What should be the concerns of epidemiology?

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Panum was a remarkable man. He studied the measles epidemic on the Faroe Islands in 1846. Disastrous as this epidemic was for the people, it was a unique opportunity for field investigation. The isolation of the islands and the restrictions on commerce imposed by the Danish government had resulted in the exclusion of measles for 65 years. The population was small and the inhabitants were scattered over a number of islands. No other exanthem was present to confuse the picture. Thus, there were vast potentialities for investigation needing imagination, ability and energy. Panum had all of these. His investigation is a model of how to do a good study—and locate it in the appropriate setting so that not only theoretical but also practical lessons can be learnt.

Panum first described the environment of the Faroes—the living conditions, housing, health, diet, occupation, etc., of the Faroese. He used the available vital statistical data to determine the relevant physical and mental health attributes of the population—thus putting measles into the appropriate context. From this analysis he showed that the population was remarkably healthy, considering the environment they lived in, and that this was largely due to the absence of infectious disease.

At the time of the outbreak, although measles was considered to be spread by direct contact, it was still believed that it could arise spontaneously. Panum was able to destroy this belief and determine the length of the incubation period. Panum’s observations led him to conclude that control was feasible and rational, by isolation.

In his study of measles he thus developed epidemiological knowledge and then applied it to control of a disease.

Panum was only 26 years old during his time in the Faroes. After this he worked as a physician and physiologist. The latter is his real claim to fame in Denmark. But his interests did not stop there—he was greatly concerned to improve medical education in the Scandinavian countries and to apply scientific methods to medicine. Thus, the questions this symposium poses would have been close to his heart.

The acceptance of the importance of epidemiology is a recurrence of an old phenomenon. When I was a medical student, almost 50 years ago, there was only the odd lecture (in both senses of the word) and it was a subject considered to be of little importance. By contrast, the subject was taken very seriously in most countries in the latter half of the 19th and in the first 30 years of this century. The explanation for this change in attitude is almost certainly associated with the conquest of many infectious diseases and the change in incidence and prevalence of chronic diseases. Epidemiological concerns with infectious disease were important, relevant and accepted in the past—and in some countries, until quite recently, considered the only domain in which epidemiology played a role. It is only since the end of the World War II and the work of such individuals as Bradford Hill and Doll on the role of cigarette smoking and cancer, Morris on coronary heart disease, and many others that a clear role in non-infectious disease has been recognized. I have dealt with this previously.

If we are to discuss the future, and the options open to us, it is necessary first to consider what our tasks really are. I consider that our subject has three main purposes:

To be concerned with knowledge of the causes and associations of disease and abnormal function.

With the knowledge of the associations and causation to contribute to the formulation of preventive strategies—whether it be in infectious diseases such as cholera or in non-infectious conditions such as cancer of the lung. We are, and should be, involved with the testing of aetiological hypotheses and the efficiency/effectiveness/efficacy of interventions.

To be involved in decisions on planning, evaluation and organization of services to communities and individuals such as quarantine, occupational hazards, primary, secondary and rehabilitative services, utilizing knowledge from epidemiological studies.

Beaglehole and Bonita considered possible directions for public health. They described two options—a broad focus on the underlying social causes of health and disease or towards a narrow medical focus. They concluded that in most countries public health was focusing on a narrow disease focus route—but advocated that only if public health assumed a broad focus with a ‘firm sense of identity’ was there any future for the subject. The same applies for epidemiology, the basic discipline of public health. It is crucial for our subject that we tackle broad issues and do not restrict ourselves to narrow expressions or concerns. The great dangers that we face are that we become concerned only with methodology, the secondary analysis of data, or the assessment of diagnostic tests or interventions.

Trying to forecast a future for anything is always hazardous, but tempting. I do not see myself in the role of a prophet but I wish to consider what I think are the major areas and concerns that our own subject should tackle. While wanting to do this I must introduce a note of caution. My experiences have been almost entirely in the developed world, and I retired from full-time employment 4 years ago; thus my ideas are limited and may be somewhat out of date.

The problems that we face as epidemiologists are many. Firstly there are those of our own making. Many studies are dependent on poor designs, and thus suffer from all the problems of an inadequate research strategy. Because of poor designs for investigating possible causes of disease, researchers may discover small relative risks, publicize these widely and are then challenged by counter proposals or held up to derision. The problem is that small relative risks may be important, but again
the may not be, and the findings may have been obtained by incorrect, fallible methods or by data dredging, or trawling, of data collected for other purposes. The problem with such studies has been discussed at length and I do not intend to digress further—except to emphasize that all of us, as scientists, have societal responsibilities. We should thus concentrate on efforts to investigate properly important problems and not jump onto any popular, current bandwagon or merely report findings because we have the methods for doing a particular study. Linked to this method of working is the temptation to do an exploratory study, produce dramatic results and publish these widely, without concern for the possible errors, or the problems of a study with insufficient power to produce a clear result.

