A predictive model for future inventions

By Dr. William Burns - Science Think Tank

Summary

Can we design a machine that creates successful inventions?

This page picks up this impossible-sounding question. Leveraging dormant Latourian and other under-specified science and technology studies agendas, I illustrate how we might synthesise our knowledge of science and innovation with a view to assembling its objects in advance (rather than disassembling them in retrospect).

The proposed output of this synthesis – a mathematical game that can reliably predict the future of technology.

In my view, the AI model of training algorithms to mimic the human discovery process is not the only means towards this goal; could we not also use our formal knowledge of science and innovation to design such an algorithm? How many useful inventions came from mimicking nature and indeed, how many came from mimicking humans?

This project is a long-running idea going back in my mind to at least 2009, which I occasionally return to with the hope of eventually distilling an argument (and a model) over the next few decades.

Introduction

Can we design a machine that creates successful inventions? At the least, can we design a machine that reliably identifies inventions that will succeed in future?

In this article I will investigate how the science and technology studies (STS) community could build on more than 40 years of analysis to make a significant intellectual intervention in this sphere, one that they have not made so far despite being well placed to do so.

My central argument is for a shared commitment to build quantitative models that gamify STS and thereby answer crucial questions about the future of technology.

I emphasise quantitative and I mean it seriously because it puts to work the physical metaphors the field already used to talk about the future such as momentum, landscapes, diffusion, systems, networks, and so on, now playing out, of course, in a contemporary capitalism obsessed with algorithms.

The models I propose would not share many similarities with the ideas of technology foresight (Miles, 2010). They would, however, form part of long-standing scientific traditions that seek to understand the future, which Rindzevičiūtė dubbed ‘the will to predict’ (Rindzevičiūtė, 2023, 1-14).

Besides the potential practical value of a gamified version of STS, were it to exist, it would serve to answer what could be read as the late Bruno Latour’s prescient call for a synthetic theory of science, technology and society, near the start of *Give me a laboratory and I will raise the world*, 41 years ago.

‘The time has now come for the analysts of scientists at work to deal with the naive but fair criticism put to them by scholars interested in ‘macro’ issues. But there is of course no way that we can easily conciliate such profoundly different perspectives and methods. In particular, it is impossible for observers used to laboratory studies to leave this firm ground where so much has been achieved and simply dive into ‘macro’ problems, computing gross national product percentages, citations and rewards and so on.’ (Latour, 1983, 142-143)

That call was for all I know a throw-away remark, irony, or straw dog on Latour’s part. He did not explore the idea evoked by the first two sentences perhaps because as he wrote in the third, he thought it impossible. Indeed, if you read his next few lines after the quoted section above you will see he immediately headed back into the laboratory; it was justifiably his life’s work.

Contrary to that Latourian quotation, although perhaps in-keeping with his complicated positivist, materialist, ethos, I think it is now both necessary and within our capacity to solve the impossible problems. My proposed gamification offers one means to do so.

‘We are finally starting to learn how to give a post hoc narrative thick description of what should have been visible in the gathering that brings a thing together (similarly, after the shuttle’s explosion a tough inquiry was pursued). And yet we still don’t know how to assemble, in a single, visually coherent space, all the entities necessary for a thing to become an object…when we have learned how to do that, we might finally get our (material) materialism back – and our cosmic things to boot. That’s when the plot will really thicken.’ (Latour, 2007, 142)

My algorithmic synthesis would entail the following sub-routines which will be explained in depth beneath:

- An anthropology of innovation that took account of its very large, multidisciplinary literature with an eye for insights on how future inventions would be shaped including the changing penetration of different epistemic modes of invention such as the linear model, technological DIY and trial and error.
- A mathematical game of socio-technical momentum which could plausibly run on a computer. It would take Thomas Hughes at his literal word but also draw on what we learned over the intervening years.
- An implementable model of complexity in its historical, sociological and mathematical forms as it relates to the systems in which we wish to intervene as well as the impacts of our proposed interventions on those systems.

A taxonomy of epistemic modes of invention

I will first explain what I mean by epistemic modes of invention and why this matters for an algorithmic conception of STS.

The phrase refers to my effort to create a taxonomy of the processes inventors use to invent (Table 1). Some of these modes will be emergent and not as such extensively defined while some will be normative with an extensive literature describing how to achieve them. The relative weights of these modes in any inventive process could then be used to game out if the process would succeed.

Table 1 – taxonomy of epistemic modes of invention

Mode*	**Description**
Imitation, cloning, reverse engineering	A topic characterised by moralising but with reverse engineering perhaps the most neutral term.
Jugaad, technological DIY, frugal innovation, grassroots innovation	Low-resource settings demand a particular kind of inventive process that is regularly seen as sub-optimal.
Trial and error, brute force	Medicines and pesticides developed by screening natural products or potential outputs of chemical plants, ideally at great scale.
Tinkering, bodging, hacking	Phenomena which might have similarities to *jugaad*.
Intuition, gut feeling, genius	Intuition plays an acknowledged role in drug discovery (Kutchukian et al., 2012). Genius is a feature of hagiographies of inventors but one that cannot be completely dismissed.
Serendipity, luck, guessing, stab in the dark	*Le hasard ne favorise que les esprits préparés.* Scientific autobiographies are likely to be a good source of claims about 'serendipity'. There is discussion of the theme in the philosophy of science (Ross & Copeland, 2022) (Copeland, 2019).
False premises	What would later be seen as false premises underlie the invention, e.g., antique herbal remedies re-networked as drugs (Taylor, 2005) (Lei, 1999)

Linear model	Recent scientific knowledge and expertise is the author of invention (see next section).
Bildung, Praxis und Forschung	Intentional combination of knowledge, practice, research and probably other qualities.
Maintenance	While maintenance is prevalent, the prevalence of maintenance as an author of invention is unknown.
Computer-aided design, artificial intelligence (AI)	AI is an unproven modality but various computer-aided methods are commonplace, e.g. in drug discovery (Messeri & Crockett, 2024) (Tautermann, 2020).

*Click on link for Google Books Ngram (as one example of initial efforts to quantify trends). An Ngram combining a selection of keywords is available here. The long-term trajectories of maintenance and genius are notable; removal of these keywords from the plot allows for a finer-grained view of the other keywords which shows the linear model has plateaued.

Each epistemic mode would constitute an intentional but possibly unresolved mix of thoughts, physical actions and social processes that were believed by an observer (or the inventor) to advance the inventive process. They would need to be primarily understood, even if quite impressionistically, as an insight that would aid, rather than confuse, the construction of a computer model.

If an invention was traced to having a glass of beer in Zürich with a patent clerk named Einstein, having a glass of beer in Zürich with a patent clerk would not be classified in the taxonomy as an epistemic mode because it would lead to an absurd model that encouraged beer drinking in Zürich with patent clerks.

Instead, we would associate this event with what we deemed the notable aspect of the invention process from the perspective of building a generalizable model, for example, in the fictional Einsteinian case, an exchange of scientific knowledge.

The imperfections of this approach are evident, for example, I have not used the relevant terminology of economics (Godin, 2008). The epistemic modes are not a philosophical, economic or sociological commentary. Nor is it my intention to produce a psychological account of creativity (Simonton, 2021).

Rather, the goal is to open a rough but workable space for a quantitative model of invention that can be used to gamify assessments of future technology as well as design interventions to help (or hinder) specific inventive processes. Once a workable prototype computer model was available, revision of the above modes would probably occur.

I specifically used the word, invention. The word came back into use intermittently after rhetorical hiatuses where innovation dominated writing on the topic. I chose it because it reflects back on the title of the classic book by John Jewkes and his collaborators, *The Sources of Invention,* in which they gathered detailed information about inventive processes and sought to extract meaning from that information (Jewkes et al., 1958). That is also my goal although by different methods.

