

“TARGETED RESEARCH”: AN OXYMORON THAT NEEDS TO BE DISCUSSED*

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It has been a great honor to represent you during this last year, and it is a great challenge to stand before you now. Table 1 presents some of the multiple titles I have considered for this presentation over the last 10 months. I had a difficult time, in early August, when I had to select a final topic and fax my actual title to the Scientific Program Chair, Bill Petri. I originally thought I would speak on a main research theme of my laboratory and colleagues over the last 10 or 12 years, to present a scientific synthesis of a continuum of studies. Bill Petri strongly encouraged me to use the Presidential Address as a scientific forum.

You can see from Table 1 the progression was from straight science, to representative science and why we should do it, to a discourse on why it makes sense to use national monies to do science on international topics, to different perspectives within our society. However, when I actually had to get my title in to Bill, I decided not to talk about idiotypes, but rather to talk about the terms basic research, strategic research, targeted research, and applied research.

Most of you know these terms well, so why should I bore you with what they mean to me? Currently, these different types of research are being bandied around a lot by special interest groups, Congress, and the Executive Branch, and some very critical decisions are being made, based on these terms. Different peoples' concepts of what these terms mean are going to have profound effects on how biomedical research gets done. I think we ignore the current usages of these terms at the risk of great peril, both to our discipline, and more importantly, to the goals of our discipline.

As you can see in any recent issue of *Science* or *Nature*, there is, currently, a lot of talk about basic research versus targeted research versus

applied research. This waxes and wanes, but usually accompanies changing times and tight financial times, when hard decisions must be made concerning where to put increasingly scarce research monies. Also, these same terms and concepts are at the heart of an age-old topic of many Presidential Addresses presented to this Society over the last 20 years, that is, the basic versus applied or bench-to-field polemic, and I love a good tradition.

In this address, I would like to define these terms, show how they relate to each other, and address potential problems associated with them. Then, I will propose a new way of codifying an approach to dealing with these terms in what may be a mildly heretical, but productive way. I think we can satisfy public policy on research, and at the same time achieve the transcription of basic science into product development, and the translation of tools into control programs.

What is research? Research is the careful, systematic, diligent, protracted study and investigation of some field of knowledge, which is undertaken to discover or establish facts or principles. Research is studious inquiry. Basic (fundamental, essential) research is a study into the essence of some field of knowledge. Basic research asks how things work. To me, strategic (planned, directed) research is a subcategory between basic and applied research. Strategic research is intended to achieve a stated goal, but is done with the understanding that much remains to be learned before that goal is likely to be attained. My definition of targeted (objective- or goal-oriented) research is a study that is focused on a specific objective, and only seeks to gain that objective. There are, however, many current definitions of this particular figure of speech, and we will get back to this later. I consider applied (utilized, practiced) research to be studies that involve the implementation and evaluation of information in actual practice.

If you will then, research is something you do, and science is the information you get, if you

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TABLE 1
Chronology of possible titles for the Presidential Address

Month	Possible title
January	Anti-idiotypic interactions as a basis for chronicity
February	Anti-idiotypic interactions as a basis for chronicity
March	Anti-idiotypic interactions as a basis for chronicity
April	Anti-idiotypic interactions as a basis for chronicity
May	Parasitic infections: Bonanzas for biomedical research
June	4235 Providence Lane, Tucker, Georgia 30084
June	Organisms that stretch the envelope of host-parasite relationships, offer exceptional insights
July	Global villages, and our own backyard
July	What have you done for me lately? Our domestic agenda
August	The world's policeman should worry about the world's health
August	Science, medicine, and public health: Nobody ever said triangular relationships were easy
August	Research and control: Fostering productive nondirectionality in a melting pot of science, medicine, and public health
August	Research and control: A conceptual continuum faces reality

do research right. Since you can almost always learn more about something, as long as you are careful and evaluate what you do, implementation of the information learned can also be considered research. Here we start getting into an area with blurred edges that often runs to a continuum of these types of research. One of the points I want to make is that the classification of these types of research along this spectrum should be based on both the types of work being done, and its goals. It is essential to define the immediate (or short-term) versus long-range goals of the different types of research. In this presentation, I want to emphasize that I am specifically addressing only biomedical science.

