

Best Science for the Most Neglected: What can we learn from the DNDi model at 10 years?

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Ten years ago, I was working as a physician in a remote hospital in Sudan, faced with an epidemic of a fatal wasting disease called Leishmaniasis. At the time, I had just one antiquated, toxic drug to treat my patients. The world looked pretty bleak then. A decade later, things have significantly changed in the field of neglected tropical diseases (NTDs) thanks partly to product development partnerships (PDPs) like DNDi. A recently released report on DNDi provides an excellent snapshot of its 10 years' experience and just why it has made such an impact.

DNDi is not only an example of a successful PDP; it is also an experiment in conducting R&D with a very different paradigm - innovation in innovation. This new model was desperately needed in order to find solutions for diseases that were neglected by the 'market' – thus leading to a fatal imbalance of R&D output to disease burden and needs.

The DNDi model essentially has four key principles: a patient centred business model; 'access'-to knowledge and products; financial and scientific independence; and collaboration with partners including those from endemic countries. These principles have led to some important features that have in turn helped drive success, including short- and long-term disease portfolio strategies; a diverse range of public and private donors; and an access policy that 'de-links' – or separates – the cost of R&D from the final price of products.

After just 10 years, the proof of DNDi's success is in the six new treatments delivered and 12 new chemical entities (NCE) in the pipeline, all for less than €183 million. Incredibly, even taking into account drug candidate attrition, DNDi anticipates that the cost to bring a new chemical entity from discovery to the market to be just between €100 million and €150 million. There are caveats to this figure, however, including considerable in-kind contribution costs associated with the DNDi model, and assumptions of success. Nonetheless, it is a far cry from the billion dollar figure often quoted by pharmaceutical companies as the cost of R&D. DNDi's success indicates that when done well, a PDP can be a very cost-effective model that can deliver new tools, without necessarily resorting to conventional wisdoms of what is essential for innovation.

It is necessary, however, to draw a few cautionary notes as well. The field of NTDs represents such a lack of commercial interest as to actually make it a non-threatening field for public and private collaboration. Industry can play a constructive role and actually work together with minimal expenses, while reaping rewards in corporate social responsibility as well as benefiting from important advances in science and innovation. The same may not hold true for diseases like tuberculosis or hepatitis where commercial considerations start to creep back in and proprietorial approaches to research predominate.

Furthermore, the PDP model in general is highly dependent on continued long-term public and private funding. This poses real challenges in a period of economic hardship. It is far from certain that DNDi itself will be able to deliver in such a successful way without sustainable financial resources. The DNDi model may only have been possible over the last 10 years considering its portfolio and size. The next challenge will be for the organisation to continue to keep the integrity of its model while coping with institutional growth, staff turnover and an evolving portfolio.

It is worth reflecting how PDPs can work in the next decade, taking into account some of the broader policy discussions now in play around innovation and access. A recent report by the World Health Organization's Consultative Expert Working Group on Research and Development: Financing and Coordination, made several interesting recommendations that could significantly benefit the PDP model.

For instance, a solution that could address the concern around resources may be to consider innovative and sustainable funding mechanisms to support R&D for neglected diseases and other public health priorities (such as TB or even new antibiotics in general). Existing institutions like UNITAID could be the blueprint for such a mechanism, which could also benefit from the support and contribution of emerging economies as well as endemic countries - India, China and Brazil are good examples of both.

In addition, incentive mechanisms which also promote the attractiveness and not just 'de-risk' R&D for neglected public health priorities could be created. Examples include prizes to developers who collaborate and contribute their technologies that then meet specific milestones in the R&D cycle or eventually become successful products. Management of intellectual property to promote open approaches to R&D could also be encouraged through mechanisms like patent pools.

Finally, in order to facilitate research, regulatory harmonisation and fast track mechanisms could be developed. PDPs should work together in their common interest to promote such mechanisms - which could range from regional regulatory bodies that evaluate files for approval for a group of countries, to even a supranational regulatory entity, based on the current WHO prequalification (PQ) model, which can focus on specific public health priorities on behalf of concerned endemic countries.

The true paradigm shift would be to find ways to make needs-driven R&D for public health priorities attractive in its own right, and to use public and private partnerships – epitomized by PDPs – to promote coordination, develop R&D strategies, pool IP and technologies and foster global collaborations. A PDP 2.0 model would therefore be a worthwhile vision for DNDi and others to aspire towards.

It is worth reflecting that one of DNDi's achievements has been in developing, to phase III clinical trials, an old abandoned NCE, fexinidazole. This NCE had initially been identified with anti-parasitic activity back in the 1970s. It is now being tested by DNDi in patients with sleeping sickness. Sadly, our broken R&D system is exactly why such a drug never originally saw the light of day. It spent more than 30 years sitting, undeveloped, on a shelf for lack of commercial interest, while doctors like me were left using old, antiquated and toxic treatments. It's why countless numbers of patients have died and why diseases that we actually know so much about remain untreated and unchecked. Perhaps it's time to also learn the lessons of what can be used from the DNDi model in other fields of research so that our science and technology can serve public health needs and people, and not just the market.