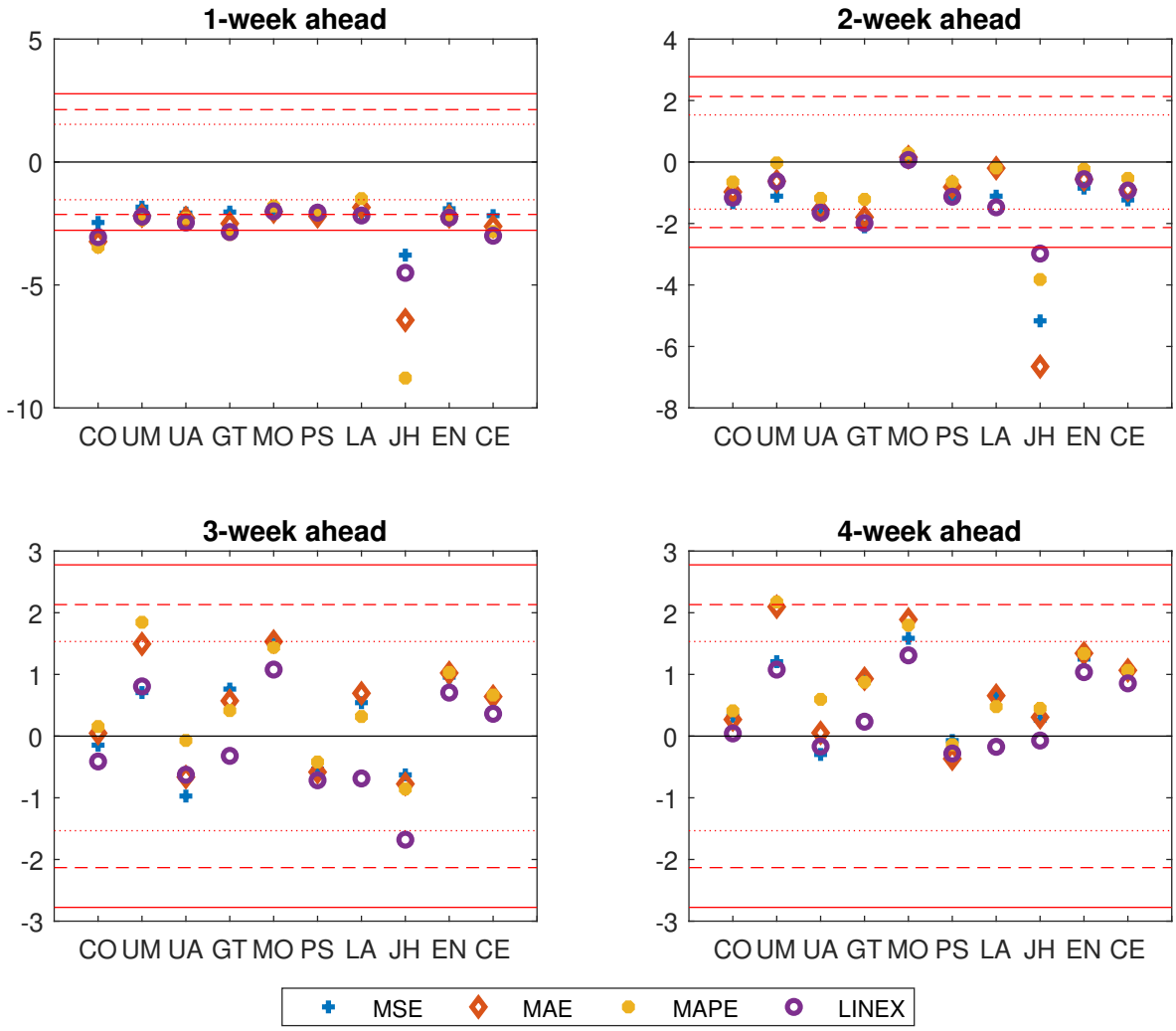
This figure reports the test statistic for the test of equal predictive accuracy using the weighted periodogram estimator (WPE) of the long-run variance and fixed-m asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to October 31, 2020.

Figure 6: Forecast evaluation WCE - Second evaluation sub-sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is November 7, 2020 to March 20, 2021.

Figure 7: Forecast evaluation WPE - Second evaluation sub-sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted periodogram estimator (WPE) of the long-run variance and fixed-m asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is November 7, 2020 to March 20, 2021.

respectively, ±1.56, ±2.09 and ±2.57 for fixed-b asymptotics and ±1.53, ±2.13 and ±2.78 for fixed-m asymptotics.

First of all, we can observe how the results are not substantially different adopting a WCE or WPE estimator, as already documented in Table 5. Figures 4-7 also show that changing the loss function to quadratic or absolute percentage does not have great impact on the evaluation of predictive ability. On the other hand, the results are different if the linex function is used, as in this case for forecast horizons larger than 1-week ahead the null hypothesis is almost never rejected at the 5% significance level.

Considering the forecast horizon, it is clear that the simple polynomial benchmark outperforms all the teams, sometimes significantly so at the 1-week horizon, and often at the 2-week horizon. However, as the forecasting horizon is moved to three and four weeks, the teams improve their performance with respect to the polynomial benchmark. In the first evaluation sub-sample, the Georgia Institute of Technology, Deep Out-break Project (GT) and the University of Massachusetts, Amhers (UM) teams, and also the Ensemble and the Core Ensemble outperform the benchmark at almost any level of statistical significance when the quadratic and the absolute percentage loss functions are used.

In the second evaluation sub-sample, when we use the quadratic and the absolute percentage loss functions, we still document more accurate predictions for several teams, for example for the University of Massachusetts, Amhers (UM), for the Northeastern University, Laboratory for the Modeling of Biological and Socio-technical Systems (MO) and for the ensembles, although these findings are seldom statistically significant. On the other hand, neither the forecasting teams nor the ensemble forecasts outperform the benchmark significantly, if the linex loss function is used. This seems to be mainly due to the fact that most forecasting teams (and the ensembles) under-predicted the fatalities, and this is more penalized with this loss function. Our empirical findings may also be viewed as offering support for the results discussed by Elliott and Timmermann (2004), who show how the equal weights ensemble is

less appropriate in the presence of asymmetries in the loss function and in the distribution of the errors. Notice, however, the teams submitting forecasts to the CDC were advised that their point forecasts would be evaluated with the absolute value loss function, and therefore it is fair to conjecture that their predictions do not optimize the quadratic, the absolute percentage or the linex loss functions. Had the teams been told that an alternative loss function was to be used to evaluate the forecast accuracy, then their point predictions might have been different. For example, if the linex loss function was used, the predictions would in general be larger.

In general, we conclude that the ensemble forecasts deliver some of the best performing predictions. They often achieve statistically significant outperformance against the benchmark. This is the case for the 3- and 4-week ahead predictions during the first evaluation sub-sample, when losses are evaluated using the absolute value, quadratic or absolute percentage loss functions. Even when the outperformance is not significant, ensemble predictions perform relatively well, in the sense of not underperforming the benchmark, even during the second evaluation sub-sample when the forecast errors are larger. Between the two ensemble forecasts, the wider ensemble obtained by the CDC performs slightly better against the benchmark compared to the Core Ensemble, illustrating once again the gains from combining a large number of predictions.

4.2 Comparing the two evaluation sub-samples

Comparing the results of the tests for equal predictive ability across the two sub-samples, we note that the null hypothesis is more difficult to reject during the second evaluation sub-sample. In particular, whilst during the first sub-sample (Figures 4 and 5), many forecasting teams and the ensemble forecasts outperform the benchmark at the 3- and 4-week horizons, this is no longer true in the second sub-sample (Figures 6 and 7).

Heightened incident deaths, associated with the increased transmissibility of the new strains of

the virus that emerged in late 2020 and in 2021, may have affected the statistical performance of the tests of equal predictive ability in two ways: by making the task of forecasting more difficult (as evident by the larger MAEs in the second sub-sample in Table 6) and by inflating the long run variance of the test statistic (thus reducing the power to detect a significant difference for given MAE differential). Of course, the purpose of the tests of equal predictive ability is not to compare predictive ability across different periods, so a more pronounced failure to reject the null hypothesis during the second evaluation sub-sample is not evidence that the models were less valuable in this period.

To examine further the reasons for the apparent change in the forecastability of the epidemic, in the bottom panel of Table 6 we report the ratio of the MAE of each forecasting team and the MAE of the benchmark. This enables us to compare the relative performance of each forecasting team and the benchmark across the two evaluation sub-samples. Considering for example the 4-week forecasting horizon, we can notice that for some forecasting teams and the ensembles the ratios are considerably smaller for the first evaluation sub-sample compared to the second evaluation sub-sample. This is true for all the loss functions considered (MAE, RMSE, MAPE, and MLinex), and suggests that the forecasting environment was different across the first and the second evaluation sub-samples.[8]

4.3 Additional experiments

In the Appendix, we consider several additional experiments and empirical exercises. In particular, in Appendix C, we perform the tests of equal predictive ability for the full sample, instead of considering each evaluation sub-sample separately. In Appendix D, we use an alternative benchmark model, obtained based on fitting an AR(1) model to the log incidence of weekly deaths. Finally, in Appendix E, we use the CDC ensemble forecast as the benchmark model, to test formally the null of equal predictive ability of the forecasting teams and the

[8] In Appendix B we report RMSE, MAPE and MLinex of the forecast errors across the two different evaluation sub-samples.

ensemble forecast.

