The latter are the recognized authorities in their fields. They know because they do. And others recognize that they do. I don't think the dichotomies university-industry or big-little (science) have much to do with the distinction between basic and applied science (look at the author bylines in the journals). Decisions based on this distinction should be made by, or at least in consultation with, the individual recognized researchers.

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. . . While exploring the quicksand between particle physics and building better mousetraps, the classical boundries of research and science are left unperturbed. Science is that on which research is done; science and research involve things hard enough to stub one's toe on . . . a Spinco, a microscope, or a mousetrap. To me, research is the Almighty Scientific Method in action, and if this is true, objective evaluation of a teaching program or a systems approach to delivery of health care are as much research and as much science as a frontal attack on the mystery of the gene. Classical research and science they are not. But the more that research, education, and service are considered as functional aspects of the same animal, the easier it becomes to measure the dollar value, or applicability, of the total effort. At one extreme, a study may involve no more service than providing jobs and no more education than how to operate an electron microscope. At the other, where students are involved in research on a problem of local service, the learning, discovering, and helping have a positive feedback relationship toward each other. The value of the system includes the people in it. . . .

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Statistical Randomization in the Behavioral Sciences

In his letter (24 Feb.) Stanley takes issue with my protest (Letters, 21 Oct.) against the invalid uses of inferential statistics in studies employing (i) nonrandom samples and (ii) nonrandom assignment of experimental treatments to sample units. According to Stanley, "hypothesis testing in studies involving nonrandomly chosen 'grab groups' is feasible. If it were not, the results of many comparative experiments in the behavioral sciences would be difficult to interpret statistically, because any differences whatsoever might be attributed to chance fluctuations."

The invalid use of inferential statistics *is* feasible, possible, and, indeed, popular in the behavioral sciences. I hasten to agree with the consequence Stanley points out: many studies *are* "difficult to interpret statistically." But this is because these "quasi-experiments" to use Stanley's term ("pseudo-experiments" might be less misleading) violate the assumptions of inference. Descriptive statistics are perfectly appropriate in these studies, but inference to other populations is a matter of opinion or authority rather than statistics.

In the field of educational research to which Stanley addresses his remarks, these violations have led to seemingly endless series of equivocal, nonreplicable studies. Important questions such as the effect of class size and teaching methods on student learning remain unanswered. Instead of tightly controlled, randomized experiments, investigators continue to pursue statistical significance with ever larger samples in quasi-experiments. In addition, the generality of other nonrandom behavioral science research for human affairs is open to question, especially if the rest of us do not behave like white rats, pigeons, college students, pig-tailed macaques, or Maori tribesmen.

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Growth Incentives in the "Have-Not" Nations

In his address delivered at the December Washington meeting of the AAAS ("The ever widening gap," *Science*, 24 Feb., p. 959), Blackett reiterated the prevailing and plausible opinion that the population problem in the poor countries in the South is due to the export of modern medicine by the rich countries in the North, which has resulted in health, too much and too soon, rather than wealth.

On the basis of simple arithmetic, it may seem plausible to construe a