But we should remember that in this activity whether we call it invention, innovation, discovery, new thing, or another term is not particularly important, because an attitude of imprecision on the part of the analyst also connotes openness to actors' categories as the core of the approach, even if some indeterminacy results (Jewkes et al., 1958, 13-26).

For example, I have reflected actors' categories or those that I think actors recognize because this lack of originality on my part is important if we are to follow words around, for example, by automated text mining.

Turning, now, to the issue of populating Table 1 with data. If the anthropology of innovation taught us one thing, it is that inventors are quite opaque about the approaches they actually used, tending to emphasise particular kinds over others for their own reasons (Hoholm & Araujo, 2011, 134).

This means we have an uneven account of such modes in the primary literature. The secondary literature also has particular aims, again producing a limited picture that probably has least to do with the relative weights of these various modes in inventive processes.

We must therefore read attentively and recognise that terminology and framings vary but nevertheless there is quite a lot of useful information already recorded.

In terms of the broad sweep of analytical writing, great attention has probably been paid to imitation, cloning, etc. although sometimes configured in response to a particular political, legal or moralising agenda in which it might be designated as IP theft or economic espionage (Gilli & Gilli, 2019) (Forden, 2007) (Nasheri, 2005).

Reverse engineering is probably the neutral term for the phenomenon; it has a large practitioner literature which describes how it should be done (Wang, 2011). Evolutionary design might qualify as a form of imitation but with gradual, iterative modifications being foregrounded (Ziman, 2000). It evokes a natural process in which nucleic acid is copied but with very small changes that gradually accumulate over time. Overall, we might guess that imitation of various kinds is perhaps the dominant epistemic mode.

There is other literature on *jugaad*, technological DIY, frugal innovation, and possibly related terms (Radjou et al., 2012). This is particularly interesting because it is often epistemic in its focus while also describing the co-creation of useful knowledge and artefacts.

Akakpo (2019) describes technological DIY (*bricolage*) as 'the technique of the resourceful person in search of the minimum means of survival' and gives as an example the Togolese 'spider technology' (*technologie de l'araignée)* whereby there is 'artisanal and fraudulent extension of the electricity network, in areas not covered by the power company' (Akakpo, 2019, 10-11).

‘We must ask,’ Akakpo writes, ‘…about the place reserved for technological DIY in contemporary African societies; it is a paradox, an indicator of under-development…yet also crucial for national economies and social development…seeing its massive place in modernity, technological DIY reveals itself to be an impressive if messy site of innovation and production of know-how.’ (Akakpo, 2019, 11)

Jugaad-type ideas have also received criticism as suboptimal and even hazardous methods that, in a kind of neo-Nkrumahrist way of thinking, we might even consider racist (possibly parallelling older more obvious colonialist terms like ‘appropriate’ technology) (Santos, 2023) (Sharma, 2022) (Calkins, 2021) (Zeleza, 2003, 14).

Overall, the signal feature of *jugaad*-type approaches is a lack of funds and, as such, the epistemic mode is forced upon the inventor by financial circumstances. But it is a very significant epistemic mode and perhaps even one of the most prevalent globally.

What could be called electrical spider webs, cited above, are the paradigmatic example in provisioning electrical power and a crucial grid-distribution technology in the world today, but there are, of course, other examples which illustrate the dominance of *jugaad*.

We should also add on this point there is a whole range of poorly-specified methods that include hacking, tinkering, guessing, serendipity, intuition, false premises, and so on. These modes might actually be important but we probably know little about them in formal terms, although not nothing at all, noting for example that hacking is relatively prominently discussed (Conz, 2006).

A recent Harvard Business School working paper, Krieger, et al. (2021), invoked ‘corporate innovation efforts (that) rely only indirectly on science (e.g.) experimenting, tinkering, optimising, and inventing without the aid (or constraints) of the republic of science’ (Krieger et al., 2021, 1) but then did not explore this interesting point at all.

Trial and error, brute force, heuristic methods, etc. are also quite dominant forms of invention, citing drug discovery and discovery of pesticides, in which natural products or the gamut of possible outputs from a petrochemical plant were screened for useful activity. Indeed, in those fields, this mode has almost certainly proved paramount.

It seems no surprise that the pharmaceutical industry is regularly ranked as one of the highest in terms of R&D intensity. If its central inventive method was read by its senior managers to require trial and error, successful execution of that method would logically benefit from enormous throughput as materialised by robots, computer infrastructure and, at least in the past, a large quasi-bureaucracy of experts capable of managing and documenting trials.

With practically unlimited capitalisation behind it, the pharmaceutical industry now seeks ever-greater automation driving ever higher through-put, which Labant (2020), dubbed ‘fully automated luxury drug discovery’ (Labant, 2020).

Combinatorial possibilities in a trial and error modality might however require more inventive capacity than could be bought even if one disposed of the entire global stock of capital.

Another mode in my taxonomy, *Bildung, Praxis und Forschung,* obviously signifies the intent of combining knowledge exchange, practice, and research (and no-doubt other qualities – the phrase varies slightly). It is observable in such places as agricultural research stations, experimental farms and ecovillages where research and production are notably interlaced. There is a small secondary literature on these sites but we probably need to know more about how the concept has been supported and managed (Andreas & Wagner, 2012) (Smith, 2005).

Maintenance and repair has received attention mainly with a view to recovering it as an underappreciated aspect of technology in use. Its value here would be in understanding how the practice authored inventions, such as was the case with development of electric vehicles in the 1970s (Marhold, 2021).

However, we do not actually know what the prevalence of this mode might be outside of a few examples, as the author of the article on the electric vehicles explains due to what are seen as various methodological deficiencies in the social study of science.

It is useful overall to recognize that an inventive process would perhaps involve all of the modes mentioned, with varying penetrations of each over time or in other dimensions, and that they can obviously intermingle with each other.

The taxonomy is intended to facilitate a quantitative model in which we can understand the relative penetration of different modes. The ordering in the table above is by descending penetration globally, but it is entirely guesswork, as no numerical data exist.

Particular sites of invention might seem to favour particular modes. Tinkering is perhaps evoked by reference to multipurpose domestic settings such as garages, kitchens, etc. where inventive processes occurred. It might also be suggested by more intentional formulations such as maker-spaces and hacker-spaces.

Some of the modes could be described as ‘non-pedigreed’, that is to say not involving academically-credentialed technologists, to co-opt the term used by Rudofsky in his classic, *Architecture without Architects* (Chanis, 2023).

Certain parts of an organisation might be permitted to invent, while others not. In universities and government laboratories, for example, a single kind of expert, holding a PhD and a research grant, is uniquely empowered to invent to the exclusion of other kinds of employees. But, overall, there are not always general rules on who uses which mode.

Discussion of the innovating entity or site of innovation, as opposed to the inventing mode, sometimes elides distinctions such as those made by Freeman and Soete in their influential textbook *The Economics of Industrial Innovation* (but I do not want to put words in mouths) (Freeman & Soete, 2012, 103-105).

The authors argued for the importance of corporate or bureaucratic inventors (in the twentieth century) and contrasted this idea with the *Sources of Invention* by right-leaning economist Jewkes, and his collaborators, who they wrote had instead highlighted individual genius (Jewkes et al., 1958).

Yet the advantage of a trial and error approach is that it is amenable to scale and massed capital and indeed would benefit from both, and would therefore be something capitalists applaud.

