Social epidemiology: no way back. A response to Zielhuis and Kiemeyer

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Sir—Zielhuis and Kiemeyer (Social Epidemiology? No Way1) inaccurately represent my position2 in the recent debate with Cooper and Kaufman.3,4 Contrary to the authors’ claim, I stated that the criticism of social epidemiology’s ‘lack of explanations’ could have been directed to other sub-specialties as well (e.g. psychiatric epidemiology). I also stated that ‘despite its current increase in recognition (e.g. Lynch et al.’s notable study on the long-term effects of economic hardship), social epidemiology is a contested discipline, as the relevance of studying social facts within epidemiology is still being intensely debated. Epidemiologists still clash over their discipline’s status as a socionatural science.’ Social Epidemiology? No Way seems to be the most recent round of a scientific and political fight that can be traced to the origins of the discipline.5 Yet, epidemiology is social by definition. The death of an organism is a biological fact, but dying from drinking contaminated water or from a gunshot wound is a social fact as well, making the study of population health a biosocial (or socionatural) science.6 The problem does not reside, then, in the adequacy of social (economic, political, cultural) explanations in epidemiology. Rather, it is the systematic shunning of social science that is surprising and alarming.2

During the 20th century, such an eminent epidemiologist as Milton Terris always considered epidemiology ‘social’ and thus found the term ‘social epidemiology’ redundant.7 However, growth in the substantive knowledge,8 academic departments (Michigan, Harvard), and number of scholars devoted to the study of social determinants of health justifies today’s separate term ‘social epidemiology’.8 Similarly, ‘mind’ has been an intrinsic part of psychology since its origins (Weber, Fechner, James), but not until the 1960s did ‘cognitive psychology’ blossom as a distinctive subspecialty within psychology, following the efforts of psychologists and non-psychologists alike (e.g. Simon, Newell, Tversky, Neisser, Chomsky).

Why, then, attack a central part of one’s discipline? Let me suggest an externalist explanation.9 Public health is a public good, and in societies dominated by private economic interests there is little incentive to promote it. On the other hand, clinical medicine is easily marketable (insurance, medical technology, hospital industry, pharmaceutical companies); just to highlight how heavily politics enters into our debate, among these industries we find some of the top contributors to US presidential election campaigns. In these societies, epidemiologists (upper-middle-class professional and managerial employees) enjoy professional status, funding, and influence, insofar as their substantive ties are closely linked to biomedicine. Who needs to get into messy controversial topics such as the health effects of poverty, income inequality, discrimination, violence, immigration, anti-union activity and patriarchy, which could jeopardize the status of the discipline, its funding and the influence of its practitioners? Not surprisingly, prominent epidemiologists would like to leave the study of the social determinants of health to other disciplines.10

Zielhuis and Kiemeyer are also incorrect when they imply that I attribute the failure of identifying underlying mechanisms exclusively to social epidemiology. It is precisely the authors’ recommendation that ‘epidemiologists can use social determinants of health (income, stress)’ without further justification that I find most questionable.2 My criticism was extended to epidemiology as a whole and even to medical sociology. In the area of medical sociology, I mentioned a study that considered ‘education’ a perfectly exogenous variable. Almost as disturbing as the continuous use of ‘race’ as an implicit biological category, is (as I commented) the commonsense belief among US ‘baby boomer’ academics that social inequalities in health would be
eliminated if everyone had the ‘talent’ or ‘will power’ to obtain a PhD. This assumption is common in epidemiological studies of drug use among minorities, welfare recipients and the homeless. Rather than looking into the social mechanisms underlying the associations with ‘race’ and ‘education,’ some epidemiologists persist in the pragmatic use of indicators that reinforce lay myths (i.e. that race is a biological category; that those who cannot make ends meet have some intrinsic deficiency such as ‘low intelligence’ or ‘laziness’). If any MD behaved with similar ethics towards a patient, she could be sued on the spot. But in a few areas of epidemiology, research with poorly conceptualized indicators that either justify health inequalities or promote the removal of health benefits in oppressed populations (welfare recipients) is rewarded with funding, publication and prestige.

Not surprisingly, epidemiology’s official philosophers seem to care only about interpersonal micro ethics of interest to clinicians, such as clinical trials, euthanasia, and genetic counselling, while macro-ethical issues such as the health effects of racism, class, war, or exploitation are seen as ‘off limits’. Thus, social epidemiologists often double as philosophers and historians to defend their discipline (e.g. Krieger, Kaufman).

As would happen in any other field of study, it is difficult to disagree with the criticism of some of what passes as social epidemiology—for example, the exaggerated aetiological claims that often accompany findings based on psychosocial constructs measured with self-reports (‘control’, ‘sense of coherence’, ‘hostility’, ‘perceptions of inequality’ and the like). However, rather than scorning social epidemiology as a field, we should recognize the contribution of the many epidemiologists who devote their careers to the study of social determinants of health. Among them we find sociologists such as Amick, Williams, Eaton, House, Link, Dohrenwend and Schwartz; epidemiologists such as Susser, Syme, Rose, Cassel and Tyrolet (the fathers of the discipline); and others that have contributed decisively to the advancement of such studies (Szklo, Comstock, Shy, Terris). In addition, social epidemiologists advanced our knowledge in the hard days of late 1970s and 1980s, when everything ‘social’ was under attack (Davey Smith, Kaplan, Marmot, Sorlie, Stansfeld, Morris, Arber, Blaxter, Macintyre, Wing, Berkman, Wilkinson and Johnson, among others).

The sharp separation between disciplines recommended by Zielhuis and Kiemens is untenable. A scientific discipline that cannot find closely related disciplines is most likely non-scientific (e.g. astrology, parapsychology). Thus epidemiologists in the last decade have successfully incorporated concepts (income inequality; e.g. Lynch, Wilkinson, Kennedy, Kawachi), measures (social class, area socioeconomic position, racism, work organization; e.g. Krieger, Lynch, Landsbergs) and statistical methods (multilevel analysis; e.g. O’Campo, Diez-Roux, Yen) from the social sciences.

Finally, let me disagree with the authors’ own characterization of their article as ‘controversial.’ In order to be controversial one needs to go against the grain. The authors’ views are in fact consistent with epidemiology’s conservative leadership.

Luckily, social epidemiology is too developed and its substantive topic too important to retreat into oblivion.

References

Alcohol consumption and plasma homocysteine: What’s brewing?

Sir—We would like to complement recent publications on a potential beneficial health effect of moderate alcohol consumption on the cardiovascular system with data on the relation between alcohol consumption and the plasma total homocysteine concentration (tHcy). A high tHcy is associated with an increased risk of cardiovascular diseases, therefore, it is important to know how lifestyle factors might influence tHcy.

Observational studies indicate that alcohol consumption might be related to tHcy in a J-shaped fashion:3 alcoholics have a very high tHcy and moderate alcohol consumers (≤4 glasses/