Targeting research to address societal needs:
What can we learn from 30 years of targeting neglected diseases?

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This version: 9 March 2022

Abstract

Neglected diseases have been characterised as a misalignment in the research system because so little research is directed towards such a large burden of disease. By focusing on one of the most extreme cases of misalignment, we highlight opportunities and potential pitfalls of targeting research towards specific social outcomes more generally.

We trace how the category of neglected diseases came to prominence, largely framed as a problem to be addressed by scientific research. This mobilised R&D investment but there have been unintended consequences, particularly for broader health system strengthening and research capacity building efforts.

These developments may contribute to a 'tragedy of the evaluation commons', where the effectiveness of broad remit research programs remain poorly characterised. This exacerbates a lack of evidence, relative to targeting specific diseases where the evaluation challenge is narrower. Research targeting then, is intimately tied to evaluation practice.

A new research and policy agenda oriented towards broader research evaluation may support further investments not just for biomedical R&D in high-income countries, but also for researchers in other countries, for interdisciplinary, applied and social sciences, and ultimately, for poor patients.

Keywords: public research funding, research targeting, neglected diseases, research capacity building, health system strengthening, research evaluation

1 Introduction

Over the last half century, there has been wide appreciation for the role of research in stimulating innovation, supporting economic growth and improving health. Recognising the diffused effects of research has provided justification for public funding of research, and rationales for increases in the scale of that public funding (Arrow, 1962; Bush, 1945; Nelson, 1959).

Whilst funding for biomedical research has seen remarkable growth, that growth has recently slowed down and *research targeting* – the notion of setting priority areas to address with research – has taken on greater importance. Across the world, research targeting is on the rise, not just in biomedical research but in scientific research more generally. Appetite for targeting publicly-funded research and steering its direction towards addressing specific social goals is growing (Kuhlmann & Rip, 2019; Sarewitz, 1996; Stirling, 2009).

There has been increasing attention on the direction of scientific and technical change for addressing some of society's most complex and urgent problems, ranging from mitigating climate change to meeting the Sustainable Development Goals, from pandemics to other Grand Challenges (Lund Declaration, 2009). Conceptual and policy efforts have taken the form of a 'developmental state' (Block, 2008; Fuchs, 2010), calls for 'responsible research and innovation' (Stilgoe et al., 2013), 'mission-oriented' research (Foray et al., 2012; Mazzucato, 2018; Sampat, 2012), and 'transformative innovation' (Schot & Steinmueller, 2018).

This shift in science policy efforts also relates to different meanings of 'public' in public research; to the conceptualisation of R&D programs as policy levers for steering researchers towards delivering public aims; and to the relationship between public research funding and the need to transform research systems to address complex challenges such as sustainability (see Bozeman's, Reale et al.'s and Bührer et al.'s chapters respectively, chapters 2, 10 and 12 in this *Handbook*).

To learn more about targeting publicly funded research, we examine 'neglected diseases' as an extreme case of a societal need, for which there has been a clear market failure that justifies publicly funded research, and a clear track record where targeted R&D investment has been sustained over time. The term 'neglected diseases' was first used in the late 1970s with the launch of a network of laboratories devoted to researching the 'great neglected diseases of mankind' (Keating, 2014; Molyneux et al., 2021). However, it was not until the late 1990s – when neglected diseases were widely characterised as a misalignment between research priorities and societal needs (CHRD, 1990) – that they became more generally recognised as a problem.

Whilst this helped to direct attention towards funding for R&D as a way to redress the misalignment, there may have been some unintended consequences, particularly for health system strengthening and research capacity building. Additionally, the difficulty of evaluating such efforts, compared to the relative ease of evaluating more targeted research, may contribute to a 'tragedy of the evaluation commons', where wide remit priorities are overlooked despite their usefulness to multiple stakeholders.

The evolution of neglected diseases, as its own distinct category of research, illustrates some of the dynamics and tensions associated with the targeting of public research funding. These forces may also be playing a role in less conspicuous cases of misalignment between scientific research and societal needs.

The challenges of targeting and evaluating research in this extreme case may well be a subset of a more general phenomenon. As such, the promises and pitfalls of targeting R&D towards neglected diseases should be of broader interest to research policy scholars.