5 Conclusion

We evaluate the relative predictive accuracy of real-time forecasts for the number of COVID-19 fatalities in the US, produced by several competing forecasting teams for two evaluation periods: June 20, 2020 to October 31, 2020 (first evaluation sub-sample) and November 7, 2020 to March 20, 2021 (second evaluation sub-sample). Ensemble forecasts, that combine all available forecasts using an equal weights scheme are also included. Since sample sizes are small, we use fixed-smoothing asymptotics for the limit distribution of the scaled expected loss differential between two competing forecasts. We find that none of the forecasting teams outperform a simple statistical benchmark at the 1-week horizon; however, at longer forecasting horizons some teams show superior predictive ability.

The ensemble forecasts deliver some of the most competitive predictions. Whilst they do not yield the best forecasts overall, they are competitive in the sense of delivering predictions that significantly outperform the benchmark at longer horizons during the first evaluation sub-sample, and also never performing statistically worse than the benchmark even in the second evaluation sub-sample. In this sense, the Ensemble forecast may be seen as a robust choice. We also document that the broad based Ensemble published by the CDC is more accurate than the Core Ensemble, that only pools forecasts from the teams that we include in our exercise.

Overall, our results indicate that forecasts of the COVID-19 epidemic are valuable but need to be used with caution, and decision-makers should not rely on a single forecasting team (or a small set) to predict the evolution of the pandemic, but should hold a large and diverse portfolio of forecasts.

A natural extension of our analysis is to evaluate the interval forecasts with different level of

coverage submitted by the forecasting teams to the CDC. This would require choosing an appropriate loss function, for example the weighted interval score (see Bracher, Ray, Gneiting and Reich 2021), and applying the same alternative asymptotics we have used here. In particular, recent work by Coroneo, Iacone and Profumo (2019) shows that fixed-smoothing asymptotics may also be employed successfully to evaluate density forecasts.

Another interesting extension to the current work is to consider the predictive accuracy for a panel data of forecasts (since the forecasting teams predict not only the national spread of the disease, but also the regional evolution of the epidemic). Timmermann and Zhu (2019) propose methods for testing predictive accuracy for panel forecasts. In particular, they develop a panel-data Diebold-Mariano test for equal predictive accuracy, that pools information across both the time-series and cross-sectional dimensions. Our analysis could, therefore, be extended to the evaluation of a panel of regional predictions.

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A Monte Carlo study

In this appendix, we evaluate the size and power properties of the tests that we use in our empirical study. We simulate forecast errors as in Diebold and Mariano (1995), Clark (1999) and Coroneo and Iacone (2020). In particular, we first simulate a vector of forecast innovations from a bivariate standard normal, $(v_{1t}, v_{2t})' \sim N(0_2, I_2)$. We then introduce contemporaneous correlation by taking

$$\begin{pmatrix} u_{1t} \\ u_{2t} \end{pmatrix} = \begin{pmatrix} \sqrt{k} & 0 \\ \rho & \sqrt{1-\rho^2} \end{pmatrix} \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix},$$

and serial correlation by taking

$$\begin{aligned} e_{1t} &= \frac{\sum_{j=0}^{q} \theta^j u_{1t-j}}{\sqrt{\sum_{j=0}^{q} \theta^{2j}}} \\ e_{2t} &= \frac{\sum_{j=0}^{q} \theta^j u_{2t-j}}{\sqrt{\sum_{j=0}^{q} \theta^{2j}}}. \end{aligned}$$

We use both an absolute value loss function, i.e. $d_t = |e_{1t}| - |e_{2t}|$ and a quadratic loss function, i.e. $d_t = e_{1t}^2 - e_{2t}^2$. We compute the test statistics with WCE estimates of the long run variance, and use critical values both from standard asymptotics and from the application of fixed smoothing asymptotics. The expected loss differential is zero for $k = 1$, so we use $k = 1$ to evaluate size and $k = 1 + c/\sqrt{T}$ for $c = 1, \ldots, 30$ to evaluate power. We use $T = 20$ as this sample matches the dimension of our two samples in the empirical analysis. We also use $T = 40$ to assess the consequences on size and power of the decision to split the sample to address the instability in the second part. In all cases, we use 10,000 replications, $\rho = 0.5$ and $q = 5$, and investigate the effect of autocorrelation by using different values of θ.

Size results are in Table A1, and power results are in Figures A1 and A2 for the absolute value loss, and in Figures A3 and A4 for the quadratic loss. Several interesting results can be

Table A1: Size of tests with standard vs. fixed-smoothing asymptotics

Absolute value loss function

	T=20				T=40			
θ	-0.5	0	0.5	0.75	-0.5	0	0.5	0.75
WCE Standard	0.143	0.113	0.142	0.192	0.118	0.096	0.115	0.141
WCE Fixed-b	0.062	0.050	0.065	0.097	0.062	0.047	0.057	0.078
WPE Standard	0.127	0.119	0.127	0.144	0.101	0.094	0.097	0.110
WPE Fixed-m	0.050	0.048	0.052	0.057	0.053	0.049	0.049	0.054

Quadratic loss function

	T=20				T=40			
θ	-0.5	0	0.5	0.75	-0.5	0	0.5	0.75
WCE Standard	0.133	0.112	0.134	0.175	0.114	0.098	0.109	0.133
WCE Fixed-b	0.051	0.044	0.051	0.072	0.051	0.046	0.049	0.064
WPE Standard	0.112	0.110	0.115	0.126	0.095	0.096	0.091	0.098
WPE Fixed-m	0.040	0.041	0.039	0.041	0.043	0.048	0.043	0.043

Note: empirical rejection frequencies for tests of equal predictive ability at 5% nominal size for various MA(5) processes with different values of θ. The top panel refers to tests that use an absolute value loss function, and the bottom panel to tests that use a quadratic loss function. The table reports WCE estimate of the long run variance and with critical values from standard or fixed-b asymptotics, and tests that use WPE estimate of the long run variance and with critical values from standard or fixed-m asymptotics.

observed:

- Tests with standard asymptotics are severely oversized, even in the sample with 40 observations and no autocorrelation. In addition, as the sample size decreases and the autocorrelation increases, the size properties deteriorate even further.

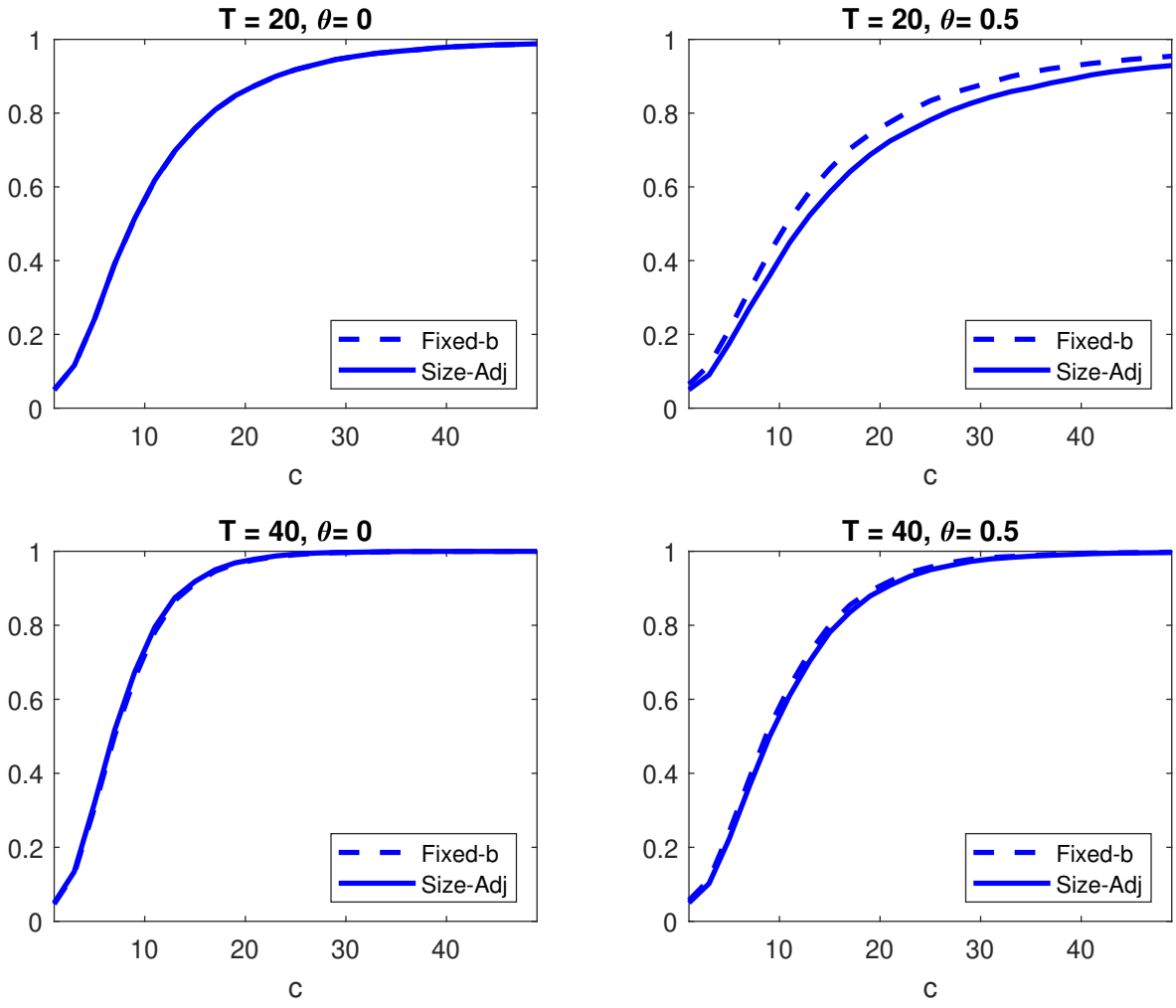
- Both versions of the tests with fixed-smoothing asymptotics have good size properties even in the extremely small, $T = 20$ sample. However, the test with WCE may still be oversized in cases of relatively large dependence ($\theta = 0.75$) and when the sample is as short as the ones in our empirical study, especially when the absolute value loss function is used.

