Exploration and Exploitation in Scientific Inquiry:

Towards a Society of Explorers

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Abstract

This essay argues that scientific systems have two main functions typical to self-organising adaptive and complex systems: Exploration for and exploitation of information. The self-organising nature, or spontaneous order, of scientific systems was prominently conceived by polymath Michael Polanyi. Revisiting Polanyi's philosophy of science reveals why scientific freedom is still today as important a value as ever, even though the notion of "freedom" itself must be revised. Namely, freedom of inquiry should serve to maintain a diverse and adaptive balance between exploration (for knowledge) and exploitation (of knowledge). This essay argues that current trends within science policy and scientific communities, from impact assessments to targeted research funding, are often inherently biased towards advancing exploitative functions over explorative activities. Concerns are raised over whether these exploitative biases suppress the explorative nature of scientific inquiry, and thus disturb the self-organisation of scientific systems by favouring hasty and sometimes negligent exploitation. Further concerns are raised as to whether these impaired adaptive capacities of scientific systems lead to reduced resilience of broader society. Finally, Polanyi's vision of a Society of Explorers, where free exploration is vindicated and safeguarded, is revived in a 21st century context.

1 Introduction

The present text argues that scientific communities have two main functions typical to adaptive and self-organising systems: *Exploration for* and *exploitation of* information. As many other systems with characteristics of spontaneous order, the scientific enterprise and its constitutive particulars, scientists, find themselves in a struggle to find an appropriate, functional and creative balance between these two endeavours. I argue in this essay, drawing on polymath Michael Polanyi's conception of science as a mode of spontaneous order and cases of other self-organising systems found in the natural world, that current trends in science policy and scientific communities are to a great degree—indeed, perhaps to too great a degree—biased towards advancing exploitative functions of science at the cost of maintaining its explorative edge. This phenomenon, which I call the ”exploitative bias” in scientific systems, is epitomized by cuts in basic research, the proliferation of assessments of the social or economic ”impact” of science and prestige-gaming, along with a general tendency of moving from basic research to targeted or “strategic” research and funding. The main concern here is that in an era where our environment is perhaps more unpredictable than ever experienced before, we might be disproportionately favouring focused exploitation over more diverse and even random exploration. Consequently, we might be disturbing the efficient self-organisation of scientific systems and risking to lose the adaptive capacity of the scientific enterprise. Since science, in turn, is a core adaptive function of society, this is a direct concern for civilisation at large.

As implied in its subtitle, “towards a society of explorers,” this essay in particular provides grounds for the vindication of the explorative function of scientific systems. This should not be interpreted as a devaluation of the exploitative function—the case is merely that the former is perceived to be under greater pressure (furthermore, this essay proceeds to a vindication of *careful* and *slow* exploitation). The central argument proposed in this essay is that to maintain its adaptive capabilities and resilience in a rapidly changing world, society requires significant degrees of explorative freedom in scientific inquiry. This builds on a possibilistic (and, as is elaborated below, serendipitous) notion of scientific discovery. Only by, as Polanyi noted, letting scientists loose to explore a large variety of unexpected paths of scientific discovery can we expect the scientific system to encounter the novel ideas and tools required to adapt to the fundamentally unpredictable challenges of the future. Thus, scientific institutions, norms and practices should serve to make epistemic exploration, as well any consequent exploitation, a reliable, careful and diverse process.

This essay progresses in a threefold manner. Firstly, Michael Polanyi's philosophy of science is revisited to exemplify the self-organizing nature, or spontaneous order, of scientific communities and scientific inquiry. Polanyi's philosophy is, essentially, a justification for relatively (although not absolutely) autonomous scientific inquiry, although its contemporary shortcomings are also discussed. Second, two cases of naturally emerging self-organization, ants and lymphocyte cells, are examined. Here we have two particularly illustrative cases demonstrating a successful balancing act between explorative and exploitative functions. These examples work as analogies for vindicating the explorative functions of scientific inquiry. Finally, I proceed to discuss these examples in the context of scientific systems and their current trends (exploitative biases), providing grounds for advancing what Polanyi (2000, 19) called a "Society of Explorers"—a society maintaining freedom of exploration as a foundational value.

2 The Spontaneous Order of Scientific Systems

Multidisciplinarity and interdisciplinarity are notions which have suffered from a significant degree of inflation in recent years. Today, there exists little scientific work which does not lay claim to these titles. In Michael Polanyi, however, we have a multidisciplinary polymath in one of its purest forms. I argue that it is in particular this hybrid scientist-philosopher nature of Polanyi's work which makes it particularly relevant to revisit at the dawn of the upcoming socioecological crises and socio-political responses, which seem to include new waves of centrally directed science policies. Michael Polanyi

(b. Polányi Mihály, 1891–1976) was a Hungarian-British scholar who made significant advances in physical chemistry, economics and philosophy. Born a Jewish Hungarian, he was a part of the so-called "Hungarian phenomenon" of early 20th century exceptionally bright Budapest intellectuals (including the likes of Leo Szilard, Eugene Wigner, John von Neumann, Edward Teller and Karl Mannheim) who fled the turbulent post-First World War Hungary, many of them only to find themselves soon fleeing their new home countries—in Polanyi's case, Weimar Germany—during the social and political upheaval of the 1930's (Nye 2011).

