Response to letter by Haber et al. requesting retraction of PNAS paper

Here is a response to the specific points (1-7) raised in the letter drafted by Haber et al.

1) In one critical example, the paper asserts that "after April 3, the only difference in regulatory measures between NYC and the United States lies in face coverings on April 17 in NYC." This is verifiably false, based on widely available (e.g., HIT-COVID[2]) sources. … It is flatly untrue that there were no other regulatory differences between NYC and the rest of the US on those dates.

Response:

The paragraph where this sentence is extracted from starts as follows:

We further compared the numbers of daily new cases between NYC and the United States (excluding the data in NYC) from March 1 to May 9 (Fig. 3). The daily numbers of newly confirmed infections in NYC and the United States show a sharp increase in late March and early April.

The point is that "United States" in the above sentences refers to the entire country, in terms of total number of cases accumulated, minus the cases in New York City. Regulatory measures refer to federal measures; not to state measures. We have a second paper where we look in detail at the state measures; obviously, we are aware of the measures state-by-state, as we have a full paper devoted to these measures. In summary, what is flatly true is that the sentence was taken out of context, and hence the objection does not make any sense whatsoever.

2) "With social distancing, quarantine, and isolation in place worldwide and in the United States since the beginning of April, airborne transmission represents the only viable route for spreading the disease." In fact, in April, many regions (e.g., Sweden, parts of the United States) were not in lockdown, and quarantine and isolation were not in place in most parts of the world.

Response:

Here again, the sentence is taken out of context; it refers to the figures where we plot the number of cases. Quite obviously we are aware of the situation in Sweden and the rest of the world, including Korea, China, etc.

3) The analysis ignored the lag between changes in disease transmission and changes in reported case counts … Dates of policy implementation are extremely poor proxies for mass behaviors, including social distancing and mask use … Dates of policy implementation were concurrent with an enormous set of changes across society which plausibly affected reported incidence of COVID-19 …

Response:

Indeed, we agree that there is a question with the lag, which we address explicitly in our second paper. In practice, people began using masks days before they were mandated, as can be seen in many news reports. The point is that we used official government data to specify the dates used in the figures; we did not invent any dates and of course we did not invent any data as well. We are obviously aware of the potential complications with the ways people accept the mandates; on the other hand, the data show what happens averaging all these complications.

4) Case counts were modeled with a simple linear regressions, which is not consistent with infectious disease dynamics.

Response:

A simple inspection of the data indicates a remarkable linearity in the portions of the figures we highlight. We wonder what the authors of the letter mean by consistency with "infectious disease dynamics"; obviously, the observed dynamics is undoubtedly linear at certain times, in spite of the author's wishes.

5) Demographics, policies, and contact behaviors in Wuhan, Italy, New York City, and the USA are inappropriately treated as being nearly equivalent to one another with respect to the epidemic.

Response:

In our paper we merely compare the statistics, showing very clear linearity in some portions, as stated above. In which ways do the authors imagine that we should change the numbers to account for the differences in culture, language, time-of-day, etc.?

6) No measures of statistical uncertainty are measured or presented, which is a departure from scientific norms and particularly concerning given an analysis based on only three regions.

Response:

As stated repeatedly, the remarkable linearity in the data is quite sufficient to make our case. We provide R^2 data, so to add "statistical uncertainty" does not seem particularly useful.

7) Any one of the above issues in isolation would be cause for serious concern, but in combination, they are alarming.

Response:

We agree that any one of the above issues in isolation is cause for serious concern, but it is concern about the incredible simplistic and erroneous nature of the proposed criticisms. And we

also agree that in combination, they are alarming; it is truly incredible how the authors could come up with such naïve ideas, merely because no COVID-19 epidemiologist was among the authors of our paper.

We would be glad to further discuss with you any other issues related to the scientific validation of our findings. What might not be worth your time are further allegations connected with practices of some groups that display a peculiarly naïve pseudo-scientific culture.

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