I think the overall, long-range goal of biomedical research is to improve or maintain health. For some it is focused on the health of individuals (medicine), and for some this means the health of populations (public health). Because of what we do, this most often involves

the health of those persons who live in the tropics, or who have parasitic or arthropod-borne diseases. It would seem that our daily work stands the best chance to achieve this lofty, long-range goal through a series of interim phases. Those phases are depicted in Figure 1 as Discovery, Development, and Deployment. They are what happens when you do basic research, targeted research, and applied research, respectively. Here they are presented as overlapping, in motion along a progression in time. This is how most people see biomedical research, and it is not illogical to think this way. Yet, in Figure 2 and some later figures, I represent these same concepts as Venn diagrams. I think this may better portray reality. This shows the three types of research as coexisting and overlapping in time, without a clear progression. This seems to be especially true currently, when we have such an information explosion, with rapid progress being made all the time. Later, I will try to make a case for their coexistence in both time and space. First, I would like to talk about each type of research, and then come back and try to fit them together into a planned approach.

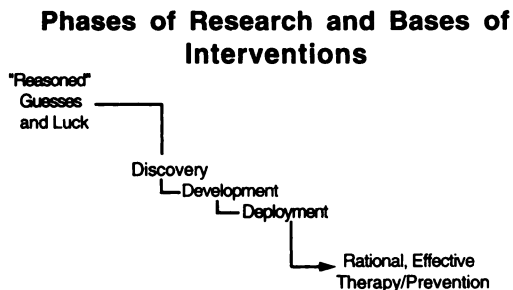


FIGURE 1. Schematic presentation of the three phases of research as an overlapping continuum between two ends of the intervention spectrum.

BASIC RESEARCH, OR DISCOVERY

The role of this level of research is to learn new things, and to combine the knowledge gained into new understandings about fundamental processes. It is inherently a non-directional process. The non-directional, random pursuit of knowledge is the essence of basic research, but it is also what gets us into trouble the quickest with policy and the public. When

Interactions Among Types of Research

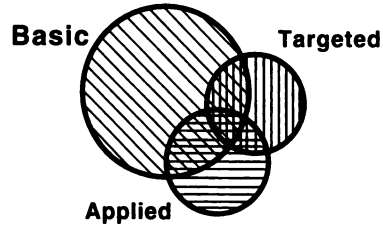


FIGURE 2. Schematic presentation of the three types of research occurring simultaneously in time, as overlapping, nondirectional facets of a program that is primarily in its discovery phase. Presented as a Venn diagram.

we extol this virtue of basic research, most people see the Hollywood version of a mad (or at the very least, eccentric) scientist throwing together whatever is handy, and poof, out comes a time machine, or a magic potion. This misconception can only be addressed by an educational process that we need to be willing to undertake.

We know that following knowledge where it leads does not mean casting around willy-nilly. It means that your mind-set is not set. It means you do rational experiments, try things based on previous findings, and plan real studies, but also that you are ready to follow bizarre leads. You need to be ready to do so, because you usually do not know what the questions should be, much less what the answers should be. Good basic research draws on clear hypotheses, straight-forward experimental methodologies, and understandable interpretations. Furthermore, we all know there are many existing checks and balances on the quality and randomness of modern-

day, basic, biomedical research. Peer review and the funding climate, combine to make sure this is not a laissez-faire business. The rub comes when this predetermines how and what questions are being asked at the level of basic research.

I would like to take a few minutes, to address one of the real advantages we have because we do basic research on some terrific host/parasite relationships. These systems are proving to be great windows into how things work, that is, they are fast becoming popular biological systems in which to look for fundamental principles and mechanisms in biology. This not only leads to new information on the diseases we care about most, it also contributes to basic biology, and makes our parasites more likely to achieve household word status, at least among biomedical scientists. I think we should all promote our systems much more strongly in terms of their ability to contribute to broad-based, fundamental biomedical science. Allow me to give you some examples of this. Table 2 lists a few findings of fundamental importance that have been shown first or most easily using parasite infections as model systems. The last one listed (cross-reactive, regulatory idiotypes in schistosomiasis and Chagas' disease) is not exactly widely accepted, in the same manner as the others, but please indulge me.

