Clearly the question of increased survival following vector control has major implications and it deserves priority attention. The reality is that evidence to date is insufficient to make an informed assessment. It is useful to recall that in attempting to measure relatively small differences, such as 20–30% reductions in overall mortality in an environment of high variability and strong secular trends, the methodological problems encountered are considerable[12].

So, what should we recommend to implementing agencies? We agree with Rogier and Trape that improving access to timely and effective treatment for all malaria patients represents a most desirable goal. The African reality is that public health services have been only partially successful in the fight against malaria morbidity and mortality. Future prospects are no better. Should we therefore wait another decade and the certainty of 10 million child deaths? Or should we move ahead with insecticide-treated bednets, which, besides being highly effective in the short term, are also in high demand because of the reduction in nuisance biting? Our choice is easy: get the nets.

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Concerns on Long-term Efficacy of an Insecticide-treated Bednet Programme on Child Mortality

Can interventions of insecticide-treated bednets (ITBNs) reduce child mortality in Africa? The answer is 'yes' when considering the results of three large controlled trials[1–3] and of a national programme carried out in The Gambia, which showed that it is possible to have a significant impact on child mortality even in non-controlled conditions and on a national scale[4]. However, concerns on long-term efficacy of an ITBN programme on child mortality have been raised by several authors[5,6]. Trape and Rogier[6] believe that variations of morbidity and potential mortality from malaria are weak compared to the considerable range of transmission levels and that only a considerable reduction of transmission (much higher than that achieved by ITBNs) would be able to reduce, on a long-term basis, the burden of malaria. Thus, according to them, the only effective ways of fighting malaria in Africa are the improvement of health services and a more rationale use of antimalarial drugs. Nobody denies that these are important aspects of any health project aiming at controlling malaria morbidity and mortality. However, although several questions about the efficacy of ITBNs remain unanswered there is no reason to delay the implementation of an intervention that could prevent thousands of deaths in Africa[7]. The evidence on which Trape and Rogier[6] base their hypothesis is not totally convincing and relies mainly on the results of a large study carried out in Brazzaville, where it was shown that extreme differences in malaria transmission may be associated with only minor differences in severe malaria incidence rates[8]. However, it should be noted that these were much lower than those reported from other regions of Africa[9]. The widespread use of antimalarials in Brazzaville may have reduced not only the incidence of severe malaria, but possibly any difference between areas with different transmission. Other reports on the differences of malaria-specific mortality between areas of variable transmission are more difficult to interpret as estimations are likely to be biased and imprecise[10].

The factors that determine whether a child develops mild or severe malaria are complex and multifactorial[11] and the relationship between clinical disease and death is not well understood. This is why an estimation of the long-term impact of ITBNs, which is based on the incidence of uncomplicated malaria attacks in two villages with different transmission, is likely to be approximate. Another point when considering malaria mortality is whether the number of clinical attacks experienced over an entire lifetime really matters. Their distribution over time, particularly during the 'vulnerable' period before five years of age, is probably more important. From the figure presented by Trape and Rogier[6], it seems obvious that in areas of high transmission (Dielmo) the incidence of clinical malaria before five years of age is much higher than it is in areas of low transmission (Ndiop): a two-year-old child in Dielmo will experience six clinical attacks per year, while in Ndiop a child of similar age will have only two attacks per year. In the same manner, the classification of different areas of transmission by means of the annual entomological inoculation rate (EIR) should be treated with caution, as this figure is not informative on the distribution of infective bites over shorter periods of time. Therefore, on the basis of the available data, we believe it is unjustified to dismiss ITBNs as ineffective on a long-term basis, when they have been defined as one of the most promising tools that have emerged in recent years for the fight against malaria.

The long-term benefits of ITBNs are unknown and it is unlikely they will be as great as those observed among highly immune children immediately after an intervention[12]. The possibility of changing the clinical spectrum of disease, from severe anaemia to cerebral malaria in areas of intense transmission and with an ITBN intervention, has been raised[9]. There is an urgent need to solve these questions by monitoring the impact of ITBNs for at least 4–5 years. This, and not estimations based on doubtful assumptions, is the only way of knowing the right answer.