Section 2 provides a brief history of how the case for addressing neglected diseases with targeted research was developed. Section 3 examines some unintended consequences. Section 4 discusses how research evaluation can lock-in further research targeting. Section 5 concludes that targeted research efforts may leave behind other complementary investments, which does not necessarily reflect their lower priority but may instead indicate greater evaluation complexity.

2   A historical and conceptual account of neglected diseases

Public research funds targeting neglected diseases have increased from an estimated 1 billion USD in 1986 (CHRD, 1990) to 2.6 billion US$ in 2019 (Policy Cures Research, 2020). Whilst the doubling in research funding for neglected diseases is remarkable in itself, focusing only on its growth takes neglected diseases largely as a given in terms of what they are, and the range of ways in which they might be addressed. Here, we describe how neglected diseases as a category was developed, how it became characterised as a misalignment in the research system, and how that conceptualisation came to dominate wider global health policy.

The conceptualisation of neglected diseases and attempts to address them have co-evolved over time. In the 1970s, post-war development organisations (e.g. WHO, Rockefeller Foundation) grew increasingly concerned that the health advances seen in the developed world could not be transferred to developing country contexts, and focused their efforts on the need for 'appropriate' technologies (Mahoney & Morel, 2006).[i]

In 1990, the Commission on Health Research for Development (CHRD) published an influential report, which highlighted a severe lack of R&D funding devoted to addressing the health problems of developing countries compared to those of industrialised countries (CHRD, 1990). This was later referred to as the "10/90 Gap" because less than 10% of health research funding was devoted to diseases which caused 90% of global disease burden (GFHR, 2000; MSF, 2001).

To address the 10/90 Gap, the CHRD report proposed: national research, especially in developing countries; international collaboration between scientists to address global challenges; building research capabilities to address developing country problems; and improving international arrangements for monitoring, assessing and promoting the health problems of developing countries (CHRD, 1990). Notably, increasing targeted research funding could be considered as only one of the possible implications of the report's findings.

Another prominent report reinforced the view of illness in the developing world as an economic problem that warranted policy intervention and proposed increasing government spending on health (World Bank, 1993). It argued for more scientific research, but with the proviso that a higher proportion of both international and national research support should be directed towards the needs of developing countries such as epidemiology, preventive medicine, the development of childhood vaccines and inexpensive medical technologies, and research to generate local solutions to local problems.

The World Bank report also introduced the disability-adjusted life year (DALY), combining mortality and morbidity into a single measure, which could then be used to compare national, regional, and global disease burdens (Chen et al., 2015). Along with the ability to draw on science and technology statistics (first standardised in 1963 with the introduction of the OECD Frascati Manual (Godin,

2005)), DALYs made it possible to view diseases in terms of how much R&D funding they received relative to the burden of disease they caused. This allowed advocates and analysts to compare diseases directly and observe that some fare better than others in research funding allocations (Gross et al., 1999).

These and other high level reports (WHO, 1996, 1999), alongside advocacy efforts that exploited the ability to compare diseases in terms of research funding and burden of disease, began to build momentum for addressing the health problems of developing countries, and in particular for addressing them by investing in R&D. These efforts served to conceptualise the challenge of neglected diseases principally as a problem of R&D shortfall. Moreover, the R&D shortfall had an obvious culprit: market failure.

The *rationales* for targeting neglected diseases were thought to reside largely in the public sector, but there was a recognition that many *capabilities for addressing neglected diseases* were largely in the private sector. By the early 2000s, there was a growing perception that public-private partnerships could offer institutional solutions (Chataway & Smith, 2006). A sub-type of the public-private partnership known as the product development partnership (PDP) arrived with the emergence of new global health actors such as the Bill and Melinda Gates Foundation. Notable examples are the International AIDS Vaccine Initiative, and the Medicines for Malaria Venture (Chataway et al., 2007; Hoogstraaten et al., 2020).[ii]

PDPs began to develop their own unique capabilities (knowledge brokering and integration) for addressing the *innovation system failures* associated with neglected diseases and not just the *market failures* (Chataway et al., 2007)*. As such, PDPs have been characterised as "organisational experiments… social technologies" (Chataway et al., 2010) that "drive product development for neglected diseases" (Hanson et al., 2012). In this PDP model, not only is a disease targeted, but the way of targeting it (i.e. the product) is also specified, focusing on largely technology-intensive solutions.