Why do we find these fundamental, new mechanisms so obviously displayed in these particular host/parasite relationships? I think it is because we are dealing with interactions that have taken both partners to the limit. These stand-offs have exploited all possible biological systems of each partner. What we see (in any given combination), is what has (so far) worked best for each.

TABLE 2
A few prominent recent contributions from host/parasite basic research

Field	Finding	Disease
Molecular Biology	Mechanism of antigenic variation	African trypanosomiasis
	Posttranscriptional RNA editing	African trypanosomiasis
Biochemistry	GPI*, lipid-anchored proteins	African trypanosomiasis Leishmaniasis
Immunology	Th1/Th2 CD4 lymphokines and cytokine profiles	Leishmaniasis Schistosomiasis
	Dominant cross-reactive idiotypes and neonatal repertoire alterations	Schistosomiasis Chagas' disease

* GPI = glycosyl phosphatidyl inositol.

TABLE 3
Some oxymorons to take along

Jumbo shrimp
Pretty ugly
Military intelligence (from the 1960s and 1970s)
Business ethics (from the 1980s)
Moral turpitude (from the time of Eden)
Deafening silence
Guest host
Bad grammar
Standard deviation
Subjective scientist
Enough money
Adequate budget
Targeted basic research

Automobile manufacturers do not sponsor their cars in the world of racing just to race. Nor is it just to promote their cars. They do it to push their products to their limits, to discover and use what works best for a particular purpose. The purpose might be speed, or endurance, or even (dare we hope) fuel efficiency. Evolutionarily, when parasites push themselves to the limits of host systems, they probably push until something breaks, or retaliates. In most cases, the confrontation we study represents the current status of a very long coexistence of trial and error. Whatever the basis, the wonders of parasite-related research have become more and more apparent, to a wider audience, by their contributions to basic biomedical science.

In the reverse, what we see as emerging infections result from recent encounters of two systems. Neither has had time, or reason, to adjust to the other, and we usually see it as an explosive clinical event, with an acute outcome.

TARGETED RESEARCH, OR DEVELOPMENT

I will not dwell on the term strategic research, which I consider somewhat more focused than basic research, and less demarcated than targeted research. I will, therefore, go on to targeted research. Please remember that my definition of targeted research is an investigation that is focused on a specific objective. I will argue that this is a very good thing, when it is employed in a well-defined manner. However, in the title of my address I called targeted research an oxymoron. My point is that this term is currently most often used to describe targeted basic re-

TABLE 4
Perspectives on targeting basic research

Case for earlier, tighter focusing (targeting) of basic research
1) Curtail meaningless meandering
2) Limit tangential forays
3) Will get you more rapidly where you want to go
4) Will cost less money
Case against earlier, tighter focusing (targeting) of basic research
1) Curtail crucial meandering and knowledge
2) Limit tangential spin-offs
3) May not get you where you want to go
4) Will cost more money

search, which I definitely consider to be an oxymoron. An oxymoron is a figure of speech in which opposite or contradictory ideas or terms are combined. Some examples of more accepted oxymorons, such as jumbo shrimp and deafening silence, are given in Table 3. You can see that some oxymorons are relatively time-dated, and there are some that can be readily understood by scientists.

Table 4 shows some of the opposing arguments for and against targeted basic research, or focusing research efforts very early in the process, prior to a clear definition of specific objectives. If these two arguments sound similar, they should. They are just two different perspectives on the same point. One states that without focus you never get anywhere, and at best you take much longer than you should to get somewhere. The other view also says you may never get anywhere, and if you do, it may not be where you need to be. These opposing views of the same object can only be reconciled through education that eventually leads to an appreciation of the other viewpoint.

As commonly used today, the term targeted research is a figure of speech primarily referring to targeted *basic* research, or "go find something that will solve my problem." It is most often used to focus on a given disease, but it is used with the intention of solving what is usually a very complex problem. It is a mechanism to justify putting monies into studies to solve a *particular* problem, but not a *defined* problem. Herein lies my difficulty with the current usage. The difficulty arises because the level of definition is not defined.