The second problem that we face is that it is now possible to have access, relatively easily, to large databanks collected either for other purposes, or as a by-product of routine care. Examples are hospital patient data banks, or the findings of large health or social surveys undertaken by administrators for their own purposes. The dangers of relying on such data are that usually there has been no attempt at any systematic validation or quality control (e.g. variability in diagnosis, observer-subject error in the measurement of blood pressure) which may bias interpretation of the findings. Furthermore, many of the studies using such easily available data are often not based on biologically or clinically sensible hypotheses and yield ‘so what associations’. This latter problem is one not limited to use of readily available data but it is also a problem of some carefully designed studies which measure ‘interesting’ characteristics and their associations with disease without being concerned with what one does with the findings. For instance, possible genetic findings in a study which then implies either undertaking genetic manipulation, birth control or wide-scale population screening to identify the few at possible risk, at great expense.

The third problem that we face is the vast explosion of genetic knowledge in the past 5 years or so.

It is not my intention to be a Luddite and to dismiss the possibilities for improved disease control and improvements in the quality of life, but I would like to consider what our problems are today, and in the immediate future. Like Panum, we should be concerned with the environment in which our populations live and what is important.

Since the end of World War II there have been vast advances in epidemiological knowledge and methods. But it should be of concern to us that so much of our effort has been on a relatively small number of conditions. A great deal of effort has been devoted to the study of cardiovascular disease, respiratory disease and cancer. Probably some of our findings have contributed to the improvement in health and diminution of mortality for these conditions. Molecular biology will, undoubtedly, enable us to understand even better the mechanisms of these conditions; it may help in their control and treatment. I am ignorant of the basic facts and therefore do not intend to comment on this.

But the concentration of epidemiological effort on these conditions—of which I too am culpable—has meant that many other matters that lend themselves to epidemiological enquiry and control have been neglected. Reviewing the changes in health over the past 100 years we were struck that there was little progress in certain areas. Mental health (or illness) is a prime example. This is an area of enquiry of great complexity and difficulty. There are problems of definition, identification, conflicts, etc., which beset those epidemiologists who have attempted to do good studies. But it is often forgotten that the greatest advances in the prevention of mental illness have been through the application of epidemiological knowledge and the control of measles and rubella.

There are many other examples of common conditions which we have neglected in the recent past. If we are to consider the future, then we should be concerned with identifying not only areas to which we can make major contributions, but also the strategies which led to our successes.

Infectious disease has always been of prime concern to epidemiologists—whether it be Panum in Denmark, Snow in England or Langmuir in the USA. However, this area has demonstrated that it is not only study of a condition that is important, but also its control. Thus the main methodology of surveillance practised by our past ‘greats’ implied the systematic collection, analysis, and interpretation of outcome-specific data, closely integrated with their timely dissemination to those responsible for preventing and controlling disease or injury. This is what Panum did. Thus I believe that if we are to be concerned with the future then it is not a choice between population or molecular methods or increasing emphasis on one or the other. It is that the appropriate methods be applied to identify the causes and mechanisms of conditions which influence or affect the health of populations and then to determine how these can be used in order to control or mitigate the effects of the condition. As a discipline we have tended to emphasize and glorify the scientific aspects of our subject, but have neglected its application—Panum would not have been impressed. For our development we need to become more concerned with how to apply our findings and test the effectiveness of preventative measures. If we are to follow in his footsteps then certain things follow.

We have been successful in the development of epidemiological methods of investigation and their application. We must develop additional methods also to test the effectiveness of control measures, over and above those commonly used e.g. randomized controlled trials.

The number of conditions studied by epidemiologists needs to be expanded and we must not hesitate to press for resources to investigate and control conditions of little concern to political or commercial interests, but which are of importance to our societies.

When undertaking epidemiological studies we should become more concerned with the ability to apply the findings, rather than only the scientific merit or nicety of the study.

All this implies that, like Panum, we should be concerned with education and training of future generations within the context of our societies. Panum’s concern to improve the standing of medical education and its scientific base is a close parallel.

And finally it is important to recognize that our discipline does not exist in a vacuum. We must be concerned with knowledge of, and application to, social, environmental and biological factors in the conditions we study.

References

1. Panum PL. Observations made during the epidemic of measles on the Faroe Islands in the year 1846. Delta Omega Society 1940 (distributed by the APHA).
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