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Epidemiology—is it time to call it a day?

George Davey Smith and Shah Ebrahim

The advent of the new millennium prompted various discussions of the future of academic disciplines, including epidemiology and other public health sciences.[1–5] We missed this opportunity to make prognostications, but have chosen this first issue of 2001 for our initial editorial comment since, having succeeded Professor Peter Pharoah as editors of the *International Journal of Epidemiology*, this is the first issue which exclusively contains material which we have processed from the Bristol, UK, office. Of course Peter's remarkable record as editor of the *IJE* is difficult to follow, a fact reflected in the need for two of us to attempt to complete the tasks so admirably carried out by Peter alone.

Although millennia are clear socio-cultural constructs, their expiry provides an excuse for making sweeping statements about the past and future which natural reticence would (and probably should) constrain at other times. However because such phenomena can be considered as socio-cultural constructs does not mean that they do not have real effects: the symbolic power of centennial or millennial transitions allow for changes in the way the world is viewed which have concrete effects. In the case of the latest millennial transition the coincidence of the calendar shift with the announcement of the sequencing of the human genome[6,7] provides what we believe will be seen as a more than symbolic change in the way (wo)man views wo(man). There is much discussion in the lay and academic press which is clearly based on the notion that since 26 June 2000—the date sequencing of the human genome was announced—the world really has changed: new cures for cancer are coming soon; the selection of drugs and medical treatments will be tailored to the genome of the client; the development of chronic disease can be postponed by gene therapy; and the cause of all diseases will ultimately be revealed as genetic.

Viewed through DNA-tinted spectacles, is epidemiology a discipline with a glorious past, but decreasing relevance as description (epidemiological) is replaced by explanation (gene expression)? In 2001 would we be justified in advising a potential researcher committed to making a contribution to population health to enter epidemiology training rather than head straight for the nearest polymerase chain reaction console? As the official journal of the International Epidemiological Association the *IJE* cannot afford to skip this question—to which others would be keen to answer yes. The progenitor of one of the more remarkable (and influential) branches of late 20th century epidemiological research—the fetal origins of adult disease,[8] which features as a theme subject in this issue of the *IJE*—thinks that improved understanding of the cause of coronary heart disease will come from animal experimentation, not epidemiology.[9] What does that say to the rest of us who have made more modest contributions to our field? Taking stock of the past triumphs and tragedies of epidemiology may go some way to help provide an answer to our enthusiastic potential colleague considering a future in epidemiology.

Department of Social Medicine, University of Bristol, Canynge Hall, Whiteladies Road, Bristol BS8 2PR, UK.

Contributions and contexts of epidemiological advances

The successes of epidemiology can be considered with respect to particular health problems, or to its broader influence on the methodology of understanding and dealing with health problems. Here we consider five cases which we have selected because of their iconic status, because of the lessons they have for the future development of epidemiology, or both.

Cholera: getting it right for the right reason?

It is traditional to start considerations of the successes of epidemiology either with cholera or smoking, although the framework for thinking about the determinants of population-level phenomena such as death rates, fertility and population change was clearly in place by the early 19th century.[10] The term epidemiologist was apparently not used until the 1860s[11] and the profession only emerged in the early part of the 20th Century;[12] however, the foundation of the London Epidemiological Society in 1850 indicated that a notion of what epidemiology was existed by the mid-19th century.[11] Initially proposed as a society to direct anti-cholera activities—to be named the Epidemic Medical Society—in the event it was instigated as a society concerned with the cause and prevention of many diseases, and also with issues which today would be considered to be health services research.[11] Certainly the work of Thomas Proudfoot[13] and Henry Gaulter[14] during the 1832 British cholera epidemic and of John Sutherland,[15] John Snow[16] and William Budd[17] during the mid-century epidemic was of a type that we would now recognize as epidemiological, even though their contemporaries and more recent commentators pointed out the limitations of their methods.[18,19] Snow's work had considerable influence, which in the long term reaped public health benefits (although the actual influence of the famous removal of the Broad Street pump handle may have been less than has been generally supposed, see Figure 1).[20] Interestingly, bodies of empirical and theoretical work which reached conclusions we would now consider to be epidemiologically 'wrong' also had positive public health effects; those that thought cholera was caused by airborn miasma were as likely to demand appropriate sanitary reform as those who saw it as a contagious disease.[21] John Snow recognized this, accepting that anti-contagionist theories of cholera causation were *'in a hygienic sense … as useful as what I believe to be the real truth'*.[22] While 'correct' epidemiology does not guarantee the implementation of effective public health policy (and *vice versa*) it lays the ground for continuing advances in understanding which do yield benefits. Thus miasma (as