The development of the neglected disease category first (and foremost) attracted attention to what was previously neglected, and secondly framed the problem principally as a shortfall of R&D investment. The category also set in motion the development of new institutional forms to address the problem and created a demand for better data and analyses to support funders who might want to invest in neglected diseases.

With improvements in the availability of data[iii], a new body of literature emerged highlighting misalignments between biomedical research and societal needs (Viergever, 2013). One of the first contributions deploying this approach compared disease-specific research funding by the National Institutes of Health, with measures of US disease burden (Gross et al., 1999). It found some diseases were relatively overfunded (e.g. AIDS, breast cancer, diabetes mellitus, and dementia) and others were relatively underfunded (e.g. chronic obstructive pulmonary disease, perinatal conditions, and peptic ulcer). Notably though, the authors left it largely to the reader and commentators to decide whether their analyses constituted misalignments.

Extending the approach beyond the US, Røttingen et al. (2013) found "only about 1% of all health R&D investments were allocated to neglected diseases" and a "persistent imbalance between R&D investments and needs-based priorities". The extreme disparity meant they could be explicit about misalignment. So, compared to Gross et al. (1999), they were more forthright about policy implications: *"The need to align investments in health research and development (R&D) with public health demands is one of the most pressing global public health challenges"* (Røttingen et al., 2013).

A stream of further studies built on this work. One compares the production of global health knowledge (in the form of publications) to 'the global market for treatment' (based on multiplying DALYs by gross national income per capita) (Evans et al., 2014). Another adds a 'neglect factor', based on the ratio of disease burden to R&D expenditure (von Philipsborn et al., 2015). More recently, since the inception of the WHO's Global Observatory on Health R&D, studies have analysed funders, recipient countries, grant type and duration, product type, and collaborations (Adam et al., 2019; Ralaidovy et al., 2020). All of these studies highlight conspicuous misalignments between biomedical research and global health needs in various ways, suggesting a degree of robustness to their overarching claims.

Probing what might be driving these kinds of misalignments, Yegros et al. (2020) investigated the influence of geography, industry and publication incentives on health research efforts globally. They found that "diseases which are more prevalent in high income countries [HICs] generate ten-fold more research attention than those in low-income countries" and that researchers receive more citations when they work on HIC-diseases. This is likely to be a reflection of research activity and funding being heavily concentrated in HICs, however it also suggests that academic publishing might incentivise researchers to focus on diseases of the rich, even researchers in low- and middle-income countries (LMICs), regardless of local LMIC priorities.

Despite the proliferation of research identifying misalignments, it can remain difficult to discern what 'correct' alignment would look like, and to what extent, if at all, the growth of targeted research will be able to address these societal needs. As such, all but the most extreme misalignments will remain highly contested. These questions should be included in a future research agenda in this area. An important strand for such an agenda is to recognise that analytical misalignments sometimes arise from the categories being used in the analysis. There is a need to appreciate *the nature of the categories* being targeted for research.

Diseases have come to occupy premier policy relevance in organising social resources towards addressing health problems. As a category, they have their own sets of causes and consequences. They bring together an array of symptoms, pathogens, treatments through a process of diagnosis (Rosenberg, 2002; Thagard, 1999). This means that there are multiple ways of grouping diseases into classes.

We highlight the prominence of neglected diseases as a category where "classifications should be recognized as the significant site of political and ethical work that they are" (Bowker & Star, 2000). The category of neglected diseases is *doing work*, contributing to efforts to define a problem and attract more investment to address a moving target (Best, 2019; Blume, 2003; Hacking, 1995; Mackenzie, 2006). Quantification of both disease burden and research investment allows classification into categories that facilitate commensurability. Both classification and quantification have been conceptualised as social processes, requiring social and intellectual effort to construct and maintain (Espeland & Stevens, 1998; Porter, 1995; Power, 1997).

For the purposes of targeting resources, focusing on diseases can make problems more comparable, and cast light on previously overlooked allocation choices. A variety of disease category labels have been deployed in a similar vein to the neglected diseases category to highlight the plight of those marginalised from health gains. Consider, for example, the variety of disease classes listed in Table 1 and the varying conceptualisations they invoke.

