Comment

The buckyball effect

Gregory A Petsko

Address: Rosenstiel Basic Medical Sciences Research Center, Brandeis University, Waltham, MA 02454-9110, USA.
E-mail: petsko@brandeis.edu

Published: 10 January 2001

Genome ***Biology*** 2001, **2(1)**:comment1001.1–1001.2

The electronic version of this article is the complete one and can be found online at http://genomebiology.com/2001/2/1/comment/1001

In the spirit of the season, I've been wondering what happens to old toys when new ones arrive during the holidays (I know, the *Toy Story* movies explored this question from the point of view of the toys, but bear with me). After all, the old toys are the ones the children once couldn't do without, yet there aren't enough hours in the day to play with all of them plus the new ones, so something must get neglected, at least relatively. At the end of November last year, I met someone who reminded me about this, and made me realize the connection between the discarded toys and the effect of genomics on what is 'hot' in biology.

There are a number of reasons that academic scientists travel as much as they do. One reason is to experience that sense of being valued that only accrues to someone away from his or her home institution (familiarity breeding, if not outright contempt, a kind of devaluation of the currency of one's repute). Another is to discover new people with interesting ideas. I was giving a seminar at Temple University in Philadelphia when I was introduced to David R. Dalton, an organic chemist who proved to be a delightful and stimulating conversationalist. For some reason we got around to discussing fads in science and he promptly remarked "Yes, the Buckyball Effect!"

I knew what buckyballs are: buckminsterfullerenes or fullerenes for short. The term describes a range of recently found (mid-1980s) forms of carbon including C_{60}, a roundish molecule made of 60 carbon atoms arranged in a truncated icosahedron (one of the 13 Archimedian solids), like the vertices of a soccer ball. Such molecules were named after Buckminster Fuller, who invented the geodesic dome, which has a similar structure. But I had never heard of the Buckyball Effect.

"Oh, that's my own term," Dalton said. He went on to explain that he had always wondered what happens to old scientific areas of investigation when a new one comes along. Within a year or two of the discovery of buckyballs by Smalley and his associates, there were hundreds of people working on them in labs all over the world. But before they started working on buckyballs, all of these people were working on something else, something that they believed was important (or at least, something that they told the funding agencies was important). What happened to all of those projects? After all, he said, one can't do everything. If a lot of effort goes into a new area, a number of old areas once thought important enough to warrant funding and research activity must get neglected in comparison. Did these areas suddenly become less important? Were they never really all that important? And if they were important and still are important, how will that work get done now?

Genomics, it seems to me, provides a classic example of the Buckyball Effect. So great has been the publicity afforded to genomics research, and so generous does the funding for it appear, that it has become the hottest area of biology almost overnight. Granted, genome sequencing is losing some of its novelty, but the pull will only increase as genomics morphs into proteomics and structural genomics and functional genomics.

What is the origin of this faddish behavior? Are scientists lemmings in whom the herd instinct is stronger than their love for a particular subject? Do they have the attention span of five-year-olds? Is it the charm of the unfamiliar? Or is something more subtle at work? Much of the attractive power of a hot new field comes, of course, from the promise of easier access to funding. Most scientists tend to choose the subjects of their work - or at least slant the way they present those subjects - according to the advice 'Deep Throat' gave to journalists Woodward and Bernstein during the Watergate investigation: "Follow the money." But I think there's a deeper draw too. Scientists like to work in hot fields because to do so enhances their importance, and their sense of self-importance. One is so much better dinner-party company if one is working on something that the lay public has heard of and believes to be interesting and worthwhile.

One feels somehow superior to other scientists who are working in fields that have less cachet - and the transition from hot field to backwater can occur with startling swiftness in biology these days.

But the field as a whole pays a heavy price for this constant shifting of mobs of researchers into the latest fad. Older areas that were important and still are important lose talented people, funding (it is, after all, a zero-sum game, and don't let anyone tell you differently), and visibility. In response to these losses they can become defensive, conservative, and inner-directed. Because everything in science is cyclical, eventually most of these fields will come back into fashion, but when they do there will be a dearth of good researchers working there. Just ask the virologists, who languished for decades until oncogenes were discovered. Or the microbiologists, who were believed passé until drug-resistant pathogens began exploding over the health-care landscape.

It seems to me that we ought to learn from these and countless other examples. Funding agencies need to resist the temptation to pour most of their resources into the newest fields. It's happening too much at the US National Institutes of Health, which is rapidly becoming uninterested in microbial genetics, microbial physiology, mechanistic enzymology, and fundamental biophysics, to name but a few areas, at the expense of genomics in all its guises and research involving human cells or targeted at human disease. If present trends continue, it will soon cede title to such areas almost entirely to the National Science Foundation, a great organization but one with far fewer resources and a much broader community to spend them on.

It's also happening in universities, which, despite their mandate to preserve traditional areas of investigation whenever these have value, are rushing to throw their own resources at genomics-based initiatives, to the serious neglect of 'older' subjects that are less fashionable. To be sure, the less faddish fields can survive partially by allying themselves with genomics or adopting genome-wide approaches, but the Buckyball Effect still applies. What about the important, fundamental work in those fields that is not being done because everyone in it is rushing headlong towards genomics? Were those subjects never as important as their proponents once claimed? Shifts in emphasis are inevitable and healthy, but if we want to have the rigorous underpinning in chemistry, physics and biochemistry that must underlie the 'new' genome-based biology - if it is to have the intellectual impact we expect of it - the old toys must not be forgotten.