- Both tests with fixed-smoothing asymptotics achieve power close to the unfeasible

size-adjusted power when the absolute value loss function is used. When the quadratic loss is used, the WPE with fixed-smoothing asymptotics has slightly less power that the size-adjusted reference in the smallest, $T = 20$ sample.

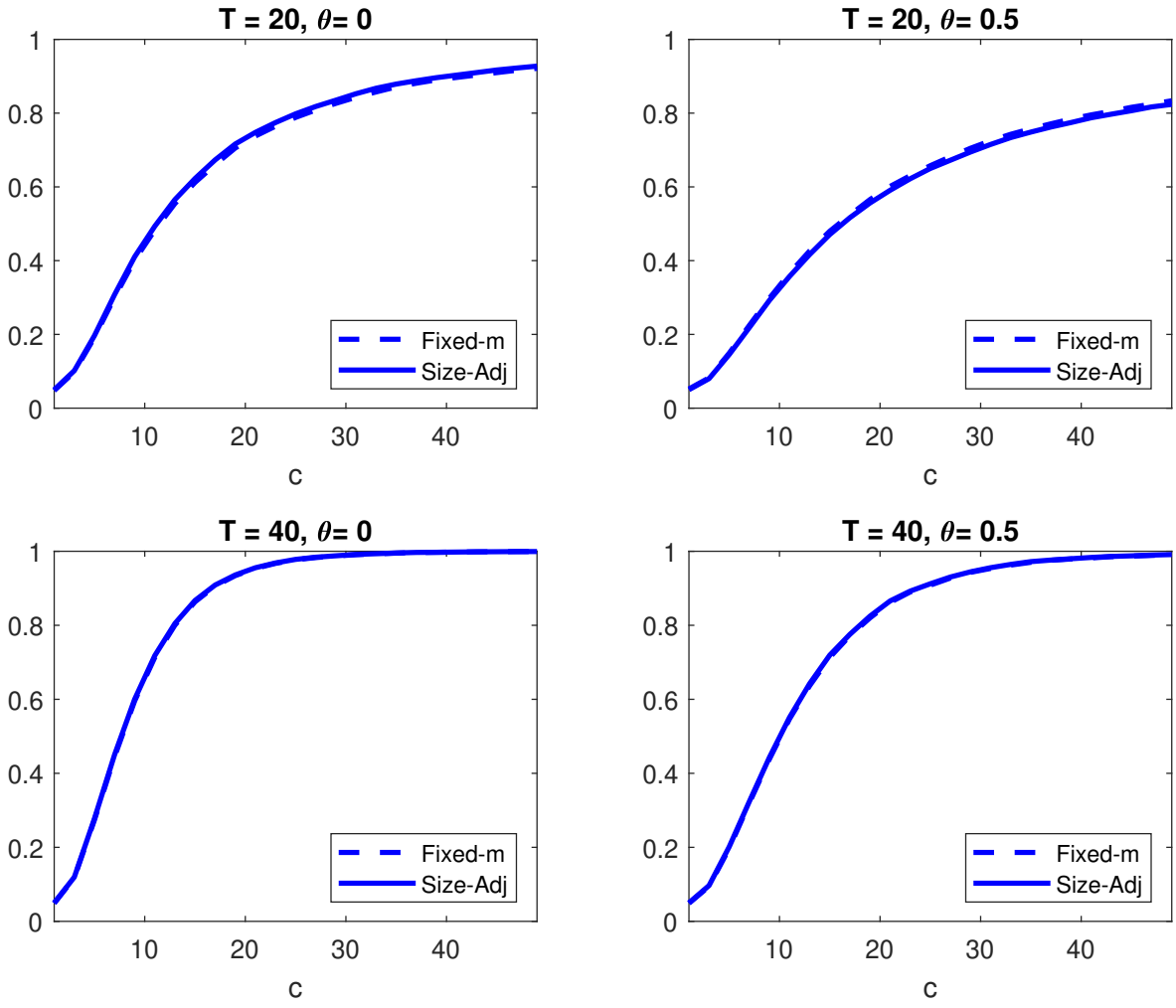
- The WCE test has more power than the WPE test, especially in the smallest sample.

Figure A1: Finite sample local power with WCE and absolute value loss



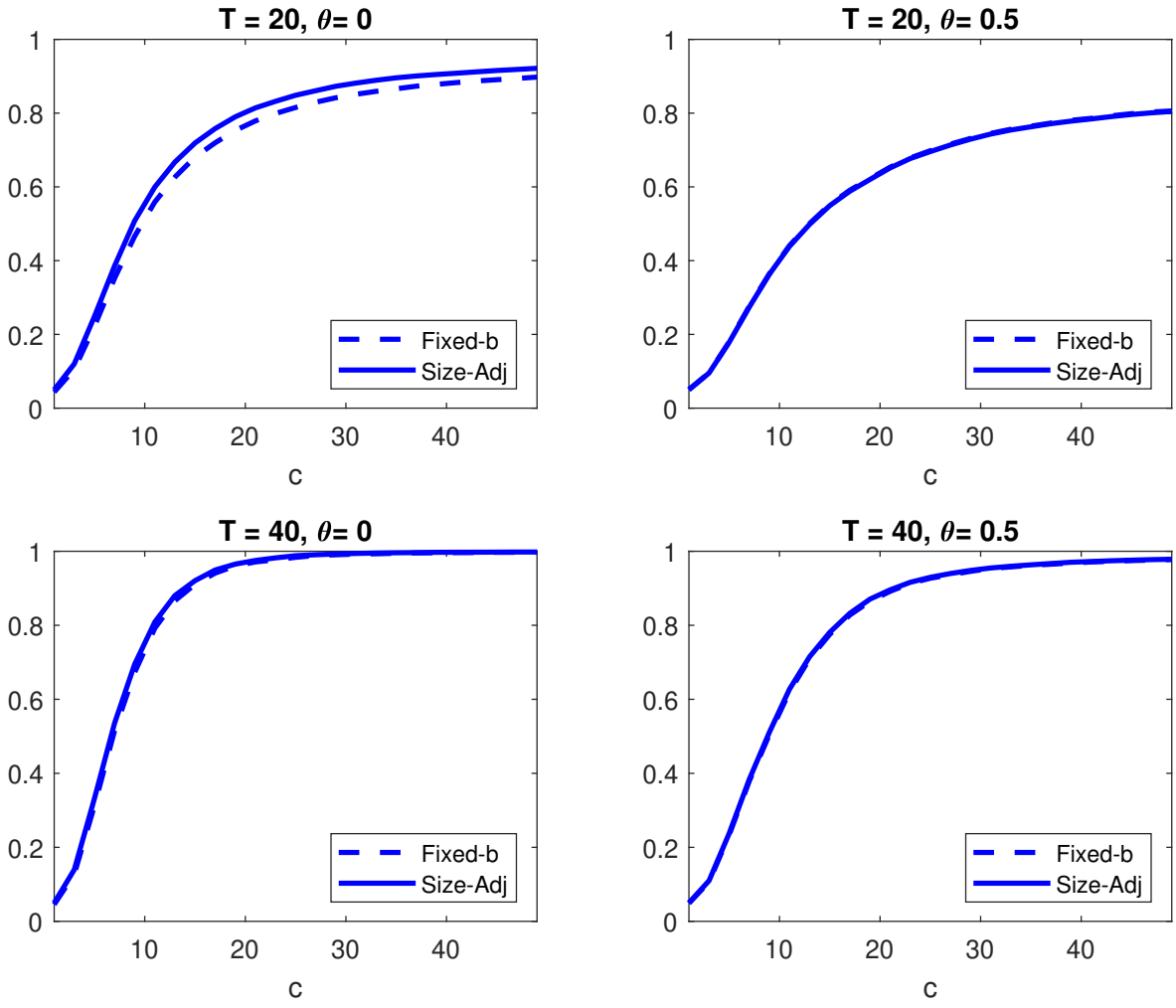
Note: The figure displays empirical rejection frequencies at 5% nominal size for deviations from the null by $c/\sqrt{T}$. Size-Adjusted refers to the case in which simulated size-adjusted critical values are used. The long run variance is estimated using the WCE with Bartlett kernel. Results are reported for sample size of 20 observations (top plots) and 40 observations (bottom plots), and for no autocorrelation (left plots) and $\theta = 0.5$ (right plots).

