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Commentary

A biased analysis of the Swedish management of Covid-19

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The Swedish handling of Covid-19 has been heavily debated both in the international media and scientific journals. Given the considerable differences in opinion, it is exceedingly important that scientists approach the subject from an academic unbiased perspective. In the article *Swedish policy analysis for Covid-19* [1] the unbiased perspective is unfortunately lacking when the authors argue that the Swedish strategy has been successful and that the WHO are now proposing the strategy as a future solution. I would argue that this conclusion is reached through a number of questionable as well as incorrect assumptions that are imperative to highlight in a comment to the paper.

Firstly, Sweden's policy strategy for handling Covid-19 was heavily based on a combination of letting healthcare capacity regulate the strictness of interventions, in combination with protecting the elderly and vulnerable. Kavaliunas *et al* argue that this approach is evidence-based given that it has been argued for in the literature [2]. However, to my knowledge, such a strategy has never been attempted or tested in regards to a global pandemic. Rather, such a utilitarian perspective where individuals' liberty is weighed against the nation's public health is highly unusual in real-world crises when societal actors generally determine that it is "*morally wrong*" to not save as many of the victims as possible [3]. Instead, as was the case in most other countries, a deontological, precautionary perspective is most often the proposed approach when "*cause and effect relationships are not fully established*" [4]. Given the considerable aleatory and statistical uncertainty in regards to a new virus, much of the risk and crises literature, as well as the experience from previous global pandemics, would argue that the evidence-based approach would be based on the precautionary principle and duty-ethics. However, this perspective is entirely absent from the article.

Secondly, it is an undisputed fact that the number of Covid-19 cases and deaths is five to ten times higher in Sweden than in the other Nordic countries. Although fatality rates can be determined differently in different countries meaning that comparisons can be faulty, all Nordic countries use practically the same registration system. As such, Nordic comparisons are highly relevant, not least given the similarities in terms of healthcare capabilities, culture, population density, working conditions, etc., between Sweden, Finland, Denmark, Norway and Iceland.

Although the fatality rates are considerable in Sweden, in the article, the large number of deaths is simply referred to as a "drawback" of the successful Swedish strategy. As of the beginning of November 2020, at least 6000 Swedish citizens have been confirmed dead as a result of Covid-19. I would argue that formulating these deaths as a "drawback" to a Swedish success story is ethically very questionable and that these excess deaths are a clear indication that Sweden's strategy failed given that a key element of the strategy was to protect its vulnerable citizens.

Thirdly, despite controversy regarding the policy and ethical element of herd immunity, Kavaliunas *et al* presents several modelling articles showing that the Swedish strategy would result in large parts of the population (specifically Stockholm) being immune by May 2020. As such, herd immunity would be relatively close and therefore help to explain the reduction in cases during late spring. However, the authors fail to present that these models were heavily criticised (not least by Prof. Tom Britton who authored one of the modelling studies) when empirical studies showed that only 7,3% of the Stockholm population had been exposed to the virus at the end of May [5]. As such, another important element of the rhetoric surrounding the strategy can be questioned namely that although the Swedish strategy had resulted in many deaths during the spring, other countries would "catch up" during the autumn given that some form of herd immunity had been achieved. However, as is seen during the autumn of 2020, both Swedish morbidity and mortality rates are again higher than its neighbours.

Fourthly, given that the second wave of Covid-19 during the autumn in Sweden so far does not seem to be milder than in other comparative countries, the conclusion in the article that "*it is possible to conclude that 'flattening the curve' has been successful*" is dubious at best. It is true that mortality and morbidity cases reduced during the summer. However, so was the case in all comparative countries and this is most likely due to a combination of summer holidays (for both workplaces and schools), increased socialising outside and in particular warmer weather [6]. On the contrary, Kavaliunas *et al* present no empirical evidence nor statistical calculations that show that the Swedish strategy has contributed to "flattening the curve". I would argue that making claims about the success of a risk management strategy without empirical evidence is a very serious problem in this article.

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Lastly, the authors highlight that the WHO have proposed that the Swedish model may be a plausible future strategy for other countries. Although there are elements in the Swedish strategy that in hindsight were correct (for example keeping schools open for younger age-groups) and that are now proposed to other countries, the authors fail to admit that the WHO have also criticised Sweden. For example, criticism has been raised regarding the failure to follow recommendations regarding testing and tracing in the early stages of the pandemic [7] as well as Sweden's continued defiance in following the evidence-based knowledge regarding the use of face masks in healthcare settings [8]. When reading the article, it seems as if the WHO are unreservedly positive to the Swedish strategy, which is clearly not the case.

In summary, the article *Swedish policy analysis for Covid-19* is unfortunately heavily biased and often lacks the weighing of different perspectives or views. It also fails to acknowledge that the Swedish strategy was in fact not evidence-based. In fact, neither the overall strategy, nor parts of the strategy, were evidence-based simply because there was no knowledge or evidence surrounding Covid-19 when the strategy was implemented. As such, it cannot be evidence-based. In such situations, when societies are faced with a new risk with high levels of uncertainty, the true evidence-based approach is a deontological strategy in similarity to the management of Covid-19 as seen in a majority of other countries. It is symptomatic for this article that the acknowledgement of this basic understanding of the management of rare societal risks is lacking.

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