Health Research in Tanzania:
How Should Public Money be Spent?

A COHRED issues paper

David Harrison

COHRED Consultant
South Africa

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For copies of this publication and/or further information, please contact:

The Council on Health Research for Development (COHRED)

Mailing Address:
c/o UNDP, Palais des Nations, 1211 Geneva 10,
Switzerland

Physical Address:
International Environment House
13-11, Chemin des Anémones
CH-1219, Châtelaine, GE
Switzerland

Ph: +41 22 917 8558
Fax: +41 22 917 8015
Email: cohred@cohred.ch

This document is also available as a PDF at:
http://www.cohred.ch

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Table of Contents

Acknowledgements 1

Executive Summary 2

Health Research In Tanzania: How Should Public Money Be Spent? 5

Introduction 5

PART I: Developing a Public Investment Portfolio for Health Research 7

1. Where Should Investments be Made? 7
   - 1.1 Priorities for diseases and injuries 8
   - 1.2 Priorities for improving health service delivery 11
   - 1.3 Priorities that address socio-cultural determinants of health 12
2. What Types of Investment Instruments Should be Used? 14
   - 2.1 R&D in response to priorities of disease and injury 14
   - 2.2 R&D in response to service problems 19
   - 2.3 R&D that addresses socio-cultural determinants of health 22
3. How Much Public Money Should be put into Each Investment Instrument? 24
   - 3.1 Estimate the direct costs of each project 24
   - 3.2 Estimate the "cut-off point" for an investment portfolio that stays within budget 25
   - 3.3 "Topslice" for indirect costs of research 25
   - 3.4 Allocate investments to priorities 26
   - 3.5 Establish revenue centers for direct expenses 27
   - 3.6 Monitor the allocation of investments 28

PART II: How Tanzania Can Implement its National Investment Portfolio Efficiently 30

4. Conceptual Framework for Greater Efficiency 31
   - 4.1 Enhance research outputs 31
   - 4.2 Decrease costs 33

5. Three Striking Opportunities For Greater Efficiency 35
5.1 Clear gaps in the current national investment portfolio could be filled 35
5.2 A countrywide initiative could stimulate local demand for research 40
5.3 Poor communication can be improved fairly readily 41
6. Practical Ways to Seize the Opportunities 43
6.1 Design team-based incentives to fill investment gaps 43
6.2 Establish a national initiative for district-based problem solving 46
6.3 Work to improve communication 47

Conclusion 50

Appendix A
Methodology for setting priorities for health research in Tanzania 51

1. Method Used in 1999 51
2. Recommendations for Improving Inter-Rater Reliability in Priority Setting 51
2.1 Rank-order health service & socio-cultural problems at district, but not national level 51
2.2 Introduce an intermediate step for ranking research topics 52
2.3 Distinguish between selection criteria that reflect potential returns and risks 52

Appendix B
A national health research portfolio for Tanzania 53

I. Research in Response to Priorities of Disease and Injury 53
II. National Research in Response to Service Delivery Problems 58
III. National Research in Response to Socio-Cultural Determinants of Health 59
IV. Local Research to Address Service Delivery Problems and Socio-Cultural Priorities 60

References 61

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Executive Summary

Tanzania cannot afford to waste its scarce resources and must ensure that public funds spent on health research lead to better health for its people. In 1999, the National Forum on Health Research conducted a process of priority setting for health research, which established a ranked list of topics regarded as most important for Tanzania. The challenge now is to translate that list into a research agenda expected to realise greatest social benefit. Essentially, this involves two iterative steps. The first is to define a public investment portfolio of R&D expected to maximise improvements in health. The second is to ensure efficient implementation of the portfolio, so that expected benefits actually materialise. The purpose of this report is to describe how each step can be carried out.

Analysis of the results of priority setting showed strong consensus on the main diseases causing morbidity and mortality in Tanzania. Malaria is the clear priority, and infectious diseases still dominate the health profile. The priority setting process clearly defined the *scope* of a public investment portfolio with respect to health problems to be addressed, although the validity of precise rankings is arguable:- results from household-based surveillance suggest that upper respiratory tract infection is over-represented as a determinant of total burden of disease, and HIV/AIDS and associated tuberculosis under-represented. Provided the list of priorities is not regarded as immutable, it is still a good standard against which to assess current investment patterns.

Participants did not reach the same level of agreement on service delivery priorities and socio-cultural determinants of health. This probably reflects wider variation of conditions across the country, and suggests that a local, context-specific approach may be more effective in setting a corresponding research agenda.

Aligning R&D with national priorities sets research efforts on the right track towards maximal social benefit. However, expected returns will only be realised if investments are made in forms of R&D most appropriate for Tanzania. The strategic emphasis identified through priority setting is to improve the use of existing health interventions and allocate resources more equitably. New product development and finding cost-effective applications for interventions known to be efficacious are important, but secondary objectives.

While the final investment portfolio will be shaped by practical considerations, it should bear a close resemblance – both in scope and strategic direction - to the research agenda agreed to by the national meeting. In the short- to medium-term, the national portfolio will continue to be shaped largely by donor funding. However, it can serve as a clear expression of Tanzanian intent and could significantly mould the health sector reform program if it were accepted as its research arm.

Having defined the R&D portfolio expected to maximise social benefit, the National Forum on Health Research should ensure its efficient implementation. Tanzania can boost returns on its current investments both by enhancing outputs and reducing costs. Enhanced outputs can be achieved by stimulating demand for research and increasing its supply. To date, strategies to stimulate a demand for research have not received as much attention as efforts to increase supply, yet hold the key to substantial efficiency gains. Supply-side strategies have tended to focus on building up new resources, but more could be done to allocate existing resources better.

Cost reduction could lead to substantial efficiency gains, and the major transaction costs associated with research in Tanzania are incurred in communicating information.

Striking opportunities exist to realise greater returns from current levels of investment in health research.

First, there are **clear gaps** in the present national investment portfolio, both in terms of the scope of funding and the type of R&D instruments employed in addressing priorities. Filling these gaps will improve efficiency of allocation of research funds. In terms of diseases, more should be invested in addressing the priorities of acute respiratory infection, diarrhoeal disease and anaemia. With respect to R&D instruments, there is an obvious need for more operational research aimed at improving technical efficiency and achieving greater equity in resource allocation.

Analysis of R&D trends over time suggests that prevailing incentives will not fill the gaps identified, and the National Forum on Health Research will need to provide added motivation for researchers. Team-based incentives should be designed to encourage researchers to respond to deficiencies in the current R&D portfolio. Given resource constraints, incentives will need to be a mix of financial benefit and psychic reward, but will only provide the right motivation if the individual's share of collective benefits exceeds the personal gains of working alone. This may be achieved by preserving the typical rewards of science such as peer-recognition, supplemented with other incentives such as better access to new information through collaboration.

Second, despite pockets of R&D efforts, there is **no sustained national program** to improve equity of resource allocation and efficient use of existing tools at **local level**. A program of district-based problem-solving, sharing knowledge and learning from each other would not only fill in some of gaps in the spatial distribution of research, but may also increase returns to R&D by stimulating demand across the country. Research should be locally initiated as part of each district's development plans, and should form part of a multi-pronged process of support to improve service delivery. Lessons learnt should be actively shared across the country, and proactive national leadership is required to make this happen.

Third, **communication is constrained** by tangible deficiencies in infrastructure, as well as by invisible barriers between research organisations. Dismantling these barriers could boost R&D outputs and reduce transaction costs. A practical place to start is for the National Forum on Health Research to agree on a few common outputs, including a series of learning briefs distributed regularly to every district in the country and an annual review of progress in addressing disease priorities. A deliberate process of technical and user support is required to make the most of electronic networks and information resources, but this does not replace the value of face-to-face interaction. Taken together, these communication strategies can help promote a stronger "culture of learning" in the Tanzanian health sector.

The cost of access to global knowledge is high, and the National Forum should work steadily to keep costs down. This includes negotiating favourable terms for software licences and attempting to moderate the isolating effects of the international agreement on Trade-related Aspects of Intellectual Property Rights.

Tanzania faces daunting pressures to overcome poverty and improve health. In this context, health research can only be justified if it leads to better health. For the majority of people, the

research that could make the biggest difference is **practical problem-solving** – helping districts to get more out of their budget allocations by improving efficiency and targeting resources to those most in need. Contributing to new product development and finding cost-effective applications for efficacious interventions are important objectives, sustainable through prevailing incentives. Designing additional incentives to fill obvious gaps will not jeopardise existing research disciplines, and should in time stimulate the overall demand for research.

At present though, the strategic emphasis of health research in Tanzania is inefficient in improving health. Significant opportunities exist for the newly established National Forum on Health Research to implement an investment portfolio for R&D expected to maximise social benefit.

Health Research In Tanzania: How Should Public Money Be Spent?

Introduction

Even more than rich economies, Tanzania cannot afford to waste its scarce resources. The country wants to get greatest social benefit out of its public investment in health research, and has defined 'social benefit' as better health for those who need it most (NIMR 1999a).

Yet it is hard-pressed to demonstrate good returns on its current expenditures.

There is now strong national consensus that resources should be allocated to address relevant problems in a way that leads to significant improvements in health (NIMR 1998). In this regard, Tanzania is not alone. Wealthy countries such as Japan and the United States allocate federal funds for R&D according to national priority, and Canada is trying to implement a national health research portfolio that maximises expected benefits (OECD 1991, Rosenberg 1994, CIHR 1999). The obvious differences between Tanzania and rich countries is that the latter have more resources and lower social discount rates for future benefits, allowing them to accommodate greater uncertainty in their R&D portfolios.[1] This means that proportionally more funds can be allocated to exploratory research and relatively less in trying to solve pressing problems. Yet even in these countries, there is growing demand for greater accountability for public funds invested in R&D and for more efficient use of that money (Gibbons *et al* 1994, Dasgupta & David 1994). For Tanzania, public spending occurs in the context of immense poverty and unmet basic need – per capita GNP is about US $200 – and investment in R&D has to be justified by positive returns.

At the outset, it is important to note the substantial role played by development aid in Tanzania. External financing now accounts for about 30% of the total development and recurrent government budget, up from 5% at the time of independence in 1961 (Mwisongo *et al* 2000). Several bilateral donors are committed to a sector-wide approach (SWAP) to improving health, and a first phase of "basket grants" of (US) 50¢ per capita is now being implemented in 37 of Tanzania's 126 districts. While a significant part of the aid *is* in the form of non-repayable grants, loan obligations have mounted as well. External debt rose from 50% of GDP in 1981 to over 150% in 1988. These economic realities have a major impact on health research in Tanzania. Donor funding bolsters national research efforts by supporting a number of relevant research programs. However, it also complicates efforts to ensure that – overall - public resources are directed towards greatest social benefit. For example, a researcher earning a government salary may compete for funds matching donor requirements, not necessarily in line with country priorities. More precisely, donor and national interests usually coincide in broad terms, but often diverge in the details. Sometimes, this divergence reflects a deliberate standpoint of the foreign financier. For instance, analysis of the health costs of user fees may be an important part of health sector reform, but flies in the face of blanket advice by the World Bank. Often though, it is not a deliberate effort by donors to push a different agenda, yet North-South research collaboration still tends to reflect the longer-term

[1] Rich countries stand to gain less from R&D trying to achieve better health for today's generation because their present health status is already good. On the other hand, low-income countries would benefit greatly from significant improvements in health status today – and less value can be placed on deferring benefits to future generations. In economic terms, this means that low-income countries have higher social discount rates for future benefits (see section 1.1 for further discussion).

perspective of rich countries than the immediate needs of poorer ones. Some donors may even welcome a clearly defined research program regarded by Tanzania as most valuable.

For the purpose of defining a portfolio for health research, it is useful to think of donor funding as part of public investment. The justification for this thinking is that donor funding almost always results in a substantial commitment of public resources (through direct cost-sharing or utilisation of personnel trained at Tanzanian taxpayers' expense). Furthermore, donor funding to the health sector is intended to further the development objectives of the Tanzanian government and should therefore be in line with national priorities.

Believing that a concerted approach can achieve better health outcomes, a variety of interest groups established the National Forum for Health Research in 1998. The Forum is a voluntary alliance of stakeholders in health research, and includes the health ministry, the National Institute for Medical Research, medical training and research centres, other research institutions, private institutions and NGOs, donors and community representatives. Among other things, it is responsible for establishing and reviewing research priorities, and advising policy and decision-makers on the allocation of funds (NIMR 1998). In early 1999, the Forum supervised a process of setting national priorities for health research, which resulted in a ranked list of ten priority:

- Diseases and injuries causing the greatest morbidity and mortality
- Health service delivery problems contributing to persistent disease
- Socio-cultural (behavioral) factors leading to illness.

For each, specific research topics were identified and ranked to provide the basis for an agenda for health research in Tanzania. Appendix A describes the process.

The challenge now is to translate that broad research agenda into a plan of action. Maximising the value of health research requires that resources be allocated to projects that yield the greatest expected benefit - defined as: [the returns to each project under ideal conditions] x [the probability that each study will be successfully implemented]. Thus, even if resources are allocated to those projects ranked highest were implementation perfect, benefits can be expected to materialise only if the research is *implemented* efficiently.

In essence then, maximising the value of health research involves two iterative steps. The first is to define an investment portfolio of research expected to produce greatest benefit within budget constraints. The second is to ensure that the research is implemented most efficiently. Drawing on the priority setting process to date, this report proposes ways of carrying out both steps.

PART I: Developing a Public Investment Portfolio for Health Research

Conceptualising the management of public resources for health research as a diversified investment portfolio helps people think through the potential benefits and risks of every project. It encourages managers to work constantly towards better returns by comparing the expected benefits of different investment options (Eyzaguirre 1996). It enables them to deal with the inevitable uncertainty of research outcomes by selecting a "risk-profile" appropriate for Tanzania. In developing its investment portfolio, Tanzanians will need to answer three questions, namely:

- Where should investments be made?
- What type of investment 'instruments' (R&D) should be used?
- How much public money should be put into each R&D instrument?

These questions serve as the framework for this section.

1. Where Should Investments be Made?

The priority setting process conducted in 1999 sought to answer this question by defining the scope of public investments in health research [Table 1].

Table1: National health research priorities for Tanzania, as defined by participants in the priority setting meeting (1999)*

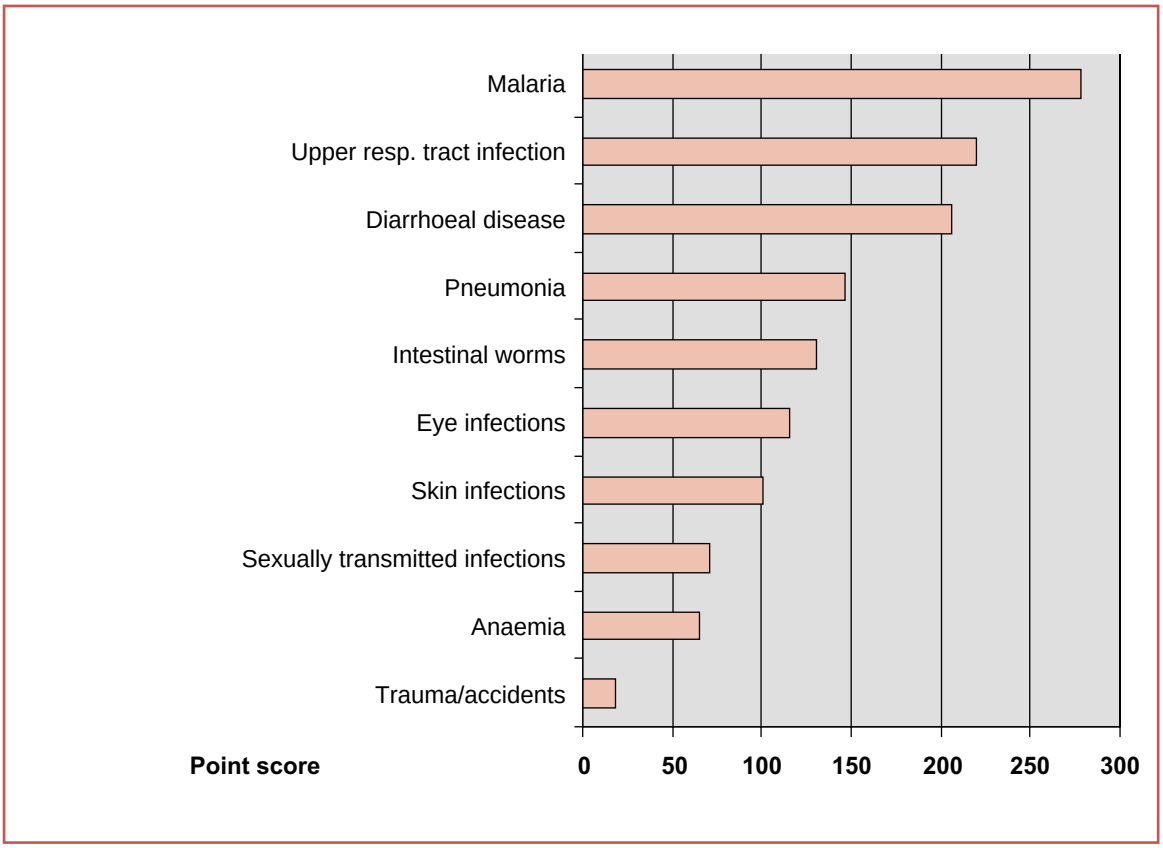
Diseases and injury	Delivery problems	Socio-cultural determinants
1. Malaria	1. Poorly trained personnel	1. Food taboos in pregnancy
2. Upper respiratory tract infection	2. Lack of equipment & drugs	2. Poor latrine usage
3. Diarrhoeal diseases	3. Lack of transport for supervision & distribution	3. Poverty linked to individual behaviour
4. Pneumonia	4. Allocation of funds for preventive services	4. Polygamy
5. Intestinal worms	5. Low impact of health education	5. Ignorance and high illiteracy
6. Eye infections	6. Impassable roads	6. Gender inequality
7. Skin infections	7. Poor building maintenance	7. Witchcraft
8. Sexually transmitted infections	8. Inadequate water supply	8. Inheritance of widows
9. Anaemia	9. Poor environmental sanitation	9. Low acceptance of family planning methods and high fertility
10. Trauma / accidents	10. Too few health facilities	10. Use of local herbs
11. Bilharzia		
12. TB/HIV		

* These priorities and rankings are from the Tanzania Essential National Health Research Priority Setting Workshop, Arusha, 15-21 February 1999

1.1 Priorities for diseases and injuries

Rankings were based on methods outlined in Appendix A. Individuals in three working groups allocated point-scores to each priority which enabled participants to both rank-order the problems and assign relative weights to the importance of each problem [Figure 1].