Figure A2: Finite sample local power with WPE and absolute value loss



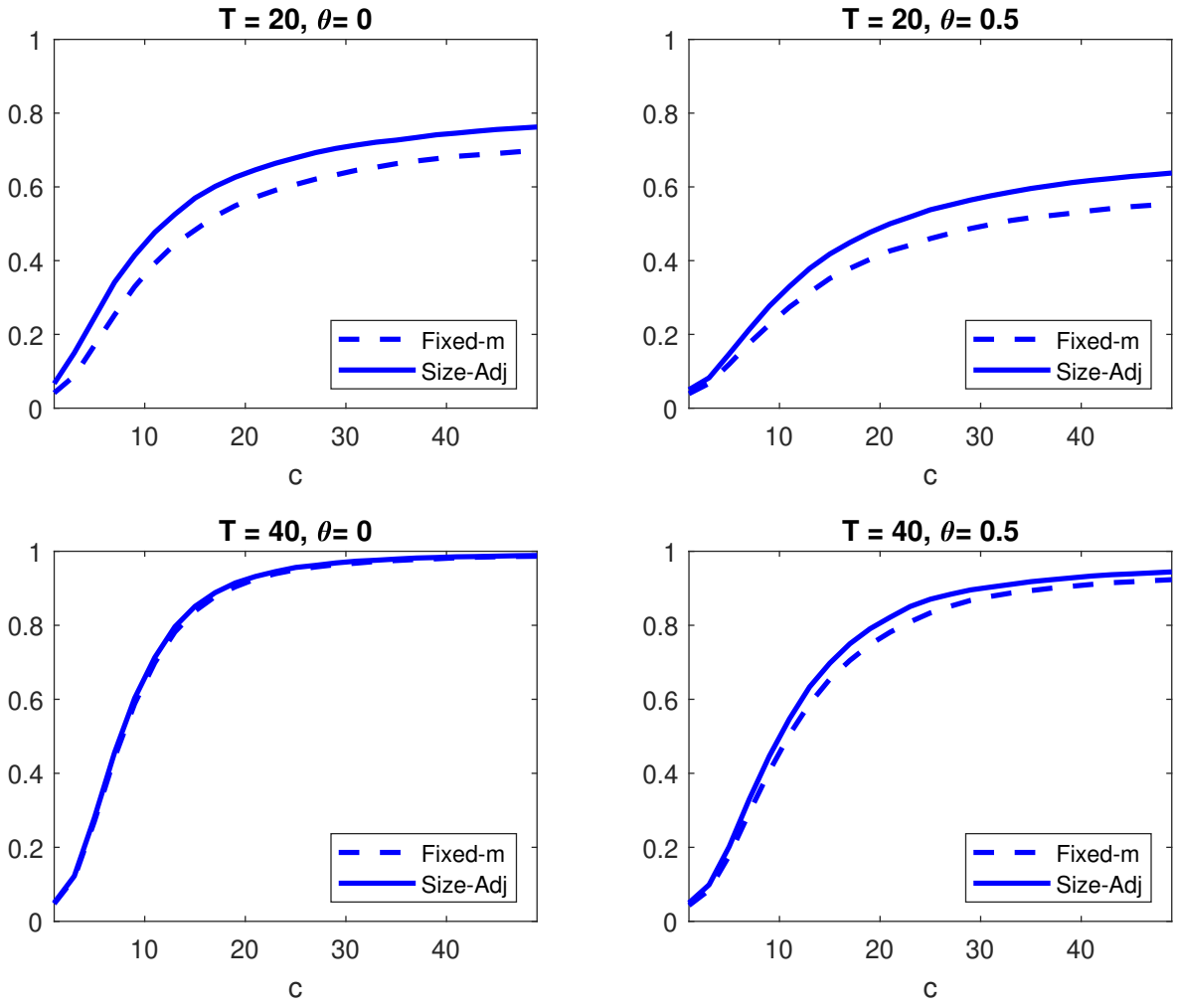
Note: The figure displays empirical rejection frequencies at 5% nominal size for deviations from the null by $c/\sqrt{T}$. Size-Adjusted refers to the case in which simulated size-adjusted critical values are used. The long run variance is estimated using the WPE with Daniell kernel. Results are reported for sample size of 20 observations (top plots) and 40 observations (bottom plots), and for no autocorrelation (left plots) and $\theta = 0.5$ (right plots).

Figure A3: Finite sample local power with WCE and quadratic loss



Note: The figure displays empirical rejection frequencies at 5% nominal size for deviations from the null by $c/\sqrt{T}$. Size-Adjusted refers to the case in which simulated size-adjusted critical values are used. The long run variance is estimated using the WCE with Bartlett kernel. Results are reported for sample size of 20 observations (top plots) and 40 observations (bottom plots), and for no autocorrelation (left plots) and $\theta = 0.5$ (right plots).

Figure A4: Finite sample local power with WPE and quadratic loss



Note: The figure displays empirical rejection frequencies at 5% nominal size for deviations from the null by $c/\sqrt{T}$. Size-Adjusted refers to the case in which simulated size-adjusted critical values are used. The long run variance is estimated using the WPE with Daniell kernel. Results are reported for sample size of 20 observations (top plots) and 40 observations (bottom plots), and for no autocorrelation (left plots) and $\theta = 0.5$ (right plots).

B Comparing forecast errors across sub-samples

Table B2: RMSE across sub-samples

	RMSE							
	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	1546	2374	2980	3660	6938	8932	11801	16086
UM	1315	1575	1944	2866	6893	8190	10092	12303
UA	2587	3682	5327	7102	7508	11031	15089	19321
GT	2382	2577	2962	3872	5904	7953	10153	13374
MO	1238	2181	3814	6142	5322	6416	7872	9828
PS	1992	3405	5619	9366	5711	8552	12761	17911
LA	1877	3537	5750	8192	5391	7520	10205	14451
JH	1653	1827	4148	8685	9821	11338	13195	16037
EN	1114	1551	2093	2758	5872	7646	9337	11790
CE	1374	1639	2066	2838	6284	8006	9947	12404
PO	862	2326	4749	8064	2968	6617	11397	17600
	Relative RMSE							
	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	1.79	1.02	0.63	0.45	2.34	1.35	1.04	0.91
UM	1.53	0.68	0.41	0.36	2.32	1.24	0.89	0.70
UA	3.00	1.58	1.12	0.88	2.53	1.67	1.32	1.10
GT	2.76	1.11	0.62	0.48	1.99	1.20	0.89	0.76
MO	1.44	0.94	0.80	0.76	1.79	0.97	0.69	0.56
PS	2.31	1.46	1.18	1.16	1.92	1.29	1.12	1.02
LA	2.18	1.52	1.21	1.02	1.82	1.14	0.90	0.82
JH	1.92	0.79	0.87	1.08	3.31	1.71	1.16	0.91
EN	1.29	0.67	0.44	0.34	1.98	1.16	0.82	0.67
CE	1.59	0.70	0.44	0.35	2.12	1.21	0.87	0.70
PO	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: The table reports root mean square errors (RMSE) of forecast errors for each team, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The top panel shows the RMSE level, and the bottom panel shows the RMSE relative to the RMSE of the benchmark model. The first evaluation sub-sample is from June 20, 2020 to October 31, 2020, and the second evaluation sub-sample from November 7, 2020 to March 20, 2021.

