Witness Name: Neil Ferguson

Statement No. :2

Exhibits:none

Dated:11/7/2023

UK COVID-19 INQUIRY

MODULE 2

SECOND WITNESS STATEMENT OF PROFESSOR NEIL FERGUSON

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1. I, Professor Neil Ferguson, Imperial College London, Exhibition Rd, South Kensington, London SW7 2BX , will say as follows:
2. This is my second statement to the Inquiry in relation to Module 2. I have been asked to address a series of matters that have not been set out in my first statement and take this opportunity respond to the questions. Given the length and detail of my initial statement I will simply set out the question and then provide my response. When considering these replies it will be important to note the wider background and details in my first statement.
3. Given the Inquiry has requested responses to specific issues which do not fit naturally into first statement, I will address each in turn.

4. The Inquiry asked me to respond to views expressed by Professor Leon Danon (University of Exeter) that Imperial College and LSHTM researchers dominated early SPI-M-O work during that pandemic, and by Professor Thomas House (University of Manchester) that Imperial College was “overwhelmingly dominant” throughout the pandemic, including with respect to publication, funding and data.
5. I first note that while the MRC Centre for Global Infectious Disease Analysis (MRC GIDA) at Imperial College is large, it is no more dominant in the area of infectious disease modelling than many other UKRI funded research centres and units are within

their respective disciplines. For many years, UKRI has recognised that there are advantages to funding centres of excellence in specific area: (a) such centres provide a breadth and depth of expertise it is difficult for smaller groups to reproduce, which provides a more stimulating and varied training environment for early career researchers; (b) they allow investment in equipment, technical staff and infrastructure which could not be justified in a smaller group; (c) they provide capacity to respond to urgent priorities in an integrated and coherent manner.

6. Secondly, I would note that while nearly all European countries made use of infectious disease modelling to inform policy-making, no other country engaged the number of groups that were involved in providing advice via SPI-M-O; many countries involved a single group, a few others a handful. I refer the Inquiry to paragraphs 464-473 of my first module 2 statement. At no time was the Imperial group the sole source of advice to SAGE, which even in its earliest days had representation from multiple universities (including Oxford, LSHTM, KCL, Edinburgh and Imperial).

7. All Imperial College COVID Response Team (ICCRT) reports solely made use of publicly accessible data. It is true that the size of MRC-GIDA allowed us to collate data from scientific preprints and multiple government websites faster than smaller groups might have been able to, but at no point during the pandemic did I receive a request from Thomas House for access to data or an email indicating the types of data he might be interested in and whether we knew of where it might be found. In general, we tried to respond promptly and helpfully to requests from other scientists throughout the pandemic.

8. Thomas House refers to "multiple serious errors in models and estimates presented to the Government, both early and late in the pandemic", albeit without specifying what errors. I would note that late in the pandemic, all SPI-M-O groups had access to the same data; indeed, by virtue of his work with the ONS infection survey, I believe he may have had slightly more rapid access to the results of that survey than we did. Speculating regarding what other errors he might be referring to, I am aware of his view that the epidemic doubling time was underestimated prior to the week of 16th March; I address this issue at length in paragraphs 219e, 226, and 255-258 of my first module 2 statement. My (retrospective) assessment of the epidemic doubling time in mid-March 2020 is that it lay somewhere between 3.7 and 4.2 days, within the range explored by Report 9.

9. There were instances where the conservatism of SPI-M-O and SAGE in requiring comparable estimates from multiple independent groups led to delays in confirming

estimates with significant public health and policy implications. I would highlight the delay between MRC GIDA publishing its first estimate of the infection fatality ratio (IFR) of SARS-CoV-2 on February 11th 2020 (ICCRT report 4) and SPI-M-O reporting estimates of IFR in its consensus statement (after March 1st). I refer the Inquiry to paragraphs 128-140 of my first module 2 statement for details.