If you focus sufficient funds on a given topic

you can create interest in working on that topic. For well-defined questions *within* a broad topic, this can work very well. For the broader, lesser understood questions in an area, it usually does not yield quick, applicable results. It can shift some discovery efforts toward finding out things that may pertain to the overall topic, but when funding is tightly restricted within such an area, this amounts to a brute force, or *throw money at it* approach to things. I believe this is a very expensive way to do this kind of business, and also leads to unrealistic expectations by administrators and the lay public.

It is understandable that public outcry can influence policy decisions. Public officials can feel good about allocating monies based on the anxiety produced by vocal special interest groups, and even more so by responding positively to broad-based, special interest groups. Donor agencies and countries also have a right to expect progress toward a goal when they invest in programs and commit their funds to help achieve that goal. Nevertheless, if this targeting is done in a tightly restrictive manner, too soon, I do not believe that it serves the best interests of the interest groups, and it should be our obligation to show them why.

If funding is going to be more and more designated by disease, or by given objectives related to certain aspects of a disease, and it may well be, we will greatly need to increase our efforts to educate public officials, the public, and donors about the nature of research. Unfortunately, for several reasons, this is not easy. There is often an invisible barrier erected around us, as soon as we seek to do so. Much of this is our inability or unwillingness, to express ourselves in everyday language to lay people. Also, when we try to do so, it is usually to secure more funding. This always raises the specter of vested interest, which is very difficult to overcome. Nevertheless, we must get better at educating the public in regard to how research works, and perhaps we can do so by focusing on the different types of research I have been discussing.

Targeted research, by my positive (non-oxymoron) definition, is clinically relevant, medically-related, or public health-related research, but it is reserved for well-defined topics that are portions of more complex problems. It is an essential part of pushing ahead to attain the long-range goals in any program. It is crucial to stimulate highly focused, targeted research on topics

that are understood well enough to be likely to benefit from such an approach. However, to be cost-effective, it must rely on the nearest things we have to another oxymoron, the sure bet.

The type of effort I am calling targeted research is tailor-made for product development, tool design, and process evaluation. This involves developing new methodologies, techniques, and organizational approaches. As the World Health Organization/Tropical Diseases Research Program (WHO/TDR) has done, I think separate monies need to be available for investigators who wish to pursue this phase of the work. When evidence indicates the likelihood of success in a given area, incentives will greatly assist forward progress. Funds for this type of targeted research, or to use the WHO/TDR equivalent name "Product Research and Development," should be made available through set-aside monies. I think their availability should be widely publicized, but their allocation carefully scrutinized. If no obvious products are ready to be produced, the set-aside funds should be reallocated to allow the funding of more diverse basic research projects, which in turn can generate evidence for future product development projects.

There is a related area that I think will become more and more essential in areas that deal with orphan diseases, such as many of those on which we focus. This is the need to ensure and facilitate interactions between academic/government scientists, and industrial scientists. Effective associations between these groups could play a major role in the proficiency with which we move a product forward, but such affiliations are not yet common. Nevertheless, they may well provide the most efficacious avenues for forward progress in some areas, and we should work harder at creating such opportunities.

APPLIED RESEARCH, OR DEPLOYMENT

Applied research, or in WHO/TDR terminology, Applied Field Research, refers to the initial deployment stages of the process. This means the utilization, or putting into practice, of available interventions, based on what has been learned about a disease, its transmission, and its medical or public health implications. I would argue that this is still very much research, if an evaluation component is built into the program. In terms of achieving the long-range goals of



FIGURE 3. The beginning stages of operation "Stumps With Effort And Tools" (SWEAT). Presentation of the tools available at the beginning of SWEAT, which were deemed capable of accomplishing the task, if SWEAT were given a sufficient level of commitment.

most biomedical research, this is an absolutely essential part of the effort. In this area, I strongly agree with the New Targets and Management Structure of the newly designed WHO/TDR. Applied research must take place as soon as suitable tools are available. Always, the central question is "When is it time?" This, again, comes back to balancing the use of a brute force approach (often expensive and never ending), against the availability of cost-effective and sustainable methodologies. Figure 3 provides an object lesson on the appropriateness of available tools for a given job.