Table B3: MAPE across sub-samples

MAPE

	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	0.24	0.35	0.43	0.54	0.40	0.46	0.63	0.90
UM	0.19	0.24	0.30	0.45	0.32	0.37	0.44	0.55
UA	0.27	0.47	0.78	1.07	0.35	0.50	0.68	0.89
GT	0.22	0.26	0.32	0.42	0.30	0.43	0.63	0.84
MO	0.20	0.37	0.57	0.87	0.27	0.33	0.40	0.51
PS	0.30	0.50	0.72	1.04	0.30	0.45	0.75	1.10
LA	0.28	0.51	0.86	1.20	0.26	0.38	0.62	0.91
JH	0.24	0.26	0.66	1.42	0.58	0.73	0.85	0.87
EN	0.17	0.20	0.25	0.30	0.29	0.39	0.48	0.62
CE	0.21	0.23	0.28	0.37	0.32	0.42	0.54	0.68
PO	0.12	0.33	0.70	1.19	0.16	0.36	0.67	1.06

Relative MAPE

	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	2.00	1.06	0.61	0.45	2.50	1.28	0.94	0.85
UM	1.58	0.73	0.43	0.38	2.00	1.03	0.66	0.52
UA	2.25	1.42	1.11	0.90	2.19	1.39	1.01	0.84
GT	1.83	0.79	0.46	0.35	1.88	1.19	0.94	0.79
MO	1.67	1.12	0.81	0.73	1.69	0.92	0.60	0.48
PS	2.50	1.52	1.03	0.87	1.88	1.25	1.12	1.04
LA	2.33	1.55	1.23	1.01	1.63	1.06	0.93	0.86
JH	2.00	0.79	0.94	1.19	3.63	2.03	1.27	0.82
EN	1.42	0.61	0.36	0.25	1.81	1.08	0.72	0.58
CE	1.75	0.70	0.40	0.31	2.00	1.17	0.81	0.64
PO	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: The table reports MAPE of forecast errors for each team, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The top panel shows the MAPE level, and the bottom panel shows the MAPE relative to the MAPE of the benchmark model. The first evaluation sub-sample is from June 20, 2020 to October 31, 2020, and the second evaluation sub-sample from November 7, 2020 to March 20, 2021.

Table B4: MLinex across sub-samples

MLinex

	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	0.05	0.10	0.16	0.29	0.11	0.17	0.40	0.85
UM	0.04	0.05	0.06	0.14	0.07	0.11	0.20	0.39
UA	0.12	0.25	0.48	0.84	0.09	0.21	0.46	1.00
GT	0.20	0.20	0.19	0.27	0.06	0.13	0.33	0.82
MO	0.04	0.10	0.25	0.57	0.05	0.08	0.14	0.25
PS	0.08	0.22	0.38	0.76	0.06	0.17	0.46	1.03
LA	0.06	0.22	0.70	1.74	0.05	0.13	0.39	0.95
JH	0.04	0.06	0.26	0.82	0.26	0.46	0.77	0.93
EN	0.02	0.04	0.06	0.10	0.06	0.11	0.19	0.36
CE	0.04	0.04	0.06	0.12	0.07	0.13	0.25	0.45
PO	0.01	0.10	0.54	2.22	0.02	0.08	0.31	0.88

Relative MLinex

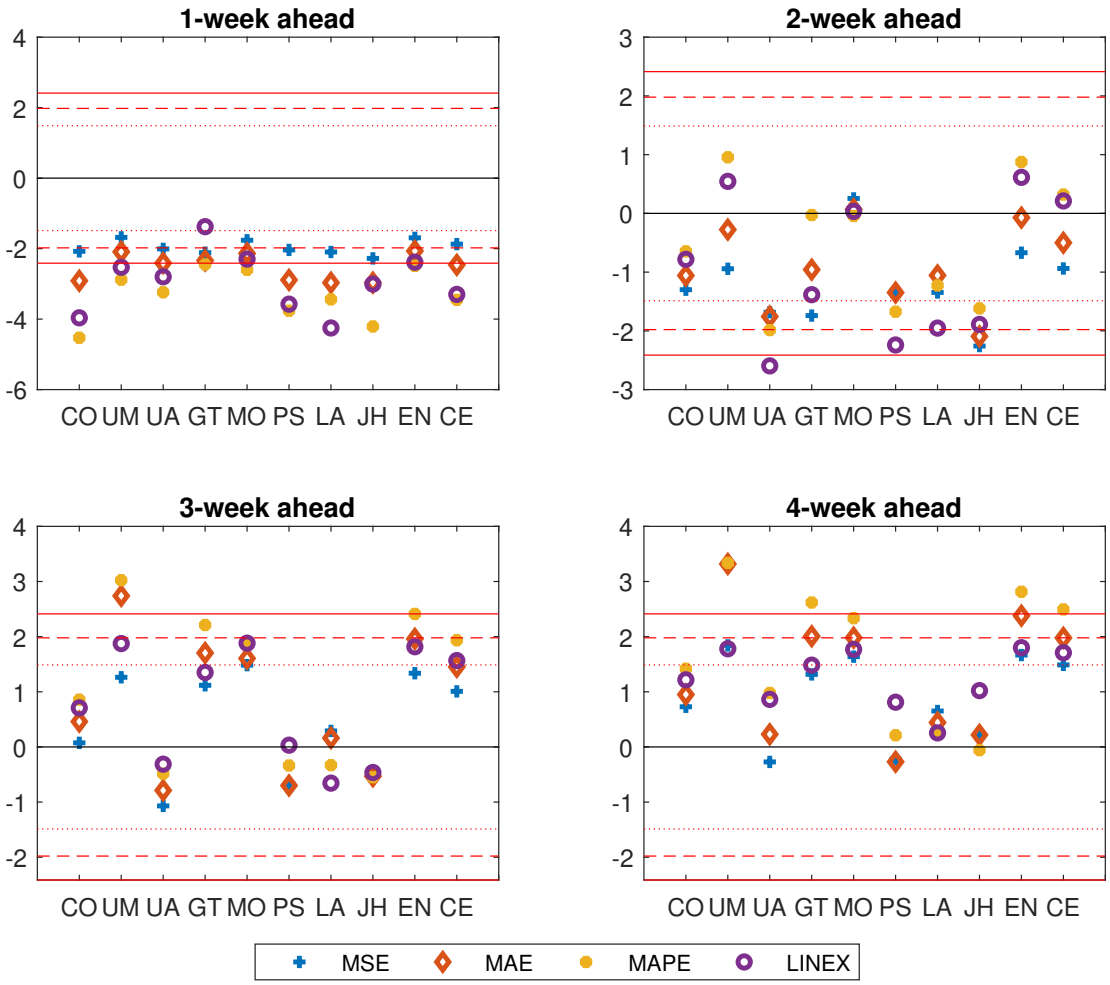
	1st sub-sample				2nd sub-sample			
	1 week	2 weeks	3 weeks	4 weeks	1 week	2 weeks	3 weeks	4 weeks
CO	5.00	1.00	0.30	0.13	5.50	2.13	1.29	0.97
UM	4.00	0.50	0.11	0.06	3.50	1.38	0.65	0.44
UA	12.00	2.50	0.89	0.38	4.50	2.63	1.48	1.14
GT	20.00	2.00	0.35	0.12	3.00	1.63	1.06	0.93
MO	4.00	1.00	0.46	0.26	2.50	1.00	0.45	0.28
PS	8.00	2.20	0.70	0.34	3.00	2.13	1.48	1.17
LA	6.00	2.20	1.30	0.78	2.50	1.63	1.26	1.08
JH	4.00	0.60	0.48	0.37	13.00	5.75	2.48	1.06
EN	2.00	0.40	0.11	0.05	3.00	1.38	0.61	0.41
CE	4.00	0.40	0.11	0.05	3.50	1.63	0.81	0.51
PO	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: The table reports MLinex of forecast errors for each team, the Ensemble (EN), the Core Ensemble (CE) and the polynomial (PO) forecasts. The top panel shows the MAPE level, and the bottom panel shows the MLinex relative to the MLinex of the benchmark model. The first evaluation sub-sample is from June 20, 2020 to October 31, 2020, and the second evaluation sub-sample from November 7, 2020 to March 20, 2021.

