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## DDT and Malaria Prevention

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In his commentary “Global Status of DDT and Its Alternatives for Use in Vector Control to Prevent Disease,” van den Berg (2009) raised concerns about the impact of DDT (dichlorodiphenyltrichloroethane) and its derivatives on human health, in spite of the fact that DDT has been used widely for seven decades and no properly replicated and confirmed study has found any specific human health harm. Given the enormous and proven public health benefits arising from the use of DDT in disease control, it is incumbent on van den Berg to cite human health harm from DDT exposure that fulfills the basic epidemiologic criteria for a clear and unambiguous cause-and-effect relationship. In the absence of such evidence, van den Berg’s concerns should be ignored.

van den Berg reported on high levels of human exposure to DDT among those living in sprayed houses but presented no evidence of actual human harm arising from that exposure. van den Berg is not the first to consider this issue. Many prestigious and authoritative groups and individuals with no ideologic agenda have evaluated potential DDT harms over the last 70 years, and have consistently found no evidence of DDT harm that would cancel out the enormous health benefits of its use in malaria-endemic countries. Yet van den Berg (2009) stated that “initial work suggests that nonoccupational exposure through (IRS) is associated with impaired semen quality in men” but failed to mention that the association is exceedingly weak. In addition, van den Berg neglected to report on the evidence of growing populations wherever DDT has been used in malaria control, a fact that would undermine the idea that DDT significantly harms human fertility.

With respect to other health effects, such as early pregnancy loss, fertility loss, leukemia, and various cancers, van den Berg (2009) admitted that in “many cases the results have not been consistent between studies,” but he went on to state that “these accumulating reports bear much concern, particularly in relation to chronic effects.” This is a stunning embrace of belief over science. van den Berg’s conclusion defeats the very purpose of epidemiologic standards for decisions about cause–effect relationships. Accumulations of data and results of distinct studies that provide weak or no evidence of harm to human health do not argue for a hidden cause–effect relationship; they argue instead that no cause–effect relationship exists.

Where DDT has been used in malaria control over many decades, populations have grown and health outcomes have improved. Yet van den Berg (2009) stated, without reference, that “a gain in infant survival resulting from malaria control could be partly offset by an increase in preterm birth and decreased lactation, both of which are high risk factors for infant mortality in developing countries.”

van den Berg addressed the potential environmental harm that could be caused by DDT use in malaria control. Although he noted that DDT is sprayed indoors for malaria control and is used in small quantities, van den Berg (2009) stated that “DDT sprayed indoors may end up in the environment.” If some residues are found outdoors, they will be demonstrably concentrated in soil within the first few meters from the house. Documentation from many studies shows that DDT movement is likely to be so small and so gradual as to be insignificant (Smith and Webley 1969; Viera et al. 2001). DDT use in malaria control is by definition used indoors, whereas environmental management, which is promoted as an alternative to the use of insecticides for malaria control, is by definition performed outdoors (e.g., larvivorous fish, drainage of wetlands). If van den Berg has evidence to show that small uses of DDT sprayed indoors is more harmful than massive environmental changes outdoors, he should have included it in his review.

van den Berg’s discussion of insecticide resistance and repellency and irritancy of DDT is both confused and confusing. Citing a personal communication, van den Berg (2009) wrote that “the development and spread of insecticide resistance is much slower when vector populations are under effective control . . . suggesting that suppressing vector proliferation helps prevent or delay the development of resistance.” To conclude that insecticide resistance is forestalled by using insecticides to suppress vectors is absurd. It is a demonstrable fact that spraying insecticides to kill insects will select for resistance. van den Berg seemed to suggest that DDT’s repellent actions may contribute to vector resistance, but he is wrong. Repellent action does not lead to toxic resistance because repellency reduces mortality, and it is mortality that exerts selective pressure for resistance.

Alternatives to DDT are always welcome. van den Berg (2009) correctly noted that operational capacity is a barrier to introducing these alternatives and that many countries

have implemented health sector reforms that have decentralized decision making, planning, and budgeting. He relied on case studies (Barat 2006) of four countries to argue that decentralization can benefit malaria control. However, elsewhere, these case studies have been criticized and shown to be based on unreliable and even false data (Attaran et al. 2006). IRS and other malaria control operations require, to a significant degree, centralized decision making, planning, and budgeting. The trend of decentralization since the 1970s has not only limited the scope of IRS but has also been a factor contributing to the gradual increase in malaria transmission around the world. In addition, as documented by the World Health Organization (1986), decentralization was vigorously opposed by many malaria scientists. It is unreasonable to argue that the very factors that undermined IRS programs are reasons to further limit IRS.