10. I disagree with the claim of Thomas House that Imperial group has any sort of "control over publication, funding and data". With respect to publications, all SPI-M-O groups published pre-prints and/or reports during the pandemic, and we exert no control over the (typically anonymous) peer-review process for scientific journal articles; indeed, like many groups, we deprioritised journal articles in the first few months of the pandemic in favour of published results as soon as we had them. I equally reject the idea that we "control" funding; MRC GIDA scientists are undoubtedly successful in raising research grant income, but this is nearly always peer-reviewed (an exception being a few philanthropic gifts) and the vast majority is competitively awarded. Indeed, I would argue that in some contexts, our scale disadvantages us, given that funders are reluctant to make multiple awards to one centre in an single funding round. I have addressed the data issue above.

11. The Inquiry further asked me to comment on the delays some SPI-M-O groups had in accessing the NHSE sitrep data. Paragraphs 251 and 252 of my first module 2 statement refer to access to these data. I note (exhibit 12 submitted with that statement) that I requested and gained approval from Jonathan Van Tam for "SPI-M" to gain access to that data on 16th March 2020. We (and I believe LSHTM) were sent the data from that point onwards; the Inquiry will need to discuss with NHSE and the SPI-M-O secretariat why there was a delay in other SPI-M-O groups gaining the same access.

12. The Inquiry asked me whether I agree with Professor John Kay's statement that models should not be relied upon to provide accurate forecasts, and that they should instead be used "to give people better insights into what is going on.". John Kay is an renowned economist, so I will defer to his judgement in relation to economic modelling. With respect to other disciplines, my answer is that "it depends". While MRC GIDA contributed to SPI-M-O's ensemble-based medium term projections from mid-March 2020 onwards and ran a dashboard giving short-term (<4 week) forecasts of COVID-19 deaths for most countries for much of the first two years of the pandemic, the large majority of our research was focussed on better understanding SARS-CoV-2 transmission and epidemiology, and in undertaking scenario modelling for government

(of which Report 9 is an example). I refer the Inquiry to paragraphs 44-47 of my first module 2 statement for further information.

13. The Inquiry asked me to respond to concerns raised by Dr Richard Horton, editor of the Lancet, that SAGE did not appear to be giving due attention to scientific papers published by Chinese scientists early in the pandemic, or reaching out to those scientists. I disagree; I and several other scientists participating in SAGE (notably Jeremy Farrar) were in frequent contact with Chinese colleagues. We were fully aware of both data and scientific publications emerging from China; indeed, in January and February 2020, MRC GIDA had between 4 and 10 staff and students doing nothing but monitoring new postings on Chinese government, preprint and journal websites. The articles published in the Lancet were indeed helpful, but none provided estimates of the infection fatality ratio (IFR) associated with SARS-CoV-2 infection. Rather they provided case fatality ratio (CFR) estimates – the proportion of lab confirmed cases who were dying. The challenge with CFR estimates is the definition of a case; early in the pandemic, China had limited testing capacity and therefore used a case definition (used to authorise testing) which included pneumonia or severe respiratory disease. I always anticipated that a majority of SARS-CoV-2 infections would be mild or even asymptomatic. In that context, the CFR estimates published in the Lancet were not highly informative of what the population scale public health impact of a SARS-CoV-2 epidemic would be. This is why I prioritised research to estimate the IFR in late January 2020. I refer the Inquiry to paragraphs 125-138 of my first module 2 statement.

14. The Inquiry further asked me whether I agree with Richard Horton's view that there was a "mismatch" between the "urgent warning" from China in January 2020 and the "somewhat pedestrian evaluation of the likely severity of the outbreak" in the evidence published by SAGE.13. I do and found it frustrating myself at the time. I refer the Inquiry to paragraphs 139-147, 196 and 203 in my first module 2 statement for details.