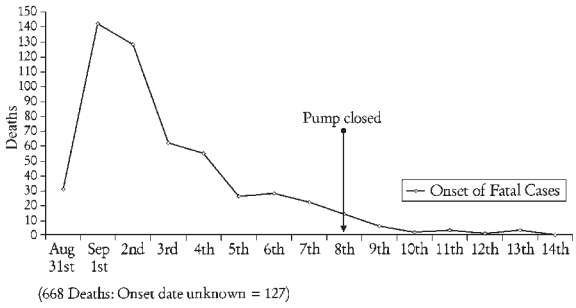

Figure 1 Cholera outbreak in Golden Square, Broad Street, London 1854. The pump handle was removed when the epidemic was waning and appears to have had no effect, although the Reverend Henry Whitehead, who produced these figures, thought that the closure of the pump may have prevented recurrence of the epidemic[20]

opposed to simple contagionist) theories of cholera prompted interventions which lessened the severity of future cholera outbreaks, but continued reliance on an unmodified miasma paradigm would have constrained the development of later thinking regarding the control of infectious disease (for example, the development of vaccinations or specific chemotherapeutic interventions). Being right for the wrong reasons can be fine as far as short-term public health outcomes is concerned, but can be a severe impediment in the long term. That John Snow and others were right for the right reason was an important advance for both epidemiology and public health.

Smoking: the end of an epidemic or new vistas for the little white slayer?

The widespread adoption of cigarette smoking did not reflect a huge increase in tobacco consumption, rather it occurred through a shift in the mode of production and marketing of tobacco. Each Bonsack cigarette rolling machine—introduced in the early 1880s—could produce more than 100 000 cigarettes a day, greatly increasing the ability of cigarette companies to make profits through selling huge numbers of cheap cigarettes (the per cigarette rate of profit fell, and this technology could only be introduced if consumption was drastically increased). Snuff, tobacco chewing and pipe smoking decreased during this shift in the way tobacco was used, rather than there being a big increase in total tobacco-use. Widespread uptake of cigarette smoking was followed years later by increases, from initially low levels, in lung cancer mortality. By 1935 Fritz Lickint discussed time-trend, ecological and clinical data which strongly implicated cigarette smoking as a cause of lung cancer,[23] and two ground breaking case-control studies were published in Germany in 1939[24] and 1943.[25] The second of these is translated into English for the first time in this issue of the *IJE*. The definitive case-control studies reported from the US and UK in 1950 provided strong confirmation of the earlier findings.[26–30] The distribution of occupational cancers had drawn attention to the environmental origins of adult chronic diseases long before,[31] snuff use had been identified as a cause of nasal cancer in 1761[32] and pipe smoking was a well recognised cause of mouth cancer by the mid-to-late 19th century.[33,34] However, the identification of the (relatively) low level and very long-term risks of a widely practised aspect of population behaviour was of enormous importance for how the causes of chronic disease were conceptualized and investigated, both in epidemiology and public health.[35] The slow but steady acceptance of the link between smoking and lung cancer—and later between smoking and other conditions, in particular coronary heart disease and respiratory disease (sufferers from which had been in the control group of some of the 1950 studies)—stimulated declines in smoking rates, followed by falling lung cancer mortality, in some countries. However the economic forces underlying the introduction of cigarette smoking also resisted the production and dissemination of knowledge that smoking kills,[36] and these companies are now aggressively marketing cigarettes in the lucrative and less resistant markets of newly industrializing countries, heralding future epidemics of diseases previously rare in these settings.[37]

Fetal origins of adult disease: the rebirth of social physiology?

The use of biological markers as indicators of the physical and economic health of a society has a long history. Writing in 1829 René Villermé stated that *'Human height becomes greater and growth takes place more rapidly, other things being equal, in proportion as the country is richer, comfort more general, houses, clothes and nourishment better and labour, fatigue and privation during infancy and youth less; in other words, the circumstances which accompany poverty delay the age at which complete stature is reached and stunt adult height'*.[38,39] Short stature was considered both an indicator of an unhealthy population and as indicative of a potentially catastrophic level of poor physical capacity in the event of military conflict.[40] Similarly birthweight was seen as a key indicator of population health; as one French authority wrote at the end of the 19th century *'From the viewpoint of humanity, from the viewpoint of the increase of the population, from the viewpoint of the evolution of the French race, it is necessary, it is urgent, that the public authorities intervene to protect the pregnant woman during the last three months of her pregnancy, and the fetus during the last three months of its gestational life'*.[41,42] The potential health consequences of poor intrauterine and early postnatal development have, indeed, long been discussed. In 1913 in her extraordinary book *Pedagogical Anthropology* Maria Montessori referred to birth weight as an indicator *of 'the hygiene of generation'* and the weight gain of the new born child as *'a valuable prognostic of the child's life'*.[43] More generally, the influence of early-life development on later health was considered common-sense wisdom in epidemiological circles in the first half of the 20th century.[44] For example in the report of a historical cohort study in 1944 Antonio Ciocco concluded that the findings of this study should *'reinforce the views held by many that disease in adulthood is often bought about by the cumulative effects over a long period of time of many pathological conditions, many incidents, some of which take place and are even perceived in infancy'*.[45] Ciocco was working at a time when much of social medicine was concerned with the influence of the social environment on the health and biological capacity of individuals; as one of its key figures, John Ryle, wrote *'The comparison of social class with social class in respect of height, weight, the routine clinical examination of systems, radiographic appearances, the common disabilities, and of mental and physical function tests… should have much to teach us'*.[46] The initiators of the celebrated Peckham Health Centre—a pioneering project in integrated health care, much lauded today as a model that

should be recreated—called their first book *Biologists in search of material.*[47]

