

- ²⁴ Hardell L, Walker MJ, Wallhjalt B *et al.* Secret ties to industry and conflicting interests in cancer research. *Am J Ind Med* 2007;**50**:227–33.
- ²⁵ Michaels D. Scientific evidence and public policy. *Am J Public Health* 2005;**95**:S5–7.
- ²⁶ Michaels D, Bingham E, Boden L *et al.* Advice without dissent. *Science* 2002;**298**:703.
- ²⁷ Michaels D, Monforton C. Manufacturing uncertainty: contested science and the protection of the public's health and environment. *Am J Public Health* 2005;**95**:S39–48.
- ²⁸ Michaels D, Monforton C, Lurie P. Selected science: an industry campaign to undermine an OSHA hexavalent chromium standard. *Environmental Health* 2006;**5**:10.1186/1476-069X-5-5.
- ²⁹ Monforton C. Weight of the evidence or wait for the evidence? Protecting underground miners from Diesel particulate matter. *Am J Public Health* 2006;**96**:271–76.
- ³⁰ Clapp R, Hoppin P, Kriebel D. Erosion of the integrity of public health science in the USA. *Occupational Environmental Med* 2006;**63**:367–68.
- ³¹ Stolley PD. A public health perspective from academia. In: Strom BL (ed.). *Pharmacoepidemiology*. New York: Churchill Livingstone, 1989.
- ³² Pearce N, Crawford-Brown D. Critical discussion in epidemiology: problems with the Popperian approach. *J Clin Epidemiol* 1989;**42**:177–84.
- ³³ Rothman KJ. Conflict of interest: the new McCarthyism in science. *J Am Med Association* 1993;**269**:2782–84.
- ³⁴ Rothman KJ, Evans S. Extra scrutiny for industry funded trials – JAMA's demand for an additional hurdle is unfair - and absurd. *Br Med J* 2005;**331**:1350–51.
- ³⁵ Barnes DE, Bero LA. Why review articles on the health effects of passive smoking reach different conclusions. *J Am Med Association* 1998;**279**:1566–70.
- ³⁶ Stelfox HT, Chua G, O'Rourke K, Detsky AS. Conflict of interest in the debate over calcium-channel antagonists. *New Engl J Med* 1998;**338**:101–6.
- ³⁷ McMichael AJ. Prisoners of the proximate: loosening the constraints on epidemiology in an age of change. *Am J Epidemiol* 1999;**149**:887–97.
- ³⁸ McMichael AJ, Beaglehole R. The changing global context of public health. *Lancet* 2000;**356**:495–99.
- ³⁹ McMichael AJ, Haines A. Global climate change: the potential effects on health.[comment]. *Br Med J* 1997;**315**:805–9.
- ⁴⁰ McMichael AJ, Powles JW. Human numbers, environment, sustainability, and health.[comment]. *Br Med J* 1999;**319**:977–80.
- ⁴¹ McMichael AJ, Smith KR, Corvalan CF. The sustainability transition: a new challenge. *Bull World Health Organization* 2000;**78**:1067.
- ⁴² Susser M. Does risk factor epidemiology put epidemiology at risk? Peering into the future. *J Epidemiol Community Health* 1998;**52**:608–11.
- ⁴³ Susser M, Susser E. Choosing a future for epidemiology: I. Eras and paradigms. *Am J Public Health* 1996;**86**:668–73.
- ⁴⁴ Susser M, Susser E. Choosing a future for epidemiology: II. From black box to Chinese boxes and eco-epidemiology. *Am J Public Health* 1996;**86**:674–77.
- ⁴⁵ Planck M. *Scientific autobiography and other papers*. New York: Greenwood Press, 1949.
- ⁴⁶ Pearce N, Merletti F. Complexity, simplicity, and epidemiology. *Int J Epidemiol* 2006;**35**:515–19.
- ⁴⁷ Rothman KJ, Greenland S. edn. *Modern epidemiology*. 2nd edn., Philadelphia: Lippincott-Raven, 1998.
- ⁴⁸ Saracci R. Everything should be made as simple as possible but not simpler. *Int J Epidemiol* 2006;**35**:513–14.
- ⁴⁹ Last JM. Guidelines on ethics for epidemiologists. *Int J Epidemiol* 1990;**19**:226–29.
- ⁵⁰ Neutra RR, Cohen A, Fletcher T *et al.* Toward guidelines for the ethical reanalysis and reinterpretation of another's research. *Epidemiology* 2006;**17**:335–38.
- ⁵¹ Prineas RJ, Goodman K, Soskolne CL *et al.* Findings from the American College of Epidemiology's survey on ethics guidelines. *Ann Epidemiol* 1998;**8**:482–89.
- ⁵² Soskolne CL, Sieswerda LE. Implementing ethics in the professions: Examples from environmental epidemiology. *Sci Eng Ethics* 2003;**9**:181–90.

