Epidemiology in a changing world: implications for population-based research on mental disorders

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Introduction and objectives. Population-based research on mental disorders needs to keep pace with trends in general epidemiology. At present, this requirement is complicated by uncertainty within the parent discipline about its future development. The present study examines proposals for new directions in strategy and methods and considers their significance for psychiatric epidemiology.

Method. Narrative review, cross-checked by search of English-language journals of epidemiology for new trends and developments reported in the years from 2000 onwards.

Results. The proposals reviewed here are divided into three groups:

1. A new research paradigm of ‘eco-epidemiology’, which includes both individual risk factors and macro-environmental systems that mediate population levels of health and sickness.
2. Improved ‘translation’ of research findings – i.e. more rapid and effective implementation of epidemiological evidence into health policy and practice.
3. Adaptation of epidemiology to a globalised economy, with firmer regulation of funding and resources.

Conclusions. Each of these proposals has implications for psychiatric epidemiology. Workers in this field, however, are still preoccupied by relatively specific problems of definition, measurement and classification, and so far the current debates in general epidemiology are scarcely reflected. The proposals outlined above call for:

- a working model of eco-epidemiology as it relates to psychiatric disorders;
- implementation strategies to encourage more active participation in epidemiological research by community health services and caregiver organisations;
- international collaborative projects that offer practical benefits in training and service facilities for the countries taking part.

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Introduction

There are good grounds to argue that psychiatric epidemiology should be recognised as an integral part of its parent discipline. Mental disorders constitute a major public health problem of our society, yet in many countries are still not taken seriously by policy-makers. The importance of this sub-specialty will be accepted only if it can adapt to current trends and advances in mainstream epidemiology. Long-term economic problems, moreover, will bring an increasing need to share resources with the wider epidemiological community and make common cause with its members.

The issue is, however, complicated by the fact that medical epidemiology itself is now at a crossroads,

and there is much argument and debate as to how the discipline can best meet the challenge of our times. According to Pearce (2007), the scope and vision of epidemiology have narrowed down, partly due to the boom in biogenetic research but also because of a fixation on methodology. Epidemiology, he declared, has become a set of methods, and epidemiologists restrict themselves to the kinds of question that can be answered by these methods: ‘It is as if the house is burning down, but we are focusing on developing better theories on how to change the light bulb.’ Such comments suggest a widespread dissatisfaction with current limitations of concept and method in the discipline.

This loss of direction is not unconnected with profound changes taking place in the global context of epidemiology. To begin with, human demography is undergoing a rapid transformation. Over the past 40 years, life-expectancy at birth has risen by a mean of

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12 years, world population has nearly doubled and the total is now predicted to reach 10 billion (10^{10}). Meanwhile, despite growing resistance to antibiotics and the rapid spread of infectious epidemics by modern transport systems, the spectra of global morbidity and causes of death have shifted broadly towards non-communicable diseases (Horton, 2012).

At the same time, prospects for human health are increasingly subject to 'planetary overload'; that is, the effects of man-made climate change, land degradation, depletion of natural resources and loss of biodiversity (McMichael, 1993). Hence public-health interests call for an ecological perspective, and increasingly for regulation of man's economic activities.

Epidemiology's role in preventive medicine, moreover, appears to be on the decline both in scientific achievement and in practical applications. Whereas in the 20th century big strides were made in the fight against disease, the case-control differences found in more recent studies have been mostly too small to provide a basis for public-health action. The demonstration of links with individual risk factors, so effective in the earlier research, seems unlikely to prove as successful with today's plethora of 'small risk' associations, given the complexity of the causal webs (Maziak, 2009). With respect to dietary, life-style and prophylactic medication, epidemiologists are uneasily aware that 'people don't take us seriously anymore, and when they do ... we may unintentionally do more harm than good' (Taubes & Mann, 1995).