Table 1: Categories of disease and their performance

Category of disease	A description of the category label and its social performance
Preventable diseases	More than a quarter of the world's population does not have access to essential medicines (Chan, 2017). By highlighting that some diseases can be treated with basic drugs, this gives rise to the idea of preventable disease, premature deaths, or in Paul Farmer's terminology, "stupid deaths" (Farmer, 2003).
Neglected diseases	These are distinguishable from preventable diseases because adequate treatments are not available yet. The focus is switched from access to existing medicine, to R&D for new or better medicine.
Rare and tropical diseases	Rare diseases have small market size because they affect few people. In contrast, tropical diseases have small market size because they affect a large number of poor people. So, the label serves to emphasise its geographical incidence rather than its relation to poverty or how deserving its patients might be.
Emerging diseases	This category serves to prompt urgent responses, marshal resources from security agendas that often have larger budgets, and encourage the strengthening of a global monitoring and detection apparatus.
Big three diseases	A sub-category that commands more than 2/3 of all global R&D funding for neglected diseases (Policy Cures Research, 2019) and highlights differing degrees of neglect within neglected diseases (von Philipsborn et al., 2015).

Despite attempts to define neglected diseases, in practice the grouping is nebulous and quickly decomposes into other, often overlapping, categories. The term 'neglected diseases' can be a shorthand reference to neglected tropical diseases (NTDs), which in turn are categorised differently across stakeholders (Hotez et al., 2020; Molyneux et al., 2021; WHO, 2021). Neglected diseases can also refer to a wider set of poverty-related and neglected diseases (PRNDs), where they include 'The Big Three': malaria, tuberculosis, and HIV/AIDS (Cochrane et al., 2017). The Big Three are big in the sense that, together, they cause the high morbidity in terms of DALYs (as opposed to high mortality). Hotez (2013) argues that we should go further and talk about 'The Gang of Four' to include the morbidity of NTDs. To complicate definitions further, some HIV/AIDS R&D funding is omitted because the disease is prevalent in HICs as well as in LMICs, and so some HIV/AIDS research funding cannot be attributed to being part of an effort to address neglected diseases (Moran et al., 2009).

This provides an illustration of the ways in which *we define the category of neglected diseases matters* – for example, if we exclude some investment in HIV/AIDS R&D, and if we emphasise morbidity (rather than only mortality), neglected diseases seem more neglected.

3 What do we know about unintended consequences of focusing research funding on specific social outcomes?

We identify six possible unintended consequences of targeting research funding to specific diseases. The discussion below suggests that many of these are likely to be relevant to research targeting more generally.

Firstly, the plethora of new actors and organisations targeted towards specific diseases or products has created coordination and prioritisation challenges. This has resulted in "gaps in funding and distorted priorities" and has left LMICs "in the position of having to accept the net result of multiple uncoordinated decisions rather than benefiting from careful planning and investment" (Hanson et al., 2012). If successful, continued increases in R&D funding for neglected diseases will lead to a need

for large-scale clinical trials of new treatments and products and these will require even greater funding and coordination as well as local clinical trial capabilities (Moran et al., 2005). Thus, even if the main policy focus is on providing targeted R&D funding to solve a specific health problem, coordination and prioritisation challenges must be addressed.

Secondly, focusing research funding on addressing neglected diseases has increased the need to address social determinants of health, strengthen health research systems, and build research capacity. Addressing these needs may even achieve more, in terms overall health improvements, than efforts targeted at a particular disease (Chataway et al., 2019; Ncube & Chataway, 2019).

One possibility is that research funding targeting particular diseases has come in addition to these other priorities which have not seen similar increases in funding support. However, a more serious possibility is that targeted funding displaced these other priorities. It remains difficult to disentangle these two possibilities, but it is clear that targeting to particular diseases has become a general trend in global health.