Figure 1: Infectious diseases still dominate the epidemiological profile



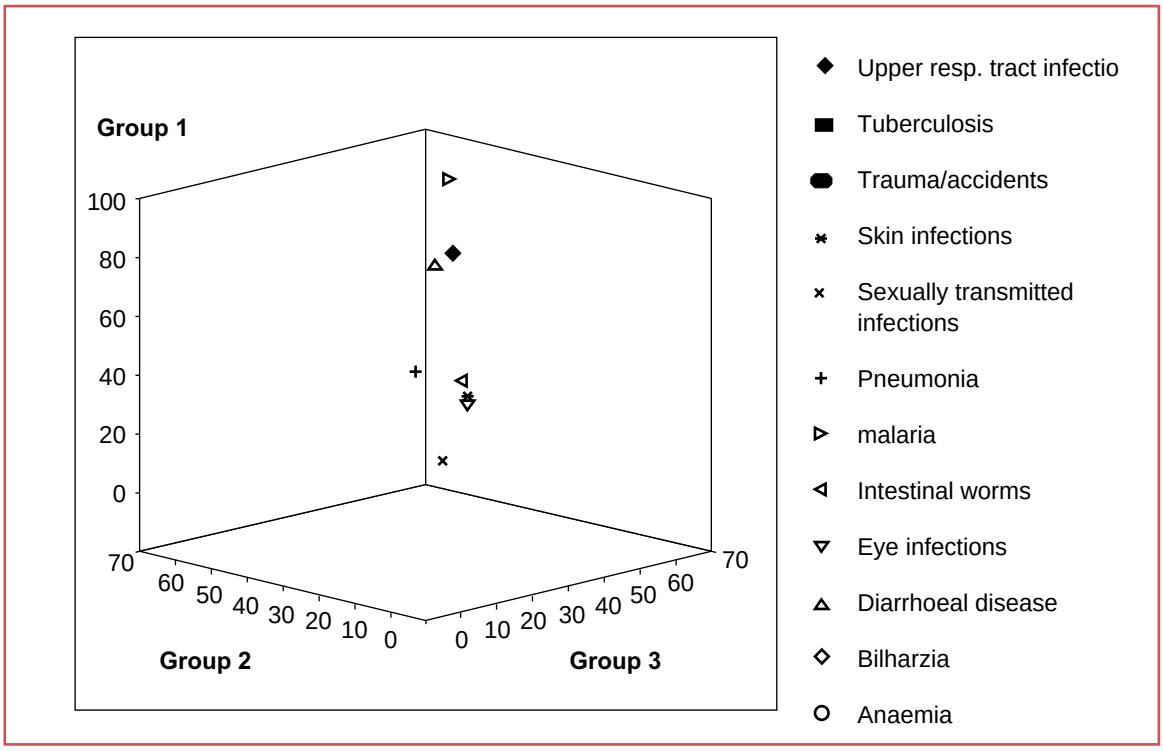
Despite different rating methods adopted by the three working groups, almost everyone agreed on the ten greatest health problems in Tanzania [Table 2]. One group's ranking coincided totally with the aggregate rankings. Another group was in agreement with nine out of ten final rankings (it rated trauma and accidents higher than bilharzia), while the last group agreed with eight out of ten (rating trauma/accidents and tuberculosis higher than bilharzia and anaemia) [Figure 2].

Table 2: Disease rankings among the 3 groups were highly correlated

Correlation (r)*	Group 1	Group 2	Group 3
Group1	1.00	0.88	0.95
Group 2		1.00	0.95
Group 3			1.00

* All significant $p < 0.01$

Figure 2: Malaria is the clear priority[2]



All the priorities pertain to pressing problems. None relates to problems that may occur in the future as a result of risk-taking behaviour today. A preoccupation with current problems is to be expected from the data sources used for priority setting:- district health officers are principally concerned with the issues they face day-to-day, while the national health information system is retrospective. But this does not necessarily mean that the method used for setting priorities is wrong. In low-income countries like Tanzania, rates of return are generally high at the margin. For resources to be allocated efficiently, the public sector needs to use a high discount rate in estimating the present value of long-term projects. The implication is that projects with large short-term benefits are preferred to projects with equally high long-term benefits. Arguably though, it may be far more cost-effective to prevent problems related to risk-taking behaviour now than to treat the end-results later. For this reason alone, it would be useful to consider health problems just beginning to loom on the horizon. Formal risk assessment is probably not worth the effort, but simply flagging likely problems of the future allows them to be an explicit factor in determining national priorities. For Tanzania, these include the effects of:

- Tobacco smoking
- Ecological damage
- Occupational hazards

However, a greater threat to the validity of rankings is systematic bias in the health management information system (HMIS), used to depict current mortality and morbidity. The HMIS reflects attendance at health facilities rather than the contribution of specific illnesses to the total burden of disease. This bias may account for upper respiratory tract disease being ranked

2 Each group developed its own rating scale, so the scales of axes differ for Figures 2-4

second, despite the insignificance of uncomplicated coughs and colds as determinants of the total burden of disease. A second source of systematic bias related to the HMIS is that people use alternative forms of health care for problems like sexually transmitted diseases and AIDS, resulting in under-reporting of the prevalence of selective diseases. Evidence from district surveillance systems (DSS) in Tanzania suggests that the bias arising from facilities-based data is real and significant. DSS data are collected from *households* in seven districts every four months, providing a population-based source of health information. And there are several important discrepancies between DSS data and the nationally prioritised rankings. For instance, TB and associated HIV infection, ranked 11th by workshop participants, is now the dominant cause of disease in Dar es Salaam – surpassing even malaria in importance (Ministry of Health 1997). Similarly, anaemia is a major cause of childhood mortality, and ranks higher than 9th in the surveillance districts.

Despite the high level of consensus at the national priority-setting meeting, the disease rankings are not immutable and should be interpreted in the light of potential biases. Having said that, no priority setting process can be perfect and the national meeting clearly defined the *scope* of national priorities (1 – 12), if not the precise rank order. I suggest that the following qualifications to the final rankings of disease priorities be considered.

- First, there can be little objection to combining upper respiratory tract infection (URTI) and pneumonia as a single disease category, namely "acute respiratory infection (ARI)", since pneumonia is the principal complication of URTI. Whether ARI is ranked 2nd, 3rd or 4th is immaterial - the point is that ARI is a leading priority.

- Second, HIV & AIDS are currently hidden within the disease category "sexually transmitted infections (STI)" and appear again in 11th spot in association with TB. There is rapidly accumulating evidence that AIDS is a major cause of mortality and morbidity in Tanzania and should made more explicit in the priority listing. Rewording the priority "STI" as "sexually transmitted infections, including HIV/AIDS" would make an unambiguous statement that AIDS is a substantial problem in Tanzania.

- Overall, malaria is still the undisputed priority, and research investments should reflect this fact. However, the relative importance of ranked priorities may vary from district to district, and the final investment portfolio should be sensitive enough to accommodate local variation.

These qualifications do not represent a drastic departure from the outcome of the meeting, but may prevent funds being diverted to problems that actually contribute little to the burden of disease.

The validity of *future* priority setting processes may be enhanced by two relatively simple strategies. Although data from the Adult Morbidity and Mortality Study were available to participants, incorporating information from district surveillance systems more systematically could supplement the facilities-based HMIS with a population-based perspective. A second strategy is to resubmit tentative priorities synthesised at the national meeting to district management teams for review and challenge.

1.2 Priorities for improving health service delivery

In contrast to the strong agreement on disease priorities, consensus on the top ten service delivery problems was weaker [Table 3].

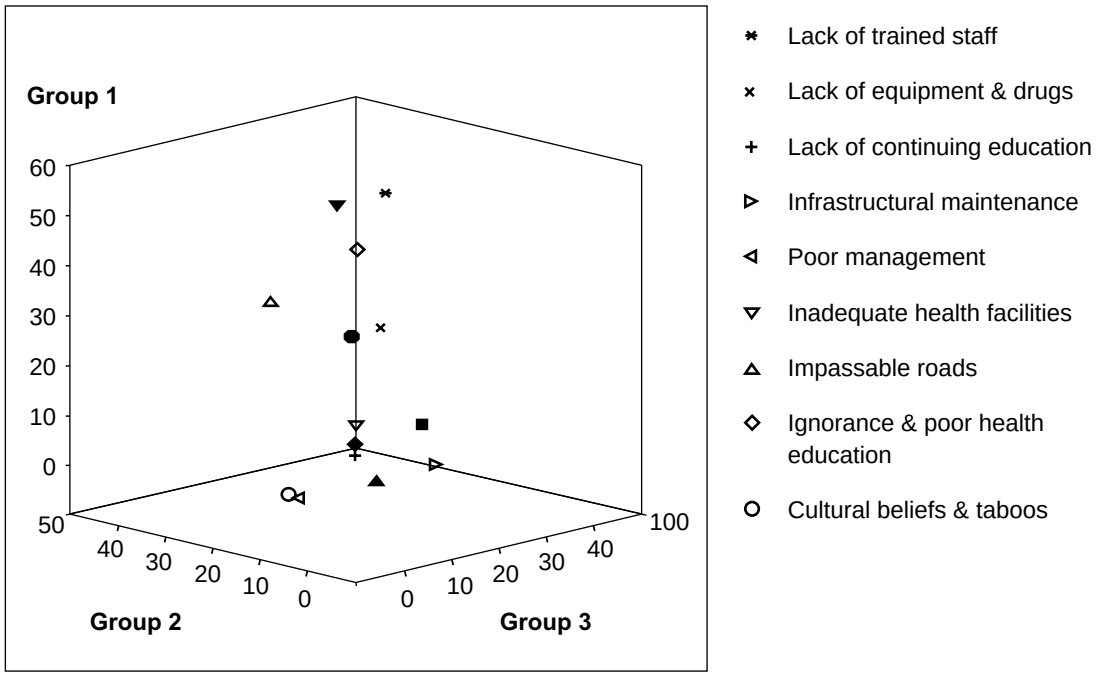
Table 3: Rankings of delivery problems were less strongly correlated

Correlation (r)	Group 1	Group 2	Group 3
Group1	1.00	0.41	0.30
Group 2		1.00	0.78*
Group 3			1.00

* Significant p< 0.01

This wider dispersion of priorities, across the *same* groups that achieved virtual unanimity on priority diseases, may reflect the local and site-specific nature of many service delivery problems [Figure 3]. It may also be due to the fact that, unlike the health management information system for disease priorities, there is no common data collection instrument for service delivery priorities. Thus there is less chance of participants reaching harmonious conclusions simply because they are "reading from the same songbook".

Figure 3: Some service delivery priorities are common, others differ between groups



The implication of this variability in response is that, while there may be general agreement on the types of service delivery problems to be addressed, a more decentralised process is required to generate a research agenda specific enough for each region and district.

1.3 Priorities that address socio-cultural determinants of health

The correlation between group rankings for socio-cultural determinants of ill health was effectively zero [Table 4]. Although it appears that there is fair agreement between Groups 1 and 3, this may just be a chance finding (p = 0.25).

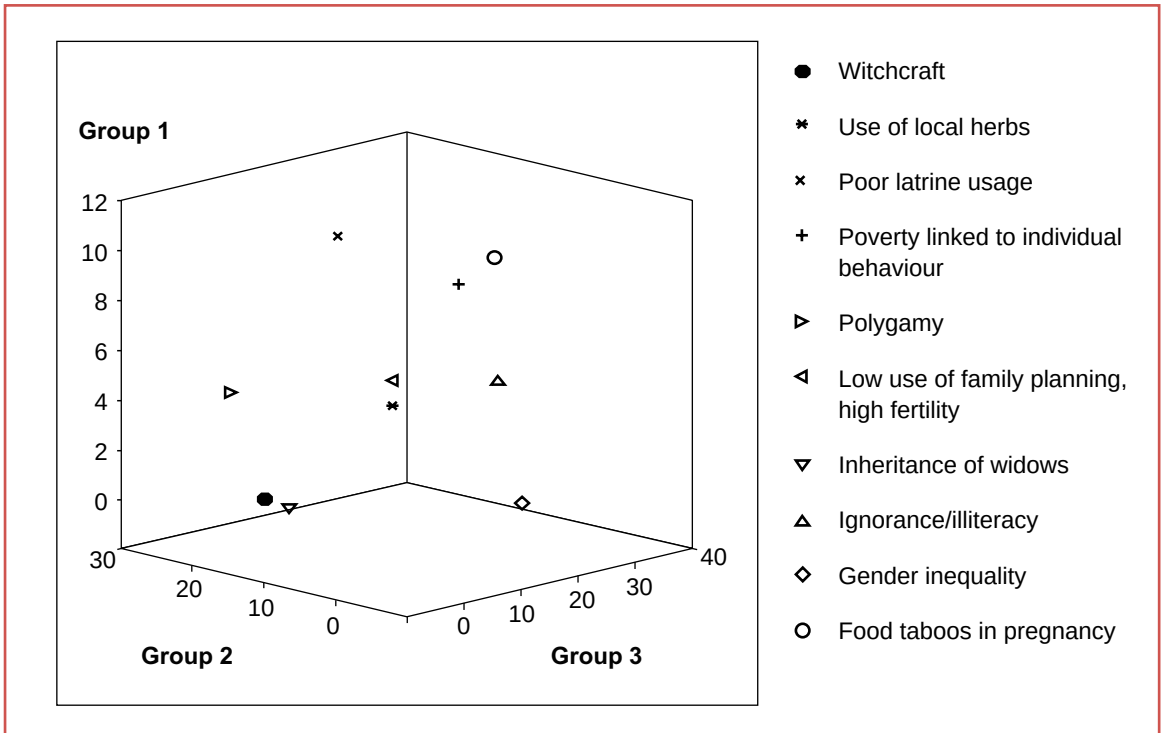
Table 4: Rankings for socio-cultural problems were uncorrelated across groups

Correlation (r)	Group 1	Group 2	Group 3
Group1	1.00	0.06	0.40
Group 2		1.00	- 0.09
Group 3			1.00

* None significant at p = 0.05

This means that the relative importance assigned to each socio-cultural priority by each group cannot - in any meaningful way - be aggregated into a composite rank-order [Figure 4]. For this reason, the order assigned in plenary to socio-cultural factors [Table 1] should be regarded as arbitrary.

Figure 4: Socio-cultural priorities varied widely across groups



This dispersion of priorities suggests that, as with health service delivery problems, a more decentralised process is needed to determine a specific research agenda.

In summary

The process of priority setting achieved a high level of agreement on the main diseases causing morbidity and mortality in Tanzania. It clearly defined the *scope* of a public investment portfolio in terms of the health problems to be addressed, although the validity of precise rankings is arguable. In any event, the final investment portfolio should be sensitive enough to accommodate spatial variation in disease priorities.

The participants did not reach the same level of consensus on health service and socio-cultural priorities. This probably reflects wider variation of conditions across the country, and suggests that a local, context-specific approach may be more effective in setting a corresponding research agenda.

2. What Types of Investment Instruments Should be Used?

2.1 R&D in response to priorities of disease and injury

Even if disease priorities for research are valid and investments are allocated accordingly, it is *still* possible that research with low expected social benefit receives the lion's share of funding. For instance, an esoteric piece of work on some biochemical change resulting from malaria may be justified on the grounds that it addresses a national priority, but it would surely fail a benefit-cost test. Yet even highly relevant research does not automatically pass muster. For example, the present value of future health benefits from long-term commitments to new product development should be heavily discounted. In a country carrying a huge burden of preventable disease, failing to apply high discount rates to expected future benefits causes inefficient resource allocation. But on the other hand, global investment in R&D to address diseases of the poor is pitifully low – about 4.5% of total public spending on health research (WHO 1996). Without a stream of new interventions in the pipeline - and were *all* efforts directed at using existing tools more efficiently - the future burden of disease may be higher than expected. This is particularly true for malaria where drug sensitivities are constantly changing, and Tanzania's good biomedical research infrastructure *can* contribute to aspects of vaccine development and drug efficacy.

So the next step in developing an investment portfolio that maximises expected social benefit is to determine the profile of research expected to be most beneficial for Tanzania. The WHO Ad Hoc Committee on Health Research (1996) argued that disease persists for one or more of three reasons:

- Knowledge of disease process and causes is inadequate; or
- Existing 'tools' or interventions are inadequate; or
- Existing tools are not used efficiently.

