AUTHORS’ RESPONSE

A further plea for adherence to the principles underlying science in general and the epidemiologic enterprise in particular

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In a recent Commentary in the Journal of the National Cancer Institute, we argued that false positive findings are common in epidemiologic studies of environmental and occupational determinants of cancer and that, given the non-experimental nature of epidemiologic research, this should come as no surprise.1 We presented several examples of likely false positives and called for, among other things, increased epistemological modesty in the reporting and interpretation of study findings, particularly when they are new, with little supporting biologic rationale, and arise from a plethora of comparisons.

Vineis, in an article in this issue of the journal,2 has misconstrued and misrepresented our recommendations for straightforward scientific skepticism and epistemic humility. (We will focus on the substance of the Vineis article and leave to readers the judgement about whether its underlying tone was appropriate to scientific discourse.) A number of his points obfuscate rather than clarify the issues we raised. Although Vineis suggests otherwise, we are well aware of the principles and practical issues involved in assessing causality in exposure-disease relations. Nowhere have we argued that epidemiologists ‘just restrict themselves to a mere description of “possible” risk factors’; in fact we encourage inferences about exposures as ‘causes’ of diseases, but only when such conjectures are based on valid, reliable evidence rather than on sporadic, unrelicated, ‘cherry-picked’ findings.3,4 We look forward to a cessation of the recent development in which epidemiologic studies cannot be criticized without initiating letters and other ‘thought pieces’ accusing researchers who express methodological skepticism or reservation of representing ‘the vision of some powerful groups with vested interests’, or being involved in manufacturing uncertainty.

Vineis’ attempts at the philosophy of science notwithstanding, we find his arguments lack coherence and clarity. He distinguishes cognitive (explanation) from practical (inference) concepts of epistemology, citing a paper by Russo and Williamson5 for this dichotomy, but we can find nothing in their work that is materially inconsistent with our Commentary.

Vineis suggests that there is a ‘tension’ between cognitive and practical objectives, and that expressed skepticism regarding the validity or reliability of epidemiologic research about the cause of a particular cancer must be sacrificed and give way to ‘intersubjectivity’. That is, he advocates conceding to collective opinion on issues of cause and effect, citing a committee’s consensus report on global warming, a topic far afield from our Commentary. Vineis darkly warns of ‘dangerous individualistic skepticism’, a phrase with disconcerting anti-scientific implications. While a detailed response to this anti-science sentiment is beyond the scope of this response, we note that skepticism, not ‘consensus building’, is intrinsic to the scientific ethos, and that science is not advanced by committee—only policy and politics are. Consensus is, at its core, a political process. Consensus opinion is an attempt to be evidence-based, but often reflects vested interests and various forms of advocacy empty of scientific merit.

We are well experienced with the evaluation process laid down for the IARC Monographs and other similar
organizations that render opinions on scientific matters, and in our Commentary we did not claim that these evaluations are made on the basis of single studies, as suggested by Vineis. Individual observational studies may, however, sometimes play an overly important role in such deliberations, particularly when authors of those studies are voting members of the decision-making committee. Systematic approaches, such as the ones used in the IARC Monographs and other agencies, are, if done carefully and critically, helpful in separating the wheat from the chaff in epidemiologic research. But committee reports and their conclusions in themselves should not be misconstrued as science; they are consensus documents and opinions with an eye towards closure. In contrast, science is inherently open-ended, provisional and tentative in its findings and conclusions.

Vineis also cites a recent paper by Vandenbroucke in which randomized trials are listed at the top of the hierarchy of study design validity when addressing ‘practical’ questions, although he ignores the fact that Vandenbroucke clearly had in mind study designs to evaluate the intended effects of therapeutic or intervention agents. Epidemiologic studies assessing the roles of environmental and occupational carcinogens cannot afford the luxury and advantages of employing randomized controlled trial designs. In environmental and occupational cancer studies, it is non-experimental epidemiologic studies that occupy the top of the hierarchy. Unfortunately, there is abundant evidence from the field of cancer prevention that what appeared to be strong and consistent effects in observational studies have not been confirmed when tested in randomized trials.

Finally, Vineis’ statement that ‘Claiming that most epidemiological findings are false positives seems to suggest that epidemiology overall is junk science’ (his terminology, not ours) is a stunning misinterpretation of our Commentary. Our own extensive research activities in epidemiology over the decades speak clearly against this groundless accusation. In contrast, our objective is to make epidemiology a stronger, sounder science by suggesting enhanced vigilance of its methods and a modicum of modesty when interpreting and reporting findings. In our view, overinterpretation of results of individual studies occurs frequently and damages not only the reputation of our discipline but also the advancement of knowledge in cancer etiology. While Vineis argues that the epidemiologic community is well aware of the problems we have raised in our Commentary and has developed tools to address them, recent empirical research and perusal of any epidemiology journal strongly suggests otherwise.

Paying lip service to the known and documented problems of non-experimental research is not enough. Vigilance is required to guard against the hubris that comes with any enterprise that enhances prestige and reputation, such as publishing in the scientific literature and the consequent media attention that often follows. The lack of an earnest appreciation of the inherent shortcomings and weaknesses of epidemiologic research serves only to provide opportunities for critics outside our field to claim that epidemiology often produces ‘junk science’, and there are many such critics waiting for the opportunity. It is the scientific and professional obligation of epidemiologists not to gratuitously provide such opportunities for our critics. If we do not evaluate the emperor’s apparel, others stand ready to do it for us.

References