out the National Halothane Study, we have not been able in these laboratories to confirm the findings of Cohen with respect to the conversion of "Fluothane" to the butene derivative. We have found no significant evidence of such conversion of "Fluothane" to the butene by contact with copper and oxygen, or with copper oxide, under conditions much more severe than those which obtain in anesthetic practice.

Although the concentration of the butene derivative may increase by evaporation, it should be borne in mind that the quality-control specifications for "Fluothane" require it to be 99.9 percent pure. No other ingredient may be present in amount greater than 0.05 percent (an amount corresponding to a concentration of 5 parts per million in 1-percent "Fluothane" vapor, as used in the maintenance of anesthesia). This rigorous specification for "Fluothane" is the result of careful work on the details of the manufacturing process over a period of years; this work is continuing.

Of the 100 specimens of "Fluothane" drawn from all types of anesthesia vaporizers—copper, chrome-plated copper, stainless steel, and glass—some of which had not previously been drained for periods of up to 18 months, only one specimen was found to contain a concentration of the butene derivative in excess of the quality-control specification for freshly prepared "Fluothane"; this one sample contained 0.058 percent. The mean butene content of all the specimens examined was 0.029 percent.

Because of the importance of this matter we have thought fit to write this preliminary letter; a detailed report, including both chemical and toxicological data, is in preparation.

W. A. SEXTON
Imperial Chemical Industries, Ltd., Cheshire, England
WILLIAM G. HENDRICKSON
Ayerst Laboratories, New York

The Social Stimulus to Creativity

What the social engineer, designing scientists, wants to know is the optimum of gregariousness needed for creativity. There are many things of which a servomechanism like man can get too much and also too little, and people are one of them. Do 12 students make a better seminar than 4 or 20? From what sized audience will your productive drives profit most? From 20 who can talk back? 100 who cannot? 5000 who may even not have been there, unless the TV audience rating turns out to have been right? When President Garfield said that the ideal university would be a student on one end of a log with Mark Hopkins on the other, was he thinking about Mark Hopkins or only about the student? Would another student in the middle of the log have stimulated Mark Hopkins more? We need to know more about the kinds and dimensions of small groups that best promote creativity, and which groups work best with which scientists.

It is almost impossible to imagine a scientist's contributing to knowledge without any social stimulus at all. Would such a scientist be a robot, capable of energizing himself from handy natural resources and set to run through the concomitant variations of the n parameters of the system he was designed to investigate and then to file the results in his magnetic memory? That would not be science, for it would be asocial: there would be no communication. Or would the isolated creative mind be Descartes' as he moved from one to another of his 24 Dutch hideouts with his address known only to Father Mersenne and a few others? Not a bit of it. Descrates corresponded with his peers, got into controversies, and cultivated the acquaintance of some whom he admired (one philosopher, one princess), although he kept himself free to claim soiltude for meditating and writing when he wished. He needed both privacy and social stimulus—now one, now the other.

There are many creative individuals whose minds work best with constant stimulation from small groups of others with common interests—disciples sometimes, or peers. It has often been remarked that the researcher gets along better when he can also teach, and this is true for the investigator who likes to do his teaching in his laboratory, perhaps sitting on a table—a scientific equivalent of Mark Hopkin's log. We all know laboratory men who cannot help teaching even when there are no classes.

Yale's psychologist Clark Hull got enormous support from his "in-group," which came to extend far beyond New Haven. In the days of its maximum vivacity, when Hull would come to speak in Cambridge, Massachusetts, the Harvard Yard would bristle with Hullians who had come along, not to learn the current truth, for they already knew it, but to see it succeed. They were rooters. Certainly Hull was stimulated by disapproval, too, yet it is hard to imagine his being as effective as he was without his in-group.

At a much broader level there have been the academies and societies that have been formed to stimulate scholarship and research. In 1660 the Royal Society began as a small group of men who met together to inform and animate one another and to establish a journal for the publication of discovery. The French Academy was not very different, and on this side of the water comparable groups were formed later in Philadelphia and Boston, both of which, in spite of their present enormous sizes, have managed to keep some of the advantages of the social activation of creativity. In general, the society that succeeds in stimulating creativity grows, for scientists look upon social stimulation as a good natural resource, and they join up. Eventually the society gets too large, gaining some advantages that go with size but losing many of the advantages of intimate friendly discussion.

Then it is that new little societies form, in an attempt to recapture the lost stimulation. They may come into being by fission from the parent, or they may emerge from some in-group that has been starved for social activation. Any scientist can fill in, for his own field, the details of this paradoxical social growth, where the healthy scientific society loses its original usefulness by its inevitable growth and is supplanted by younger and smaller groups of a new generation, which in their turn become too large. This paradox of growth's defeating its original purpose is not merely a symptom of the present explosion in science. It has also been the pattern of the proliferation of the sciences themselves, as each came to include too much for the individual human mind to grasp, and as smaller sciences were needed.

