AlphaGo Zero shows how business is losing the innovation game

Corporate laboratories once bankrolled basic fundamental research of the highest importance

Undercover Economist

8 HOURS AGO by Tim Harford

It is hard not to be impressed — and perhaps a little alarmed — by the progression. In 1997, IBM’s supercomputer Deep Blue beat the world’s greatest chess player, Garry Kasparov. It was a hugely expensive piece of hardware, closely tended and coached by humans.

Go is a far harder game for computers to master than chess. Yet when the AlphaGo programme emerged with muted fanfare in 2016, it comfortably outclassed the world’s best Go players after a few months of training.

Then last week, the AI research firm DeepMind unveiled AlphaGo Zero. It is faster, uses less hardware, beat its predecessor AlphaGo by 100 games to none, and is entirely self-taught. What is more, it achieved this performance after just 72 hours of practice.

The bewildering progress of AlphaGo Zero has fed an already-febrile anxiety about a robot takeover causing mass unemployment. Yet that anxiety sits uneasily with the high employment rates and
disappointing productivity growth we see in the US and particularly the UK. There are plenty of jobs, but apparently not a lot of innovation.

There are various possible explanations for this paradox, but the simplest one is this: AlphaGo Zero is an outlier. Productivity and technological progress are lacklustre because the research behind AlphaGo Zero is not typical of the way we try to produce new ideas.

Mr Kasparov’s own perspective on this is fascinating. In his recent book, Deep Thinking, he quotes the late computer scientist Alan Perlis: “Optimisation hinders evolution”. In the case of computer chess, Perlis’s maxim describes researchers who chose pragmatic short-cuts for quick results. Deeper, riskier research was neglected. IBM’s priority with Deep Blue was not knowledge, but victory — and victory was a scientific dead end.

That is a shame. Computing pioneers such as Alan Turing and Claude Shannon believed that chess might be a fertile field of research to develop artificial intelligence in more meaningful areas. This hope was quickly sidelined by brute-force approaches that taught us little but played strong chess.

It is easy to see why a commercial company would have had little interest in the early pattern-recognition techniques now refined by AlphaGo. Mr Kasparov describes an attempt to use them in chess; observing that grandmasters promptly won games in which they had sacrificed their queens, the machine concluded that it should sacrifice its own queen at every opportunity.

Yet in the end, these pattern-recognition techniques have proved far more powerful and generally applicable than the methods used by the best chess-playing computers; the question is whether we wish to change our world, or merely win a chess game.

This is not just a cautionary tale about chess. Corporations have reined in their ambitions elsewhere. Corporate research laboratories once bankrolled fundamental research of the highest importance. Leo Esaki of Sony and IBM won a Nobel Prize in physics, as did Jack Kilby of Texas Instruments. Irving Langmuir of General Electric won a Nobel in chemistry. Bell Labs boasted too many Nobel laureates to list — along with Shannon himself. It was a time when companies weren’t afraid to invest in basic science.

That has changed, as a research paper from three economists — Ashish Arora, Sharon Belenzon, and Andrea Patacconi — shows. Companies still invest heavily in innovation, but the focus is on practical applications rather than basic science, and research is often outsourced to smaller outfits whose intellectual property can easily be bought and sold.
Corporate researchers produce more patents but they are less visible in the pages of learned journals. As Prof Arora puts it, research and development has become “less R, more D”. The AlphaGo research, he says, is an exception. And this matters because most basic research ends up being commercially useful eventually. We like the golden eggs, but we may be starving the golden goose.

All this need not be disastrous if other research bodies such as universities fill in the gap. Yet this is not something to take for granted. As the economist Benjamin Jones has documented, new ideas are harder to find. One sign of this is the complexity of research teams, which are larger, full of increasingly specialised researchers and ever costlier.

Perhaps it is naive to simply exhort companies to spend more on fundamental research — but somebody has to. One interesting approach is for governments to fund “innovation prizes” for breakthroughs. Such prizes mobilise public funds and public goals while deploying the agility and diversity of private sector approaches. But such prizes only work in certain situations.

Professional sport has made fashionable the practice of “marginal gains” — rapid optimisation in search of the tiniest edge. It turns out that corporate research took the same turn decades ago. There is nothing wrong with marginal improvements, but they must not be allowed to crowd out more speculative research. Science is a deeper, messier practice than sport. We must continue to devote time, space and money to bigger, riskier leaps.

*tim.harford@ft.com*