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Limits to Growth: In Biology, Small Science Is Good Science

Commentary

These days, many people grow up believing that bigger is better. Giant factories that produce Wonder Bread have replaced thousands of corner bakeries, driven by the increased efficiency of scale. There is an unfortunate tendency to extend this view to the biological research community, and I have on occasion heard a major symposium speaker introduced in glowing terms as the coauthor of more than fifty papers per year. While I can admire the energy and management skills required to maintain a very large laboratory, the best biology is rarely done in this way. With a few notable exceptions, the biochemists and molecular biologists I most respect run relatively small laboratories and publish when they have something important to report. As I shall argue here, doing good science is very different from producing bread, and there are compelling reasons why large laboratories are in general less efficient and less interesting than small ones. To reflect this fact, I believe that changes in funding patterns and expectations would be useful in the biological sciences.

Several factors combine to make large research groups inefficient. As the size of a research group increases, more and more of its leader's time has to be spent on such nonintellectual endeavors as helping with job applications, finding and accounting for funds, and other organizational matters. Less and less time is left for thinking about science, let alone keeping up with a voluminous research literature. In the crush of such overcommitment, I have sometimes found myself encouraging my associates to do obvious rather than innovative experiments, in order to be relieved of having to spend too much of my time in worrying about their projects. Moreover, as a laboratory grows, one becomes less and less familiar with many of the techniques being used—and thereby less able to judge either their potential or their limitations.

In the worst case, a large laboratory can become a place that simply provides equipment and supplies to younger researchers, with very little or no research advice or guidance. As matters now stand, most scientists who have reached a certain level of accomplishment can find the funds to set up such a laboratory. If one is "lucky," he or she will attract enough outstanding young people into such a group to maintain a self-sustaining level of research productivity. Grants can sometimes even be renewed with minimal input from the principal investigator, being largely "ghostwritten" by younger colleagues. Eventually, an NIH program project grant might be obtained, giving more permanence to the operation. In such cases, the group leader can become a true "science manager"—an individual who makes very little contribution to the actual science being done, but who spends nearly full time arranging for funding, travelling to give seminars and symposium talks, and processing manuscripts from the laboratory for publication.

I believe that laboratories of the above type should be strongly discouraged in U.S. biological science. When rewarded with too much money, there is very little impetus to choose priorities carefully, as is required to use one's limited intellectual resources wisely. Moreover, because of the need to maintain the operation at a certain level of activity, it is inevitable that most of the work being done is rarely innovative or outstanding. Some large laboratories tend to jump quickly to exploit the original observations of others, believing that their extensive resources will enable them to compete effectively.

Many large laboratories represent a poor training environment for young scientists. Graduate students and postdoctoral scientists are treated as though workers in a factory, contributing strictly to their own part of the production line. This does not prepare them to function as independent scientists and may even impede their development by preventing the acquisition of habits of independent research at a crucial point in their careers. Even those rare individuals who succeed can become disillusioned and cynical, when they see their own ideas and efforts credited to a group leader who made no scientific contribution to the research that they performed.

It is also important to realize that large laboratories are often very wasteful of resources. Per capita productivity is important because the total number of scientists who can be supported is limited. As an NIH study section chairman who has reviewed numerous grants, I find it extremely difficult to sort through computerized listings of "other grant support" to decide whether the present grant request from a large laboratory is really justified. Does a particular project require five postdoctoral fellows or can it be done just as well (or better) by three? The two major criteria that are applied at present are those of "scientific overlap" (is the proposed work already covered in another funded application?) and "competency and quality" (is the investigator able to carry out the proposed work and is it worthwhile?). If the answer to the first question is "no," and the answer to the second question is "yes," the grant is approvable with a high priority *regardless of the total level of other grant support.*

I believe that these two criteria are insufficient in several respects. First, any worthwhile project is bound to produce some unexpected results during a three to five year granting period. Ambitious individuals who have mastered the system will seize on each such novel result as an opportunity for seeking a separate grant to explore all of its possible ramifications, rather than include these studies in the original project where they often belong. Yet there will be no scientific overlap in the formal sense, because the unexpected was of course not included in the original specific aims. Most importantly, the question of competency is crucially related to how much energy and attention can be devoted to the new research proposal. If a proposal will increase a laboratory's size from (say) fifteen to twenty researchers, I contend that there is a strong likelihood that the project will not receive the type of attention from the principal investigator that is required to make it outstanding, regardless of the quality of the application.

There is a rational way of dealing with these problems.

I suggest that the NIH (and other agencies) set a formal ceiling on the total amount of funding from all sources (including private foundations, program project grant allocations, etc.) that may be used to support the laboratory of any individual principal investigator. The limit should reflect the amount of research with which one investigator could be closely involved on a day to day basis. With current costs, one might envisage a limit in the region of \$300,000 to \$400,000 per year. Although funding above this level could still be possible, it would require evidence of some very exceptional merit or need—for example, a requirement for especially expensive reagents or animals.

Such a plan would of course save funds and thereby allow more scientists to be funded. By setting a limit to the size of the laboratories that most of us could hope to run, it would force each of us to spend more time on science and less on grant writing, local negotiations for more laboratory space, and other aspects of scientific administration. The net result would be a better general research environment, as well as more opportunities for independent work by young scientists.

It is crucial to recognize that many important research results start as surprises whose implications can easily be missed, and that money is no substitute for careful observation, thoughtful analysis, and scientific skill. Moreover, a single innovative and original publication is worth much more than ten obvious ones. One could argue that the surest way to destroy a young scientist would be to give him or her eight technical assistants in constant need of supervision and advice, and the motivation to work on three different projects at once. Science is not a business and bigger is not better. What we want to encourage from the best young people is perhaps one paper per year—one that makes a real contribution and will be worth reading even years after its publication date. Any value system based on acquiring the largest research team, or on maximizing either total grant support or publications, is counterproductive to good science and should be viewed with alarm.

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