Polanyi, an acclaimed natural scientist who had by the mid-1930's made great contributions to theories on x-ray diffraction, chemical kinetics and adsorption, was deeply concerned by the totalitarian social and political developments of his time. Amongst the atrocities typical to this era was the subjugation of scientific inquiry under central state direction. Polanyi's distaste with these developments led to his interest in the self-organisation, "free" or "spontaneous" ordering, of scientific communities. Perhaps it was precisely his first hand encounters with scientific inquiry, including state-of-the-art laboratories, mentor–pupil relations, and so forth, which provided for his lucid descriptions of how scientific communities, opinions and truths are constructed and organised. His motive for exploring the self-organising nature of scientific systems, however, was most of all a political one. This was most profoundly sparked by a discussion with Bolshevik revolutionary Nikolai Bukharin, who exclaimed to Polanyi his desire of subjugating scientific inquiry under centrally planned Five Year Plans. Reminiscently, Polanyi (2009, 3) writes:

> ”I first met questions of philosophy when I came up against the Soviet ideology under Stalin which denied justification to the pursuit of science. I remember a conversation I had with Bukharin in Moscow in 1935. Though he was heading toward his fall and execution three years later, he was still a leading theoretician of the Communist party. When I asked him about the pursuit of pure science in Soviet Russia, he said that pure science was as morbid symptom of a class society; under

> socialism the conception of science pursued for its own sake would disappear, for the interests of scientists would spontaneously turn to problems of the current Five-Year Plan."

The vindication of scientific autonomy became a concern which Polanyi pursued for the rest of his life as a scientist and philosopher. Having spent a significant amount of time at the exceptionally autonomous Kaiser Wilhelm Society in Weimar Berlin (also known at the time as the "City of Science"), a scientific institute which until the Nazi uprising embraced and adorned values such as freedom of inquiry and scientific autonomy, Polanyi was deeply troubled by the emerging subjugation of scientific systems under central governance. Here science would not be pursued for its own sake and instead would serve the purposes of central direction or totalitarian rule. (Nye 2011.) Science, for Polanyi, was a spontaneously arising order, and any attempts at centrally directing it towards prescribed goals would most likely prove to be futile, regressive, or worse, destructive.

Figure 1: Professor Michael Polanyi exploring (on a hike) in England ca. 1933. Image use rights obtained from Manchester Archives.

To make the definitive case for portraying science as a spontaneously arising order, Polanyi made in his seminal work *The Logic of Liberty* (Polanyi 1951, 35) the illustrative example of a jigsaw puzzle. Suppose, Polanyi (1951, 35) observed, we were to "piece together a very large jigsaw puzzle which it would take one person several days or even weeks to complete." Moreover, suppose this task was extremely urgent and important, with "the discovery of some important secret being dependent on the solution". Undoubtedly, the solution would lie in the deployment of some particularly skilled team to complete this task. But the essential question remains: How should this team be organized? Polanyi (ibid.) argued that the only means by which this task could be finished quickly would be to "get as many helpers as could conveniently work at one and the same set and let them loose on it", each to follow their own initiative. Each member of the team could now observe directly the progress made by all the others, and accordingly set themselves new problems and initiatives in light of the latest structure of the so far completed puzzle. Simply, the "tasks undertaken by each would closely dovetail into those performed by others". The individual performances of each member of the team would form an emergent order, a "closely organized whole", even with each member merely following their independent judgement.

The take-home message of the jigsaw example is, of course, that scientists are continually adjust their respective line of research to the results achieved up to date by fellow scientists, and that the network of these minute interactions form the evolutionary and emergent spontaneous order which we know by the name of scientific consensus (Polanyi 1951, 88). Similarly to how a collection of thousands of individual starlings (a "*murmuration*", to be precise) adjust their behaviour in relation to their respective surroundings, forming a mesmerising unified order, scientists continually adjust themselves to their immediate, and to a lesser extent, distant, surroundings. Particularly fascinating for Polanyi was the question of how thousands of individual scientists sustain the systemic unity of science. Namely, Polanyi speculated that there must be a force of "self-coordination by mutual adjustment" underlying any scientific community (Polanyi 2009, 71).

Ultimately, Polanyi’s work on the self-organization of science resulted in the definition of the *principle of mutual control,* or the notion that scientists self-organise by adapting to their local environment: “Each scientist is both subject to criticism by all others and encouraged by their appreciation” (Polanyi 2009, 72). Whilst only scientists working in closely related fields are competent and legitimate to exercise direct authority over one another, the scientists’ personal fields effectively form ”chains of overlapping neighbourhoods”, which extend over the entire range of science. Although closely related neighbourhoods will be effected more radically by change in neighbouring fields, ultimately the whole web of science is connected by these overlapping authoritative connections of varying strengths, or ”mutual control”. By these means, Polanyi (2009, 67) writes, “scientists scattered over the globe […] coordinate themselves to produce a systematic expansion of science”. This self-organization not only maintains a mediated consensus amongst scientists, but also protects science against any external forces challenging the authority of science.

Such external forces challenging the authority of science, of course, abound. As a wealth of scholars and scholarly studies have described, particularly within the fields of sociology of scientific knowledge, science and technology studies and philosophy of science, scientific discovery does not happen in social isolation. Indeed, we find that scientists and the scientific community are not lone actors in the scientific enterprise, and that its spontaneously emerging dynamics of mutual control and adjustment are shaped significantly by external forces such as governments, ministries, national academies and, to no lesser extent, markets. I wish to emphasise here, paralleling Polanyi, that there is nothing prescriptively pathological in the state-involvement of science or other external influences—quite the contrary, given that it is likely that scientific autonomy would largely cease to exist without external funding and particularly state protection. Even Karl Popper, an adamant defender of open science, writes that the “*free competition of thought*”, for Popper a prerequisite for scientific progress, is largely dependent of its safeguarding by political institutions (Popper 1986/1957, 154-155). Yet when scientific opinion is shaken by extraneous central direction of science, we are stepping onto thin ice. As Polanyi (1951, 77) writes, “extraneous direction of science is mischievous precisely to the extent to which it is effective”.