The Committee suggested research & development (R&D) instruments that would best respond to these inadequacies by:

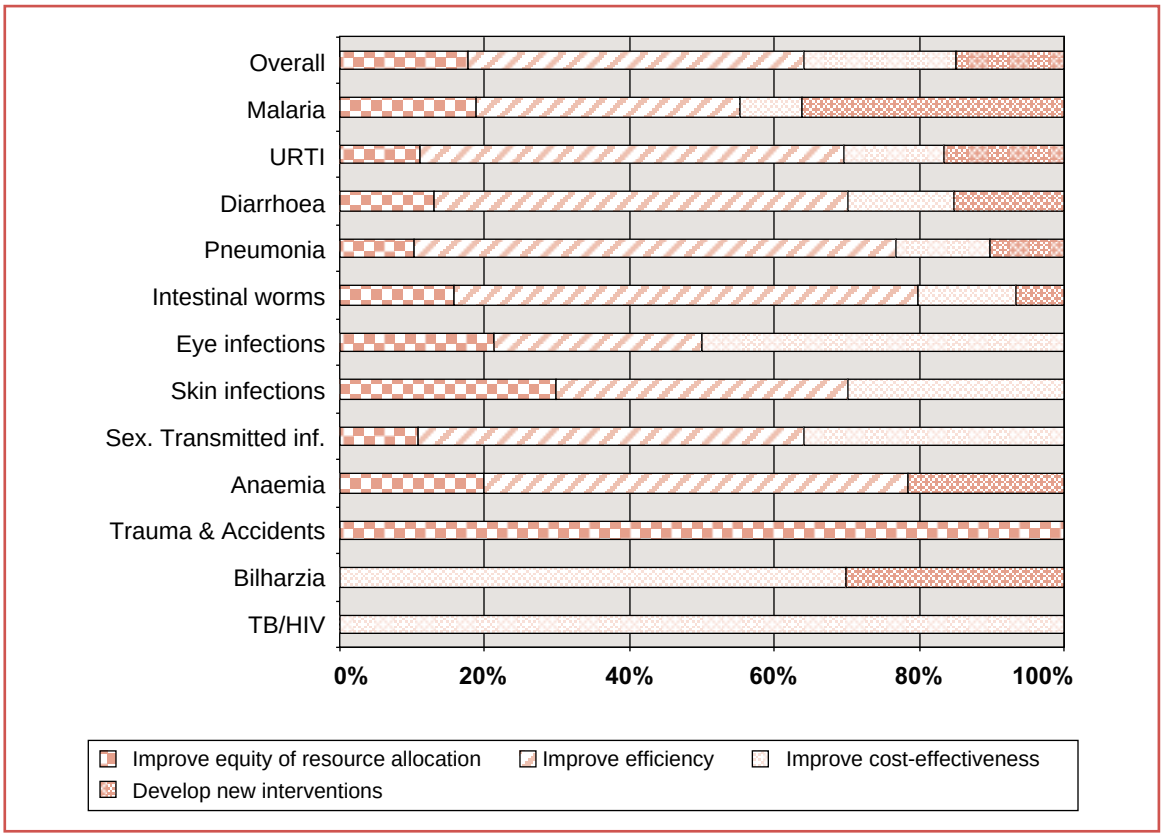
- Developing *new health products* or interventions (discovery and invention)
- Adapting interventions known to be efficacious but still too expensive, so that they become *cost-effective* (innovation)
- Achieving greater *efficiency* in the use of existing interventions (implementation R&D).

In the Committee's view, both concepts of technical and allocative efficiency are implicit in the third objective. However, it may be helpful to make a clearer distinction between technical efficiency (putting inputs to best use, regardless of allocation) and targeting resources to areas of greatest need - defined by the Committee as 'allocative efficiency'. In conventional economic terms, allocative efficiency is achieved through market incentives based on people's willingness to pay, and using this term to refer to allocations targeted to the greatest burden of disease may lead to misunderstanding. But more importantly, that sector of the population most in need of resources is likely to be partly obscured by a health information system that tends to be most complete in wealthier areas, and by more strident and politically connected interest groups. Both factors make it very difficult for the true distribution of societal demand to be revealed, and contribute to substantial neglect of problems of the poor in both global and national health research agendas. Unless there is an explicit redistributive component to the

research portfolio, current trends will prevail. And so, to the three types of R&D instruments outlined above I add a fourth, namely to achieve greater *equity* in resource allocation. For low-income countries like Tanzania, strategies to achieve greater efficiency and equity in resource allocation will almost inevitably be one and the same. In the absence of an explicit agenda for equity however, resources will continue to be allocated inefficiently for the reasons above.

In further working group discussions, participants identified and ranked a research agenda for each national priority. To paint a broad stroke picture of Tanzania's preferred investment portfolio, I categorised each topic as one of the four R&D instruments described above. More accurately, this categorisation should be based on the specific questions underlying each research topic, as a single topic may raise questions of both efficiency and cost-effectiveness for example. This level of accuracy is however not possible without knowing the specific intent of workshop participants. Nevertheless, the main purpose of categorising topics is to establish the general direction in which Tanzania should be moving. And in this respect, the strategic emphasis is clear. Nearly half (46.5%) of all points allotted by participants were to improve efficiency of resource use. Almost a fifth (17.6%) were allotted to improve equity of allocation. Tanzania subscribes to the principles of *Essential National Health Research (ENHR)*, a strategy for promoting health and development on the basis of equity and social justice (COHRED 1990). And these emphases are consistent with ENHR [Figure 5]. Note that for malaria, there is greater emphasis on new product development (36%) and finding cost-effective interventions (9%) than the overall trend. But even for malaria, over half of the preferred R&D response is improving efficiency of use of extant tools and allocating resources more equitably.

Figure 5: The research portfolio places emphasis on greater equity and efficiency



However, responses across groups were poorly correlated, and at first glance it appears that groups held a number of diametrically opposing views [Table 5]. Much of the apparent discrepancy is probably due to the ranking method used, in which each group identified and ranked its own research agenda. What emerged were three discrete lists of research priorities with some overlap of items, but also considerable divergence of ideas [Table 6]. Final rankings were approved in plenary, effectively legitimising them - but perhaps obscuring the diversity of responses.

Table 5: Rankings for a specific *research agenda* seem to show divergent opinions

Health priority	Correlation between research priorities identified by the 3 groups		
	Groups 1 / 2	Groups 1 / 3	Groups 2 / 3
Malaria	0.054	- 0.582*	- 0.250
Upper respiratory tract infection	- 0.699	- 0.005	- 0.495
Diarrhoeal disease	0.316	- 0.648	- 0.476
Pneumonia	- 0.980***	0.675	- 0.696
Intestinal worms	- 0.781*	0.086	0.347
Eye infections	- 0.493	- 0.993***	0.591
Skin infections	0.168	- 0.222	- 0.233
Sexually transmitted infections	- 0.319	- 0.042	- 0.772*
Anaemia	-	-	- 0.985**
Trauma and accidents	-	-	-

* p<0.05 ** p< 0.01 *** p< 0.001

The following analysis may give a better sense of the strength of consensus and areas where opinions diverge.

A strategic distinction is between those research topics on which there is complete agreement, and those that reflect the strong concerns of some interest groups. Topics broadly supported can be used to initiate implementation of the investment portfolio, and those accepted unanimously can usefully serve as 'flagships' for the new approach. Research topics identified by all three groups are **bold underlined text**; those identified by two groups are coloured text; and those only recognised by one group are in plain text [Table 6]. With few exceptions, most notably the strong case made by one group for participating in pneumococcal vaccine development, final rankings coincided with composite scores. This suggests that the final plenary session served to express 'intensity of feeling' about research topics, and modified the portfolio accordingly. But it is also clear from the workshop report that some participants felt very strongly about topics such as maternal morbidity that did not make it onto the national agenda, but may well be regional or district priorities. In Part II, I set out proposals for accommodating sub-national priorities.

Table 6: Research topics in response to priority diseases & injuries

	Equity	Efficiency	Cost-effectiveness	New products & interventions
Malaria	• In pregnancy • In under fives	• Vector control • Case management • Intermittent treatment • Health seeking behaviour • Quality control • Association with anaemia	• Herbal treatment • Clinical vs lab indices • Choice of drugs • Highland malaria	• **Drug resistance** • Vaccine • Pharmacokinetics
URTI	• Environmental conditions	• Case management • Health seeking behaviour • Predisposing factors	• Antibiotic sensitivity	• Vaccine development
Diarrhoeal disease (DD)	• Water & sanitation	• Management of acute DD • Aetiology • Knowledge, attitudes & practices (KAP) of caretakers • Food handling & law • Impact of interventions	• Antibiotic sensitivity • Food hygiene inspection	• Vaccine development
Pneumonia	• Environment	• Case management • Risk factors • Home care	• Antibiotic sensitivity	• Vaccine development
Intestinal worms	• Epidemiology	• **Environmental control** • Program evaluation • Program replication • Human behavior • Nutritional • Child development		• Drug efficacy • Immunology
Eye infections	• Mapping of eye disease	• Case finding & management • Onchocerciasis	• Impact of vitamin A supplementation • Sustainability of interventions	
Skin infections	• **Epidemiology**	• Case management • Community KAP • Prevention	• Chemical & detergent survey • Environmental pollution	
Sexually transmitted infections	• Epidemiology	• Case management • Program evaluation • Prevention & control • Health seeking behavior	• Evaluation of syndromic mx • Impact of community strategies	• Drug resistance
Anaemia	• Epidemiology	• Program evaluation • Worms & malaria	• In pregnancy • W.r.t nutrition	
Trauma & accidents	• Epidemiology			

Workshop participants ranked research topics in terms of their expected benefit, using a mix of criteria that anticipated potential *returns* to the research project and the likely *risk* that the research would be unsuccessful. Criteria reflecting potential returns included:- contribution to

new knowledge; magnitude of the problem at hand; and the urgency of solving it. Risk-related criteria included:- feasibility; political acceptability; and expected applicability of results.

In retrospect, it may have been helpful to distinguish between selection criteria that try to gauge potential returns and those that gauge likely risk. Although the rankings assigned are indicative of *expected benefit,* it is now not possible to differentiate between research regarded as high-returns / high-risk, high-returns / low risk and medium-returns / low-risk. Should risk conditions change, there is no way of re-evaluating the expected benefit of a research topic omitted from the agenda, or re-ranking those included. By separating the two, it should be possible to evaluate potential returns under ideal conditions of implementation and then to account for risk by discounting expected benefits. This would allow for regular reappraisal of the research portfolio - without having to redo the entire exercise every time there was a substantive change in external conditions.

Owing to the stringency of risk-related criteria, the portfolio adopted by participants appears quite practicable for Tanzania. One concern is that these criteria may exclude research topics that are potentially very important, but for which there is no obvious or immediate "window of opportunity" – and my view is that the only criterion that should be used to discount potential returns is *existing capacity to do the research*. To make political acceptability an explicit criterion immediately limits the usefulness of research. Research is intended to challenge prevailing notions of what works and what doesn't. This is not to suggest that national research managers should be indifferent to the political environment, but the degree to which the research agenda is constrained by political considerations is the degree to which good science is compromised. My reservation about using the criterion of 'applicability of research results' is more pragmatic. There is no evidence to suggest that any one type of strategic research[3] is more applicable than another type (Weiss 1977). A common fallacy is that "downstream" or "more applied" health research has more certain outcomes and more chance of being implemented. Health policy and systems research probably contributes to improvements in health in much the same way that biomedical research does – by expanding the pool of knowledge, increasing the probability of change, and preparing for "windows of opportunity" (Weiss & Bucuvalas 1980). It's fair to assume that the uncertainty of results *not* being applied is roughly the same across the research portfolio described above, and a reasonable approach would be to hold this risk constant in allocating weights. In contrast, the risk that research will fail because of inadequate national capacity to produce the outputs should be accounted for, because it *does* vary across the portfolio. In summary, 'risk' should be narrowly defined as the probability that the research project will fail to meet its objectives because of inadequate national capacity to undertake the project.

The topics listed in Table 6 are broad areas of research that need further refinement and definition. In this regard, and *if* the National Forum on Health Research had strong control over the allocation of resources, it could exercise one of several options. One would be to proceed to defining the precise research questions related to each topic. Researchers would then be invited to bid for funds without any further say in problem definition. In this way, the Forum could guarantee that the portfolio of projects corresponded closely with national intent articulated through the priority setting process. An added advantage is that the costs associated

3 Strategic research excludes basic or fundamental research i.e. research that contributes to global knowledge, but has no obvious or immediate application. A problem-driven process of priority setting excludes basic research from a public agenda. Note that basic research is sometimes confused with biomedical research, which is wrong. Much strategic research is biomedical in nature.

with different research options could be readily estimated and available funds optimally distributed. At risk would be the personal incentives that drive scientists, such as a high degree of autonomy and satisfaction of professional curiosity. A very different approach would be for the Forum to advertise the broad research topics as listed in Table 6, and to invite researchers and potential user groups to further define the specifics of the agenda. The risk here is that a researcher-initiated agenda might deviate significantly from the R&D trajectory expected to reap highest returns.

In reality, foreign donors call the shots to a large extent. Although government funds provide for research salaries and associated benefits, Tanzania relies heavily on external resources for *direct* research expenses. Consequently, the national research profile is more a reflection of prevailing donor interests than country-initiated concerns. But in the short- to medium-term, specific research questions generated from the topics identified could play an important role in reconciling donor and national interests – particularly if donors recognised the investment portfolio as the research arm of the health sector reform program currently underway. One of the weaknesses of sector-wide approaches to health reform (SWAPs) is that they often fail to account for local variation. Another is that they place great store in management and structural change, but do not necessarily tackle the factors impacting most on quality of care in individual districts. A portfolio of health research reflecting national priorities yet emphasising the importance of locally defined projects, could assist in making the health sector reform program really responsive to Tanzanians' needs.

In the medium-term, Tanzania should be looking to national governance of its R&D portfolio. And as the National Forum develops, it may have more direct say in the allocation of resources for R&D. In that situation, an option to keep the portfolio aimed at highest benefit yet preserve personal incentives for scientists, would be to specify a detailed research agenda but retain the possibility of funding researcher-initiated projects as well. These projects should *still* be required to meet the criterion of high-expected social benefit. Appendix B defines specific research questions corresponding with the generic topics identified by workshop participants. This appendix is largely by way of illustration, intended to provoke precise definition of the problems underlying the topics identified.

2.2 R&D in response to service problems

The above research portfolio is a response to the priority diseases identified, and does not address the health systems and socio-cultural priorities listed separately. My view is that a research agenda for these priorities requires a different approach, for the following reasons:

- Priorities and research topics are poorly correlated across groups [Tables 3 & 4], suggesting considerable local variation in research priorities
- Many of the topics listed inevitably overlap with those in the research portfolio for priority diseases, because they have the same objective in mind, namely to reduce the burden of disease. For instance, improving "case management" is a research topic identified for most of the disease priorities. Similarly, several of the **service delivery** priorities address components of case management - such as prescribing and dispensing practices, use of health educators and assessing the skills gaps of health professionals. For many problems, it's simply a case of saying the same thing in a different way, and divorcing "service management issues" from improvements in health status obscures the central purpose of the health system.

A starting point is to distinguish between research that requires countrywide data collection and/or entails national policy analysis, from research that needs to be done locally. Obviously, all national research eventually has local implications - and local research projects should be the building blocks for national policy analysis. The research is not substantively different, but the level of focus is. Perhaps it's useful to think in terms of health systems research that 'zooms in' on a specific problem or 'zooms out' to place it in wider context [Table 7]. 'Zoom-out' research can be organised into a national research portfolio. Table 7 defines a number of national research surveys, which are summarised in Appendix B, part II.

On the other hand, 'zoom-in' research has to arise from the specific problems being experienced by district management teams if it is to have real practical application, and attempts to impose a nationally agreed-upon research agenda on districts will almost always fail. A research agenda customised to local needs will also allow for important research that did not make it onto a national agenda to be considered. In Part II, I propose a national mechanism for supporting health systems and socio-cultural research that is locally initiated.

Table 7: A national health services research (HSR) portfolio, and typical HSR topics that may be locally initiated

Health service problem	National research agenda (zoom-out)	Local research (zoom-in)
Inadequately trained personnel	• Distribution of staff per 10 000 population (by district, staff category and by level of care) • Assessment of skills gaps	• Factors leading to relative & absolute staff shortages
Lack of equipment & drugs	• National sample survey of equipment & drugs in clinics* • National hospital survey of state of equipment and availability of drugs** • Improving the efficiency of the national drug management system	• Inventory of equipment in district facilities • Improving the efficiency of the district drug management system • Prescribing and dispensing practices of health workers
Lack of transport for supervision & distribution	• Development of national guidelines for transport management	• Improving the efficiency of district transport management and use
Few funds for preventive care	• Costing of health services • Guidelines for allocating funds among service functions and levels	• Cost sharing and its consequences • Alternative sources of financing district health services
Ignorance and low health education	• Impact of information and communication strategies	• How effective are current modalities for motivation and how can they be improved? • Impact of information and communication strategies (local) • Assessment of use of health educators • Ways of obtaining community responses to health education methods
Impassable roads	• Initiative for multisectoral planning and action to improve roads and communication (national)	• The impact of poor communication and road infrastructure in complicating pregnancies • Referral patterns from district to tertiary facilities • Initiative for multisectoral planning and action to improve roads and communication (local)
Building decay	• National facilities maintenance survey**	• Evaluation of use of cost-sharing funds for building rehabilitation when cost-sharing is implemented • Local facilities maintenance survey
Inadequate water supply	• National sample survey of basic infrastructure*	• Assessing most appropriate technology for harvesting rainwater for clinic use • Community participation in improving water supply to clinics
Poor environmental sanitation and water supply	• Evaluation of coverage of existing water supply initiative	• Multisectoral interventions to maintain water and sewage systems
Inadequate health facilities	• Distribution of health facilities per 1000) population (by district and type of facility) • National facilities maintenance survey**	• Local facilities maintenance survey

* These can be components of one national sample survey of clinics, and in fact the recently completed situation analysis of 37 districts for the health sector program is probably adequate

** These can be components of one national hospital survey

2.3 R&D that addresses socio-cultural determinants of health

Similarly, a few national level research projects pertain to socio-cultural impacts on health, but most topics are local-context specific [Table 8].