15. The Inquiry asked me why ICCRT Report 1 did not include asymptomatic individuals, and to comment on the doubt raised by Dr Tom Irving (SPI-M-O Secretariat) as to whether there could have been >1000 symptomatic cases with disease severity of a level requiring hospitalisation without detection…it's the only part that doesn't add up to me". I address the asymptomatic issue in paragraph 120 of my original module 2 statement. Reports 1 and 2 made use of data on infections detected in travellers to countries outside China. Nearly all such cases detected in the first few weeks of the pandemic were symptomatic: testing of asymptomatic travellers was at that point a

rarity. Indeed, the three international cases (in Thailand and Japan) informing the analysis of Report 1 had all been hospitalised.

16. We were always aware that there was highly likely to be a broad range of symptom severity in SARS-CoV-2 infection, from lethally severe to asymptomatic. The estimation of the number of infections in Wuhan based on cases detected outside China was therefore going to depend on the case definition used for defining a suspect cases by the countries testing travellers. Thailand and Japan hospitalised the first three cases detected outside China (though precise details on symptom severity were not available for all three). Hence our estimates of the size of the Wuhan outbreak were of cases in Wuhan of equivalent or greater severity than those three cases detected in Thailand and Japan. As to Tom Irving's comment, I would note that the criteria for hospitalisation of cases of a novel pathogen imported into a third country might not match those initially adopted at the epicentre of the outbreak, and that testing capacity was severely constrained in Wuhan at the start of the epidemic there. See also paragraph 120 of my first module 2 statement.

17. The Inquiry asked me to explain how the model on which Report 1 was based was equipped to "cover the many uncertainties involved in generating these estimates in the report." I would correct the Inquiry's apparent interpretation of my email to Patrick Vallance and Chris Whitty on 16th January 2020. I stated "Hopefully we have covered the (many) uncertainties involved in generating these estimates in the report.". "Covered" means commented on or directly accounted for. The analysis of report 1 benefitted from using an extremely simple statistical model. We presented 5 scenarios exploring uncertainties in the number of exported cases from Wuhan, the population catchment of Wuhan airport, the detection window within which exported cases might be expected to test positive. Four other key uncertainties were highlighted in the caveats section of Report 1.

18. The Inquiry asked me to comment on why NERVTAG did not recommend the PHE risk assessment be raised to high by 21st February 2020, and whether I agree with the suggestion made in a Reuters article that "for more than two months, the scientists whose advice guided Downing Street did not clearly signal their worsening fears to the public or the government. .

19. The PHE risk assessment had very specific criteria which needed to be met and was intended to quantify the current (rather than potential future) infection risk facing the UK population. I and nearly all members of NERVTAG felt the criteria listed under

"moderate" best matched contemporaneous evidence of the situation as it stood then. If the Inquiry is asking whether I judged the risk facing the UK to be "moderate" in any more broadly defined sense, the answer is no. I would refer to the Today programme interview I gave on 12th February (among others) where I stated my assessment that we were in the early stages of a lethal global pandemic. Paragraph 514 of my first module 2 statement discusses both that interview and the "balancing act between focussing on explaining the potentially serious public health implications versus emphasising uncertainty and the preliminary nature of findings".

20. The Inquiry asked me to comment on the view of the Commons Health and Social Care and Science and Technology Committees that the initial "wrong" policy proposed by scientific advisers, and adopted by the UK government, was to take a "slow and gradualist approach" to introducing non-pharmaceutical interventions, and to respond to wider concerns that SAGE may have been subject to a degree of 'groupthink'. This raises a number of complex and important issues which I therefore discussed at length in my first module 2 statement. I refer the Inquiry to sections F, G, M and N of that statement. Paragraphs 423-437 give my views on lessons which should be learned by government in assessing risk and the costs and benefits of action and inaction in civil contingency situations. Paragraphs 438-452 give my views on scientific advisory structures in crises. To summarise the points I think are most relevant:

    a. I don't believe there was "groupthink" in how SAGE operated, but I can see how the meeting summaries might give that impression: those summaries emphasised areas of consensus more than areas where there were a diversity of views.