A corporate inventor could in theory undertake more diffuse kinds of research based on intuition, of course. But you have to harbour doubts about the ability of unproductive mavericks to operate in a corporate bureaucracy (unless there was some kind of special protected space such as through the transfer of profits to philanthropic research activities as occurs with IT billionaires).

Equally, an individual type inventor or a small team could undertake trial and error methods, but it would be slow because they would lack the capital and organisation for the necessary through-put.

Looked at from another angle, a corporate inventor would be able to obtain any amount of recondite information about scientific matters through a range of measures such as employing consultants, building partnerships, buying IP, and so on, thereby implying a more rationalised approach.

These measures would not be available to a small team for financial reasons. Hence, the autonomous innovator would be at an information disadvantage and more reliant on guesses.

As can be seen, by foregrounding my own epistemic classification we can begin to ask underpinning questions about the relations between capital and invention. Evidently, in some cases there would be a positive correlation between level of capital and epistemic mode such as the case with trial-and-error and the linear model. If you have more cash you can do more of them.

In other cases, such as *jugaad*, the correlation appears to run the other way. That is to say, if you have no money, you are stuck with *jugaad*. But we should also acknowledge on this point that Akakpo is in one reading almost eliding an expanded or more capitalised form of *jugaad*, for example, as could be hypothesized through a World Bank-type program led by the chief engineer of the Togolese spider web technology.

This is not an outlandish idea. The South African government funds the “grassroots innovator” defined as “an individual who undertakes innovations to solve local challenges using local resources and capabilities through working outside the realm of formal innovation institutions”.

There are evidently a whole range of possible scenarios where the direction of correlation is not known or, if it is presumed, might still be explored with interesting implications. The point, which has value for gauging the logic of investments, is not that we have obtained a spectacular general insight but that we have an emergent means to model it in detail.

To conclude, it is going to be difficult in some cases to determine the epistemic bases of a particular inventive process. At the least, the determination will need a lot of esoteric knowledge.

Fortunately, the people that have relevant esoteric knowledge on observing and documenting the inventive process are found among the wider STS, history of science and technology, and innovation studies communities who have been developing appropriate methodologies for decades, although regrettably not always able to synthesise their insights for the benefit of at least the sincere outsiders who plan and execute science and research strategies.

Besides a re-examination of the secondary literature in STS, history of technology, economics, and so on, there is indeed a very large primary literature relevant to the topic written by inventors, investors, journalists, and others that can be read again with an eye for the telling detail.

Given the scale of documentary resources available, citing the famous thick description of anthropology, I am not making a call for more primary research but rather to see what we can uncover from what has already been written.

It is possible that we have a good idea about the epistemic bases of invention as well as their relative contributions through time. Yet, as far as I am aware, no one, in public at least, has read across this entire span of such literature with the goal of generating insights along the lines I suggest.

Ex-ante **technological determinism and the linear model**

Astute readers will have noticed I so far omitted the linear model from my discussion despite referencing it in Table 1. This is because it has particular explanatory significance for our theme here which is worth unpacking in greater detail.

Evidently, the linear model has received a lot of attention in the science policy literature (Godin, 2006). Edgerton (2004) pointed out it had never existed as a sort of fully-fledged normative model (Edgerton, 2004). This indeed could probably be said for some of the other epistemic modes that I proposed in the table above, which are often diffuse or emergent and that I tried to classify.

But in my taxonomy, to the contrary, I made the linear model, most assuredly, an explicit, normative model that seeks to take recently-acquired scientific knowledge and, by strategic and intentional means, make it the primary author of invention.

In this light, what are technology readiness levels, investment series rounds, or the sequential phases of clinical trials, but glimpses of the linear model as a means to convert recent scientific knowledge into inventions?

They are defined with considerable solidity by networks of legal contracts and, most notably in the case of clinical trials, a regulatory apparatus that sets the rules. Unlike many innovation theories which are analysts' categories, these named institutions evidently have great importance for the actors themselves.

My modal linear model also takes decidedly solid form in many deliberate efforts such as the creation of technology transfer offices, the promotion of the impact agenda, translational research, open innovation, even the entire 'neo-liberal' university (Mirowski, 2011). Deep tech, with its apparent interest in recondite scientific information also distinguishes the concept in more recent years (de la Tour et al., undated).

The linear model is therefore configured as a description of the present and, contrary to the idea that we long abandoned it, we are actually its greatest advocates.

It is rather on this particular idea that what I would call *ex-ante* technological determinism depends, if we are to read the future of technology from current scientific knowledge. It is therefore something that ought to be bottomed out empirically as much as possible in view of our overall goal.

We might therefore ask what is the function of the curve describing the penetration of the linear model, and what predictions can we make for its future trajectory.

If we found an invention proposed on the basis of current scientific knowledge but in a sector where the linear model was weak (and there were few plans to expand it), then this finding would count against the possibility of predicting future inventions from current science on that point.

Many decades ago, the American Project Hindsight was not very positive about recent scientific research as a contributor to innovation, that is to say, in the terms adopted here, on the penetration of the linear model (Zachary & Moore, 2019, 61).

We need more recent data but they are, however, sparse. As Zachary and Moore (2019) pointed out, few seriously engaged with the implications of Hindsight or a related project, TRACES, nor carried out more up-to-date studies (Zachary & Moore, 2019, 64).

A slightly newer analysis by Mansfield (1991) reported that ‘one-tenth of the new products and processes commercialised during 1975-85 in the information processing, electrical equipment, chemicals, instruments, drugs, metals, and oil industries could not have been developed (without substantial delay) without recent academic research’ (Mansfield, 1991, 11).

An updated analysis by the same author covering the years 1986-1994 detected an equivalent figure of 15% (Mansfield, 1998, 774). The other important point to note from this study is that penetration of the linear model (in our terms, not those of the original author) varied by industrial sector with a peak of 31% in ‘drugs and medicinal products’ but a scant 5% in ‘electrical’ products (Mansfield, 1998, 774).

A good although old article by Cohen, *et al.* (2002), cited data from Narin, *et al.* (1997) which ‘concluded that the linkages between industrial R&D and current public research (conducted in either academia or government labs in the prior 10 years) grew dramatically between the late-1980s and early-to-mid-1990s’ (Cohen et al., 2002, 3).

It could be the case, therefore, that the percentage of inventions derived intentionally or strategically from scientific knowledge increased from a possible late-twentieth century baseline of around 10% but with quite heterogeneous changes according to different kinds of inventions.

Whether this function is now asymptotic or could shift up or down substantially is impossible to know. If it turned out it had not risen much, despite ramping policy efforts around technology transfer, translational research, open innovation, and so on, the implication would be that the mode had reached its maximum feasible penetration, at least in some fields, or that perhaps other phenomena such as de-industrialization had somehow decoupled scientific knowledge from invention in a counter cyclical manner.

A *theoretical* maximum could also be due to the degree of unreliability of scientific knowledge in a given field, i.e., while acknowledging the occasional salience of false premises, a proportion of that knowledge could never author inventions. Indeed, there is an unproven sense that the reliability of scientific knowledge in general is declining. This would, in turn, undercut a drive to increase the penetration of the linear model because such efforts would lead to an excessive focus on unworkable inventions and, thus, diminishing returns. Actors in the system would, instinctively, reduce the amount of time they spent trawling the scientific literature, in favour of trial and error, for example.

If we considered a scenario where the linear model was driven to 100% penetration in the sense that all available global inventive effort was directed to its pursuit, what inventions would we expect to result? This situation is difficult to predict not least because we do not know if the sum of epistemic modes equals unity nor have we gauged the reliability of underlying scientific knowledge. But these are just the kind of problems our model could game-out.

Gamifying Thomas Hughes

If there was one field where STS could stake its claim most readily, it would be in the social construction of the future. Herein, we find very obvious resources for our model.

