Author’s Response
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The purpose of my editorial on chronic diseases and calls to action¹ was to identify ways in which the Chronic Disease Action Group’s plans, described in last of a set of papers published in the Lancet in 2007² might be derailed. I contributed to the plans described in the Lancet paper as a member of Chronic Disease Action Group and this was intended to be my first contribution to advocacy for the group as acknowledged in the editorial.

Asaria and colleagues³ clearly agree with much of the editorial and helpfully correct my misreading of the predicted proportion of deaths due to chronic diseases among those aged ≥70 years between 2005 and 2030. On affordability, I contrasted the high costs of investing in chronic disease interventions with the modest gains in economic productivity as a potential barrier to action. Asaria and colleagues point out that attributing an extremely low monetary value of US$2000 to a death averted balances the investment needed with the societal gains. This is a totally rational economic argument that may hold the attention of governments and remove this potential barrier to action. Let us hope so. The counter argument that deaths averted at younger ages will result in more disability, misery and expense due to deaths postponed to older ages is popular—some data and modelling of likely trends in physical disability and dementia syndromes would be helpful.

I concur with Asaria and colleagues’ view that budgetary reallocation is possible and pointed out that India, in particular, should view health spending ‘not a luxury... but a means of reducing health inequalities’. Given the global economic downturn, I hope India’s plans to spend 15% of GDP on health in the 2008–09 budget are not ignored.

De Backer and colleagues⁴ criticize my ‘rather haughty attitude’ with regard to the publication strategy of the European heart disease prevention project. I apologize to De Backer and colleagues for loose phrasing of this paragraph that is capable of being interpreted as disparaging of the work of Geoffrey Rose and others who set up the European factories study. However, my comments about publication bias and suppression of negative findings related to the quasi-experimental studies of similar design and intervention to the North Karelia study that were never published in peer-reviewed journals.⁵,⁶ I am well aware of the many publications arising from the European factories studies, having reviewed them for a Cochrane systematic review of the effects of prevention for coronary heart disease.⁷ The conclusion of the European factories study, that ‘advice to reduce risk factors is effective to the extent that it is accepted’, is clearly true and is reflected in the much better effects of such interventions in people who have already suffered a cardiovascular event⁸ who are presumably much more motivated to accept advice.⁹ The fundamental point is that the effect of the European factories intervention by intention to treat (surely the rational analysis for public health action) was a 7% reduction (P = 0.8) in CHD deaths, consistent with the pooled findings: relative risk 0.97 [95% confidence interval (CI), 0.89, 1.06].

Fortunately for developing countries more effective interventions are now available for reducing cardiovascular risk, comprising better anti-hypertensive and cholesterol-lowering drugs, wider acceptance and implementation of health protective interventions in the form of tobacco controls, reduction in saturated dietary fat content of foods and reduction in hidden dietary salt.

The IJE welcomes papers describing the results of evaluations of interventions for chronic disease prevention and continues to support the Chronic Disease Action Group.

References
The hygiene hypothesis is an intriguing attempt to explain the rise in allergy and asthma (and other immunological diseases) in populations undergoing changes in the environment towards increasing cleanliness, use of vaccinations and antibiotics, lower rates of infections, etc. In many ways this idea is appealing and seems to offer a unifying explanation of many epidemiological observations. However, in a recent issue of the *IJE*, Douwes and Pearce summarise different controversies relating to the hygiene hypothesis in their commentary on the observed recent decrease in asthma in Australia. As pointed out by Douwes and Pearce, recent downward trends of asthma/allergy prevalence reported in some countries seem unlikely to be explained by a decrease in hygiene and they conclude that new aetiological theories may be required.

The hygiene hypothesis assumes that exposure to micro-organisms—by their binding of their antigens to innate immune receptors—skews the immune system towards a Th1 response favouring a ‘non-allergic’ immune response. In other words, increased exposure to antigens from microbes ‘protects’ against allergy/asthma. Although experimental data has often supported this idea, the hygiene hypothesis does not seem to explain all-time trends and differences in prevalence between and within different populations. This author has recently proposed that one of the reasons why the hygiene hypothesis fails to explain many epidemiological trends is due to the possibility that it—in its current version—does not take into account reduced exposure to the quantitatively most important environmental airborne antigens, i.e. antigens of common allergens such as allergens from pollens, house dust mites, animals and moulds. Reduced allergen exposure may be one of the factors underlying the higher risk of IgE-mediated allergic disease in populations with urbanised, Westernised and affluent lifestyle. This lower allergen exposure results in failure to induce and maintain immune tolerance to common environmental allergens and increased risk of allergic disease in genetically susceptible individuals. Several lines of evidence support this idea.

First, numerous experimental animal studies have shown that exposure to high allergen doses (introduced by the oral or respiratory route or by injection) induces tolerance, whereas exposure to low allergen doses confers an increased risk of IgE sensitization. Second, many, but not all, observational epidemiological studies suggest that exposure to pets decreases the risk of pet allergy. Third, randomized intervention studies of house dust mite allergen avoidance (both primary/secondary and tertiary prevention) do not support that avoidance has beneficial effects on sensitisation or clinical outcomes. Finally, there is a large body of evidence to support that the induction of immune tolerance in allergic patients can be obtained by allergen-specific immunotherapy (large doses of allergens introduced via the oral, mucosal, intravenous or subcutaneous routes) underlining the pivotal role of the concept of immune tolerance induction by high-dose allergen exposure.

The complexity of this issue should be acknowledged to the greatest extent possible. Many apparently contradictory data might be explained by the possibility that the dose–response relationship between allergen exposure and sensitization is bell-shaped, i.e. increasing exposure in the lower range increases risk of sensitization, while increasing exposure in the higher range decreases risk of