

Who is at the receiving end of our innovation?

Innovation. A good, solid word, that. Not as smarmy as many others in the lexicon of corporate buzz-speak (“proactive”, “incentivize”, the hardy perennial “empowerment”), but glitzy enough to get papers accepted, bring in grant money and successfully sell far-fetched concepts and ideas. And rightfully so, maybe – it’s what makes research exciting and keeps researchers on their toes; without innovation, without the continuous drive to approach problems in new and inventive ways, research would be reduced to nothing more than bookkeeping, an intellectually sterile task.

As applied to “health care for all”, though, I can’t help but wonder if it’s not a hollowed out term – a term designed to cash in on the grant money without necessarily delivering the goods. Allow me to clarify: biotech projects focussing on healthcare challenges (HIV being a case in point) in the developing world tend to rely heavily on new, emerging – yes, innovative technologies, without fail claiming that these pioneering approaches will lead to a substantial improvement in the healthcare situation of any number of patients. I used to buy into this idea – after all, I’m a molecular biologist working on HIV, I work with genome-wide microarray profiling systems, recombinant fluorescently tagged viruses and magnetic cell separation techniques all day long. Innovation is what I *do*. I remember choosing specifically for biotechnology at university precisely for its cutting edge allure, the idea that in such a fast-moving field sufficient intellectual effort and highly advanced technology could make an actual contribution to the living standards in many of the worst-hit areas in the world.

But do they? Do they really? Is innovation in research, the constant development of new ways of doing science, really the answer to the problems of the developing world? Or are we maybe fooling ourselves (and our funders) and is the application of ground-breaking, cutting-edge technology in resource-poor settings no more than a justification for us to use said technology, without actually delivering a return for the people in the afflicted regions of interest? Certainly there are returns for the researchers in question, in terms of high-ranking publications, patent applications and scientific status, but how much of this flows back to the people who are actually involved? And, more importantly, are these innovative high-tech approaches to solve extant issues really what we’re waiting for?

I remember a drowsy course in parasitology, long ago, during which we were shown a set of illustrations which piqued my interest – a geographical map of sub-Saharan Africa showing the regions at risk for trypanosomiasis (in humans colloquially known as sleeping sickness), and a graph depicting the incidence of trypanosomiasis in – I think – the Democratic Republic of Congo over the past 80 years or so. What was striking is this: risk for

trypanosomiasis is strongly associated with political instability. The higher the civil unrest in a specific region, the higher the rates of trypanosomiasis become. Worse still: trypanosomiasis dropped to its lowest incidence rates in the DRC during the relative political stability of the colonial years, when – despite the evils of colonialism – strict logistical measures were taken to control disease, and leapt up to staggering heights soon after independence and all associated civil unrest. Now, this is a dangerous thing to say. It's a *terrible* thing to say. As a Belgian, I am only too aware of the nightmares of our colonial history and would never dream of defending this period of national shame. Nevertheless, what these graphs suggest is that the underlying *biology* of the disease, i.e. the aspect which we're now targeting with our innovative and costly approaches, may not really be the challenge at hand.

In other words, these data raise a set of simple yet fundamental questions: if you want to combat a disease such as trypanosomiasis, is it better to fund an innovative molecular biological analysis of the interplay between parasite and the host immune system in the hopes that it will one day yield a possible (but probably expensive) therapeutic strategy? Or is it preferable to alleviate the social conditions of the regions at risk using non-innovative approaches (prevention strategies, logistical support, etc.) which we already have at our disposal? With the added benefit that trypanosomiasis is not the only disease which can be tackled in this fashion – many, many diseases go hand in hand with poverty, famine and war, and would be considerably reduced as social conditions are improved.

To take it to the personal level: should I really have studied biotechnology when I wanted to provide some form of aid (perhaps young and naively – I think William Blake's lines "Can I see another's woe/And not be in sorrow too?/Can I see another's grief/And not seek for kind relief?" even came to mind at some point) to the disease-stricken regions of the world – a choice which so far has not delivered any actual benefits to anyone in the field? Or should I simply have studied Economics, skipped the lengthy PhD-process of scientific advancement and attempted to contribute something at the logistical level?

I would like to stress that I do not know or pretend to know the answers to these questions. Presumably, there is no correct answer, or if there is it will be along the predictable lines of "both approaches are needed to efficiently combat disease in resource-poor settings". Nevertheless, I cannot help but feel frustrated about our focus on technological innovation as the be all, end all solution for the developing world's problems. On the one hand frustration that opportunities are being missed: while money is being spent on academic publication-oriented questions, albeit interesting ones, many problems could actually be solved using extant knowledge and extant technologies, albeit decidedly unsexy ones. On the other hand, but this might merely be a private gripe of mine, frustration concerning the dubious phrasing used in all project applications everywhere, in which new-fangled technologies are being pushed forward as the solution to all the developing world's problems, when in fact they are just the newest toys that we'd like to play with.

A project we're collaborating on now, for instance, focuses on HIV and tuberculosis co-infection in sub-Saharan Africa, and is an intellectually challenging amalgam of advanced molecular and cellular strategies designed to unravel a specific disorder associated with antiretroviral therapy in a resource-poor setting. It's an innovative project, certainly, with an accordingly high price tag. From a research point of view, it concerns work which indubitably needs to be performed. However, was it entirely honest of us to describe it in the project application as a direct contribution to HIV and therapy management in the field? Wouldn't it have been *more* honest to say that we will in the first place improve our track record in this field of research, rather than improve standards of care? And wouldn't the local population suffering from this disorder have been better off if the same kind of effort devoted to research was also devoted, again using non-innovative approaches, to improving their quality of life, thus bridging the gap between their current situation and the ten or fifteen years down the road when our results are translated into actual solutions.

In conclusion, I would like to state that in no way am I advocating a reduction in the research efforts in the "health care for all" context: I have always believed and continue to believe that scientific progress will shape the future – indeed, it is not an overstatement to say that it might determine whether we *have* a future. What I am questioning, however, is our focus on technological innovation as the quintessential solution to many of the developing world's challenges, a habit which in the long run might be more self-serving than public interest-serving. Selecting the best strategy for providing optimal health care for all in disregard of our own academic or other track records might be our biggest challenge yet.