A useful review by Urueña (2022) in the pages of this journal asserted that ‘The future is a battlefield that is continuously settled in the present’ (Urueña, 2022).

We see an idea that the future is somehow encoded around us, in this case not exclusively in the devices themselves, but in the social world. Whether or not we accept the warlike analogy as apt, it nevertheless brings us to the problem of how we might reliably decode the present in a way that allows us to recover predictive data.

Urueña outlines what he sees as ‘sociotechnical momentum’ which obviously draws on the work of Thomas Hughes on ‘technological momentum’ thereby referencing back to the Marxist technological determinism literature to which Hughes responded (Hughes, 1994).

Hughes indeed appeared to triangulate between what he portrayed as social constructivists (or even social determinists) and the Marxists. He argued that he could accommodate both ideas in his vision. Whether or not we agree with Hughes, and I think those debates have rather faded from view now decades later, my point is to move towards a quantitative model. The momentum metaphor invites us to do so even if perhaps we are being lured to our doom.

Hughes defined the four qualities of momentum as: acquired skill and knowledge; special-purpose machines and processes; enormous physical structures; and organisational bureaucracy (Hughes, 1994, 108). In other words, an assembly that would be literally heavy if you piled it up on a giant weighing scale. Hughes expressly stated that momentum was ‘embodied in large physical structures’ such as dams and large chemical complexes (Hughes, 1994, 110).

But momentum is not just a quality of mass but also of acceleration and it is here indeed that Hughes evokes the idea of acceleration as something approximating the rate of expansion of these qualities (‘as the system became larger and more complex thereby gathering momentum’ (Hughes, 1994, 108)). The only factor that could ‘deflect or break’ substantial momentum, according to Hughes, was a historical event of large proportions such as the Great Depression.

As can be seen, our model will need to put a number on the instantaneous size of the four qualities defined by Hughes as well as a function describing their past expansion (or contraction) that would allow us to plot a possible future course.

The simplest approach, and I am not joking, would be to read Hughes literally and calculate the weight in tonnes of the inventive assembly of people and things and determine how that weight varied through time (the latter with a view to determining its acceleration or deceleration).

If a high-tech company fired 1000 employees over a week, say, the combined weight would lose roughly 10 tonnes per day (based on average adult humans); this would evidently be a deceleration. The weight of buildings and equipment dominates over humans so the shuttering of a manufacturing site, if accomplished quickly, would constitute an abrupt braking motion that would outpace even a colossal recruitment of staff.

The calculation ought to be possible even if deconvolving the precise weight of factories, laboratories, supply chains, and so on, could prove extraordinarily laborious and perhaps insurmountable unless there was significant computing power available (or a convenient proxy could be derived).

You would also have to come to some conclusions about what was in and out of the sociotechnical system under consideration. Saying everything is somehow connected to everything else is a claim that does not help us. We need instead to decide what matters. A really good model would allow us to play around with this parameter but would add complexity.

Furthermore, as can be seen, it is not yet clear if such effort would produce useful data. This could therefore be assessed in terms of a series of runs on historical data sets where the outcome was known; other kinds of even more complicated calculation, such as incorporating measures of complexity of the supposed technological system, would then have to be tried if the result was sub-par.

Hughes, for instance, referred to ‘political interests (that) reinforced…institutional momentum’ (Hughes, 1994, 112). He also argued that as the technological systems grew larger and more complex they tended to shape society rather than being shaped by it, while also referring to the potential for inertia and citing the ‘iron cage’ from Weber’s account of bureaucracy. Overall, his worldview is rich with physical metaphors that court mathematical description.

The latest review by Urueña cited at the start of this section benefits from the intervening 40 years of analysis. Urueña again pursues the mechanical metaphor but breaks it down into three parts, namely: mass (the human and non-human actors in the sociotechnical system); direction or orientation of the vector; and velocity (rate of expansion).

But I think the most important contribution is the opening up of the question of predictive rather than explanatory models. Hughes was essentially an historical argument whereas we can now move towards a future-oriented configuration.

One question lies with reading incipient potential for momentum in a slow or randomly moving object that presumably would represent a future technology before it had gained a significant directional vector.

On this point, Urueña identifies a new factor, ‘anticipatory practices’. ‘Despite being fictitious in character, futures re-arrange “the mass” of sociotechnical systems and provide it with directionality. They enable/constrain the orientation and speed of development of scientific-technological activities by gearing them towards satisfying particular agendas and social orders’

The physical language he explores in his article is not just the simpler mechanical metaphor of Hughes, but one of landscapes, topographies, interactions and relations. This, equally, suggests the possibility of a mathematical model but one that is above the level of high school physics.

Evidently these and other complications could be included somehow in our calculation as it is certainly not beyond our imagination to derive quantitative proxies. That being said, if a simple model suffices for our purpose, it would not require complication.

Urueña indeed returns to the basic Newtonian idea that once a sociotechnical complex has gained momentum it is difficult to modulate and that the best time to modulate it is therefore right at the start.

Such metaphors therefore still have salience even in this more sophisticated world. The start, before momentum has been gained, is the crucial point of intervention for weak vectors such as citizens seeking reform, when the system would respond to light taps.

Urueña’s canonical example is not dams as evoked by Hughes but the internet. The weight of all the data centres and associated electrical generation and transmission capacity, rare earth mines, chip factories and so on could well exceed the weight of Hughes’ dams.

This speaks to the idea that sociotechnical systems (and sociotechnical imaginaries) became literally heavier (and bigger in other dimensions) over the last hundred years.

This aspect was suggested by Showers (2011) who argued for the emerging concept of the ‘hubrisphere’ in which contemporary socio-technical imaginaries exceeded the scale of any existing classification of socio-technical systems. Her example was the long-proposed Grand

Inga Dam which would be the largest machine in history with the signal potential of destroying the Atlantic Ocean (Showers, 2011).

Whether or not the behaviour of such giant imaginary sociotechnical systems can be modelled on the basis of presumably smaller systems in which New Deal-era dams embedded themselves, is not known.

In space you can apparently set a capsule in motion with the touch of a finger. Whereas, under the Earth's gravity, the capsule needs thousands of tonnes of thrust to reach orbit. The metaphor of the light touch at the start of motion has its limits when there is another force involved. The Newtonian imagery, while tempting, is perhaps not working and other mathematical formulations would need to be tried.

'The high socio-technical momentum of the internet hinders the possibility of shaping nowadays the co-construction of future socio-technical worlds not permeated by this technology…In a nutshell, STM stresses that proposing and pursuing highly disruptive directions finds its most immediate constraint or limit in the socio-material and organisational characteristics of sociotechnical systems and their tendency to self-preservation and self-perpetuation' (Urueña, 2022).

Regrettably, I am not yet aware of anyone having taken Hughsian analyses at their word in public and perhaps, given the potential scale of the calculations and the uncertain pay-off, that is no surprise.

Detecting the crown galls

An algorithm capturing the above insights would, I think, give us the tools to draw conclusions about the probability of a particular invention succeeding and also the most appropriate strategies to obtain it. In other words we have somewhat assembled the parts of the objects.

However, no matter how much technological momentum was obtained, no matter how much guile we deployed, both success and failure would remain possibilities.

Taking the salience of the linear model as a starting point, which assumes that recent science authors inventions (and we must complicate this later), our initial model would logically run the entirety of available patents, grants and loans catalogues as well as scientific literature databases, against a subroutine which would, in turn, feed into the modules covering epistemic modes and sociotechnical momentum, thereby gaming out future pathways towards invention and use.