Table 8: A national research portfolio addressing socio-cultural determinants of health, and socio-cultural research typically requiring local definition

Determinant of health	National research projects	Local research
Food taboos in pregnancy		• Knowledge, attitudes and practice (KAP) studies • What health messages would be effective in changing local food taboos? • What are appropriate media for communicating messages?
Poor latrine usage		• What factors hinder the construction and use of latrines? • Design of latrines appropriate to local environments • [Correlation between hygiene status and disease burden]*
Behaviour-linked poverty	• [Correlation between income & health status]*	• What income generating activities may alleviate poverty? • How can the household responsibilities of women receive a fair share of household finances?
Polygamy	• Describing and mapping the practice of polygamy in Tanzania	• Exploring the relationship between polygamy and the spread of HIV/AIDS • KAP studies on polygamy
Ignorance and high illiteracy	Impact of economic and political reforms on social services	
Gender inequality	• Correlation between gender inequality (income, education) and health status in communities	• KAP study on early marriages with a view to law reform • Impact of early marriages and gender inequality on the health of women and children
Witchcraft	• Assessment of the extent of witchcraft in Tanzania	• Development of communication resources and IEC programmes on witchcraft beliefs • KAP studies on witchcraft • Assessment of relationship between witchcraft and mortality rates
Inheritance of widows	• KAP study on widow inheritance among various tribes	• Relationship between widow inheritance and the spread of HIV/STI
Low acceptance of family planning		• KAP studies • Factors influencing acceptance of family planning
Use of local herbs	• What local herbs are commonly used to treat illness? • Are they effective?	

In summary

Tanzania has agreed to place emphasis on strategies that achieve greater equity in resource allocation and improved efficiency of resource use [Figure 5]. It has identified and ranked research topics expected to achieve greatest improvements in health status [Tables 6-8].

The National Forum on Health Research should decide on how prescriptive it intends to be in framing the research portfolio. Detailing a specific research agenda, yet retaining the flexibility to fund researcher-initiated projects seems to be the most practical way of maximising public returns while enabling researchers to accrue personal benefit as well. In the short- to medium-term, the national portfolio may play a more limited role, but can still be an important instrument in negotiations with donors. It could have a significant impact in shaping the health sector reform program if it were accepted as the program's research arm.

Although a number of national research studies were identified to address service delivery problems and socio-cultural determinants of health, much of the research effort should be tailored directly to local needs and realities. For this reason, it is not possible to develop a definitive research agenda for these components of the overall portfolio, and a national mechanism should be established for supporting locally initiated research.

3. How Much Public Money Should be put into Each Investment Instrument?

The research portfolio established through the priority setting process needs to be translated into a diversified *investment* portfolio that responds to new opportunities as they emerge, is cognisant of budget constraints, and takes existing funding commitments into account.

The first task is to estimate the costs of direct expenses related to the research identified at the national priority-setting meeting, and to truncate that list according to rankings so that it is financially feasible. This paring process should make provision for the indirect costs associated with running research institutions, and total public funds should be "topsliced" for this purpose. I suggest that a reasonable ratio of indirect to direct costs is 1:2.[4] The portfolio may be further refined to account for future health problems that warrant action now, to exploit transient opportunities generating unusually high expected benefits, or to "sunset" less relevant projects. Finally, revenue centers should be established to channel funds for direct costs and to allow for monitoring of financing flows.

Although this may seem like a theoretical exercise, in that neither the National Forum nor the Tanzanian government has control over most financing for direct expenses, it does provide a basic framework for financial management of R&D. Without costing the research agenda identified through priority setting, it remains a "wish list". Without understanding financing flows, there is no way of determining whether resources are gradually being aligned with national priorities. The following steps lay a foundation for financial accountability, regardless of the source of revenue.

3.1 Estimate the direct costs of each project

Ballpark estimates of direct research costs will provide a good sense of how of the research portfolio can be tackled. Based on my own experience as research program officer in South Africa, I have estimated direct costs associated with each research question mooted in Appendix B. By way of illustration, these estimates are summarised in Table 9.

4 I base this suggestion on my experience working with research institutes and universities in South Africa, where indirect costs associated with research were typically 40-60% of direct costs.

Table 9: Ballpark estimates for direct research costs (per annum)

Research topics	Total estimates	Health priorities	Health service problems	Socio-cultural determinants
Identified by > 1 group National level		1 010 000	460 000	370 000
Sub-total	1 840 000			
All other ranked research Local research		1 205 000	460 000	370 000
Sub-total	2 035 000			
TOTAL	**3 875 000**	**2 215 000**	**920 000**	**740 000**

3.2 Estimate the "cut-off point" for an investment portfolio that stays within budget

If the suggested ratio of baseline to competitive funding is adopted, two-thirds of total funds for health research should be available to meet direct costs. The research agenda [Appendix B] should then be trimmed accordingly, but with due recognition that ranking uncertainty increases as one goes further down the list. For instance, there is no way of knowing whether a research topic ranked near the bottom of priority health problem #1 should be rated more highly than the first ranked research topic for priority health problem #2. Instead of a rigid cut-off point, it would be preferable to identify a zone of research options with similar expected benefit for which the actual investment decision is based on practical considerations described below.

At present, total budget estimates are very uncertain because there is no central database on funding for health research. Developing such a database is one of the tasks assigned to the National Committee on Health Research Coordination (see Part II). Once this is completed, the Forum will be in a far better position to assess total expenditure on health research. I have pieced together a conservative estimate of current annual spending on health research in Tanzania - about 9.6 billion shillings (US $12 million). Although a fair amount goes to a few well-funded programs, total spending on health research is not insubstantial – and *could* fund a large part of the portfolio.

3.3 "Topslice" for indirect costs of research

Tanzania can deal with the overheads associated with running a research institution in two ways. One option is to cost general administrative, management, maintenance and support expenses and spread them across all publicly funded research projects. All funds are then competitive, and institutions are wholly dependent on their ability to attract investments. Although competitive funding encourages diversity and discovery (Nalebuff & Stiglitz 1983) and is a mechanism for aligning research with national priorities, 100% competition is probably not a good option for Tanzanian institutions. In the first instance, the fragility of the R&D infrastructure will not tolerate dramatic fluctuations in funding across institutions from year to year. National capacity, already weak in certain research disciplines, may be severely jeopardised if even one institution does not secure adequate funding for a relatively short period of time.

And while baseline funding for institutions acts as a drag on incentives to do good research, it creates a degree of stability and security that permits risk-taking by researchers (Dasgupta & David 1994). It may also reduce destructive rivalry between research organisations. A more viable alternative is to have a mix of baseline funding for "indirect costs" of maintaining research institutions and developing a competitive system for direct investments in research. There are numerous ways of defining "baseline funding" and the "direct" and "indirect" costs associated with research. For clarity, I propose that baseline funding be regarded as funding for *indirect* costs. These are the costs associated with overall management, general financial and clerical administration, building overheads and janitorial and other support services. *Direct* costs of research include researcher time, administrative and support costs *directly* associated with that research, and the operational costs of laboratory or fieldwork. For researchers based in universities, it may be most practical to view the institutional subsidy from the ministry of higher education as the baseline allocation - and not to attempt any complicated funding formulae.

For years to come, historical trends and donor interests will determine the balance between baseline and competitive funding, and a high proportion of public money will fund indirect costs. But as the Forum assumes greater fiduciary responsibility for both public and donor funds, it will be better placed to manage the interface between competition and collaboration, providing incentives both for individual discovery and collective innovation.

3.4 Allocate investments to priorities

The investment portfolio proposed in Appendix B is the best guide we have for achieving maximum health benefits for the people of Tanzania through research. Yet it remains a guide, because actual investments will be determined by considerations, such as:

- Opportunities that exist leveraging additional benefits
- Actual feasibility of specific research questions
- Researcher interest and availability
- Quality of proposals and recommendations from peer-review
- Current research that may not be in line with priorities, but for which there is ongoing financial commitment.

An inevitable question is whether it is better to invest in a few big projects or to spread resources across a wider portfolio. The advantage of fewer projects is that more resources can be concentrated on resolving the major problems, and research may be conducted more thoroughly. The risk of "putting all eggs in a few baskets" is that even a single failed research project can have a disastrous effect on national research capacity. This approach also means that fewer problems are tackled. On the other hand, a diversified research portfolio spreads the risks and potential benefits, but may leave researchers a bit thin on the ground. On balance, I recommend that the National Forum for Health Research adopts a portfolio that is realistically costed yet relatively spread out, for the following reasons:

First, unlike commerce-oriented R&D typically linked to manufacturing processes, the

Tanzanian emphasis is on relatively low-cost problem solving. And even to the extent that Tanzanian scientists *are* working to make a contribution to global knowledge, limited resources are not insurmountable obstacles to major discovery. Rosenberg (1994) makes the point that most breakthroughs have emerged from simple – even primitive – research settings.[5]

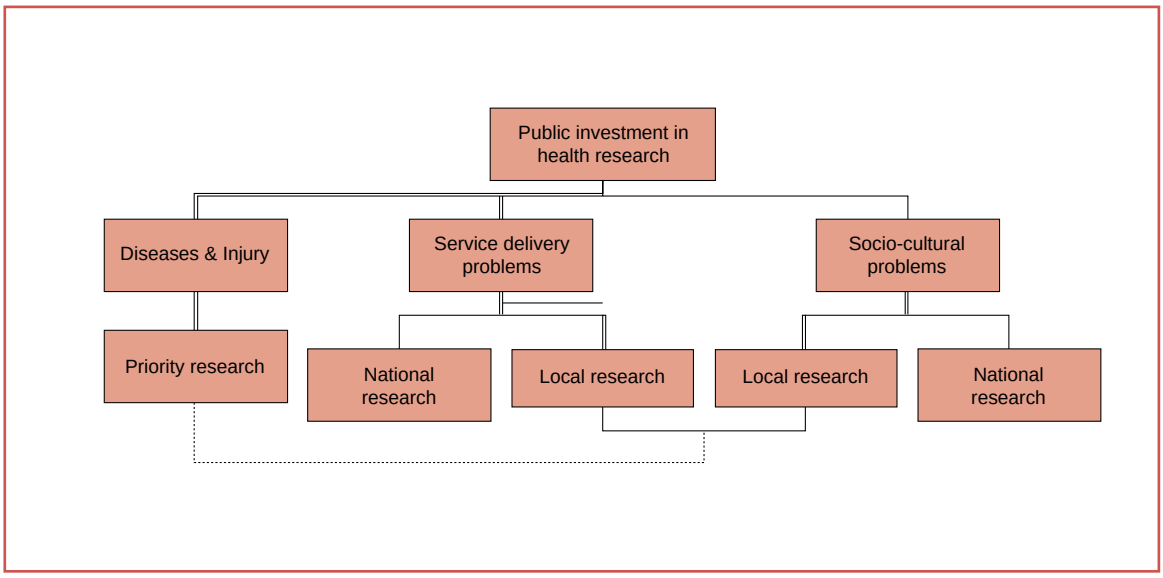
Second, additional resources are often used to strengthen institutional capacity. In low-income countries, there are considerable opportunity costs associated with allocating resources to more institution-building and at the margin, money could probably be more effective financing another small project.

Third, a country like Tanzania will battle to create and maintain a "critical mass of researchers" in the conventional sense, and must look to a different approach. A typical attribute of scientific endeavour is increasing returns to scale for human and financial investments in R&D (Dasgupta & David 1994). Yet it is unrealistic to develop centers of excellence in any one institution, and new communication technologies offer the opportunity to establish R&D networks across institutions and research projects. Internationally, such alliances are increasingly recognised as a new form of "critical mass" (Clark 1990). Specific recommendations for Tanzania are discussed more fully in Part II.

3.5 Establish revenue centers for direct expenses

Revenue centers should be defined to allow for funds to be invested in line with priorities. Figure 6 outlines a proposal for allocating and tracking public funds.

Figure 6: Proposed revenue centres for direct health research expenses



5 I am not suggesting that poor R&D infrastructure is an acceptable state of affairs, but simply pointing out that Tanzania has the potential for breakthrough discoveries of global significance, despite limited resources. Recognising this potential alone is not enough to warrant allocation of marginal resources to exploratory R&D when other problem-oriented projects have greater expected benefits.

This proposal is consistent with the national research portfolio outlined in Appendix B. Investments in locally initiated research can be channeled through three revenue streams, namely funds allotted for priority:

- Health service problems
- Socio-cultural determinants
- Diseases that require local analysis, and which are consistent with locally-initiated research programs.

Designating funds for local research is important in ensuring that resources for R&D are allocated more efficiently (see Part II).

3.6 Monitor the allocation of investments

The R&D profile illustrated in Figure 5 establishes ballpark estimates for allocating public funds across investment strategies. For example, warning bells should sound if there is overwhelming agreement that the country's priority is to improve the efficiency of existing interventions, yet most resources go to developing new ones. One annual responsibility of the National Committee for Health Research Coordination should be to produce graphs that compare the R&D profile established through priority setting with that of the projects actually funded. Provided that the system of classification is uniform from year to year, this simple illustration will be a powerful gauge of the degree to which public investments aim to maximise social benefits.

I suggest that these graphs become part of an annual publication that reviews national progress with respect to each of the ten disease priorities. This recommendation is discussed more fully in Part II.

In summary

The design of a public investment portfolio for health research is a logical extension of the priority setting process initiated in 1999. While the final investment portfolio will be shaped by practical considerations, it should bear a close resemblance – both in scope and strategic emphasis - to the research agenda agreed to in the national meeting.

Once the direct costs of individual projects have been estimated, it is possible to more-or-less circumscribe the range of feasible options. Before allocating funds to specific research projects, I recommend that a third of the total budget for health research be "topsliced" to provide baseline institutional funding.

The National Committee for Health Research Coordination might choose to review final allocations to:

- Address future health problems that may warrant action now (such as the health effects of environmental degradation and relocation of people)
- Exploit opportunities that would enable Tanzania to leverage even greater expected benefit
- Make provision for phasing out non-priority research.

The R&D profile [Figure 5] will serve as the basis for monitoring the performance of "portfolio managers". Significant deviation from this profile requires careful review. However, the custodial role of national research managers is only one aspect of their work. By far the most important are the strategies they use to ensure that research projects are conducted efficiently. Part II of this report proposes ways in which the National Forum for Health Research in Tanzania can accomplish this goal.

PART II: How Tanzania Can Implement its National Investment Portfolio Efficiently

The national investment portfolio for health research developed in Part I is expected to yield the highest returns for Tanzania, given existing capacity to implement the studies. However, much of the potential gain may be lost if the research program is poorly implemented.

The Committees for Health Research Coordination and Ethics in Health Research are executive bodies of the National Forum for Health Research.[6] Both will play a central role in ensuring that the investment portfolio is implemented efficiently, by:

- Enhancing research outputs; and
- Reducing the costs of research.

These twin objectives are the basis for recommendations in Part II; thus it may be helpful to clarify my understanding of how R&D outputs can be enhanced and costs reduced.

6 The functions of the Coordinating Committee are to: develop strategies for health research coordination; implement national guidelines for health research; maintain a national database on health research projects; develop guidelines for partnership in research; provide an inventory on existing communication systems and institutional capacity in Tanzania; and enhance communication among research institutions.

The functions of the Ethics Committee are to ensure that all research abides by human rights, that ethical guidelines are adhered to, to make recommendations on ethical issues and disseminate information on the rights of communities and individuals with respect to health research.

4. Conceptual Framework for Greater Efficiency

4.1 Enhance research outputs

For the purpose of this discussion, research outputs are enhanced if they lead to greater social benefit (holding costs constant). The gist of my argument is that, in order to maximise social benefits far greater effort should be made to stimulate the **demand** for research. Considerable potential also exists to bolster supply simply by reallocating and leveraging existing resources.