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A Call for Integrated Approaches to Controlling Malaria

Jean-François Trape is to be congratulated on yet another outstanding large-scale population-based study on malaria epidemiology[1]. Such information helps to rationalize data from numerous studies and helps focus on real issues concerning malaria control. His analysis of the situation in Senegal, comparing the prevalence of illness and severe disease under differing conditions of entomological inoculation rates, draws attention to two facts. First, the epidemiology of severe malaria along with incidence of clinical conditions is complex, and is dependent upon intrinsic and extrinsic mechanisms affecting humans and mosquitoes. Second, although there is implication of a single approach to malaria control, effective control of malaria cannot depend on a monotypic strategy. It is this second point I would like to emphasize.

There is a danger in focusing on a monotypic approach. Clearly, reducing transmission in certain situations, as a result of sporadic and short-term interventions using insecticide-impregnated bednets (IBNs), has caused reduction in many indicators of malaria disease, but he queries whether long-term effects would be sustainable, bearing in mind the changing immunological patterns in the human population that may result from reducing transmission. An important conclusion from his work is that control of this disease should now pass from the experimental to the functional; instead of being in the hands of agencies and organizations with numerous scientific agendas, it should move to a stage of national planning in those endemic countries where malaria control is a high priority. The process should become a national concern.

Those of us who have run national programmes know that there must be a plan of operations based on demographic, epidemiological and economic conditions, and the components of the intervention must combine effective case detection and treatment with vector control and reduction of transmission.

To find evidence of programmes that involved vector control and effective treatment, and data on their successes one should look into the past to see the long-term effects of such integrated activities. Apart from many other parts of the world, malaria has been effectively controlled in numerous African countries, for example (among others) Swaziland, Eastern Transvaal, Zimbabwe, Zambia and Zanzibar. The programmes utilized both arms of the intervention and were considerably successful. Thus, as transmission patterns declined and when clinical cases became evident, adequate drug supplies were available in rural clinics, and chemotherapy prevented an upsurge in severe malaria. Many such programmes are still in force, others were disbanded because of cost; however, they demonstrate that the concerns raised by Trape[1] can be surmounted in a nationally planned, integrated strategy. We should be careful to consider his excellent presentation of data as it should be seen, a call for integrated approaches to controlling malaria, and not as one may fear, ammunition for those who consider diagnosis and treatment as the main vehicle for malaria control and may propose, heaven forbid, a decline back to tactical variant no. 1 of the WHO (1974).

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Which Strategy for Malaria Control in Africa?

Can malaria mortality be decreased in tropical Africa by vector control? The answer is 'yes' if we consider the short-term impact on mortality of direct interventions, such as house spraying or impregnated bednets, which have decreased malaria transmission. However, the answer is 'no' if we consider the results of epidemiological studies which have compared malaria mortality rates between populations exposed to significantly different intensities of transmission. My belief is that the contradiction is only apparent, each approach revealing different but complementary aspects of the complex host/vector/parasite relationship in malaria, and that intervention trials principally measure a short-term effect, whereas epidemiological studies better reflect the equilibrium that results from the development of premunition[1]. Since fluctuations of transmission remain associated with fluctuations of morbidity and mortality whatever the epidemiological context, the short-term follow-up of interventions that decrease transmission does not permit the prediction of the future evolution of mortality, whereas epidemiological analysis does permit prediction of the strong trends of this evolution. In most epidemiological contexts observed in tropical Africa, even a substantial reduction in transmission cannot significantly reduce the burden of malaria for the whole community[1].

Nevertheless, can impregnated bednets contribute to malaria control in Africa? Brian Greenwood (this issue) presents a series of arguments in favour of continuing impregnated bednet programmes. Likewise, Christian Lengeler *et al.* (above), and Umberto D'Alessandro and Marc Coosemans (above) believe that the evidence accumulated so far is not sufficient to warrant abandoning these programmes. Their arguments have weight: the immediate benefit is indisputable, some medium- or long-term benefits remain possible. Who could refuse to try to save the lives of 500000 African children each year[2]? Ideally, any strategy of malaria control must aim at optimizing a series of actions concerning: (1) patient care (health education at the community level for a suitable attitude toward disease; network of health structures; availability of personnel, equipment and drugs; adequate decision making and therapeutic schemes); and (2) disease prevention, of which vector control is one of the major components. Theoretically, these two categories of measures are complementary, and they synergize in areas of instable malaria. However, in the context of tropical Africa,