Clearly there was a common way of thinking about human biology which saw the physical capacity of individuals within a population as the outcome of social processes. This tradition fell from favour among epidemiologists after the second world war, when the focus shifted decisively onto adult behavioural patterns and other discrete risk factors. Some epidemiologists continued to produce research in the earlier broad tradition, for example those whose studies demonstrated that short height[48,49] or deprivation in childhood[50] were associated with risk of coronary heart disease. However, the work of David Barker and colleagues from the mid-1980s onwards, showing that intrauterine development is related to adulthood chronic disease, represented a new development in chronic disease epidemiology. Its hallmark is a developmental approach firmly rooted in the unfolding biological potentials of individuals from their conception onwards. While there was some existing basic science research on fetal development and later health indicators in animal models, the epidemiological work enormously stimulated this now vibrant field of biological inquiry.[9] The current issue of the *IJE* contains a series of papers and commentaries relevant to the fetal origins of adult disease. Along with the progress in the specific area of fetal growth and later disease, there has been greater consideration of how social and biological lifecourse experiences develop together.[51] The co-evolution of research in epidemiology, social biology and basic biology in this field demonstrates the potentials of synergy which could advantageously be seen in other domains, in particular that of understanding how genes and environment together influence the health of individuals and populations.

Ageing populations in the developing world: the paradox of success

The eradication of smallpox[52] and the expanded programmes of immunization[53] have certainly been triumphs of public health—essentially applied epidemiology—in the poorer parts of the world. Other successes have been the widespread application of sanitary reforms: slit latrines and deep tube wells.[54] In examining the nature of health problems in the developing world, however, the western eye has focused on those parts of the whole that are easier on the senses. The major problem is, of course, poverty and its various manifestations: starvation, ignorance, disease and premature death. It is in this arena of health policy determined by international aid agencies, and of governments shackled in the chains of debt-servicing agreements, that the determinants of poverty—in particular, the structural inter-relatedness of poverty with the needs of the wealthy parts of the world to remain wealthy—are simply ignored.[55] Rabindranath Tagore, the great Bengali poet, writing on East and West put it so: '*We cannot define to ourselves what we are as a whole, because we know too much…But in a foreign land we try to find our compensation for the meagreness of our data by the compactness of the generalization which our imperfect sympathy helps us to form*'.[56] The 'compact generalization' of the last century was population control, more recently re-branded with wider remits than simply family planning.[57] Western policy makers were convinced that population control was the key to increased economic prosperity, reduction in infant and maternal mortality, and avoidance of demographic entrapment whereby populations experience rapid growth in size due to reductions in mortality without commensurate reductions in fertility.[58] Family planning programmes have been successful in their own terms, when coupled with economic development.[59] Indeed the real success of decades of health improvement schemes, and economic development when allowed to occur, has been the ageing of the populations of the world's poorer countries.[60] Only in sub-saharan Africa are no gains in life expectancy being made, indeed favourable trends are now disappearing in some instances.[61] Despite this 61% of the world's elderly people aged 60 years and more now live in the developing world and this will rise to 70% by 2025. By 2020, countries like Cuba, Agentina, Thailand and Sri Lanka will have higher proportions of over-65s than the US does today.[62] Sadly, those international, bilateral and non-governmental agencies that purport to promote development are curiously silent when it comes to celebrating the fundamental success of their work—population ageing—and are remarkably unwilling to focus on the new challenges and needs faced by countries in rapid demographic transition.

Population and clinical epidemiology

In addition to particular successes of epidemiology, the importance of methodological developments should be considered. The widespread introduction of the randomized controlled trial (RCT) revolutionized the evaluation of medical therapies and contributed to the introduction of effective medical care and the elimination of the ineffective or harmful.[63] RCTs have also helped establish the aetiological nature of associations seen in observational epidemiological studies, for example the reversibility of the associations of blood pressure and circulating cholesterol level with coronary heart disease. However the development of methodology was not automatically followed by its optimal application, and a large proportion of RCTs were simply too small to have the power to detect effects of real public health importance.[64] The advocacy and introduction of meta-analysis (combining the results of RCTs of the same issue) and mega-trials (with large enough numbers of participants randomized to detect meaningful effects) has been of considerable importance in this regard.[65] The insights gained from RCTs, meta-analyses and mega-trials—including the realisation that some therapies considered to be effective because of biologically-based mechanistic reasoning, clinical experience or observational studies were either useless or harmful—stimulated the development of clinical epidemiology[66] and its successful re-branding as evidence-based medicine.

