Rethinking the terms non-communicable disease and chronic disease

We welcome Ackland et al's timely discussion on the terms “non-communicable” and “chronic” disease. Their argument is that conditions currently labelled “non-communicable” are in fact “communicable” because the risk behaviours that underlie them are highly transmissible. Thus they argue for a change in label, from “non-communicable diseases” to “chronic diseases”.

Their argument, however, confuses one classification system, which is based on cause (namely, communicable diseases compared with non-communicable diseases compared with injuries), with a second classification system, which is based on effect (namely, acute conditions compared with chronic conditions). Their argument also overlooks the growing consensus that chronic conditions include certain communicable diseases, such as HIV/AIDS. In fact, certain non-communicable conditions are acute in nature, while certain communicable conditions require chronic, ongoing care. For example, HIV/AIDS clearly has an infectious aetiology but requires long term management by the healthcare system. As such, it has a great deal in common with type 2 diabetes. Conversely, acute appendicitis is a “non-communicable” disease that requires an urgent health care response quite similar to bacterial meningitis.

Their argument is that certain non-communicable diseases are in fact “communicable” because the risk factors that underlie them are highly transmissible. Thus they argue for a change in label, from “non-communicable diseases” to “chronic diseases”. Their argument also overlooks the growing consensus that chronic conditions include certain communicable diseases, such as HIV/AIDS. In fact, certain non-communicable conditions are acute in nature, while certain communicable conditions require chronic, ongoing care. For example, HIV/AIDS clearly has an infectious aetiology but requires long term management by the healthcare system. As such, it has a great deal in common with type 2 diabetes. Conversely, acute appendicitis is a “non-communicable” disease that requires an urgent health care response quite similar to bacterial meningitis.

References

Authors’ reply
The letter by Unwin et al in response to our paper makes some important points about the attributes of disease classifications. We agree that, unfortunately, it is commonplace for classifications based on cause and those based on effect to be interchanged by the public health community. This not only causes confusion in the minds of public health policy makers, but promotes bureaucratic apathy towards resourcing prevention programmes for diseases that contribute so significantly to the burden of disease. Why would a jurisdiction dedicate scarce resources towards preventing and controlling diseases that are long term (chronic) and where causation is unclear (non-communicable)? Many jurisdictions will only take on the concept of an epidemic seriously when there is a clear link to the traditional concept of “communicable” or infectious disease.

So it is very important to encourage use of language in describing diseases that is of practical value to both clinical researchers and policy makers. This language should encourage everyone in thinking about public health responses to diseases from both the perspectives of disease prevention and disease control. While international classification systems are unlikely to change quickly, we are seeking a shift towards more appropriate use of terminology when describing major disease states.

Our desire is to encourage people to think about the upstream issues of causation of diseases in order to motivate a more pragmatic focus on public health interventions. Indeed we support use of the term “chronic” to emphasize the long term implications for the health system, but more importantly wish to discourage use of the term “non-communicable” where there is mounting evidence for the non-microbial, communicable nature of many chronic diseases. Qualifying some chronic diseases as being “transmissible” may be helpful in highlighting the role of social, cultural, and societal factors as disease vectors. Herein lie the opportunities for improving public health responses and interventions. We are glad that Unwin et al support our thinking to promote the use of the term “chronic” to denote conditions requiring long term care, which is more readily understood by the public health community.

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Epidemiology of SARS: the missing pathogen?

This is indeed a strange disease. The epidemiology suggests it to be of relatively low infectivity, but high severity. This in itself is odd, especially if the causative agent is a virus and the principal mode of spread by coughing/droplet. Also odd is the undoubted existence of “superspreaders”, who can infect very many of their contacts—I can’t think of any parallels to this in respiratory virology.

Perhaps the SARS virus obeys the usual rules of droplet transmitted respiratory infections, and is of high infectivity. However, because of shared antigens, a proportion of the population has an acquired resistance to the new virus, having already been exposed to another, relatively innocuous, virus that provides immune protection. It is possible that the proportion of humanity immune or partially immune to SARS could be as high as, say, 95% if the second virus were a very common one, for example, one of the coronaviruses that causes coryza. This would explain the seemingly low, unexpectedly so, infectivity of the SARS agent.
Perhaps this also explains “superspreaders”. Picture humanity divided into two categories:

1. Those who have met a common related coronavirus, and consequently have a degree of immunity to SARS, say for the sake of argument ± 99% of the population.

2. Those who have not met it, and have no immunity. If the defenses of the first group are overwhelmed by exposure to a huge SARS virus inoculum, perhaps they would contract a modified form of the disease, quickly recruit their immune systems to respond to a recognized infectious agent, be likely to recover, not shed large amounts of virus, not be all that infectious. The second group would get the disease in an exuberant form, excrete quantities of infectious material, be likely to succumb before their immune system could meet the challenge—the superspreaders—always allowing that other factors may be implicated in superspreader events, such as a pre-existing condition in the initiator, or variation in the likely number of contacts, this greatly increased by hospitalisation before the risk is known.

The invocation of a high (but presumably variable and incomplete) level of acquired immunity to the SARS agent in affected populations explains other apparent anomalies of the 2003 epidemic—Why did some regions (Russia, Japan, Indonesia, Thailand, Malaysia, etc) not have any cases, despite a high intensity of air traffic with the epicentre of the disease? Why did some affected regions (Guangdong, Hong Kong, Toronto) struggle to contain the disease, when others (Singapore, Vietnam) had no such difficulty? Above all, how was a pandemic averted when in the earlier stages of the spread of SARS this outcome appeared very much a probability?

