as to how culture might become the third leg of the social epidemiological stool.

Conclusion

Eckersley provides a thought-provoking synthesis of a wide range of findings. Many will probably remain unconvinced. Showing evidence of changes over time in population averages of psychosocial factors such as anxiety, depression, and sense of control does not lead to the conclusion that cultural changes are responsible. As he notes, changes in culture may be the by-products, rather than the determinants, of changing rates of mental distress. There are other non-cultural explanations with which to compete. However, the stark spatial and social patterning of all health behaviours is consistent with the possibility that cultural software is a contributor. Currently, epidemiologists lack even basic knowledge about cultural trends. The measurement of those trends will have to move beyond aggregating survey responses if we are to proceed. More importantly, social epidemiologists will be required to abandon either/or explanatory frameworks when considering the role of cultural factors relative to material deprivation. Envisioning how culture and material conditions interact synergistically will prove difficult for epidemiology, but may yield significant improvements in our ability to account for population patterns of health.

References

fundamental social and political processes that unfold over time, in particular places, and in response to certain problems and challenges. Culture is not in this view independently causal or deterministic. It is highly contingent on the forces of history and especially vulnerable to the applications of social power. To employ the culture concept as an explanatory factor, then, one must also be clear as to the nature of these historical contingencies. Is it Western culture that makes people sick, or certain historical processes—capitalism, for example—that leads to the empowering or salience of certain ideological systems, which are then identified as potentially pathogenic? If so, why not then talk about capitalism and leave culture out of it? Put most directly, what does a cultural explanation add that a more specifically social one does not? Does, as Eckersley argues, ideas about ‘cultural fraud’ or ‘individualism’ offer us much more explanatory power than, say, locating pathogenic processes in consumerism?

Cultural explanations also founder when faced with the facts of social inequality. Individuals, social groups, communities, ethnicities, genders, all stand in unequal relationships to the engines of cultural change. Simply put, some people and some groups have more power over the content of culture than others. This power, often as not implicit to the circulation of ideas, justifies certain principles that are consistent with the social and economic interests of the elite. The issue here epidemiologically seems to be not whether Western culture is broadly pathogenic, but how such cultural ‘risk’ is patterned within a population. Some groups undoubtedly benefit from individualism, for example, and thus probably do not experience its more deleterious effects.

In the same way that culture is not democratically constituted, it is not evenly distributed within a society. Especially in this era of rapid globalization it is very much an open question whether and to what degree different groups come to adopt, aspire to adopt, or reject the systems of meanings that circulate increasingly in the global system. Among the pressing research priorities in modern anthropology is understanding how locals—groups, co-ethnics, communities—construct cultural identities in the context of such transnational flows. Adoption, acculturation, assimilation, rejection, resistance, and transformation are all possibilities. In research conducted with Pacific Island migrants to the west coast of the US, for example, I found that cardiovascular risk seemed to correspond not to the adoption of features of American culture, but the struggle to maintain distinctly Samoan cultural practices and identity in a market economy. Is Western culture implicated in this process? Yes, but not in as straightforward a way as suggested in Eckersley’s essay. If elements of Western culture are indeed pathogenic, then it is clear from an epidemiological viewpoint that understanding the distribution of these elements, and how they are interpreted at the local level is a critically important research question.

Pushing this argument a bit further, it seems relevant to ask, paraphrasing the seminal work of Rose, whether elements of culture should be employed to explain the causes of incidence across populations, a cause of cases within populations, or make the difficult attempt to link the two. The idea of a ‘Western’ culture speaks to population-level analysis, but apropos of previous comments, culture is variably distributed within populations, and groups stand in various relationships of consistency, or inconsistency, with dominant cultural systems. To what degree does this influence disease risk either within or between populations? It is this kind of difficult analysis, merging analytical perspectives of structure and agency, which has dominated social science for much of the last three decades. In the context of bringing the concept of culture to bear on disease risk, Dressler and his colleagues have, with some success, attempted to identify cultural models as they are held by groups within complex class societies and to measure the degree to which individuals are consistent in their behaviour with these cultural models affect cardiovascular and mental health. As with the work I did with migrants, the results of this research suggest that what is pathogenic about culture is not only or even necessarily its content, but whether individuals are able given their social and economic situation to act in a fashion consistent with the values and beliefs of the groups to which they belong and with which they identify.