The aims of this short review are twofold: first to outline some important trends towards change and renewal in contemporary epidemiology, and then to consider their possible implications for population-based research on mental and behavioural disorders.

Challenges for epidemiology: problems and proposals

A check of specialist journals in this field indicates four principal trends. One of these, a convergence of epidemiology and biogenetic research in complex aetiological studies, is treated by some authors as a substantive new discipline (e.g. Palmer, 2004), but by others simply as 'refinement and elaboration' (McMichael, 1994); this issue, however, is beyond the scope of the present review. The three trends considered here are as follows.

Eras and paradigms: a new conceptual approach

Susser & Susser (1996a, b) at Columbia University, New York, adapting Thomas Kuhn's theory of changing conceptual frameworks in science, urged the

need today for a new such 'paradigm' in epidemiology, to follow on from miasma theory and the sanitary movement in the early 19th century, microbial causal theories of disease in the mid-to-late 19th and early 20th centuries and the era of non-infectious disease epidemiology, with its 'black box' model of research, up to the present day. The black box model, they argued, is now outdated and due for replacement. Instead the authors foresaw a new era of 'eco-epidemiology', which would encompass different levels of organisation, neatly compartmentalised one within another, like a conjuror's nest of Chinese boxes. 'The practical implication', they argued, '... is that an exclusive focus on risk factors at the individual level within populations – even given the largest numbers – will not serve. We need ... to be equally concerned with causal pathways at the societal level and with pathogenesis and causality at the molecular level.'

More recently, March & Susser (2006) enumerated the elements central to eco-epidemiology as follows: 'multiple levels of organization; the individual life course, as reflected by consideration of early antecedents and concepts of embodiment; equal consideration of communicable and non-communicable diseases ... the imprint of historical time, and the dynamic relationship between macro causes (e.g. societal change) and micro causes (e.g. genetic mutations).'

This highly ambitious scenario is flawed in a number of respects. Multi-level theorising, as indeed March and Susser remark, is not a wholly new departure, but was present in embryo throughout all eras of epidemiology from Hippocrates onward and explicit, in particular, in the writings of both Snow on cholera (infectious) and Goldberger on pellagra (non-infectious). The black-box model itself is not a scientific paradigm in Kuhn's sense, but simply a metaphor for empirical approaches to the prevention of diseases before their aetiology and pathogenesis have been worked out. 'Chinese boxes', another metaphor, is meant to symbolise the various organising systems active in human populations, from the societal to the genetic, but is problematic because actually neither these systems nor the data sets that can be used to represent them are arranged in such neat, orderly hierarchies. Furthermore, a schema that encompasses all such systems, including intra-cellular processes and human gene complements, is so overinclusive as virtually to negate the hypothetical-deductive method of enquiry.

In view of these weaknesses, the notion of an all-embracing multi-factorial causation has failed to gain wide acceptance. Nonetheless, there is growing awareness of the need to apply a more pragmatic

concept of eco-epidemiology, in which the study of individual risk-factors is combined with analysis of macro-environmental systems – whether natural or man-made – that mediate population levels of health and sickness. Thus, Koopman (1996) considered the discipline to be ‘in transition from a science that identifies risk factors for disease to one that analyses the systems that generate patterns of disease in populations’, and the implications have since been spelt out in terms of research design (McMichael, 1999), statistical analysis (Pearce, 2011) and strategies in preventive medicine (Rose, 2001).

Implementing epidemiological knowledge in policy and practice

Proposals for change have not been confined to concepts and methods of epidemiology, but have extended to the processes by which its conclusions are implemented. In the history of medicine, translation of new scientific knowledge has been traditionally a hit-and-miss affair, with few clearly established pathways and only a small minority of researchers willing to venture into the intervening ‘valley of death’ (Butler, 2008). Things may be changing, however, now that a new construct of translational medicine (TM) has been taken up by industry, national governments and many medical faculties. International symposia have been held, specialist journals set up, degree courses established and pharmaceutical divisions dedicated to advancement of this project (Henderson, 2011).

