#29 On the referendum & #4c on Expertise: On the ARPA/PARC 'Dream Machine', science funding, high performance, and UK national strategy[1]

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'Two hands, it isn't much considering how the world is infinite. Yet, all the same, two hands, they are a lot.' Alexander Grothendieck, one of the great mathematicians.

'There isn't one novel thought in all of how Berkshire [Hathaway] is run. It's all about … exploiting unrecognized simplicities.' Charlie Munger, Warren Buffett's partner.

'He [Licklider] was really the father of it all.' Robert Taylor.

'Computers are destined to become interactive intellectual amplifiers for everyone in the world universally networked worldwide.' Licklider, 1960.

'Class-2 arguments': where both sides can explain the other person's view to the other person's satisfaction (Taylor).

'[M]uch of our intellectual elite who think they have "the solutions" have actually cut themselves off from understanding the basis for much of the most important human progress.' Michael Nielsen, author of Reinventing Science.

'The ARPA/PARC history shows that a combination of vision, a modest amount of funding, with a felicitous context and process can almost magically give rise to new technologies that not only amplify civilization, but also produce tremendous wealth for the society. Isn't it time to do this again by Reason, even with no Cold War to use as an excuse?' Alan Kay, a PARC researcher.

'*I have to live in the same cage with these monkeys*', a professor replies to a plea for help in a bureaucratic war.

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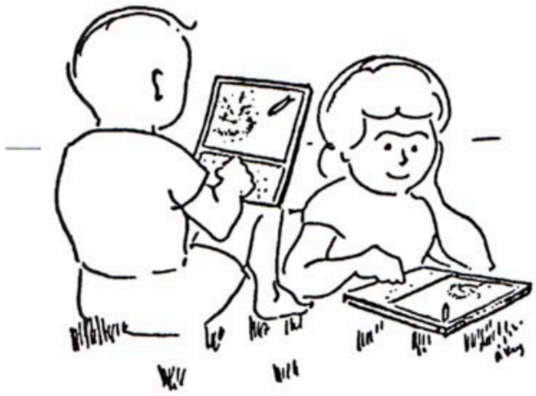
[1] I've given this paper an odd numbering because it fits in both series of blogs.

FIGURE 11.27 Children with Dynabooks from "A Personal Computer for Children of All Ages" [Kay 1972]



Introduction

Post-Brexit Britain should be considering the intersection of 1) ARPA/PARC-style science research and 'systems management' for managing complex projects with 2) the reform of government institutions so that high performance teams — with different education/training ('Tetlock processes') and tools (including data science and visualisations of interactive models of complex systems) — can make 'better decisions in a complex world'.

This paper examines the ARPA/PARC vision for computing and the nature of the two organisations. In the 1960s visionaries such as Joseph Licklider, Robert Taylor and Doug Engelbart developed a *vision of networked interactive computing* that provided the foundation not just for new technologies but for whole new industries. Licklider, Sutherland, Taylor *et al* provided a *model (ARPA) for how science funding can work*. Taylor provided a *model (PARC) of how to manage a team of extremely talented people* who turned a profound vision into reality. The original motivation for the vision of networked interactive computing was to *help humans make good decisions in a complex world*. This story suggests ideas about how to make big improvements in the world with very few resources *if they are structured right*. From a British perspective it also suggests ideas about what post-Brexit Britain should do to help itself and the world and how it might be possible to force some sort of 'phase transition' on the rotten Westminster/Whitehall system.

The story is interesting for a few reasons...

1. It's interesting to look at the few human projects that are *extreme outliers in effectiveness*. What principles characterise great decision-making and high performance organisations? Why do large public and corporate bureaucracies find high performance so hard (e.g why has the British state been unable to cope with Brexit)? This is very relevant to the question: *how to make a transition from normal to high performance government, including much better individual and team decision-making?*

Almost no politicians and officials spend any time thinking about these crucial questions at useful levels of abstraction. (A scientist who read a draft pointed out it would be interesting to consider 'survivor bias' with this — i.e examples where people copied PARC and failed.);

2. It's interesting to look at how *trivial amounts of money spent on edge-of-the-art science/technology research* can create new industries and significantly change the trajectory of human civilisation *if it is spent in certain unusual ways*. The ARPA/PARC budgets were trivial compared to the *trillions of dollars of value* they created (PARC cost ~$15 million per year in real terms). The ecosystem of basic science funding, venture capital, startups and so on that turns science into products and companies actually works in ways that contradict strongly held views of pro- and anti-market politicians. The Left tends to misunderstand the role of decentralised decisions among scientists, entrepreneurs and investors. The Right tends to underestimate the role of patient taxpayer funding of basic science from which specific industries and products evolve, years or even decades later: it likes the principle of 'free markets' but doesn't understand the practicalities of how serious modern science is done and why incentives mean normal companies and VCs are very conservative about funding *technical risk* and tend not to produce things like the internet. Economists tend, like businesspeople and politicians, not to know the history of science research. Interestingly, even many scientists know little or nothing of the history of how ARPA/PARC worked or why it succeeded.

3. It's interesting to consider why the old ARPA approach is so rare despite its enormous success. Why do so few even *try* to learn and copy it? Why did ARPA itself abandon its original operating principles even while it trades on its successes from decades ago to ask for more money? This is a subset of the broader question: why in general do so few even try to learn and copy from the extreme outliers in human performance? This is connected to issues I've written about elsewhere including most obviously *incentives and the transparency of information*: for example, in professional sports, new tactics (and some other performance improvements) are transparent and people are highly incentivised to learn/copy. (Sports, unlike science research, is also embedded in our culture and elitism is embraced by the masses (Kay).) But a counter-example: the Buffett/Munger principles of corporate governance are highly transparent (they write and discuss them constantly) and people are highly incentivised to learn, and there is no disputing their expertise/success — yet almost nobody has copied them. Clearly there are other very powerful incentives at work.

4. It's interesting in the context of Brexit. UK institutions are in the process of experiencing a 'hard reboot' as the legal basis for their operation is radically changed. The bulk of SW1 on *both* sides of the Remain/Leave and party divide has tried to ignore reality but 'in the end reality cannot be fooled'. The parties and officials are having to reconsider many policies and operating assumptions

that they were much happier ignoring. There are therefore many *opportunities* to change the basic orientation of the country and to improve normal government bureaucracies and policies more radically than has happened since World War II. There is also an urgent *need* to replace EU membership with a new national strategy and strong public support for a very different direction to anything offered by the parties and Whitehall. MPs and officials trundle through their days as if 'working normally' is reasonable but the public and the business world know that the same people operating in the same parties and bureaucracies will produce the same results: persistent failure. Brexit only happened because a critical mass realised *the system* has failed and had a sense that '*The fish rots from the head*'. Most of the necessary new thinking will have to come from outside Westminster/Whitehall and somehow be inserted into its institutions, as helpful viruses are now inserted to kill cancer cells.

5. It's interesting to consider how huge breakthroughs occur in computer science in the context of the AI arms race effectively underway. China recently announced that it will invest over $150 billion pursuing world leadership in artificial intelligence by 2030. Many of those right at the edge of this field think that progress may be much more rapid than they say publicly, or than economists and politicians expect.

The ARPA/PARC story connects to the paper I wrote last year on 'systems management'. In the 1950s and 1960s, ICBMs and the space program demanded new approaches to managing very large complex projects, some of which evolved from 'operations research' and the Manhattan Project. These ideas have been largely forgotten in America and Britain — e.g witness the debacle of ObamaCare rollout in America or Universal Credit in Britain which occurred because those in charge had forgotten or never learned lessons from over 50 years ago.

In China there is a focused attempt at the apex of power to combine a) the lessons of systems management from Apollo, which are studied and applied much more intensely there than in the West, b) the lessons from ARPA/PARC about funding science, and c) making China the world leader in data science/AI within 15 years. In the 1970s Russia's military thought more deeply than Washington about the *operational* implications of American technological breakthroughs around computing, stealth, precision strike and so on — but, crucially, Russia could not organise itself to take useful action (cf. Soviet Marshal Ogarkov, about which I'll blog soon). China is trying to think more deeply than Washington about the implications for government of technological innovations that started in America — and, crucially, China *can* organise itself to take useful action. (Cf. *'AI nationalism' by Ian Hogarth* on implications of AI/ML for geopolitics.)

Washington is paralysed. Brussels is focused on the survival of the euro and its 1950s model of bureaucratic centralism. **Post-Brexit Britain should be considering the intersection of 1) ARPA/PARC-style science research and 'systems management' for managing complex projects with 2) the reform of government institutions so that high performance teams — with different education/training ('Tetlock processes') and tools (including data science and visualisations of interactive models of complex systems) — can make 'better decisions in a complex world'.**

This would provide unifying purpose, wealth, security, a better educated society, and a reverse of the long-term stagnation of its governing institutions. Britain could contribute usefully to the world's biggest problems instead of continuing its embarrassing trajectory. It could overcome problems illuminated so brightly by the post-referendum shambles. These ideas do not depend on whether one thinks Brexit a good or bad idea — both perspectives are reasonable.

The hardest thing is not the first-order question 'how to do things much much better'. We *know* how to do scientific research much better and we *know* how to create high performance teams that could do government much better. The hardest thing is 'how to do what we know works really well, based on a few simple principles, when incentives and culture mean *almost everybody with power and money in the parties and bureaucracies will fight intensely to stop you doing things much much better*'. We do *not* know how to overcome reliably this deep second-order meta-problem.

The May/Hammond team is likely to be replaced some time between October 2018 and August 2019 and it is likely that a chunk of Labour MPs will jump ship. There will be a chance for a small group to face reality and change the political landscape with new priorities and a new approach to the whole problem of high performance government.

This paper summarises lessons from lots of books, papers, and interviews particularly drawing on ideas from Alan Kay, one of the participants who has made a big effort to explain the history. It considers the principles behind the ARPA/PARC success, similar principles in the Soviet Union, some ideas about schools/education, and sketches a few ideas for those planning for the post-May/Hammond world.

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'The Dream Machine' and 'Dealers of Lightning'

'Every time I had the chance to talk, I said the mission is interactive computing. I thought this is going to revolutionise how people think, how things are done.' Licklider.

'As a leader of engineers and scientists he had no equal. If you're looking for the magic it was him.' Chuck Thacker on Robert Taylor.

'The designers said, the display? That's crazy, the display is peripheral! I said, No, the display is the entire point.' Taylor.

'The people here are used to dealing lightning with both hands.' Alan Kay.

The two main books on this subject are *The Dream Machine* (TDM) and *Dealers of Lightning* (DoL). I have also read some oral histories and papers by Alan Kay (see end for links).

TDM tells the story of the history of computing from World War II to the 1990s but the heart of TMD is the story of Joseph Licklider, known to almost everyone as 'Lick'. In 1962 Jack Ruina was head of a small new science and technology agency set up as part of the panic over Sputnik — the Advanced Research Projects Agency (ARPA). His main work was missile defence and nuclear testing but he was asked to set up a small project in the obscure field of 'computing' that few in the Pentagon were interested in. He talked to Licklider about his ideas and Licklider explained his vision. Ruina gave him $10 million per year to distribute, at the time the biggest funding block for fundamental computer science research in the world. Licklider had worked in the emerging field of computers since World war II including some of the leading projects of the 1950s such as Whirlwind, Lincoln, and the SAGE air defence system that emerged from the work of Turing and von Neumann during and immediately after the war. (For some pre-history, cf. *The birth of computational thinking* and *The crisis of mathematical paradoxes, Gödel, Turing and the basis of computing*.)

In 1960 Licklider had written *Man-Computer Symbiosis*. More than any other paper this set out the vision for the growth of the modern computer industry. It sketched a vision: '*Computers are destined to become interactive intellectual amplifiers for everyone in the world universally networked worldwide*.' It explored *how humans and computers could work together to maximise their separate skills*. He sketched a series of basic technologies that were needed and this vision guided the community for decades -- in some ways it still does, witness the extraordinary *Dynamic Land* lab created by Bret Victor in San Francisco recently. Even now nearly 60 years later it has a remarkably fresh feel partly because despite all its wonders the industry has still not realised all Licklider's vision. 'Why' is another interesting issue concerning how markets work, what they incentivise people to optimise for, and why, as Kay says, they sometimes take us away from 'what we need' and trap us with 'what we want'.

In just a few years at ARPA, Licklider spread generous funding across American universities to create new groups dedicated to this vision of interactive networked computing. He described a *vision*, provided *patient long-term funding*, created a *community* to develop the ideas, and helped *network* the community with physical meetings as well as sketching what he called 'the intergalactic computer network' which became the

internet. His successors at ARPA — Ivan Sutherland (inventor of the legendary Sketchpad) and Robert Taylor — believed in his vision and continued to fund it. Both had worked on military simulators and display systems which meant both understood his obsession with *interactivity*.

