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Yes! More research is needed; but not just any research.

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Rightly so, the third and final overarching recommendation of the WHO Commission on the social determinants of health is a call for more and better research[1]. It calls upon the international community to "Measure and understand the problem and assess the impact of action". While I rejoice that the Commission has elected to give that much visibility and significance to research as an instrument for "Closing the gap in a generation", I am a little disappointed by the fact that research on interventions has not been given more importance in this general call for increasing knowledge. Indeed, I am afraid that this call will result in more descriptive studies documenting the social gradient of health for yet another health indicator, social stratification index, or geographically identifiable population. While the Commission's report attempts to engage the international community in challenging action, its call to the scientific community remains somewhat conservative. I do not think that we need more measures of the problem, especially in Western societies. We certainly need to understand it better, but first and foremost, we need to develop knowledge about how to act on, and solve the problem.

For the past 30 years, since the publication of the Black Report, evidence about the social gradient of health has been replicated for several health indicators, ranging from risk factors, to health care access, to health and health care outcomes, and a great variety of index of social stratification, in almost all western societies. Although there are some exceptions to the general rule, such as the higher incidence of breast cancer in women of higher socio-economic status, results are extremely robust. The social stratification of a society is reflected in the health of the groups defined by this stratification in such a way that those at the top of the hierarchy usually enjoy a better health than those directly underneath, and so on until the bottom of the social ladder. This is indeed one of the most robust evidence upon which founding health policies and programs. We do not need any more research to document another petty association. What we need, if we really want to further describe the phenomena is longitudinal analyses. We need to answer the question of whether social health inequalities are increasing or decreasing in any given society. This is a key question, unfortunately it is a much more difficult question to answer for two reasons. The first and most obvious one is the lack of appropriate data. There exist only a handful of jurisdictions for which individual socio-economic data can be linked to any kind of health data (vital registries, health care utilisation and so on) on a population basis. The second is the necessity to conduct a societal debate on what constitutes unjust inequalities. Indeed, there exist numerous indexes to appraise inequalities in a distribution and each of these indexes is related to a specific conception of social justice. Because any time comparisons may lead to different conclusions depending on the index used, conclusions of evolutional studies are linked to a conception of justice that qualifies inequalities.

I fully agree with the Commission that we need more research to sharpen our understanding of the mechanisms by which social inequalities transform into health inequalities. To do this however, we need to engage more broadly with colleagues from other disciplines. If we want to go beyond merely documenting the strength of the association between social and health variables, we need to develop theories and models that explain how social conditions affect physiological processes and health. There are a handful of explanations that are being explored in relation to either behavioural/cultural hypotheses that point to lifestyles and social support as primary mechanisms or structuralist/materialist hypotheses that identify resources, their distribution and stress as key to the social stratification of health inequalities. These are important areas of research because they orient actions in very different

directions. What is too rarely acknowledged, is that these two broad sets of hypotheses are closely related to the structure/agency dualism that our colleagues from the social sciences have been struggling with for a century. Aetiological research on health inequalities and the social determinants of health require key collaborations not only between social and public health researchers, but also with a very broad range of health scientists from psychologists to molecular biologists in order to correctly identify the mechanisms to be targeted by public health interventions.

At the end of the day, the contribution of all this research will be only marginally significant in our endeavour to "Closing the gap in one generation". There is a pervasive illusion in public health that once we know what to intervene on, we know how to do so. What we forget most often is that descriptive as well as aetiological research is limited; they can only provide a theory of the problem. What practitioners and policy makers need in order to respond positively to the Commission's imperative is a theory of the solution. This is not just a theory of the problem translated in action terms. Indeed, as decades of counterintuitive evaluation results have shown, there is often little correspondence between the knowledge of mechanisms by which health is produced and that of how to tinker with those mechanisms so they produce more health or more health equity. Unfortunately, this kind of population health intervention research is sorely lacking[2]. A recent study based on the grant proposal database of the Canadian Institute of Health Research (CIHR), the main funding agency for health research in Canada, has shown the depth of our research deficit regarding how population health interventions work[3]. Between 2001 and 2006, only 6 % of research proposals out of the 1244 population health research grant proposals received by CIHR were related to interventions. But what is even more disheartening is that the success rate of this handful of projects was half the overall success rate. So, not only is the research community marginally interested in studying how to intervene on population health, those who are making the effort to maintain appropriate partnerships with intervening organisations that are developing intervention research proposals that test theories of the solutions are much more often unsuccessful in securing research funds than colleagues who are doing descriptive or aetiological research.

Yes! Research is needed to address the pressing problem of social health inequalities. But no! We do not need more of the same research describing the problem as we have been doing for the past 30 years. What is required is a drastic shift in the scientific community to develop the capacity to conduct population health intervention research on health inequalities. This research necessitates the constitution of interdisciplinary teams that are working with organisations whose mandate is to intervene on health inequalities. This would be insufficient, because intervention research that studies theories of solution seems to be much more difficult to get funding for. What is really required is the courage and political will to address head-on the problem of our lack of knowledge concerning the actions that will effectively lead to reducing health inequalities. The Commission's report constitutes a sounded and evidence-based theory of the problem. Still missing, however, is an equally evidence-based theory for the solution.

References

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