To elaborate on this, and to highlight the crux of Polanyi’s work on scientific freedom, let us return to the example of the jigsaw puzzle. Imagine now someone, believing in the “paramount effectiveness of central direction”, were to intervene with the puzzle-piecing, trying to improve the task-at-hand by applying methods of central administration (Polanyi 1951, 35–36). This would be most unhelpful, and indeed it would be deeply disturbing towards the puzzle-piecing team. As Polanyi notes, it is impossible to plan in advance the necessary steps by which the puzzle should be pieced together. Therefore, all that the central administrator could by any means achieve is form a hierarchical order directed from a single centre. Each member of the puzzle-piecing team would henceforth have to wait for individual directions from this superior body, and remain in standstill until instructions were ordered. ”In effect”, Polanyi (ibid.) describes, “all participants except the one acting as the head of the organization would cease to make any appreciable contribution to the piecing together of the puzzle. The effect of co-operation would fall to zero”.

For Polanyi, a paramount case of failed central direction was the Lysenko affair[1], which seemed to very profoundly trouble him. Lysenkoism, now regarded an entirely non-scientific political movement, rejected the Mendelian concept of the gene and assumed instead the heritability of acquired characteristics. This was in Polanyi’s (1951, 61) mind a “rapid destruction of a branch of science, caused clearly by the fact that the conduct of research had been placed under direction of the State”. Scientific inquiry was subjugated under the values of the Communist party, namely dialectical and historical materialism (with which Lysenko’s ideas were claimed to resonate) instead of maintaining its status as an open-ended inquiry. Not only did science lose its explorative edge under

[1] Since Polanyi’s writings, the Lysenko affair has admittedly become somewhat of a cliché when obstructing to state involvement in science. See Feyerabend (1993/1975: 160): “They [scientists] oppose all political interference, and they fall over each other trying to remind the listener, or the reader, of the disastrous outcome of the Lysenko affair.” Indeed, as I discuss in this paper, opposing all political interference is not necessary. However, opposing the most “exploitatively biased” interference (of which Lysenkosim is an extreme case) might be generally justified.

State supremacy, but it was also exploiting a false prophet. Under state control, Mendelism was refuted as mere “metaphysics”, a “bourgeois science” (Polanyi, 1951, 64). This was not self-organised puzzle-piecing, but instead a centrally directed attempt at approaching a prescribed picture which simply was non-coherent with any empirically justifiable reality. Consequently, Soviet plant-breeding stations were operating on regressive lines which were “abandoned as fallacious in the rest of the world about forty years earlier” (Polanyi, 1951, 66). Polanyi, writing at the dawn of the 1950’s, hoped that one day the Soviet Government would recognize the error of their attempts. Unfortunately, this did not occur until thousands of Soviet scientists were imprisoned, fired or executed for their opposition of Lysenkoism.

Is it then, Polanyi (1951, 36) asks, all that can be said that the “avenues of potential discovery” are most effectively explored if we let scientists loose to choose their own problems? Polanyi, quite laconically, answers his own question: “In a way it is.” That is, the spontaneous order of scientists in the pursuit of discovery really is akin to a puzzle-piecing team working on a jigsaw puzzle. However, as Polanyi himself notes, one major difference exists between the two endeavours. A jigsaw puzzle, with certainty, has a definite and final solution (or else it is a faulty one). Science, on the other hand, is a *fundamentally incomplete* endeavour. The scientific puzzle is a process of constant dynamic evolution and has no uniform purpose as such (see e.g. Butos and Koppl 2003). There is no way to statically fix this puzzle together correctly, and there is at least not to our knowledge no means by which it can reach an ultimate solution (and, inductively, we could never be certain whether such a state were reached). As philosopher of science Nicholas Rescher (1998, 25) writes: “Thus our scientific exploration of an immensely complex world imposes upon us a cognitive task of potential endlessness–one that literally knows no limits.” This is writ in the very essence of scientific progress: We cannot possibly know, at the time of inquiry or discovery, where science is heading. In the terms of complexity scientist Stuart Kauffman (2008), the scientific enterprise is constantly exploring the configuration space of the *adjacent possible*, a truly novel state space which is not comprehensible or probabilistically definable until it emerges from the self-organising processes of scientific inquiry. Interestingly, this implies that whatever piece of the puzzle a scientist is trying to fit, the scientist always remains, at the time of discovery, fundamentally uncertain about what this piece exactly is and what its implications (or "impact", as discussed below) shall be for the rest of the puzzle.

The incompleteness of scientific inquiry was a paradox which seemed to thoroughly inspire Polanyi. If the jigsaw puzzle of science is fundamentally incomplete, how do scientists know where to seek for new pieces? Here, Polanyi introduced his perhaps most famous epistemological concept: Tacit knowledge (see Polanyi 1969, 1974 and 2009). Polanyi (2009, 21–22) continues: How exactly is it that scientists have the ability to see problems of which "the rest of humanity cannot have even an inkling", or problems which will lead to great discoveries? As Polanyi (ibid.) contemplates, all research must necessarily start from a problem, but how is it possible to identify a problem, let alone a good problem? In response, Polanyi made the now famous case that scientists embody considerable amounts of "tacit", or embodied yet inexplicable, knowledge, which affords intimations of coherence with a "hidden reality" which we cannot, at the time of perception, explicitly specify. In other words, at the time of discovery of a problem, scientists are often unable to specify or explicate the root of this discovery, yet they are able to make the discovery regardless. There is a degree of serendipity, or skilled randomness, here, where, paraphrasing the 18th century historian Horace Walpole, scientists are always making discoveries, by *accidents and sagacity*, of things which they were not in quest of. As Polanyi elaborates, "We make a discovery and yet it comes as a surprise to us," but for such discoveries to take place to begin with, we require not so much explicit expectations as serendipitous and tacit exploration (Polanyi 1997, see also Forstater 2003). Scientific discovery, in this sense, consists of the serendipitous "actualization of potentialities" latent in the environments we explore (Polanyi 2009, 89; for recent discussion on serendipity, see Copeland, 2017; Meyers, 2007; Merton and Barber, 2004). If we do not open-mindedly place our explorers "in the midst of potential discoveries”[2], we cannot expect to encounter such discoveries (Polanyi 2009, 89). This accidental sagacity, it seems, is one of the more underappreciated aspects of scientific inquiry. Incidentally, and importantly, it is also an explorative one.