Sometimes, the tools available are sufficient, if the will is strong enough, and the decision to go ahead with those tools is also tempered by a perceived inability to get other tools within a reasonable period of time, or for a reasonable price. So you go ahead with your entrenching tool and axe, and work at getting out the stump of a fallen pine tree, if you live, for instance, in north Georgia. This case pretty much represents

the brute force approach, using available, cheap, but suboptimal tools. The methodology depends on input that may, or may not, be sustainable, depending on who is trying to sustain it.

The decision to move into applied research and control efforts also involves the murky arena of policy decisions. Nevertheless, I believe that many of those of us who think of ourselves as basic scientists need to remain in the game well into this phase. Although this is not often done, I think it should be. We owe it to ourselves to gain the satisfaction of seeing things through (even if we are not the ones doing it), and we owe it to our areas of interest to offer our insights and perspectives on the more public health aspects of our field. Interactions between these camps of our overall discipline are not always predictable, but can lead to enlightenment on both sides. They can also lead to renewed insights into one's own area of expertise.

Figure 4 depicts that by the end of a successful implementation phase we sometimes dupli-



FIGURE 4. Approaching the consolidation and certification stages of "Stumps With Effort And Tools" (SWEAT). Presentation of the tools finally committed to SWEAT. Note the duplication of some tools, and the loss of efficacy of some tools during the campaign.

cate our available tools, and some of the tools we relied on break, or become outmoded. Still, there is an obvious satisfaction associated with the productive use of tools to accomplish an obvious task. Figure 5 tries to make the point that we cannot afford to become complacent in such struggles, or a renewed effort may be necessary. Vigilance is the price of freedom.

The decisions to go forward into applied research, and eventually on to real control efforts, must be made on a case-by-case basis. In each instance, determinations must be made based on public policy, the degree of disease burden inflicted, the level of commitment, and the amount of effort (both human and financial) needed in relationship to the potential for realization of the long-term goal. Table 5 lists what seem to me to be the decisions that need to go into the determination to move ahead into a deployment phase. The International Task Force for Disease Eradication, under the auspices of the Carter Center of Emory University, and Global 2000 of

the Carter Center, has a set of criteria to evaluate the potential eradicability, or elimination of transmission or morbidity, of a given disease¹ that is quite similar in intent, to mine.

In our field, we have some excellent examples of decisions that were made to go ahead, and are succeeding. Dracunculiasis is being attacked through a multilevel eradication program involving village-based health workers, health education, chemical control of the copepod vectors, water filtration, monthly case reporting and case containment, and safe water supplies. As can be seen in Figures 6 and 7, kindly provided to me by Ernesto Ruiz-Tiben from the *Guinea Worm Wrap Up* (unpublished data), Ghana is making spectacular progress in the elimination of Guinea worm. Based on annual data (Figure 6), the number of people being debilitated by the fiery serpent in Ghana have been declining steadily over the last five years, and this is true for essentially all of the 18 countries with active Guinea worm transmission. This extraordinary



FIGURE 5. Evidence that the obvious success of operation "Stumps With Effort And Tools" (SWEAT) must not lead to complacency. There is a continued need for constant, efficacious surveillance systems, and the ability to respond swiftly and appropriately to new threats, or be faced with new SWEATs.

achievement is due to an extremely active campaign being waged by members of our Society and tens of thousands of their colleagues, and is recorded by the monthly case reporting (Figure 7) that is an essential component of this program. This effort is moving toward its goal of Guinea worm eradication by the end of 1995.

TABLE 5

The decision to commit to applied research and deployment

We must ask, and continuously ask again, the following questions for each disease, and even for the same disease in different endemic areas

- 1) Do the available tools work?
- 2) Are the available tools efficacious enough to yield a favorable cost/benefit ratio?
- 3) Is the effort to employ the available tools sustainable? Vertically? Horizontally?
- 4) Is the commitment to the effort sufficient?
- 5) What is the likelihood of developing more efficacious tools? When?