I believe that a coherent mathematical model of the SARS epidemic could be constructed from the above theory. This of course would necessarily lend it validity, but it may be worth looking at.

Even if this aperçu should prove anywhere near the mark, it still would fail to answer another question—Will epidemic SARS recur? On the other hand, the possibility of a pandemic might be seen to recede.

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doi: 10.1136/jech.2003.015040

Is the inverse care law no longer operating?

The inverse care law, proposed by Julian Tudor Hart in 1971, states that “the availability of good medical care tends to vary inversely with the need for it in the population served.” A number of authors have now claimed to have found instances of the inverse care law operating in practice. Given that evidence that this “law” has gained in the healthcare literature over the past 30 years, we were surprised to note that Jordan et al failed to make reference to it in their recent article on the relation between access to services and health.

In this report, access to services was measured as both straight line distances and car travel time to the nearest GP surgery and hospital as well as the access domain of the index of multiple deprivation 2000, which combines measures of straight line distances to the nearest pharmacy, primary school, food shop, and post office. Among urban wards, the authors report a consistent inverse association between distance to services and both mortality and limiting long term illness (LLTI) in people aged 0–64 years—although this association was negligible in terms of the relations between LLTI and distance to hospitals.

Both premature mortality and LLTI are markers of need for health services in themselves. In this population, they are both strongly associated with deprivation in the UK, and therefore a much broader marker of need for health services. The results of Jordan et al suggest that areas with greater need for health services are nearer to and have greater access to, or concentration of, both health and wider social services. This is in conflict with the inverse care law, which would predict that distance to services should be greater, and therefore access poorer, in areas with higher levels of need.

Are the results of Jordan et al evidence that the inverse care law is no longer operating in the UK? Is it possible that over the past 30 years, we have managed to redistribute primary care services, in particular, so equitably that instead of deprivation, poor health and greater need for services being associated with poor access to services, it is now associated with greater access to services? Alternatively, is it possible that the inverse care law has rarely operated in practice in the UK in recent times and that “evidence” for it has misinterpreted the original formulation of the law and generalisability of use of services, rather than provision of them?

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References


Do socioeconomic conditions reflect a high exposure to air pollution or more sensitive health conditions?

We read with great interest the recent paper by Martins et al concerning the relationship between socioeconomic conditions on air pollution adverse health effects in elderly people in Sao Paulo, Brazil. These results are very interesting and may promote understandings of which social category of people are most sensitive to air pollution. The authors suggest that socioeconomic deprivation represents an effect modifier of the association between air pollution and respiratory deaths in elderly people for an increase of 10 μg/m³. They conclude that poverty represents an important effect modifier that should be taken into account when determining the health consequences of environmental contamination. We agree with these conclusions. Nevertheless, the question is to know if poor people died because they are more exposed, or because they are more exposed to addictive behaviours, such as smoking habits. When air pollution is considered, socioeconomic characteristics as an effect modifier can take two aspects. Firstly, people with low socioeconomic status may be more sensitive in terms of health effect because they have associated diseases and people with certain diseases had a greater risk of dying during an episode of increased air pollution than did members of the general population. Furthermore, people living in underprivileged sectors would have both more limited access to health care and greater exposure to air pollution. Exposure to air pollution is the second aspect of the interpretation of the effect modifier. Jerrett et al argue, low socioeconomic conditions may be associated with manufacturing and so with a higher workplace exposures, but also with a lower accessibility. In addition, persons with lower socioeconomic characteristics may be exposed to a complex mix of pollution from indoor sources, as well as outdoor pollution because of traffic, industry, and waste burning in developing countries. It seems interesting to explore the link between individual exposure and socioeconomic characteristics because these two factors are strongly correlated.

More studies are needed to investigate this effect modifier and particularly the significance of this effect. To understand this effect we will need individual data on risk factor but also data on individual exposure to have a good interpretation of the results and to have policy implications.

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Authors’ reply

The comments of Filleul and Harrabi on our paper reflect the major concerns about the role of socioeconomic conditions in the association between air pollution and health effects and keep the topic on discussion. Low socioeconomic status evolves different and complementary aspects that can act synergistically to aggravate health conditions. For instance, being more vulnerable to diseases and having less access to health (geographical and economic considerations) are factors that, in general, are concurrent among poor people and can contribute to death. In addition, if they also are more exposed to air pollution, and probably not only air pollution (e.g. outdoor but indoor) but water pollution, we have the whole picture of what it is to be in the lowest socioeconomic levels of any society. If the discussion focuses only on levels of exposure it reflects the concept of linear dose-response relation between PM$_{10}$ and respiratory diseases that is well known and accepted. In men born between 1906 and 1938 and screened between 1970 and 1973 a strong correlation with socioeconomic conditions and health measures. In the 1970s and 1980s, there were different people” may react differently to the same risk factor. 

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Siblings and adult mortality and stroke risk

In men born between 1906 and 1938 and screened between 1970 and 1973 a strong relation between greater number of siblings and mortality risks was found by Hart and Davey Smith. These risks, however, were also related to adverse behavioural socioeconomic and health indexes. A great number of children, particularly in lower socioeconomic classes and in the earlier decades of the previous century, implies reproduction at earlier and more advanced maternal age and shorter interpregnancy intervals. These aspects have been neglected in studies on number of siblings in relation to constitut-