The familiar modern in-group that supports investigation nowadays is more the result of the scientific explosion than of the magnetism of a leader who compels loyalty. Joint endeavors in research resulting in multiple authorship, are the symptoms not only of the need to combine research efforts in order to achieve publication in the limited space avail-

able but also of the need for social stimulation. These joint undertakings do not always work, for attempted cooperation inevitably provides the opportunity for dissent; yet many of them work very well indeed. There are also the in-groups whose members succeed in common enterprise through correspondence, without being in the same place or able to become joint authors. These persons write papers, largely to one another, and they may develop so neologisitic a form of communication that outsiders cannot easily understand them or are unwilling to make the effort. So it is that the strengthening of communication within the in-group sets up barriers against outsiders, limiting the range of communication.

When the in-group begins corresponding by mimeograph, the desire for a wider social response seems to become almost irresistible. The author writes his paper and then mimeographs or dittoes or Xeroxes 100 copies and strews them abroad, sometimes just as they left the machine, sometimes with a general wistful request for comment or criticism, sometimes even with a "Dear-Dr.-Jones-Sincerely-yours" letter attached. It is hard for the older recipient, who has survived from the age of gracious epistolary manners, to know what to do with such a letter, personal and yet become standard operating procedure. He has been reduced to a statistic, and is no longer a person, for the author hoped for five replies out of 100, and the 95 were never expected to respond graciously to the implied R.S.V.P. Still, the method works. It provides casual communication, leaving journal publication for more carefully developed contributions. After all, this is also the method of ungracious nature, which scatters thousands of seeds for every one that eventually brings forth fruit.

There are some who see the present scene in American science as consisting of an enormous number of little in-groups, cooperators, and joint authors, each concentrating on some special enterprise, persistently "learning more and more about less and less," each group with its code of neologisms and laboratory slang—successful fanatics perceiving the larger world of their science somewhat dimly. Is this bad? It is the modern social version of the motivational predicament in science where egoism works against objectivity. The answer to the problem raised is surely that both these opposing tendencies need to be reinforced. Science as a whole requires cybernetic guidance. The fanatics of the in-groups get good work done. It has always been true that enthusiasm is the friend of action, even though it be also the enemy of wisdom. The dedicated individual, or the group small enough to feel its own selfhood, knows how eventually to penetrate the scientific frontier. Nevertheless, the effort toward generalization must go on at the same time if science is to continue to progress. Research must be published. The papers must be brought together and summarized—in annual reviews, then in books, after that in encyclopedias and handbooks—until the broadest principles are absorbed into the body of science and dealt out to sophomores in the textbooks. Depth and breadth are both worthy goals and in general are to be striven for by a division of labor among persons or through varied activity on the part of the same person. After a decade of intensive research, the fanatic stops to write his book. The incompatibility between the two activities is, at worst, only motivational. The complacent critic who merely views the scene had better do his wishing by hoping for both—for the social stimulus of the myopic in-group and the broader, more objective perspective that goes with erudition. Some scientists seem to manage to cultivate both of these conflicting values—alternately, more or less. In any case there is always posterity just around the corner, waiting to correct myopia.

EDWIN G. BORING
Harvard University,
Cambridge, Massachusetts

Intellectual Excitement: The "New" versus the "Classical"

One extremely important point is constantly overlooked or simply not emphasized in discussions of the "new" versus the "classical" in science, perhaps especially in biology [see E. Mayr, *Science* **141**, 765 (1963)]. This is the responsibility of proponents of classical fields to revivify their own fields so that the classical now burns with a competitively bright light in attracting newcomers and funds.

Before going further I would state my agreement with most criticisms of the "bandwagon tendency in American science." There are fads today in science. There are "Young Turks" whose arrogant enjoyment of their own field is matched only by their uncomplicated ignorance of other fields. There are fund directors who are overly zealous in associating the resources they control with "glamor fields."

But let us realize that glamor fields do not arise *de nove*; somewhere, imaginative hard work broke through. And Young Turks and fads typically gain their inspiration and enthusiasm from a genuinely exciting body of creative work. We cannot turn our backs on the advances which generate bandwagons; we cannot ask them to slow down, not in the slightest. It behooves other areas to catch up.

This returns us to the main point, the responsibilities of those who rightfully insist on the continuing significance of the classical fields. It is true that catching up—if that is the right phrase—will be tough in the face of rampant bandwagons tending to siphon off personnel and monies in their own support. But catching up is not impossible and is probably no more difficult than the initial efforts of those who labored to create the new field in the first place. In any case it is certain that simply "more financial and moral support for the classical areas" is not the answer. These two ingredients are important. However, the most important ingredient is intellectual excitement. This will not reappear in the classical areas through the use of intellectual feather dusters which simply reshift accumulated dust but which do not touch the form or content of underlying ideas. Perhaps intellectual sledgehammers are more the tool of choice to refurbish, remold, and rejuvenate hardened outlooks. Population genetics was the sledgehammer that remade taxonomy into the new systematics, as Mayr—a major architect in the remaking—well points out. Money and sympathy will not of themselves revivify invertebrate zoology (the specific example cited by Mayr as an understaffed field). The first necessity, as seen by one whose special interests are in the lower metazoans, is for invertebrate zoologists themselves to retool intellectually, if this should be necessary, and to rethink and recast their own field—to create their own breakthrough.

EARL D. HANSON
Shanklin Laboratory of Biology,
Wesleyan University,
Middletown, Connecticut