International Journal of Risk & Safety in Medicine 33 (2022) 217–221
DOI 10.3233/JRS-227016
IOS Press

Viruses without borders and the medical research agenda

Jacob Stegenga
Department of History and Philosophy of Science, University of Cambridge, Cambridge, UK
E-mail: jms303@cam.ac.uk

Received 25 June 2021
Accepted 29 June 2021

Abstract. The COVID-19 pandemic has shown us that there are numerous research questions—empirical, political, and philosophical—that need addressing both prior to, during, and after a pandemic. The current organisation of medical research has hindered our ability to efficiently answer these questions. This in turn suggests that there ought to be changes to how the medical research agenda is set.

Keywords: COVID-19, pandemic, research agenda, well-ordered science

Keywords

As the COVID-19 pandemic gathered force in the spring of 2020, the president of the European Research Council, Michael Ferrari, angrily resigned. Though the reasons for his departure were disputed (and some say he was fired), Ferrari claimed that the European Commission had been funding an "uncoordinated cluster of initiatives". He had proposed that the ERC should set up a dedicated COVID-19 funding stream, with funds distributed rapidly and in a top-down manner circumventing the agency's usual peer review process. In response to Ferrari's resignation, Christian Ehler, a member of the European parliament, claimed that this would have been "a contradiction to the legal basis of the ERC [1]". Whichever side one believes, the COVID-19 pandemic has highlighted problems with the medical research agenda and the mode of allocation of research funding. The pandemic has shown us that there are numerous research questions—empirical, political, and philosophical—that need addressing both prior to, during, and after a pandemic. The status quo organisation of medical research has hindered our ability to efficiently answer these questions. The medical research agenda ought to be better organised.

To establish this, I begin by reviewing some simple facts about this pandemic. These facts are relatively uncontroversial, though when considered together I will argue that they motivate a significant re-structuring of some aspects of the organisation of medical research. Although my focus is on the COVID-19 pandemic, some of the following considerations are more general. Problems with the structure of medical science have been clear to critics, and the current pandemic has crystallised those concerns. After laying out these facts, I then note a variety of questions which we will want answered for future pandemics, and I propose one strategy for answering these questions which would be arguably superior to what was in place during the COVID-19 pandemic.

Fact 1. In a very short period of time, an infectious disease can cause a massive impact on global morbidity and mortality, and because of the potentially exponential nature of infectious disease transmission, the rate of change of this increase in disease burden can itself increase rapidly. COVID-19 is not like most chronic diseases in this respect, obviously. Thus, policy level responses to mitigate the impact of disease burden must be developed and deployed rapidly. This in turn entails the next fact.

Fact 2. Because of the rapidity of some infectious disease outbreaks, there can be little time to generate quality evidence about the effectiveness or harms of policy interventions. This was the case for COVID-19 and is in general true. There can be a relatively narrow temporal window during an infectious disease outbreak in which we can learn about important parameters of the disease, such as the R value and the case fatality rate, and patient-level parameters, such as the range of pathological presentations and development of immunity, before the pandemic becomes widespread and jurisdictions around the world develop policies in an attempt to mitigate the consequences of the pandemic.

Fact 3. Rapidly spreading respiratory viruses disregard national borders. Even with the unprecedented global travel restrictions in 2020 and 2021, the novel coronavirus spread rapidly around the world. Thus even if a country were totally self-interested in its research funding policies, that country ought to be motivated to learn about an infectious disease as the disease manifests outside its national borders.

Fact 4. We know little about the effectiveness and harms of infection mitigation strategies that have been—and at the time of this writing, nearly 1.5 years after the start of the pandemic, continue to be—widely employed during the COVID-19 pandemic, such as mask wearing, school closures, and travel restrictions. This uncertainty contributes to social and political tension, and risks contributing to the deployment of harmful and relatively ineffective interventions. Thus we should learn more about the effectiveness and harms of infection mitigation strategies.