    b. As I emphasised in my first module 2 statement, the adoption of NPIs to achieve long-term epidemic suppression was a paradigm shift in the management of a pandemic, and not one which had ever been planned for previously. The magnitude of the social and economic costs associated with such a strategy may have added to a degree of natural conservatism in SAGE considerations – *i.e.* waiting for more data and analysis to be more certain the situation warranted such a response.

    c. There was little systematic consideration of the costs of inaction versus the costs of action in the early months of the pandemic; by PHE, SAGE or (as far as I can judge) by government more generally. I emphasise the word "systematic" - there was a fair amount of discussion of when NPIs might need to be introduced, but this wasn't linked to a systematic analysis of the range of

epidemic scenarios consistent with data and evidence then available. The costs of over-reacting versus under-reacting were never formally assessed, for instance. I would note that MRC GIDA did not have the capacity to undertake such analyses at the time; looking forwards, this type of real-time, adaptive risk assessment and decision analysis best sits within government (UKHSA, DHSA and the Cabinet Office).

d. Throughout 2020, SAGE suffered from having little sense of what the high level strategic objectives of Government were in managing the crisis, or indeed, on some occasions, of whether there were any such objectives. If SAGE had been informed that avoiding NHS surge capacity being exceeded was a key policy objective (or "red line") at the outset, then conclusions about the need to adopt suppression policies might have been reached a little faster. I would note that keeping healthcare demand within NHS capacity limits was never part of UK pandemic planning before 2020; the (sometimes unstated) planning assumption was that extreme triage measures would be adopted to manage demand.

e. At least in 2020, the precise role of SAGE in informing policy formulation was not as clearly delineated as it might have been. While Patrick Vallance, Chris Whitty and the SAGE secretariat always made it clear that SAGE participants were there to offer scientific rather than operational advice (including on the likely effectiveness of policy options), by late February 2020 I gained something of a sense that SAGE was nevertheless being partially relied upon to inform government of the "best" policy. The difficult with that situation is that without a clear understanding of the criteria against which one is evaluating the costs and benefits of policy options, it is impossible to define "best". Thus SAGE participants – and the co-chairs in particular – were left somewhat subjectively balancing the many different, often unstated, objectives of policy. In my view, this issue was further exacerbated by the "Chinese wall" between SAGE and COBR (for the independent participants on SAGE).

f. There was (and in my view, still is) too much focus on reasonable worst case scenarios (RWCS) in guiding planning, rather than preparing policy responses to a range of severity scenarios and then narrowing policy options as more data accumulates which allow the "true" scenario to be better resolved. A particular issue in March 2020 was that SAGE's compromised decision to adopt the Imperial IFR estimates as the RWCS for COVID-19. Since the RWCS is viewed as being unlikely to happen (almost by definition), this may have contributed to

the gradualism referred to in the Commons report. See paragraph 146-147 of my first module 2 statement.

g. I think there would be benefits from separating the functions of providing analysis of an unfolding crisis from providing high-level, strategic scientific advice and constructive criticism of the evidence inputs being generated from within and outside government. In reality, PHE's limited capacity to provide epidemiological analysis meant that it did not act in its expected role as the normative source of analytical inputs into SAGE (and perhaps government more widely) in the early months of the pandemic. Thus John Edmunds, Graham Medley and myself took on much of the responsibility for inputting epidemiological evidence into SAGE – in the case of John and myself, summarising work of our respective teams, and in the case of Graham, that of SPI-M-O collectively. This was arguably not optimal. First, John and I were working very long hours running our teams (and doing analyses ourselves), and so had less time to dedicate to providing critical strategic input. Second, while both of us are scientifically self-critical by nature, we clearly were not independent from the work we were presenting. The issues with sub-group chairs (such as Graham Medley and Peter Horby) reporting the conclusions of their groups are slightly different. While that role is clearly needed, it also risks those individuals feeling a degree of ownership of the outputs of their groups, and therefore, in my view, needs to be balanced by additional SAGE participants who can offer challenge without having been involved in generating the evidence or reports being considered.

