would be an error to believe that serendipity, a precious gift, may be developed in the laboratories and in the libraries as a tool for research.

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To Remer, an attorney, goes the last word in these columns on this subject, at least for a while.—ED.

Basic Research on a Problem

Why does Norman Storer [*Science* **142**, 464 (25 Oct. 1963)] think that basic science is threatened by a "powerful imposter, 'basic research *on a problem*' "? (Italics are Storer's.) "Basic research on a problem" may be a contradiction in terms when applied to the physical sciences, where basic dimensions are well established, interactions among dimensions are minimal, and, therefore, completely adequate research designs can be prosecuted. In contrast, in many areas of the life sciences one must assume that there may be hundreds of variables which determine a particular phenomenon, and that many of these variables interact strongly with many others. In these areas, an ideal experiment would have to deal with a host of permutations of variables simultaneously. This being impossible, the researcher must select some rubric or organizing principle which will allow him to design a human-sized piece of research. A practical question often serves this purpose excellently.

Storer implies that a piece of basic research—and in the life sciences it must be done piecemeal—is more likely to involve breadth of variables or "heuristic cross fertilization" or "progress along all fronts" or "the interstitial areas of science" than is an attempt to answer a practical question. In most of the life sciences the reverse is true. In our clinic, a cardiologist, a neurologist, a biochemist, an ophthalmologist, and I (an experimental psychologist) are trying to find some immediately applicable answers to questions about the relations in both directions, between performance in certain very demanding occupations and cardiovascular diseases and defects. We have obtained help from a psychoanalyst, an audiologist, a biophysicist, and others. So far, our attempts have led us to dozens of areas, many not usually dealt with in our specialties: anthropology, biometrics, "competition" in its many forms, dietetics, ethnology, "fatigue" in its many definitions, genetics, hydraulics—the list is long and still growing. Our practical questions force us to go where the relevant variables lie. It is hard work, much harder than the cozy "basic" filigree-work that each of us might be doing in his own little corner. Aside from research results, each of us is getting the broad education in life science that could be offered by "pure" research environments, but seldom is.

It is wrong to think that applied science contributes less to basic science than vice versa. To take one example from experimental psychology—probably not the best one: In World War II, the British asked some of their applied psychologists to find out why sonar operators missed many physically detectable signals, and what could be done about it. The answers, in the form of some excellent experiments by Mackworth and others on "vigilance" in monotonous situations, not only provided powerful basic knowledge about the dynamics of consciousness but, more importantly, helped force experimental psychologists to face basic facts which had been evaded in the simple-minded stimulus-response and sensory psychologies then fashionable in many academies.

Scientific research is only good or bad, not basic or applied. Good science consists of original thinking about careful observations in interesting situations, and frequently a practical question is the stimulus for it. The false dichotomy of "basic" and "applied" too often reinforces a preciousness or snobbery in the graduate student that narrows his vision for years after leaving the academy. Some never recover.

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Do Antimalarial Sprays Explain Declining Death Rates?

Your issue of 19 April [*Science* **140**, 281 (1963)] carried comments by D. S. Greenberg on the report of the Panel on Population Problems of the Committee on Science and Public Policy, "The Growth of World Population," published by the National Academy of Sciences (1963). The report is described as "brief, lucid, honest, and humane, and, on a subject that often elicits excess and astonishing logical gymnastics, it is informed, restrained and responsible. . . . it is not unlikely that the Academy report will exert considerable influence for a long time. . . ." Therefore it seems most unfortunate that the report should perpetuate a misleading description of the interrelation of economic and demographic transition in low-income countries, citing the demographic experience of Ceylon as example. Thus:

"The precipitous decline in the death rate that is occurring in the low-income countries of the world is a consequence of the development and application of low-cost public health techniques. . . .

"The use of residual insecticides to provide effective protection against malaria at a cost of no more than 25 cents per capita per annum is an outstanding example. Other innovations include antibiotics and chemotherapy, and low-cost ways of providing safe water supplies and adequate environmental sanitation in villages that in most other ways remain relatively untouched by modernization. The death rate in Ceylon was cut in half in less than a decade, and declines approaching this in rapidity are almost commonplace. . . .

"The result of a precipitous decline in mortality while the birth rate remains essentially unchanged is, of course, a very rapid acceleration in population growth, reaching rates of three to three and one-half percent."

Ceylon has been the classic example for those who would give modern medicine and public health undue credit for inducing a "population explosion." The postwar coincidence of a dramatic drop in the death rate and the introduction of insecticides has misled many. The decline in the death rate in Ceylon preceded the large-scale spraying of residual insecticides by at least 6 months. The postwar reduction in mortality was about the same in the malarious areas treated and the nonmalarious areas not treated with insecticides. There is no evidence that control of malaria was the sole or even the major factor inducing a "population explosion"; rather, malaria control has prevented the million of debilitating but usually nonfatal attacks of malaria that had been occurring annually. Thus malaria control had the paradoxical effect of reducing population pressure by removing an insurmountable obstacle to the development of the major part of the island, which had remained relatively sparsely populated. Actually, the sharp postwar decline in the death rate was a return

to the long-term downward trend, which had been interrupted as a result of wartime deprivation. The experience of Ceylon fails to substantiate the thesis that postwar mortality has been reduced quite independently of the levels of living. Attention is invited to detailed published evidence (*1*).