Despite the successes of clinical epidemiology the importance of a population approach to disease control has retained its importance. As Jerry Morris stated: *'The stakes are high: quite small shifts in population distributions of blood pressure or blood cholesterol to the left … could well confer substantial benefits on community health, diminish suffering and lighten the burden on services out of all proportion'*.[67] Geoffrey Rose developed these ideas particularly elegantly in his 1985 *IJE* paper *Sick individuals and sick populations.*[68] He went on to point out that the determinants of the distribution of disease within populations could be different to the determinants of the variation in disease rates between populations. In a population where 100% of people smoke 20 cigarettes per day the factors which influence who gets lung

cancer would probably relate to occupational histories, genetic polymorphism, exposure to air pollution or socio-economic factors. However the massive difference in lung cancer rates between a population in which 75% of people smoke and one where 25% of people smoke would almost entirely be attributable to smoking. The importance of influencing disease rates at the population rather than individual level is illustrated by a consideration of the disappointing outcomes of attempts to reduce coronary heart disease risk through health education and multiple risk factor interventions.[69,70] Conversely the substantial downward secular trends in coronary heart disease mortality rates in many countries indicate that societal-level change can have a substantial impact.

False alarms or no alarms?

No discipline advances smoothly and seamlessly, and epidemiology is not an exception to this rule. Here we discuss two examples, one in which epidemiology failed to make the positive contribution which at one time it was thought it could provide, and a second where the findings of observational epidemiology have been misleading and possibly detrimental to population health. We think these examples usefully provide lessons on how epidemiology may serve a more useful purpose.

Peptic ulcer: failing to take the final step?

The rise of peptic ulcer in the first half of the 20th century in Britain and other countries was—together with the initial increases in coronary heart disease and lung cancer—of major public health concern. The first edition of Jerry Morris' seminal textbook *Uses of Epidemiology* in 1957 was much concerned with this increase,[71] building on important epidemiological analyses by various authors.[71–73] By the mid-1950s peptic ulcer rates began to fall and by the second edition of Morris's textbook a mysterious decline was noted.[74] Particularly elegant and insightful work was carried out by Mervyn Susser and Zena Stein[75–77] which identified clear birth cohort patterns in the rise (and then fall) of peptic ulcer disease in Britain, with duodenal ulcer lagging approximately 10 years behind gastric ulcer in terms of the birth cohort with the peak disease rates. An analysis of data from 19 countries showed similar cohort patterns in all countries, with some variation between countries in when the rises and falls started, but a consistent pattern being seen with respect to the difference between gastric and duodenal ulceration.[78]

The detection of birth cohort phenomena in disease or mortality rates gives important clues as to disease aetiology—specifically, suggesting aetiological factors operating early in life. Thus in 1934 in one of the first formal cohort analyses Kermack and colleagues demonstrated that in England and Wales mortality rates declined in a cohort-specific way for those born around 1845 onwards.[79] Thus for 10 year olds mortality started to decline in 1855, for 20 year olds in 1865, for 30 year olds in 1875, and so on. The clear implication is that some improvement occurred in early life circumstances which led to successive birth cohorts being less susceptible to disease and death at all ages. Kermack and colleagues considered that 'good environment in childhood builds up a stronger constitution and raises the standard of physique of the adolescent to a substantial degree'.[79] Interestingly they considered whether the patterns they saw reflected the newborn children of each successive cohort being healthier, but they ruled this out because infant mortality—as an indicator of vitality of the newborn—was the only mortality age-band which failed to show the cohort regularity, not starting to fall until a later period. Kermack and colleagues speculated that a decline in infant mortality would not be seen until mothers who experienced more favourable early life experiences bore their own children. Other early cohort analyses demonstrated that tuberculosis mortality fell in a cohort-specific fashion,[80,81] which Springett interpreted as indicating that most TB causing death at older ages was acquired during earlier life.[81]

Despite the work of Kermack, Springett and others the initial interpretation of the birth-cohort trends in peptic ulcer rates was strongly influenced by the prevailing paradigm: that chronic disease was of non-infectious origin and was dependent on the mode of life in adulthood. As we have seen above this was a successful strategy for uncovering the cause of the rise in lung cancer mortality, which interestingly also showed evidence of birth-cohort patterns,[82,83] reflecting the uptake of cigarette smoking by successive generations of young adults. Thus the birth cohort patterns observed by Susser and colleagues were interpreted as reflecting the particular experiences of the British birth cohort demonstrating highest disease risks: the first world war as young adults, then the depression of the 1930s and the second world war in middle age.[75,76] The existence of birth cohort phenomena in many other countries cast doubt on this suggestion, however. The notion that important determinants of peptic ulcer disease occur early in life was therefore advanced.[78]

In 1967 Susser concluded that the apparent multifactorial aetiology of peptic ulcer—with contributions from diet, alcohol, cigarette smoking, emotional strain, personality and genotype did not *'exclude the possibility that a major single causal factor waits discovery'*.[76] In retrospect he was correct: *Helicobacter pylori* infection, generally acquired in childhood, is strongly implicated in peptic ulcer disease, and the prevalence of infection is declining in a cohort-specific fashion in countries with declining peptic ulcer incidence.[84] Eradication of the infection successfully treats symptoms and promotes ulcer healing[85] and the adoption of this radical—i.e. non-palliative—treatment reduces health care expenditure.[86] The identification of *H pylori* therefore represents a major advance in understanding and controlling an important disease. This advance was made by a pathologist and a clinician, with no input from the extensive body of epidemiological research on this important public health topic.[87] Epidemiological investigation had essentially no impact on unravelling the aetiology and developing treatment and prevention strategies for a disease which showed an epidemic rise in industrialized countries during the last century.