Van den Berg (2009) concluded that “environmental management and other non-chemical methods within [integrated vector management] strategies . . . will increase the sustainability of control efforts and assist in achieving malaria elimination objectives.” There is little evidence to support such a statement; in fact, the supposed solutions proffered and their modes of delivery have contributed to the weakening of malaria control programs and the global increase in malaria. After almost 70 years of use, DDT—when used in IRS programs—remains one of the safest and most effective methods of saving lives from malaria. van den Berg’s assessment makes no constructive contribution to advancing the goal of controlling a very preventable disease.

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## DDT and Malaria Prevention: van den Berg Responds

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In their letter, Tren and Roberts raise a number of issues. The stakes in the use of DDT (dichlorodiphenyltrichloroethane) are high in terms of both malaria control and the side effects on human health and the global environment. In my review (van den Berg 2009), I attempted to balance malaria-control objectives and the risks of side effects. The manuscript was extensively reviewed by environmental and health experts even before being submitted to *EHP*. Therefore, the review is neither a case for or against the use of DDT.

The benefit of DDT in protecting people against malaria infection is beyond doubt. Therefore, any decision to replace DDT with alternatives must be based on evidence of the risks and benefits. The more we learn about DDT and its alternatives, the more critical we have to become in decision making.

Regarding health effects of DDT, Tren and Roberts point out correctly that clear and unambiguous cause–effect relationships have been lacking. However, this should not be interpreted as a lack of risk. Studies have depended mostly on epidemiologic data, many using case–control studies but lacking a solid control group. A major difficulty has been to establish differences in the level and period of past exposure, a prerequisite for hypothesis testing. For example, despite many previous studies, only recently has breast cancer been attributed to past DDT exposure (Cohn et al. 2007), but some caution with interpretation is still warranted. In addition, in a contemporary review of 494 studies, Eskenazi et al. (2009) concluded that there is a growing body of evidence that exposure to DDT and DDE (dichlorodiphenyldichloroethylene) may be associated

with breast cancer, diabetes, decreased semen quality, spontaneous abortion, and impaired neurodevelopment in children.

Exposure to DDT in relation to indoor residual spraying (IRS) is of particular concern. In my review (van den Berg 2009), I referred to a recent study from South Africa (Aneck-Hahn et al. 2007) that showed a very high body burden of DDT in men living in houses routinely sprayed with DDT. Tren and Roberts highlight the potential of DDT to accumulate in the domestic environment, the location where human contact with DDT is likely to occur. Notably, data on exposure and health effects in young children, pregnant women, and other susceptible groups are still lacking in relation to IRS. At the time of my review, the only available data on health effects were on semen quality (Aneck-Hahn et al. 2007), which I used merely as an indication of health effects in relation to DDT use in IRS. I did not speculate on the impact of semen quality on human fertility or population growth.

Regarding environmental effects of indoor residual spraying with DDT, I quoted recent studies that reported on releases of DDT into the environment, not just in the domestic environment. Nevertheless, I pointed out that these studies need verification. Any alternatives to DDT need to be subjected to an evaluation of the side effects, especially when they involve drastic measures such as drainage of wetlands. Most alternative methods, however, have minor environmental effects (Rozendaal 1997).

In response to comments of Tren and Roberts on insecticide resistance, I need to verify two points. First, keeping vector populations under control by reducing proliferation may prevent or delay the onset of resistance development in the adult stage, but this requires further study. Second, in my review (van den Berg 2009), I mentioned that a repellent effect of DDT will reduce the risk of resistance development.

Tren and Roberts question whether decentralization can benefit malaria vector control. Indeed, the logistic requirements of IRS make this intervention particularly suitable for vertical programs, and as I pointed out in my review, it will be a major challenge to conduct and sustain IRS in a decentralized setting. Still, the experience from South Africa shows that a central program of vector control can coexist with a decentralized health system (Biscoe et al. 2005). Moreover, in Zambia, spray operators are drawn from local communities (Chanda et al. 2008). The key is to harness the potential of decentralization for vector control while providing support for IRS, where necessary. In the context of integrated vector management (IVM), the process of systems analysis, decision making,

and monitoring favors a setting that is decentralized, allowing the development of a locally tailored vector control strategy and involving local actors. Barat (2006) provided a useful analysis of the success of four decentralized programs, even though, as pointed out by Tren and Roberts, the actual benefits in terms of a reduction in malaria cases may have been overstated.

In their final comment, Tren and Roberts dismiss the contribution of environmental management and other nonchemical methods in a malaria elimination strategy. When transmission reaches moderate to low levels, the main interventions will gradually be targeted only to high-risk areas, causing a reduction in the use of chemical insecticides. At decreasing transmission levels, alternative methods that reduce vector populations (e.g. environmental management, larval control) will increase in relative importance. At low levels of transmission, the human population will lose its immunity to malaria; consequently, a decrease in vector density is expected to cause a decline in malarial disease. As I indicated in my review (van den Berg 2009), modeling studies have predicted an important incremental effect of alternative methods when used in conjunction with ITN or IRS, even under conditions of intense transmission.