Donors typically prefer 'vertical' disease-specific interventions, rather than 'horizontal' approaches, which would focus on strengthening entire health systems (Clinton & Sridhar, 2017). Vertical programmes are perceived as being easier to monitor and control, with results that are easier to measure. They are favoured by donors despite "near universal consensus that optimal health systems are the key to improving health" (Clinton & Sridhar, 2017). With around 40 different global health PDPs, all implementing largely vertical strategies that collectively pull resources into particular disease areas, bottlenecks in health systems and research capacity are accentuated (Ncube & Chataway, 2019). One could see this as a natural consequence of the way neglect was constructed as an issue in need of addressing with R&D, as discussed in section 2.

Thirdly, focusing research funding on neglected diseases helps sustain the false assumption that research capabilities can be accumulated automatically as a side effect of research targeting LMIC concerns - the mythology of 'learning by doing' (Bell & Pavitt, 1993; Scott-Kemmis & Bell, 2010). One reason the assumption does not hold is simply because remarkably little neglected disease research is in fact undertaken in LMIC countries. A recent analysis of the research funded by 10 major funders of health research revealed that 98.9% of biomedical research grants were allocated to researchers in HICs and only 0.2% to those in LICs (Ralaidovy et al., 2020).[iv] Another reason is that not all forms of research activity will actually build capacity to the same extent or in the same ways. So, even if more research is localised in LMICs, it will not necessarily contribute to building capabilities without due consideration of the research system in which it is embedded (national or global). A more dedicated, and often broader effort that explicitly targets research capacity building is often needed.

Over recent years, more direct efforts at building research capacity building have been made (Cochrane et al., 2014; Ghaffar et al., 2008; Marjanovic et al., 2013). However, building research capacity is itself prone to overly narrow targeting. In practice, the challenge is often reduced to promoting knowledge transfer or technical training "without parallel investments to develop and sustain the socioeconomic and political structures that facilitate knowledge creation" (Mormina, 2019).

One result of this focus is the salience of the "brain drain" problem, whereby LMIC researchers emigrate to HICs (Ghaffar et al., 2008). An alternative would be to conceptualise knowledge production as a more collective process. This might instead emphasise weak local demand for trained researchers, and create an imperative to "strengthen the different social, political and economic structures that make up a nation's innovation system" (Mormina, 2019). National system

differences are important because research funding priorities can differ between HICs and LMICs. In HICs, funding rationales based on scientific excellence and national relevance can both be pursued, whereas in LMICs scarce resources may mean pursuing multiple rationales is not feasible (Chataway et al., 2019). Balancing capacity building imperatives and addressing national priorities may not align with current perceptions of scientific excellence (Chataway & Daniels, 2020). In response to this problem, some have called for a more pluralistic view of research excellence, one that includes research capacity building (Kraemer-Mbula et al., 2020).

Fourthly, researchers themselves may react to research targeting in unanticipated ways. Examples include: "symbolic compliance"/ window-dressing (modifying the language used to describe research without changing the content of the research); "hoarding" (gathering more resources than needed to cope with funding cuts and the risk of failure) (Gläser, 2019); "bootlegging" (using resources that were meant for one purpose to fund another) (Hackett, 1987); maintaining research portfolios (enabling researchers to drop unfundable lines, start fundable lines and change existing lines of research) (Gläser et al., 2010); constructing "doable" problems linked to "fundability" (Fujimura, 1987). Researchers may also engage in goal displacement (in which gaining a high score according to evaluation criteria becomes the goal, displacing other research goals); in task reduction (suppressing tasks which are not counted in the evaluation); or structural changes to publication activities and research capacity-building (de Rijcke et al., 2016). Laudel's chapter in this Handbook also analyses "researchers' strategies for building and maintaining their funding portfolios" (Laudel, chapter 18 in this *Handbook*).

Fifthly, targeting research may have contributed to the rise of project-funding. This has become an increasingly important mechanism for supporting research. This mechanism is not only likely to cause a narrowing of goals, as researchers feel pressured to respond to funder priorities, but also suffers from instability, which may discourage researchers from staying in underfunded research areas (Richard Whitley et al., 2018). Even for those that might venture away from well-funded and stable research environments, project funding mechanisms tend to discourage researchers from moving into fields where they might not have a track record (Luukkonen, 2012). Of particular relevance to neglected diseases, LMIC researchers may be forced to adapt their research in more extreme ways due to the scarcity of resources in their research environment and the "high degree of authority over research agendas and methodologies" imposed by funders and research partners from the Global North (Laudel, chapter 18 in this *Handbook*).

