

Dear Dr Martin,

Thank you for the opportunity to comment on the summary document of the GEH conference. I would like to add some comments to those formally submitted by Robert Meyers, EPA's Principal Deputy Assistant Administrator for Air and Radiation.

I strongly support, and would like to add to, Robert Meyer's recommendation that NIEHS pursue a global environmental health research strategy targeting indoor air pollution resulting from the use of traditional solid fuels such as wood, waste biomass, dung, coal, or charcoal.

It is not surprising to me that this issue and the diseases associated with it were rated so highly by the experts you convened. Even those health outcomes for which there is strong evidence (ALRI, COPD, and lung cancer, in the case of coal use) are based on what would be considered very few studies by EPA standards when we review ambient air quality standards. But there are many other likely health outcomes from the inhalation of this smoke for which there is even much less evidence, including tuberculosis, cataracts, asthma, low-birth weight, perinatal mortality, otitis media, and cardiovascular disease. Your report highlights many of these.

Organizations have been working to disseminate improved cooking stoves for decades. The motivation for most of this work in the 1980s and 1990s was largely one of energy efficiency – for purposes of decreasing pressures on local biomass resources (e.g., deforestation, desertification) and to save women time. Since 2000, there has been a substantial increase in activity in this field. The Partnership for Clean Indoor Air (PCIA) – the effort that EPA worked to launch at the 2002 World Summit for Sustainable Development – is certainly one manifestation of that, but it is by no means the only one. Other leading organizations have either entered, re-entered, or ramped up efforts.

Why the increased activity in this field? In my mind, the clear answer is evidence that began to emerge in the 1990s regarding the health impacts of indoor air pollution. As the person who initially developed PCIA, I can state with absolute certainty that without that knowledge, the United States government would not have put its weight behind PCIA: EPA would not have proposed PCIA; we would not have succeeded in gaining the broad support across the Administration to launch it; and, we would not have been able to identify sufficient partners to launch PCIA in Johannesburg. This emerging health knowledge was also a primary motivation for the recent entrance of other leading organizations into this field.

That said, it is equally clear to me that what we know today about the health impacts of indoor air pollution is grossly inadequate. Leaders in developing countries must constantly make excruciatingly difficult decisions about where to spend very scarce budgets. A decision to spend a dollar on indoor air pollution is a decision to not spend it on another pressing need, and it is very hard to justify expenditures on addressing indoor air pollution when the health evidence is much less certain than for other priorities. In addition, we do not currently understand the dose-response relationship for any of the pollutants in indoor smoke at the concentrations to which people are exposed. As a result, we cannot confidently understand, let alone quantify, the benefits of any given intervention. For example, if a manufacturer can sell 10 million clean stoves that are used full-time and maintained constantly, and these stoves will reduce daily exposures to harmful smoke by 50%, how many lives will be saved? That figure critically depends on the very poorly understood shape of the dose-response curve at the relevant exposure levels.

It is in this broad context that I view NIEHS's conference report. A health research agenda targeting indoor air pollution would provide enormous benefits to this field. Most critically of course, it would help to answer the key questions related to health outcomes that the field faces. Those results would in turn very likely catalyze much greater investment to effect solutions. This is the precise dynamic we have witnessed over the past decade, only now the implementation side of this field has matured substantially. For example, commercial solutions are displacing government subsidies and clean stove and fuel designs are maturing. Here at EPA, I am leading a process to transform PCIA into a bigger, independent, and sustainable entity. If NIEHS targets its global environmental health research strategy in this field, there would be enormous synergies to exploit as all these efforts move forward. As a result, NIEHS's research on indoor air pollution would foster much greater investments across the field and impacts on the ground, and that is an enormously exciting possibility.

Finally, I am confident that NIEHS health research on indoor air pollution will yield important spillover effects for this growing field.

Leaders in the field would gain greater confidence to justify their investments. New private sector companies and donors may be inspired to enter the field. The very practice of field research on indoor air pollution requires collaboration among a wide range of actors, many of whom would not otherwise engage in this field, let alone collaborate. And, as you outline the many research priorities and begin to achieve results, you would no doubt foster a much broader research community on these issues.

I congratulate NIEHS on its decision to develop a global environmental health research strategy, and I strongly encourage NIEHS to focus its research on the health risks associated with indoor air pollution. The knowledge we currently have is grossly inadequate, and the results your research delivers will be enormously important in justifying and stimulating large-scale solutions to the issue.

Respectfully,  
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