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## **Global health beyond the Millennium Development Goals: a provocation to conference participants**

We are looking forward to welcoming you to a conference on future needs in health research in Copenhagen on April 28th and 29th. The conference aims to take stock of the current landscape in public health research and consider how well it meets the current and emerging needs of individuals, families and communities in developing countries. Thinking forward, the conference will make recommendations that could guide training and investment in public health research to increase its relevance and utility in a changing world.

The conference organisers hope for open (and even heated) discussions which will help build a strong, positive agenda for the future of public health research, centred on pragmatic actions that could reduce or overcome some of the obstacles we currently face in ensuring that health research contributes as much as it possibly can to human development.

In order to stimulate those discussions, we are providing a deliberately provocative "diagnosis" of some of the challenges we face. The list does not seek to be comprehensive or rigorously evidence-based; some of the challenges we raise have been around for decades while others are speculative looks at the future. We expect conference participants to disagree with many of these contentions. We hope that where you disagree, you will respond in discussions during the conference with practical examples which show how obstacles have been overcome. Where you believe a diagnosis is correct, we hope you'll prescribe a practicable cure. Above all, we hope this provocation will stimulate rich, challenging and ultimately productive debate when we meet.

### **Diagnosis one: We're doing the wrong research**

Public health research will contribute most to development if it seeks to answer the questions that are most relevant to human welfare. We might therefore expect the needs of the communities who carry the greatest burden of ill health to be a dominant influence in choosing research questions. However, that is rarely the case. There are other, stronger influences on the choice of public health research topics; many of them distort the research agenda and reduce the opportunity for health research to focus on the questions that matter most. Among those influences are:

#### **Money**

"He who pays the piper calls the tune." The majority of public health research in developing countries is paid for by taxpayers in rich countries, through research councils, overseas development agencies or international institutions. Politicians in rich countries make choices that appeal to their electorates first; the needs of developing countries are secondary. This can have an especially pernicious effect on

research that meets the needs of the most marginalised, because they almost by definition belong to politically underrepresented and often unpopular groups.

A handful of private foundations are making a growing contribution to funding public health research, and their share may well increase over time. Private foundations tend to fund the research that most interests their leaders. The Bill and Melinda Gates Foundation, the largest single private funder of global health research, is said to concentrate more on technological solutions than on systemic approaches, for example, while the Wellcome Trust is said to favour "bench science" over research with a strong social or behavioural component.

Private foundations have been at the forefront of calls for researchers to demonstrate the utility and the value of their work; public funders are following suit. Unless the global economy picks up, this trend is likely to continue. This makes it harder to get funding for basic science around emerging health threats or for research investments that may not show a "return" for a decade or more. It is also hard to get funders to invest in the more intangible parts of the research process -- engagement with communities where the research will take place, discussion of research results with the communities, local decision-makers and others who might be able to use those results to improve lives.

You can't do high quality, ethically sound health research without cash. Researchers naturally write proposals that they think funders will buy. The agenda will always be skewed towards funders' interests.

#### **Questions raised:**

- Is there a way of aligning the interests of funders in rich countries with the interests of the people who bear the brunt of ill health in poor countries?
- What can we do to influence voters (and heads of foundations) to demand needs-based research?
- How can we make an effective case for investment in research that may not demonstrate value for many years?
- Who's responsibility is it to undertake, and pay for, the public engagement work that situates research in its social and political context and makes it more likely that results get use?

#### **Fashion**

Some diseases and development issues are just sexier than others. This is partly because they are easier to "sell" to the electorates in developed and developing countries alike, so they bring in the money. But voter preferences tend to be relatively constant: children open more purses than adults, women, especially mothers, excite more sympathy than men. "Development fashion", in contrast, changes over time, coalescing around a particular issue, disease, population group or even geographic area, often as the result of political posturing.

Since 2000 the Millennium Development Goals have determined what is at the top of the development fashion parade. Several of these relate to health. This has influenced the health research agenda; many researchers have focused on testing

interventions that may help meet the goals while a whole body of methodological work has focused on measurement of progress towards the goals.