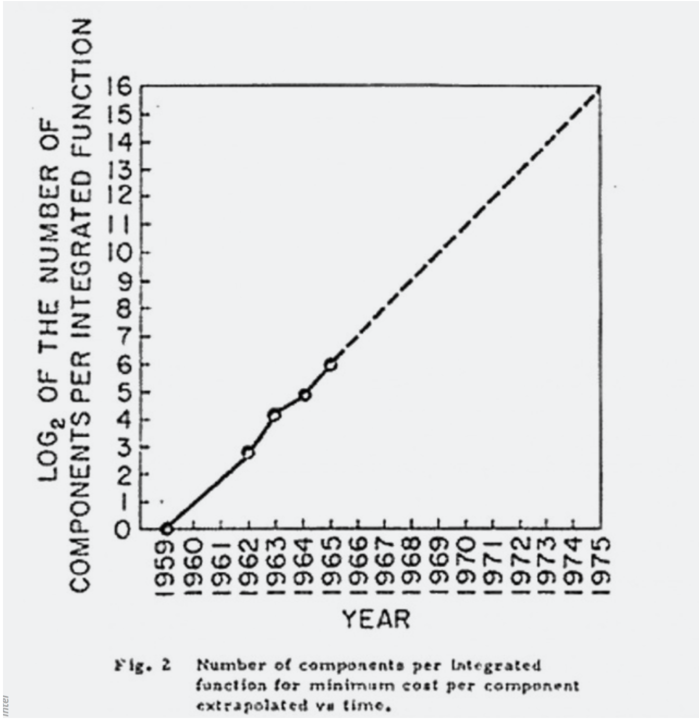
One of those Licklider funded was Doug Engelbart (so coincidentally did Taylor when at NASA). Engelbart wrote '*Augmenting the Human Intellect: A Conceptual Framework*' in 1962. He wrote: 'By "augmenting man's intellect", we mean increasing the capability of a man to approach a complex problem situation, to gain comprehension to suit his particular needs, and to derive solutions to problems.' Like Licklider's 1960 paper Engelbart's had great influence. Licklider read it and sent Engelbart money

> 'Lick was the first person to believe in me. And he was the first person to stick his neck out and give me a chance. In fact, if he hadn't done that, if he hadn't stuck his neck out and given me money, I don't think anybody ever would have done so.'

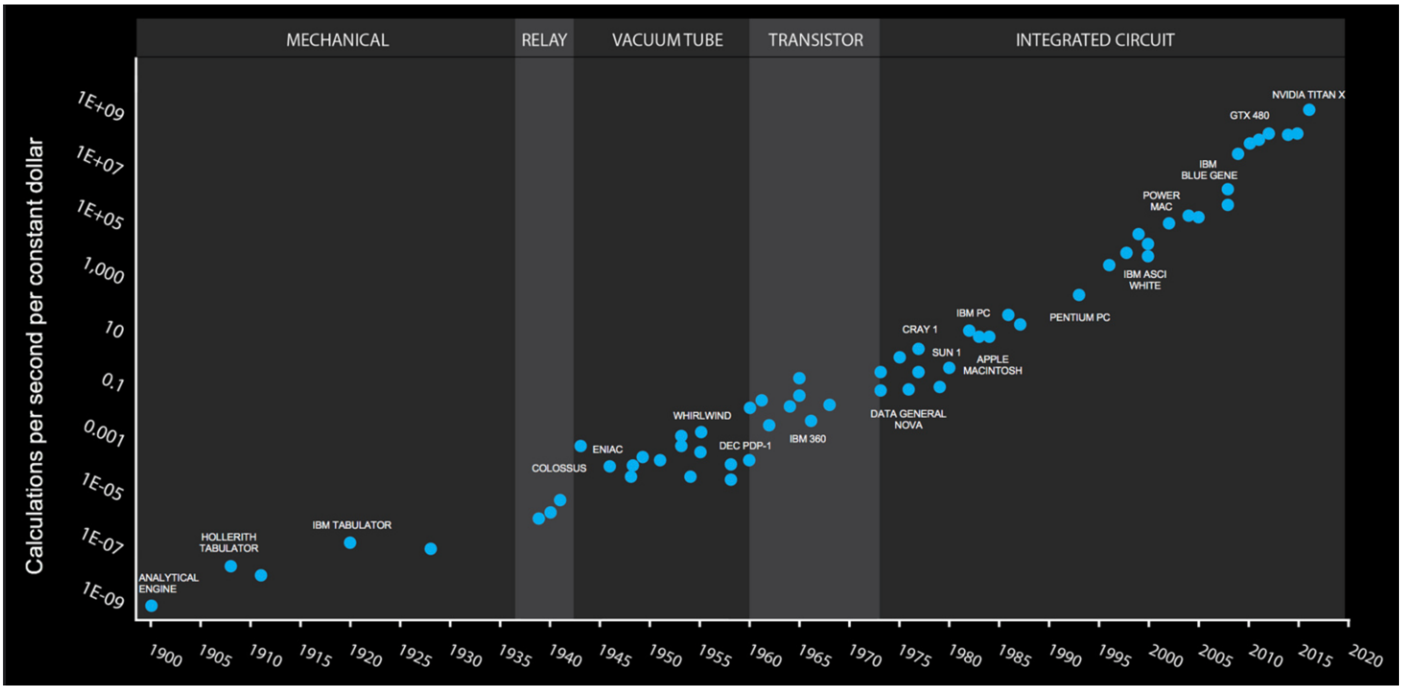
In 1968, Engelbart did a public demonstration of interactive computing including word processing, hyperlinks, windows, the mouse, and even two people editing a document online live. This showed the community that a completely different sort of future was possible and was crucial in the development of the PC over the next decade and was extremely influential. Fifteen years before the Mac it first showed what might happen. The demo cost at least hundreds of thousands of dollars in today's money. Taylor simply committed ARPA to fund it even though it was technically against Pentagon rules. It became known as 'the mother of all demos'. (Cf. this by Bret Victor on Engelbart and this on the 1968 demo.)

Another offshoot of Licklider's community building was a research centre that Xerox set up in Palo Alto called **PARC (Palo Alto Research Center)**. George Pake was chosen to run it and Pake recruited Robert Taylor to run the computer science division. Taylor then went on a recruiting spree to hire the very best people he had seen during his time at ARPA. Xerox chose Palo Alto partly because of the take-off of the semiconductor industry and proximity to Stanford. In 1947 at Bell Labs, Shockley had co-invented the transistor. He moved to Mountain View (in modern 'Silicon Valley') and started a company but he was a lousy manager and eight of his researchers left to create Fairchild Semiconductor in 1957. The 'integrated circuit' was invented in 1958 by Jack Kilby of Texas Instruments and in 1959 one of Fairchild's co-founders (Noyce) discovered how to mass produce ICs by etching thousands of transistors simultaneously onto the surface of a single silicon wafer. By 1967 Fairchild had 12,000 employees and revenues of $130 million per year. In 1968, two of the co-founders, Noyce and Gordon Moore (of Moore's Law), left Fairchild to start Intel.

Moore's original chart for what became known as 'Moore's Law'



Moore's Law updated (logarithmic scale): performance per dollar improves by ~x100 per decade since 1940, so over ~75 years that's a factor of about a thousand trillion (10^{15})



PARC was nearly closed before it had started. Xerox was already in financial trouble and was looking for savings. The embryonic experiment was saved at a Board meeting by the intervention of a director — a

Nobel-winning scientist who told the Board that closing it to save a few million was stupid given 'This is the most promising thing you've got.' It survived. Over ~5 years from 1970-75 the PARC group created:

- the first recognisably modern standalone PC (the Alto),
- bitmap display (i.e a display where the colour of each pixel is programmed, which your PC uses),
- the familiar modern graphical user interface (GUI) with icons, pop-up menus and drop-down menus, scroll bars, overlapping windows etc,
- object-oriented programming and Kay's Smalltalk programming language,
- 'what you see is what you get' word processing (interestingly they did *not* develop a spreadsheet program which, via VisiCalc, was so important for Apple: 'One important idea that would have been a natural application for the Alto was simply over-looked: spreadsheets. Probably this happened because except for the annual budget planning and keeping track of purchase orders, there were no applications for spreadsheets in the research laboratory.' Lampson),
- laser printing (after the guy who figured it out at Xerox HQ, Starkweather, was forced to drop his research and he transferred to PARC),
- the Ethernet networking system (which Bob Metcalfe ('Metcalfe's Law') then developed commercially),
- they contributed to the development of the TCP/IP protocol that is the foundation of the internet,
- and they developed the overall ARPA vision and the ideas of *personal* computing and 'the office of the future'.

'This Alto system did not have a detailed plan, but it was built in pursuit of a clear goal: **to make computers better tools for people to think and communicate**' (Lampson, emphasis added.)

Xerox failed to exploit practically this entire goldmine. After false starts and delays it exploited laser printing which was closest to their core business and which Xerox management understood (it was the only thing PARC produced which involved *paper*). Almost everything else was unexploited until Bill Gates and Steve Jobs applied Microsoft and Apple to developing these innovations. When Jobs accused Gates of stealing ideas from Apple, Gates replied with a famous analogy: that Jobs had broken into their rich

neighbour's house only to find that Gates had stolen the TV — i.e Microsoft had stolen from PARC, just as Apple had done.

Here is a summary of some principles I have extracted from the main books and papers. 'They' means Licklider and Taylor unless it's clearly referring to something else. I have quoted liberally and many of these quotes should be on the wall of anybody with funding responsibilities. My goal is not to explain the detail of how the technologies were developed but to explain *the most important lessons about the high performance processes and organisations*.

A vision acts like a magnet field to align 'all the little iron particle artists'. Licklider set out a long-term and extremely ambitious vision — most science funding is short-term and incremental. He did *not* try to create a detailed 'roadmap' that everybody wanting his funding had to follow. Taylor led everyone towards Licklider's vision of interactive computing.

> '[A] great vision acts like a magnetic field from the future that aligns all the little iron particle artists to point to "North" without having to see it… The pursuit of Art always sets off plans and goals, but plans and goals don't always give rise to Art. If "visions not goals" opens the heavens, it is important to find artistic people to conceive the projects' (Kay).

Kay and Lampson, two of the crucial researchers, discussed the importance of overall vision in the context of the fascinating question — *why has it proved impossible to reproduce the PARC experience*, even with a large subset of the same people.

> Lampson (comparing his time at PARC and later at DEC, also with Taylor after both moved from PARC to DEC with others): **In the PARC world, it was fairly straightforward, because we had this ill formed idea that we were going to have the Office of the Future... Later on, when I was working for DEC, it was a lot less clear cut how the things fit together...** We did a lot of interesting work … but it never really came together into any larger whole. I am not quite sure why that is. My guess is that we were just lucky at PARC… The environment was just as good when we were working for DEC... But **there wasn't any overarching goal to shoot for, at least we never found one**. I think that's pretty much been true since then, as well. So I don't know what to make of that.
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> Kay: Well, Licklider's vision was a great vision, the ARPA dream as a vision….You just have to have a lot of people who work together. PARC was about as good as it gets in my experience. But I think it was so easily held together by the magnetic [field], just lined up all the different iron filings that were individual people at PARC. We were all kind of pointing at north.

Lampson: Yeah, that was cool… **But then it's an interesting question: why weren't we able to do it again?** Because, as I say, I think we did a lot of good things at SRC, but there certainly wasn't any "whole is greater than the sum of its parts" effect.

Kay: From my standpoint I was able to put together groups as good as the one I had at PARC, but I never was next to the rest of PARC. The rest of PARC for us was critical, absolutely critical. We needed to have all you super-smart people doing this kind of tolerant interest and disinterest, it made all the difference in the world for what we were able to think about and try to do. So I think there's critical masses and critical masses, and having the right kind of diversity in these. So I think there is a size below [which] you can't go.

Lampson: I think that makes sense… **The goals seem to me to be the thing that's central.** For example, we could have certainly found an institutional way to collaborate with you guys in the '80s, if there'd been a motivating goal to make that happen. But there never was. I guess the interesting question is: **Was that just a historical accident that isn't going to be repeated?** Certainly Microsoft research [which he went to after DEC] hasn't succeeded in coming up with anything like that.

It is interesting that 'practical' applications often come from artistic visions, such as the Licklider/Engelbart vision for interactive computing, or from esoteric/theoretical ideas, such as the spark provided by the Gödel/Turing abstractions in the 1930s for the start of computer science. Bureaucracies tend to focus immediately on near-term 'applications' that can be managed with conventional processes. They divert attention and funding away from the esoteric and visionary but this turns out to be self-defeating.

A recent UK example of this problem was the 2010-15 government's acceptance of funding changes that diverted more money to mathematics 'applications' and less to the esoteric world of 'pure' maths. Every living British Fields Medallist pointed out why this was foolish but ministers, typically neither understanding or caring, just accepted the decision of Whitehall officials. Many advances in the practical field of, say, cryptography, depend on prior advances in the extremely esoteric world of number theory. You are killing future golden geese if you try to make all your funding 'more practical', but these future golden geese have no voice or vote.

Goodwill. '[W]hen I think of ARPA/PARC, I think first of good will, even before brilliant people' (Kay). This underlies other points below. If you trust people, you do not micromanage them and how they spend money. You focus on 'progress, not progress reports' and so on. Kay's point resonates with many similar statements by Buffett/Munger about their success.