4.1.1 Expand the demand for health research

Gibbons *et al* (1994) state that although the context for research cannot be regarded as a standard economic market, it is nevertheless a "social market" in which researchers produce outputs used by different types of consumers. The model behind much of the research effort in low-income countries is a supply-driven one. It is based on the assumption that if we can train enough researchers and build enough institutional capacity, research outputs will be put to good use. Market-driven (economic) incentives are thought to provide much of the impetus for innovation, and - whether intentional or not - an implicit assumption of supply-side strategies, is that the market will do the rest. This assumption draws on conventional economic wisdom that the main market failure in R&D is under-investment in basic research – because basic research has no obvious commercial application and therefore requires public financing (Pavitt 1991). Yet the experience of low-income countries is that under-investment in 'upstream' research is not the only "market failure", and the demand for research expected to meet an enhanced supply often fails to materialise (Alvendia 1985, Bhagavan 1992). Public officials, the media, industry, community groups and other potential users rarely seize the opportunities to capitalise on new knowledge. This weak demand is reflected in low national investments in R&D, low salaries for researchers, and limited use of research findings. Newly trained researchers find little incentive to remain in universities and other public research centres. Those that do remain find difficulty in sustaining enthusiasm for life-long learning and innovation and many settle into a bureaucratic mode of working with little potential for new discovery, further suppressing the aggregate demand for research (Acemoglu 1997). Supply-side capacity building strategies that do nothing to stimulate the demand for research are unlikely to achieve expectations, and may actually further distort allocations by creating incentives for scientists to capture much of the benefit from research as private gains. Without public demand for useful research, strengthening institutions may help create personal empires and fostering private incentives may lead to self-aggrandisement of researchers.

Bowles & Gintis (1996) refer to this mismatch between supply and demand as "coordination failure". Innovation theorists echo this concept of disequilibrium, describing inefficient research as uncoordinated "pushing and pulling" – being tugged in different directions by the respective motivations of researchers and users. Researchers "push" R&D in the direction of their own interests and scientific incentives. Market oriented users "pull" research in the direction of applications they expect will yield highest returns. In this situation, the research leadership can be instrumental in efficiently "integrating push and pull" (Baskerville & Pries-Heje 1997).

However, science and technology managers have traditionally focused on detailed financial, physical and human resource planning: How many researchers do we need? What institutional capacity is required? What level of investment in R&D is sufficient? Now, there is realisation that the main purpose of research leadership is in stimulating interaction among researchers and between researchers and users (Neufeld et al 1995, Segal 1987). In time, demand-induced research should translate into greater public benefit to society and more private

benefits to researchers. As researcher remuneration increases, so will its cost to society. But these costs are outweighed by the added public benefit – a "win-win" situation.

Greater researcher-user interaction is a necessary, but insufficient condition for stimulating demand, as this exchange is only productive if it is accompanied by **active learning**. Learning - the application of knowledge - is now regarded as the major factor in global productivity. Some have viewed the changing basis for economic growth as an unprecedented opportunity for poorer countries: "Regardless of current capabilities, individuals, firms and countries will be able to create wealth in proportion to their ability to learn" (Johnson 1994). Not only can the use of knowledge promote economic growth, but can also lead to better social outcomes. For instance, the World Development Report 1998/9 cites Costa Rica as a country that has achieved better than expected health as a result of a systematic policy to disseminate and use health-promoting knowledge (The World Bank 1999). In the words of innovation guru Peter Drucker, "the comparative advantage of less developed countries no longer lies in lower labour costs, but in the application of knowledge" (Drucker 1994). My own view is that these optimistic projections miss a fundamental point that the *ability* to assimilate foreign technologies is itself a function of socio-economic development (Birdsall & Rhee 1993). For most low-income countries, predicting fast-track development and "leapfrogging" into the 21st Century is naïve; slow but sure economic growth, accompanied by steady improvements in education and health, is the basis for long-term development (Tanzi & Chu 1998). Despite my skepticism, two insights are highly relevant for Tanzania. The first is that strategic research is most efficient when it is constantly interacting with, and learning from, real-life experience. Sharing, exchanging ideas and results as they emerge can be a powerful impetus for efficient research outcomes. The second is that considerable efficiency gains can be achieved simply by applying knowledge already available within Tanzania. This insight reinforces the dominant message emerging from Part I that using existing tools more efficiently is the key to better health in Tanzania – and learning from good practice is itself an effective instrument.

4.1.2 Expand the supply of health research

Nurturing the supply of health research is an important way of enhancing research outputs, but the focus of supply-side strategies is often too narrow. For instance, building up a stock of resources for R&D is often emphasised at the expense of allocating them most efficiently. Alliances have been forged with international partners to the detriment of national consortia. And leveraging resources has been equated with gaining access to donor funds, with inadequate attention given to creating a synergy of national efforts. A different, entrepreneurial mindset opens up new possibilities for Tanzania. This new approach views the health research leadership not as information bankers, but as "knowledge entrepreneurs", who aim to squeeze as much social benefit as possible out of every shilling. Research leaders may be thought of as investment portfolio managers, whose tasks are to:

- Constantly redirect resources towards options expected to give the highest returns
- Seize on new opportunities offering unusually high expected benefits
- Achieve economies of scale and risk sharing through innovative partnerships.

By executing these tasks, research managers in Tanzania can add substantial value to current investments in R&D.

An effective strategy for reallocating resources toward greatest social benefit is to design appropriate **incentives**. Although some researchers in low-income countries can compete on the international market, most have low opportunity costs and salaries are poor relative to

other professions in the country. Given the state of the economy, there is little prospect of increasing financial remuneration, so efforts need to be directed towards improving the "psychic reward" of being a researcher. However, individual rewards and incentives are difficult to institute and, in any case, are inconsistent with the team-based approach advocated in this report. Where interaction and collaboration are the driving forces for innovation, personal financial incentives may well be counter-productive, and team incentives are more efficient (Gibbons *et al* 1994).

Amabik (1999) suggests that the strength of team incentives depends on the:

- Amount of challenge they give
- Degree of freedom around the process of R&D
- Way teams are designed
- Level of encouragement
- Nature of organisational support.

Carefully designed, team incentives could enhance outputs by redirecting effort towards greatest social benefit and improving the quality of R&D. Specific recommendations for Tanzania are discussed in section 6.1.

In sum, stimulating demand for research and reallocating resources to maximise expected social benefit should increase the returns to R&D. A second way of increasing returns is to reduce the costs of doing research, holding outputs constant.

4.2 Decrease costs

In real terms, researchers in low-income countries face higher costs than their counterparts in wealthier countries do. These differentials may be caused by higher:

- Financial costs (almost all financial transactions are more expensive)
- Economic costs (*transaction* costs are greater, particularly in communication and collegial interaction)
- Political costs (researchers may incur personal and professional costs in environments where free speech is repressed).

In Tanzania, there is a high level of academic freedom and criticism of the government is tolerated. Political costs are therefore not a major obstacle to good research. However, other costs are a major cause of inefficiency in R&D, yet many can be reduced relatively easily.

Dasgupta & David (1994) argue that the main transaction cost in research is in **communicating information**, and their argument is vividly illustrated in Tanzania. Not only is communications infrastructure poor, but researchers find it difficult to tap into global R&D networks with little interest in low-income countries (Gibbs 1995). Furthermore, the shift away from knowledge as a public good to knowledge as a sellable commodity is pushing up the costs of acquiring information. The agreement on Trade-related Aspects of Intellectual Property Rights (TRIPS) enforced by the World Trade Organization, now compels low-income countries to pay market prices for externally produced information and adhere to international conventions on copyright, information services, and databases (UNDP 1999). In real terms, poor countries pay far more for the same information than wealthier countries do. This is of particular

concern to basic science research, and may limit the ability of Tanzania to contribute to international initiatives for vaccine and drug development. However, not all excessive costs are externally imposed, and in the following section I argue that the highest costs are incurred by poor communication among research programs within the country. An optimistic take of the situation is that potential exists for communication costs to be reduced fairly easily. This is one striking example where greater efficiency can be achieved. There is obvious room for efficiency gains in others areas as well, also described in the following section.

In summary

Research efficiency is improved by enhancing outputs and reducing costs. Strategies to stimulate a demand for research do not receive as much attention as efforts to increase supply, yet hold the key to substantial efficiency gains. Supply-side strategies tend to focus on gaining access to new resources, but pay less attention to using existing resources better. Improving efficiency will require the research leadership to adopt a less custodial and more entrepreneurial role in using resources for R&D.

The major transaction costs in Tanzania are incurred in communicating information. Although this is partly due to infrastructural difficulties and international isolation, many of the costs are self-imposed through inadequate interaction within the country. The potential exists for current outputs to be dramatically boosted through new learning partnerships.

5. Three Striking Opportunities For Greater Efficiency

Although I only spent a short time in Tanzania, I was struck by three opportunities for enhancing research outputs and reducing costs.

- First, there are **clear gaps** in the current national investment portfolio, both in terms of scope of funding and the type of R&D instruments employed in addressing priorities. Filling these gaps will improve efficiency of allocation of research funds.
- Second, despite pockets of R&D efforts, there is **no sustained national program** to improve equity in resource allocation and efficient use of existing tools at **local level**.

A program of district-based problem-solving, sharing knowledge and learning from each other would not only fill in some of gaps in the spatial distribution of research, but may also increase returns to R&D by stimulating demand across the country.

- Third, **communication is constrained** by tangible deficiencies in infrastructure, as well as by invisible barriers between research organisations. Dismantling these barriers could boost R&D outputs and reduce transaction costs.

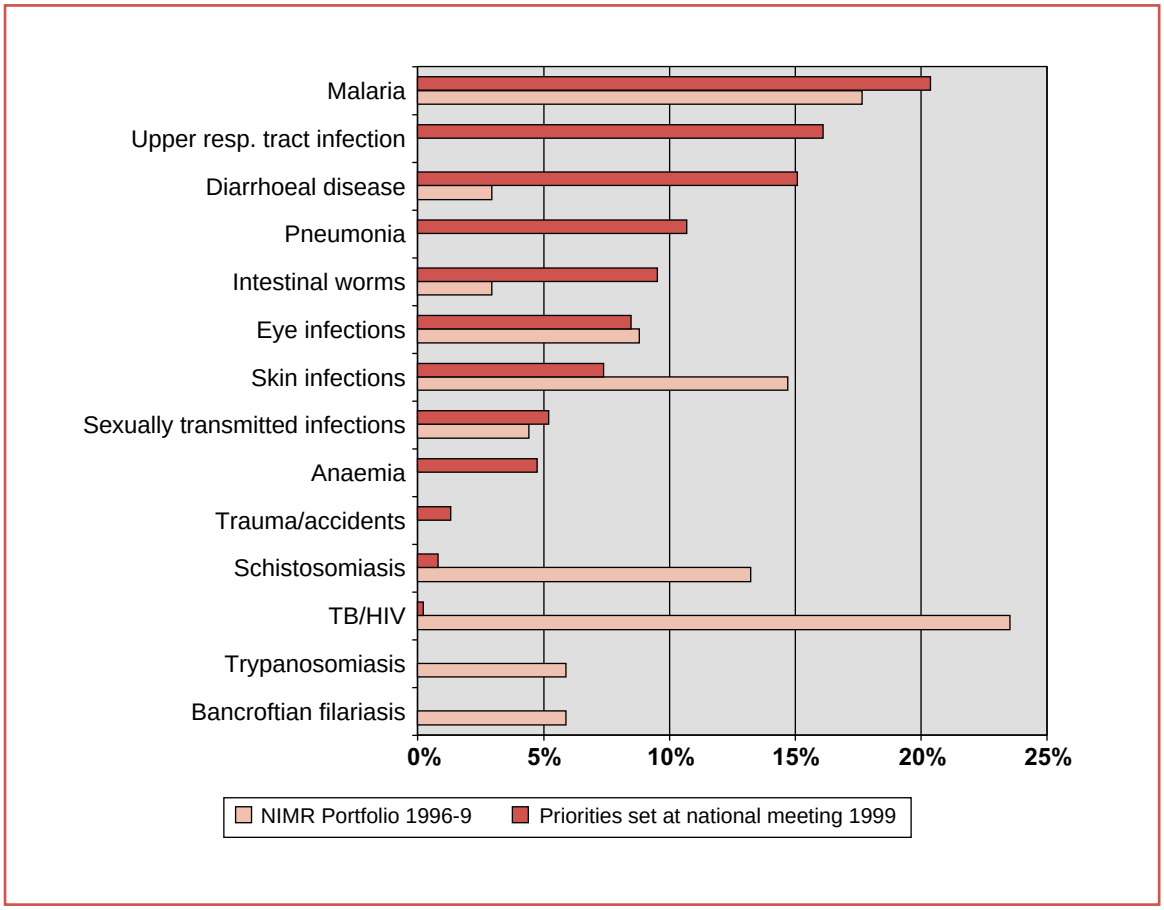
Following is the evidence behind each observation. As there is no national database of health research, evidence is pieced together from a number of different sources – and is still incomplete. Nevertheless, all information at hand tells a consistent story, and in the absence of evidence to the contrary, reinforces the arguments I make.

5.1 Clear gaps in the current national investment portfolio could be filled

Compared with the national investment portfolio developed in Part I, the existing research portfolio has glaring deficiencies with respect to both **disease priorities** and the **R&D instruments** expected to attain greatest social benefit.

In terms of **disease priorities**, there is under-investment in R&D responding to acute respiratory infections and diarrhoeal disease. Anaemia is also neglected, despite its prominence as a cause of death. All three diseases hit children hardest, although anaemia is also a major problem in adults. This suggests that malaria is so dominant a cause of death in children that it obscures the extent of mortality and morbidity due to other infectious diseases. Figure 7 illustrates the profile of research done by the National Institute of Medical Research (NIMR) from 1996 to 1999 (NIMR 1999b). Note that this profile simply reflects the number of discrete projects, not the size of investments. It is nevertheless an indicator of how much attention is paid to different disease priorities.

Figure 7: Major causes of childhood mortality – acute respiratory infection, diarrhoeal disease and anaemia – are neglected



Although the current profile mirrors some aspects of the past, with major investments in tropical diseases like onchocerciasis, Bancroftian filiariasis and schistomiasis, there is evidence that NIMR *has* responded to new and re-emergent diseases. There has been considerable investment in HIV/AIDS research and associated tuberculosis.[7] And although few projects have been undertaken to date, diarrhoeal disease is now a focus of NIMR's Mwanza Research Centre.

In addition, there is increasing activity in health policy and systems research aimed at promoting equity and efficiency (6 out of 74 projects).

Other health research institutions include the University of Dar es Salaam, Muhimbili University College of Health Sciences, Sokoine University of Agriculture, Kilimanjaro Christian Medical Centre and Ifakara Health and Development Research Centre. Although I did not have access to *their* current research portfolios, all institutions are represented at the Annual Joint Scientific Conference at which research findings are presented. The theme of this year's Conference was "Health sector reforms: Challenges for health research in the 21st Century (NIMR 2000)."

7 Although TB and HIV research do not feature in the top ten disease priorities identified at the national priority setting meeting, this probably reflects systematic bias in the methodology used to establish priorities (see section 1.1)

The high proportion of health policy and systems research projects (42%) is therefore to be expected. However, research topics related to specific diseases once again illustrate neglect of childhood illnesses other than malaria - only 4 of 54 projects addressed such.

All pointers suggest that greater investment in research related to acute respiratory infection, diarrhoeal disease and anaemia should improve efficiency of R&D allocations.

In terms of R&D instruments expected to attain greatest social benefit, there is an obvious gap in investments aimed at **promoting efficient use of existing tools**. Once again, I have cobbled together evidence from diverse sources (NIMR, TEHIP, AMMP, and conference presentations). Collectively, these programs account for over half of total funding for health research. Educational institutions are under-represented, yet there is little to suggest that their patterns of R&D are substantively different from the programs listed above. The following series of Figures (8a – 8d) is an analysis of all publications emanating from the Amani Research Centre since its inception in 1949 (Mboera 1999).[8] Amani is one of five research centres run by NIMR. Located about 400 km from Dar es Salaam, it is situated on a hill in the Eastern Arc Rain Forest of Tanzania. Its disease focus is malaria, Bancroftian filariasis and ochocerciasis – and while focus areas differ from center to center, the *type* of research conducted at Amani is fairly typical of that done by the others. For this reason, an analysis of trends over time at Amani gives a good sense of trends in the strategic direction of the NIMR as a whole.

The real value of this series of graphs is that it points to whether the R&D objectives of Tanzania are gradually shifting towards efficiency and equity, possibly as a result of heightened interest by international health organisations in efficiency in the 1980's and equity in the 1990's. If this shift is occurring, there may be little need for deliberate effort by the National Forum on Health Research to bring the national portfolio in line with greatest expected benefits, as international incentives would already be playing this role. However, if the trends are equivocal or inadequate, the Forum may need to create additional incentives within the country itself. Of the four graphs, those relating to malaria and "other" diseases are probably most instructive in that interventions (of variable efficacy) targeting these problems have existed for some time. The use of ivermectin in the last decade, originally developed as a veterinary product, has made onchocerciasis more amenable to therapy, and one would expect a change in its research profile over the next decade as well – away from new product development and towards efficient use and equitable drug distribution. Diethylcarbamazapine, used to treat Bancroftian filariasis (elephantiasis) is a relatively toxic and allergenic drug and efforts to find safer drugs and simpler interventions are warranted.

8 I am indebted to L. Mboera for his recently completed annotated bibliography of every publication resulting from research projects at Amani Research Centre (Mboera 1999). Note that the analysis is based on publications, not discrete research projects, so that a single project may be represented several times in the data.