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Reference

1. H. Frederiksen, "Malaria control and population pressure in Ceylon," *Public Health Rept.* **75**, 865 (1960); "Determinants and consequences of mortality trends in Ceylon," *Public Health Rept.* **76**, 659 (1961); "Economic and demographic consequences of malaria control in Ceylon," *Indian J. Malariol.* **16**, 379 (1962).

Medical Education: Open Minds and Fragmentation

E. Grey Dimond's provocative letter [*Science* **142**, 445 (25 Oct. 1963)] should raise eyebrows as well as hackles among medical academics. He exposes for all to see that complacency, at least, is not one of the major sins in the medical schools, and for that we should be grateful. But Dimond should consider that, somewhere along the line, already-existing facilities have produced physicians who have gravitated into many varied fields after leaving medical school. Even now we have physicians trained under apparently hopeless curricula who nevertheless work in those ivory towers which are so essential to the continuing development of medical care, not only for the individual, but for society as a whole.

The crux of the arguments pro and con must not be disregarded, namely, that what we are ultimately striving for is *care*, care of the individual over his lifetime. Some provide this obliquely and impersonally, in the laboratory. Others provide more personal care at the bedside or in the office.

Some medical faculties may be excessively egotistical in their presumptions, believing that what happens during the 4 years in medical school alone will set an unalterable pattern for the growth of the individual physician as he matures. This is simply not so. Much that determines what a physician will do in his profession occurs after he leaves the ivied halls; much depends upon the training he received long before he reached the medical school. Unfortunately, much of what physicians learn in medical school is no longer believed to be true a dozen or so years later. The habits they acquire there, however, and their basic orientation to the patient as the ultimate focus of attention are things they must always carry with them.

Many physicians now in basic medical research institutions have developed their research interests and talents after graduation. It is a truism that most freshmen medical students want to be family doctors. Most seniors have had more doors opened to them and have thereby developed more diversified interests. Relatively few end up as family doctors.

It would seem to me that a more useful argument for medical faculties to address themselves to would be, Which students should be admitted to medical school? For regardless of what is taught, what really matters is how the physician will approach problems in medicine in an ever-changing milieu. By choosing bright students with open minds, medical schools will provide the raw material for the development of individuals with a broad spectrum of interests and talents in the field of medicine, ranging from the enzyme chemist with an M.D. and a Ph.D to the country practitioner with an M.D. and a Rotarian's pin.

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Dimond points up a broad problem in medical education: how much training should be given in basic biological and physical-chemical disciplines in proportion to the various clinical disciplines. Dimond is a proponent of more training in the basic sciences at the expense of the clinical sciences. I am skeptical of this proposal for two reasons. First, the medical student already spends about half his time in the preclinical studies after about two years of the same in his college preparatory work. In contrast he has only about two years to devote to a large mass of clinical studies.

But the more important reason is related to the underlying, though only implied, concepts of the physician involved in the controversy. The kind of "biological scientist" Dimond's program would produce would be one geared to working only in a team of highly trained and specialized men in a medical center, though how these would be integrated is an open question. At any rate, these would be a kind of elite at the top of the pyramid. As they could not actually take care of the total load of work, a need and an impetus would be created for second-class practitioners to perform the rest or the bulk of the work. These would most likely be technicians of limited background, training, capacity, and function. This kind of fragmentation is nothing new. Long ago, in Egypt, a small elite of engineers and architects planned the pyramids and other great works; the job was then finely segmented and performed by slaves. In recent years similar fractionalization has been partially and variously instituted, notably in industry and in the engineering profession. Similar manifestations have taken place in the medical profession. A recent writer described the German doctor as a hospital-referring agent. In Czarist Russia (the Soviets changed the situation quantitatively only) the large bulk of physicians were poorly trained; we would call them medical technicians. The relatively few who were highly trained were located for the most part in the medical centers. In contrast, in this country, as in England (the National Health Service notwithstanding) and in spite of specialization, the family doctor is highly trained and still does an integral job in a large segment of the practice of medicine.

There are many challenging problems in medical education. I believe Dimond's suggestions are related to some free-floating, aggressive trends in our culture, finding expressions even in science in many forms of atomization, forms which are neither very new nor as benign as they appear. Rather we should confront these problems with the insights newly acquired by depth psychology about the nature and complexities of the human individual, which point to integration and sanity in work as in life.

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Glauber's comments elude me, at least in relation to what I thought I was saying in my letter. I am afraid he extrapolates my words to fit his arguments, free-floating and otherwise, and I must reject the role in which he wishes to place me. Any thoughts of mine on medical education have been based on the belief that the major product should be a personal physician.

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