Intensity. It is striking how the ~25 key people recount their PARC experience. In every account I have read, the person regards those 5-10 years as the most intense and creative and fun of their professional lives and unprecedented in terms of their interaction with other extremely able people. There were no office hours but people worked all hours of the day and night without being forced — tales of Los Alamos in 1943 and SkunkWorks in the 1960s/1970s are similar. This sort of experience is memorable but it also involves **sacrifice**. Thacker described how PARC had 'the intensity of a startup' and this meant *sacrifice* — 'I feel very sad that I didn't get to know my first daughter except at the two o'clock [a.m] feeding.' Many of PARC's researchers worked a 30-hour day and would almost live at PARC — Metcalfe, inventor of the Ethernet, held the unofficial record by staying there continually for over two months. It was normal just to work until exhausted, sleep, and continue.

In a famous talk on how to do great research, Hamming described how the necessary work inevitably means sacrificing personal relationships.

> 'The more you know, the more you learn; the more you learn, the more you can do; the more you can do, the more the opportunity — it is very much like compound interest. I don't want to give you a rate, but it is a very high rate. Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime… I spent a good deal more of my time for some years trying to work a bit harder and I found, in fact, I could get more work done. **I don't like to say it in front of my wife, but I did sort of neglect her sometimes; I needed to study. You have to neglect things if you intend to get what you want done. There's no question about this**. [Emphasis added]
>
> 'The steady application of effort with a little bit more work, *intelligently applied* is what does it. That's the trouble; drive, misapplied, doesn't get you anywhere… Just hard work is not enough — it must be applied sensibly… Most great scientists are completely committed to their problem. Those who don't become committed seldom produce outstanding, first-class work.'

On one hand, great teams like PARC or SkunkWorks are what they are because of the intense feelings they spark but this also is clearly a powerful reason why it is hard to repeat — people can only work like this for a limited time and lots of people never want to work like this at all.

'People not projects'. Licklider focused on *funding people*, not imposing his own specific projects, though he did insist on the overall vision of *interactivity*.

> 'Some of the professional bureaucrats were dumbfounded. They had a history of giving grants to individual people in twenty-thousand-dollar chunks. But Lick was talking about millions of dollars and whole teams of people. It was as though these folks had encountered this alien creature: friendly, but strange' (Taylor).

***Great* people, not good people.** 'I really did not care very much what they knew or what they were interested in. My bias was toward very bright people' (Licklider). 'Never hire "good" people because ten good people together can't do what a single great one can do' (Taylor). Steve Jobs took this approach and searched for people like Wozniak who were '50 times better' than a 'good' engineer. PARC's main breakthroughs came from about 25 people according to Butler Lampson who was one of them. Donald Knuth has described PARC as 'The greatest by far team of computer scientists ever assembled in one organization.'

Great *connected* people, not isolated people. They deliberately built a community. 'It was more than just a collection of bright people. It was **a thing that organized itself into a community**, so that there was some competition and some cooperation, and it results in the emergence of a field' (Licklider). Taylor's only strict management rule for PARC was everyone had to come to a weekly meeting so everyone knew what everyone was doing.

Kay talked about the crucial importance of this in the quote above — the 'super-smart people doing this kind of tolerant interest and disinterest' in different projects. Another of the stars, Thacker, described it:

> 'Projects that were exciting and challenging received something much more important than financial and administrative support. They received help and participation… As a result, quality work flourished, less interesting work tended to wither.'

Carver Mead, the legendary Caltech physicist who spent some time at PARC said:

> 'There was a lot more teamwork than in academia. It was about getting things done, not about publishing papers.'

Taylor also gave everybody at PARC a say in hiring new people — hiring was a community decision and taken extremely seriously. Weeding people out also involved the community. Taylor would get everyone to rank people into quartiles and those consistently in the bottom quartile would be eased out by helping them find another job.

> 'People who stay together in a group are motivated by having other good people around them, and they're discouraged if they have to work with someone who isn't quite up to it. So, **if you can**

> **get rid of people who are not so good, the spirit of the place is improved.** You can see it just sort of pick up. The other good thing is it enables you to hire new people. If you don't do this, then after a while, you will arrive at a state where you have no more head count, so there's no way to improve your organization. Thus when you do hire someone, you try to hire them with a conscious effort to make your organization better.' Taylor

This is totally opposite to how government works. The answer to many problems in government is simply that the calibre of people dealing with 'very important problem X' is shockingly low but who will say 'you need to fire (almost) all these people and get fewer but much brighter people on X then give them freedom to work things out without interference from relative dummies', when you know saying this a) will almost definitely not happen but b) will create huge problems for *you*? In politics, the scale of 'smart people' is completely wrong. This is not the place to go into this issue but in essence: *political bureaucracies' and parties' 'HR' processes exclude most of the most able people in society, defend incompetence, drive many of the best out in despair, and promote many of the worst to crucial jobs* where they influence the next generation creating vicious circles that are hard to escape. In general, this is the single most under-appreciated aspect of 'why government is so bad' and generally *the problem is not even recognised* and people in politics wrongly think they are surrounded by talented people 'like me'.

'Great connected people' seems central to the crucial question: *why is it so hard to reproduce efforts like PARC.* There are powerful nonlinearities with talent. If one looks at the very far edge of the tail, people like von Neumann and Grothendieck are different even from other Nobel and Fields Medallists. The great Soviet physicist Lev Landau said that people like Einstein can have ten times the influence of another Nobel Prize winner who in turn can have 10 times the influence of someone who nearly hits this level and so on. If you put people at the edge of the scale together *then have them helping and looking for flaws in each other's work* — like Los Alamos, PARC or Grothendieck's famous seminar — then you create further positive feedback loops.

But some obvious problems kick in. It is very, very hard psychologically for most people in field X to accept that they are not among the 'best' people in field X. How do you have productive conversations about this without causing huge resentments? Who will recruit such people? How many people are comfortable hiring people they know are much more able than them?

These issues are closely connected to bureaucratic cancers.

No micromanagement, bureaucratic cancers treated like the enemy. Licklider did not micromanage those he funded. Ruina, Licklider's boss at ARPA in the 1960s, did not micromanage Licklider.

> Q: Can you tell me something about your mode of operation with Licklider, your interaction with him? How thoroughly involved were you in setting his program, in setting his budget?
>
> RUINA: Not very much. My expectation is only once a month, at most, that I would see him… **[M]y management style was to let people, especially those people that I had confidence in, to leave them alone. That was the case as far as Licklider was concerned**.

Licklider was very demanding and did not tolerate laziness but he never micromanaged:

> 'On the frontier, man must often chart his course by stars he has never seen. Rarely does one recognize or discover a complex problem, formulate it, and lay out a procedure that will solve it — all in one great flash of insight.'

One of the scientists funded by ARPA said, **'They wanted progress not progress reports.'** Ivan Sutherland, Licklider's successor, described the entanglement of quality of person and lack of micromanagement:

> 'The principal thing I learned about that kind of research activity is that the caliber of people that you want to do research at that level are people who have ideas that you can either back or not, but they are quite difficult to influence — that in the research business the researchers themselves know what is important. What they will work on is what they think is interesting and important. You can maybe convince of something's interest and something's importance, but **you can not tell them what to do** and get good research. Good research comes from the researchers themselves rather than from outside.'

Taylor's odd personality was clearly crucial to this coordination problem and the PARC stars give much credit for their success to Taylor. Many described him as a 'connoisseur of talent', someone who was not such a person himself but had a mysterious taste for 'who are the best people'. When Lampson was asked 'what did you think Taylor did?', he replied 'Damned if I know. It's just magic.' This is a peculiar talent itself and perhaps as rare as very high IQ. I suspect that his *management* talent was connected to *not* being a technical genius himself — perhaps this made it easier for him because his own ego was not engaged in the same way it would be if you asked a great physicist to recruit and manage other great physicists.

Kay described Taylor's management approach:

> **'[Taylor's] true genius was in being able to "lead by getting others to lead and cooperate"** via total commitment, enormous confidence in his highly selected researchers expressed in all directions, impish humor, and tenacious protection of the research… PARC was highly concentrated with regard to wealth of talents, abilities, vision, confidence, and cooperation. There was no real management structure, so things were organized to allow researchers to "suggest" and "commit" and "decommit" in a more or less orderly fashion…
>
> 'Bob didn't want people who had to be managed. He liked people who were outspoken, who were very confident, who would argue back with him. And true to his word, he did not meddle with people's technical decisions. His idea was that he was there essentially to manage the personalities, to keep people from killing each other…'

The old ARPA avoided bureaucracy and many turf wars because it did not build its own labs and bureaucracy. ARPA funded other labs. They wanted to build a community *then return to it themselves*, not build a powerful bureaucracy and live in it. They did not have the modern machinery of grant forms in which scientists have to set out detailed plans in advance then have a huge regulatory structure watch their every move which is how *almost all contemporary UK science funding works* with all the inevitable bureaucracy, conservatism, waste and so on (see below). They thought in terms of 'milestones not deadlines' (Kay).

Speed. You can move very fast if you avoid bureaucratic cancers. The famous meeting in which Taylor got approval from Herzfeld (then director of ARPA) to build the ARPANET (forerunner of the internet) took about 20-30 minutes. At the end of it Herzfeld, who had watched von Neumann speak about the future of computers after the war and believed in Licklider's vision, simply told Taylor to get on with it and he could have the million dollars he wanted. **No forms. No budget. No 'roadmap'. No committee. No lawyers.**

The Alto was conjured out of the PARC aether incredibly quickly. After Thacker wrote his1972 memo about a prototype for a new personal computer, which became the Alto, he bet that he could build it in *three months*. Charles Simonyi took PARC's ideas on word processing to Microsoft where he built Word. After seeing the Apple II run VisiCalc (the first spreadsheet) he realised the speed of change outside PARC. He met with the 22 year-old Bill Gates and felt the difference between the dynamism outside Xerox and the deadness at Xerox HQ.

> **'It [Xerox] was like going into the graveyard or retirement home before going into the maternity ward [Microsoft]. I could smell it and feel it. You could see that Microsoft could do things one hundred times faster, literally, I'm not kidding. Six years from that point we overtook Xerox in market valuation.**'

One of the most striking aspects of government is the way in which official timetables are insanely slow and senior officials hate and will bitterly resist attempts to do things much faster. Often one is presented with a timetable 'to do X in Y years' and if you say something like 'we could do 80% of this in three months not three years let's start today' you are seen as a lunatic or an enemy. As Alan Kay wrote:

> '**Because of the normal distribution of talents and drive in the world, a depressingly large percentage of organizational processes have been designed to deal with people of moderate ability, motivation, and trust... [A]dministrators seem to prefer to be completely in control of mediocre processes to being "out of control" with superproductive processes. They are trying to "avoid failure" rather than trying to "capture the heavens".'**

There are *huge* nonlinear returns from speed. If you can speed things up by a factor that seems 'totally unrealistic', even 'mad' according to those in charge of a normal bureaucracy, then you can get huge qualitative performance changes that more than make up for some bureaucratic untidiness (and you also disorientate competitors). This is extremely hard to do for the same reasons it is generally hard to create high performance organisations.

Progress required subverting rules/laws. Although the Pentagon was much less bureaucratic then than now, it still had many rules that got in the way such as prohibitions on soliciting proposals, supposedly to guard against ethics abuses. Licklider ignored these rules, travelled around the country, asked people to write to him and even helped write funding proposals to approve himself. For example, he sorted out the funding for a major MIT project *in a week* and helped them write their own proposal: 'Nowadays, of course, this sort of involvement in a proposal would land Lick[lider] in jail for massive violations of the federal procurement laws' said the head of the project speaking years later. All modern bureaucratic systems are obsessed with processes that theoretically minimise wrongdoing. They usually not only fail to minimise wrongdoing — and in fields like defence procurement they have massively encouraged it — these processes also slow everything down and thereby make everything much more expensive.

My paper on Mueller and Apollo goes into this in detail. One of Mueller's great principles was to prioritise speed and this also saved huge amounts of money, while regulations and budget processes supposedly to 'save money' and 'stop corruption' actually meant delay and more waste. Most government processes are now so infested with lawyers that normal attempts at reform are doomed.

A visible and unhealthy sign of this vicious circle is that the world of intelligence and special forces increasingly relies on completely separate processes to procure and develop things in order to escape the horror that everyone else has to suffer. For example, JSOC (US classified special forces) uses procurement

systems *outside normal laws*. After 9/11, Congress turned a blind eye to many laws being broken because, suddenly and briefly, *effectiveness was more important than process and signalling*. There have been similar experiments recently such as the Rapid Capabilities Office but I don't know how effective they have been. Mueller said that he could never repeat the Apollo program with the legal system of the 1990s and could only do it if the program were 'black'. But the spread of 'black' procurement has its own drawbacks including the way in which officials classify information *not* to protect it from the enemy but *to prevent scrutiny of the fraud, incompetence, and/or waste. What does it say about your procurement system when you have to subvert it to get useful things done without lawyers crushing everything?* (This recent case at UCL is an interesting case study: attempts to prevent abuse waste a fortune and tie everybody up in ludicrous processes.)