Figure 8a: Malaria research is heavily concentrated on new product development and assessing drug sensitivities[9]

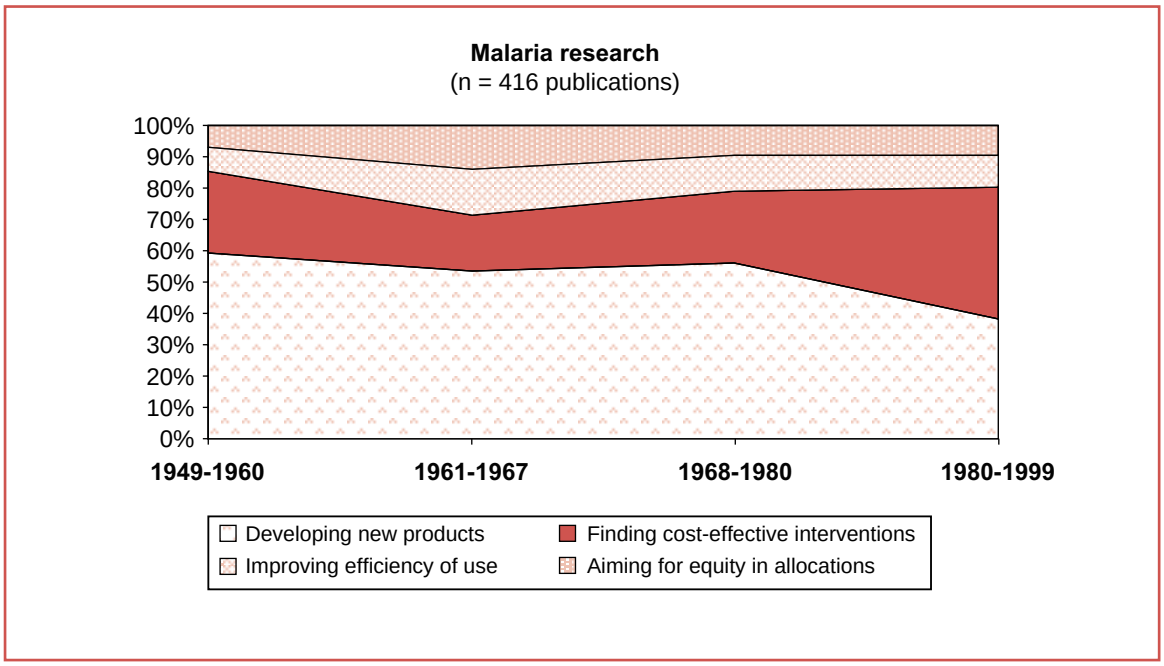
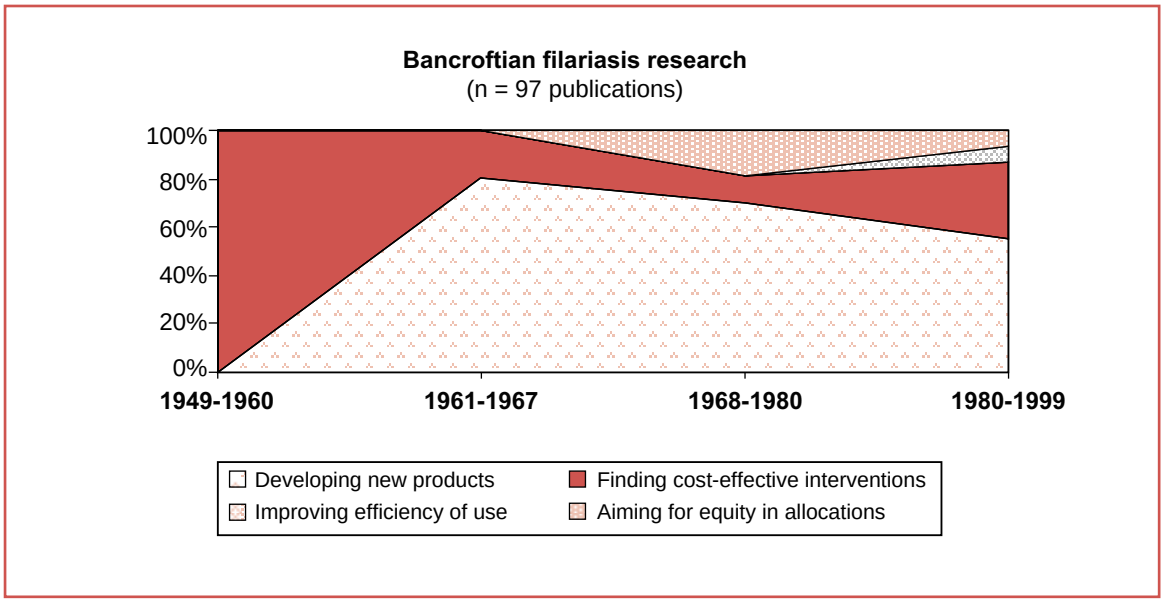


Figure 8b: Safer drugs are needed for Bancroftian filariasis, appropriately reflected in the research profile



9 All epidemiological studies aimed at assessing the prevalence and distribution of disease have been categorized as "equity studies". Drug sensitivity analyses are categorised as efforts to compare cost-effectiveness of different treatment regimens (although cost is usually an implicit consideration). Descriptive vector studies are regarded as contributing to new product development, as are descriptive studies of biochemical and physiological changes associated with disease. All research was regarded as strategic, although this definition was stretched for some studies that had little obvious application.

Figure 8c: Ivermectin is efficacious against onchocerciasis – the challenge now is to get the drug to those who need it most

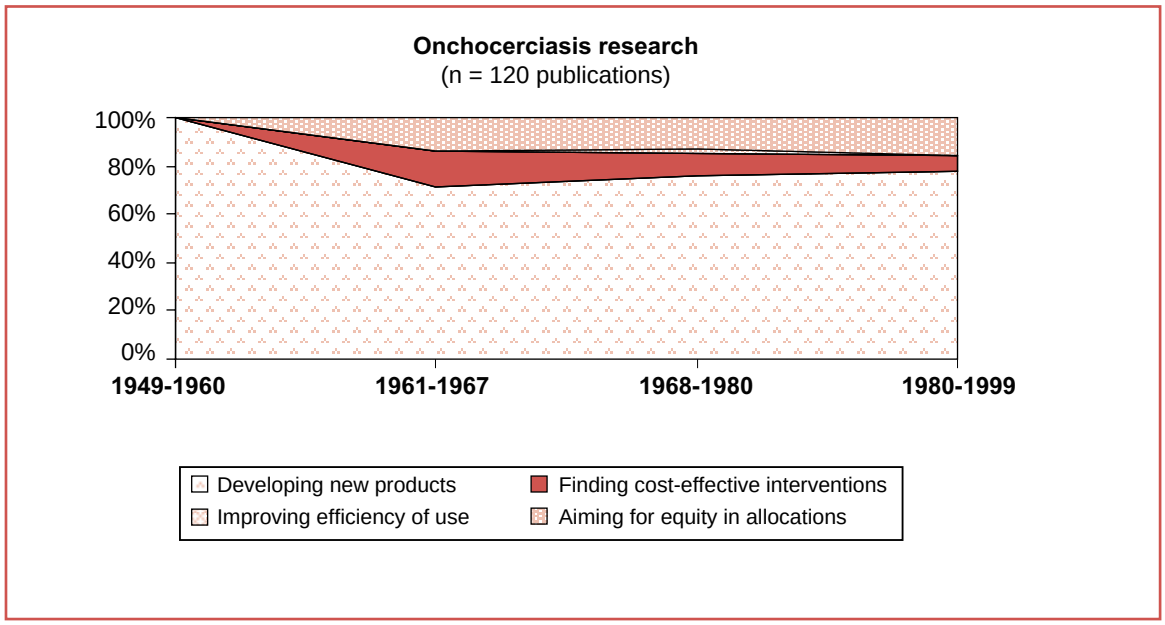
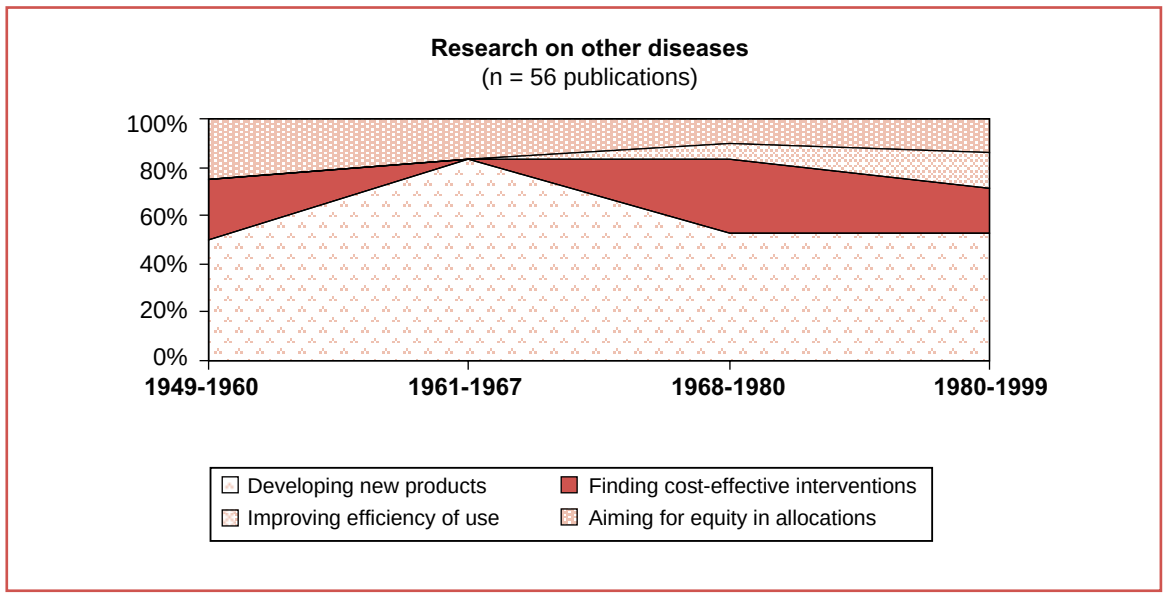


Figure 8d: The proportion of effort devoted to improving efficiency of resource use and equity of allocation is largely unchanged over time[10]



Amani's R&D profile for malaria reflects a shift from new product development to improving cost-effectiveness of known pharmaceuticals. This is largely due to a decline in exploratory and descriptive vector studies, and an increase in studies evaluating drug sensitivity. However, there may be a slight trend towards fewer publications dealing with equity or efficiency.

10 Time intervals reflect discrete periods in health policy in Tanzania. Political independence occurred in 1961, and 1967 marked the start of greater government involvement in health service provision. By 1980 primary health care was established as the main form of service delivery. Since then, structural adjustment and its associated curtailment of state services have dominated.

On the other hand, there are signs of growing interest in equity and efficiency studies with respect to diseases other than the three major ones tackled at Amani. The strategic emphases for Bancroftian filariasis and onchocerciasis still rest heavily on efforts to develop new interventions. Overall, trends are quite equivocal and there is little to suggest that there will be a substantial change in emphasis without intervention by the research leadership. The implication is that international incentives have reinforced the focus on developing and testing new products – or at least been ineffective in stimulating relevant efficiency and equity studies – and strong national incentives are required to reallocate resources to maximise expected benefits.

The question remains whether *other* research organisations are filling the gap in equity and efficiency-related research. The Tanzanian Essential Health Intervention Project (working in conjunction with the Ifakara Health and Development Centre) and the Adult Morbidity and Mortality Project (AMMP) are two large programs undertaking surveillance of all major health conditions, including childhood infectious illness other than malaria. The main objectives of both TEHIP and AMMP are to provide evidence for resource allocation within districts and at national level. As such, they can be regarded as promoting equity and efficiency of allocation. However, they do not work actively with district management teams to improve *technical* efficiency; and only concentrate on 7 of 126 districts (5.5%). The Tanzanian Food and Nutrition Centre places strong emphasis on program implementation within its scope of activities, and international organisations like UNICEF, the World Health Organization and the African Medical Research Fund (AMREF) do support research aimed at improving operational efficiency. To some extent these institutions *do* fill the gap, yet their combined research efforts still fall far short of the investment portfolio expected to maximise social benefits – which places nearly half (46.5%) the emphasis on improving efficiency and a further fifth (17.6%) on promoting equity.

Opportunity exists to improve the efficiency of allocation of public funds for R&D by:

- Investing more in resolving the problems of acute respiratory infection, diarrhoeal disease and anaemia
- Implementing a concerted program of research aimed at improving operational efficiency of health service delivery.

5.2 A countrywide initiative could stimulate local demand for research

Despite the spatial variation in service delivery problems and socio-cultural determinants of health – and even in disease priorities – research is unevenly distributed and concentrated in relatively few districts. While this concentration of effort may be an attempt to maintain a "critical mass" with scarce resources, far more could be done to ensure that research results are regularly synthesised and shared among all districts. This finding is consistent with the first observation, in that a national program for improving operational efficiency of service delivery has to be rooted locally yet have in-built mechanisms for "knock-on" impacts to other districts. A collaborative program could enable researchers and district management teams to learn from problem-solving activities in districts across the country, and is a practical way of stimulating the demand for health research. As the system of "basket grants" is rolled out, district health budgets will be boosted by 15 - 25% in most areas. This is an opportunity to ensure that spending plans target greatest need and are implemented more efficiently.

The Health Research Users' Trust Fund (HRUTF) is a mechanism for district managers to access research money and potentially, this Fund could facilitate learning how delivery problems

can be solved. Its explicit goal is to fund demand-induced research. At present though, its mode of operation is retroactive, waiting for responses to its request for proposals. In the future, a revamped HRUTF could play a proactive role in a countrywide district support program aimed at improving service delivery.

Opportunity exists to develop a broad-based portfolio of research aimed at solving local problems, enabling district management teams and researchers to interact with and learn from one another. If this opportunity is seized, the national investment portfolio should realise better returns both because resources will be allocated to initiatives expected to maximise social benefit and because the demand for research should be strengthened.

5.3 Poor communication can be improved fairly readily

Our trip to Amani Research Centre illustrated many of the obstacles to communication experienced by researchers in Tanzania. The road to Amani requires a high-clearance vehicle, and in rainy weather a four-wheel drive. The crank-handle telephone connects to a manual exchange and for a day of our visit, there was no electricity. With the exception of six current journal subscriptions, the most recent publications in the library are over a decade old.

Despite this apparently bleak picture, there is cause for optimism. A recent grant to NIMR will soon allow for all research centres to be connected by wireless telephone. All centres have good computers and training abroad has meant that many young researchers are well acquainted with new information technologies. Personnel trained as managers of information repositories - who now need help in becoming "network managers", staff libraries. Not only do they need to capture and catalogue information, but to actively share knowledge. Their new role requires content knowledge, new analytical skills and an understanding of information technology (Batt 1997). Nevertheless, all the ingredients for better use of new information and communication technologies are beginning to fall into place. Provided that the necessary technical and user support can be developed, Tanzania should begin to overcome many of the infrastructural obstacles to communicating research.

However, my observation is that the main barrier to effective communication in Tanzania is not externally imposed, but is created by the tendency of research programs to "go-it-alone". I was struck by the lack of day-to-day interaction and knowledge sharing among major programs, even housed in a single research compound. Although the argument can be made that pursuing research objectives with singular purpose prevents distractions, my sense is that valuable opportunities for amplifying returns on R&D investments through collaboration are being lost.

Regular newsletters and conferences are ways of communicating "codified" knowledge, and research institutions in Tanzania now publish a number of publicly available outputs. These publications are well-done and well-received, but do not replace the need to share "tacit" knowledge through informal contact and interaction - now recognised as a vital ingredient for innovation (McDonald 1998, Gibbons *et al* 1994). It is this form of "tacit knowledge diffusion" which requires more attention in Tanzania. Informal interaction may take numerous forms, including face-to-face contact, personnel exchanges, site visits, electronic discussions, joint projects, and designing common research outputs and publications. Of course, meetings may become ends in themselves, and networks are typically most successful when they are built around specific functions or tasks (Day 1997).

In terms of interaction between research institutions and service providers, the Tanzanian Public Health Association seems well placed to facilitate exchange, particularly through its strong linkages with regions and districts. This role would strengthen research/service links already well established at national level.

Opportunity exists to achieve substantial efficiency gains through better communication, effectively:

- Reducing the costs of interaction
- Stimulating greater demand for research.

Following are strategies for making the most of the three opportunities described above.

In summary

Three striking opportunities exist to improve efficiency of health research in Tanzania.

First, the R&D portfolio will realise higher returns if more is invested in the priorities of acute respiratory infection, diarrhoeal disease and anaemia. Most needed is operational research aimed at improving technical efficiency and achieving greater equity in resource allocation.

Second, by responding to local demand, operational research could achieve high returns – especially if efficiency gains were replicated in other districts through a countrywide process of learning.

Third, improved communication and interaction among researchers, and between researchers and district managers could reduce transaction costs and stimulate demand for research.

Seizing these opportunities would go a long way in maximising social benefits as the new investment portfolio is implemented.

6. Practical Ways to Seize the Opportunities

There are a number of practical ways in which the Tanzanian National Health Forum can seize the opportunities for realising higher returns on investments in R&D. By implementing these strategies - which all require collaboration among its partners - the Forum will itself be strengthened as a national mechanism for maximising the social benefits of health research.

6.1 Design team-based incentives to fill investment gaps

Although financial remuneration is a compelling incentive for researchers, other forms of "psychic benefit" are important as well. In designing incentives appropriate for Tanzania, the national health research leadership should be cognisant of a range of motivating factors:

First, to a large extent, research efforts "follow the money". Re-allocating resources to fill investment gaps will stimulate researcher interest in neglected areas. A system of competitive funding (described in section 3.3) could help ensure good quality research and efficient implementation of the investment portfolio. Given the importance of donor funding, a strategic move is to get the national portfolio accepted as the research component of the health sector reform program - enabling the National Health Forum to fill current investment gaps quite rapidly. In the long term, the National Health Forum will need to decide how best to use donor funding.