Fact 5. Knowledge about virus transmission and the effectiveness and harms of infection mitigation strategies will to some degree be transferable to future pandemics. For example, if a future pandemic is caused by a virus with similar transmission dynamics to the novel coronavirus, then what we learn now about those transmission dynamics and how to minimise such transmission will be relevant to understanding that future pandemic. Thus, we have added incentive to maximise the study of infectious diseases and infection mitigation strategies in the present.

Fact 6. On some epistemologies of assessing effectiveness of interventions, such as that of evidence-based medicine, we need high quality evidence from population-level experiments, such as randomised trials. Plausibly, such epistemological considerations apply to testing the effectiveness of infection mitigation strategies just as much as they apply to testing the effectiveness of medical interventions such as pharmaceuticals. However, to use randomised trials to assess some mitigation strategies, such as school closures, would require a large number of actors to work together according to a coherent experimental plan. Based on the rapidity noted in *Facts 1* and *2*, that plan should be in place prior to the pandemic.

Fact 7. During the COVID-19 pandemic, numerous research groups around the world quickly began researching a range of interventions, especially pharmaceuticals. This is of course fine and good. Yet, as commentators noted, this involved many small, disparate, hasty trials, many of which were testing the same few drugs. Databases of trials on COVID-19 show that thousands of trials were initiated to test pharmaceutical treatments for COVID-19, despite the fact that pharmaceutical interventions for

respiratory viruses are in general extremely unlikely to be effective at all, and even more unlikely to be highly effective; yet very few trials were initiated which test behavioural or social interventions on infection mitigation, such as social distancing tactics, despite the fact that such interventions have been so widely implemented (see, for example, www.bessi-collab.net). Thus, the status quo distribution of pandemic research resources during COVID-19 has not been optimally efficient.

Fact 8. The majority of research funds in medicine is controlled by industry [2]. These funds tend to be used to develop pharmaceuticals and other kinds of interventions and devices that can generate profit for the industrial sponsors of such research. Most countries also have national funding agencies which devote funds to both basic medical science and to clinical studies. On the international scale, some health agencies do not have research as a primary mandate while other international health agencies that do have a research mandate tend to be focused on specific diseases. And as illustrated by Ferrari's resignation from the ERC, many large research agencies have careful and slow systems for distributing research resources.

Fact 9. Policy responses to the COVID-19 pandemic were laden with philosophical presuppositions, pertaining to questions of inter-generational justice, or how vaccines should be allocated, or the reliability of epidemiological models, or whether the burdens of lockdowns were distributed equitably among various social groups, or whether liberal democracies should deploy lockdowns at all. For example, should children be kept out of school and adults kept out of work in order to minimise the mortality of a demographic group which is already very close to their average life expectancy? Or to consider another example, are randomised trials necessary to establish the effectiveness of interventions, as evidence-based medicine supposes, or does this view merely represent the "dictatorship of the methodologists" as the infamous researcher Didier Raoult claimed when his studies on hydroxychloroquine were criticised? We need well-developed answers to such moral, political, and epistemological questions.

When taken together, these facts suggest that it would be extremely valuable to know much more about infectious disease pandemics—both empirical facts and a range of developed philosophical positions—and such knowledge can be acquired prior to, during, and after a pandemic. Indeed, we can conveniently delineate three fundamental stages of pandemic research—pre-pandemic, pandemic, and post-pandemic—and prioritise a range of questions to be addressed in each stage. Here I suggest just a few such questions, merely for illustration—the full range and prioritisation of such questions ought to be determined by a well-founded system of research prioritisation (the articulation of which is itself a philosophical question in need of an answer—one prominent approach is Kitcher's [3] "well-ordered science" approach, though for criticism see the works by Larroulet Philippi [4] and Shaw [5]).

