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