21. The Inquiry asked me to comment on the 'Eat Out To Help Out' scheme, whether SAGE was consulted on the scheme, the effect of the policy on transmission of the virus, and other occasions where government failed to consult SAGE on important policy initiatives. I have not examined the epidemiological impacts of the "Eat Out to Help Out" scheme myself, but the University of Warwick study is elegant and I see no obvious flaws in its analysis. With respect to other occasions SAGE was not consulted, I would note that SAGE was rarely consulted on details of specific social distancing rules ("bubbles" being an exception). However, in my view, the more strategically important failures to consult were in relation to the May 2020 COVID Alert system (paragraphs 299 and 300 of my first module 2 statement) and the local Tier system in October 2020 (paragraphs 316-319 of my first module 2 statement).

22. The Inquiry asked me to address (a) whether the models used by SAGE in the early months of the pandemic took adequate account of COVID-19's disproportionate effect on the elderly, (b) why shielding was not included in the models, (c) why schools but not care homes were included, and (d) whether the public health response was sufficiently targeted at those who were most vulnerable.

- In relation to (a), yes, all Imperial and LSHTM models took account of this from mid-February 2020. At the time, those were the only well-developed COVID-19 transmission models in use by SPI-M-O to model the impacts of NPIs.
- In relation to (b), I would note that shielding was included in the model used for Report 9 from late February 2020. I refer the Inquiry to paragraphs 189, 190 and 193 in my first module 2 statement.
- In relation to (*c*), I refer the Inquiry to paragraph 193, 194, 342 and 112 in my first module 2 statement. I also note that John Edmunds and I both highlighted the importance of infection control in care homes in SAGE meetings in February 2020 (see paragraph 193 of my first module 2 statement). We included care homes in our main UK transmission models as soon as we had the time and data to do so (from May 2020 – see paragraphs 194 and 112 of my first module 2 statement).
- In relation to (d), I am aware of Professor Mark Woolhouse's view that more could have been done to make shielding of the elderly (especially of those in care homes) a more viable policy option, and his group undertook some interesting modelling of the intensive use of testing and shielding of carers to improve the effectiveness of shielding overall. However, in my view, the fundamental challenge had little to do with how much or little modelling was done, but in the operational challenges of making shielding of the elderly sufficiently stringent that social distancing could be relaxed in the rest of the population. In April 2020 we undertook intensive modelling of "exit from lockdown" strategies, including age-stratified NPIs. I refer the Inquiry to paragraphs 291-293 of my first module 2 statement. Even if such shielding was over 80% effective, we found that age-stratified policies (letting, say, under 45s live life as normal) would lead to overwhelming healthcare demand – the "leakage" of infection between generations simply being too large. A particular challenge is that those most vulnerable to severe COVID infection (of any age) also have some of the highest contact rates with the health and social care system. So while we examined targeted approaches involving shielding (Report 9 looked at exactly that in the "mitigation" options it considered), I didn't specifically advocate for such

policies – though I would comment that I tried to avoid advocating for specific policies in general.

23. The Inquiry asked me why ICCRT Report 9, published on March 16th 2020, concluded that *"epidemic suppression is the only viable strategy at the current time."* given that, three days earlier, SAGE recorded that it was *"unanimous that measures seeking to completely suppress spread of Covid-19 will cause a second peak"*. I refer the Inquiry to paragraphs 200-227 in section G and much of section H in my first module 2 statement for a full discussion of these issues. "Viable" in that sentence referred to a policy which met the UK Government's by then articulated goal to avoid NHS capacity being overwhelmed. The conclusion is not inconsistent with the early concerns about suppression causing a second peak; indeed, much of the modelling of suppression in Report 9 examined strategies (involving repeated use of suppression measures) to manage second (and third) peaks. As the final paragraphs of Report 9 make clear, we never viewed suppression strategies as anything more than the least bad option, given the social and economic impacts of such policies and the fact that they locked us into to that strategy until vaccines were available.

