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Title: Low quality, stigmatizing research prevents effective responses to covid-19

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Abstract:

Evidence-based responses to covid-19 must be based on high-quality research. Low-quality research is particularly dangerous when it feeds into stigmatizing narratives, including blaming immigrants for spreading infection.

Studies about COVID-19 and immigrants are guaranteed media attention in Norway

In recent weeks, we have seen immigrants blamed for covid-19 infections, blame that is shaped by broader (and stigmatizing) ideas attached to specific ethnic groups. A prescient example is *The Journal for the Norwegian Medical Association's* recent publication of "COVID-19 infections in Oslo's neighborhoods," by Sjøgaard and Kan on 25 March 2021 (1). The authors conclude that immigrant saturation alone, and not socioeconomic status or high-density residency, is a risk factor for infection in Oslo. A thoughtful critique by Aambø was published parallel to the Sjøgaard and Kan article (2). Aambø drew attention to the authors' heavy explanatory focus on individual behavior over established structural factors, such as the stress of discrimination, and the individual expression of stress through allostatic load.

The choice to emphasize individual, rather than structural, factors to explain high infection rates amongst immigrants is symptomatic of a larger problem of low methodological quality in much of the recent covid-19 research -- and the destructive narratives such research bolsters. As early as May 2020, systematic reviewers raised flags about massive amounts of poor-quality primary research on covid-19 that was being published at record speed (3). Health policymakers require high quality evidence in order to design effective infection prevention and control strategies. In Sjøgaard and Kan's article, both the analysis and the conclusions are incorrect. We draw attention to three particularly problematic areas.

Incorrect analyses

Sjøgaard and Kan choose to "predict" infection rates in Oslo neighborhoods by analyzing data on three highly collinear (i.e. statistically similar, $r = 0.9$ for all) variables collected during the same time period -- immigrant saturation, SES, and high-density residency, the latter combining overcrowding and multiple-generation homes. There was no a priori hypothesis or analysis plan reported, no proof that the authors were not simply "p-fishing", no N reported of their independent variables, and it is not clear the aggregate data they used are even describing the same people. By inputting three highly related variables into the same regression, the authors essentially measure the same construct multiple

times, and the resulting over-fit model is incapable of differentiating the influence of one independent variable over another. The classic risk of multicollinearity is that some variables will mistakenly appear to be statistically insignificant in the resulting analysis, while a sole variables will appear significant. In the case of Søgaard and Kan, two of the variables (SES and high-density housing) appear insignificant and only one appears significant (immigration saturation). Despite acknowledging that they have used statistically identical variables in their analysis, the authors nonetheless incorrectly choose to report that immigrant saturation alone is correlated with infection, and that low SES and high-density residency are not. In the most forgiving interpretation, Søgaard and Kan's bivariate analysis demonstrates unfortunate but unsurprising associations between infection and three similar indicators of neighborhood deprivation: immigrant saturation, low SES, and high-density residency.

Discriminatory speculations

Second, Søgaard and Kan connect incorrect findings to spurious and stigmatizing conclusions. After making an unfounded connection between immigrant saturation and infection, the authors suggest that increased infection is in fact caused by (not only associated with) "cultural/ethnic background", "other norms for physical and social nearness", and "systematic misunderstandings of infection and containment measures". Stated more directly, the authors, while having no basis in their data for such a claim, suggest that Oslo neighborhoods with high immigrant saturation are infected because immigrants do not understand hygiene or social distancing, and because they are ethnically and culturally different. The authors have made an unquestionably discriminatory accusation against different ethnic groups.

Dangerous policy implications

Third, the authors conclude by writing that they have "rapidly and simply identified associations that can be useful for health authorities". Herein lies the most problematic aspect of the research: it may very well be used by policy makers. The logical policy response that follows from the study is to focus on immigrant saturation, rather than other, underlying factors such as poverty and overcrowding. This "immigrants drive infection" narrative was propagated by Norway's major national and regional media channels within 48 hours of journal publication (including [Aftenposten](#), [VG](#), [Vårt Oslo](#), and [Nettavisen](#)). In times of health crises, when it is necessary to quickly plan robust and effective public health responses, it is essential that the evidence on which such responses are based use transparent and appropriate analytical methods, and that they do not fall prey to stigmatizing explanations that have no bearing in the data. We are surprised that Søgaard and Kan's article was published.

1 **Declarations**

2 All authors have approved this manuscript and declare no competing interests. No data is available to
3 share. No funding was received.

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