Fortuitous Errors

I read the following passage in the April 12, 2009 issue of *The New York Times*:

> But I was fascinated by his teenage adventure, largely forgotten today, when he and his friends found the first route through southern Utah's maze of canyons, discovering the last unknown river in the continental United States, the Escalante, and the last mountain range, the Henrys. They were the first to peer into that phantasmagoric expanse of Bryce Canyon and the first to cross what is now Capitol Reef National Park.
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> At one particularly tricky canyon crossroad, they tried to convince a Ute Indian to act as a guide, "for the labyrinth ahead was a puzzle," Dellenbaugh later recalled. After the man wandered off, the group pressed on anyway, trusting to their spirit and wits.

Discovery is a relative term and not as binary as it sounds. It's traditional—at least when I was in grade school—to envision "Columbus discovering America," but this renowned explorer, despite four trans-Atlantic voyages, never realized that he was still thousands of miles from Asia, failed to comprehend what he had inadvertently discovered, and lost eponymic fame to a later explorer who published more widely. Yet, his "discovery" had a momentous impact on the world.

Research is somewhat like exploration, and, with that analogy in mind, I think we can learn a few things from the explorers whose voyages were not aimed at Hyatt Regency conference rooms. Here are some observations for your consideration.

First, as demonstrated by Columbus, the greatest discoveries are often made by mistake, especially while looking for something else. This isn't always the case, but it happens often enough that we ought to give it some serious thought. I'd love to write a proposal that says, "If you give me some research money, I'll think about lots of really hard problems, and I might discover something useful by mistake." Instead, proposals are expected to delineate a compelling problem and then provide a detailed and thoughtful description of novel and promising ideas and techniques that will be brought to bear. All of this must be based on a review of the literature that should be as thorough as humanly possible—failing to cite relevant work can doom your proposal to the rejection pile.

Developing these elements in a convincing way is far from trivial. New ideas need to be backed with credible evidence. On the other hand, if these ideas are too well supported, then the reviewers may think that the work isn't new enough. In Columbus's case, he succeeded by using erroneous results from the literature, which underestimated the Earth's circumference by a factor of two. But that premise succeeded in convincing the reviewers, and he got the funding. Needless to say, he and his crew were lucky that there was a place to land where he expected to find Asia.

Second, research that does not "succeed," either on purpose or by mistake, may also be successful. When I look back at early papers, I see what looks like waves of effort pushing up against rocks on the shore. Ideas are investigated, partially developed, but don't quite make it over the breakers. Some research is attempted too early—by

From left: Eric Feron, Wassim Haddad, Panagiotis Tsiotras, and Dennis Bernstein at Georgia Tech.

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From left: Jeff Shamma, Dennis Bernstein, Allen Tannenbaum, and Wassim Haddad at Georgia Tech.

From left: Dennis Bernstein, Anil Rao, and Rick Lind at the University of Florida.

which I mean that the infrastructure isn't quite available to allow the ideas to be fully developed. In other cases, the researchers simply ran out of time, money, energy, or optimism. But these partial advances are often valuable by suggesting what does and does not work and by motivating others to try harder and longer.

The most contentious aspect of research concerns who gets credit. Is it the early researcher whose insights hinted at the solution? Is it the master technician who, through tenacity and brilliance, worked out the full details? Or is it the expert in some application who recognized the practical ramifications of the research? In rare instances, two or three of these archetypes are rolled into one, but more likely the full story involves the efforts of a collection of individuals.

The final—and ironic—aspect of research is often expressed by the simple question, "Why did it take so long?" I imagine a band of hunters chasing a quasi-invisible prey until finally someone spots it and then everyone sees it. I've seen this scenario happen over and over, and it makes me wonder why it takes so long to recognize the obvious. Apparently, what's "obvious" after the fact isn't really so obvious.

Discovery and creation are fraught with lucky mistakes, partial successes, hazy credit, and, finally, a depressing sense that it was obvious all along. If I'm lucky enough to stumble onto something "new" and useful, I'll expect that all of these things will play a role. I hope I'll be first, but then I'll remember that my Ute guide probably had better things to do that day.

Dennis S. Bernstein

Prophet and Loss

To give another example, a few years ago, an economist wrote a paper revealing that good weather in New York City had a small but statistically perceptible positive effect on stock prices. If the scholar had kept his mouth shut, he could have arbitraged that piece of information to play the odds and make lots of money, assuming he had adequate capital initially. Instead he made a contribution to the community of scholars. The moment word of this effect got out—thanks to the Internet—his opportunity for financial arbitrage was gone, not to mention the argument of his paper. All investment bankers on Wall Street (and savvy investors on Main Street) now accounted for New York City weather in their models, and the effect disappeared.

—Dalton Conley, "The Social Limits of Scientific Knowledge in an Age of Easy Information," *The Chronicle Review*, December 5, 2008, p. B4.