

# Is bigger better?

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What is medical science all about? Surely it is about the value chain, which begins with basic research and ends—if there is an end—with a useful product. There is a widespread perception that scientists do a lot of basic research, but neglect the application of their findings. To remedy this, a number of organizations and philanthropists have become dedicated advocates of applied or translational research and preferentially fund large consortia rather than small teams or individual scientists. Yet, this is only the latest round in the never-ending debate about how to optimize research. The question remains whether large teams, small groups or individuals are better at making ‘discoveries’.

To some extent, a scientific breakthrough depends on the nature of the research. Einstein worked largely alone, and the development of  $E=mc^2$  is a case in point. He put together insights from many researchers to produce his breakthrough, which has subsequently required teams of scientists to apply. Similarly, drug development may require only an individual or a small team to make the initial discovery. However, it needs many individuals to develop a candidate compound and large teams to conduct clinical trials. On the other hand, Darwin could be seen to have worked the other way around: he had an initial ‘team’ of ‘field assistants’—including the crew of *HMS Beagle*—but he produced his seminal work essentially alone.

Consortium funding is of course attractive for researchers because of the time-scale and the amount of money involved. Clinical trials or large research units may get financial support for 10 years or even longer and in the range of millions of dollars. However, organizations that provide funding on such a large scale require extensive and detailed planning from researchers. The work is subject to frequent reporting and review and

often carries a large administrative burden. It has come to the point where this oversight threatens academic freedom. Principal investigators who try to conduct experiments outside the original plan, even if they make sense, lose their funding. Under such conditions, administrative officials are often not there to serve, but to govern.

There is a widespread perception that small teams are more productive in terms of published papers. But large-scale science often generates outcomes and product value that a small team cannot. We therefore need both. The problem is the low level of funding for individual scientists and small teams and the resulting cut-throat competition for limited resources. This draws too many researchers to large consortia, which, if successful, can become comfort zones or, if they crash and burn, can cause serious damage.

Other factors should also inform our deliberations about the size of research teams and consortia. Which is the better environment in which to train the next generation of scientists? By definition, research should question scientific dogmas and foster innovative thinking. Will a large consortium be able to achieve or even tolerate this?

Perhaps these trends can be ascribed to generational differences. Neil Howe described people born between 1943 and 1980 as obsessed with values, individually strong and individualistic, whereas the younger folks born after 1981 place more trust in strong institutions that are seen to be moving society somewhere. If this is true, we can predict that the consortium approach is here to stay, at least for some time. Perhaps the emergence of large-scale science is driven by strong—maybe dictatorial—older individuals and arranged to accommodate the younger generation. If so, it is a win–win situation: we know the value of networking and interacting with others, which comes naturally in the ‘online age’.

A down side of large groups is the loss of individual career development. The number of authors per paper has increased constantly. Who does the work and who gets the honour? There is often little recognition for the contribution of most people to publications that arise from large consortia, and it is difficult for peer-reviewers to assess individual contribution. We must take care that we measure what we value and not value what we measure.

While it is clear that both large and small groups are essential, good management and balance is required. An alarming trend in my opinion is the inclination to fund new sites for clinical trials, to the detriment of existing facilities. This does not seem to be reasonable or the best use of scarce resources.

In the long-term interest of science, we need to consider the correlation of major breakthroughs compared to incremental science with the size of the research group. This is hard to measure, but we must not forget that basic research produces the first leads that are then developed further into products. If the funding for basic science decreases, there will soon be a dearth of topics for ‘big science’.

Is there a way out of this dilemma? I would like to suggest that organizations currently funding large consortia allow investigators to set aside a percentage of the money to support basic, curiosity-driven research within these consortia. If they do not rethink their funding strategy, these organizations may find with time that there are few novel discoveries for large groups to explore.

#### REFERENCE

Howe N, Strauss W (1997) *The Fourth Turning: An American Prophecy*. Broadway

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