Lastly, the pathways for addressing a disease are not defined by mere dint of having targeted the disease category with research. The multiplicity of routes to the target, as well as the nature of the target itself can be contested, and they matter for both the effectiveness and legitimacy of the pathways. Non-local research funding allocations shape research targets and priorities in ways that may not be the most appropriate for local contexts in LMICs (Cochrane et al., 2017). For HIC funders, the "choice of target area can reflect their own understandings rather than the needs and demands of low- and middle-income countries themselves" (Hanson et al., 2012). This is unlikely to produce long term solutions to the problems that matter to those living in LMICs (Cochrane et al., 2017) and it may result in the pursuit of technical options that seem strange and could subsequently lead one to misidentify the problem as a failure of research translation or 'implementation'.

A stark illustration can be found in podoconiosis, where exposure of bare feet to alkalic clay causes inflammation and progressive swelling (Deribe et al., 2020). Since there is some of degree of genetic susceptibility, there is genetic research into podoconiosis. However, people would not contract podoconiosis if they had shoes to wear in areas of irritant soil, and "footwear remains an unaffordable luxury for residents of most affected areas in the tropics" (Davey et al., 2007). For

want of shoes, genetic research can seem a peculiar approach to pursue. This peculiarity is not well addressed by a framework that emphasises only a misalignment between research supply and societal needs. That framework would suggest more targeted research, whereas the issue in fact relates to the kinds of research (or non-research) approaches brought to bear upon the problem.

Targeting research towards specific diseases has not corrected an imbalance between laboratory science and social science that has been identified as a problem by many (Gilson et al., 2011; Ncube & Chataway, 2019). It may even have reinforced and exacerbated some imbalances, as the prominence of a particular line of research is more pro-actively enhanced and reinforced through disease-based framing.

For example, Chagas disease changed "from a problem of precarious living conditions, to a problem of fumigation, and then a problem of basic research" (Kreimer & Zabala, 2007). Kreimer (2016) argued that by "positioning the production of knowledge about the parasite's DNA center stage", researchers displaced "other solutions to the Chagas problem, such as systematically fumigating rural houses". He concluded that: "In fact, less prestigious but more useful research could be conducted, such as the development of new kinds of insecticides. But such research would not allow the researchers to participate in international scientific networks" (Kreimer, 2016). The study illustrates the influence of the global science system, even on locally-oriented research topics.

To address these issues there is a need to build up local research capabilities, not only to absorb the knowledge being produced in HICs, but also to orient research in ways that better contribute to solving LMIC problems (Cochrane et al., 2017). In some ways, this is a re-iteration of the call made thirty years ago. The influential 10/90 report argued that "Strengthening research capacity in developing countries is one of the most powerful, cost-effective, and sustainable means of advancing health and development" (CHRD, 1990).

4 (Targeting can create) a tragedy of the evaluation commons

Funding targeted to specific diseases often leads to evaluation based on metrics for specific disease-related outcomes. For example, funding for the provision of HIV/AIDs antiretroviral drugs can be evaluated by measuring reduction in HIV/AIDs disease burden. Moreover, focusing on specific diseases and the impact of R&D for those diseases can help maintain political support for research. Evaluations of this kind can offer a clear indication of return on investments.

In contrast, investments aimed at broader health system strengthening, research capacity building and overall health outcomes, are much more difficult to evaluate. The difference exacerbates a lack of evidence on the returns to health system strengthening and research capacity building efforts, relative to that of investments targeting specific diseases where the evaluation challenge is narrower. Part of the appeal of research targeting, then, is intimately tied to evaluation practice.

Evaluation difficulties stem from at least two sources that we discuss in turn: the involvement of a wider set of stakeholders; and data collection from LMICs with conceptual frameworks that highlight complementarities across systems.

Firstly, the tragedy of the evaluation commons is that, although complex evaluations of broad-remit programs can be of benefit to multiple stakeholders, their delivery is costly and labour-intensive. Moreover, it is harder to justify costs if benefits cannot be easily appropriated by those who pay for the evaluation.