Following the epidemiological transition from infectious to non-infectious disease, powerful (and in other cases highly successful) ways of thinking about the aetiology of chronic disease of adulthood directed thought away from infectious agents, despite the existence of the model of cohort effects in tuberculosis, with predominant acquisition of infection in earlier life not revealing itself until considerably later in life.[81] The possibility that we are being similarly diverted from uncovering the causes of disease because of the power of contemporary models of disease causation should be considered if we are not

Figure 2 One view of the value of epidemiology

to witness further examples of epidemiological research fading into irrelevancy.

Micronutrients, hormones and health: abstractions too far?

An aspect of epidemiology which has received much criticism is the apparently indiscriminate identification of particular aspects of daily life as dangerous to health.[88–90] As Figure 2 illustrates, this concern is not limited to academics or public health practitioners, and it is this aspect of epidemiology which led the medical journalist James Le Fanu in his pop-history *The rise and fall of modern medicine*[91] to suggest that the solution to the 'fall' of his title was to close all departments of epidemiology. The sometimes contradictory nature of findings from different epidemiological studies[92] feeds into this disillusionment. This is particularly the case when extensive and widely publicised research suggests a health-protective action of an exposure which is not realised in intervention studies.

Take, for example, the extensive research on beta-carotene consumption and the risk of cardiovascular disease. Observational studies revealed strong apparently protective effects of beta carotene, but long-term RCTs found that, if anything, beta-carotene increased cardiovascular disease risk (Figure 3[93]). There are now a series of similar examples: hormone replacement therapy, vitamin E and vitamin C intake in relation to cardiovascular disease, or fibre intake in relation to colon cancer among them. What these examples have in common is that the groups of people who were apparently receiving protection from these substances in the observational studies were very different from the groups not using them, on a whole host of characteristics of their lives. Belief that these differences could

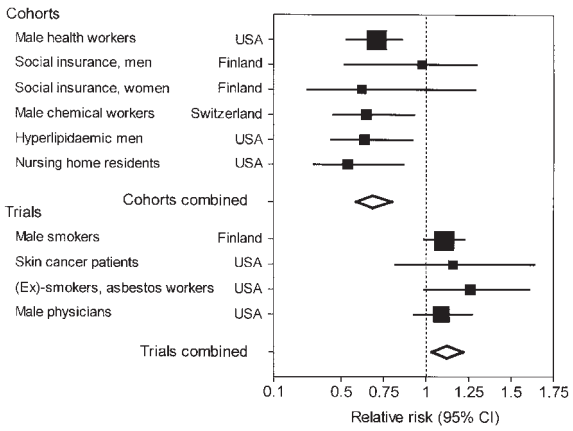

Figure 3 Meta-analysis of results of observational cohort studies of beta-carotene intake and cardiovascular mortality and of randomized controlled trials of the same issue[93]

be summed up in measures of a few 'potential confounders' and adequately adjusted for in statistical analyses, fails to recognize the complexity of the reasons why people differ with regard to particular and general characteristics of their lives. Low birthweight, growth in childhood (and final adult height), persistent infections acquired in early life (such as *H pylori*) or the failure to acquire infections (leading to immunological programming increasing the risk of atopy), lung function, degree of adiposity, a habitus which embraces particular dispositional characteristics (including attitudes, health-related behaviours and mood), modes of self-presentation, and ways of dealing with misfortune,

may seem to fall within different categories, but they are all essential components of life trajectories which influence health. An epidemiology which appreciates the necessary interconnections between these different domains of life will avoid the dead-ends to which research strategies based on the abstraction of individual life trajectories from their contexts can lead.[94,95]

It would be gratifying if the refutation of observational studies by randomized controlled trial evidence in these areas led to a critical evaluation of approaches which abstract single elements—which are almost always behavioural, psychological or therapeutic—from the complexity of the life and times of people, and relate these to single health outcomes. It is likely, however, that as in many decaying research programmes,[96] auxiliary hypotheses will be mobilized to explain each apparent 'mistake', on a case-by-case basis rather than there being a re-evaluation of aspects of the broader paradigm within which the discipline operates. If the former is the case the *Dictionary of Epidemiology*[97] should introduce a simple noun which describes a residually confounded association of no causal significance into its next edition.

Moving on: advancing or retreating into the 21st century?