The available methods and tools are sufficient to do this. In a few minutes this Society will be proud to inaugurate the awarding of its Certificates of Recognition to the industrial and operational partners who are providing these tools.

I will not describe the efforts to control mor-

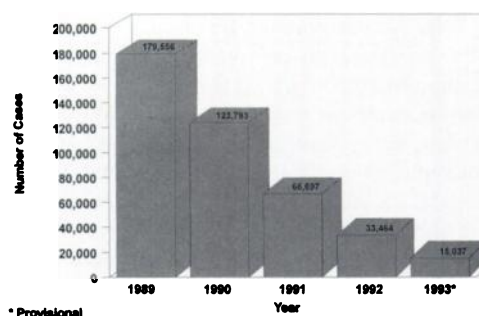


FIGURE 6. Annual number of reported cases of dracunculiasis in Ghana over the last five years in the face of intense efforts to report and eradicate this debilitating disease (source: Ernesto Ruiz-Tiben).

**Ghana Guinea Worm Eradication Program
Number Of Cases Of Dracunculiasis Reported By Month**

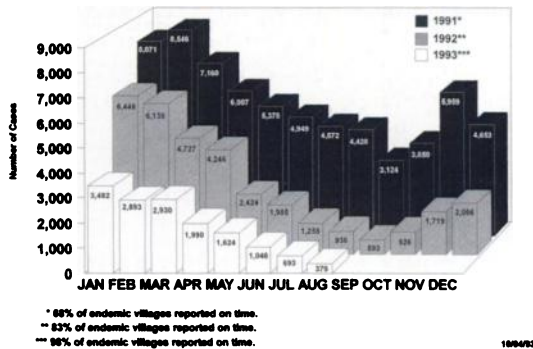


FIGURE 7. Actual number of cases of dracunculiasis is reported in Ghana by month, during the last three years, also showing the increasing percentage of endemic villages that made their monthly reports on time (source: Ernesto Ruiz-Tiben).

bidity associated with onchocerciasis, which rests both on the Onchocerciasis Control Program, and the Mectizan donation program of another of our awardees later today. It, too, is an excellent example of what I am describing as applied research, or the deployment phase.

THE PROBLEM

I have stated and integrated the three D's, Discovery, Development, and Deployment, and the three types of research with which they correlate. I have given them my definitions, and shown how important each is in trying to achieve the overall goal of biomedical science. So, is there a problem, and if so, what is it? There are two broad indications that there is a problem, or that there is perceived to be a problem. The first comes from within our Society. As I mentioned early on, it has become an almost perennial portion of Presidential Addresses to debate, in some form, the dichotomous argument of bench versus field, or basic versus applied, or research versus public health. Usually, we give ourselves strong marks for our research (meaning basic science) and weak marks (or worse) on our efforts to carry things further (meaning expanding into the realm of applied research and control programs). Maybe its only presidents of this Society that think there is a problem, but even that could be seen as a problem. The second indication that there is a prob-

lem is actually why I chose to address this issue. One only need look at the recent WHO/TDR reorganization, done to address this problem, or pick up any weekly scientific journal or even the local newspapers, to see the desire of special interest groups and donors to have their goals addressed. Each year larger and larger proportions of the budgets of agencies that deal with biomedical research are targeted. It seems fair to say there is already a problem, and that it is growing.

THE PLAN

I think that the progressive goals that we all seek are to do good basic research, to transcribe those findings into appropriate concepts and tools, and to translate those insights and tools into practical use. I have tried to make the case against inappropriate targeting of funds that seeks to target basic research. We need a policy that addresses why this does not work, even though we, the public and the donors all want it to. What sort of structure might encourage this? If we start with good definitions of what is meant, and a good understanding of what is expected, I think we can begin to deal with this issue.

To start with, I think we must ensure that those engaged in Discovery, Deployment, and Development work together, side by side. If we are to capitalize on the full spectrum of research, and thereby facilitate the achievement of the long-range goals of biomedical research, the disciplines that tend to focus on the different types of research, or the 3D's if you will, need very much to nurture continual rapport. We need to foster the flow of information and mutual respect from one to the other. This comes from working together.