24. The Inquiry asked me to comment on whether the two year timescale considered by ICCRT Report 9 was appropriate, with reference to the observation of Professor Mark Woolhouse that *"*no-one could predict the course of this epidemic over such a long timescale*"*. The scenario modelling included in Reports 9 and 12 were never intended to be precise predictions of the trajectory of the epidemic over a two year period. See paragraphs 45, 46, 519 and 520 of my first module 2 statement. I would draw an analogy with climate change models; we can only predict detailed weather patterns reliably up to two weeks in advance, but that doesn't invalidate the use of climate projections to examine changes in mean climate variables under a range of hypothetical scenarios. I wouldn't disagree with Mark Woolhouse's statement, but knowing him, I do not believe he viewed either report 9 or 12 (or all the later scenario modelling undertaken by SPI-M-O groups, including his own) as representing precise forecasts of the epidemic trajectory two years into the future. However, just because one can't make precise predictions over that timescale does not mean scenario modelling over that timescale is worthless; a key problem in the UK policy response to the pandemic was use of an overly short-term time horizon over which to evaluate the costs and benefits of intervention options. I refer the Inquiry to paragraphs 433-435 in my first module 2 statement for more discussion of this issue, and paragraph 48 below.

25. The Inquiry asked me to respond to criticisms that some of the assumptions regarding virus transmission and the effect of NPIs in Report 9 were arbitrary and/or inaccurate. I refer to paragraphs 216-220 of my first module 2 statement for a full discussion of Report 9. Paragraph 219c discusses the assumptions made regarding transmission in different settings. The reductions in contact rates associated with different policy options were chosen to be illustrative and matched the modelling previously undertaken by SAGE. Given we were modelling unprecedented policy options, there was an absence of data with which to derive actual estimates of the impacts of the different NPIs modelled. With the benefit of hindsight and a huge amount of accumulated data, we would stand by most of the assumptions made. Exceptions are the extent of transmission in young children (see paragraph 219a of my first module 2 statement) and the effectiveness of social distancing of the elderly (later called shielding – see paragraph 219b).

26. The Inquiry asked me to respond to criticisms of Report 9 made by Prof Johan Giesecke, including about the contribution of asymptomatic infections to transmission. I first note that I know Johan Giesecke well (he sat on the scientific advisory board of MRC GIDA for many years), and so was saddened that he made such remarks back in May 2020 without out discussing his concerns with me first. In fact, the assumptions we made about asymptomatic transmission in Report 9 were later largely validated. I would refer to paragraphs 216-220 of my first module 2 statement for a full discussion of Report 9. Paragraph 124 discusses the validity of the assumptions the modelling in Report 9 made about asymptomatic infection and transmission. I also note that while some scientists and public health clinicians in Sweden felt that they were adopting a "herd immunity" strategy in March/April 2020, the results from the first serological surveys conducted in that country indicated that in reality, a suppression strategy had been followed: less than 10% of the Swedish population had immunity after the first wave, and, as in the rest of Europe, R had been reduced to below 1 by reductions in social contact rates – albeit Sweden adopted a more voluntary approach to achieving this than most other countries.

27. The Inquiry asked me to comment on the one month delay between the Publication of Report 9 and publication of the model source code. This was caused by a combination of time pressure (I and the researchers working on that model were working 18 hour days throughout March and April, with the code continuously being updated to address urgent policy evidence needs) and the fact that Microsoft (via their Github subsidiary) had offered to help develop the code with us. Prior to release, we and the Github

engineers refactored the code to make it easier for external users to understand and use. I note that the most important conclusions from Report 9 were not dependent on model code – Report 12 used a completely different model (which was also made open source), as did the very similar results obtained by LSHTM (again using a different model).