Whether epidemiology will be a progressive or a decaying scientific programme in the 21st century will depend on how it deals with a series of key challenges it faces. As before, our examples reflect our own interests, and are far from comprehensive.

Genes—explaining all of it or none of it?

How should epidemiology face up to the challenge of the post-genome sequencing world? One way would be to deny that change had occurred. This can sometimes be a surprisingly successful strategy in the short and medium term: Charles Creighton (author of the magisterial anti-contagionist *History of Epidemics in Britain*[98]) retained his scepticism of germ theory until his death in 1927, and remained venerated by at least one of the leading epidemiologists of the day.[99] But in the long run the ostrich approach is doomed, and surely genomics in the late 20th century is the homologue of bacteriology in the late 19th century. The latter led to no rapid transformation in population health or even therapeutics, but in the long run transformed our view of the dynamics of health and disease in populations. The same may be the case with the sequencing of the human genome: the earlier predictions of rapid advance in control of disease are perhaps unlikely to be seen.[100] However a greater understanding of mechanisms of disease development and thus advances in therapeutics may be anticipated.

The high profile of genetic research will surely increasingly influence epidemiology, indeed it is already noticeable that relative risks only a little above unity get treated with considerably more excitement (and journal receptivity) than if similar relative risks were associated with other exposures. This makes some sense, as confounding is a less serious problem in most studies relating polymorphisms to disease than in studies of behavioural patterns or environmental exposures. The population attributable risk of such polymorphisms will, however, probably be low.

The interaction of genomic and environmental factors is much emphasized in most approaches to including genetic configuration in epidemiological studies. This focus on the necessary interplay of genome and environment—which should perhaps be extended to include random developmental noise[101] —should not obscure the need to sometimes stand back and observe the big picture: the interplay of gene and environment needs to be considered against the backdrop of large changes in disease rates that occur within populations, the yawning gap in health status between poor and rich countries and the substantial health gaps according to ethnicity, socioeconomic position and gender within countries. Consider, for example, classical approaches to the genetics of obesity. Twin studies—comparing concordance within monozygotic (MZ) and dizygotic (DZ) pairs—estimate that around two-thirds of the variance in obesity is genetic in origin.[102] However consider trends in obesity within countries (Figure 4); in the US, for example, obesity rates have increased from 12% in 1991 to 19% in 1999: a truly remarkable increase in less than 10 years.[103,104] Such changes are not confined to affluent parts of the world, but in countries as diverse as Nigeria, Morocco and Croatia, large increases in pre-school childhood obesity have been reported.[105] These dramatic and rapid changes clearly indicate the influence of changing environmental factors, since there will have been no change in the genetic makeup of the population over these periods. Similar findings—of a high apparent genetic determination and rapid changes in disease prevalence over time—have been seen for diabetes.[106]

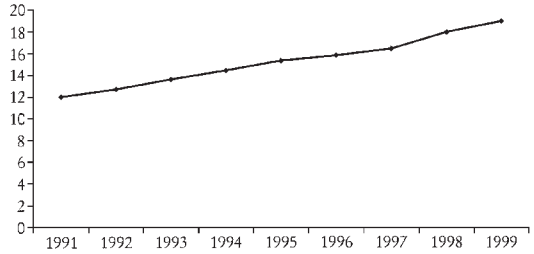

Figure 4 Prevalence of obesity in US adults from 1991 to 1999[103,104]

The apparent discrepancy between a large amount of variance accounted for by genetic factors and the clear environmental determination of population disease rates illustrates the potential dangers of concentrating on one facet of the gene-environment dyad. There are several reasons for how such discrepancies arise. Firstly, the statistical models used to generate heritability estimates may be misleading: they can make untenable assumptions about equal similarity of the environment of MZ and DZ twins, ignore the environmental influence of intrauterine experiences and, most importantly, ignore gene-environment and gene-gene interactions. Indeed, there has been some reduction in the estimates of heritability of obesity made by some authorities, with early claims of 80%[107] being reduced to less than a third.[108] Secondly, the contribution of genetic factors to disease rates is an area where Geoffrey Rose's distinction between the determinants of disease rates for a population and factors influencing who gets a disease within a population is crucial. With very general shifts in the population to higher energy intake/energy expenditure ratio—illustrated, for example, by the consistency of increases in obesity in the US within ethnic, gender, socioeconomic and geographical area

of residence subgroups over the 1990s[103,104]—the variance between individuals can remain strongly genetically based, but this can make a minimal contribution to the population burden of obesity (or diabetes), which is surely what epidemiologists and public health workers should be concerned with. While epidemiologists embrace the new insights provided by access to genomic information, and where possible contribute to understanding of disease mechanisms and therapeutic advances, the big picture—that health and disease are as much social as biological phenomena—should not be lost from view.

Saved from irrelevance by new methodologies?