Politically, we are dealing here with a question of tangibility. How are we to convince those political and social institutions investing in science that we should let a spontaneously ordered scientific community autonomously advance its explorative functions? Indeed, must not, Polanyi (2009, 92) asks, such inquiry “appear adrift, irresponsible, selfish, apparently chaotic?” Should we not instead just exploit any “impactful” knowledge we are currently aware of, knowledge which we are certain to advance social or economic well-being or other utilitarian values? To provide grounds for better understanding and appreciation of explorative functions in self-organising systems such as science, I move on now towards a brief inquiry into some natural systems which succeed precisely because of their explorative cutting edge, lymphocyte cells and ant colonies. These analogies illustrate how exploration, and the degrees of freedom and randomness that afford it, are fundamental for a system’s capacity to adapt to a rapidly changing environment.

3 Exploration and Exploitation

By now it has been established that scientific inquiry and discovery bear significant traits of spontaneously arising order, or, in other terms, that science is a largely self-organising complex and adaptive system. Moreover, it is one constantly in the process of entering a previously unexperienced and novel configuration space. I have hinted above that such systems have two main functions, exploration for and exploitation of information. I have also proposed briefly above that there is a

[2] Polanyi, with his discussion of anticipated ”hidden realities” and potentialities for discovery, was approaching a line of thought similar to what emerged later as the concept of the ”affordance” in ecological psychology (Gibson, 1979). Affordances, or functionally relevant features of the environment, ”invite” certain types of behavior: similarly to how toys ”afford” playing for children, some environmental features might afford discovery for a scientist equipped with sufficient skill-sets (or tacit knowledge). Discoveries are, in this respect, unexpected “encounters with the world” (Reed, 1996).

tendency in the current climate of science policies to underemphasise the import of explorative functions (and conversely, overemphasise the value of specific exploitative functions). However, much yet needs to be elaborated. Particularly, to vindicate the role of the explorative functions of science, it is worth to begin with appropriate analogies how natural self-organising systems make use of explorative activities and subsequently draw appropriate parallels with scientific inquiry. Drawing on the work of complexity scientist Melanie Mitchell (2009), the two cases briefly illustrated below are lymphocyte cells and ant colonies.

I begin with the former. The human immune system serves the main function of protecting the body from pathogens, or "viruses, bacteria, parasites, and other unwelcome intruders" (Mitchell 2009, 172). The immune system, of course, is a hugely complex one, whose functions are played out by "trillions of different cells and molecules circulating in the body, communicating with one another through various kinds of signals" (ibid.). Since the system as a whole is far too complex to discuss here, let us focus here on a single type of white blood cell, the lymphocyte.

Created in the bone marrow, two major types of lymphocytes are antibody-releasing *B cells*, which fight viruses and bacteria, and regulatory and invader-killing *T cells.* As any other cell in the body, lymphocytes' surfaces are covered with receptors that can bind to certain shapes of molecules that the cell might encounter (Mitchell 2009, 173). This is the means by which cells, in general, receive information: "whether or not a receptor binds to a particular molecule depends on whether their physical shapes match sufficiently" (ibid.). Should a lymphocyte encounter an antigen (a pathogen molecule), the molecule binds to one of the lymphocyte's receptors, resulting in the lymphocyte recognizing the particular antigen.

Of course, there are astronomical numbers of potential pathogens which could invade the body, and thus there are no means for an immune system to predesign a perfect, or even remotely functional, set of lymphocyte receptors. Here we encounter the explorative function of the immune system: The immune system "*employs randomness* to allow each individual lymphocyte to recognize a range of shapes that differs from the range recognized by other lymphocytes in the population" (Mitchell 2009, 174; emphasis added). In other words, by employing randomness and large degrees of freedom in the configuration of receptor shapes, this exploratory system allows for the binding of lymphocyte receptors to an otherwise unintelligible amount of antigens.

Here is where the self-organizing beauty of the immune system starts to unravel. Once a threshold of sufficiently strong binds is exceeded with B cells, whilst similarly bound T Cells give B cells ”go ahead" signals, B cells are activated and start to release antibody molecules into the bloodstream. These molecules "bind to antigens, neutralize them and mark them for destruction by other immune system cells" (Mitchell 2009, 174). But the story of self-organization only starts here, since activated (i.e. successful) B cells now migrate to the lymph node where they divide rapidly, producing large numbers of similarly shaped daughter cells—this is, by the way, the reason for swollen lymph nodes when you are ill—of which many carry random mutations that further alter the shapes of receptors. These altered receptors are then "tested" on antigens within the lymph node, with the unsuccessful dying after a short period. However, the surviving enhanced copies of B cells are unleashed into the bloodstream, where the aforementioned iterative process is repeated. This cycle of enhancement by "natural selection" is continued from days to weeks until pathogens are eradicated from the body. In terms more familiar to us by now, after random exploration, the system exploits this acquired information to finally rid itself of unwished-for pathogens.