I should emphasize at this point that I am not speaking as your official representative, neither am I speaking as an employee of the federal government. My opinions do not necessarily reflect those of my current or past employers, nor of this Society. They are my opinions, and if you do not think them worthy of more consideration, they may very well stay just that. I am speaking as an individual. As such, it is my intention to make a proposal that will engender discussion of whether we should promote an overt change in the perspectives underlying the current federal funding system. My proposal will require agree-

ment that there are different types, or stages, of research, definition of what is expected from each type of research, and determination of what we think each federal agency should be doing in regard to these types of research.

The first part of my proposal is in direct conflict with what I call the "Separatist Theory of Research Agencies." This is when the executive and/or legislative branches of our government stridently call for assurances that no two governmental agencies are doing the same thing. The periodic resurrection of this cry as a national policy makes me think that what we do is being seen through the eyes of Henry Ford. The types of research I have discussed are being categorized as parts of an assembly line. Each investigator is expected to add on his or her interchangeable piece of information, until a product emerges at the other end of the line, and is used. If this was the way research worked, it would make sense that no two agencies should be paid to put on the same part.

However, my main premise today is that to achieve the long-range goal, to facilitate the most timely and cost-efficient progression from basic-to-applied field research, we must strive to be certain that the different types of research are integrated whenever and wherever possible. It is logical, from an assembly line mind-set, to think that duplication and overlap are bad, but if one considers the real nature of research and those who do it, it is wholly counterproductive to separate the different overlapping phases of the process. To do so almost ensures that you would only achieve the goal with the utmost difficulties and delays. Rather, I would like to make a very strong case for the tripartite necessity, the essential need, to have these types of research coexist, to abide jointly in both time and place.

It is, of course, true that different federal agencies do have different primary roles to play. Otherwise there would be just one big agency, and I would not have needed to uproot my family and laboratory and move to Atlanta last December. I think the second part of my proposal acknowledges the differences in the missions of different agencies, addresses the Separatists' concerns over these different roles, and still provides for the interchange that I think is essential if we are to facilitate, rather than hinder, the basic-to-applied transition that is critical to achievement of our long-range goals.

We can do this by understanding the roles of

TABLE 6
Proportional biomedical science funding (intramural and extramural) by Public Health Service agencies for achievement of long-range goals

Type of research	Agencies*					
	NIH		CDC		FDA	
	Intra	Extra	Intra	Extra	Intra	Extra
Basic	75%	75%	35%	0%	15%	0%
Targeted	20%	15%	50%	30%	30%	15%
Applied	5%	10%	15%	70%	55%	85%

* NIH = National Institutes of Health; CDC = Centers for Disease Control and Prevention; FDA = Food and Drug Administration.

the different types of research, and funding them all in each institution, but doing so in proportion to the roles of the agencies. Table 6 presents subjective examples for Public Health Service agencies. Obviously, real decisions regarding actual allotments of the types of research for each agency would engender much debate. I believe that once determined, adherence to the set percentages, and allowing the state-of-the-art to set goals within those percentages, would greatly assist in achieving the research goals of all agencies. This type of proportional solution also would be applicable to both intramural and extramural funding. Along with this approach, it would be essential that the agencies also coordinate among themselves, regarding research progress and areas of emphasis. This example deals only with Public Health Service agencies, but also could be applied to other research funding components of the federal system, the Department of Defense, the Department of State, etc., and could even be self-applied to non-governmental organizations and philanthropic foundations.

I suggest that we think of this approach like the composition of a stock portfolio. Some parts are for growth, some for income, and some for security. Different mixes of these types of investments are often recommended, depending on the amount available to invest, and the investor's stage of life. It is critical to attain the right individual mix for each person. I propose that the same would be true in this system for each agency.