An issue of increasing salience is the degree to which epidemiology contributes to understanding how the world is (and contributes to ameliorating the negative features revealed) or is a bag of increasingly complex tools for carrying out isolated studies of particular diseases in particular populations. Some senior epidemiologists have been critical of the obsession of epidemiology in the last quarter of the 20th century with methods rather than with health. Reuel Stallones memorably detected a '*continuing concern for methods, and especially the dissection of risk assessment, that would do credit to a Talmudic scholar and that threatens at times to bury all that is good and beautiful in epidemiology under an avalanche of mathematical trivia and neologisms*',[109] a view echoed by others.[110]

The codification of epidemiological methods and attempts to develop more rigorous ways of thinking about what we actually are doing when we set out to study diseases in particular ways —represented by several recent authoritative texts[111,112]— is certainly contributing to improved rigour in our discipline, which should not be decried. Comparing these texts with the products of an earlier period[71,99,113] illustrates the extent to which the health of populations has faded from the epidemiological landscape, however. Epidemiology will surely be best served by an approach which applies the most robust available methods to the most important health problems; the danger of allowing what *can* be studied according to certain methodological principles to become what *is* studied is a very real one. Applying methods which are appropriate for determining if individual-based treatments produce individual benefits—essentially randomized controlled trials—to the question of how to reduce socioeconomic and ethnic inequalities in health within a population represents a category error, with potentially serious consequences.[114]

The final test of new methodologies is if they lead to advances in understanding the determinants of disease among individuals and populations and contribute to their alleviation. This is an empirical question—but one which is very rarely asked. A formal evaluation of the impact, positive and negative, of the mountains of paper devoted to methodological refinement in epidemiology, would represent an important service to the discipline.

How will the *IJE* attempt to remain of relevance?

Journals must react to the changing environment in which they function if they are to remain of relevance, or even if they are to survive. The future for biomedical journals, as for all science journals, is currently uncertain. The internet is revolutionizing the ways in which information is disseminated and accessed. The *IJE* has largely functioned as a forum for publication of peer-reviewed articles reporting original research. There have been few review articles published, few editorials, few letters and little auxiliary material. Currently we must reject around 80% of submitted papers because of space limitations. It is unclear if we are best serving epidemiology by maximising the number of papers reporting original research which we publish, as against other material which, citation analysis suggests, is often of greater relevance to our readership. Among the 80% of papers which we reject, a sizeable proportion are of perfectly acceptable scientific standard but simply cannot be fitted into the available journal space. We feel, however, that use of the internet can help alleviate this problem. To get scientifically valid and worthy research into the public domain we plan to form links with BioMed Central (http://biomedcentral.com). This is a string of interconnected peer reviewed journals that publish primary biomedical research free and immediately on the internet. BioMed Central provides an alternative to the current system of biomedical publishing, by which the public pays twice for the results of research—first to fund the research itself, and then to access the findings. Furthermore, for many individuals and institutions in poorer countries that cannot afford journal subscriptions, we think publication in BioMed Central will reduce their effective disenfranchisement from the global knowledge base. Publication in BioMed Central will also reduce the time and effort involved in getting work peer reviewed and published. At the moment, authors and their community of peers take part in an often wasteful and laborious cycle of submission, peer review, rejection, and resubmission to different journals, each with slightly different requirements and audiences, and all limited by page restrictions. BioMed Central has no such limitations.

What should our policy be regarding the type of material we publish? In other areas, for example poetry, it may be easier to impose a distinctive and attractive editorial stamp. In Box 1 we present the editorial policy of one hypothetical poetry journal. However, we perhaps need a more structured approach to increasing the relevance of the *IJE*.

We are particularly concerned to relate current epidemiological research to what has gone before. Like many scientific disciplines, epidemiology has perhaps not learnt from history as much as it could have done. There is a strong sense that new research is of more importance than older research, but the scramble to read the latest issue of a journal could disguise the fact that much could be gained from a more leisurely perusal of dusty old volumes. As the Nobel prize winning immunologist Peter Medawar said in his *Advice to a young scientist*[115] *'the present devours the past'*. We intend to address this by reprinting historical contributions to epidemiology—together with commentaries on them—from which lessons can be learnt. In the current issue we print a translation of a case-control study of smoking and lung cancer, first reported in Germany in 1943, together with supporting material. We would particularly value suggestions from readers regarding material which we should consider reprinting. We are especially interested in less well known contributions (rather than the rather few well-trodden classics which are constantly anthologized) and we particularly welcome material not originally published in English, which we will have translated for publication in the *IJE*.