Another, perhaps more intuitive, analogy of exploration and exploitation can be found in the operation of ant colonies. Very briefly[3], the foraging of many ant species works more or less in the following way. Foraging commences with ants in a colony setting out to explore in different, *random* directions. When a food source is encountered, the ant returns to its nest, leaving behind "a trail made up of a type of signalling chemicals called *pheromones*" (Mitchell 2009, 176–177). When other ants

[3] For a more detailed description of ant foraging, the reader is directed to the aptly named brilliant book "Journey to the Ants: A Story of Scientific Exploration", by Hölldobler and Wilson (1994). A simple agent-based NetLogo model is also openly available for hands-on exploration of ant foraging (Wilensky, 1997).

encounter this trail, they are likely to follow it to the potential food source; if the food source still exists and is located, the ant reinforces the trail back to the nest with its respective pheromones. If a trail is not reinforced during a certain time period, the pheromone trail evaporates. Note that by this relatively simple function, colonies of ants effectively accumulate and communicate information regarding the locations and quality of food (ibid.). In other words, the ant colony randomly explores the surrounding space for food, whilst simultaneously exploiting information of existing food sources.

What these self-organizing systems have in common is what Mitchell (2009, 183) calls the "continual interplay of unfocused, random explorations and focused actions", or in other words, random exploration and focused exploitation. In fact, it is precisely this intricate self-organizing maintenance of balance between the two functions which makes these self-organizing systems so successful in adapting to rapidly changing environments characterised by high uncertainty. This balance is known as the *parallel terraced scan* (Rehling and Hofstadter, 1997), or the "simultaneous exploration of many possibilities or pathways, in which the resources given to each exploration at a given time depend on the perceived success of that exploration at that time" (Mitchell 2009, 182). However, it is important to emphasise that without active random exploration, ant colonies would quickly run out of food sources to exploit and the immune system would be incapable to identify and destroy pathogens. In other words, explorative and exploitative functions are fundamentally interlinked, and afford each other's' existence. Whilst Mitchell (2009) discusses how similar processes appear in a great number of information processing systems, one particularly striking example relevant for our purposes here is left untouched. This is the self-organizing nature of science, it too, I argue, being a feat of continual interplay between "random explorations" and "focused actions" (Mitchell 2009, 183) driven by scientists' perceived needs.

For scientific communities to encounter unexpected and potentially important or even revolutionary discoveries, they need to let scientists loose into "the midst of potential discoveries," as illustrated by Polanyi's jigsaw example above. With a sufficient amount of well-equipped scientists exploring these tacitly guided pathways, establishing connections with yet unknown hidden realities or potentialities (or affordances for discovery, see footnote 2), some are bound to—similarly to ants foraging for food sources or lymphocytes engaging with pathogens—encounter discoveries from which they can leave a trail of "pheromones"[4] back to the rest of the scientific community. As discussed above, the scientific community has an in-built tendency to adjust itself to such new findings (although not always without controversy and authoritative scepticism, as perhaps most notably discussed by Kuhn, 2012/1962). This is where the exploitative function of science commences, starting with what Polanyi called self-coordination by mutual adjustment. Aware of the new "pheromone trail" of scientific discovery, scientists equipped with this new knowledge may start the iterative process (not unlike the case with lymphocytes) of step-by-step elaborating (exploiting) this finding, and eventually using it as a new basis for exploration. Quoting (Polanyi 2009, 67), "[We] have here the paradigm of all progress in science: discoveries are made by pursuing possibilities suggested by existing knowledge." Yet any existing knowledge is necessarily born from explorative functions. Without exploration, or epistemic foraging (with enough degrees of freedom), there is nothing worthwhile or informative to exploit.

The question of how we should seek to optimise the relation between the two functions of exploration and exploitation in scientific inquiry remains unanswered. That is, how should we seek to balance or optimise between the "simultaneous exploration of many possibilities or pathways" with appropriate exploitation, based on the perceived success of these explorative activities? Unfortunately, no complete answer to this question exists today, and arguably finding one is impossible even on the most deeply philosophical or theoretical level. Although optimising between exploitation and exploration has been studied considerably within probability theory (in the context of so-called "multi-armed bandit problems"), the case remains that whilst optimisation is possible in static and stable conditions, the same does not apply to the dynamic conditions (such as that of scientific

[4] Today, this trail could consist of anything from personal connections or social networks to online citation indices.

inquiry). As Cohen et al. (2007, 934–935) note, whilst "[r]egulating the balance between exploitation and exploration is a fundamental need for adaptation in a complex and changing world," no "universally optimal algorithm has been described that prescribes how to trade-off between exploration and exploitation in non-stationary environments". On similar lines, Kauffman (2008, Ch. 10) discusses the shortcomings of probabilistic reasoning at the face of emergent novelty: When a non-ergodic system, such as science, enters the adjacent possible, or a previously unvisited and emergent state space, the sample space of all the possible outcomes is undetermined and we cannot make fully reliable probability statements pertaining to optimisation strategies. Thus science always lies at the face of uncertainty, and decisions pertaining to exploration–exploitation trade-offs remain fundamentally incomplete.

Since the very essence of science arises from its dynamic and novel nature—we cannot, inductively, have but an inkling of where a scientific problem will take us, how a discovery can be made or how a novel discovery will manifest itself after a run of decades—any prescribed attempt at optimisation is most likely to lead to disappointing results. This was, in essence, Polanyi's case against central direction of scientific inquiry, and interestingly Polanyi also maintained a strong opinion about how we should arrange the "parallel terraced scan" of scientific inquiry, which was by means of the "principle of mutual control" discussed above. Simply, Polanyi (2009, 72) suggests, let the mutually adjusting authoritative relations spontaneously arising within the scientific community decide when to explore and when to exploit, or when to transfer "resources from areas where standards are lower, to points at which they are higher".

Polanyi's traditionalistic suggestion is, however, not unproblematic. In fact, its evident conservatism is what seems to hinder much of theoretical and methodological progress today. Indeed, how do we proceed when, as Feyerabend (1993/1975) lamented, scientific inquiry of the self-organising sort gets "harderned" and "intolerant", approaching a normal-scientific near-equilibrium state which allows for very little explorative advances? In other words, the self-organisation of scientific inquiry seems to often arrive at standstills caused by traditionalistic scientific institutions themselves.