Commentary: Epidemiology needs the patients to survive

J W W Coebergh

Accepted 12 July 2007

According to Pubmed, the pessimistic and rather emotional paper of Ken Rothman on the presumed rise and fall of epidemiology in 1980¹ was his 59th after being in the 'business'

for almost 10 years. He was undoubtedly speaking on behalf of many of his colleagues at the time in expressing the threatened demise of his profession. By 2007, having become an influential teacher, he has been involved in 209 articles on a wide range of subjects, often in the domain of congenital defects, early life exposures, pharmaco-epidemiology and disease aetiology. In 1981, he was still optimistic enough to found the New

Professor of Cancer Surveillance, Erasmus MC Rotterdam, Department of Public Health, PO Box 2040, 3000 CA Rotterdam, The Netherlands.
 E-mail: j.coebergh@erasmusmc.nl

England Epidemiology Institute which was to successfully train a large number of post-graduate students in the subsequent 20 years, and he became the first editor of *Epidemiology* in 1990. Today, he is still actively teaching and advising all over the world and co-authoring articles. So, if he did not live up to his own prediction or sought to refute it, were his alarming early warnings appropriate?

From the same period I recall a long, worried letter of my tutor Hans Valkenburg (founding Professor of Epidemiology in Rotterdam) to the Dutch Ministry of Justice on the same topic. He was attacking the potential for mis-interpretation of epidemiological findings by members of the legal profession in pursuing patient rights. But, in my perception it is not only the legal profession, but also some of our own professional colleagues that misunderstand their position and start meddling with research proposals, thus giving lay people the impression that proposed research is of doubtful benefit and may even be dangerous for patients. In my view, this happens partly out of a combination of well-intentioned curiosity, misguided animosity and risk aversion. So, what we see is that a well intentioned concern for privacy or patient safety degenerates into bureaucratic time-consuming behaviour. How to address that, is the question.

What was missing from the paper by Rothman was an understanding of the new directions of epidemiology. Of course that is easier to see now after another 25 years of experience and, especially in the last 10 years, battles for less and better rules that leave room for giving substance and clarity to the interpretation and implementation of results. Self-developed guidelines, self-regulation and more consistent, responsible behaviour did the rest, although in a few countries, e.g. in former Eastern Europe, political changes made distrust of professionals the key to progress.

But by re-publishing this subjective paper, the question arises: did it change anything it was attacking or was it just the first symptom of a new epidemic of rules which had to run its course? It seems the latter. Of course, in retrospect it is easy to see that during the late seventies several major trends came together: the emergence of patient concerns and rights since the 60s as part of the trend towards individualism and litigation, the rights and needs of research subjects (on which another Rothman wrote an insightful book 'Strangers at the Bedside'²), and the culture of risk aversion, so eloquently described by the philosopher Wildavsky³. Add to that the emergence of the digital age in which the trade of the biomedical epidemiological investigator is not only to extend and globalize knowledge, but also to draw public attention to the vested interests of big business in both health-promoting and health-damaging products. Last but not least, there are powerful government interests in search of control of this ever expanding sector with so much public money involved and issues of equality. It is probable that all these factors influenced the maturation of epidemiology at large, a maturation which generally occurred only during the 80s and 90s, pioneered by a number of well-known researchers in the 60s and 70s. Is it surprising that Rothman does not mention the contribution of Scandinavian and British epidemiology at all—it was certainly there. Or is it indeed a case of America 'uber alles', here? Interestingly, in 1980 the USA congress needed Doll and Peto⁴

to help and restrain the lawmakers from the totally crazy world of an inflated fear of chemical and dietary cancer risks suggested by 'more research demanding' lobbies paid by big tobacco and other vested interests, as we know from the insightful book of Proctor on the history of the wars against cancer.⁵ Has epidemiology really been flourishing in all these controversies? And what about the tremendous amount of epidemiological research on the health effects of low-dose radiation during the 80s? How has this fomented distrust against high technology?

No one can deny that the bureaucratic nightmares sketched by Rothman have become reality to some extent, all over the world, implying that we are dealing with a genuine phenomenon, the question of trust. But where is it least affected? My impression is that it is in Scandinavia, consisting of relatively small populations with quite close links between the public, lawmakers and well-trained researchers with access to many cohorts of people with greater or lesser exposures, and where health care is still a social enterprise. The roles of commerce and business in society might be another big difference. Wasn't American medicine in 1981 on the eve, or already in full swing, of becoming a very commercial activity of promotion, of exaggeration—in fact a culture of hope and fear? In a recent Economist article⁶ it was estimated that the USA in 2002 spent an annual amount of about 350 billion dollars on the regulation of health care (including litigation) and of innovation and research of which only about 50% generates benefits at all (and often to the wrong people, such as lawyers). Isn't this our current future nightmare, in all parts of the post-industrialized world?

The question might, however, be: could unrestrained, professionally determined work by epidemiologists such as Rothman have prevented the risk-aversion and risk-exaggerating inequality promoting approach of modern medicine and biomedical industry in the USA at large. Why did not epidemiology team up more with the disciples of donabedian with his sound theories on assessment of quality of care, pleading for population-based study designs?⁷ Perhaps, we need even more of the desirable huge follow-up studies and biobanking, and better post-marketing surveillance to create more clarity on predictive value of new biomarkers and health risks of modern technology with, of course, a reasonable degree of financial independence. We would spend only fractions of that money with much better results for society.