Development fashion undermines rational health research because it concentrates resources around single illnesses or conditions, and sucks oxygen away from other issues that may provide fewer photo opportunities but be more important. When something is in fashion it sprouts an industry of its own, with dedicated research programmes, university departments, NGOs etc. These tend to persist even after the issue has fallen out of fashion, leaving a fragmented health system and research landscape in its wake.

The fragmentation that results from development fashion makes it hard to situate a health condition (and therefore health research) in the broader context of poverty, environment, governance, and the many other contextual influences which will determine how useful a disease-specific research question is, or how likely it is that research results will contribute to changes in welfare.

It takes a while for a health issue to become fashionable; if the research agenda is dictated by development fashion, that can mean that researchers are perpetually looking at yesterday's problems.

### **Questions raised:**

- Do we understand why some things become fashionable while others don't?
- How could we intervene in the process, to make research into hand washing or oral hygiene as "sexy" as research into maternal mortality or HIV?
- When funding comes in disease-specific streams, how do we move to a "horizontal" approach in health research? Do research institutions have the courage, for example, to pool funding across research projects so that grants for fashionable research subsidise unfashionable research? Do they have to lie to funders about it?

### **Research culture**

There is nothing new in the idea that human health is influenced by far more than pathogens. Cash, knowledge, environment, politics, beliefs, health services and any number of other things affect our physical and mental well-being. And yet for many, "health" research is synonymous with biomedical research, a discipline with its own sub-culture.

Dominating the sub-culture is the tyranny of the randomised controlled trial (RCT), the "gold standard" for biomedical research. This research design is ill suited for learning about aspects of health and health service provision which are deeply contextual. The health research establishment therefore systematically neglects research that can answer questions about complex interactions between health, policies and culture. Where such research is attempted (for example research into the newly-fashionable "social determinants of health") the methods and models used tend to be linear cause and effect models common in RCTs. We have the tools to measure poverty as a contributor to ill-health, but not to measure circular relationships in which ill-health also contributes to poverty, for example.

In addition, it is sometimes impossible to provide "gold standard" evidence. If an intervention is known to work (and sometimes simply if it assumed to work and has

been enshrined in policy) it would be considered unethical to randomise research participants not to receive it. We can not, for example, generate "gold standard" evidence in favour of sterile injecting programmes for heroin addicts because we know, even without an RCT, that sharing needles spreads blood-borne pathogens. It would be unethical to randomise some drug injectors to share needles and other to use sterile needles; this leaves us with strong but imperfect non-trial evidence.

While funders increasingly ask for evidence of research "impact", they continue to use publications and citation indices as metrics. This further undermines the professional incentives to engage in the "softer" parts of the research process such as community engagement. Publication in peer reviewed journals and presentation at conferences have become a substitute for dissemination of results to communities and discussion of new knowledge with policy makers.

The culture of biomedical research is centred ever more around large, expensive, multisite studies that require huge human and financial capital. This means research is increasingly concentrated in the hands of large, bureaucratic and often risk-averse organisations.

### **Questions raised:**

- What methods are most appropriate for non-clinical aspects of health research?
- How can we increase acceptance of those methods within the biomedical establishment, including in academia and among funders of health research?
- What training is needed to increase skills in this area, and can that training be provided as part of "mainstream" research education?
- How can we create encourage and support daring and innovative research outside the constraints of established research organisations?

## **Diagnosis two: Research results will never be the most important determinants of health policy**

Even more so than research itself, health policy and practice exists in a political, economic and social context. Individuals making health policy are influenced by a very wide variety of factors; research data are usually one of them, but rarely the dominant one. Apart from anything else, it rests on the assumption that politicians and governments would want to prioritise health interventions that deliver the greatest amount of health to the greatest number of people in the country. That is demonstrably not true in a very large number of countries around the world.