Whilst the traditionalistic notion of scientific autonomy proposed by Polanyi is perhaps a good baseline for advancing exploitative functions of scientific inquiry, we should not be naïve about the notion that these authoritative relations within science, which Kuhn so aptly described in his classic work[5], can also stifle explorative functions and prevent disturbances. This is particularly the case with the more rigid scientific disciplines, where esoteric epistemologies and orthodox demarcation criteria can largely preclude explorative inquiry and the development and implementation of novel methodologies.

I wish to emphasize that new advances have been proposed for tackling these challenges, or to unjam the proverbial scientific pinball machine. To my mind, the most promising approaches reside in randomised scientific funding mechanisms (which would allow for broader and bolder epistemic exploration, see Avin, 2018), the promotion of cognitive diversity in scientific inquiry (since this affords the exploration of a broader epistemic state space, see Pöyhönen, 2017), a revived appreciation of the role of luck in scientific inquiry and success (Pluchino et al., 2018; Copeland, 2017) and, more broadly, Open Science and early publishing (since these together allow early, broad and accessible distribution of scientific leads, diversifying the epistemic landscape and affording new intentional and serendipitous encounters for people both inside and outside of academic institutions, see Copeland, 2017). These together are somewhat akin to injecting Feyerabend's (in)famous "dose of anarchy" into the scientific system, although as hinted above I would rather perceive it metaphorically as nudging a jammed pinball machine.

[5] Kuhn himself was, however, very fond of scientific exploration. Funnily enough, in the preface of *Structure* (Kuhn 2012, xl-xli) Kuhn notes that if it weren't for "random exploration" and "exploring fields without apparent relation to history of science" he would have not come by his revolutionary ideas.

Perhaps then, instead of striving for quantifiable "impact" by advancing exploitative functions (which the scientific community has been quite capable of achieving by itself, even though not unproblematically, as discussed below), external intervention should particularly seek to *safeguard and increase explorative advances*. In other words, state intervention should be *exploratively*, and not *exploitatively*, biased. As is discussed below, to some concern this is quite the opposite of what is happening within the current climate in science policies.

4 Exploitative Biases

"History is merely a list of surprises," I said. "It can only prepare us to be surprised yet again."

Kurt Vonnegut Jr., Slapstick

The world around us is changing swiftly, perhaps more so than ever experienced before by our species. Today's technology is the museum piece of a very near future, our biosphere is changing more rapidly than ever before in human existence and social and economic order is, it seems, in midst of turbulences not incomparable to those experienced by Polanyi's generation. If history, as Kurt Vonnegut Jr. writes, is for us but a "list of surprises," it seems like the history of tomorrow will be more surprising than ever before. In the face of such contingencies, how can we know which problems our scientific community should seek to address today to answer the quandaries of tomorrow? Specifically, it seems to me, this question arises in the context of targeted research policies and the associated notion of the "impact" of scientific research.

There seems to exist an excessive optimism in much of today's science policy that such problems can be readily identified. Certainly, it seems, we know what to expect: We are all familiar with the list of known unknowns including the likes of climate change, social inequality, mass extinction and digitalisation (see e.g. Barnosky and Hadly 2015 for a great overview). Undoubtedly, these identified challenges should serve a basis for scientific exploitation. Like ants encountering a new food source, our best equipped researchers have found and identified these problems and there is nothing at all wrong with setting strategic goals or funding and appropriate science policies to address these concerns. Nor is there any prescriptive evil in expecting that the scientific community of today has, to an extent, the moral responsibility to conduct "impactful" science when dealing with these concerns. After all, the history of science is ridden with cases where the scientific community, armed with its brightest intellectuals, has sought to apply its expertise to tackle some predefined problem. The Manhattan Project, conceivably, is the most striking example of such a case. Strategic science, we notice, is often relied upon in times of crises, and most remarkably so for state-led purposes.[6]

Crises, it seems, invite exploitative scientific activities. This, perhaps, builds on a natural reaction of humans to grab for something tangible when in distress. However, I maintain that taken *too far* this exploitative bias is a fallacious and regressive mind-set. Now, more than ever, we should seek to explore. We might have identified some unknowns facing us, but we might not yet know the possibly more important unknown unknowns, the most surprising and unexpected proverbial "pathogens" or discoveries pertaining to new sustainable or regenerative forms of life lurking ahead of us. Only by letting our scientists loose, with significant degrees of freedom and patience, to set out on unexpected paths to establish contact with these hidden realities, where discovery might be expected but also where it is uncertain, can we prepare to adapt or respond to the unforeseeable events of tomorrow. We must restore our faith in explorative science—not science subjugated under buzzwords such as scientific or social impact or "missions," but science as a carefully and slowly emerging self-organising spontaneous order with significant degrees of explorative freedom.

[6] Although Polanyi (1951: 44) acknowledges more benign cases of applied research, such as the development of telecommunications. However, even in the case of telecommunications (particularly post Second World War) this "applied" research was often extremely explorative by nature. For instance, Bell Laboratories – arguably the lead innovators of 20th century telecommunications research – fostered exceptional creative freedom (see Soni and Goodman, 2017). The same could be argued of the Manhattan Project, which drew its applications from large bodies of basic research.

A ghost of Laplace's demon seems to haunt science policy, with an overconfidence towards the idea that we can, by identifying exactly the crucial scientific problems of today, predict or tackle the relevant problems of tomorrow. The whole notion of funding "impact-driven" or "strategic" science, for instance, draws from this notion that we could, somehow, know what is or shall be of importance or utility in the future. National examples of such strategic science policies abound, and more recently such an ideal was advocated internationally by the High Level Group (European Commission 2017, 6) in the context of adopting a "mission-oriented, impact-focused approach to address global challenges" by setting research and innovation "missions" (for EU's upcoming 9th Research and Innovation Framework Programme).