C Full sample testing results

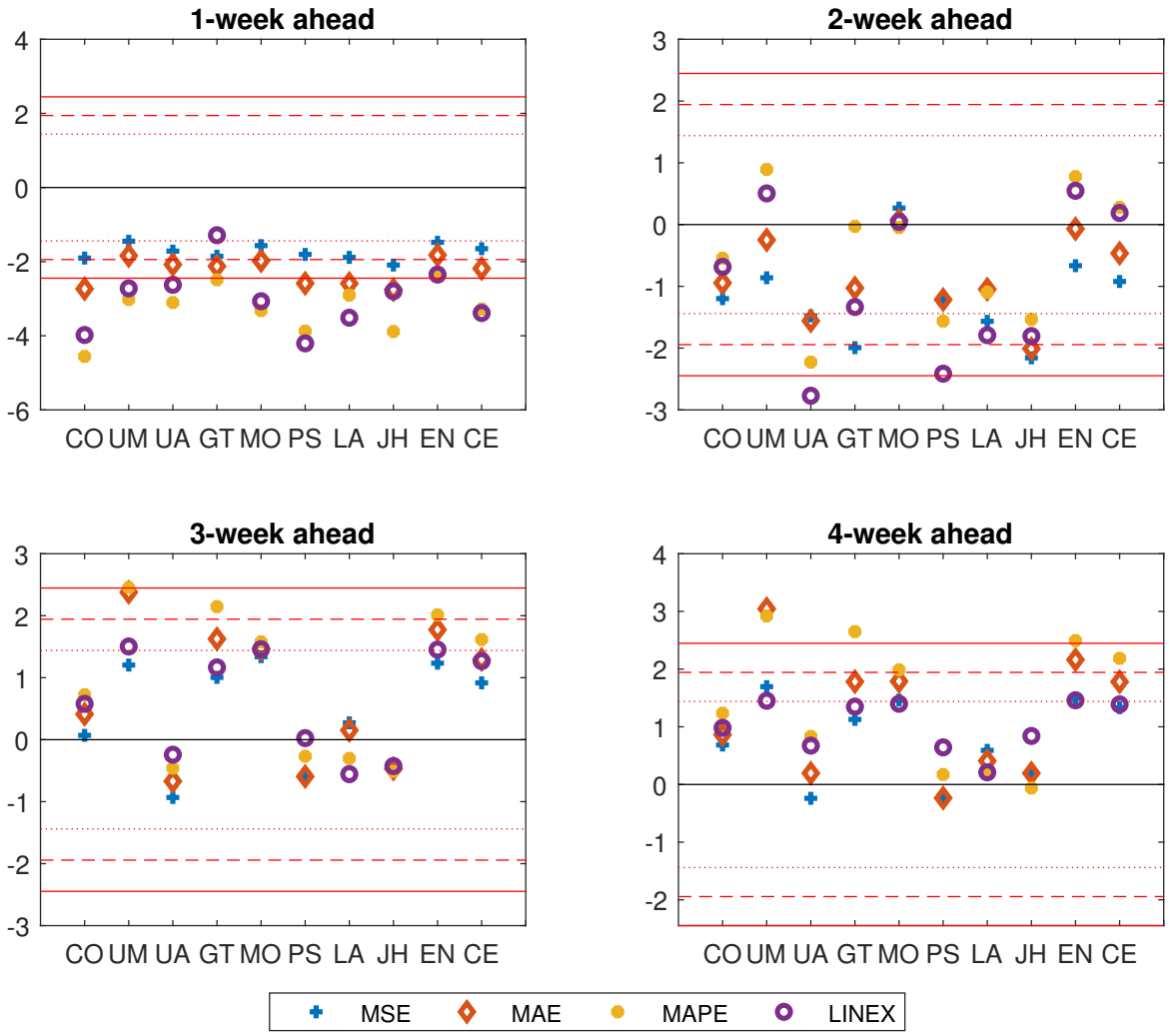
Testing results on the full sample from June 20, 2020 to March 20, 2021 are reported in Figures C5-C6. Results are very similar to what is obtained for the analysis on the two sub-samples. In particular, in the short-run, the benchmark outperforms the other forecasts, while in the long-run, some teams and the ensembles outperform using some alternative loss functions. In particular, Figure C5 documents how in the short run, the benchmark model outperforms the team and the ensemble predictions. At 4-week ahead, according to MAPE, some teams and both ensemble forecasts significantly outperform the benchmark. While, according to MAE, only University of Massachusetts-Amherst forecasts outperform the benchmark. A similar pattern is documented by Figure C6.

Figure C5: Forecast evaluation with WCE - Full sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to March 20, 2021.

Figure C6: Forecast evaluation with WPE - Full sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted periodogram estimator (WPE) of the long-run variance and fixed-m asymptotics. The benchmark is a second degree polynomial model fitted on a rolling window of 5 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the polynomial model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to March 20, 2021.

D AR(1) benchmark

In this appendix, we use an AR(1) benchmark instead of the the polynomial function in (4). In particular, we fit an AR(1) model to the logarithm of weekly incident deaths, as follows

$$Y_t = \mu + \phi Y_{t-1} + \epsilon_t \tag{5}$$

where $Y_t = \log(y_t - y_{t-1})$ and y_t denotes the cumulative weekly deaths. We then obtain predictions for y_t, as follows

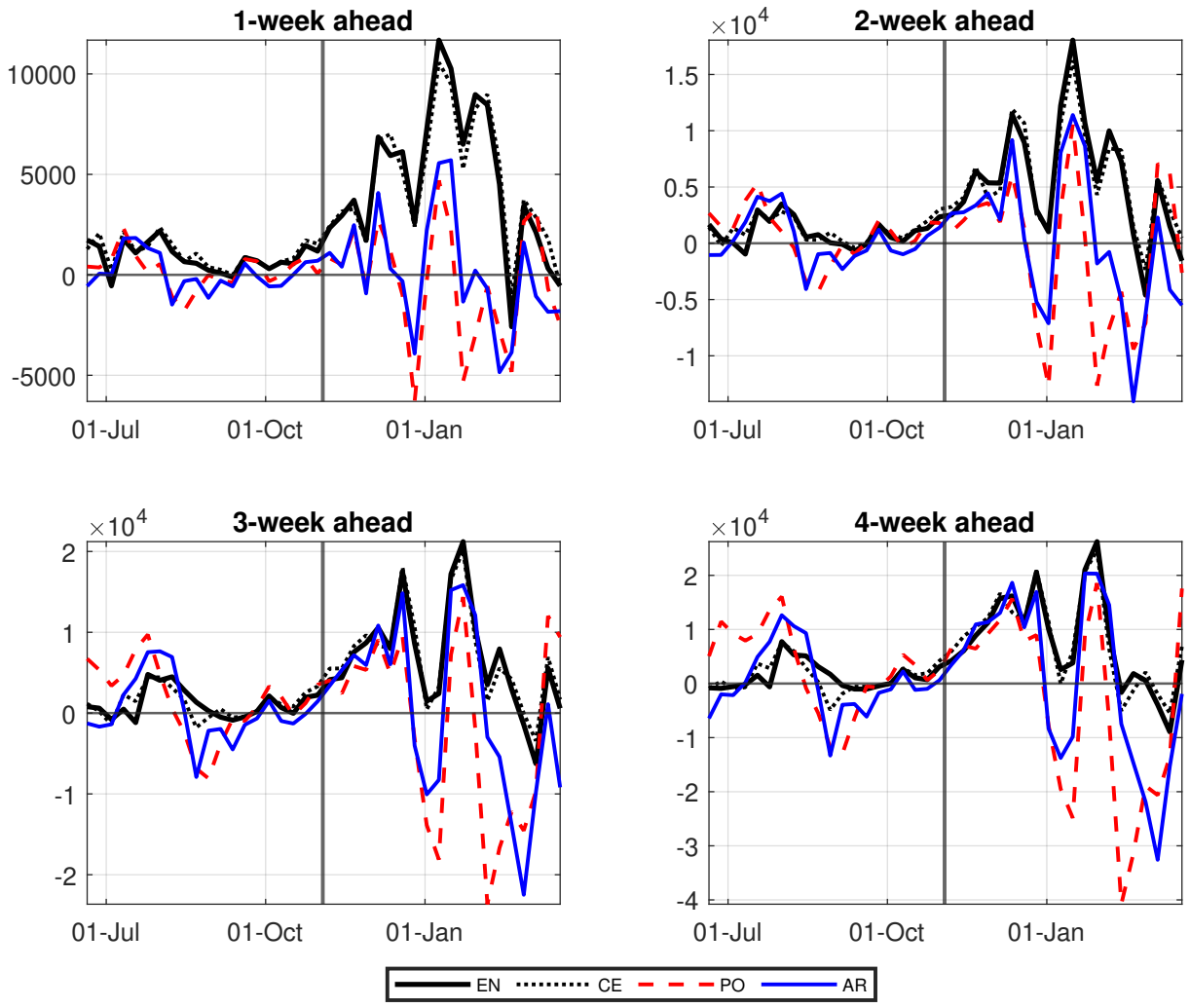
$$y_{t+h|t} = y_t + \sum_{j=1}^{h} \exp(Y_{t+j|t})$$

where $Y_{t+j|t}$ is the j steps ahead prediction from (5). As for the polynomial benchmark in Section 3.3, we use a rolling window of five observations to estimate the coefficients.

Figure D7 plots the forecast errors for each forecasting scheme (computed as the difference between the realization and the point forecast). The figure indicates that the AR(1) benchmark has forecast errors similar to those of the polynomial benchmark used in the main text.