47. The Inquiry asked me to clarify Imperial College modelling of the pandemic in Sweden, to comment on the (often reported but incorrect) assertion that researchers at Uppsala University used "the Imperial model" to model Sweden, and whether it was appropriate for us to attempt to model large numbers of countries in Report 12. In relation to the Uppsala University issue, I refer the Inquiry to paragraphs 111 and 270-277 in my first module 2 statement which fully address this question.

48. In relation to the more general issue of what we predicted for Sweden, I refer to the same paragraphs, noting that while Sweden relied heavily (but not entirely) on voluntary social distancing measures, the net result of their policies was still epidemic suppression, namely bringing R to below 1. Therefore the relevant projection from report 12 for Sweden are those provided under the Suppression tab of that spreadsheet. Two projections of deaths in the first two years were provided, associated with different triggers for the implementation of NPIs: 3,395 and 14,518. In reality, Sweden recorded 9,816 COVID-19 deaths by the end of 2020, and 15,345 by the end of 2021.

49. More generally, I think Report 12 served a vital purpose in highlighting to policymakers and the more general readership the long-term nature of the threat pose by the pandemic and the consequences for public health policy. Indeed, my only regret is that it wasn't more acted upon. Excluding vaccines, COVID-19 policymaking was characterised by short-termism in many countries, including the UK; by September 2020 the UK government (and those in many other countries) the key message from Reports 9 and 12 (and the work of LSHTM and Warwick University) appear to have been forgotten: namely, that suppression only works when measures are in place, that transmission will rebound when measures are lifted, and therefore policymakers adopting suppression policies need to expect the need for multiple rounds of intensification of NPIs until vaccination can be rolled out. The modelling in Report 9 and Report 12 was never intended to be detailed forecasts of the pandemic two years ahead; there was no way we could predict exactly what policies different countries would adopt, never mind virus evolution or when vaccines would be available. However, I would nevertheless strongly defend the utility of such scenario modelling,

for similar reasons that climate projection modelling is also of critical value. In addition, as it happens, while Report 12 neither accounted for more transmissible and lethal variants or the availability of vaccines within 12 months, the 20 million deaths projected globally under the mitigation (rather than suppression) scenario it considered compare well with the 15-20 million excess deaths estimated to have been caused by the COVID-19 pandemic in its first two years.

50. The Inquiry asked me to respond to criticism of ICCRT Report 13 (later published in Nature), including that the analysis ignored voluntary behaviour changes and exaggerated the impact of lockdown. I refer to paragraphs 285-288 in my first module 2 statement, which address these issues. With scientific colleagues in a number of institutions, we have iteratively refined our initial analysis of the impact of non-pharmacological interventions on transmission of COVID-19. Perhaps the most significant development was decomposing "lockdown" (which varied dramatically between countries in terms of the policies that term represented) into the constituent NPIs defining each "lockdown". While our understanding of the relative effectiveness of different measures has evolved, this has not qualitatively changed the 3.1 million deaths averted estimate we made back in June 2020.

51. The Nature paper (Flaxman et al, Nature, 2020) reporting our first analysis noted that the effects of spontaneous behaviour change were not estimated; there was little data with which to do so at the time (shortly after the peak of the first wave), though some our later analyses examined temporal changes in the effectiveness of NPIs by comparing the first and second waves of the pandemic (Sharma et al, Nat Comms, 2021). We reject the criticism by Stefan Homburg and Christof Kuhbandner which was based on an analysis which failed to account for the high variance in the delay distribution from infection to death, something which results in exactly the type of apparently gradual change in epidemic growth rates (as estimated from deaths) that those individuals noted.

Statement of Truth

I believe that the facts stated in this witness statement are true. I understand that proceedings may be brought against anyone who makes, or causes to be made, a false statement in a document verified by a statement of truth without an honest belief of its truth.

Signed: Personal Data

Dated: 11th July 2023