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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" and 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 is 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 cannot help but wonder if it is 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 health-care challenges in the developing world (HIV being a case in point) tend to rely heavily on new, emerging – and yes, innovative – technologies, claiming without fail that these pioneering approaches will lead to a substantial improvement in the health-care situation of any number of patients. I used to buy into this idea. After all, I am 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 biotechnology at the 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 this 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 targeted? And, more importantly, are these innovative, high-tech approaches to solve existing issues really what we are waiting for?

I remember a drowsy course long ago in parasitology, during which we were shown a set of illustrations which piqued my interest – a 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 greater the civil unrest in a specific region, the higher the rates of trypanosomiasis become. Worse still, trypanosomiasis dropped to its lowest incidence rates in that country during the relative political stability of the colonial years, when – despite the evils of colonialism – strict logistical measures were taken to control disease. Incidence rates leapt up to staggering heights soon after independence and the onset of all the associated civil unrest. Now, this is a dangerous thing to say. It is 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 that we are 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 the parasite and the host immune system in the hope that it will one day yield a possible (but probably expensive) therapeutic strategy? Or is it preferable to alleviate dire social conditions in the regions at risk using non-innovative approaches (prevention strategies, logistical support, etc.) that we already have at our disposal? – with the added benefit that trypanosomiasis is not the only disease that 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 to the disease-stricken regions of the world? (Young and naïve, 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.) This choice has so far not delivered any actual benefits to anyone in the field. 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 and end-all solution for the developing world's problems. On the one hand is the 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 existing knowledge and technologies (albeit decidedly unsexy ones). On the other hand is frustration (but this may merely be a private gripe of mine) 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 would like to play with.

A project we are collaborating on now, for instance, focuses on HIV and tuberculosis coinfection 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 is an innovative project, certainly, with an accordingly high price tag. From a research point of view, it concerns work that 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? Would it not 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 would not the local population suffering from this disorder have been better off if the same kind of effort devoted to research were also devoted – again using non-innovative approaches – to improving their quality of life, thus bridging the gap between their current situation and the 10–15 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 research efforts in the context of health care for all. 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 may 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 that, in the long run, may be more self-serving than public interest-serving. Selecting the best strategy for providing optimal health care for all while disregarding our own academic or other track records may be our biggest challenge yet.

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