If you want big successes, you have to accept failures. The ARPA approach necessarily involves a lot of 'failure' and 'waste'. Trying to minimise failure can make breakthroughs harder. 'It's "baseball" not "golf" — batting .350 is very good in a high aspiration high risk area' (Kay). It's important to remember that we are now completely familiar with the Licklider vision (even though very few know his name) as it is now all around us, but at the time it seemed flaky and foolish even to many experts. This is normal for genuinely new things.

When thinking about how to fund science now, it would be obvious today to put a load of money into, say, AI/machine learning/deep learning. These fields clearly should be funded. But it would *not* be following the ARPA-PARC approach just to shovel more money behind existing projects looking for incremental improvements, particularly when industry is funding this anyway. Funding hot topics is the *obvious* thing to do. It is easy now to say 'let's fund quantum computers' but imagine how hard it was in the early 1980s when Feynman and Deutsch created the field. Funding like the old ARPA now means funding visions that seem as crazy now as the PC once seemed — 'it's just a quirky niche', 'it's a toy' and so on — because things that will turn out to have huge value will be lurking in this space. It's really hard psychologically and bureaucratically to fund 'crazy' ideas and this makes it hard to get support for funding truly original ideas despite all the historical lessons.

Management is a different skill to research. Even phenomenally effective people have very different talents. When thinking about overall systems you need to think about ways to gain from different sorts of talent. Licklider was an extraordinary visionary but a hopeless administrator. Taylor was a brilliant manager of a research team. Some of his researchers said that while Taylor was a genius at managing 'in' and 'down', he was not so good at 'up' and 'out' — i.e up the hierarchy and outside his team. On the other hand, one of his crucial roles was protecting his team from the bureaucracy and this was bound to create tensions with the rest of PARC and between PARC and Xerox HQ.

Creating high performance teams that excel in a hostile external environment almost inevitably requires and creates certain attitudes that will create tension. It is naive to think extreme efforts can be nurtured without causing 'tension' or that Taylor could have achieved what he did without 'making waves'. It is also perhaps just impossible for the person who has to 'make waves' also to manage external diplomacy as well as this could theoretically be done. Indeed, the sort of person who could act as a 'perfect diplomat' for a high performance team with the outside world would inevitably have to make compromises. Taylor himself was a very extreme outlier who seems to have been rarer than a great researcher so it is probably not realistic to think you are going to find people with his skills who also have some further important skill. Nobody is good at everything and not all problems can be solved.

It's incredibly powerful but incredibly hard to structure arguments so people pursue truth, not 'winning'. Taylor's approach to meetings is extremely productive and extremely rare. Almost the only compulsory aspect of the very freewheeling PARC (no 'work hours', no HR, no org chart) was that everyone had to attend one meeting per week (known as the 'Dealer Meetings'). This was 'a weekly meeting for many purposes, the main one was to provide a vehicle for coordination, planning, communication without having to set up a management structure for brilliant researchers who had some "lone wolves" tendencies' (Kay).

Taylor insisted that fierce disagreements always kept to the issues and avoided personality and motive. **Taylor built a culture in which people converted 'Class-1 arguments', in which people don't really understand the other's perspective properly (i.e most normal conversations), into 'Class-2 arguments' in which both sides had to be able to explain *the other person's view to the other person's satisfaction.*** People learned 'to argue to illuminate rather than merely to win' (Kay). As Taylor explained, the combination of hiring brilliant people then getting them to interrogate each other was a hard-to-beat process: 'If there were technical weak spots, they would almost always surface under these conditions. It was very, very healthy.'

In politics, nobody even tries to structure discussions to learn. People don't try to keep track of what is being discussed, what assumptions are made, what predictions are implicit and so on. Political discussions normally bounce all over, from politics to policy to communication to personal anecdote to immediate crisis to distant speculation before being curtailed for people to run to the next meeting without having resolved anything. They rarely end with any agreement even on disagreement. They tend to be just jibber-jabber. Imagine if people tried to structure discussion more sensibly as happens in other fields and combine it with Class-2 arguments and 'Tetlock processes' for auditing decisions.

Problem-*finding* and 'studies into what to study' are crucial. By funding 'people not projects' they created space for people to search for the most important problems. *Problem-finding* is at least as

important as problem-solving and 'the scientists find the problems not the funders' (Kay). Short studies to scope where serious money should be spent were very important at ARPA. Lukasic was head of ARPA 1971-75. In a later interview he described the importance of studies: i.e explorations before you start spending large amounts of money on real programs.

> 'ARPA … also did studies. The studies weren't very expensive, because you can buy a lot of study for a few hundred thousand dollars, whereas it may take you twenty million dollars to build a new telescope or something. **So I always paid a lot of attention to the studies, because the studies guided you to decide how you should spend the other money.** The studies were like the trigger. So if a study said, the problem is in vulnerability of aircraft instead of the standoff weaponry, then that was important, because then you would spend ten million dollars on this subject instead of that subject. So I always paid a lot of attention to the study work … simply because they enable you to make better decisions about alternatives… And so **the studies are ... rather like physics**, actually. They're the thing that enables you to look at the deeper going, less visible mechanisms, so you can decide, these things are happening here and those things are happening there, but it's the studies that enable you to show what the connections are… **One went into studies for the same reason that one went into physics instead of chemistry, because it was getting at the essentials of the thing**, more empirical than theoretical, but that's all right, physics is an empirical science too. So there's really a very close relationship between studies, systems analysis, and physics… **So all of the studies were like a few percent of every office, and I tended to watch those few percent more than the rest of it, because if you got those right, lots of people could spend five or ten million dollars well**.'

JASON was the informal and largely classified network of great scientists that looked at which areas were feasible for research. It operated on the basis of summer studies conducted over a couple of months during academic holidays. Its studies were used to set agendas at ARPA and throughout military funding of science and technology. It is probably not coincidental that, I am told by researchers who have been part of it, ARPA's decline tracks the declining influence of JASON. Whitehall has successfully prevented attempts to create a UK version of JASON. (See some unclassified JASON studies HERE.)

Patience can be rewarded relatively fast. It is interesting that the 'patient' funding provided by ARPA actually produced revolutionary breakthroughs in a relatively short time. Between Licklider's arrival at ARPA in 1962 and the creation of the Alto system by ~1975 is just a dozen years. Think of what might have been if the UK had learned from this story in 2006! Of course 12 years is a much longer planning horizon than that of politicians, parties, and officials, never mind the 20-30 years that some high payoff funding has to envisage. Success requires somehow not just creating projects in the right way but

preserving a dynamic structure that can preserve good work amid the constant turbulence of politicians and officials coming and going. The history of specific projects often shows that some survive or get binned because of little quirks such as one unusual person saving it.

'**Success' needs much more than technical success.** It is interesting that both Licklider and Taylor had a broad perspective on knowledge and research. They did *not* just focus on the technical aspects and neither was a pure 'quant' character. Both realised that it was vital to consider the *interaction of man and machine*, not just focus on machine. Engelbart's focus was **'human augmentation'**.

It is striking that Bret Victor's *Dynamic Land* is in this tradition. When you walk around Bret's lab in Oakland, you think immediately 'Wow wow wow, this is the future.' It is arguably the closest parallel to being able to walk into PARC ~1973-5. It is extremely innovative technically but ***humans*, not computers, are the focus**. A far-sighted UK minister/official should throw a million dollars a year to Bret just for the privilege of occasional access to that building and a chat (see Dynamic Land HERE).

Constraints can be valuable. Success does not come from just throwing money at problems. Often constraints produce success. For example, immediately after PARC started they hit a big problem. Xerox wanted to get into computing (good) but they made a terrible blunder by buying a computer company that was going downhill fast (SDS), a blunder that led to a billion dollar write-off. Taylor's researchers needed their own computer to start work on. They wanted to buy a minicomputer made by a leading competitor to SDS. Xerox management said No Way. To escape the impasse, the researchers decided to build their own computer. This turned out to be an extremely valuable exercise that honed skills later vital for the Alto.

It is also striking how often the most interesting projects work in suboptimal, even awful, facilities. PARC, SkunkWorks, the Los Alamos facilities for the Manhattan Project, MIT's 'Building 20' (where, for example, Bose did early work on speakers using ARPA computing resources) were all the sort of places people just smashed walls down to build something they needed. Great endeavours rarely seem to come from gleaming corporate HQs and it even seems that building such things may be a sign that innovation in the company is dying. *Is it feasible that the unfinished / ad hoc physical environment helps people innovate?* It will be interesting to see if this proves right about Apple and Amazon. (It probably means nothing but it's interesting that Grothendieck would describe 'beautiful mansions' in his writing but he worked in awful rooms.)

Superproductivity requires getting a few big things right at the same time. The approach of focus on the very best people, getting them to self-organise, long-term patient funding, the lack of process/bureaucracy and so on were all connected.

> ‘The key idea was to have a great vision yet not try to drive it from the funders on down, but instead fund “people not projects” by getting the best scientists in the world to “find the problems to solve” that they thought would help realise the vision… ARPA/PARC had two main thresholds: self-motivation and ability. They cultivated people who “had to do, paid or not” and “whose doings were likely to be highly interesting and important”. Thus conventional oversight was not only not needed, but was not really possible. “Peer review” wasn’t easily done even with actual peers. The situation was “out of control”, yet extremely productive and not at all anarchic.’ (Kay)

You’ll fail if you hire the best people then manage them in a ‘normal’ way, or if you trust unmotivated mediocrities to self-organise. ‘Normal’ management will stop you assembling the right people; the right people make most ‘normal’ management redundant.

Successful research has little inherent connection to successful business. You can do great research and fail at business and become the world’s most valuable company without innovating at a deep level or doing ‘the best that we can see can be done’. PARC was able to move so far and fast in its first five years because it was explicitly shielded from commercial pressure. When he took the job as overall PARC boss, Pake told Xerox that there’d be nothing of value for at least five years but if there was nothing of value within ten years it would have been a failure. Taylor insisted on a legal agreement that HQ could not interfere for five years. Five years was enough for that group to change the world but this did not translate into commercial success.

Execution and selling often determine commercial history more than the quality of ideas. Apple and Microsoft were far, far behind PARC in 1980 but Jobs and Gates could make decisions and execute to a very high level while Xerox could do neither.

> ‘We had demonstrated all the ideas of high-speed transistor computers and shown that you could make computers much better than anything done with vacuum tubes by far. And we thought the world would be waiting with open arms for this. But nobody cared! It turns out that it takes more than ideas. You've got to sell your idea’ (Olsen re the pioneering TX-0).

Peter Thiel has made a similar point recently: many startups with great products fail because technical founders don’t focus enough on sales and distribution and they scorn marketing. (Selling the idea is a bigger problem in Britain than in America.)

Interestingly, Bill Gates had enormously profited from PARC but he deliberately set up MS Research to be different. He said he wanted it to be connected to *product development*. This meant he avoided some of the PARC/Xerox problems but it also meant that despite hiring some great people it never came close to

PARC-style breakthroughs and Microsoft missed big things — even huge things like the significance of the internet. If you set something up like PARC, you will generate ideas you cannot use and which competitors might use to attack you, but if you don't then the ideas will emerge elsewhere, including with competitors.

Gates and Jobs were extremely effective in some domains relevant to commercial success but this is not the same as deeply understanding the field or producing 'what people need'. Both missed the significance of PARC's work on external networking — its connection to the ARPANET and work on the internet — and internal networking — the Ethernet connecting the Altos. **It is fascinating that some aspects of the Licklider/Engelbart vision still have not been implemented** — for example, the original NLS system shown by Engelbart in his famous demo is more advanced in some ways than modern systems such as GoogleDocs. Bret Victor gave a brilliant talk on how computing went down particular branching histories from the 1970s that were clearly suboptimal. This is connected to the broader issue that markets are wonderful at discovering and serving *consumer preferences* but this can distort technological development away from more promising paths and *what civilisation really needs* (see below).

'The Right' tends to ignore that the high tech market ecosystem depends on government funded basic science. Politicians, think-tankers, pundits etc on 'the Right' tend to be ignorant of the contribution of government funding to the development of technologies that appear in markets years later. Almost every significant element of things like the iPhone were first developed by basic science funding. VC companies rarely take *technology risk* in the way some simplistic free-marketeers imagine — they take *market risk*. 'Technical risk is horrible for returns, so VCs do not take technical risk. There are a handful of examples of high technical risk … but they are few. Today, VCs wait until there is a working prototype before they fund, but successful VCs have always waited until the technical risk was mitigated. **Apple Computer, for example, did not have technical risk: the technology worked before the company was funded**. Market risk, on the other hand, is directly correlated to VC returns.' (See this great blog about how the VC industry really works, described as the best piece on the industry he's read by no less a VC figure than Marc Andreessen.)

Applications deriving from big advances in basic science — like, say, lasers from quantum mechanics — tend to be unpredicted and largely unpredictable in principle. Restricting focus to 'practical applications' means in practice 'things we can hazily see the outlines of at least today' and this means you are inherently cutting yourself off from wondrous things you cannot see at all and therefore cannot budget for. Labs like Bell Labs, which nurtured many hugely important breakthroughs, have largely been closed because of financial pressures. Those on the Right tend to think of this as a 'natural' product of market forces but we should be asking — *why are we structuring incentives so that we stop making valuable discoveries*? See below for a further thought on the *structure of companies*.