- One option is to view donor funding as **line item support**. In other words, having determined the scope, scale and risk profile of the national investment portfolio, foreign funding may be used to supplement revenue from government. This is the current approach of the health sector reform program.
- A second option is to view donor funding as an opportunity to **achieve economies of scale**, not possible with limited government funds. Funding could be used to create national or regional R&D alliances, or to enable greater participation in multilateral initiatives.
- A third option is to see foreign investment as a way of **sharing risk**, which may be financial or political in nature. It may create the opportunity to adopt a "riskier" research portfolio than would otherwise be possible (UNCTAD 1990). This could support for research projects that, despite potentially high returns, failed to meet the threshold of expected benefit because they ranked poorly in terms of existing capacity. In addition, foreign investment could provide legitimacy and support to researchers who are working on projects that may be politically sensitive, such as descriptive surveys of resource allocation across districts.

These options are not mutually exclusive, but a deliberate approach may enable the National Health Forum to harness the powerful incentives of donor funding – which at present are not always oriented to maximising social benefits.

Second, R&D teams will operate most effectively if each team-member's share of collective benefits is greater than the benefit of working alone. Incentives that typically drive **individual** scientific endeavour are financial motivation, peer recognition and promotion linked to publications (Dasgupta & David 1994). Incentives driving **teamwork** include the advantages of communication and interaction with colleagues, and a sense of mutual purpose in a concerted national endeavour to improve health (Amabik 1999). By providing a combination of these incentives, the research leadership can help assure that the benefits of teamwork exceed the

gains of working alone. One strategy is to establish and give national recognition to R&D teams for each of the disease priorities identified, with members drawn from various disciplines and institutions. By way of illustration, Table 10 shows the possible composition of a national team to address the priority of acute respiratory infection (URTI + pneumonia).

Table 10: R&D team composition for acute respiratory infection

Objective of Strategy	Equity	Efficiency	Cost-effectiveness	New products & interventions
Degree of emphasis*				
Research topic**	Environment	• Case management • Risk factors • Home care	Antibiotic sensitivity	Vaccine development
Team member	Epidemiologist Community advocate	• Health systems researcher • Epidemiologist • Nursing practitioner • Medical practitioner • Health service manager	Biomedical researcher Ministry official	Biomedical researcher Foreign research partners

* See Figure 5

** See Table 6

The success of these teams depends on several factors:

- The first is the degree to which a **common interest can be crafted**. Although researchers will be working on separate aspects of the problem of acute respiratory infection, their efforts may be enhanced by interaction among each other. For example, biomedical chemists and health systems researchers need to work together to develop a streptococcal vaccine thermostable enough for situations in which the cold chain may be disrupted (Lederberg 1995). An added advantage of cross-disciplinary collaboration is that common interests may keep researchers focused on the task at hand, namely to improve health status. Without this problem focus, there is a tendency for research to drift away from its intended trajectory (McMaster *et al* 1997).
- A second criterion for success is the **ability of team members to communicate easily** with one another. The Coordinating Committee should facilitate opportunities for both formal and informal communication between team members.

These opportunities may include:

- Regular face-to-face discussion and updates on progress
- An electronic forum for team discussion
- Exchange visits by team members to their respective sites of work.

Linking team members together may provide the functional basis for plans to roll out electronic infrastructure. Over time, the Committee's role in facilitating contact should diminish as new friendships and collegiality lay the basis for continuing interaction.

- But at the same time, lessons learnt and research results should be formally synthesised so that knowledge can be shared more broadly. And a third success factor is the **production of joint outputs that receive national recognition**. For example, an annually published national review of progress with respect to each of the ten health priorities may be a good way of focusing teams on health outcomes and imbuing the teams with a sense of prestige and authority. This publication could become a flagship for the National Health Forum, describing:

- Trends in the extent and distribution of morbidity and mortality;

- New policies, service delivery strategies and patterns of resource allocation; and

- Relevant research findings with respect to each priority.

Widely distributed, it could become the standard reference for health and health care in Tanzania.[11]

- A final criterion for success is **recognition by academic institutions of non-journal publications**, such as the proposed annual review, in merit awards and promotion processes. The current emphasis on international journal publication acts as a disincentive to sharing knowledge within Tanzania, and the National Ethics Committee should work with universities to create a system of accreditation for national and regional publications that promotes both objectives of good quality research and local knowledge diffusion.

A third motivating factor for researchers is strong demand for research outputs. This is discussed more fully in the following section in the context of a recommendation to support more local problem solving, but two additional strategies to stimulate demand are mentioned here. The national health services research portfolio (Appendix B, part II) creates opportunities for new linkages between researchers, legislators and the media. For example, regular surveys of the quality of clinic and hospital care may be of considerable interest to national parliamentarians. Over time, these surveys could become an important input into budgetary allocations. In conjunction with the health ministry, the National Coordinating Committee may convene a meeting with the relevant health and finance parliamentary sub-committees to identify financial and service indicators that could serve as a gauge of progress over time. These indicators could be the basis for collaborative research, with outputs synthesised annually in time for the parliamentary vote on health sector appropriations.

Similarly, a meeting with media editors could explore ways in which survey results could receive more substantive coverage by newspapers, radio and television. This could be followed up by closer interaction between individual journalists and researchers. In this regard, the National Coordinating Committee could usefully collaborate with the two non-government organisations working to improve media coverage and the substance of journalism in Tanzania.

As “knowledge entrepreneurs”, members of the National Coordinating Committee should be constantly on the lookout for opportunities to increase social returns on investments. Yet the Committee’s current terms of reference place more emphasis on a custodial role, and maximising social benefit remains an implicit objective. The risk is that more time will be spent developing national databases than designing and implementing the type of incentives

11 The *South African Health Review* is an example of an annual review of health policy developments and trends, linked to national priorities (URL: http://www.hst.org.za/sahr/)

described above. Both are important, and the National Coordinating Committee will need to find a balance between its custodial and entrepreneurial roles. Arguably though, for a country like Tanzania with scarce resources, the greatest rewards will come from fulfilling the latter role.

6.2 Establish a national initiative for district-based problem solving

In responding to the opportunity to develop a broad-based portfolio of research aimed at solving local problems, I suggest that a new initiative be established under the auspices of the Tanzanian National Forum for Health Research. Its goal would be to support a program of problem solving in districts across the country – trying to address the factors that most affect access and quality of service delivery. In doing so, it would help fill the most obvious R&D gaps and lead to more efficient implementation of the investment portfolio expected to maximise social benefits.

As Part I illustrates, the research agendas of each district need to be customised to their specific problems and should be an integral part of the strategic plans of each district's management team. Research will then become part of the "production process", rather than a stand-alone activity within districts. This integration has implications for the way in which districts are supported. Typically, research is funded as a separate entity, distinct from efforts to facilitate implementation of its findings. When the required research is "hands-on" problem solving however, this separation becomes an obstacle to implementation. Linked to activities facilitating implementation - including technical support, better communication and access to information – research has a far better chance of achieving expected changes. Therefore I recommend that research be supported as *one* element of a multi-pronged package of support, customised to needs of each district. Practically, this may involve hiring skilled facilitators to work with one or two district management teams each, guiding the process from problem identification to implementation of research recommendations. In addition, it may be necessary to contract other technical expertise for specific tasks such as improving Integrated Management of Childhood Illness (IMCI). These recommendations for linking research with action are consistent with both current theory and practice of innovation (Pfeffer & Sutton 2000, Miller & Morris 1999), and have been shown to be valid in the context of a similar district-support program in South Africa.[12]

Obviously, there will be considerable overlap in many technical areas – improving the efficiency of drug management, for example – and the national leadership should design cross-district research activities as well. For example, an individual skilled in drug management may work with several districts in improving the efficiency of drug supply. Furthermore, it will probably not be feasible to establish research activities in every single district, and mechanisms should be established to create a "knock-on" impact across the country. "Knock-on" mechanisms may include regular publications such as "learning briefs" which reflect district experiences, cross-site visits by district teams to other see how management systems can be bettered, and regular interaction with regional and national managers.

A proactive program of district support also implies considerable changes to the way local research is nationally coordinated. For instance, a coordinated national program is consistent

12 The Initiative for Sub-District Support in South Africa is an example of a national program for district-based problem solving. All its publications are available at URL: http://www.hst.org.za/isds

with the idea of a revenue centre for local research, which pools funding from several different sources (section 3.5, Figure 6). These sources may include:

- Allocations for local research addressing **disease priorities** - channeled through the NIMR
- A substantial proportion of the **Health Research Users' Trust Fund** (retaining part for nationally initiated projects)
- Additional contributions from **donors** as part of the health sector reform program.

To be really effective this initiative requires a small but skilled secretariat, which draws on the diversity of expertise available in research and service organisations. A logical core for this secretariat is the personnel of the health systems research department of the NIMR, the Tanzanian Public Health Association and the Health Research Users' Trust Fund, as well as an appropriate person from the ministry of health (possibly through TEHIP or AMMP). If the right people are chosen to be part of this secretariat, it could become a dynamic initiative for improving health services throughout Tanzania.

6.3 Work to improve communication

A starting point for better communication among research organisations is to agree on a **few common outputs**. For example, regular learning briefs of one or two pages are an effective way of extracting the major implications of research for use by policy makers, health managers, the media and advocacy groups. Findings from R&D supported by a range of different institutions could be summarised in one easily recognisable format. Distributed to every district in the country, these briefs could promote dialogue between researchers and service providers.[13]

A second common output, already described, is an annual review of progress with respect to each priority disease that would serve to consolidate the efforts of inter-disciplinary R&D teams.

Capacity for regular publication already exists. Publication skills reside in the communications arms of NIMR, Muhimbili, Ifakara, TEHIP and AMMP, while the Tanzanian Public Health Association has a widespread network for information dissemination. Working together, these organisations could greatly enhance communication without much additional expense - managing a common web-site; publishing and disseminating information; working with researchers to improve the presentation of their outputs; and converting documentation to electronic format.

As **electronic systems** expand, resource centres will be able to publish information in a variety of formats (html, pdf and email) and to link users and researchers together in active discussion groups. However, the evolution of effective electronic networking is not easy, particularly in resource-poor environments like Tanzania. Technology users require continuing technical support to make sure that PCs and modems are working, but as importantly, they need to be assisted to use information technologies efficiently. My experience in establishing *HealthLink*, a national electronic network in South Africa, is that university-based researchers

13 This recommendation is based on the success of a learning brief series in South Africa *called Kwik-Skwiz*. Intended for busy district managers, it summarises lessons learnt through the Initiative for Sub-District Support as they arise, and is in great demand (URL: http://www.hst.org.za/isds/publications).

and doctors are accustomed to sharing information with each other and their networks quickly develop a momentum of their own. On the other hand, participation by nurses and other health workers has been harder to achieve, constrained by unfamiliarity with new technologies but also the hierarchical nature of the nursing profession which discourages questioning and challenging of authority. Providing a triad of practical information, user support and technical back up is the key to successful use of new information technologies (HealthLink 2000).

Despite new technologies, **face-to-face interaction** remains a powerful instrument for learning – enabling people to build trust and teamwork in resolving problems in common. Well-planned, purpose-specific site visits can help initiate and sustain collaboration, particularly in implementing the district support program.

All of the above strategies aim to network local people and share local knowledge. In addition, **access to international sources of information** could be readily improved. For instance, young researchers receive masters or doctoral degrees from abroad and return to research centres with very limited, outdated libraries. Introduction of a wireless telephone system to several of NIMR's centres is imminent, creating the opportunity for researchers to access store-and-forward email systems, if not on-line Internet services. A market may well exist for the beautifully preserved historical collections of medical journals in research centres, and I recommend that NIMR explore the feasibility of selling them to raise funds for state-of-the-art resource centres. Where possible, software should allow for direct access to relevant and available information rather than only providing citations, and CD-ROM technology now allows for major scientific databases to be available on site (Ngwainmbi 1999). Obviously, the reliability of the electricity supply at each centre should be taken into account, and there may still be a need for printed publications. In this regard, it may be possible to request international journals to provide free subscription to their printed and electronic outputs. The *Canadian Medical Association Journal* has set a precedent by sending copies without cost to libraries in a number of low-income countries (Haddad & Macleod 1999), while *The Lancet* has recently introduced an electronic edition to encourage participation by researchers in low-income countries.[14]

Generally though, the cost of access to international information is prohibitive, and the National Health Forum should seek **favourable countrywide licenses** for computer software and other information databases. In addition, Tanzania should actively participate in regional and international responses to modify the conditions of the Trade-related Aspects of Intellectual Property Rights (TRIPS), and should attempt to negotiate qualified exclusions from some of the most severe provisions. In this regard, it may be helpful for the National Committee for Ethics in Health Research to document examples where national scientists are constrained by an excessive and unfair burden of externally imposed costs.

"Knowledge diffusion" is now widely touted as the key to innovation and development (World Bank 1999) and the pre-eminent challenge is to develop a *culture of learning*. Simply put, this means creating an environment in which people are constantly curious, comfortable to challenge assumptions and findings, and willing to make, and learn from, their mistakes. Trust between team members is a crucial attribute, enabling people to divulge failures and share breakthroughs in thinking. Improving communication will lower transaction costs, improve efficiency of R&D implementation and help Tanzania move closer to maximising social benefits.

14 The Lancet Electronic Research Archive (URL http://www.thelancet.com/newlancet/eprint).

In summary

In response to the striking opportunities to realise better returns from the national investment portfolio for R&D, I propose the following:

First, team-based incentives should be designed to encourage researchers to fill the gaps in the current R&D portfolio. Given resource constraints, incentives will need to be a mix of financial benefit and psychic reward, but will only provide the right motivation if the individual's share of collective benefits exceeds the personal gains of working alone. This can be achieved by preserving the typical rewards of science such as peer-recognition, supplemented with other incentives such as better access to information sources through collaboration.

Second, an initiative for district-based solving will strengthen the national investment portfolio considerably, directly addressing current deficiencies in equity- and efficiency-oriented R&D.

Research should be locally initiated as part of each district's development plans, and should form part of a multi-pronged process of support to improve service delivery. Lessons learnt should be actively shared across the country, and national leadership is required to make this happen.

Third, improving communication will reduce the costs of research and promote "knowledge diffusion". A practical place to start is for the National Forum on Health Research to agree on a few common outputs, including a series of learning briefs distributed regularly to every district in the country and an annual review of progress in addressing disease priorities. A deliberate process of technical and user support is required to make the most of electronic networks and information resources, but this does not replace the value of face-to-face interaction. Taken together, these communication strategies can help promote a "culture of learning" in the Tanzanian health sector.

The cost of access to global knowledge is high, and the National Forum should work steadily to keep costs down. This includes negotiating favourable terms for software licences and attempting to moderate the isolating effects of the international agreement on Trade-related Aspects of Intellectual Property Rights.

Conclusion

The two most evocative images from my visit to Tanzania were the sight of an exhausted and emaciated man carrying his unconscious wife on his shoulders towards a district hospital, and witnessing life in a rural village dominated by poverty and malaria. For me, these images crystallise the essential question for health research in Tanzania: Can health research be justified in the face of such unmet basic need?

Certainly health R&D can't be justified on the basis of its contribution to educational & scientific capacity alone, despite the benefits of basic science research trickling down through the educational system (Garrett & Gransquist 1998). For at the margin, investments in primary and secondary education will produce higher returns (Psacharopoulos1994). Neither can health research be justified on the basis of its contribution to economic productivity, despite the "large effects" of R&D on social welfare (Temple 1999). For R&D only becomes a major factor in economic growth once a country reaches a threshold level of productivity (Birdsall & Rhee 1993). Health research in low-income countries can only be justified if it returns positive benefits to the health status of their people - and Tanzania's definition of social benefit from health research as **better health for those who need it most** is appropriate (NIMR 1999a). This is not to imply a simple deterministic link between health research and health outcomes, and uncertainty is an ever-present factor. Nevertheless, it is possible to accommodate uncertainty in a diversified national investment portfolio of health research.

In order to maximise social benefits, Tanzania must ensure that health research:

- Addresses disease priorities
- Effectively improves health - through new interventions, better use of existing ones and fairer distribution of resources.

In other words, not only must investments be aligned with national priorities, but investment must also be made in the *type* of R&D instruments expected to improve health most.

For the majority of Tanzanians, typified by peasant villagers and the exhausted man carrying his sick wife, the research that could make the biggest difference is **practical problem-solving** – helping districts get more out of their budget allocations by improving efficiency and targeting resources to those most in need. I'm not dismissive of the need for new product development and finding new ways to make efficacious interventions cost-effective, and their relative importance is well demonstrated through the national priority setting process. Tanzania needs to preserve its national capacity to use these R&D instruments. However, it is clear that the prevailing incentives of science and technology favour *these* instruments and neglect others potentially very important for health and development in Tanzania. The implication for the National Forum on Health Research is that its main task is to design incentives leading to more R&D aimed at improving equity in resource allocation and efficiency in use. Given the striking opportunities to attain higher returns from current investments in health research, there is no reason why additional incentives should jeopardise the existing capacity of any research discipline. On the contrary, better alignment of R&D with expected social benefit should in time lead to stronger demand for *every* type of research.

Can health research be justified in the face of unmet basic need? Only if it improves the health of Tanzanians, and does so efficiently and equitably.