A paradigmatic and well-documented example of text mining for unexpected connections occurred long before AI was available, noting 'Arrowsmith', financed by the US National Institutes of Health (Smalheiser, 2005).

In this project, which played out in the 1980s and 1990s, Don Swanson and his collaborators analysed the American MEDLINE medical literature database to search for unexpected pairs of topics that their system classified as related to one another.

Some of the unexpected pairs, which Swanson called ‘undiscovered public knowledge’ such as fish oil and Raynaud syndrome, were later considered proven by experimental work such that fish oil indeed remedied Raynaud syndrome.

However, despite the promising early successes that even attracted the US Navy to trial the system as an aid for its intelligence analysts, the Arrowsmith-type approach never became a dominant feature of biomedical research. This was despite many decades of interest in it specifically as well as other predictive algorithms (and of course the rising scope of literature databases and improvements in computers).

Even today, where we have practically unlimited amounts of computing technology as well as enormous and highly-capitalised IT firms, inventions tend to be attributed to people rather than automated methods. This implies that it is conceptually difficult to make automated predictions based on the scientific literature.

The known limitations of Arrowsmith would surely only become greater if we sought to make predictions not just of unexpected linkages between medical topics but with the goal of identifying future inventions. Indeed, given there is believed to be a delay of several years, even decades, between the emergence of scientific knowledge and its realisation as an invention (Slote Morris et al., 2011), it would be an extraordinary oracular achievement to make any such prediction.

This brings me to the crown galls, which is an illuminating example concerning the development of genetically modified crops by Monsanto. The inventive process of the crops began decades before Monsanto managers had presumably even considered its implications, and from many directions, such as, the invention of glyphosate; investigations of the crown gall disease; and the biochemistry of an enzyme that metabolises glyphosate, EPSPS (Heck et al., 2003, 139).

Agrobacterium is a natural genetic engineer that causes ‘crown galls’ on woody and herbaceous plants. Crown galls were studied without thought of the invention they would engender (the other antecedents noted, such as glyphosate, also had back-stories (Adams, 2023), but I just picked one here to illustrate its model-building potential).

The key discovery in terms of the crown galls was made in the 1950s and is attributed to an American scientist called Armin C. Braun who worked for the Rockefeller Institute (Chrispeels & Meins, Jr, 2014).

Braun was interested in crown galls because of what he saw as their mechanistic connection to a human disease, cancer (Braun, 1969, 135-139). His insight was to note that the crown gall tissues continued to grow abnormally even when the infectious bacterium was absent.

As we later realised, the bacterium had inserted DNA into the plant cell nucleus, which in modern parlance, is to say it was a genetic engineer that could be used to get engineered DNA into plants.

The oddity of this story decades before the Monsanto inventions describes the difficult problem of identifying the proverbial crown galls in our current scientific literature; in somewhat Latourian terms, predicting the re-networking of crown gall-like things from curiosities interred in the niche interests of enthusiasts, to technologies important to capitalism.

As far as I know, Braun never considered this particular future for his discovery. We cannot impugn him for lack of prescience; it was logically impossible for him to have done so.

In light of my discussion about epistemic modes, any approach to detection of proverbial crown galls relies on an unrealistic historical assumption concerning the saliency of the linear model.

As would be suggested by the crown gall example, and many others, a number of epistemic modalities proved crucial progenitors of the invention, certainly the linear model, but also luck, intuition, tinkering, trial and error and false premises (Chilton, 2001). Reliance solely on the linear model as a means to predict future inventions would therefore tend to fail. The invention was not encoded in the then contemporary scientific knowledge, or at least, not all of it was.

The point here is not the obviousness of this insight that we cannot predict the future based on so little data, which everyone knows, it is that we broke down the insight into a form that could be mathematically modelled.

The question for our initial subroutine is not to predict future inventions. It is instead to identify the potential space in which inventions could occur. It is for the other subroutines concerning the epistemic modes and sociotechnical momentum to make the actual predictions about which inventions shall occur.

Herein, I suggest that two concepts are important in gamifying this topic, namely, the complexity of the systems that we are trying to determine; and the impact of our determination methods on that complexity. We will first explore these ideas theoretically and then, in closing, return to the crown galls to see how it plays out.

It is obviously a cliche to talk about complex or even wicked problems which accordingly call for complex solutions, interdisciplinary research and so on. There is a whole literature condemning reductionist approaches. But these are for the most part moralizations that while having a grain of truth to them, lack the quantitative qualities that we need.

This leads me to wonder if we actually know enough about the structure of complexity, both in terms of its intellectual history as well as matters such as its measurement and control. If true, this would be, in a way, surprising because of the notably cybernetic imagery of actor network theory (ANT), even though of course we do not actually know, formally, nor did we all agree, that ANT is a complex theory (or some other kind of representation).

But such a line of thought, in which we set aside uncertainties as well as epistemic differences between academic worldviews, takes us into an area where further precisions can be made.

‘A system in which large networks of components with no central control and simple rules of operation give rise to complex collective behaviour, sophisticated information processing and adaptation via learning or evolution.’ (Mitchell, 2009, 13)

By necessity, to answer this challenge, a wide-ranging literature will have to be woven together, including from the natural scientists and engineers who are obviously very interested in the topic. Unlike the discussions above, this is somewhat outside the usual STS wheelhouse even in principle and I will treat it very briefly in reference to two recent papers.

One of the most interesting quantitative proposals that I read in this sphere came from Böttcher (2018), who developed a complexity scale for biological systems including human societies, which I think is remarkable although has not been taken up (Böttcher, 2018).

His stated purpose was to explore the evolution of biological complexity as well as identify features that would guide the search for extraterrestrial life. The paper is interesting for our purposes on two points.

- Existing methods of calculating complexity were not applicable given all the necessary factors in complex biological systems could never be adequately defined. Therefore, Böttcher had developed a method which he believed was a satisfactory proxy and that could be solved numerically.
- By this method, a complexity diagram was presented spanning simple biomolecules through multicellular organisms to human societies (figures 3 and 4 of the paper).

This approach implies that a complexity space could be constructed in which we would locate any system of our choosing and by this means we could compare it to other such systems.

A second insight comes from a paper by Topcu, *et al.* (Topcu et al., 2022). The core argument of the authors is that decomposing problems in a complex system increases the complexity of the system. In other words, there is a connection between complexity and the methods proposed to address it.

Picking up again the story of the crown galls, besides the early success in engineering a couple of traits, namely, glyphosate resistance and Bt toxin, in a handful of commodity crops, which proved profitable, no further modifications appeared from Monsanto.

A small range of GM crops, like the herbicides in which they are literally soaked, have become exhibits in our living museum of past innovation; they are ‘sweated’ and depreciated assets, but they do not engender further innovation

It is a reasonable although unproven notion that the reason was not regulatory scepticism, or a poor scientific effort, but the technical difficulty of the underlying task, at least relative to a business case built upon financial return in a large chemical company (Reynolds & Szerszynski, 2011).

Monsanto tried, but as far as we know it proved too difficult for them, even with their knowledge, expertise and capital, to engineer major staple crops like rice or wheat by any known molecular methods. Without doubt, expert opinion suggests complex genetic systems that many crops exhibit make targeted genetic engineering difficult (Li et al., 2021, 1, 11). Interventions did not therefore produce the desired effects and, understandably, the company gave up.

That sense of giving up might also perhaps be read into the group of scientists who are determined to carry the torch. For example, having abandoned wheat as unworkable, there is a new focus on varieties like *Camelina sativa* for which there is no major current use but which a presumption is made concerning future use typically concerning biofuels or changes to the human diet (*C. sativa* was once a source of oil for lamps) (Zhang et al., 2017, 648).

