Twitter Thread by Prof Francois Balloux

Prof Francois Balloux
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is an excellent scientist and a responsible professional. She likely read the paper more carefully than most. She grasped some of its strengths and weaknesses that are not apparent from a cursory glance. Below, I will mention a few points some may have missed.
1/

> I've recently come across a disinformation around evidence relating to school closures and community transmission that's been platformed prominently. This arises from flawed understanding of the data that underlies this evidence, and the methodologies used in these studies. pic.twitter.com/VM7cVKghgj
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> — Deepti Gurdasani (@dgurdasani1) February 1, 2021

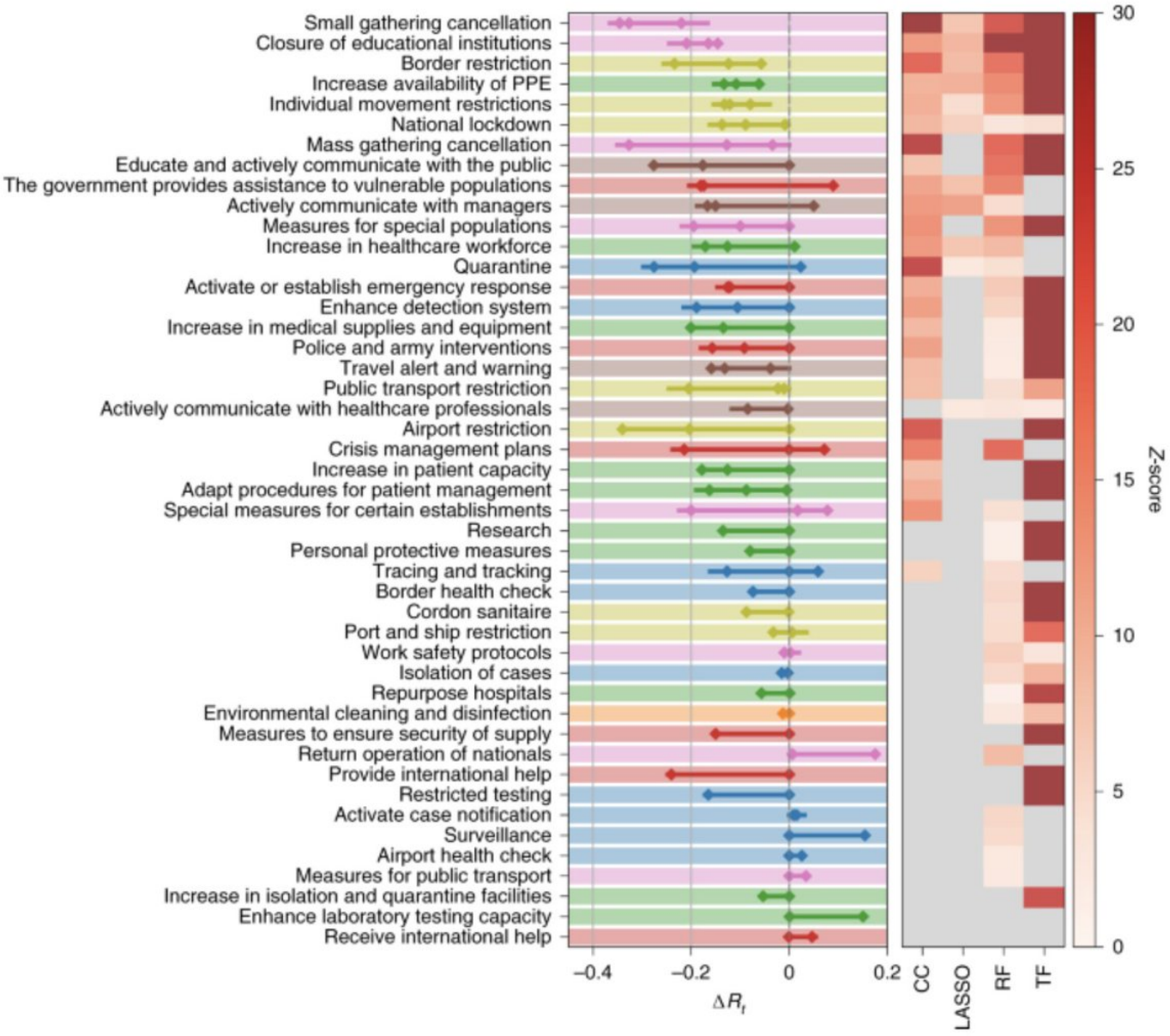
The paper does NOT evaluate the effect of school closures. Instead it conflates all 'educational settings' into a single category, which includes universities.
2/

The paper primarily evaluates data from March and April 2020. The article is not particularly clear about this limitation, but the information can be found in the hefty supplementary material.
3/

> such long-term effects (over timespans of more than a month) in the current work. Furthermore, note that our analysis considers mostly data from March and April 2020 where many countries experienced surges of case numbers that most likely hindered effective contact tracing and other case identification measures. This also applies to the relative ineffectiveness of quarantining people

The authors applied four different regression methods (some fancier than others) to the same data. The outcomes of the different regression models are correlated (enough to reach statistical significance), but they vary a lot. (heat map on the right below).
4/

Fig. 1: Change in R_t (ΔR_t) for 46 NPIs at L2, as quantified by CC analysis, LASSO and TF regression.



The effect of individual interventions is extremely difficult to disentangle as the authors stress themselves. There is a very large number of interventions considered and the model was run on 49 countries and 26 US States (and not >200 countries).

5/

Because the heterogeneity of the effectiveness of individual NPIs across countries points to a non-independence among different NPIs, the impact of a specific NPI cannot be evaluated in isolation. Since it is not possible in the real world to change the sequence of NPIs adopted, we resort to 'what-if' experiments to identify the most likely outcome of an artificial sequence of NPIs in each country. Within the TF approach, we selectively delete one NPI at a time from all sequences of interventions in all countries and compute the ensuing evolution of R_t compared to the actual case.

It is challenging to estimate the effect of interventions in the absence of a counterfactual. This difficulty is compounded by likely confounders, such as climate (not mentioned in the paper). Territories that implemented similar interventions might share comparable climate.
6/

This is a sophisticated piece of work, with both strengths and weaknesses. Though, to me, its primary value is the proposed methodological framework rather than its estimates of the efficacy of individual interventions, which efficacy remain debatable.
7/

There is room for scientific discussion about how solid the estimates presented in the paper may be. Though, accusing colleagues expressing reservations about the robustness of some of the findings of 'spreading disinformation' feels inadequate, to say the least.
8/

This paper has generated endless conflict. One intriguing feature is that all the spats are around 'educational settings'. Interestingly, the paper also claims that T&T and isolation of cases, among other measures, are completely ineffective, yet no one seems to care ... ■
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