Invited Commentary

Invited Commentary: Paths and Pathologies of Social Epidemiology

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The article by professors Galea and Link in this issue of the Journal (Am J Epidemiol. 2013;178(6):843–849) is an important contribution to the field of social epidemiology. Their 6 paths provide ample fodder for reflection, debate, and advancement. Although I agree with the thrust and spirit of their recommendations, I argue that if social epidemiology is to advance and become not just more popular but also more useful, we social epidemiologists need to first address our disciplinary pathologies. Among other things, we must embrace macro-to-micro transitions, understand and act on the principles of effect identification, conduct many more experiments, and train students to be not researchers but scientists.

causality; dynamic; identification; simultaneity

As expected, the article by professors Galea and Link (1) offers profound insights about social epidemiology. Their reflections and 6 paths forward provide a helpful springboard for scholarly meditation, debate, and advancement. My goal here is to further the dialogue about the best path (or paths) forward. Like Galea and Link, I hope that what follows strengthens the foundation of the subdiscipline and especially helps neophyte social epidemiologists to better understand and improve population health.

Nearly a decade has passed since the publication of Kaplan’s important article, “What’s Wrong With Social Epidemiology, and How Can We Make it Better?” (2), and yet nearly everything he wrote remains relevant today. Among other things, Kaplan grappled with whether all epidemiology was social epidemiology, discussed the need for better theory, articulated key methodological shortcomings, and insisted that mechanisms required elucidation. Given Kaplan’s acute insights, it is disappointing to realize that nearly a decade later, Galea and Link rightly needed to revisit and rearticulate many of the same concerns. What is different, however, and perhaps more helpful about Galea and Link’s article is its prescriptions for advancement. I focus my attention accordingly.

I completely agree with the central thrust of Galea and Link’s prescriptive paths. Social epidemiologists must continue to 1) focus on upstream causes of disease, 2) improve their methodological toolbox, 3) illuminate causal mechanisms, 4) learn from observational designs, 5) provide evidence for mitigating inequities, and 6) develop and test better theoretical models. I also agree that our training of the next generation of social epidemiologists must emphasize a story of pathogenesis that links global socioeconomic forces to groups, persons, organs, and cells; a critical evaluation of methodological assumptions; and a deep questioning of our project and products.

Because total agreement would be unhelpful, I now most humbly offer a bit of my own perspective on various paths for—actually, pathologies of—social epidemiology. First, all of epidemiology is not and cannot be social epidemiology. What makes social epidemiology a distinct subdiscipline is its focus on the social. That is, social epidemiology is distinct not because it focuses on social problems but because the exposures or treatments of interest entail social interactions, institutions, and other arrangements, such as families and gangs. Social epidemiology is different from, say, psychiatric epidemiology because whereas the former focuses on how social arrangements affect health, the latter focuses on illnesses of the mind/brain. Prescriptively, social epidemiologists must always ask, “How do our current social arrangements affect health, and what are the expected consequences change?”

Second, social epidemiologists should study the impact of macro-level forces, such as global trade, religion, and war, on health. However, such research must appreciate that macro-level forces are a function of micro-level forces, such as thoughts, behaviors, and individual actions. I believe that social epidemiology is struggling because scholars have yet to embrace...
the idea that macro-level forces affect persons (e.g., socialization) and persons affect macro-level forces (e.g., collective action). This bidirectional macro-to-micro transition is fundamental to the understanding of social systems and the success of social epidemiology. Too many scholars mistakenly assume that all macro-level forces are exogenous to individual actors and circumstances. Simply put, cultures make people and people make cultures. Ignoring either structure or agency will inhibit progress. Prescriptively, we must develop theories and methods that incorporate the simultaneity of such forces.

Third, social epidemiologists must embrace the ideas of effect identification and the limits of their reach. Not every interesting question is answerable, and too often social epidemiologists answer the wrong question (3). The fact is that the identification of independent social effects is virtually impossible without strong research designs and/or strong assumptions, most of which are neither testable nor clearly articulated. I believe social epidemiologists all too often ask fundamentally unidentifiable questions (4). The result is ambiguity in results and/or politically motivated conclusions that are not supported by data, a point I have tried to make with respect to neighborhood effects. Within the past 10 years, the basic criteria for effect identification have been clearly articulated. Devoting increasingly scarce resources to fundamentally unidentified questions endangers the discipline. Related issues include an appreciation for the limits of multiple regression adjustment, the problem of structural confounding, and the ongoing misunderstanding of statistical estimation and $P$ values. Prescriptively, we should ban the word “association” and speak only of correlations and, where appropriate, causal effects. I also think a 5-year ban on social epidemiologic grant applications that rely on multiple regression models would greatly enhance our collective creativity and insights.

Fourth, although I agree with Galea and Link that experiments are difficult, I believe that if the discipline is to thrive, social epidemiologists must do more of them. I have repeatedly argued that group-randomized designs are the canonical research design for social epidemiology. However, smaller-scale experiments are also valuable. As to the latter, I offer the insights of the video-based factorial designs of McKinlay et al. (5) and Rossi and Anderson’s method for illuminating factors that drive social judgments (6). What is more, it is only by doing things that we learn about potential pitfalls of imagined interventions. Consider the Moving to Opportunity Study (7). Apart from treatment effect estimates, we learned that when given a choice, a large number of people did not move out of poverty-stricken neighborhoods; that some target communities reject their potential new neighbors; and that of those who actually moved, many moved back to their original neighborhoods. In the face of such facts, estimates of attributable fractions, neighborhood effects, or other such measures derived from observational designs appear fanciful. Finally, when experiments are impossible, social epidemiologists interested in etiology or policy recommendations should imagine the ideal experiment their observational design aims to mimic—and it always aims to mimic one—and analyze their data accordingly. I will go so far as to say that this methodological approach is the most important advance in epidemiologic methodology in the past 20 years.

Finally, if social epidemiology is to thrive, the next generation of social epidemiologists must be trained to be scientists, not mere researchers. The distinction is critical. Researchers seek evidence to confirm what they believe to be true; they are outcome-focused. By contrast, scientists seek to discover the truth, regardless of the outcome. Scientists are passionately about the direction or statistical significance of their findings. Provided the methods are sound, the result of any scientific study is a contribution. When it comes to training, I maintain that confirmation bias is the greatest threat to social epidemiology. The subdiscipline will not—actually, must not—contribute to serious policy discussions until our science improves. No matter how disquieting, theoretical and empirical findings that run counter to hopes and dreams must not be ignored. Among other things, Gottfredson’s alternative explanation for health disparities merits attention and a more rigorous scientific response (8).

In summary, my very distinguished friends have given us a lot not only to think about but also to do. Let us hope that 10 years from now, a new generation of social epidemiologists will look back and legitimately conclude that progress was made.

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