'The Left' tends a) to exaggerate the extent to which market dynamics can be predicted and controlled and b) to ignore how easy it is to kill productivity with bureaucracy. Politicians, think-tankers, pundits etc on 'the Left' tend to ignore the extremely damaging effects of bureaucratic cancers and behave as if a commercial industry can be planned and managed like a small project. Once basic research has uncovered broad possibilities such as the internet or the PC, there are vast numbers of possible commercial applications and many aspects of consumer preference that cannot be predicted and can only be discovered via the process of competing companies. Do people want more powerful and more expensive computers or simpler and cheaper computers? Very smart people had very different answers. Silicon Valley not only nurtured technological breakthroughs, it also nurtured the modern venture capital industry — companies such as *Sequoia Capital* and *Kleiner Perkins Caufield and Byers* — which are critical in the startup ecosystem and a critical bottleneck in Europe. Recently, Paul Graham and Sam Altman created and built *YCombinator* to push this process even further and help startups at the earliest stage to find funding and support networks. Altman has also tried to push funding to hardware startups that he feels the VC industry steers clear of, even though this is riskier. These dynamics tend to be ignored on 'the Left' which also finds the intellectual elitism of things like ARPA/PARC increasingly difficult to cope with.

Funding diversity is crucial. An interesting point regarding the UK and the internet: there were people in Britain also thinking about packet-switching and so on at the crucial period in the 1960s. They could not get funding. The Post Office opposed any work on it. In America most organisations had the same attitude as the UK Post Office but America had ARPA which could ignore and evade established bureaucracies. **Whitehall has consistently blocked the creation of any funding agency such as ARPA (most recently that I know of in 2014/15) and it has also blocked creating a version of America's 'JASON'** (see above.) Given what we know about new ideas in science, it is vital that funding systems are diverse and therefore approach problems with different perspectives.

People often assume that military funding is short-sighted and 'narrow' but in fact military funding in America has often been much more far-seeing and patient than civilian funding. One of those who worked with von Neumann on the very first computers from 1945 said:

> 'Over the years, the constant and most reliable support of computer science — and of science generally — has been the defense establishment. While old men in Congress and parliaments would debate the allocation of a few thousand dollars, farsighted generals and admirals would not hesitate to divert substantial sums to help oddballs in Princeton, Cambridge, and Los Alamos.' Metropolis, 1976.

Ivan Sutherland, who ran ARPA-IPTO between Licklider and Taylor, described how diversity is connected to risk-taking:

> 'Peer review is rather more cumbersome, because it tends not to take those courageous moves. I always felt, when I was in ARPA, that one of the strengths of the U.S. government was that there were **multiple funding agencies**. If I was a researcher with some computer-related research idea, I had three or four places I could go. If I did not get along with ARPA, I could get along NIH or NSF, or with the Army, or the Navy, or whatever. It seemed to me that **a strength of the operation was that there were alternatives**. What I hope is that ARPA will not become hide-bound and tied up in a peer-review mechanism which would make it like most of the other agencies, so that it would be unable to make the courageous moves. I do not know to what extent organizational arthritis is setting in. But I sense that there is quite a lot of it. For example, the number of sole source procurement is way down.
>
> 'The need to initiate programs and then go out with RFPs in a very rather formal way, and evaluate the RFPs in a rather formal way is stifling to individual initiative. If that is true then maybe the thing to do is to take ARPA and turn it into a stable peer-review kind of organization and start something else that is not peer reviewed, so that you have a place where individual leadership can be exercised. **I do not know who in the government worries about that question. I rather suspect that no one does; that what happens happens for a variety of nonstrategic reasons, and arthritis sets in to organizations** because the bureau of the budget says, "Oh, we need a new report this year and annually hereafter," and there's enough fuss from various contractors who didn't get selected that sole source procurements go out because there are abuses to that — thousand-dollar hammers — and Congress says, "Oh, we cannot have that." **On the other hand, when you cannot have that happening there's a lot of other things that you cannot have happening too**.'

The physicist Michael Nielsen has an interesting suggestion concerning diversity — he suggests *giving a proportion of funding directly to scientists to assign to projects (other than their own) that they think are worthwhile*. This would mean a bottom-up process in which many individual decisions would move resources without the incentives or bureaucracy hardwired in existing systems.

Modern funders try to control too much. There are also problems with VC funders having a big role in deciding funding.

> 'The most important difference between the "Golden Age" funders and those of today is that the former didn't confuse responsibility with control -- they were responsible but they knew that the

researchers had to control the choice of projects and methods. The funders of today — most particularly the tech billionaires, but also execs in companies, bureaucrats in DARPA and NSF, etc — think that they have to control. This winds up with bad choices for goals and projects, and bad processes… I think everyone would agree that **making a billion dollars does not qualify a person to play professional sports, nor to be a classical violinist. Nor does it qualify a person to be able to direct fundamental research.** These are all deep skills that anyone with a billion can learn, but if they don't learn them, then having them deep in the loop is a real problem for progress' (Kay).

Further, Kay makes an interesting moral point about tech billionaires and their responsibilities:

> 'Engelbart couldn't get funding from the very people who made fortunes from his inventions. It strikes me that many of the tech billionaires have already gotten their "upside" many times over from people like Engelbart and other researchers who were supported by ARPA, PARC, ONR, etc. Why would they insist on more upside, and that their money should be an "investment"? That isn't how the great inventions and fundamental technologies were created that eventually gave rise to the wealth that they tapped into after the fact. It would be really worth the while of people who do want to make money — they think in terms of millions and billions — to understand how the trillions — those 3 and 4 extra zeros came about that they have tapped into. And to support that process' (Kay).

The importance of new organisations because of the structural failure of existing powerful organisations. Over and over in human history we see the combination of powerful new ideas emerging and existing organisations unable to absorb them properly. In this story, we see powerful ideas emerge in the 1960s and 1970s but existing organisations like Xerox and IBM could not absorb them properly. As so often, it took new organisations and young people. One of the most powerful aspects of the Silicon Valley ecosystem is the way it helps new organisations get going fast, something that has been strengthened with YCombinator. At a larger scale, this is one of the central advantages of the Anglo-American market system: it is decentralised and allows new entities to compete with old. One of the biggest problems with government and public bureaucracies is they are not subject to this pressure and they can keep failing and wasting vast resources.

Jeff Bezos said:

> 'Failure and invention are inseparable twins. To invent you have to experiment, and if you know in advance that it's going to work, it's not an experiment… Companies that don't continue to experiment, companies that don't embrace failure, they eventually get in a desperate position where

> the only thing they can do is a Hail Mary bet at the very end of their corporate existence.' Jeff Bezos, founder & CEO Amazon.

But of course most public companies follow the path of failure — that's why the statistics show that most die within a few decades. It seems to me that there is amazingly little thought about the legal and financial structure that has evolved for companies. Much of it is historical accident. These accidents become 'frozen' in laws and regulations. These then shape incentives and culture. And we see that **big businesses really do an awful job in adapting**. Practically all discussion ignores detailed consideration of **how we could experiment with different types of legal structure that create different incentives and therefore different rates of adaptation and internal culture**. Charlie Munger has pointed out that Britain incubated the Industrial Revolution without modern public companies and he and Buffett have often pointed out how the incentives in public companies are destructive. Nobody listens.

In 1942, Dave Packard — of Hewlett-Packard — attended a Stanford conference and got into a discussion on management.

> 'Professor Holden made the point that management's responsibility is to the shareholders – that's the end of it. And I objected. I said, "I think you're absolutely wrong. Management has a responsibility to its employees, it has a responsibility to its customers, it has a responsibility to the community at large." And they almost laughed me out of the room.'

80 years later conventional wisdom among Insiders is similar but the political context post-2008-crash is different. If these issues are left to the likes of Corbyn while 'conservatives' and others who support the decentralised decision-making of markets just defend the *status quo*, then political disaster is extremely likely. **Free of EU rules, this is another area the UK could usefully experiment with post-Brexit**.

Not even science funders pay attention to this story. It's interesting how little is known about this story even among scientists and *even among science funders*.

> '[A]s far as I'm aware, no governments and no companies do edge-of-the-art research using these principles… **The most interesting thing has been the contrast between appreciation/exploitation of the inventions/contributions [of ARPA/PARC] versus the almost complete lack of curiosity and interest in the processes that produced them**.'

I was recently surprised to hear someone responsible for *billions* of dollars of US science funding under Obama discussing ARPA clearly unaware of the basic history. The few who know something about ARPA tend to think that the modern DARPA is the same as the 1960s ARPA. In fact, ARPA changed significantly ~1970-1975. Partly because of Vietnam controversies, it was banned from investing in civilian technologies. It was renamed DARPA, with 'Defense' added. The 'Heilmeier catechism' that is often quoted today as the reason for DARPA's success, including the internet, was actually introduced by the new director, Heilmeier, in 1975 and was regarded at the time as symbolic of the *abandonment* of the old ARPA system that produced the internet and PC. When Licklider briefly returned in the mid-1970s he was now under Heilmeyer who was very different than the freewheeling Ruina and Herzfeld types. Licklider wrote a 1975 email to some of the community describing the different frames of reference between himself and Heilmeier that captures the problem with short vs long term planning:

> 'In my frame … it is a fundamental axiom that computers and communications are crucially important, that getting computers to understand natural language and to respond to speech will have profound consequences for the military, that the Arpanet and satellite packet communications and ground and air radio networks are major steps forward into a new era of command and control, that AI techniques will make it possible to interpret satellite photographs automatically, and that 10^{10}-bit nanosecond memories, 10^{12}-bit microsecond memories and 10^{15}-bit millisecond memories are more desirable than gold. In George's frame ... none of those things is axiomatic — and the basic question is, who in DoD needs it and is willing to put up some money on it now? We are trying hard to decrease the dissonance between the frames, but we are not making good progress.'

Licklider left ARPA a second time soon after as he disliked the new environment. By 1975 the ALTO existed and Licklider had been massively vindicated in all sorts of ways yet the system did not say — this guy did it his way and has been spectacularly vindicated, let's listen. Instead of the wider system learning, ARPA itself changed.

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The Dream Machine is well written. Alan Kay, one of PARC's stars, says it is the best account of the story. *Dealers of Lightning* is also a good read but I don't know how accurate it is on details. I would have been interested to read more about *the management of PARC by Taylor* and I see that Kay, who thinks TMD is good, said that it 'missed how and why researchers cooperated and coordinated across projects'. To compile the list of principles above I had to find bits scattered throughout TDM, DoL and many other papers and interviews. Kay gave two fascinating talks to Sam Altman's 'startup school' for YCombinator to

explain to aspiring startup founders where everything around them in the Valley came from. I highly recommend them if you are interested in this story (links at end).

TDM is rightly an optimistic story. Licklider was clearly a very rare person — generous and widely loved as well as very talented and, most rare, a genuine visionary. It is entirely reasonable to tell the story this way. But of course there are also dangerous elements in this story. As von Neumann warned just after WW2, science is morally neutral — it can be used for good and evil, and 'for progress there is no cure'. It is interesting that Licklider and Taylor wrote in a 1968 paper:

> 'Life will be happier for the online individual because the people with whom one interacts most strongly will be selected more by commonality of interests and goals than by accidents of proximity.'

In many ways this is true and good but it is also true that this quality of online networks has many negative effects, which has been a greater focus recently (though beware, practically 100% of what you read about Facebook/Trump/elections/fake news/algorithmic marketing and so on is false). Technologies that help like-minded humans network will inevitably be used by the worst parts of humanity. This is for another day and it's no reflection on the book that it ignored this.

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Fields Medallist Alain Connes on maths/science funding: learning from the Soviets and France

I came across an interview with one of the best mathematicians in the world, Alain Connes, who has also worked with some of the greatest physicists of the past half century. He described some aspects of how Soviet mathematics and science and French mathematics are organised much more productively than in America. The themes connect directly to the ARPA/PARC story.