The fact that the technology never seems to deliver a steady stream of inventions (besides the initial limited modifications developed by Monsanto), and indeed might not actually work as hoped, is rarely made explicit in the scientific literature.

One technical review published in 2020 featured without apparent irony the comment that the ‘commercial applications of genome editing of crops is in its infancy’ (Mackelprang & Lemaux, 2020, 662) – 24 years since Monsanto began selling GM crops (1996) and 40 years since that company first began investigations, which, I think, connotes a long infancy.

Obviously, multiple companies operate in this market. Based on the data on regulatory approvals (which is evidently not the same as either invention or use) – only a few dozen crops have been modified (*GM Crops List – GM Approval Database*, n.d.).

Beyond this point, it is difficult to systematically track the penetration of agricultural technologies, and this refers to the spraying of pesticides as much as the planting of GM crops. But the impression is that the number of genetic modifications in use is smaller, as is the number of pesticides (Adams, 2023, 84); it is simply that a small subset of these inventions achieved extraordinary penetration, far greater than any other invention of the past decades, citing, for example, the pollution they cause.

The deep tech agenda that appeared recently signifies the search for ‘the next big thing’ and as noted above implies a renewed commitment to the linear model. It proposes yet another solution to the genetic engineering conundrum whereby only a small number of successful modifications have proved possible and which, like the crown galls of decades ago, lies deep in recondite scientific knowledge.

In a little-cited but informative report on deep tech, by the consultancy firms Boston Consulting Group and Hello Tomorrow, CRISPR-Cas is the new ‘crown gall’ (de la Tour et al., undated, 6).

The discovery was made by Emmanuelle Charpentier and Jennifer A. Doudna; the Nobel Prize was awarded to these two scientists in 2020. However, the stories of its discovery, like the crown galls and indeed probably all such stories told by scientists, prove arcane, evoking, in the taxonomy above, many different epistemic modalities, not just the linear model (Doudna & Charpentier, 2014).

The discovery proposes ‘molecular’ scissors that can cheaply and rapidly ‘edit’ nucleic acid. Its presumed technical benefits are that it is amenable to scaling and therefore the ability to break down the complexity of crop genomes by trial and error. The metaphor of editing is crucial for the salience of the technology as an easy solution.

Yet, in the past, neither *Agrobacterium* nor the later development of another transformation technique, the gene gun, that bombards plant cells with microprojectiles carrying DNA, (Sandford, 2000) (Nelson, 2012) delivered a complete solution.

Obviously we cannot appraise claims that CRISPR-Cas will author future inventions given, at best, it is like buying lottery tickets, which is not the same as winning the jackpot.

But we could make a comment on what the emergence of CRISPR-Cas as an imaginary object does to the potential invention space defined by recent scientific knowledge; therefore, both the probabilities of it indeed being equivalent of buying a lottery ticket in the particular lottery we want to win and, if it is equivalent to buying this lottery ticket, of winning the lottery.

Invoking the ideas that we read concerning complex systems and interventions upon them, it seems that crop genetics is complex and perhaps above our capacity to rationally modify beyond very basic operations. At the same time, we need to understand how complex the proposed solution might be, given the possible relationships between complexities.

There is a parallel literature on synthetic biology that embodies a programme of action that would seem to be the worst kind of approach from a perspective of ‘facing complex systems with complex tools’, because the complexity of the prime metaphor, engineering, is I think stupendously less than even a bacterial cell, let alone a rice paddy (de Lorenzo, 2021).

CRISPR-Cas is an object that implies opening up the invention space in plant biotechnology, but it seems by its ambition to deconvolve the problem into one of brute force, it could equally push the overall goal further from our reach.

Even if we considered the complexities non-additive in this case, the probability of hitting the jackpot might be asymptotic to the infinite and, therefore, even if we had access to the entirety of the world’s capital to buy an imaginary robot laboratory that could process a billion tests a second, we still might not win.

Our model, were it to exist, would be used to investigate these vague notions systematically in terms of gaming the complexity of the system and the complexity of the solution in any case we cared to choose.

Based on historical knowledge, we could then draw the thresholds where we thought systems would become too complex to solve with the tools available. Given substantial scientific efforts to genetically-engineer a range of plants and therefore a highly-developed literature, these historical data would be relatively easy to obtain.

Was ‘biotechnology the machine to make the future’, well, perhaps not in the ways that were initially claimed, but we could at least answer that question in quantitative terms (Rabinow & Dan-Cohen, 2013). We can also see, I think, how this approach could be applied to other proposals that have nothing to do with genetic engineering.

Conclusion

Looking back at the STS literature, we have seen plenty of explanation, de-bunking, commentary, and well-articulated critique on the way ‘we’ think about technology, both its past and future (Edgerton, 2011). A relatively large number of models of sociotechnical change and innovation have been documented as well as notions of socio-technological imaginaries (Sismondo, 2020) (Sovacool & Hess, 2017).

Put another way, we already have quite a good understanding of what is going on around us. But our understanding, as far as I know, does not inform us precisely where to place bets when considering the gamut of potential future inventions presented to us. This matters not just for governments and corporations seeking to target their own investments or disrupt the investments of their rivals, but also those who wish to hold the powerful to account.

Games have often been part of the process of planning, one example being war games (Goria, 2023). The idea of serious games being used to prompt discussion about the future in fields such as decarbonization (Wagner & Gałuszka, 2020) is now a commonplace possibly aided by the technical quality of digital games as well as the already wide penetration of computer models and hobby gaming.

As I have explored in this paper, part of the process of gamification of STS forces us to confront a wide swathe of literature as we try to break down the problems we meet into a plausibly mathematical form. Gamification is therefore of value as a social process of collaborative work with a designated goal, even before we made a game, model, or simulation (I am using the words interchangeably because we are not at the stage yet of labelling outputs). This is an idea of a model as method, not as outcome.

The eventual integration of the three subroutines discussed above in an overall model would advance us a greater distance towards my proposed goal of gamifying STS.

Taking the salience of the linear model as a starting point, our initial model would run the entirety of available patents, grants and loans catalogues as well as scientific literature databases, against our complexity routines, which would, in turn, feed into the modules covering epistemic modes and sociotechnical momentum, thereby gaming out future pathways towards invention and use.

In all three subroutines, I described potential mathematical solutions that I find emergent in 40 years of STS research as well as, of course, other fields of systematic knowledge.

Analysing other invention modalities that were not, in theory, readable from current science, i.e., modalities other than the linear model, would probably require further input datasets which obviously we are yet to identify but could include the entirety of the internet.

However, if this latter task were to be achieved, even using at first a synthetic or simulated input, one can imagine a game that encompassed the entirety of the global invention space and which could be executed in forward or reverse (i.e., the future that we sought could be run back to the requirements for its inception as much as the other way).

A mammoth computational task, as would be expected, but one that would then inform us of the most probable topics where investment should be placed for the future we wanted, as well as plausible methods to accelerate (or subvert) developments. We would have beaten the casino.

Bibliography

Adams, V. (2023). *Glyphosate and the Swirl: An Agroindustrial Chemical on the Move*. Duke University Press.

