Letters to the Editor

Take a walk on the wild side of social epidemiology

From JULIE CWIKEL

The recently published review of my textbook, ‘Social Epidemiology: Strategies for Public Health Activism’ by J. Michael Oakes has some serious misconceptions. Prof. Oakes’s own edited volume ‘Methods of Social Epidemiology’ (together with Jay S. Kaufman) was reviewed in this same issue by Carme Borrell. Discerning readers of both reviews might have the strong suspicion that Prof. Oakes found it difficult to provide a balanced review of my book while at the same time needing to promote his own. For a pithy, engaging evaluation of my book written by an unbiased public health observer, please see Cullen’s recent review.

While claiming to want to develop tools in order to address ‘the most important public health concerns’ (p. xxi), the methodological volume constructed by Oakes and Kaufman bypasses the difficult issues that social epidemiologists face when they move from descriptive or analytic epidemiology heavy on number crunching to accruing ‘robust evidence’ and translating it into intervention strategies, combining both the science and the art of public health practice. For this, the activist social epidemiologist needs theory, accumulated evidence of the efficacy of interventions in diverse populations (see for example, Chapters 9 and 10 on intervention theory and the uses and abuses of meta-analysis) and a selection of tools for the design, conduct and interpretation of public health interventions. Even Prof. Lisa Berkman, arguably one of the most prominent social epidemiologists today found that moving from descriptive epidemiology into cognitive-behavioural interventions to reduce depression and social isolation following myocardial infarction does not guarantee evidence of intervention efficacy.

It appears from his review that Prof. Oakes’s style of social epidemiology and public health intervention evaluation is limited to a quick peep at P values. Out of the hundreds of interventions evaluated in the book, he questions the evidence base of two seminal studies, chosen in the introductory chapter for their theory-based interventions addressing difficult public health problems among hard to access populations, as well as their program efficacy. Prof. Oakes narrowly assessed the impact of the ASAP (Alcohol Substance Abuse Prevention Program), where a series of evaluation studies were undertaken on a group of adolescent students selected from over thirty schools and communities in New Mexico and where a quantitative evaluation of perceived risk provided evidence of a statistically significant treatment effect. This ground-breaking intervention study was the inspiration for many subsequent substance abuse community trials (note its high citation rate in Google Scholar—195!) as well as the participatory, community methods featured in Chapter 10 ‘Community-based participatory research’ (CBPR) written by Lantz, Israel, Schulz and Reyes in Oakes and Kaufman’s very own book.

Indeed, the Lantz and co-authors conclude in this CBPR review in response to the question ‘Does CBPR work?’: ‘In a recent evidence-based review of the CBPR literature related to health…found evidence of enhanced research quality in eleven of the twelve completed intervention studies reviewed…and improved intervention outcomes in two studies’. Is this ‘evidence of actual harm reduction’?

Prof. Oakes also mistakenly characterized the study of an outreach program (Sex Industry Study—SIS) that succeeded in reaching street-based prostitutes in the early years of the HIV epidemic in the San Francisco Bay Area as an ‘observational, mostly qualitative survey’. He failed to recognize that this was a mixed methods (combining between qualitative and quantitative data collection) program evaluation, designed to document how sex workers were enrolled, interacted with staff, their risk attitudes and behaviours and to devise methods to help them reduce their STD risk. The methods developed in this study became the basis for delivering AIDS prevention and testing in other San Francisco Bay Area counties (as stated clearly on p. 11 of my book) as well as in numerous locations around the world.

Furthermore, Dr Oakes’ views the use of non-probability (i.e. purposive) sampling methods as evidence of lack of rigour and empirical support. It has been acknowledged that the construction of a probability sampling frame for persons who are living and working on the streets, highly mobile and apprehensive of anyone who wants to get their real name or permanent address is exceptionally difficult. As Mutaner points out, hard to access populations are more appropriate for qualitative research methods than survey research in social epidemiology. Qualitative methods should be an integral part of social epidemiology, particularly in descriptive science and the art of public health practice. For this, social epidemiologists face when they move from descriptive or analytic epidemiology heavy on number crunching to accruing ‘robust evidence’ and translating it into intervention strategies, combining both the science and the art of public health practice. For this, the activist social epidemiologist needs theory, accumulated evidence of the efficacy of interventions in diverse populations (see for example, Chapters 9 and 10 on intervention theory and the uses and abuses of meta-analysis) and a selection of tools for the design, conduct and interpretation of public health interventions. Even Prof. Lisa Berkman, arguably one of the most prominent social epidemiologists today found that moving from descriptive epidemiology into cognitive-behavioural interventions to reduce depression and social isolation following myocardial infarction does not guarantee evidence of intervention efficacy.

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epidemiological studies which try to fathom why patients do not conform to public health or medical advice e.g., an issue that Prof. Oakes has grappled with in his own intervention trial. Interestingly enough, browsing through Dr Oakes’s other published work reveals that he and his colleagues chose to use a convenience sample (recognized as the least robust of all sampling methods) of fast-food consumers to test whether nutrition labeling is recommended. My view of social epidemiology and the appropriate methods for addressing the pressing global public health issues clearly differ from Prof. Oakes’s perspective. The ‘mixed message’ that is conveyed stems from my critical stance with regard to every methodological approach. In my book I debate the biases and fallacies associated with all types of research designs from analytic, empirical studies through to different types of qualitative research including among many other examples: the healthy worker effect, non-response in survey research, genetic determinism, faddism in research paradigms and caveats in conducting screening of populations. Each research methodology has its strong points and weaknesses and no one method is appropriate for all issues and populations that social epidemiology of 2007 should be able to address.

In this post-modern world we live in, there is room for more than one model of social epidemiology. There are many valuable chapters in Prof. Oakes and Kaufman’s edited book, but a full component of tools in social epidemiology requires the whole gamut—theory, evidence-based practice, methodological tools and above all, fairness in academic research and publishing.

References
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Response to Cwikel: keeping it real on the wild side
From J MICHAEL OAKES

I must make two points about Prof. Cwikel’s letter. First, it could be true that my assessment of her favourite studies is too tough and that my threshold for rigour is too high. I do not think so, but concede that I occasionally suffer my New England heritage. For the unfamiliar, when told ‘The sun rises in the east’, New Englanders tend to respond with ‘So far…’