Yet there are two sound logical reasons, sketched originally by Popper (1986/1957), why we should remain cautious towards such ideas: (1) To predict the relevant problems of tomorrow we would require full knowledge of the relevant problems of today (which is, in practice, impossible due to the cognitive inexhaustibility of scientific inquiry coupled with the complexity of natural systems, see Rescher 1998). More fundamentally (2), since the course of human history is strongly influenced by the growth of scientific knowledge and we cannot predict the growth of scientific knowledge itself (since, logically, successful prediction would imply making these scientific discoveries at the time of prediction), we cannot predict what is scientifically relevant tomorrow. As science enters its adjacent possible (Kauffman, 2008), we cannot wholly know what to expect.

Return to the proposed FP9 R&I missions, which "should foremost be easy to communicate and capture public imagination" (European Commission 2017, 6). One wonders how informative such missions can be if they are so easily imaginable[7]. Could, for instance, Claude Shannon's esoteric hybrids of Boolean logic and electrical circuits have been perceived to have potential for capturing,

[7] Also worth asking is this: If the problems are so easily communicable and imaginable, i.e. the scientific discovery has already been made and is communicable, is not the implementation of these problems a policy problem rather than a scientific one? Is not the success of such missions primarily a political, and not a scientific, endeavour? The risk here is that a failure of such a mission will be blamed on science, even if scientists have very little political power to put the discoveries to practice.

in the late 1930's, the "public imagination," or to have any social impact at all? I think not, yet precisely this unimaginable hybrid underlies all digitalisation (Soni and Goodman 2017.)—ironically a theme underlying many targeted or strategic research policies today. Only post-discovery have such ideas seemed "impactful," and that is indeed what makes them scientifically revolutionary: They are unpredictable and unimaginable. In an explorative fashion, Shannon reminisces his time at Bell Labs, where he made his greatest contributions to information theory: "I had freedom to do anything I wanted from almost the day I started. They never told me what to work on." (ibid.)

Simply, if we, due to overconfident exploitative endeavours, lose the randomness, serendipity and the explorative edge of science, we risk missing the unrevealed and unimaginable solutions to unpredictable future concerns. Mitchell (2009, 181) writes that "[i]ntrinsic random and probabilistic elements are needed in order for a comparatively small population of simple components to explore an enormously larger space of possibilities". In the context of science policy, I could not agree more. As exemplified above with Polanyi's jigsaw example, we are in many respects dependent on scientific exploration, or explorative advances to increase our adaptive capacity towards the dynamic, ever-changing world we inhabit. This includes research that might seem like silly, playful[8] tinkering or entirely non-impactful—as Feyerabend (1993) famously described, even Galileo's retrospectively glorified scientific advances were at his time deemed a "silly cosmology". But this also includes a diverse body of research which, particularly in the social sciences, challenges our contemporary ways of organising society and envisages alternative futures and forms of life. What I argue below is that current scientific systems are far from promoting research activities of this kind.

Firstly, some recent trends risk to narrow too greatly the latitudes of explorative activities. Here we could imagine a central controller deliberately and a priori restricting the latitudes of exploration for Polanyi's puzzle-piecing team to those epistemic landscapes where previous discoveries have been

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[8] For a fascinating account of how playfulness, curiosity and explorative creativity can lead to scientific breakthroughs, I again encourage the reader to see the Claude Shannon biography by Soni and Goodman (2017).

identified. Take, for instance, the aforementioned trends in favouring the funding of strategic or commissioned research with the aim of increasing policy-relevant scientific inquiry and providing evidence-base for policy-making. A recent opinion piece in *Nature* captures this notion, suggesting that "research funders should support policy-relevant work only when scientists have given serious thought to policy impact," that policymakers should help researchers "to set research questions and perhaps even the research plans," and finally that these "mandates will force universities to take policy impact seriously" (Tyler, 2017). Whilst in some cases this might admittedly be a preferable procedure, accepting such practices as a baseline for research funding would significantly run the risk of reducing the chances of encountering the *unexpected* sets of solutions to whatever the policy-relevant problem is perceived to be. Reducing the diversity of (explorative) scientific inquiry reduces the variety of solutions subject to potential selection and application (exploitation) in the future. Since we cannot be certain of what counts as valuable exploitative material tomorrow, we should actively maintain and safeguard high variability in explorative inquiry. As Popper (1986/1957, 159) writes:

> ”The mainspring of evolution and progress is the variety of the material which may become subject to selection. So far as human evolution is concerned it is the ‘freedom to be odd and unlike one's neighbour’–‘to disagree with the majority, and go one's own way’. Holistic control, which must lead to the equalization not of human rights but of human minds, would mean the end of progress."

Second, related to the use of impact metrics (such as citation and publication indices), another form of exploitative bias is the increase of incentives to *report* exploitative material. This is a peculiar sort of exploitative bias, since it not only restricts explorative activities but also endangers the careful and thorough exploitation of scientifically relevant information. Again, drawing an analogy to Polanyi's puzzle-piecing, we could imagine this team being incentivised towards reporting discoveries where none such exist. The implications for the self-organisation of science are obvious. Smaldino and McElreath (2016) have recently made an insightful case in exposing how practices such as impact assessments might lead to what they call the “natural selection of bad science”. The core argument follows: “when researchers are rewarded primarily for publishing, then habits which promote publication are naturally selected”. Incentives for publication quantity, as advocated by a large body of science policy today, “drive the evolution of poor methodological practices” and “high false discovery rates,” which are key components of the ongoing replication crises and ultimately undermine scientific progress (ibid.). Thinking back to the ant colony, this is little surprise: If ants were biased to report exploitative food sources where none exist (with the risk of losing their job for not doing so), false discovery rates would soar, leading to lesser exploration for real discoveries. This is also exacerbated by incentives to report “innovative”, “groundbreaking” and “novel” findings—signals which another study showed a 25-fold increase in from 1974 to 2014 (Vinkers et al. 2015, cited in Smaldino and McElreath 2016). Simply, impact-biases such as publication pressure not only hinder epistemic exploration, but also make exploitation (e.g. reporting of findings) unreliable, as is evident with the ongoing “replication crisis” in several fields of research today (ibid.).