Akakpo, Y. (2019). *Le technocolonialisme: agir sous une tension essentielle*. L'Harmattan. https://www.editions-harmattan.fr/livre-le_technocolonialisme_agir_sous_une_tension_essentielle_yaovi_akakpo-9782343165660-61864.html

Andreas, M., & Wagner, F. (2012). "For Whom? For the Future!" Ecovillage Sieben Linden as a Model and Research Project. *RCC Perspectives*, *8*, 135-148. https://www.environmentandsociety.org/perspectives/2012/8/article/whom-future-ecovillage-sieben-linden-model-and-research-project

Böttcher, T. (2018). From Molecules to Life: Quantifying the Complexity of Chemical and Biological Systems in the Universe. *Journal of Molecular Evolution*, *86*, 1-10. https://doi.org/10.1007/s00239-017-9824-6

Braun, A. C. (1969). *The Cancer Problem: a Critical Analysis and Modern Synthesis*. Columbia University Press. https://archive.org/details/cancerproblemcri00brau

Calkins, S. (2021). Toxic remains: Infrastructural failure in a Ugandan molecular biology lab. *Social Studies of Science*, *51*(5), 707-728. https://doi.org/10.1177/03063127211011531

Chanis, V. (2023). In Quest of Meaning – Revisiting the discourse around "non-pedigreed" architecture. *Tacit Knowledge in Architecture – Conference Proceedings 2023*. https://tacit-knowledge-architecture.com/object/in-quest-of-meaning-revisiting-the-discourse-around-non-pedigreed-architecture-2/

Chen, C. (2013). *Mapping Scientific Frontiers: The Quest for Knowledge Visualization*. Springer London.

Chilton, M.-D. (2001). Agrobacterium. A Memoir. *Plant Physiology*, *125*(1), 9-14. https://doi.org/10.1104/pp.125.1.9

Chrispeels, M. J., & Meins, Jr, F. (2014). *Armin C. Braun 1911-1986*. National Academy of Sciences Biographical Memoirs. https://www.nasonline.org/publications/biographical-memoirs/memoir-pdfs/braun-armin.pdf

Cohen, W. M., Nelson, R. R., & Walsh, J. P. (2002). Links and Impacts: The Influence of Public Research on Industrial R&D. *Management Science*, *48*(1). https://doi.org/10.1287/mnsc.48.1.1.14273

Conz, D. B. (2006). *Citizen Technoscience: Amateur Networks in the International Grassroots Biodiesel Movement*. Arizona State University.

Copeland, S. (2019). On serendipity in science: discovery at the intersection of chance and wisdom. *Synthese*, *196*, 2385-2406. https://doi.org/10.1007/s11229-017-1544-3 de la Tour, A., Soussan, P., Harlé, N., Chevalier, R., & Duportet, X. (undated). *From Tech to Deep Tech: Fostering collaboration between corporates and startups*. Hello Tomorrow & The Boston Consulting Group. https://dtusciencepark.dk/wp-content/uploads/2023/08/from-tech-to-deep-tech-web.pdf

de Lorenzo, V. (2021). When biology became engineering: adopting standards for living systems. *Mètode Science Studies Journal*, *11*, 61-73. https://doi.org/10.7203/metode.11.15975

Doudna, J. A., & Charpentier, E. (2014). Genome editing. The new frontier of genome engineering with CRISPR-Cas9. *Science*, *346*(6213), 1258096. 10.1126/science.1258096

Duke, S. O., & Powles, S. B. (2008). Glyphosate: A Once-in-a-century Herbicide. *Pest Management Science*, *64*(4), 319-325. https://doi.org/10.1002/ps.1518

Edgerton, D. (2004). The linear model did not exist: reflections on the history and historiography of science and research in industry in the twentieth century. In *The Science-industry Nexus: History, Policy, Implications : Nobel Symposium 123*. Science History Publications/USA.

Edgerton, D. (2011). *The Shock of the Old: Technology and Global History Since 1900*. Oxford University Press, USA.

Forden, G. (2007). How the World's Most Underdeveloped Nations Get the World's Most Dangerous Weapons. *Technology and Culture*, *48*(1), 92-103. http://web.mit.edu/stgs/pdfs/TandC_essay_on_WMD.pdf

Freeman, C., & Soete, L. (2012). *The Economics of Industrial Innovation*. Routledge.

Gibbons, M., & Johnston, R. (1974). The roles of science in technological innovation. *Research Policy*, *3*, 220-242.

Gilli, A., & Gilli, M. (2019). Military Technology: The Realities of Imitation. *CSS Analyses in Security Policy*, *238*. https://doi.org/10.3929/ethz-b-000323117

GM Crops List – GM Approval Database. (n.d.). ISAAA. Retrieved March 16, 2024, from https://www.isaaa.org/gmapprovaldatabase/cropslist/default.asp

Godin, B. (2006). The Linear Model of Innovation: The Historical Construction of an Analytical Framework. *Science, Technology, & Human Values*, *31*(6), 639-667. https://doi.org/10.1177/0162243906291865

Godin, B. (2008). Innovation: The History of a Category. In *Project on the Intellectual History of Innovation Working Paper No. 1*. https://espace.inrs.ca/id/eprint/10023/1/Godin_2008.pdf

Goria, S. (2023). Les jeux à but de partage : des jeux sérieux destinés à motiver et à accompagner les échanges de points de vue et de connaissances. *ESSACHESS*, *16*(32), 149-172. https://doi.org/10.21409/WR9K-K268

Heck, G. R., CaJacob, C. A., & Padgette, S. R. (2003). Discovery, Development and Commercialization of Roundup Ready Crop. In *Plant Biotechnology 2002 and Beyond: Proceedings of the 10th IAPTC&B Congress June 23–28, 2002 Orlando, Florida, U.S.A.* (pp. 139-142). Springer Netherlands.

Hoholm, T., & Araujo, L. (2011). Studying innovation processes in real-time: The promises and challenges of ethnography. *Industrial Marketing Management*, *40*(6), 933-939. https://doi.org/10.1016/j.indmarman.2011.06.036

Hughes, T. P. (1994). Technological momentum. In *Does Technology Drive History? The Dilemma of Technological Determinism*. MIT Press. https://archive.org/details/doestechnologydr0000unse/page/100/mode/2up

Jewkes, Sawers, D., & Stillerman, R. (1958). *The Sources of Invention*. Macmillan & Co Ltd. https://archive.org/details/sourcesofinventi0000jewk/page/n5/mode/2up

Krieger, J. L., Schnitzer, M., & Watzinger, M. (2021). Standing on the Shoulders of Science. *Harvard Business School Working Paper*, *21-128*. https://www.hbs.edu/ris/Publication%20Files/21-128_3c5e5109-4859-4a42-9f2a-bb6bd2cda80b.pdf

Kutchukian, P. S., Vasilyeva, N. Y., Xu, J., Lindvall, M. K., Dillon, M. P., Glick, M., & Coley, J. D. (2012). Inside the Mind of a Medicinal Chemist: The Role of Human Bias in Compound Prioritization during Drug Discovery. *PLoS ONE*, *7*(11), e48476. https://doi.org/10.1371/journal.pone.0048476

Labant, M. (2020, August 3). Fully Automated Luxury Drug Discovery. *Genetic Engineering and Biotechnology News*. https://www.genengnews.com/insights/fully-automated-luxury-drug-discovery/

Latour, B. (1983). Give me a laboratory and I will raise the world. In *Science Observed: Perspectives on the Social Study of Science* (pp. 141-169). SAGE Publications Ltd. http://www.bruno-latour.fr/sites/default/files/12-GIVE-ME-A-LAB-GB.pdf

Latour, B. (2007). Can we get our materialism back, please? *Isis*, *98*(1), 138-142. https://doi.org/10.1086/512837

Lei, S. H.-L. (1999). From Changshan to a New Anti-Malarial Drug: Re-Networking Chinese Drugs and Excluding Chinese Doctors. *Social Studies of Science*, *29*(3), 323-358. https://doi.org/10.1177/030631299029003001

Li, S., Zhang, C., Li, J., Yan, L., Wang, N., & Xia, L. (2021). Present and future prospects for wheat improvement through genome editing and advanced technologies. *Plant Communications*, 2, 100211. https://doi.org/10.1016/j.xplc.2021.100211