The purpose of TM as a research programme is to connect up the successive stages of knowledge transfer in product development, usually differentiated as basic scientific discovery, early human studies, early clinical trials, late clinical trials and implementation in practice (Thornicroft *et al.* 2011). This progression, based on the standard procedure for developing, testing and marketing new pharmaceuticals and vaccines, is in the main ‘product-driven’ and can be applied only in modified form to projects focused on community health and primary preventive measures. Nevertheless, the notion of ‘translational epidemiology’ has emerged as a distinct branch of TM, which contributes to knowledge transfer at each successive stage. Khoury *et al.* (2010) formulated the process as follows:

- at T0, a discovery is made (e.g. disease agent, biomarker, new vaccine, etc.)
- at T1, epidemiology explores the use of this advance in developing a candidate test or guide to medical intervention;
- at T2, epidemiology helps to evaluate the new approach by observation and clinical trials;

- at T3, epidemiology points to factors that promote or restrict applications in practice;
- at T4 finally, epidemiology helps to assess the public health impact of such applications.

This schema is logical as far as it goes, but actually is only the beginning. How and when new knowledge is put into effect in the real world will depend to some extent on policy-makers and service managers, but probably more on gradual changes in professional and public attitudes, mediated largely through unofficial channels (Ogilvie *et al.* 2009). Even so, the TM band-wagon is now rolling and has powerful backing. The US National Institutes of Health, for example, earmarked an annual budget of US\$500 million for TM research, while in Britain the Medical Council established six academic centres for the same purpose, including one for translational epidemiology (Woolf, 2008).

Adaptation of epidemiology to a changing world economy

This third trend is less clearly differentiated than the first two, but potentially the most significant, because it is directly related to the moral and ethical basis of public-health medicine.

We are living in an era of growing global inequality, fraught with heavy consequences for human society. Loewensen (2004) summed up the position a few years ago as follows:

“In the past two decades, global trade has tripled and trade in services has increased more than 14-fold ... Not all are benefitting from this change, however: globalisation under liberalized markets has generally benefitted the strong industrial economies and marginalized the weak. Average *per capita* gross national products (GNP) vary by a factor of about 12 between high- and low-income countries. Between 1960 and 1990 the poorest countries’ share of world trade fell from 4% to 1%. Debt consumes an increasing share of their scarce domestic resources, further reducing the scope for development ... The income gap between the quintile of the world’s people living in the richest countries and the fifth in the poorest has increased from 30:1 in 1960 to 74:1 in 1997... By the late 1990s the fifth of the world’s people living in the highest income countries had 86% of world GDP, 82% of world market exports and 68% of foreign direct investments, while the bottom fifth had 1% of these parameters”.

An imbalance so gross must pose serious problems for public-health medicine. The fundamental problem for low-income countries in particular is shortage of resources in money and trained manpower for health services and research, including epidemiological studies that could help to narrow the so-called ‘10/90

gap': that is, the chasm between their contribution to health research and their share of the global disease burden. The resulting disparities are especially flagrant for psychiatric disorders, because the poorest countries spend the lowest percentages of their overall health budgets on mental health (Saxena *et al.* 2007).

A partial solution to the dilemma lies in promotion of epidemiological research within global health programs, in which high-income countries bear the brunt of costs, while low- and middle-income countries benefit in terms of training and development (Razzouk *et al.* 2010). Although such projects are now being realised, in many instances they rely upon public-private partnerships in which government-supported agencies work directly with both charitable foundations and commercial groups to promote health care projects.

This trend accelerated following the 1992 UN Conference on Environment and Development (Richter, 2004), at which leading trend-setters from the World Health Organization (WHO) and United Nations argued that it was time for all 'stakeholders' in health care to move away from confrontation with one another, towards co-operation and dialogue. Since then, both WHO and the World Bank have collaborated extensively with non-governmental organisations, private institutions and the pharmaceutical industry (Nishtar, 2004).