Box 1

WHANG EDITORIAL POLICY

Mark Halliday

The editors of *Whang* invite poetry that wears purple
stiletto heels without claiming that this is heroic,
and red football jerseys with the numeral 88.
We expect the kind of momentum and alternating current
that you'd expect with your head in the mouth of
Sophia Loren in 1957. Please single-space and leave
visible margins and italicize foreign words.
Do not assume that to say "Barcelona" or "heart of night"
or "blue souffle" will open every door at *Whang.*
We look for poems that embrace God *because* God has failed
and not the other way around. Send only such poems
as you would choose in lieu of a cigarette before
execution by firing squad. But do not suppose
that facile verbal violence can make us gape and squirm.
We want poems that squeak with the labor of building
elastic altars, but not poems that mop and mow
upon the moony terrace, nor desiccated poems
that wring their hands above a carpet of twigs.
Strange is okay, but not So-Proud-To-Be-Odd.
If your work merely shuffles and titters
with chipmunks glimpsed teasingly in rearview mirrors,
please send it elsewhere. *Whang* is an outlet for sacred
lava. *Whang* is devoted to the nervous fingers of
the short shadowed person frowning in the bagel shop
at a book about Manhattan in the Twenties; but
this is far from the sort of poetry that is flecked
with marinara sauce and garlic amid exploding flashbulbs.
We are not complacent at *Whang.* Nor are we fixed.
We are incipient and pulsing. The world, for us,
is a vertigo of quicksand and we edit as freemasons
in the vale of Tempe, where love is only just before
the hour of quote loving unquote, and yet
your envelope won't even be opened if you think
it's merely a matter of boom image boom image boom image
boom. You have to care *more.* For us the dreamer is
a quincunx of trees in a gale of ink with a grace
as of owls that are not mere birds. For further guidelines
send nine dollars. If you are a churl, do not submit,
but do subscribe. We stay up late, and morning finds us
crusted with homage to fickle dancers whose hair is fizzy.
If you wish your poems returned, check the alley out back.
Know this, know this, we are not just "doing our thing",
we are not just "another eccentric mag". Things have gone
way, way past that. Life whispered "spring" and we sprang.
Do not take us for granted at *Whang*.

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There is an understandable tendency—reflected in our choice in the current issue—to only reflect on premature epidemiological triumphs, the prescience of which is later revealed to the world. We feel that at least as much can be learned from cases where epidemiology has simply got the answer wrong (in relation to current thinking on an issue) or achieved only partial understanding. Surely in reflecting on cases where sophisticated and intelligent thinkers have reached what are now seen to be erroneous conclusions we can learn much about how our current endeavours may be misguided. We discussed the case of peptic ulcer above, but many other cases exist. Before pellagra was identified as being caused by the deficiency of a particular nutrient a wide variety of causes were apparently supported by the epidemiological evidence, including an infectious basis, dissemination of the disease by insects, a hereditary (or 'racial') predisposition, poor hygiene practices, a toxin in corn, poor sanitation, beer, cane sugar, and stress.[116–118] These factors would clearly be linked to dietary deficiency but would not themselves be on the aetiological pathway (unlike the price of food, which Sydenstricker noted was related to pellagra incidence).[119] Many similar examples exist, for example the large array of risk factors identified by epidemiologists studying Downs Syndrome before trisomy 21 was identified, the factors supposedly causing AIDS which studies identified before the isolation of HIV, etc. We intend to reprint historical articles which hindsight may have revealed to be wrong, but which deserve at least as much reflection as the classics.

As is clear from Box 2, and from this issue, many other changes to the *IJE* are being instituted. We would like to increase the active engagement of readers of the journal, both through the submission of letters for publication and through suggestions for debate topics, review articles, themes for issues or more substantial developments of the journal. We hope the *IJE* can contribute in some way to ensuring that when our possible trainee epidemiologist has considered the past and potential future of epidemiology they see that, far from an eclipse of epidemiology occurring, there is new and vital work to be done, so long as the focus on population health is maintained.

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Box 2

Changes in the *IJE*

Associate editors: have increased from 4 to 15 people, drawn from a wide range of countries and disciplines, with the aim of increasing quality and relevance of what you read.

Commentaries: these are commissioned and aim to place papers in context, deal with controversies, and import perspectives from outside epidemiology.

e-IJE Web version: The site is undergoing development which will include rapid up-dating of Bulletin Board and News sections. Visit http://ije.oupjournals.org/ if you have not already done so.

Fast tracking: if work is of high quality and important, we will fast-track it within the constraints of two-monthly publication.

Historical articles: suggestions for papers, particularly if published in non-English languages, and deserving wider attention because lessons they can teach are invited for consideration.

Point/Counterpoint: these are commissioned to provide alternative viewpoints on controversial or difficult topics. Suggestions for this section are welcomed.

Reviews: systematic reviews relevant to public health, and traditional reviews particularly from people working outside of formal epidemiology, are sought.

Themed issues: each issue will have a theme with individual studies, commentaries and reviews relating to the topic.

Theory and Methods: in this section we aim to publish papers constituting methodological advances which produce benefits to understanding of determinants of disease. In the case of more technical contributions, empirical examples, rather than purely theoretical discussions, are welcomed. We also seek papers addressing theoretical controversies in epidemiology.

What we want less of: minor analyses from major studies, scientifically valid but mundane findings.

What we want more of: high quality research conducted in developing countries, hypothesis papers, epidemiology of indications and effects of intervention (i.e. public health services research), greater interdisciplinary contributions, letters.

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