As far as I can see we have come full circle. The same patient rights that were frustrating American epidemiologists about 25 years ago (and Europe a little later) should now be invoked or stimulated to let us do our work in their interest without restriction and to limit regulation. That is already happening with increasing success in emerging lobbies of epidemiologists with other biomedical scientists who recruit their own lawyers to devise self-regulation in order to create trust in the process and outcome, e.g. of biobanking.⁸ Paradoxically, we might only be able to restrain the regulators and legislators by more self-regulation and self-restraint in zealously criticizing each other. Let us instill confidence in information-hungry patients who generally appreciate being informed on content and methodology.

References

- ¹ Rothman KJ. The rise and fall of epidemiology, 1950–2000 AD. *New Engl J Med* 1981;**34**:600–02.
- ² Rothman D. *Strangers at the Bedside: A History of How Law and Bioethics Transformed Medical Decision Making*. New York: Basic Books, 1991.
- ³ Wildavsky A. *Searching for Safety*. New Brunswick (USA) and London: Transaction Books, 1988.
- ⁴ Doll R, Peto R. *Causes of Cancer*. London: OUP, 1981.
- ⁵ Proctor RN. *Cancer Wars: How Politics Shapes What We Know and Don't Know About Cancer*. New York: Basic Books, 1995.
- ⁶ The Economist. An Unhealthy burden. *Economics Focus*, June 30 2007, pp.76.
- ⁷ Donabedian A. The epidemiology of quality. *Inquiry* 1985;**22**:282–92.
- ⁸ Oosterhuis JW, van Veen EB, Coebergh JWW. Tumour banks: well-guarded treasures in the interest of patients. *Nat Rev Cancer* 2003;**3**:73–77. [Review].

Published by Oxford University Press on behalf of the International Epidemiological Association
© The Author 2007; all rights reserved.

International Journal of Epidemiology 2007;**36**:719–723
doi:10.1093/ije/dym160

Commentary: Is epidemiology really dead, anyway?

A look back at Kenneth Rothman's 'The rise and fall of epidemiology, 1950–2000 AD'

Michel P Coleman

Accepted 3 July 2007

Rothman's inspired lament¹ about the demise of epidemiology at the end of the 20th century was published in 1981, when the century still had 20 years left to run. It was a premature obituary for a science in terminal decay, written by a scientist who was already one of its leading thinkers on principles,² on content^{3–5} and on methodology,^{6–9} and who would later publish several major textbooks. I was studying for a Master's degree in epidemiology at the time, so Rothman's article was the source of some dismay, but none of my fellow students jumped ship.

One of his gloomiest predictions turned out to be spookily accurate. But although I'm no follower of Pangloss—'all is for the best in this best of all possible worlds'—for me, the remainder of Rothman's pessimism seems like an unwitting panegyric to the success of epidemiology since its renaissance as a scientific discipline.

The pioneers

Rothman, a keen student of history,¹⁰ rightly dates that renaissance to the studies of William Farr and John Snow from around 1840, but he starts with the meteoric impact of John Graunt's 1662 epic,¹¹ *Natural and political observations...made upon the bills of mortality*. Graunt quantified, for the first time, the extraordinarily high childhood mortality (one in three died under the age of five), the slight excess of male

births (14:13) and the huge impact of plague, TB and rickets in 17th-century England, and produced the first life table. All that, 300 years before computers. Lionised and admitted to the new Royal Society (founded 1660) within weeks of his *magnum opus* being published, Graunt never published anything else on epidemiology. He died in poverty after his haberdashery business was destroyed in the Fire of London (1666).

Rothman sees the personal tragedy that befell John Graunt as a cautionary tale for the science he more or less single-handedly invented. Not me—Graunt's personal fame may have been brief, but he is still an incandescent torch for epidemiology today. Graunt was the first rock star of public health. If almost two centuries passed before anyone seriously picked up his abacus,¹² that simply confirms how far in advance of his time he really was.

During a 40-year career as Compiler of Abstracts to the General Register Office of England (which still exists today), William Farr (1807–83) brilliantly exploited the death certificates that became routinely available for all deaths in England for the first time, under the 1837 law on the registration of births, marriages and deaths. He identified high mortality in certain occupations and geographic areas, and gave Snow access to data that helped him unravel the origin of cholera even though, at the time, Farr himself believed foul odours were the cause.

But Farr did much more. The first International Classification of Causes of Death, forerunner of today's International Classification of Diseases, discussed at an international conference in Paris in 1900, was a direct result of his groundwork, and that

Cancer Research UK Cancer Survival Group, Non-Communicable Disease Epidemiology Unit, London School of Hygiene and Tropical Medicine, Keppel Street, London WC1E 7HT.
E-mail: michel.coleman@lshtm.ac.uk