Figure 8 shows what the data for the intramural National Institutes of Health (NIH) program in Table 6 might look like in a Venn diagram. Figure 9 does the same for the Centers for Disease Control and Prevention (CDC) intramural program. Extramural portions of each

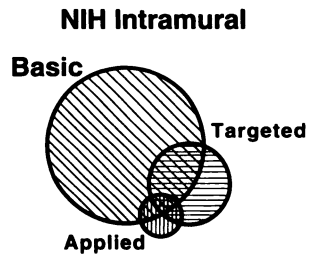


FIGURE 8. Proposed percentages (see Table 6) of personnel/money to be used for the three different types of research in the overall National Institutes of Health (NIH) Intramural Biomedical Science program. Presented as a Venn diagram.

budget might be somewhat more difficult to control in terms of percentages, because for some agencies this is (and absolutely should remain) primarily investigator initiated. In these cases, categories of grants that emphasize different types of research could be set up. Alternatively, if it became obvious that more good grants were needed in a given category, they could get a category increase in priority score. The actual percentages allocated to given types of research could be set by the Councils of NIH institutes or the Board of Scientific Councilors of CDC Centers, and they would obviously be subject to review by Congress and the Executive Branch. There would always be some judgment calls needed at the borders of the research categories, but that is already done in regard to grants versus contracts, or investigator-initiated versus requests for applications. The advantage of this system would be that it acknowledges why you are doing this, and therefore lays the foundation for making allocations based on both policy and the state of our understanding. I must reiterate that it is absolutely essential that for any such approach to work, we all need to take educating the public much more seriously. We obviously like what we do, and we think it is important. It remains a puzzle to me why we cannot seem to communicate that enthusiasm and feeling for what we do. It is an enigma we must solve to make this, or almost any other system, succeed.

This presentation on different types of research has led me to consider that the American Society of Tropical Medicine and Hygiene is in a unique situation in regard to this three D approach to biomedical research. Our members are equipped better than those of most biomedical societies to participate in fostering all three of

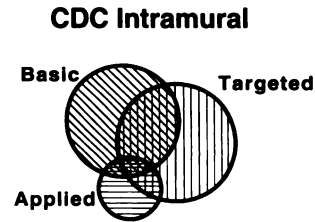


FIGURE 9. Proposed percentages (see Table 6) of personnel/money to be used for the three different types of research in the overall Centers for Disease Control and Prevention (CDC) Intramural Biomedical Science program. Presented as a Venn diagram.

the D's. We are clearly already involved in Discovery, Development, and Deployment. We are well-girded to participate in making the difficult decisions of when and how to merge from one D to the next. In part this is because we have so many members that already do more than one D. It is also due to the fact that we meet together, bonded through joint interests and passions for the wider aspects of our disciplines. As Franz von Lichtenberg said in his Presidential Address, "Our meeting is the focal point; Where all these disciplines are joined..."²

CONCLUSION

I would like to take just a minute to thank my wife, Mary Paxton and my son Tom, as well as Judith O'Connell and George Freeman, for their continuous support and understanding, and my previous and current employers and colleagues at Vanderbilt University and Meharry Medical College, the Veterans Administration Medical Center in Nashville, and the CDC, as well as the National Institute for Allergy and Infectious Diseases, WHO/TDR, and Edna McConnell Clark and Rockefeller Foundations for support and criticism through the years. I also thank Ginny Secor for preparing the slides for this presentation, and I am grateful to all of you for the high privilege of representing you since last November.

Now, I would like to end with a haiku that I hope is appropriate. It is not unique to finish one's Presidential Address with a poem. Almost seven years ago, at our Society's 35th Annual Meeting (also held jointly with ASP, but not as fully integrated as this marvelous meeting), Franz von Lichtenberg concluded his address with a poem that encompassed the wide variety,

the very diversity, of our Society.² I apologize to Franz for paying him the most sincere form of flattery, but I too will end with a poem. Thus, while I still strive to achieve the scientific honor of being mentioned in the same sentence with Dr. von Lichtenberg, I will at least go down in the Society's archives in the same paragraph that notes those who were inured to their colleagues' barbs sufficiently to recite their poetry in public.

Harvest Reflections

Russet leaves drift down
 Giving rise to swirling thoughts
 To be shared with friends
 Arigato Gozaimasu

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