Appendix A
Methodology for setting priorities for health research in Tanzania

1. Method Used in 1999

Final ranking of national priorities was based on the following:

- Forty-five out of 113 district medical officers (40%) responded to an *open-ended questionnaire* asking them to list the top ten: i) health problems causing morbidity and/ or mortality; ii) health system/ service problems; and iii) the five greatest socio-cultural problems of the district. It was generally agreed that the districts which responded were representative of Tanzanian geography, climate and economic status.
- Adult mortality and morbidity data from the *health management information system* (HMIS).
- Supplementary information *provided by meeting participants*.
- A *national meeting* of 45 participants drawn from the health ministry (national, regional, district managers and programme co-ordinators), non-government organisations, private institutions, research institutes and university departments, and religious groups (Muslim and Christian). This meeting divided into three groups of 15 to review available data and rank priorities. Each group chose its own criteria and system for ranking priorities.

2. Recommendations for Improving Inter-Rater Reliability in Priority Setting

The bottom-up process of priority setting in Tanzania provided a solid basis for an investment portfolio for health research. Following are options for further improving the reliability of responses at future national meetings:

2.1 Rank-order health service & socio-cultural problems at district, but not national level

While rank ordering for health service and socio-cultural problems made sense at a district level, it lost a lot of meaning in national aggregate. My suggestion is that these problems should *not* be ranked, but that district responses are used to establish:

- An agenda for research at the national level.
- A framework for a national program of district-based health systems research.

For example, national delegates may draw on responses from district officers to define a *national-level* research agenda, including:

- A survey of the distribution and infrastructural integrity of health facilities
- Representative survey of the quality of care in health facilities
- Exploration of alternative methods of financing district health services.

All of these examples have a local component, but involve countrywide surveys and nationally aggregated data, or are predominantly concerned with national policy analysis. Once a national-level research agenda has been teased out, topics can be ranked and priorities can be tailored to the budget. Many other research topics may have national implications, but are local-context specific. For example, assessing the use of trained health educators or finding ways to strengthen community participation in supplying water to clinics make a lot more sense in specific situations. Similarly, knowledge, attitude and practice (KAP) studies are often culture and community-specific. Ranking these topics with national-level research really compares apples and oranges. In Part II of the main text, I outline a process for implementing a district-level research agenda that is context-specific.

2.2 Introduce an intermediate step for ranking research topics

In order for group rankings to be aggregated, groups need to be provided with a common list of topics. Otherwise, research topics get presented in different and overlapping ways that are hardly comparable. For instance, research topics for 'pneumonia' include 'environment' (ranked sixth) and 'risk factors' (ranked second). If there were a common way of framing the research questions, these topics may well have collapsed into one. This could be achieved simply by collating all suggested research topics into a single list, prior to group ranking.

2.3 Distinguish between selection criteria that reflect potential returns and risks

Maximising expected benefits of health research may be defined as: [the returns to each project under ideal conditions] x [the probability that each study will be successfully implemented].

It should be possible to evaluate potential returns under ideal conditions of implementation and then to account for risk by discounting expected benefits. 'Risk' should be narrowly defined as the probability that the research project will fail to meet its objectives because of inadequate national capacity to undertake the project. Distinguishing between selection criteria that gauge potential returns and likely risk would allow for regular reappraisal of the research portfolio to respond to changes in national capacity to undertake specific research projects.

Appendix B
A national health research portfolio for Tanzania

This appendix is an attempt to define research questions from the topics identified by participants at the national priority-setting meeting (1999). In most instances, the questions are only best guesses at participants' intent. It not meant to be prescriptive, but to provide proposals that need to be debated, contextualised and made even more specific. It tries to make the conceptual leap from broad areas of agreement to a detailed strategy to improve health.

I. Research in Response to Priorities of Disease and Injury

Numbering corresponds with plenary (aggregated) rankings. Research topics **underlined in solid** received rankings by all three groups. Research topics in coloured text were ranked by two out of three groups. Those in **bold text** were proposed by only one of the three groups. US Dollar amounts in brackets are ballpark estimates of the financial costs of undertaking the research project.[15]

1. Malaria

1.1 Drug resistance: How can we design and/or improve the national surveillance and response system for antimalarial drug resistance. [$70 000]

[Participation in Stage III clinical trials for new drugs]

1.2 Case management: Once people are diagnosed and treated for malaria, why do they still die? How can we reduce the number of preventable deaths in clinics and hospitals? [$40 000]

Can we improve the use of first-line anti-malarials? [$20 000]

1.3 In pregnancy: Are there ways to limit the severity and reduce complications arising from malaria in pregnancy? [$30 000]

1.4 In under fives: Where are children dying from malaria? At home, before reaching health services? Or at clinics and hospitals? If the latter, why? [$70 000]

1.5 Herbal treatment: What is the efficacy of herbal treatments? Can active ingredients be identified for future drug development? [$90 000]

1.6 Vector control: Where is vector control breaking down, and how can that be remedied?

Can bednets be made cost-effective? [$50 000]

15 These ballpark estimates are based on my experience as executive director of the Health Systems Trust, the principal funder of health policy and systems research in South Africa. See costing assumptions at the end of this Appendix.

1.7 Highland malaria: What is a cost-effective approach to vector control in highland areas? [$35 000]

1.8 Health seeking behaviour: How can we reduce the average severity of infection at the time of presentation? [$40 000]

1.9 Intermittent treatment: How can we reduce the relapse rate for *Plasmodium vivax* and *ovale?* [$40 000]

1.10 Vaccine development: [Participation in international clinical trials for new drugs as part of the Multilateral Malaria Initiative]

1.11 Association with anaemia: How much anaemia is due to untreated low-grade chronic malaria? [$30 000]

1.12 Clinical vs laboratory Indices: What is the specificity and sensitivity of clinical diagnoses of malaria? When is laboratory diagnosis required? [$45 000]

1.13 Quality control: How can we reduce the rate of missed diagnosis for malaria? [$30 000]

1.14 Choice of drugs: What sentinel surveillance design would enable us to monitor changing drug sensitivities and keep regimens most cost-effective? [See 1.1]

1.15 Pharmacokinetics: [Testing drug interactions, absorption and clearance rates as part of international initiatives] [$50 000]

2. Upper respiratory tract infections

2.1 Case management: Where and why are children still dying from the complications of upper respiratory tract infections? [$40 000]

How can we improve the appropriateness of antibiotic prescription? [$25 000]

2.2 Health seeking behaviour: How can we communicate a core set of messages to caregivers that would enable them to recognise warning signs and seek attention earlier? [$25 000]

2.3 Vaccine development: [Participation in international efforts to develop vaccines for *S. pneumoniae* and *H. Influenzae (A)*]

2.4 Antibiotic sensitivity: Can we institute a system for monitoring and responding to changing antibiotic sensitivities? [$35 000, if done in conjunction with 1.1]

2.5 Predisposing factors: Can we do more to prevent complications of upper respiratory tract infections? Can we identify which children should get Vitamin A supplementation? [$30 000]

2.6 Environmental conditions: Do we know which children in communities die from complications of upper tract infection? Are they concentrated in informal settlements, and what can we do to reduce complicated infection? [$45 000]

3. Diarrhoeal disease (DD)

3.1 Vaccine development: [Participation in international efforts to develop a vaccine for Shigella dysentery].

3.2 Antibiotic sensitivity: Is *Shigella dysenteriae* still sensitive to current antibiotic regimens? If not, what is a cost-effective alternative? [$20 000]

3.3 Management of acute DD: Why are children still dying from diarrhoeal disease after reaching health services and post-admission? Can we reduce this mortality by reorganisation of triage procedures, even earlier rehydration or better management protocols? [$35 000]

3.4 Aetiology: Are we treating diarrhoeal disease efficiently? Do we know what proportion is viral and what is bacterial – and if so, are clinical management protocols being used appropriately? [$50 000]

3.5 Water and sanitation: Can we localise the source of infections in communities, as a start to obviating that risk? [This study needs to be part of local initiatives to improve health – difficult to cost. Estimate $20 000 for national co-ordination and synthesis of studies]

3.6 KAP studies of caregivers: How can we increase the use of oral rehydration solutions at home by 20% over the next year? [$25 000]

3.7 Food handling and law: Is food poisoning a significant problem? If so, are people being poisoned at home or by vendors? What can *effectively* be done to reduce the incidence of food poisoning? [$25 000]

3.8 Food hygiene inspection: Which is more cost-effective: a system of food hygiene inspection or a program for promoting food hygiene through support and education? [$35 000]

3.9 Impact of interventions: Are there local initiatives that specifically aim to reduce the incidence of acute diarrhoeal disease? Are they successful? What can we learn from them? [$25 000]

4. Pneumonia

4.1 Case management: Why do children who reach health services still die of pneumonia? Is there a delay in being seen or starting treatment (antibiotics and oxygen)? Is there a problem with appropriateness of antibiotic regimens? [$40 000]

4.2 Risk factors: Can we improve our management protocol for children at high risk (eg. malnutrition, tuberculosis, severe disease etc) [$30 000]

4.3 Vaccine development: [Participation in international efforts to develop vaccines for *S. pneumoniae* and *H. Influenzae (A)*]

4.4 Home care: Can we design a set of clinical guidelines for nurses that enable them to decide appropriately on which children can be managed at home and which require admission? [$20 000]

4.5 Antibiotic sensitivity: Do we have a good profile of antibiotic sensitivity in Tanzania, and is there a way of monitoring it? Can we improve our surveillance and response system? [See 1.1 and 2.4]

4.6 Environment: Do we know which children in communities die from pneumonia?

Can we use this information to shape our service interventions? [$45 000]

5. Intestinal worms

5.1 Environmental control: What community strategies would reduce human and animal infestation? [$35 000]

5.2 Epidemiology: Which areas of the country have the highest infestation rates? Can we map prevalence (by altitude, for instance) [$55 000]

5.3 Drug efficacy: Do current drug regimens eradicate the individual worm load? Can we achieve the same result more cheaply? [$35 000]

5.4 Child development: How can we make use of venues for childhood learning and development to extend mass deworming programs in endemic areas? [$25 000]

5.5 Nutritional: How can we integrate routine deworming with nutritional supplementation strategies? [$25 000]

5.6 Human behaviour: What strategies, at community and individual level, would reduce infestation? [$20 000]

5.7 Program evaluation: How successful are mass-deworming programs in Tanzania? How can we improve efficiency? [$60 000]

5.8 Immunology: (Research questions unclear to me)

5.9 Program replication: How can successful programs be replicated in other endemic areas? What's the cost? [$35 000]

6. Eye infections

6.1 Impact of Vitamin A supplementation: Can we demonstrate that vitamin A supplementation is cost-effective, and should be extended to other communities at high risk? [$45 000]

6.2 Sustainability of interventions: What is an affordable approach to the prevention, identification and management of eye disease in Tanzania? [$25 000]

6.3 Mapping of eye disease: Where are the biggest problems – and are our resources targeted accordingly? [$65 000]

6.4 Case finding and management: How can we improve our systems for screening, referral and case management? [$35 000]

6.5 Onchocerciasis: Are cases of onchocerciasis being recognised and treated early enough? If not, where's the problem? [$40 000]

7. Skin infections

7.1 Epidemiology: What is the prevalence and distribution of major skin infections (staphylococcus, streptococcus, leprosy, leishmaniasis etc) and chemical dermatitis? [$65 000]

7.2 Case management: Can we improve on current management protocols? [$25 000]

7.3 Chemical and detergent survey: What substances are used in chemicals and detergents in Tanzania? Do they comply with reasonable safety guidelines? [$25 000]

7.4 Community knowledge, attitudes and practices and prevention: What consistent messages can we convey to communities to prevent, recognise and seek appropriate treatment for skin diseases? [$25 000]

7.5 Environmental pollution: To what extent are environmental pollution and occupational hazards factors in the prevalence of dermatitis? [$25 000]

8. Sexually transmitted infections

8.1 Case management: How can we improve the efficiency of case detection, management and follow-up for sexually transmitted infections? [$60 000]

8.2 Drug resistance: Do we have a surveillance system for monitoring and responding to drug resistance? How can we improve it? [See 1.1, 2.4 and 4.5] [Participation in international clinical trials for new drugs]

8.3 Impact of interventions: What's working? Why? [$55 000]

8.4 Prevention and control: What local and international successes can we learn from? [$25 000]

8.5 Epidemiology: Is there a routine surveillance system in place for monitoring the prevalence of HIV/AIDS and other sexually transmitted diseases? If so, can we map the prevalence? [$50 000]

8.6 Evaluation of syndromic management by private practitioners: Have private practitioners adopted syndromic management practices? How can we improve practices? [$30 000]

8.7 Impact of community strategies: What are the gains from community-based strategies to prevent sexually transmitted infections and HIV/AIDS? Can these gains be replicated elsewhere? [$35 000]

8.8 Health seeking behaviour: How can we make our health services more accessible and user friendly to encourage early presentation? [$30 000]

9. Anaemia

9.1 In pregnancy: Is routine folate supplementation cost-effective? [$35 000]

9.2 Epidemiology: Which people are at most risk for pathological anaemia, and what can we do to reduce the risk? [$40 000]

9.3 Impact of interventions: Are diagnostic and management protocols for anaemia in place and working? [$25 000]

9.4 Worms and malaria: See 9.1 and 9.2

9.5 Nutrition: Is broad-based iron and folate supplementation and/or fortification of foodstuffs a cost-effective option? [$35 000]

10. Trauma and accidents

10.1 Epidemiology: What are the major causes of trauma and injury, and what are the major risk factors? Who gets injured? [$60 000]

II. National Research in Response to Service Delivery Problems

These research projects are presented in terms of rank order of the health service problem they seek to address.

1. National human resource review[16]

- ❑ Spatial distribution of main staff categories per 10 000 population (by region and district)
- ❑ Distribution of health workers by level of care (by region and district) [$65 000]

2a. National clinic sample survey[17]

Representative survey of clinics to assess:

- ❑ Adequacy of water, sewerage and communication systems
- ❑ Rating of building maintenance
- ❑ Adequacy of basic equipment and sentinel drugs [$70 000]

2b. National hospital survey[18]

Survey of all hospitals to assess:

- ❑ Adequacy of basic equipment and sentinel drugs
- ❑ Adequacy of water, sewerage and communication systems
- ❑ Rating of building maintenance [$70 000]

Note the ranking of a hospital survey that excludes the first objective drops to 7th or 8th.

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16 An analytical framework for mapping and documenting the distribution of personnel and health facilities is outlined in a series of reports (http://www.hst.org.za/pubs/rehmis.htm)

17 A useful reference in this regard are the annual South African "Equity Gauge" (http://www.hst.org.za/hlink/equity.htm)

18 For detailed methodology, see reference (CSIR 1996). The framework for this audit is described in Chapter 13 of the South African Health Review 1997, Health Systems Trust. (URL: http://www.healthlink.org.za/sahr)

2c. Review of national drug management system

- ❑ All aspects of procurement, supply and monitoring of national system (linked to district level reviews) [$35 000]

3. Review of national guidelines for transport management [$15 000]

4a. Costing of health services

- ❑ By level of care [$35 000]

4b. Development of guidelines for allocating finances

- ❑ Among service functions and service levels [$15 000]

5. Evaluation of national information and communication strategies

- ❑ Evaluation of impact of health messages conveyed through mass media [$55 000]

6. Design of a multisectoral transport initiative

- ❑ Planning and action to improve road and communication access to health facilities [$40 000]

7. Evaluation of coverage and efficiency of existing water supply initiative

- ❑ (May be linked to facilities' surveys) [$25 000]

8. Review of health facilities coverage

- ❑ Spatial distribution of clinics per 10 000 population (assuming that every district has a district hospital) [$35 000]

III. National Research in Response to Socio-Cultural Determinants of Health

1. Descriptive study of polygamy practices

- ❑ Description and mapping the practice of polygamy in Tanzania [$45 000]

2. Impact assessment of political and economic reforms

- ❑ Impact on funding and provision of social services [$40 000]

3. Regression analysis: Impact of gender inequality on health status

- ❑ Correlation between gender inequality (income, education) and health status in communities [$30 000]

4. Descriptive study of early marriage practices

- ❑ Knowledge, attitude and practice study on early marriages with a view to law reform [$35 000]

5. Descriptive study: Extent of witchcraft

- ❑ Assessment of the extent of witchcraft in Tanzania [$60 000]

6. Descriptive study: Widow inheritance

- ❑ KAP study on widow inheritance among various tribes [$40 000]

7. Descriptive study: Use of indigenous herbs

- ❑ Identification and mapping of local herb use [$40 000]

8. Assessing effectiveness of local herbs

- ❑ Public, individual and clinical perceptions of effectiveness [$30 000]
- ❑ Pharmacological studies of herbal compounds [$50 000 upwards]

IV. Local Research to Address Service Delivery Problems and Socio-Cultural Priorities

Research that addresses local health service problems and the socio-cultural determinants of health is context-specific and cannot be described in any more detail than is presented in Tables 7 and 8 of the main text. Nevertheless, locally initiated research is as important - if not more - as national research in improving health and should receive an appropriate share of investments.

Typically, a local research agenda will emerge from an analysis of local problems by the district management team or other local development organisations. Part II (main text) suggests a national mechanism for supporting local research, and enabling results and lessons to be shared.

Assumptions in costing direct expenses for research

- ❑ Costs for indirect expenses are not included
- ❑ Researcher time can be split between projects (i.e. costs reflect proportion of full-time equivalents)
- ❑ All direct costs associated with vaccine development would come from external sources (i.e. not Tanzanian taxpayers)
- ❑ Almost all projects can be efficiently completed in a year

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