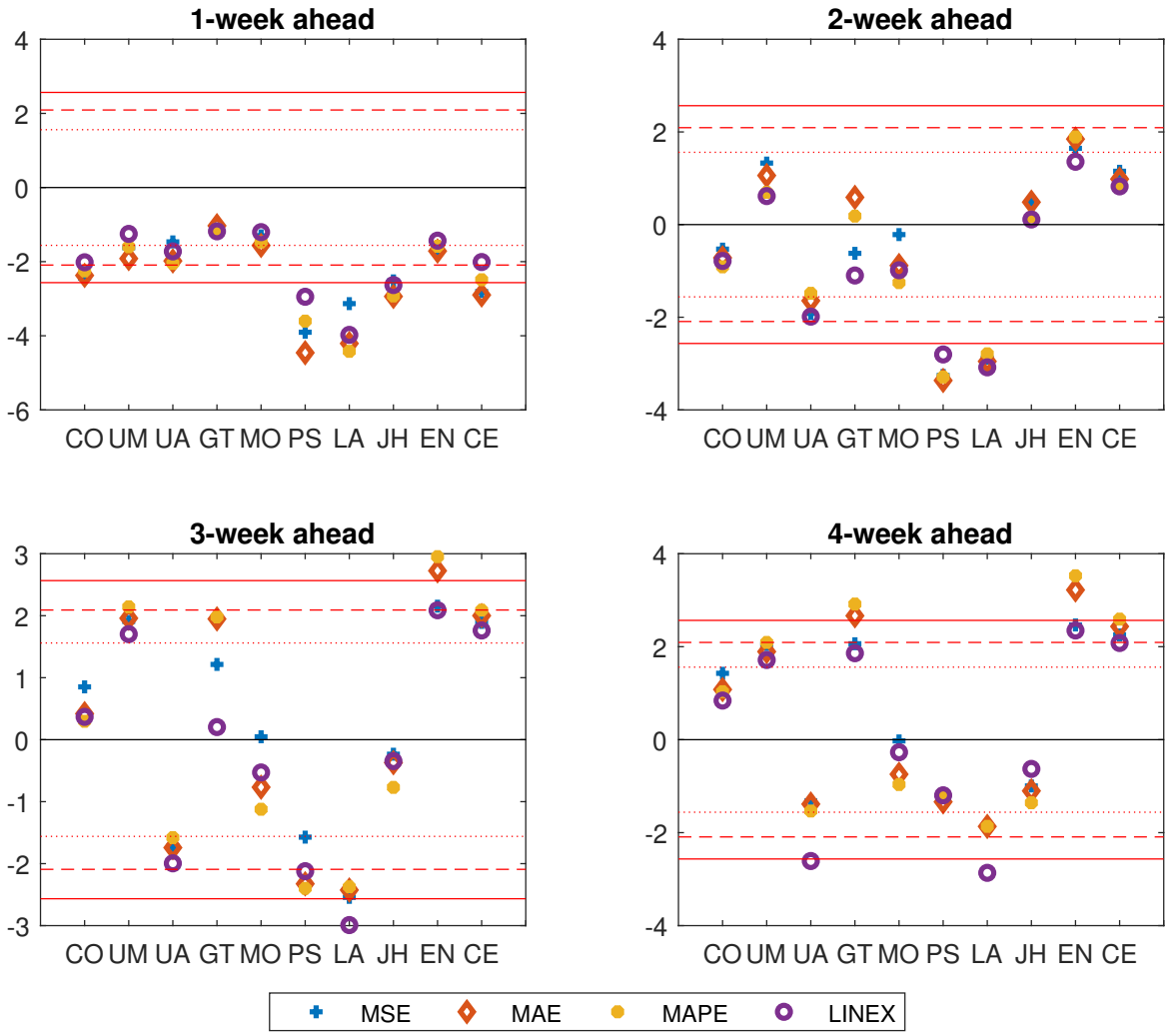
Testing results using the AR(1) benchmark are reported in Figures D8-D9. Results are similar to those obtained using the baseline benchmark model. In particular, none of the forecasting teams predicts better than the benchmark at the 1-week forecasting horizon, with the benchmark significantly outperforming several forecasting teams. Differences at the 2-week horizon are almost never significant, and in some cases the sign of the test statistic turns from negative to positive. At the 3- and 4- week horizons, some teams significantly outperform the AR(1) benchmark model.

Figure D7: Forecast errors, AR benchmark



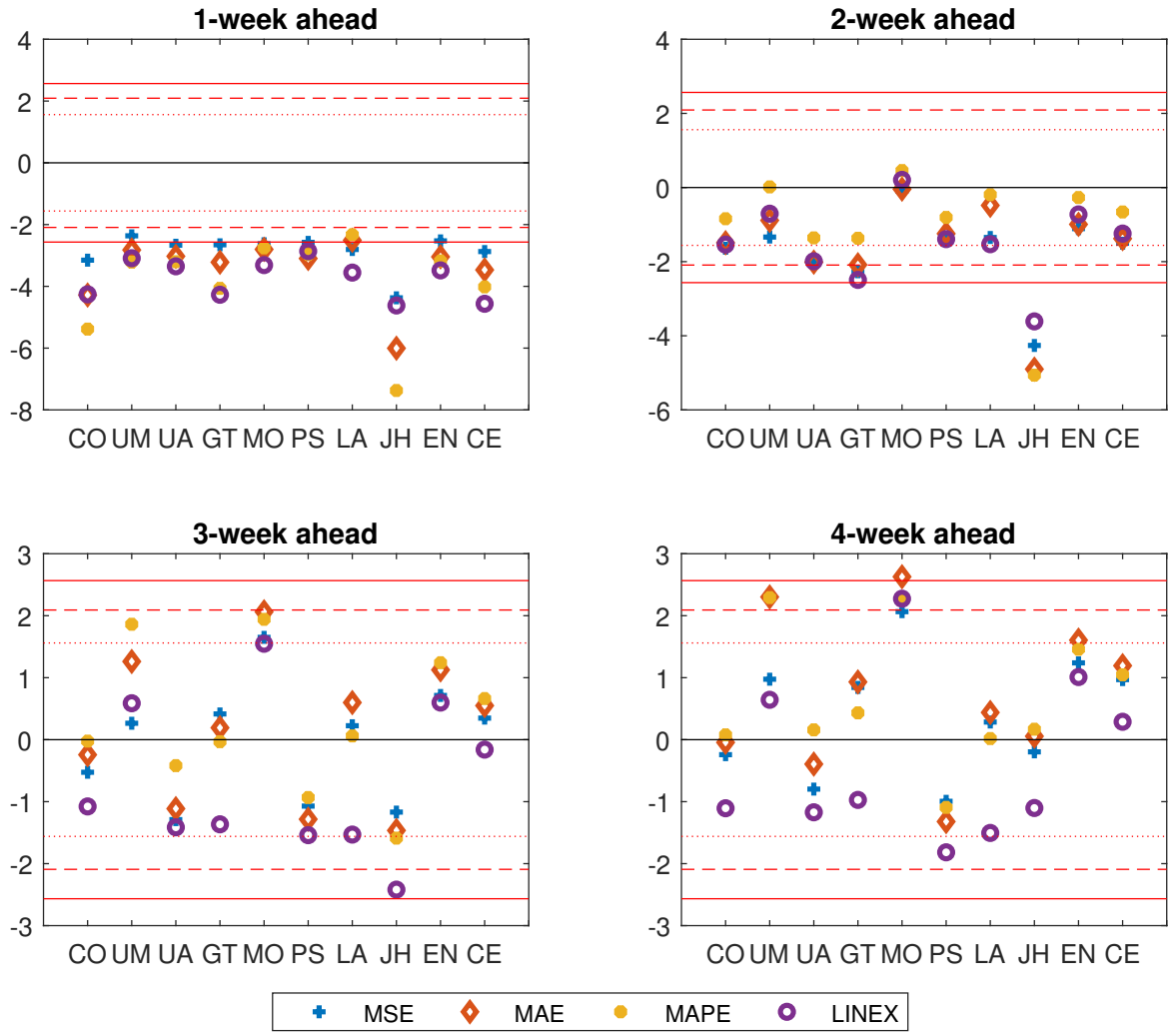
Note: Forecast errors at forecasting horizons from 1 to 4 weeks. Weekly observations from June 20, 2020 to March 20, 2021. The vertical line indicates November 3, 2020 and delimits the two sub-samples. EN denotes the ensemble, CE denotes the core ensemble, PO the polynomial benchmark and AR the AR(1) benchmark. Forecast errors are defined as the realised value minus the forecast.

Figure D8: Forecast evaluation with AR(1) benchmark - First evaluation sub-sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is an AR(1) fitted on a rolling window of 10 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the AR(1) model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to October 31, 2020.