> 'In France we have a marvel which is the CNRS. It's a place where gifted people can get positions that they can keep for the rest of their lives. The main point is that it makes it possible for people like Lafforgue to think for many years about a problem without having to produce n papers per year and apply for an NSF grant. Young people can invest in long term projects which they could never do in a system with a short time unit.
>
> 'You cannot decide beforehand whom will be a Lafforgue and you will almost automatically have other people that will produce very little. It's a rule. It is the price to pay to eliminate this

pressure to write n papers per year which is nonsense in subjects which are really difficult. It takes 5-6 years to learn such a subject and you don't produce anything in that long interval. The French system is extremely efficient in that sense that it gives to some people the ability to work without being constantly bugged by the need to produce a paper. It is totally different from other systems but it is successful. Most of the CNRS researchers in mathematics are very interesting and productive mathematicians…

'**I believe that the most successful systems so far were these big institutes in the Soviet union, like the Landau institute, the Steklov institute, etc. Money did not play any role there, the job was just to talk about science**. It is a dream to gather many young people in an institute and make sure that their basic activity is to talk about science without getting corrupted by thinking about buying a car, getting more money, having a plan for career etc. Of course in the former Soviet Union there were no such things as cars to buy etc so the problem did not arise. In fact CNRS comes quite close to that dream too, provided one avoids all interference from our society which nowadays unfortunately tends to become more and more money oriented…

'I have to teach 18 hours per year on original stuff produced during that year… **Teaching is extremely useful for several reasons**. The first is that you learn to give a talk, the second is that you are forced to check things carefully. It is not an expository talk, it has to be done with all the details. There are other reasons like getting fruitful interactions with the audience. Finally there is no way one can become lazy since it is very demanding over the years to produce each year enough material for 18 hours of original work…

'The US are successful mostly because they import very bright scientists from abroad. For instance they have imported all of the Russian mathematicians at some point… If the Soviet Union had not collapsed there would still be a great school of mathematics there with no pressure for money, no grants and **they would be more successful than the US**. In some sense once they migrated in the US they survived and did very well but I believed they would have bloomed better if not transplanted. By doing well they give the appearance that the US system is very successful but it is not on its own by any means. **The constant pressure for**

producing reduces the "time unit" of most young people there. Beginners have little choice but to find an adviser that is sociologically well implanted (so that at a later stage he or she will be able to write the relevant recommendation letters and get a position for the student) and then write a technical thesis showing that they have good muscles, and all this in a limited amount of time which prevents them from learning stuff that requires several years of hard work. We badly need good technicians, of course, but it is only a fraction of what generates progress in research. It reminds me of an anecdote about Andre Weil who at some point had some problems with elliptic operators so he invited a great expert in the field and he gave him the problem. The expert sat at the kitchen table and solved the problem after several hours. To thank him, Andre Weil said "when I have a problem with electricity I call an electrician, when I have a problem with ellipticity I use an elliptician". From my point of view **the actual system in the US really discourages people who are truly original thinkers,** which often goes with a slow maturation at the technical level. Also the way the young people get their position on the market creates "feudalities" namely a few fields well implanted in key universities which reproduce themselves leaving no room for new fields. [Isaac Barrow made a huge contribution to science by resigning his professorship so that a younger, relatively unknown person could take the position -- Isaac Newton.]

'[Re the problem of some people in the CNRS-type system doing nothing / wasting money] **This you can not avoid anyway, it is a statistical law and if you try to remove the tail of the curve you won't succeed, you'll just shift it**.

'**CNRS is extremely difficult to get into. It's extremely competitive.** But once you made it you can stay for the rest of your life and no real evaluation is performed which has obvious negative aspects. **A system which would be slightly better than the actual one** would be the following: first admit a large number of young people in CNRS for 6 years. Next, after 6 years they all would have to leave CNRS and teach in the university at various levels. Then, and only then, they would be able to apply again to CNRS. They would reenter the CNRS and be given a permanent position only in this second stage where obviously the competition would be fierce. If they would not succeed in the second stage they would just stay at the university where they teach. This could improve the system in putting more emphasis on freedom

for younger people and creating a second filter to diminish the number of people who stay in CNRS and don't produce anything. They would be in universities and teach, which is fine.'

Full text here: http://www.freewebs.com/cvdegosson/connes-interview.pdf

The failure of the Soviet Union's centralised institutions to match the decentralised information processing of competitive markets led many on the 'free market Right' to assume that *all* its institutions were inferior, and/or that its successes in science and engineering can be ignored 'because they lost'. This is a mistake.

The old Soviet Union, and to some extent modern France relative to America, lacked the institutions to create great companies building on great science but that doesn't mean we should ignore how they fund science. *Competitive markets in the Anglo-American tradition would be even more successful if their political institutions provided some funding for maths and science modelled on the Soviet and French institutions, such as experimenting with Connes's idea above, to supplement existing activity.*

There is another element of the Soviet/French approach to maths that we should learn from. When working at the Department for Education I happened to read a book about Perelman, who solved the Poincaré Conjecture in 2003, and his education. This led me to study the 'Kolmogorov schools' and similar schools in France and elsewhere. We therefore tried to engage UK universities in a project to build some experimental schools based on these ideas. Sadly, despite the fact that all top mathematicians are aware of the Kolmogorov experiment, not least because the maths and physics departments of western universities consist to an extraordinary degree of their alumni, they practically all declined to help because of the fear that they would be accused by MPs and the media of 'elitism'. Their response was: 'good luck, I hope you succeed, but I can't personally help and our vice chancellor will run a mile'. There were two exceptions: King's and Exeter. King's only happened because Alison Wolf pushed it through their bureaucracy. It is not 'Kolmogorov' in the sense of being for roughly ~1:10,000 ability pupils (who we can identify quite reliably as shown by the amazing multi-decade *Study of Mathematically Precocious Youth*) but it is for pupils with serious interest and ability in maths and the school is integrated into a serious university department that helps show pupils what real maths and science are.

Regardless of one's views on grammars, **all parties should be able to support the idea of specific schools for 1:1,000 and higher abilities in maths.**

a) This scale of selection does not have the peer effects that people worry about regarding grammar schools.

b) These sort of children often have an appalling time in normal schools and should be treated as 'special needs' just as disabled children are.

c) *We have decades of evidence and many successful institutions globally to show how to educate such children effectively*. The fact that great mathematicians do not *need* to have gone to such schools (e.g Terry Tao) is not an argument against providing the right environment for them.
d) These people produce dramatic breakthroughs that advance knowledge and civilisation. Originality is extremely nonlinear (see above, CTRL+F Einstein). **These children are a precious resource for humanity and we should treat them appropriately.**

NB. The Kolmogorov schools are NOT the 'maths hothouses' they are usually described as. Kolmogorov insisted the pupils read Homer, Shakespeare and so on. This blog explains something about them and includes a comment from an alumnus now working in the UK.

It is also the case that the top 2% of IQ, who can be identified very cheaply even with noisy tests and even at the age of 11-13, should be outside the existing national curriculum and exam system which is clearly unsuitable for them. It involves getting them to memorise lots of facts and practice answering very structured questions instead of getting them to practice thinking deeply about subjects and developing deeper skills, which would be more interesting and valuable.

Further, we know from work by one of the world's experts on statistics and risk, Professor Gigerenzer, that *it is possible to teach children with a broad range of abilities aspects of statistical thinking to a higher level than most doctors now achieve* after years of very expensive education. Maths in schools across the world is based on an old curriculum that undervalues skills in thinking about risk and uncertainty. This was the motivation behind some of the reforms we tried to make 2010-14 (in the UK department for education) such as introducing conditional probability to the National Curriculum. As with every change, those in power, from officials to universities, almost universally defended the *status quo* and opposed anything that could be interpreted as 'teaching some children more advanced skills than they are taught now'.

It is vital to remember that for most people with power in the education system, their priority is overwhelmingly to signal that they believe 'all children should be taught the same thing' — it is not 'enable each child to learn what they can'. Almost nobody with power in education policy cares about what 'useful thresholds for important skills' even are, never mind 'educating the most able'. There is no scientific program to investigate what children with below/average/above intelligence are able to learn. We do know from SMPY that *some children can absorb entire year-long normal school courses in a few weeks* but the mainstream education policy world ignores such research and engaging with it is 'career limiting'.

It should be a post-Brexit UK goal that **risk/uncertainty literacy, defined in different ways for different abilities, becomes as normal in schools as reading**. Near-universal reading used to be a controversial goal. This was achieved. There is no intrinsic reason why we could not do something

similar with basic quantitative reasoning. It would be cheap but politically extremely hard because of the normal forces of bureaucratic resistance.

Changing our culture through relatively cheap educational changes such that most of the population is able to reason quantitatively about problems with skills that are now limited to something like <2-10% of the population (depending how you define some things) is a good in itself and fits naturally with making science and technology a national priority post-Brexit.

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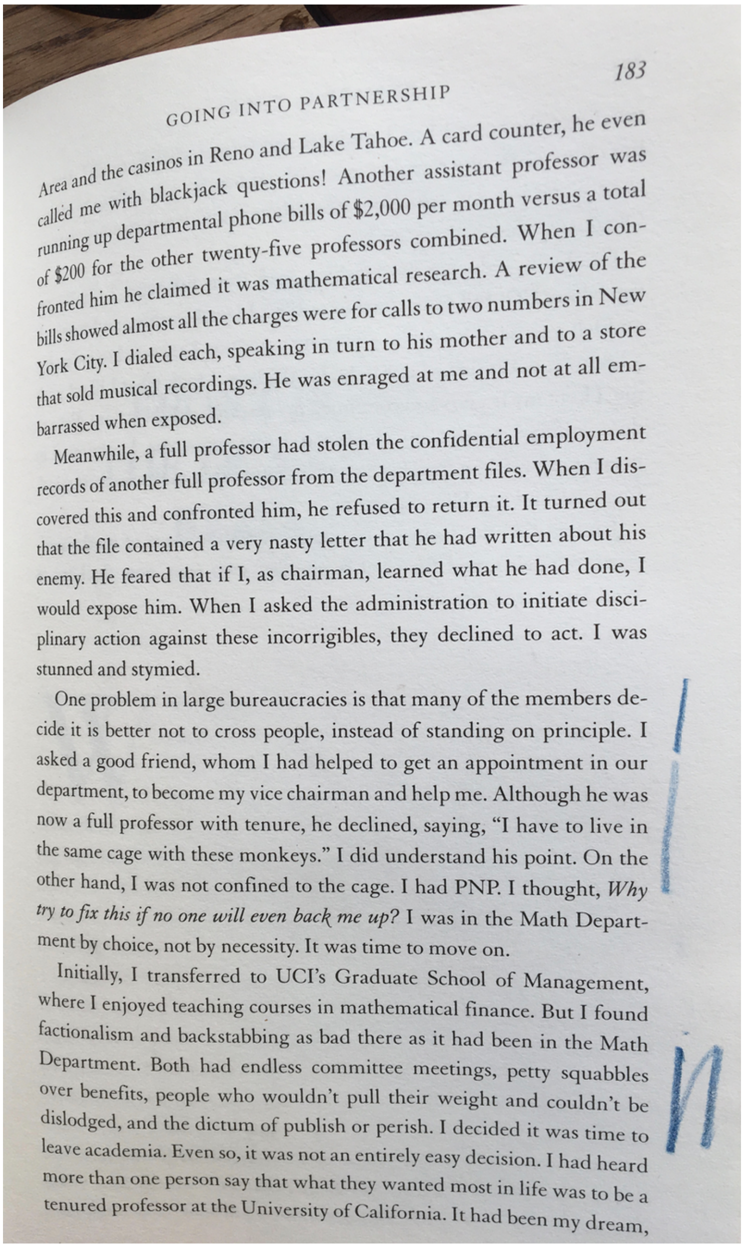
Why don't we learn from success? 'I have to live in the same cage with these monkeys'

An obvious question is -- *given its now-obvious extreme success, why do almost no science funding bodies, particularly in the UK, learn from all this?* And the general question is -- *why do so few organisations manage to learn from the examples we have of extreme performance?*

Successful scientists write reports on science funding and ignore practically all the important lessons. Often such reports are written by prestigious older figures, including Nobel-winners, who do not understand how funding systems really work and their experience is very different to those at the age when Newton or Darwin made great discoveries. Often when scientists turn from their specific expertise to questions involving organisation, they default to bureaucratic orthodoxy either in ignorance or fear of causing offence.

There is a great passage in a great memoir by Ed Thorp, the mathematician who transformed gambling then transformed 'quantitative finance', where he describes trying to deal with academic colleagues when he briefly and unsuccessfully agreed to help get his maths department out of its crisis. The same basic problem of incentive and culture, therefore the same sort of stories, pop up from Whitehall to maths departments. '*I have to live in the same cage with these monkeys*', sums up an awful lot about an awful lot.



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Area and the casinos in Reno and Lake Tahoe. A card counter, he even called me with blackjack questions! Another assistant professor was running up departmental phone bills of $2,000 per month versus a total of $200 for the other twenty-five professors combined. When I confronted him he claimed it was mathematical research. A review of the bills showed almost all the charges were for calls to two numbers in New York City. I dialed each, speaking in turn to his mother and to a store that sold musical recordings. He was enraged at me and not at all embarrassed when exposed.

Meanwhile, a full professor had stolen the confidential employment records of another full professor from the department files. When I discovered this and confronted him, he refused to return it. It turned out that the file contained a very nasty letter that he had written about his enemy. He feared that if I, as chairman, learned what he had done, I would expose him. When I asked the administration to initiate disciplinary action against these incorrigibles, they declined to act. I was stunned and stymied.

One problem in large bureaucracies is that many of the members decide it is better not to cross people, instead of standing on principle. I asked a good friend, whom I had helped to get an appointment in our department, to become my vice chairman and help me. Although he was now a full professor with tenure, he declined, saying, "I have to live in the same cage with these monkeys." I did understand his point. On the other hand, I was not confined to the cage. I had PNP. I thought, *Why try to fix this if no one will even back me up?* I was in the Math Department by choice, not by necessity. It was time to move on.

