The invisible hand

A granted wish.

Allan M. Lees

It's funny how frequently the public's idea of something is far from its reality. Hollywood thrives on this misperception, of course, but so does science. Canvas one hundred people at random about their notions of science and you'll get a litany of descriptions involving white coats, rationality and the pursuit of knowledge. In theory science is about exploring the unknown and pressing back the boundaries of knowledge, whereas in practice it's all about writing up ever more grant applications and pressing back the boundaries of plagiarism.

The dirty little secret of science is that most of it is mediocre at best. Science is, by and large, something done by those of us who don't want to expose ourselves to the hustle and bustle of commerce. Of course, saying this out loud is heresy, and if I were still a working scientist I'd be expelled from the lab or institute in which I'd managed to create some kind of refuge from the cold hard world. But I am no longer a working scientist; at least, not directly. Now I am more of a … well, one might say, prime mover.

Seven years ago my life was that of a typical young male scientist: most of my spare time was spent trying to impress the latest female intern with dates in cheap restaurants and much specious waffle about how one day I'd have a place for her in my own lab. But mostly it was all about writing grant applications; rewriting grant applications; waiting to hear back from the study groups that score grant applications; and then inevitably writing yet more grant applications after the first lot was rejected. I quickly learned that study groups were comprised of older scientists whose best work had been done years ago. They would reliably approve grants for barely incremental mini-steps that were essentially near-copies of what had already been done before. No really new or radical grant proposal was ever funded.

Everyone knows the story of Craig Venter, the man who first sequenced the human genome: he wrote a grant application for funds to sequence part of his own genome and had his grant rejected by the highest and most eminent scientific authorities on the grounds that such a thing was totally impossible … and he then carried out the first sequencing just three weeks later. The big innovations such as the silicon chip, the telecommunications revolution, software, jet transport … everything important came out of industry, not academia, because commerce must respond to basic human needs. Research science was stagnant, conservative and dead-ended.

Until me.

One evening I was sitting in my dirty bedroom, perched on a pile of old men's magazines, typing up yet another grant application when I decided to do some basic research of my own: find out the composition of the study group that would review my latest grant proposal, discover their biases, and skew my proposal to pander to their prejudices. It's something the more senior lab members had been doing for years, of course, but no one talked about it openly. As I was reading an online article by one study-group member, I came across the phrase that changed my life, and by extension the entire future of science. The eminent professor in question was bemoaning the sheer volume of grant submissions that had to be reviewed. She said: "We have to read hundreds of grant proposals each year in order to approve a mere handful. It absorbs far too much of our time."

That banal utterance changed everything. That night I abandoned my semi-finished proposal to study the effects of α-lipoic acid on a cloned passive-aggressive subspecies of *Caenorhabditis elegans* and began to write a software program to automate the evaluation of grant proposals. It took me three months of hard work, but the result was worth it. I sent it out into the world anonymously via e-mail lists and free download sites. Naturally no one would consider using it, no one would admit to using it … but within a few months it was evident that grant proposals were being processed far more speedily than before.

It would have been criminal negligence if I hadn't taken advantage of the opportunity. I made my first fortune with my revolutionary grant-proposal-generation service, the yin to my first program's yang. I accepted online bids and the winners received system-generated proposals that would get a 100% score when evaluated by (my) grant-evaluation software. As word spread, bids grew in number and size and within six months I was seriously wealthy. And I could have stopped there. Bill Gates would have stopped there. I think even Sergey Brin and Larry Page would have stopped there. But I didn't. If years of reading lads' mags had taught me anything, it was that more is better.

From time to time I had updated the algorithms in my anonymous grant-evaluation program to stop other people from writing grant-proposal-generation software that would score as highly as my own and thus undermine my highly profitable monopoly.

Now I set out to modify my program with a higher purpose in mind: by altering the scoring algorithms I could essentially determine what types of research would get funded. I, alone, could steer the direction of fundamental science across the entire scientific world. For the first time, research science could be focused on mankind's most fundamental desire. And thus I focused it.

And that is how the entire scientific establishment, some two million researchers around the world, physicists, biologists, chemists, astronomers, botanists and even neo-classical anthropologists, have come to unite as one around the study of life's single most important problem: how to achieve reliable male organ enhancement. ■

Allan M. Lees has been creating stories for his children since they were very little and he will continue to do so until they are old enough to steal a car and escape. Allan's very modest literary success to date includes several published stories, a now-deservedly out-of-print novel, a radio play, and many megabytes of wasted hard-drive space.