Rose’s primary point, of course, was to highlight and critique the different models of prevention predicated on individual vs population perspectives on disease causation. It is abundantly clear that shifting behaviours in a population, while less motivating to individuals and health care providers, is a more radical and potentially more useful approach to prevention. It is worthwhile, then, to ask whether culture is up to explaining the difference in the causes of incidence between populations, and, if so, consider whether it is either possible or practical to change cultural systems. I worry that the idea of ‘culture,’ especially if it is not carefully operationalized, does not in the final analysis contribute to prevention science. Instead, like the popular notion of social capital, the potential for blaming the victim looms large.

Eckersley, to be fair here, anticipates some of these challenges in his analysis. Although he tends to use the term ‘Western culture’ in a somewhat broad and unfocused way, it is clear from his essay that his thinking is directed more precisely to the pathogenic nature of those ideological elements that arose to buttress the growth and development of global capitalism—individualism and consumerism. This work is clearly consistent with emerging work in cultural epidemiology. The challenge, as I see it, is to move beyond the omnibus use of culture as an explanatory variable, to look more carefully at the specific elements of culture that are of suspected causal importance, examine variability in the adoption of such elements within population groups, and ask whether the degree to which individuals endorse such cultural elements modifies their risk for disease.

References
Author’s response: Culture can be studied at both large and small scales

Richard Eckersley

The cultures of scientific disciplines are like the cultures of societies: so ingrained that they appear to be the natural and right way to look at the world. Disciplines see things differently; they draw on different conceptual frameworks and approaches, which yield different evidence and interpretations.

I immediately identified with Glass’s account of why epidemiology has neglected culture.¹ It provides, I think, a strong intellectual buttress for my arguments. Janes and Dressler are gracious enough to applaud my attempts to integrate culture into the social determinants of health, but have, as anthropologists, serious reservations about how I have gone about it. Let me respond to some of these concerns.

Firstly, both attribute to me a more homogeneous or monolithic model of culture than I propose. I fully accept that culture is a fuzzy, complex, dynamic, and multifaceted thing. But I don’t agree that this is the only way to study culture.

Secondly, Janes and Dressler want us to focus on the details of population patterning and distribution, individual and group differences, and culture as local knowledge, and to explore culture’s health impacts at this level.² I have no difficulty with this approach—and, I thought, acknowledged this in my paper (much of my evidence drew on individual differences). But I don’t agree that this is the only way to study culture.

Instead of ‘zeroing in’, we can also draw back—far enough to discern the ‘large’ effects of culture on populations, for all its fuzziness, complexity, and variability. This seems especially important given the globalizing nature of the social, economic, and cultural forces shaping life today. We can draw an analogy with climate change. The anthropological view would seem to suggest that because the impacts of global warming will vary according to a range of regional and local factors it can only be studied at these levels. Obviously, this is not the case.

In offering their evidence based on studies of different ethnic groups to support their perspective, Janes and Dressler fail to address my evidence—drawn from epidemiology, psychology, and sociology. Anthropologists might dismiss notions that whole societies can be characterized by a few dominant cultural themes, but qualities such as materialism and individualism (or individualization) are common themes in psychological and sociological research.

If what Janes and Dressler say is true, what are we to make of the observation that in the new world countries of the United States, Canada, Australia, and New Zealand male youth suicide rates rose from almost identical levels in the 1950s through the 1960s to the 1990s, when they peaked and began to fall in all four countries?³¹ Or, as I mention in the paper, the strong correlations found between youth suicide rates in Western nations and several different measures of individualism, but not other cultural, social, or economic factors?²¹

Several new studies show what is at stake. Results of the 2001–03 US National Comorbidity Survey Replication show that almost a half of Americans will experience a clinical mental disorder during their lives, while over a quarter will suffer a disorder in any 1 year.²² The risk increases for successive generations; those aged 18–29 years have an estimated lifetime risk four times that of those aged ≥60 years. The researchers...