*The author has acted as advisor or expert committee member in relation to DDT and disease vector control for several United Nations agencies. This has involved a compensation for travel and consultancies.*

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## The Precautionary Principle: Radiofrequency Exposures from Mobile Telephones and Base Stations

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Dolan and Rowley (2009) reported that the precautionary principle “is not appropriate to policy on the use of mobile telephones and the siting of base stations” because there is no established health hazard from the exposure to low-dose radiation. The guidelines [International Commission on Non-Ionizing Radiation Protection (ICNIRP) 1998] provide guidance on protection only from thermal effects (when an increase in body temperature causes injury to the tissue for a short period of time). These guidelines do not cover effects on humans or the environment from nonthermal effects [i.e., effects from electromagnetic fields (EMF) or chronic exposure that do not increase body temperature]. These nonthermal effects of EMF have been well documented by Belyaev (2005) and Sage et al. (2007). Therefore, the precautionary principle is needed to protect the environment from these effects. Several reports have recommended use of the precautionary principle for these exposures [Herberman 2008; International Commission for Electromagnetic Safety (ICEMS) 2006, 2008; Russian National Committee on Non-Ionizing Radiation Protection 2008; Sage et al. 2007]. I do not agree with Dolan and Rowley (2009) that there is no plausible hazard to humans from the exposure to low-dose radiation. Clinical diseases caused by environmental exposures develop after a long period of biochemical changes; during this time, the exposed individual may or may not have symptoms. For example, in stomach cancer, biochemical changes may occur 10–20 years before the appearance of the cancer.

Dolan and Rowley (2009) also stated that risks can be seen with other activities such as “transport (including aviation) and hot showers.” These risks result from the individual’s choices and are not comparable to exposure to electromagnetic radiation from base stations, which is a constant, chronic exposure that occurs without the individual’s knowledge and permission.

The past has taught us many lessons about risk from environmental exposures. For example, the lack of full scientific proof concerning the adverse effects of asbestos and the delay of

precautionary action had devastating consequences to human health [World Commission on the Ethics of Scientific Knowledge and Technology (COMEST) 2005]. If asbestos had been banned in 1965, when the effects of asbestos on mesothelioma were plausible but unproven, the Netherlands alone would have saved approximately 52,000 victims and €30 billion for 1969–2030. An estimated 250,000–400,000 deaths from mesothelioma, lung cancer, and asbestosis caused by past asbestos exposure will occur the next 35 years in the European Union (COMEST 2005).

In conclusion, concerning the exposure to electromagnetic fields, the precautionary principle should be applied to protect humans from environmental effects of non-thermal mechanisms.

*The author declares he has no competing financial interests.*

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## The Precautionary Principle: Dolan and Rowley Respond

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We thank Zinelis for his interest in our article (Dolan and Rowley 2009). However, it appears from his comments on the

recommendations of the International Commission on Non-Ionizing Radiation Protection (ICNIRP), that he misunderstands the scientific basis and scope of the evidence used to establish those exposure guidelines. The ICNIRP (1998) stated clearly that for the frequencies relevant to mobile communications the restrictions are “provided to prevent whole-body heat stress and excessive localized tissue heating.” This is based on evidence of established health effects. In respect to claims of effects from low-level and modulated exposures, the ICNIRP (1998) stated that

Overall, the literature on athermal effects of AM [amplitude modulated] electromagnetic fields is so complex, the validity of reported effects so poorly established, and the relevance of the effects to human health is so uncertain, that it is impossible to use this body of information as a basis for setting limits on human exposure to these fields.

The ICNIRP keeps the scientific evidence under review and recently restated that the 1998 recommendations remain valid (ICNIRP 2009), again noting in respect of claims of nonthermal affects that

With regard to non-thermal interactions, it is in principle impossible to disprove their possible existence but the plausibility of the various non-thermal mechanisms that have been proposed is very low.

Zinelis makes an analogy with risks from asbestos; however, this is flawed. By way of example, animal studies show evidence of harm from exposure to asbestos (International Agency for Research on Cancer 1987), whereas in respect to radiofrequency exposures, the animal studies consistently show that carcinogenic effects are not likely, even at exposure levels above those from mobile telephones (Scientific Committee on Emerging and Newly Identified Health Risks 2009).

We do accept the involuntary nature of exposure to radio signals from base stations; this is integral to providing the mobile phone services that almost 4 billion people voluntarily use and is a matter for risk perception, not risk assessment. We conclude by reiterating that the precautionary principle cannot be used to justify measures to restrict radio frequency exposures from mobile phones or base stations when there is no scientifically plausible evidence of a hazard to human health.

*Both authors are employed by trade associations representing the mobile communications industry. The views expressed in this letter are those of the authors and do not necessarily represent the views of any organizations or companies with which they are professionally associated.*

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