STUMBLING INTO BRILLIANCE

by Danny Hillis

True breakthroughs are almost always unexpected.

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In 1928, the Scottish Scientist Alexander Fleming was studying bacteria under his microscope. But he had a problem. A strange mold had appeared on his samples, disrupting his experiment. Then Fleming happened to notice something interesting: Bacteria did not grow near the mysterious mold. Completely by accident, Fleming made one of history's great medical breakthroughs-he discovered penicillin.

In my own career as a scientist, inventor, and manager, I've seen Fleming's experience repeats itself over and over: A great innovation emerges not at the centre of a company's field of focus but at its periphery. In the 1970's for instance, I was part of a large team at MIT working on artificial intelligence. We spent countless hours developing software for language processing and reasoning. But as we were working toward our goal, we were also goofing around on the side. We had an enormous amount of computer power at our disposal, and we used it to create basic software for exchanging text messages with colleagues. We also hacked together some computer games. As it turned out, both e-mail and video gaming turned into billion-dollar industries (though we failed to see their commercial potential at the time). And artificial intelligence? It remains a research problem.

That's not to say we'd have been between off if the lab directors had just given us the computers and let us run wild. Most innovation isn't arbitrary – it's just hidden in the mass of work done in the pursuit of some other goal. When I co-founded Thinking Machines in the early 1980's we believed that the next big prize in hardware would be supercomputers with massively parallel processing – and that's where we focused our investment and our energy. But we also had to develop disk drives for those machines, and we did that by creating what's called a parallel disk array. The disk array wasn't the primary goal of our R&D efforts, just one of many by-products. As we worked on parallel processing, we learned that it could be applied to a much wider range of computing problems than we had originally thought. That, unfortunately, attracted the interest of established mainframe manufacturers, which had the scale to produce and support supercomputers more efficiently than a small company like Thinking Machines could. But the disk arrays were a different matter – they were the key to creating very large databases, and they were a simple enough product to be developed effectively by start-ups. EMC and other then-small companies focused on disk arrays as a core technology for data storage while we continued to develop parallel processing. We eventually got significant license fees for the disk arrays, but we lost the chance to lead the industry.

Everyone knows that innovation is risky, and it's rare that you arrive at your expected destination. But maybe that destination isn't so important. Maybe what you should be paying attention to are the little detours you take along the way: It's down those back roads and byways that the real payoff usually is found. Maybe, in fact, the biggest risk in innovation lies in sticking too closely to your plans.