One way to mitigate these problems is to share the costs (and benefits) of the evaluation between multiple stakeholders. However, multiple stakeholders may have goals that pull the evaluation in

different directions, and make it difficult to provide clear evidence of returns on investment. Research evaluation can be have multiple purposes that include accountability, learning, advocacy and informing strategy; their design can be narrowly or broadly framed, deploying only a few or a wide array of indicators (Marjanovic et al., 2017). These different motives, framings and designs for evaluating can remain implicit, causing problems when deciding who should do the evaluating and how it should be done.

The allure of tightly framed evaluations providing clear evidence of returns on investment may inadvertently contribute to a narrowing of the research itself because activities that are not included in the evaluation might not get done. Broader goals such as health system strengthening and research capacity building may have been displaced by the narrow goals that are measured more easily by evaluations taken on by fewer stakeholders. If these are not valued in evaluations, other more targeted investments may prevail, and the importance of health system strengthening and research capacity building may remain overlooked. The problem is pressing because the health of a population may not necessarily be improved by disease-specific success without complementary investments in systems and capacity.

Secondly, there is a lack of evidence that targeted research funding leads to better outcomes overall, and a lack of conceptual apparatus to facilitate the type of analysis that might support the necessary complementary investments. Rates of return on health research show high payback, but the evaluations are often framed around individual diseases (Guthrie et al., 2018). They do not establish a more comprehensive picture of the relationship between health research and returns to society. There remains considerable conceptual difficulty about how to measure these broader relationships. Importantly, studies of this kind have not included LMICs, and an absence of LMIC data makes that task difficult.

One study that examined the broad relationship between investment in PRND R&D, and health outcomes in LMICs, concluded that it was not possible to draw strong results about the benefits and issues on the basis of existing data. The data that would enable this kind of analysis would need to be very different and much more comprehensive than that which is currently collected (Cochrane et al., 2017).

An evaluation of the Swedish International Development Cooperation Agency component of the European Developing Country Clinical Trails Programme highlighted an additional problem, which stems from conceptual confusion about the intended outcomes of targeted funding mechanisms. It highlighted a lack of clarity on the extent to which the programme defined its mission in terms of R&D outputs and capacity building on the one hand, and broader health and societal outcomes on the other hand (Hanlin et al., 2020). A clearer distinction would have a profound impact on the type of evaluation that is required and raises questions about who should be responsible for collecting data and reporting. This kind of confusion arises from a widespread but often unevidenced *assumption* that health R&D will lead to positive health outcomes in LMICs (Cochrane et al., 2017).

In recent years, several research evaluation efforts have attempted to address this gap with broader evaluation frameworks. A Wellcome Trust report (2012) argued for integrating monitoring and evaluation into project plans from the start; involving stakeholders when deciding what to monitor and evaluate (including associated indicators and methods); and ensuring that monitoring and evaluation are properly resourced, practical, usable and proportionate.

RAND Europe developed a PRND health research and innovation system framework to evaluate the impact of the EU's R&D funding for PRNDs in terms of how public research has been translated into

new products and services. The framework highlights the "different components of a health research and innovation system that need to 'work together' to achieve desired impacts" (Cochrane et al., 2017). These include health research and innovation pathways; research and innovation system drivers; health system drivers; global health policy levers; and wider environmental drivers. This framework conceptualises the problem of addressing neglected diseases as inherently multifaceted, and public funding of targeted research as *one* response to *one* of the issues that have been identified as crucial parts of a wider health and innovation system.

The International Development Research Centre in Canada developed an evaluation framework called Research Quality Plus, which is "a systems-informed approach to defining and evaluating the quality of research, and its positioning for use and impact" (IDRC, 2016). The framework emphasises multiple dimensions of quality assessment criteria that allow for varying contexts. This approach can show that research in the global south can be both rigorous and well-positioned for use; and that capacity strengthening and excellence can go hand in hand (Lebel & McLean, 2018).