Initially, I transferred to UCI's Graduate School of Management, where I enjoyed teaching courses in mathematical finance. But I found factionalism and backstabbing as bad there as it had been in the Math Department. Both had endless committee meetings, petty squabbles over benefits, people who wouldn't pull their weight and couldn't be dislodged, and the dictum of publish or perish. I decided it was time to leave academia. Even so, it was not an entirely easy decision. I had heard more than one person say that what they wanted most in life was to be a tenured professor at the University of California. It had been my dream,

Although Xerox set up PARC, its east coast management famously failed to appreciate what PARC developed. In November 1977, six months after Apple had unveiled the Apple II, Taylor, frustrated by Xerox's inability to develop what PARC had done, organised a spectacular demonstration of the Alto for the entire Xerox senior management team. He was not given a security pass to the demo of his own project but sneaked in to watch it. The Xerox managers *wives* crowded around the Altos and played with them while the managers watched. One summed up his impression as 'I've never seen a man type that fast.' The demonstration to the whole senior management did not change the trajectory. Management remained locked into focus on its existing business. There were, of course, all sorts of conversations in which people told senior management about what was possible and what would happen if they did not act and all sorts of attempts by Xerox managers to exploit PARC's inventions.

Carver Mead was a legendary physics professor at Caltech who spent some time working with people at PARC on crucial breakthroughs in chip design. In 1979 he went to Pake who ran PARC and told him that unless he set up an incubator to develop all the amazing ideas at PARC, 'this stuff is going to be all over Silicon Valley and you won't have any part of it… I told them [Pake and Sutherland] Xerox has got to get itself together because there's no way a big company can take advantage of things moving this fast. People will get frustrated and start their own companies.'

Mead was asked to go to Xerox HQ and talk to the CEO and Chairman. He spent a morning explaining what PARC had done. 'I told them, "You'll never have a better shot. If people leave because they don't see anything happening, that'll be like a bomb going off inside PARC. The only question is whether you participate and enable it or let it happen for someone else.' What do you suggest, asked the CEO. 'Set up a venture capital arm. Smell out the technology, find it, incubate it. Take an equity position in things as they happen otherwise it'll be gone and you won't have any part of it.'

At lunch, the CEO told Mead a long story about the way a group inside Xerox had created a great product in opposition to the dominant internal bureaucracy. Mead said, that's a great story. And the CEO said, 'Carver, you don't understand, I personally spend half my time keeping the corporation from killing those guys.' As Mead says about the story:

> **'That's what bureaucracies do, they protect territory… Xerox had become so bureaucratised that it was just totally wedged — and that was why nothing could happen.'**

The CEO actually could see and understand aspects of the problem quite well. He wasn't blind. He wasn't stupid. The problem was taking effective action in the context of *bureaucratic inertia extreme enough to thwart all the powers of a CEO*. As Mead says, the CEO could not manage 'the Herculean task of trying to get this god-awful corporation to be functional, not efficient, just functional at all.' In that sort of bureaucracy, *the interaction of incentives and culture push thousands of daily decisions towards defending existing products and processes and treating new products and processes as threats to existing power and money*. There is a cumulative 'anti-innovation' dynamic in which possibilities are closed, often without any explicit decision, because it's costly for individuals to fight and people gradually get discouraged and leave causing a vicious circle. Mead spelled out exactly the problem and a solution, the CEO understood and was sympathetic and had the power to make big changes, yet the visit did not alter the trajectory. It's much easier to say 'they were stupid' than to consider why big organisations fail to act even when people see the problems.

Eventually Bill Gates and Steve Jobs realised what PARC had done, clutched their heads, ran off and copied some of it. Jobs was taking Apple public in 1979. He was told about PARC by some of his staff. Xerox

wanted to invest in Apple. Jobs demanded access to PARC in return. After disputes, Xerox senior management in the east ordered PARC to show Jobs and his team everything and he was shown many elements of PARC's creations that had never been shown to anybody external. Larry Tesler, who later joined Apple and who had good links with the hobbyist community from which Apple had sprung, said, 'They asked all the right questions and understood all the answers. It was clear to me that they understood what we had a lot better than Xerox did.' Bill Atkinson, one of the Mac developers, said, 'Knowing it could be done empowered me to invent a way it could be done.' Jobs was amazed. 'You're sitting on a gold mine. I can't believe Xerox is not taking advantage of this' he said. Later he described it as 'like a veil being lifted from my eyes. I could see what the future of computing was destined to be.' Some of what they were shown ended up in the Mac, shown to the world in Ridley Scott's legendary SuperBowl ad in 1984. It is also worth noting that Jobs was so shocked by the graphical user interface that he didn't realise he was being shown two other big ideas — Ethernet networking and object-oriented programming. (Untypically he admitted his error, sort of, a decade later.)

Apart from laser printing, Xerox couldn't exploit the TRILLIONS in potential value that their investment had created and senior managers turned on the person who had made PARC work. After years of wrangling, Taylor was forced out of PARC and many of the best people followed him.

> 'They hated [Taylor] for the very reason that most companies hate people who are doing something different, because it makes middle and upper management extremely uncomfortable. The last thing they want to do is make trillions, they want to make a few millions in a comfortable way' (Kay).

Taylor could not get Xerox to develop PARC's inventions into successful products. This seems to be at least partly because he was simply not really interested in that — he was interested in having the very best people push the very far edge of performance, the *research* rather than commercial *development*. Larry Tesler, a PARC veteran who accepted Jobs' offer to move to Apple tried to persuade Taylor that simply having the best research group would not keep PARC at the forefront.

> Tesler: I've been talking to people at Apple and hanging out in the personal computer scene. There's a lot of smart people out there who are going to run way ahead of PARC in PCs. Xerox will never catch up, even with better stuff.
> Taylor: No that's not going to happen because we have the smartest people here. I believe if you have the smartest people you'll end up ahead.
> Tesler: Bob, I've met people outside. They're very smart in this place, no question about it, but there are smart people who don't work for PARC. They do exist.
> Taylor: If you find someone as smart as the people here, just tell me who they are and we'll hire them.
> Tesler: It's not going to work like that.

Overall US science funding has moved in the *opposite* direction from ARPA-PARC. ARPA itself moved in the opposite direction (see above) even as it uses its creation of the internet to lobby for more money. NASA abandoned the successful 'systems management' approach of George Mueller that put man on the moon and reverted to being a normal government bureaucracy with normal budget/schedule overruns and fatal accidents that they failed to learn from. Almost no — perhaps actually zero? — public companies in the US or UK have copied the corporate governance principles of Berkshire Hathaway and not a single politician I'm aware of in the UK has made the case for learning from their very different approach even though Buffett and Munger are the most successful investors in the history of the world and have repeatedly explained their operating principles publicly. There are many, many such stories.

Questions: for Xerox to have behaved differently, what would have needed to be different?

- Would it have taken a CEO coming in like Jobs in his second turn at Apple who slashed a huge amount of existing projects and fundamentally simplified and reoriented the company?
- Are such developments hugely path dependent on odd individuals — i.e in >99% of possible branches of history Xerox was doomed and only some odd fluke of picking, maybe by mistake, a very unusual CEO could have saved them?
- Are our examples of extreme performance outliers fundamentally dependent on personalities that are as rare or rarer than +4 standard deviation IQ (>160) -- i.e on the order of 1:30,000 globally, so maybe ~1-200,000 such individuals world wide?
- Is this why the bureaucracy always has the last laugh, and why Robert Taylor or George Mueller are almost guaranteed to be succeeded by worse people?

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A sketch of what to do?

Occasionally a gloomy minister, official, adviser or analyst treks from SW1 to discuss science funding. We chat over lunch. At the end they ruefully say, 'Of course nothing will happen.' Why? The answer is always (regardless of the individuals) one or more of:

> 'Obviously officials HATE suggestions about an ARPA or any attempt to tackle the big problems with the funding system… Officials have got the minister under control, he won't fight… The Cabinet Office / Treasury will veto anything that sounds like ARPA, they HATE everything about it... No10 doesn't care about science — or anything long-term… Almost no MPs understand why it's important so there's no Parliamentary support... The science community won't back us up if we try — they know the existing system is awful but won't support radical changes because they fear it threatens existing funding and

the community is represented by the most senior people who benefit most from the broken current system.'

This last one is the least important. Partly it's just standard bureaucratic conservatism of funding bodies. Partly it's a function of the dominance of older more powerful people who disproportionately gain from the current system at the expense of the young and those with new ideas. Partly it's connected to the broken academic pipeline in which young scientists have to work as extremely underpaid junior servants for senior people, often exploited and without a voice. Partly it's a lack of understanding that most ministers and officials respond to the language of pressure, not the language of reason.

Partly it's a lack of moral courage. I've found in my own dealings with scientists that for every hundred who complain about X in private, very few will make a *sacrifice* to change X. An example of a hero: a professor of theoretical physics at Cambridge, Mark Warner, who risked alienating the establishment by describing publicly the collapse in standards in Physics exams for 30 years and by trying to build something to give poorer pupils resources for a serious physics education — cf. Isaac Physics, which is now flourishing. *Are scientists with the moral courage to risk career problems as rarer as great researchers? How important is this?*

I am confident that this problem would be easily dealt with IF one could somehow solve the prior problem, that is, having *a government that actually cares about science and progress* and has the will and competence to overcome institutional forces of conservatism. Precisely because the world of funding bodies and committees is so conformist and conflict-averse, and because moral courage is so rare, such a government would find little resistance to a serious plan. All those who now whinge but won't act would soon fall into line albeit for the worst reason — they are psychologically programmed to support government publicly.

Here are a few basic ideas concerning **a systematic change to policies touching on the ecosystem of basic science research, technology development and commerce**.

1) Basic science research and the creation of new industries, as per the ARPA/PARC vision and process, should be central to the UK's post-Brexit strategy.

2) There should be radical changes to the structure and degree of UK science funding to support this priority including ARPA/JASON-like institutions funding PARC-like visions in fields such as machine learning, robotics, energy, neuroscience, genetics, cognitive technologies (such as the work of Michael Nielsen and Bret Victor), and, crucially, *funding what now seem 'crazy' ideas* just as the internet and quantum computers seemed 'crazy' before they became mainstream. We should create new CNRS-style institutions or get existing excellent centres to experiment with the CNRS

funding model (based on Connes' idea above). We need entities that shift the balance of funding from older established figures defending bureaucratic empires to brilliant young people.

There are other reforms to the science funding structure that are clear. They were set out recently by two young British neuroscientists (read their whole piece here.):

- Remove a lot of the bureaucracy -- like multi-stage procurement processes for buying a lightbulb. 'Rather than invigilate every single decision, we should do spot checks retrospectively, as is done with tax returns.'

- 'We should return to funding university departments more directly, allowing more rapid, situation-aware decision-making of the kind present in start-ups, and create a diversity of funding systems.'

- 'We must take decisive steps to make the UK a magnet for talented young scientists... An increased focus on this goal, alongside simple steps like long-term funding and guaranteed work visas for their spouses, would go a long way.'

3) There should be a systematic improvement in the ecosphere of school curricula, universities, venture capital, high skilled immigration policy, planning policy, tax policy, the structure (and incentives) of public companies, intellectual property law (which in important ways supports rent-seekers and undermines innovation and is particularly badly understood by politicians/officials), and so on, combined with a systematic attack on the rent-seekers that both parties suck up to. We could play an extremely valuable role as an experimental testbed for scientific regulation outside all three major blocks (USA, EU, China) without having to obey awful EU rules like GDPR (which Whitehall obviously want us to promise to keep forever).

