Letters to the Editor

Epidemiology on the side of the angels ... or the people?

From ANNA-KARIN HURTIG and MIGUEL SAN SEBASTIAN

We thank Siemiatycky for his commentary on our study on cancer incidence in relation to residence near oilfields in the Amazon basin of Ecuador. His comments have helped us to reflect on the way we work in general and the above study in particular. We would like to share some points with the reader. He comments on three areas: strength of evidence, the replicability of the study, and the public health recommendations that can be made.

Firstly, strength of evidence: The chemical components of toxic wastes produced during oil activities are numerous and the health effects are documented only for a handful of these. Even less is known about the effects of these components in interaction with each other. This is the first study evaluating cancer incidence in relation to residence near oil fields and we found significant risks for several types of cancers, some of which have also been reported by studies on oil field workers and populations living near petrochemical plants. However, the exposure close to oil fields is different and we have to be open to the possibility that this exposure will cause cancer in other sites. That the differences in incidence between 'exposed' and 'non-exposed' counties are not bigger is not surprising. Most studies of risk factors in the general environment show only modestly elevated risks, but these effects may still be important from a public health point of view. We are well aware of that our study is no 'evidence' of causality but the study results are compatible with a relationship between cancer incidence and living in proximity to oil fields.

Secondly, replication of the study: Siemiatycky classifies the exposure of oil pollutants in our study as unique and non-replicable. This is not the case. Tropical forests and other fragile ecosystems around the world are booming frontiers for oil and gas development. Oil extraction occurs in numerous countries in the developing world such as Nigeria, Cameroon, and Peru with little control of the effect this has on the environment and health of the populations living close to these oil fields. How local populations are affected by oil exploitation activities is an enormous unexplored field of investigation; these include toxicological as well as cultural and socioeconomic effects. The question is what are the forces preventing such studies? These populations are often marginalized by ethnicity or geographically located far away from capitals and even further away from international academic institutions. Oil companies also present strong and vested economical interests. We therefore conclude that there is no lack of people exposed to oil pollutants; what is lacking are epidemiologists taking on the challenge to work with these communities.

Third, justification for public health recommendations: Siemiatycky questions that public health recommendations should be given on the basis of the study results as it does not provide 'evidence' of health effects due to an environmental exposure. Given the context, we argue that one should make recommendations, and that in doing so, the scientific credibility of epidemiology will not be compromised, rather the opposite.

The precautionary principle (PP) is a specified version of the more general principle of beneficence that obligates public health professionals to intervene to prevent harm to populations and spells out the general evidentiary conditions for those decisions in the face of empirical uncertainty. The PP is a key component of environmental and health policy decision making, particularly when complex and uncertain threats must be addressed. The PP has been defined as ‘when an activity raises threats of harm to human health or the environment, precautionary measures should be taken even if some cause and effect relationships are not fully established scientifically’. The ideal world according to Siemiatycky appears to be one where epidemiologists stand clean and far away from public health practice, moving further and further away from the public he/she is supposed to serve. This is truly a very sad development. This divorce should not be encouraged and is not desirable in any setting and particularly impractical in developing countries. The epidemiologist working in these countries has to conduct epidemiological studies, give public health recommendations, and set up control programmes. The two latter activities by nature will many times be based on insufficient evidence. What we have to discuss in order to move forward is how data can be interpreted and recommendations given when conditions are unfavourable.

Siemiatycky realizes that ‘we must make allowances for the resources and local conditions in which the investigators find themselves’. To us it means that a different approach to epidemiological studies is necessary where the context of the study, including the history of exposure and power relations, is explicitly included. The dialogue between scientists and communities will be essential for the credibility of academic institutions. The long-term commitment and combined efforts and expertise of all the partners involved can expand and refine the process, thus contributing to the health and well-being of the communities and institutions involved.

References

Response
From JACK SIEMIATYCKI

Hurtig and San Sebastián contest the three points I argued in my commentary¹ on their article.²

In response to my argument that the strength of evidence in their study was weak, they acknowledge that their results are not ‘evidence of effect’ but insist that the results ‘are compatible with a relationship between cancer incidence and living in the proximity to oil fields’. Yes they are. Indeed, results from most aetiological studies are compatible with relationships between disease and the studied factors. It is very hard to produce evidence which is incompatible with a relationship. Typically, the confidence interval on an effect estimate includes the possibility of a relationship. However, as argued previously,¹ it is also plausible that the ‘relationships’ observed in this study were the result of chance and/or bias. The study raises important questions, and it should serve to focus attention on the issue. This is why I recommended the publication of this report.

It is misleading to indicate that I classified the exposure circumstances of this study as unique and non-replicable. I pointed out that there is a continuum of uniqueness and replicability in epidemiological research, and that studies which are closer to the ‘non-replicable’ end of the continuum pose particular problems for epidemiological inference. With regard to this study, I questioned whether the exposures experienced by residents in the vicinity of oil fields in Ecuador are similar enough to those of other populations to provide the basis for assessing replicability of findings. I do not know the answer to this question; nor have the authors provided one. This is not an argument for avoiding research to replicate these findings, it is an argument for the importance of consideration of exposure in environmental epidemiology.

Industrial pollution in developing countries is an important, albeit difficult, domain for epidemiologists to address. The resources available for such research, both human and material, are indeed limited. The reasons for this are complex and deserve attention.³

Hurtig and San Sebastián maintain that the ‘precautionary principle’ (PP) provides the justification for public health intervention and that it ‘spells out the general evidentiary conditions for these decisions’. They cite a definition of the PP that contains no operational guidelines. It is not possible to engage here in an adequate discussion of the PP from a theoretical or operational viewpoint. I will only comment here on their caricature to the effect that I advocated for a divorce between epidemiology and public health. This is a gross misrepresentation. Epidemiology draws its motivation and its inspiration from public health. Nonetheless, epidemiology and public health are not synonymous. The Venn diagram circles of epidemiology and public health overlap. Some activities are clearly in the epidemiological research domain, some are in the public health domain, and some activities are in the intersecting grey zone. There are all kinds of bridges and tunnels between the epidemiological and the public health domains. Even when they are distinct, they are not divorced.

While I am sure they would decry such a perversion, Hurtig and San Sebastián’s position, if taken to its logical limit, could be used to justify dishonest science if it provides support for a particular public health intervention in which they believe. The exigencies of the public health domain should not distort the integrity of the epidemiological enterprise. Epidemiology must be rigorous, honest, critical, and self-critical. Otherwise it is not science. It must produce the facts, as it is capable of determining them. Some degree of uncertainty pertains to all epidemiological research, but not equally. The scientist must provide an honest representation not only of the findings, but also of the uncertainty attached to the findings.

But just as epidemiology should not be distorted in its scientific mission by pressures from the public health domain, nor should the limitations of epidemiology have veto power over public health measures that have other justifications. Public health authorities have the responsibility of developing policies based on epidemiological knowledge, and other pertinent scientific, social, and economic realities. Sometimes the decisions must be taken in the absence of epidemiological knowledge. In the case of the Ecuadorian oil industry, there may be strong and legitimate reasons for public health and other authorities to urgently intervene so as to alter the exposure of local populations. But it is not because of the results of this study, which, at most, provides a rationale for further investigation.

References


DOI: 10.1093/ije/dyg210

INRS—Institut Armand Frappier, Université du Québec, 531 Blvd. des Praines, Laval, Québec H7V 1B7, Canada. E-mail: jack.siemiatycki@inrs-iaf.uquebec.ca

DOI: 10.1093/ije/dyg211