Mackelprang, R., & Lemaux, P. G. (2020). Genetic Engineering and Editing of Plants: An Analysis of New and Persisting Questions. *Annual Review of Plant Biology*, *71*, 659-687. https://doi.org/10.1146/annurev-arplant-081519-035916

Mansfield, E. (1991). Academic research and industrial innovation. *Research Policy*, *20*(1), 1-12. https://doi.org/10.1016/0048-7333(91)90080-A

Mansfield, E. (1998). Academic research and industrial innovation: An update of empirical findings. *Research Policy*, *26*, 773-776. http://sjbae.pbworks.com/f/Mansfield_1998.pdf

Marhold, K. (2021). Of Buses, Batteries and Breakdowns: The Quest to Build a Reliable Electric Vehicle in the 1970s. In *The Persistence of Technology: Histories of Repair, Reuse and Disposal*. transcript Verlag. 10.14361/9783839447413-009

Messeri, L., & Crockett, M. J. (2024). Artificial intelligence and illusions of understanding in scientific research. *Nature*, *627*, 49-58. https://doi.org/10.1038/s41586-024-07146-0

Miles, I. (2010). The development of technology foresight: A review. *Technological Forecasting and Social Change*, *77*(9), 1448-1456. https://doi.org/10.1016/j.techfore.2010.07.016

Mirowski, P. (2011). *Science-Mart*. Harvard University Press.

Mitchell, M. (2009). *Complexity: A Guided Tour*. Oxford University Press, USA.

Nasheri, H. (2005). *Economic Espionage and Industrial Spying*. Cambridge University Press. https://archive.org/details/economicespionag0000nash/page/n5/mode/2up

Nelson, N. (2012). Shooting Genes, Distributing Credit: Narrating the Development of the Biolistic Gene Gun. *Science as Culture*, *21*(2), 205-232. https://doi.org/10.1080/09505431.2011.614335

Rabinow, P., & Dan-Cohen, T. (2013). *A Machine to Make a Future: Biotech Chronicles*. Princeton University Press.

Radjou, N., Prabhu, J., & Ahuja, S. (2012). *Jugaad Innovation*. Random House Publishers India Pvt. Limited.

Reynolds, L., & Szerszynski, B. (2011). Contested Agro-Technological Futures: The GMO and the Construction of European Space. In *Exploring Central and Eastern Europe's Biotechnology Landscape*. Springer.

Rindzevičiūtė, E. (2023). *The Will to Predict: Orchestrating the Future Through Science*. Cornell University Press.

Ross, W., & Copeland, S. (Eds.). (2022). *The Art of Serendipity*. Springer International Publishing.

Sandford, J. C. (2000). Turning point article: the development of the biolistic process. *In Vitro Cellular & Developmental Biology – Plant*, *36*(5), 303-308. https://doi.org/10.1007/s11627-000-0056-9

Santos, J. P. (2023). African Makers Deserve New Things. *African Arguments*. https://africanarguments.org/2023/11/african-makers-deserve-new-things/

Sharma, D. C. (2022). *Indian Innovation: Not Jugaad : 100 Ideas that Transformed India*. Roli Books Pvt. Limited. https://rolibooks.com/product/indian-innovation/

Showers, K. B. (2011). Beyond Mega on a Mega Continent: Grand Inga on Central Africa's Congo River. In *Engineering Earth: The Impacts of Megaengineering Projects*. Springer.

Simonton, D. K. (2021). Scientific Creativity: Discovery and Invention as Combinatorial. *Frontiers in Psychology*, *12*, 721104. 10.3389/fpsyg.2021.721104

Sismondo, S. (2020). Sociotechnical imaginaries: An accidental themed issue. *Social Studies of Science*, *50*(4), 505-507. https://doi.org/10.1177/0306312720944753

Slote Morris, Z., Wooding, S., & Grant, J. (2011). The answer is 17 years, what is the question: understanding time lags in translational research. *Journal of the Royal Society of Medicine*, *104*(12), 510-520. https://doi.org/10.1258/jrsm.2011.110180

Smalheiser, N. R. (2005). A. Hoffmann, H. Motoda, and T. Scheffer (Eds.): DS 2005, LNAI 3735, pp. 26 – 43, 2005. © Springer-Verlag Berlin Heidelberg 2005 The Arrowsmith Project: 2005 Status Report. In *Discovery Science: 8th International Conference, DS 2005, Singapore, October 8-11, 2005, Proceedings* (pp. 26-43). Springer Berlin Heidelberg. http://abel.lis.illinois.edu/tutorial/smalheiser_statusreport_2005.pdf

Smith, A. (2005). The Alternative Technology Movement: An Analysis of its Framing and Negotiation of Technology Development. *Human Ecology Review*, *12*(2), 106-119. https://humanecologyreview.org/pastissues/her122/smith.pdf

Sovacool, B. K., & Hess, D. J. (2017). Ordering theories: Typologies and conceptual frameworks for sociotechnical change. *Social Studies of Science*, *47*(5), 703-750. https://doi.org/10.1177/0306312717709363

Tautermann, C. S. (2020). Current and Future Challenges in Modern Drug Discovery. *Methods in Molecular Biology*, *2114*, 1-17. 10.1007/978-1-0716-0282-9_1

Taylor, K. (2005). *Chinese medicine in early communist China, 1945-63 : a medicine of revolution*. RoutledgeCurzon.

Topcu, T. G., Mukherjee, S., Hennig, A., & Szajnfarber, Z. (2022). The Dark Side of Modularity: How Decomposing Problems Can Increase System Complexity. *Journal of Mechanical Design*, *144*(3), 031403. https://doi.org/10.1115/1.4052391

Undheim, T. A. (2021). *Future Tech: How to Capture Value from Disruptive Industry Trends*. Kogan Page.

Urueña, S. (2022). Anticipation and modal power: Opening-up and closing-down the momentum of sociotechnical systems. *Social Studies of Science*, *52*(5), 783-805. 10.1177/03063127221111469

Wagner, A., & Gałuszka, D. (2020). Let's play the future: Sociotechnical imaginaries, and energy transitions in serious digital games. *Energy Research & Social Science*, *70*, 101674. https://doi.org/10.1016/j.erss.2020.101674

Wang, W. (2011). *Reverse Engineering: Technology of Reinvention*. Taylor & Francis.

Zachary, P., & Moore, J. (2019). Where Does Innovation Come From?: Project Hindsight, TRACEs, and What Structured Case Studies Can Say About Innovation. *IEEE Technology and Society Magazine*, *38*(3), 56-57. https://drive.google.com/file/d/1duDupXUAOirzTcz1WuJeYuHfLI9LWHNi/view

Zeleza, P. T. (2003). Introduction: Africa's search for modernity in the internet age. In *In Search of Modernity: Science and Technology in Africa*. Africa World Press.

Zhang, W. Z., Henry, I. M., Lynagh, P. G., & Comai, L. (n.d.). Significant enhancement of fatty acid composition in seeds of the allohexaploid, Camelina sativa, using CRISPR/Cas9 gene editing.

Zhang, W. Z., Henry, I. M., Lynagh, P. G., Comai, L., Cahoon, E. B., & Weeks, D. P. (2017). Significant enhancement of fatty acid composition in seeds of the allohexaploid, Camelina sativa, using CRISPR/Cas9 gene editing. *Plant Biotechnology Journal*, *15*(5), 648-657. https://doi.org/10.1111/pbi.12663

Ziman, J. (Ed.). (2000). *Technological Innovation as an Evolutionary Process*. Cambridge University Press. https://archive.org/details/technologicalinn0000unse_p5l2