The factors influencing the policy process are similar to those influencing decisions about research priorities: money, votes, personal and professional incentives. Where a stint in government is seen as an opportunity to put private and political party noses into the public trough, the cost-effectiveness of one health intervention over another is of very little consequence. Most of the non-pathogenic determinants of ill health, including bad health systems, share a common determinant: bad and/or corrupt government. Until we address that, nothing changes.

In theory, democratic processes reduce rent-seeking and deliver better services to people because they allow voters to hold politicians and other decision-makers to

account. But the people most in need of health services, whose interests ought to be served by equitable health research, are often at the margins of society and the democratic process. In addition, democracy promotes short-term thinking: politicians are often unwilling to make investments that might show no benefit until after the next election. This works against the uptake of research with long-term implications.

We have seen many examples where large bodies of research point clearly to a policy solution which is never adopted. The research community's response to this "unwanted research" is very often to suggest yet more research, as though the results of the 387th study will somehow change the minds of people who are ignoring the results of the first 386 studies. If scientific research results are not being used for political, economic or cultural reasons, more scientific research results will probably not change the picture.

Research is sometimes ignored because it undermines the entrenched interests of governments, large corporations, national or international institutions or the medical profession. Where that is the case, those entrenched interests often conspire to bury research results. While clinical trial registries are a baby step towards increasing transparency in biomedical research, the peer review process performs very poorly in minimising deliberate distortions in communication of research results by entrenched interests.

There has been remarkably little research into the factors which increase or impede the use of research results. Health researchers often do not have a clear sense of who the potential "end-users" for their research results are; they often give little thought to whose job it might be to translate research into policy and rarely budget for anyone to do that job.

#### **Questions raised:**

- Why do we persist in doing more research in areas where current research results are ignored for political reasons? Would it not be better to spend less on generating more data and more on lobbying?
- What research is needed to understand existing policy processes? Is it possible to develop transferable models for successful research translation in such a context-specific area?
- Who are the potential "end-users" for health research results? Is there anything that researchers can do differently to help meet their needs?
- What is and what should be the role of researchers in translating health research into policy and practice? Who else should have a role in this area, and who should pay for this work?

### **Diagnosis three: Today's training will not meet tomorrow's needs**

In order to meet the public health challenges of the future, we need to look at health in its political, economic, social and cultural context. Geography and environment will clearly be part of the picture too. Our educational systems, while singing hymns of praise to "interdisciplinary" research, still train people to belong to one

discipline or another. Our educational systems are poorly equipped to teach individuals a number of disciplines, and our professional set-up devalues generalists.

It is also difficult to find support and funding for the institutional development that is needed to create a significant corps of well-trained, adequately resourced research institutions in developing countries, especially in sub-Saharan Africa. "Partnerships" between northern and southern academic institutions tend to be unequal; for one thing, research funding is most often filtered through the northern partner, which captures large overheads and a significant proportion of principal investigator positions. International institutions also provide well-paid employment for well-trained researchers from developing countries, often outside of their own country. Strong ties (some of them dating from colonial times) between a handful of northern institutions and a handful of developing countries have concentrated research projects and training opportunities in narrow geographic areas. This creates a vicious circle of neglect, leaving many countries with little locally relevant research, and no capacity to generate it.

Many of the skills needed for health research, including analytic and data management skills, are highly transferable. This means that in countries where these skills are in short supply, health research competes with banking, information technology and other higher-paying data-rich industries for staff. At least in terms of salary and working conditions, health research is rarely the winner.

**Questions raised:**

- What must change in our academic and practical training structures to allow individuals to think across traditionally defined disciplines?
- How can careers in health research in developing countries attract and keep bright, well-trained people in the face of competition from institutions abroad and other sectors at home?
- Is there a need for greater equity in north-south research partnerships, and if so how can it be achieved?
- What can we do to predict future influences on health in developing countries, so that universities and other training institutions can begin developing the necessary skills sets for relevant research?

We hope this provocation will stimulate you to agree, disagree, debate, restate problems, propose solutions and contribute to a lively conference at the end of the month.