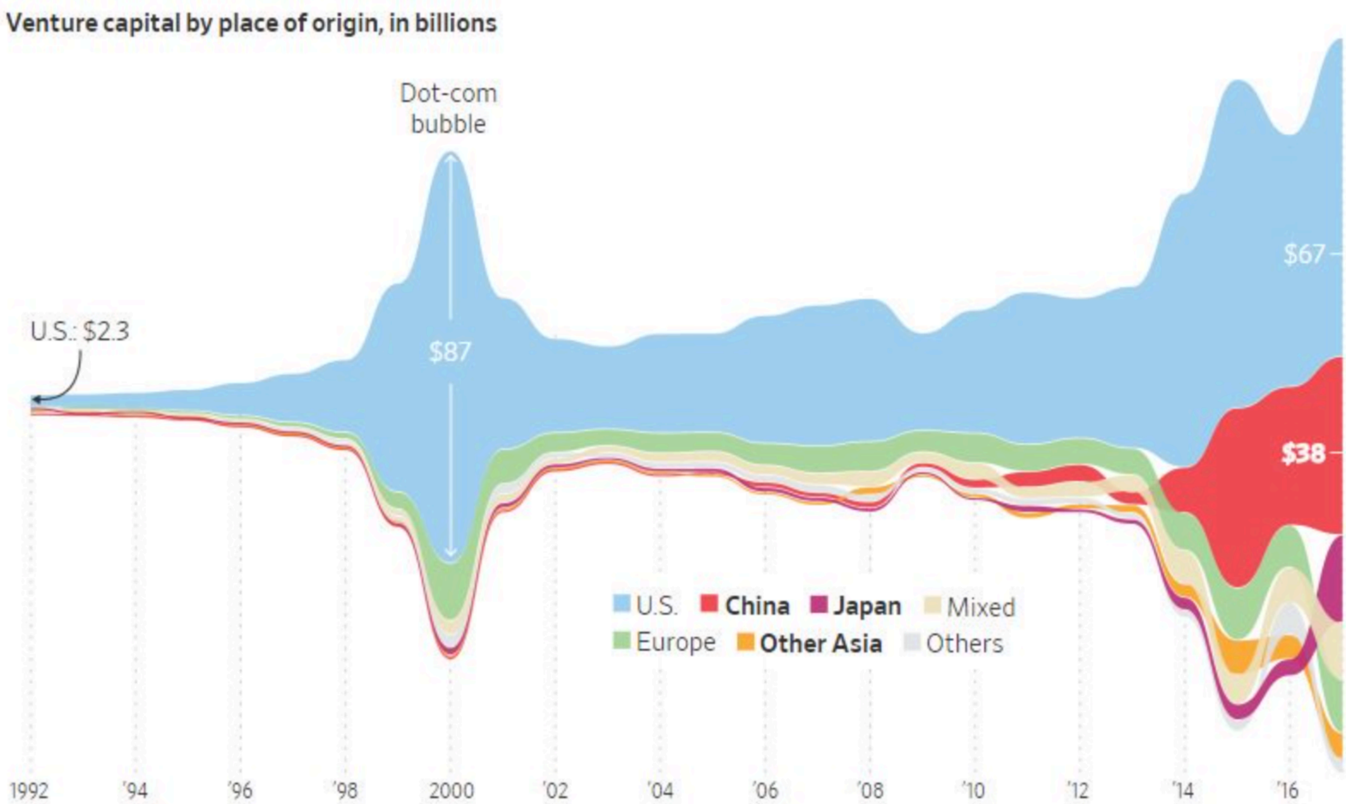
The Silicon Valley ecosystem evolved because of, *inter alia*, a combination of the accident of wartime work, the embryonic electronics industry, farsighted decisions by Stanford and ARPA, particular regulatory and incentive decisions and so on. China is evolving similar ecosystems. Coastal America and coastal China are now responsible for a remarkable fraction of innovation worldwide as Europe falls behind in most technology fields and regarding the overall ecosystem.

We need to improve the individual elements of this ecosystem and think hard about the connections and interdependencies. We have a great advantage over the rest of Europe with our universities but we are squandering this asset. We also have a strategic advantage over America in that London has a very dense network linking AI/data science startups, a world leading finance

sector, a world leading media and communication sector, world-leading scientific research, and government — sectors that in America are divided between California, Chicago, New York, and Washington. Our politicians and officials are barely aware of this asset and its dynamics. Whitehall cannot 'direct' this network. It can fund the bits that only governments can do, change regulations, build infrastructure, allow free movement for some skills and so on and overall provide a guiding and protecting hand for the ecosystem which must evolve largely bottom-up.

Competitive markets optimise for evolutionary niches, rather than necessarily for what is actually needed, and this means we need patient experiments with different incentive systems. We need more deliberate experimentation with incentives. Many areas experience chronic failures because there is no way for people who see problems to fix them. We should try to create more '*exploitable markets'* so that people can profit from predictive accuracy and thereby improve the overall system: e.g if there were ways for people to short certain financial instruments (e.g house prices), then individual incentives would be well-aligned to improving social outcomes (e.g lower volatility).

> 'Really fundamental inventions are too large for single human organizations to deal with -- what one wants is a reasonable balance between what is freely shared, and how things can be made from them. Finding these balances in business has always been a challenge. It seems very difficult for most business people to notice the amounts of larger cooperation and sharing needed for them to be able to do any business at all' (Kay).



4) Industrial society aimed at near universal literacy over a century ago. **Our technological civilisation should aim for very widespread literacy in quantitative reasoning about risk and uncertainty (>2/3 of the population?)**. Now, three-quarters of MPs cannot answer a very basic probability question like the odds of tossing two heads in a row. Performance is so low and ignorance so vast that there is a lot of low hanging fruit. It has been shown how children can master statistical reasoning to a far higher level than the normal politician and even better than expensively trained doctors who mostly fail quite basic tests of statistical expertise related directly to their core work (see above). We know a lot about how to teach a 'Maths for Presidents' style course, along the lines of Fields Medallist Tim Gowers's ideas, to different abilities. Shifting from a culture in which the ability to think about science is very rare to a culture in which basic tools for thinking scientifically are normal seems ludicrously ambitious given the state of politics but is no more ambitious than 'dreams' of previous generations. It is hard to see how we can make major changes to national government and international cooperation without it. It also fits with one of the original goals of the ARPA/PARC vision — giving children tools to learn and think about complex systems. At the very far end of the ability spectrum, we should invest in the Soviet idea of 'Kolmogorov schools'.

5) We should seek to **develop true expertise and high performance teams** at the apex of government power. This means combining *inter alia*: the lessons from the IARPA/Tetlock project about good decisions (including prediction tournaments) to improve the quality and speed of error-correction in forming and implementing policy, 'systems management', the principles of high performance, and new tools such as tools to *visualise interactive quantitive models of complex systems* in line with Bret Victor's ideas for 'Seeing Rooms' and 'Dynamic Land'. It also means the systematic destruction of the 'human resources' system underlying the modern civil service. This system guarantees failure, waste and danger.

What to do is broadly clear. Obviously a full program will involve many complex details but unlike in many areas we do already know the overall framework to aim for. As in so many areas, the hardest question is not 'what is to be done?' in terms of the basic issue but — '*how do we do it given the overall system incentivises people to ignore or prevent sensible policies and high performance?*' And — '*how to do it when political parties are not incentivised to care about important long-term things like science funding?*'

Like Xerox management we can see the problem — but can we find a way to act?! One of the central things to keep in mind about government is this: global nuclear war is about as bad as it gets yet incentives mean that governments, parties and bureaucracies do not even take issues of nuclear safety and accidents seriously. One always has to ask: *given they don't take global nuclear war seriously, how likely is it*

they will take X seriously? This meta-problem lies behind the whole question of 'how to shift from normal government towards high performance'.

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Post-Brexit Britain: an irresistible force and an immovable object

The referendum has blown up the UK Establishment's national strategy that emerged post Suez. It is an irresistible force that demands new ideas and new ways of doing government and politics.

On the other hand, Westminster and Whitehall are doing all they can to ignore this. They have largely tried for two years to keep their hands over their ears and not listen to the demand for change that the referendum embodied. The public, *far beyond the 52% of Leave voters*, wants big changes to our economy and politics but the changes they want do not fit into 'right' and 'left', hence partly why the parties are paralysed. (For example, there is much greater support for *more* high skilled immigration to support science and the NHS, *including among Leave voters*, than for 1) 'pull up the drawbridge' (~15%) or 2) 'keep the current system' (~30%).) The civil service's reputation has been undermined among some important audiences but officials have successfully (and in many instances reasonably) blamed the MPs for most of the shambles. The modern Whitehall tradition of ignoring/undermining British science regardless of who controls the government has continued. As far as reform of the system itself is concerned — the rules for the civil service and the parties — they are trying their hardest to be an immovable object.

Some have taken this so far as to argue that Brexit should simply be cancelled and the referendum ignored while others think the only way to deal with the irresistible force is to create an equally sized counter-force with another vote. Even if Westminster collectively slits its own throat by trying to cancel the referendum this would only divert this force, not quell it.

Britain should leave the EU members and Brussels to focus on the survival of the euro. We should, like California and coastal China, look to the future. If we do, we will soon not only be safer, more prosperous and more advanced than the EU — we will also be able to contribute meaningfully to humanity's biggest challenges. Debates over the EU's Single Market will soon seem nearly as parochial in the UK as they do when sitting in San Francisco. The EEC was created to deal with coal, steel, agriculture and industrial tariffs after World War II. The Single Market came into existence in the 1980s to regulate product markets in the pre-internet world. In Asia startups like Grab (car sharing / mobile payments) are transforming the economic landscape (cf. this interesting piece) but officials and MPs resolutely ignore the future that is, unevenly distributed, appearing. Brexit is the chance for a reboot of this mentality.

Remember 1) how small the resources are that can produce world-changing effects *when structured properly* — and 2) that we already know how to do this. 'All' that stands in our way is what Buffett calls 'the institutional imperative' but remember: 'two hands are a lot…'

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PS. Some EU angles

Could we greatly improve science funding without leaving the EU?
Yes.

Can we participate in the EU's science programs after leaving the EU?
Yes. Switzerland and others do already.

Do EU rules cause significant problems?
Yes. Much of the EU's science funding is criticised by scientists. E.g requirements to spread money around for political reasons often stop money being focused in centres of excellence. The bureaucracy is hideous. In some countries there's a lot of corruption, and so on.

Do EU procurement rules cause significant problems?
YES. From building schools and hospitals to defence procurement to creating a new civilian ARPA, EU procurement rules are a nightmare. They greatly strengthen the power of big powerful companies and make it practically impossible for SMEs even to APPLY for many projects. This issue gets no attention because SW1 gave up years ago on this whole field. We spend >£200 billion every year on procurement and contracting but SW1 pays absolutely no attention to how it is done. Officials are desperate not to change — they have a cosy system with a small number of big companies. When Cameron engaged in his big Potemkin review of EU powers, the issue of procurement was very deliberately left out. Whitehall was determined not to encourage MPs to think about it.

Are UK's public procurement problems all to do with the EU?
Obviously not. Another major problem is Whitehall gold plates EU rules to make things even worse and bitterly fights to stop sensible improvements. I had experience of this in the Department for Education. No Secretary of State has the power to change this — the system is far too entrenched. It could only change if the PM and Chancellor want to. Cameron/Osborne had and May/Hammond have zero interest.

Are we preparing to do procurement much better outside the EU?
Obviously not — quite the opposite. Officials want to ensure that the UK promises to stay INSIDE the awful EU system after we leave. If they have their way we will promise to follow the EU's rules forever. If a

SoS tries to start a discussion about post-Brexit procurement the Permanent Secretary squashes it. No surprise. In the entire shambolic post-referendum debate this issue has been almost totally ignored.

Could we do science regulation better outside the EU?

Yes. The EU recently introduced its Charter of Fundamental Rights. This is much misunderstood. It is separate to the ECHR but it recreates many of the same rights *in EU law with the ECJ in charge*. This gives the ECJ carte blanche to seize control of practically any aspect of science and technology regulation. From AI to genetic engineering to cryptocurrencies, it would be crazy to let UK science and technology be controlled by the ECJ interpreting the Charter over the next 50 years. It would potentially be extremely damaging to innovation and safety. It will be far more dynamic *and safer* to take back control of all aspects of science regulation. With our advantages (universities, language, legal system etc) we could play a beneficial role avoiding the problems of US, EU and Chinese regulations. (This is one of the main flaws in Ivan Rogers' otherwise accurate speech recently.)

Does the referendum mean we have to leave EURATOM?

Obviously not. This is one of the most stupid decisions of the Government.

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Further Reading

The Dream Machine.

Dealers of Lightning.

'Sketchpad: A man-machine graphical communication system', Ivan Sutherland 1963.

Oral history interview with Sutherland, head of ARPA's IPTO division 1963-5.

This link has these seminal papers:

- *Man-Computer Symbiosis*, Licklider (1960)
- *The computer as a communications device*, Licklider & Taylor (1968)

Watch Alan Kay explain how to invent the future to YCombinator classes HERE and HERE.

HERE for Kay quotes from emails with Bret Victor.

HERE for Kay's paper on PARC, *The Power of the Context.*

Kay's *Early History of Smalltalk*.

HERE for a conversation between Kay and Engelbart.

Alan Kay's tribute to Ted Nelson at "Intertwingled" Fest (an Alto using Smalltalk).

Personal Distributed Computing: The Alto and Ethernet Software1, Butler Lampson.

You and Your Research, Richard Hamming.

AI nationalism, essay by Ian Hogarth. This concerns implications of AI for geopolitics.

Drones go to work, Chris Anderson (one of the pioneers of commercial drones). This explains the economics of the drone industry.

Meditations on Moloch, Scott Alexander. This is an extremely good essay in general about deep problems with our institutions.

Intelligence Explosion Microeconomics, Yudkowsky.

Autonomous technology and the greater human good. Omohundro.

Can intelligence explode? Hutter.

For the issue of IQ, genetics and distribution of talent (and much much more), cf. Steve Hsu's brilliant blog.

Bret Victor.

Michael Nielsen.

For some pre-history on computers, cf. *The birth of computational thinking* (some of the history of computing devices before the Turing/von Neumann revolution) and *The crisis of mathematical paradoxes, Gödel, Turing and the basis of computing* (some of the history of ideas about mathematical foundations and logic such as the famous papers by Gödel and Turing in the 1930s)

Part I of this series of blogs is HERE.

Part II on how George Mueller used 'systems management' to put man on the moon, and a checklist of how successful management of complex projects is systematically different to how Whitehall works is HERE.