Another response to these problems is to 'broaden out' the inputs to research evaluation and 'open up' the outputs (Ràfols & Stirling, 2021). The development of more plural and conditional indicators would make "the perspectives or assumptions through which they are framed" more visible. Ràfols and Stirling note how the *methods used* in research evaluation and other types social appraisal[v] influence the outputs, and so argue for the use of methods such as multicriteria mapping (MCM), which is a hybrid quantitative-qualitative method. MCM involves the appraisal of multiple options according to multiple perspectives and issues, and allows for consideration of uncertainties. The resultant mappings can be used to explore any salient grouping of the inputs, alongside qualitative reasons as to *why* some options perform better or worse under some conditions. There are many other methods which can help to broaden out and open up social appraisal.

5   Conclusion

In this chapter we examined an extreme case of efforts to target research towards addressing societal needs. We noted that R&D spending oriented to these needs has increased, and explained how this was facilitated by the emergence of a target category, namely that of neglected diseases.

We also argued however that there may be fundamental limits to a research-targeting approach. Perpetually increasing the funding for R&D may not unlock changes in health outcomes because of unintended consequences resulting from targeting funding towards specific diseases (see Bozeman's chapter in this Handbook for a discussion of other reasons for the "research-health outcomes gap" in the US (Bozeman, chapter 2 in this *Handbook*)). Research targeting may suffer from a lack of coordination; it can undermine efforts to strengthen local health systems; it leaves efforts to build research capacity unattended; it may result in unanticipated reactions by researchers in response to their funding environment; and it may favour some types of research over other approaches.

A better understanding of the relationship between scientific research and global health outcomes may help to avoid some of these unintended consequences and offer alternatives to research-targeting. This includes reconsidering the way funding mechanisms are designed predominantly around principles of research excellence, which means that increasing funding levels towards a specific target does not necessarily alter the balance and composition of research portfolios. Addressing problems in low- and middle-income countries may require not just more biomedical R&D, but also other kinds of research. Portfolio composition, as well as overall portfolio size,

remains an important challenge. These problems are compounded by a tragedy of the evaluation commons, where more complex and challenging evaluation tasks are overlooked or underfunded.

We have also argued that some of these problems could be addressed by building up local research capabilities, not only to absorb the knowledge being produced in high income countries, but also to orient research in ways that better contribute to solving low- and middle-income country problems. Cooperative forms of research with a wide range of stakeholders could also help to both orient and evaluate research. Broad methods of social appraisal and consultation (such as multi-criteria mapping) could offer sense of when research is likely to be seen as a complement, or as a dominant input, for addressing a given target. This serves not only to set a direction to research but also lends greater legitimacy for pursuing research as a way of addressing social challenges.

Taken together, we see that the way a societal problem is framed and targeted by research interacts with research evaluation. Although we have focused on examining these issues in relation to the case of neglected diseases, research targeting to address societal problems is on the rise more generally. Thus, many of the issues highlighted here are relevant to the prevailing enthusiasm for targeting research towards society's "Grand Challenges", in domains such as climate change, Sustainable Development Goals and pandemics, and how the burden of such challenges are distributed across high income countries and other countries. We hope there is an appetite for developing a new research and policy agenda along these lines, for tackling the challenge of neglected diseases in particular, but also for addressing other urgent societal problems.

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[i] The WHO launched its Special Programmes "to develop or apply new technologies and strategies for the pressing health needs of people in developing countries" and the Program for Appropriate Technology in Health (PATH) in Seattle created product research and development programs to begin to address health needs in developing countries (Mahoney & Morel, 2006).

[ii] Though there are some exceptions such as Drugs for Neglected Diseases initiative, whose remit spans a range of diseases, albeit within the same class of diseases (neglected diseases).

[iii] There was a growing recognition that better data infrastructure could support research funders who might wanted to invest in neglected diseases (Moran et al., 2009). Funded by the Gates Foundation and other global organisations, G-FINDER was founded, a publicly-available tool that tracks product-related R&D funding flows for neglected diseases (Moran et al., 2009). Despite these improvements, there remains a lack of data from LMICs (Cochrane et al., 2017). The WHO has recently established a global observatory on health R&D.

[iv] This is not simply due to a low percentage of grants devoted to neglected diseases research (only 16% of grants were for infectious and parasitic diseases and only 1.1% for NTDs). Even *within* neglected diseases research, the vast majority was allocated to researchers in HICs.

[v] Appraisal can be defined as "the ensemble of processes through which knowledges are gathered and produced in order to inform decision-making and wider institutional commitments" (Leach et al., 2010).