Whilst some of these problems can and perhaps should be attributed to individual misdemeanours, the underlying institutional frameworks, which by imposition of external incentives “distort and corrupt the social processes” they intend to monitor, must also be revised (Campbell 1976, 49 in Smaldino and McElreath 2016). These exploitatively biases hinder the self-organisation of science in at least four domains: (1) Scientists waste time on exploiting false discoveries instead of carefully exploring for or exploiting real ones; (2) Society loses resources in funding such activities (with, ironically, no real demonstrable impact); (3) Scientists, innovators or public figures with interest in science are lost in the maze of exponentially increasing information content, where noise overwhelms signal (see Sarewitz, 2016). But most importantly, (4) we are not incentivising the highly fallible (and consequently often seemingly “impactless”) bold exploration for the identification of the unexpected discoveries, which might be of acute relevance for the resilience and adaptive capacities of our socio–ecological systems.

I believe Gigerenzer and Marewski (2015, 436–437) sum the case in point here: The "mindless calculation" of citation and publication counts, inter alia, fuel "a steady stream of work of average quality and keeps researchers busy producing more of the same. But it makes it harder for scientists to be innovative, risk taking, and imaginative."[9] If scientific freedom is to be taken seriously today, it must begin with a fundamental right to explore for and exploit scientific information with an appropriately slow pace, with caution and with care. Pressures for increasing quantifiable impact might in this respect to more harm than good. The scientific system is self-organising, but its self-organisation might be significantly hindered by institutional frameworks and social practices which claim to favour efficiency. A risky, slow and fallible science isn't perfect, and it might seem like a valid notion to hasten and optimise the scientific process. But "perfectionism," writes Polanyi (2009, 85), "is a program of destruction".

5 Towards a Society of Explorers

> "One wonders how the great physicists [Nobel Laureates Planck, Einstein, Perrin, Millikan, Michelson, Rutherford, Aston, Chadwick, Barkla, Heisenberg, Compton, Franck, G. Hertz, Rubens, Laue, Joliot, Fermi, Urey, Anderson, W.H. and W.L. Bragg, Schrödinger, Dirac, etc.] would have fared if, before embarking on their investigation, they had had to get a certificate of its social usefulness from a scientific directorate." (Polanyi 1951, 82–83.)

I have proposed in this article that there is intriguing potential in describing and analysing the self-organisation of scientific systems in terms of exploration–exploitation trade-offs. As Polanyi noted in the 1940's, only by affording the scientific system with significant degrees of freedom can it

[9] This is not to even mention how these impact measures are suspect to the non-meritocratic "Matthew effect", where the "rich get richer" (Merton, 1968), and are generally a poor indicator of scientific quality (Bol et al., 2018; Brembs, 2018).

explore its epistemic landscape with enough diversity to ensure efficient exploitation, and thus, emergent self-organisation. However, it is evident that the self-organisation of science is suspect to several biases and disturbances, which originate on the one hand from external institutional influences, and on the other, from scientific communities themselves. Most notable of these are what I have called in this text "exploitative biases," which include policies such as scientific impact assessments and targeted research strategies, as well as social norms which favour efficiency and quantity of output over slow and careful exploration for and exploitation of epistemically significant encounters with the world.

The broad history of science suggests that the impact or social utility of scientific inquiry is notoriously difficult to formalise a priori. As discussed above, the reasons for this are at least partly logical (as discussed by Popper 1986/1957). As such, science funders and other decision-makers should entertain patience with respect to impact assessments and strategic research policies. Exploiting new epistemic avenues is only possible after they have been identified, and identification is a slow process which requires significant degrees of freedom, and often even random or serendipitous encounters.

It has been illustrated above that maintaining a balance between explorative and exploitative functions, or the "parallel terraced scan" (Rehling and Hofstadter, 1997), increases the adaptive capabilities and resilience of the self-organising scientific system.[10] In a dynamic and changing environment, however, it is noteworthy that these exploration–exploitation trade-offs cannot be fully optimised. However, in general, maintaining significant degrees of freedom and diversity in scientific inquiry increases our chance to adapt to a rapidly changing environment. A colony of ants cannot,

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[10] A colleague of mine raised the question of whether it is reasonable to ask if the proverbial "immune system" of science, with its "lymphocyte" scientists, is in fact spreading lymphoma, i.e. whether science is creating more problems than it can solve. This is a valid case for discussion, albeit one which would require an essay of its own. My brief answer follows: in any case, we cannot halt scientific progress at this phase, where fossil-fueled technology and forms of life are at their most destructive. Moreover, the extent to which scientific exploration can be blamed for industrial applications is also ambiguous. The question remains: how to ensure that scientific "exploration" is moral, ethical and sustainable?

ultimately, rely on exploitation of a pre-set number of food sources and must always actively explore for new ones. Nor is this a concern for the scientific community alone, since society as a whole is entirely dependent on the adaptive functioning of science, even if this is not all that often evident in public discourse. Without strong degrees of freedom in scientific exploration, and time and space for careful exploitation of this knowledge, society fails weakens its adaptive capacity and resilience. Therefore, more than anything else, I argue, we should re-establish grounds for safeguarding and vindicating explorative, creative and playful life and freedom of inquiry (inside and outside of academia), in a “free and dynamic society”. With Polanyi (2009, 82–83; 2010, 19), I call this a “Society of Explorers”.

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