Pre-pandemic. We should know more about some basic scientific topics, such as dynamics of virus transmission, and the effects of infection mitigation strategies like mask wearing; we should study the epistemological merits and problems of the methods used to answer those scientific questions (such as the reliability of simulations of droplet spread, and the foundations of epidemiological modelling); we should study the psychological and social effects of isolation (as future pandemics will, unfortunately, very likely involve lockdowns similar to those we have faced during the coronavirus pandemic); we should have well-substantiated positions on some fundamental philosophical questions such as issues around intergenerational justice when a virus has a mortality age-skew and that age-skew is inversely correlated with the burdens borne by infection mitigation strategies; we should have careful philosophical assessments of the conditions under which lockdowns are and are not justified. It is worth emphasising

that the kind of research that is motivated out of a concern to be prepared for future pandemics includes both basic and applied science, and also topics that are not empirical, such as some of the philosophical questions noted here. In preparation for the pandemic phase, research plans for the pandemic phase should be developed in advance, during the pre-pandemic phase, to be rolled out immediately as a pandemic emerges.

Pandemic. We should know as much about the infectious agent as possible, such as the infection fatality rate and whether that is modulated by age or pre-existing diseases, the R-value, the mechanism of disease transmission, and the most effective infection mitigation strategies for this particular infectious agent.

Post-pandemic. We should know the long-term consequences of lockdown strategies that were used during the pandemic (isolation, suicide, economic effects); we should know about the long-term consequences of the infection itself, of course; we should know how the pandemic began (zoonotic? lab leak?); we can continue to study in retrospect many of the questions that were considered in the pre-pandemic and pandemic phases.

How should all of this happen efficiently?

Many of the concerns about medical research that have arisen during the pandemic have been a feature of medical research noted by critics for some time, and various solutions have been offered in response to these concerns. Critics have argued that the status quo mode of allocating medical research resources motivates: the pursuit of "me-too" drugs at the expense of potentially impactful interventions [6], malleable and sometimes fraudulent medical research [7–9], and the neglect of tropical infectious diseases [10].

To eliminate the corrupting influence of financial incentives on resource allocation, Brown [11] suggests socialising all of medical research and eliminating intellectual property protection (see also [12]). To eliminate the corrupting influence of financial incentives on research methodology, Moynihan et al. [13] suggest a less extreme proposal in which the testing of interventions is taken out of the control of the manufacturers of the interventions under investigation (see also [9]). To incentivise research into understudied diseases, some have proposed levies on pharmaceutical companies in which the diverted funds are dedicated to those understudied diseases, and others have suggested systems of prepayment contracts for interventions targeting understudied diseases. There may be merit to these proposals; yet, they are developed to resolve other kinds of problems, not a rapidly spreading pandemic (and some are, arguably, presently infeasible, such as the socialisation of medical research).

One strategy which could go some way to accommodating the range of issues highlighted by the *Facts* above would be a global pandemic science research institute, akin to the Intergovernmental Panel on Climate Change (IPCC), with the major difference being that the IPCC does not itself carry out original research, while the institution I am envisioning would carry out original research. This institution could be a unit of an existing organisation, such as the WHO, or it could be a newly formed organisation funded in a similar manner as WHO. *Fact 7* tells us that the status quo of research agenda setting in medicine (described in *Fact 8*) is inefficient (and we saw this clearly in 2020)—these are considerations in favour of such a global, intergovernmental institute independent of industry interests. Its primary mandate would be to study the questions noted earlier (and many more, of course), to rapidly deploy pandemic response teams in the early stages of potential pandemics, and to coordinate with various stakeholders during a pandemic to ensure that we learn as much as we can about new infectious agents as quickly as possible.

As noted, how the agenda of such a global research institution should be set is itself a philosophical question. Above I sketched some of the kinds of questions such an institution should address, but this is

merely armchair moralising—*Facts 2*, *4*, *6*, and *9* hint at the great diversity and complexity of the sorts of questions such an institution could and should address. Pandemics are merely one of many plausible existential threats to humanity, which include climate change and a nuclear holocaust. Yet, in the last eighteen months we have been reminded that a pandemic can cause profound harm and the cost of policy responses to a pandemic can be epic—we could mitigate such future harms by using a fraction of the cost of these policy responses to support a global pandemic science research institution.

Conflict of interest

None to report.

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