The fact that such collaborations, capable of raising billions of dollars, are being targeted on epidemic diseases – including tuberculosis, AIDS, malaria and cholera – underlines their importance for the developing world, but at the same time also their ultimate dependence on population-based research. Such projects, however, are focused primarily on product development; that is to say, on making available effective medication, vaccines, diagnostic biomarkers and other technologies (Mahoney, 2011). Commendable as are these objectives, the context is one in which epidemiology – and in particular current moves towards eco-epidemiology – can easily come to be regarded as of only marginal interest.

For this reason, calls are growing louder for international programs in health research and development that will be truly independent of marketing influences. Richter (2004), for example, has advocated safeguards to ensure that public-health interests remain at the centre of all financial relationships and joint ventures between publicly funded agencies and the commercial sector or wealthy corporate donors. Concrete steps, in her view, should consist in:

1. replacing the 'partnership' appellation by 'agreement', 'interaction', or other more neutral terms;

2. identifying nodal points at which conflicts of interest are likely to arise, and
3. establishing firm regulations to ensure that the public interest remains paramount in all such interactions, including management and oversight.

Psychiatric epidemiology: still a backwater?

On turning to the mental health field, one is confronted by quite other research concerns and priorities. Here, attention is concentrated on issues of case-definition, classification and diagnostic validity. As Kessler (2000) observed over a decade ago, this branch of epidemiology traditionally lags behind mainstream developments because of its difficulties in conceptualising and measuring mental disorders, and in consequence remains largely descriptive. The current debates in general epidemiology are hardly reflected, or else take on different tones. The one major paradigm-shift in modern psychiatry – basically from developmental psychology to neuroscience – was satirised by Eisenberg (1986) as a switch from 'brainlessness' to 'mindlessness'. Translational psychiatry has emerged in its own new journal as a branch of biomedical informatics, or as the link between neuroscience and clinical treatment, but with very little bearing on public health.

In the developing world, mental disorders constitute a neglected and under-researched area for public-health research. A survey of studies in this field, covering 114 low- and middle-income countries, found that over half had contributed fewer than five articles each to a total of over 10 000 relevant publications in the period 1993–2003, even though participating research groups ranked epidemiological studies of illness burden and risk factors among their highest priorities (Sharan *et al.* 2007). Impressive progress, it is true, has been made in international collaborative research, exemplified by the WHO World Mental Health Survey (Kessler & Ustun, 2008) has yielded a mass of statistical information. The wide variation among countries, however, both in survey response rates and in reported prevalence and severity distributions, must raise questions about the validity of these data.

Conclusion

What then are the broad implications for psychiatric epidemiology? In important respects, it seems, this sub-specialty is indeed something of a backwater. In order to gain recognition as a scientific discipline it must keep pace with contemporary developments in mainstream epidemiology. This does not signify that

problems of psychiatric nosology should be abandoned; only that these must share our attention with the outstanding issues of causal analysis, disease prevention and health promotion, so that methodological and substantive research can advance in parallel. Here, certain issues are calling insistently for wider debate and exploration.

First of all, a working model of eco-epidemiology is required as it relates to the field of psychiatry: one that can take account of the impacts of overpopulation, environmental damage and growing economic inequality on the mental health of populations and point the way to public-health action, whether focused on high-risk group interventions or whole-population measures – or indeed on both.

Secondly, the implementation of new knowledge by such collaborations is likely to prove most effective where there is scope for active participation in the research by community health services and caregiver organisations at national or regional level.

International collaborative surveys, finally, should be jointly negotiated and agreed by all stake-holders. Such projects should always include training components for researchers in the host countries and be constituted to foster and advance the development of research infrastructures in these countries, especially the poorest.

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Conflict of Interest

None.

Ethical Standards

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