Figure D9: Forecast evaluation with AR(1) benchmark - Second evaluation sub-sample



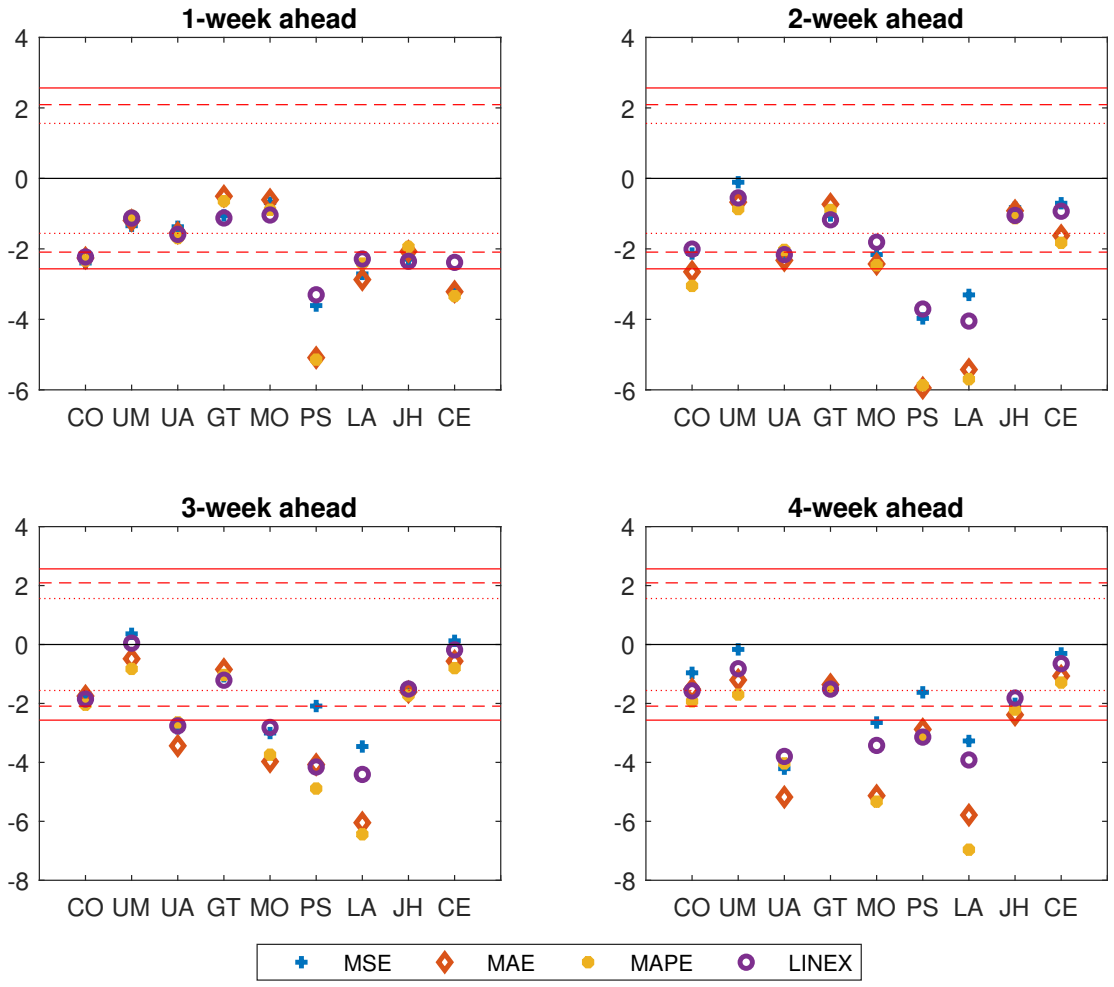
This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-m asymptotics. The benchmark is an AR(1) fitted on a rolling window of 10 observations. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the AR(1) model. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is November 7, 2020 to March 20, 2021.

E Ensemble benchmark

In this section we consider as benchmark the Ensemble forecast produced by the CDC. Thus, we are now testing for the null of equal predictive accuracy of each forecasting team and the Ensemble forecast. As noticed in Section 3, the weekly composition of the pool of models contributing to the Ensemble forecast changes, and it includes, in general, a considerably larger number of teams than the one we consider in our evaluation.

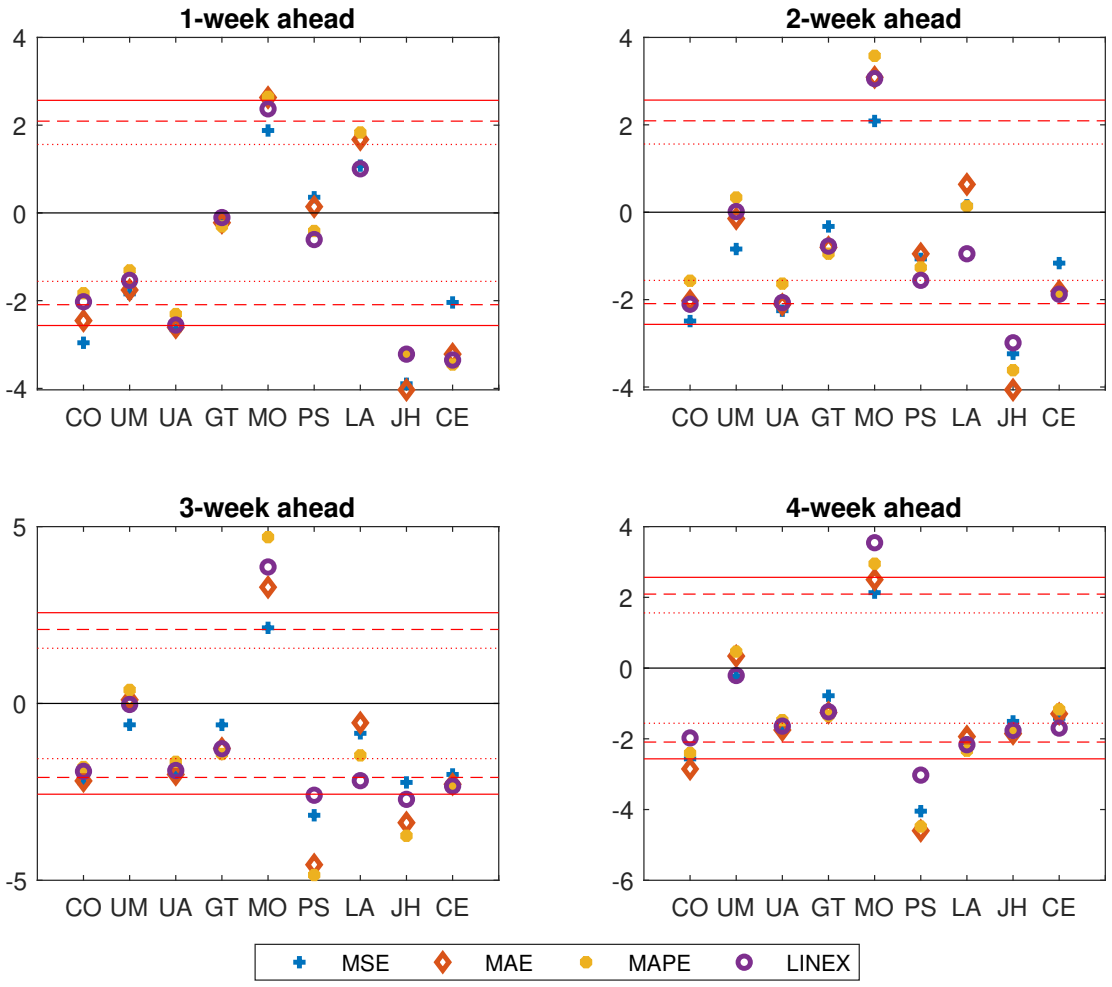
Testing results are reported in Figures E10-E11. In the first sub-sample, the Ensemble outperforms all forecasting teams at all forecasting horizons, with a significant outperformance in some cases. In the second sub-sample, results are more mixed as, while most forecasting teams are outperformed by the Ensemble, the predictions of the Northeastern University, Laboratory for the Modeling of Biological and Socio-technical Systems (MO) significantly outperform the Ensemble predictions at all forecasting horizons. Of particular interest is the fact that the Core Ensemble is also often outperformed (sometimes significantly) by the Ensemble, indicating the importance of averaging a larger number of predictions, rather than just a few.

Figure E10: Forecast evaluation with Ensemble benchmark - First evaluation sub-sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is the Ensemble forecast produced by the CDC. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the Ensemble. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is June 20, 2020 to October 31, 2020.

Figure E11: Forecast evaluation with Ensemble benchmark - Second evaluation sub-sample



This figure reports the test statistic for the test of equal predictive accuracy using the weighted covariance estimator (WCE) of the long-run variance and fixed-b asymptotics. The benchmark is the Ensemble forecast produced by the CDC. A positive value of the test statistic indicates lower loss for the forecaster, i.e. better performance of the forecaster relative to the Ensemble. Different loss functions are reported with different markers: plus refers to a quadratic loss function, diamond to the absolute value loss function, filled circle to the absolute percentage loss function and empty circle to the asymmetric loss function. The dotted, dashed and continuous red horizontal lines denote respectively the 20%, 10% and 5% significance levels. The forecast horizons are 1, 2, 3 and 4 weeks ahead. The evaluation sample is November 7, 2020 to March 20, 2021.