On the emergence of the system of modern science

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Abstract: It is widely acknowledged by both historians and scientists that in the course of the 17th century in Western Europe a new way of thinking about nature and knowledge took hold and new methods of obtaining knowledge of nature were proposed and tried. The spectacular success of this new science was recognized even at the time, and the genius of its leading practitioners was widely appreciated. There had been brilliant upsurges in scientific activity in various times and places before this, but this episode is unique in that, rather than lapsing into stasis or abandonment, it has continuously grown in both results and participants to the point where it is an integral part of modern civilization. Many reasons have been proposed for the origin and the success of the new science, but none convincingly address why this scientific revolution should have the staying power that others have not. The hypothesis developed here is that the innovations of the 17th century—changes in methodology, epistemology, ideology, and institutions—coalesced to form a radically new social arrangement in the form of a self-maintaining system of scientific processes, an arrangement that can be formally described as an anticipatory social system.

Keywords: science, scientific revolution, emergence, social systems, anticipatory systems.

I. INTRODUCTION

Over the course of the 17th century there occurred remarkable changes in the mode of understanding and the methods of investigating natural phenomena. Historians of science refer to this multifaceted transition as "the Scientific Revolution", and the capitals and the definite article are there with good reason.[1] This was certainly a "golden age" of scientific development, but there had been significant periods of scientific development before—in Greece from about the 4th to the 2nd century BCE stemming from the philosophical schools of Plato and Aristotle, in China at roughly the same time in the era of the Hundred Schools of Thought, in northern India from about the 4th to the 6th century, in the Middle East from about the 9th to the 12th century as Greek manuscripts were translated into Arabic and studied, and in medieval Europe in the 12th to the 14th century as Arabic manuscripts including those Arabic translations of Greek manuscripts were in turn translated into Latin.[2]

All of these periods of progressive intellectual activity came to an end—either they simply lapsed into intellectual stasis, as was the case with the Greek[3] and medieval European episodes, or they were not resilient enough to survive societal changes (political regime change in the cases of China and northern India, and religious in-turning as a result of external threats in the case of the Middle East). In contrast, the intellectual revolution of the 17th century has initiated a form of scientific activity which has continued to grow, in discoveries, in active participants, in application, and in societal acceptance and prestige, to this day, and which, despite major political upheavals, shows no signs of tapering off.[4] But what was different about this particular revolution, what explains its staying power? The hypothesis developed here is that the system of science as we know it originated from a coalescence of methodological, epistemological, ideological, and institutional innovations that emerged during the 17th century, forming a radically new social arrangement: a self-maintaining system of scientific processes.

Popular accounts of 17th century science emphasize the numerous significant discoveries that were made in a relatively short period.[5] And significant discoveries there certainly were. Magnetism was investigated and the Earth's magnetic field described (Gilbert), laws characterizing the (non-circular) planetary orbits were deduced (Kepler), a new theory of motion and of falling bodies was developed (Galileo), the circulation of the blood and the role of the heart as a pump was described (Harvey), atmospheric pressure was investigated and the existence of a vacuum proposed (Torricelli), relations between gas pressure and volume were formulated (Boyle), a wave theory of light was proposed (Huygens), the spectrum of white light was discovered (Newton), microscopic phenomena were observed and studied (van Leeuwenhoek and Hooke), and the integrating ideas of force and universal gravitation were elaborated mathematically (Newton). These and other scientific successes did not gain automatic acceptance—as with most discoveries that challenge the status of existing knowledge, they encountered opposition, and in general it was the younger scientists who championed the new developments. But these additions to scientific content could not have taken place had there not been radical changes in scientists' underlying conceptions of knowledge and experiment. And further, had there not occurred changes in social attitudes and institutional arrangements that worked to support these new conceptions and to leverage their fertility, the history of this new golden age would have been similar to that of earlier golden ages.

II. KNOWLEDGE AND EXPERIMENT

The Aristotelian approach to the study of nature, still prevalent among intellectuals in the 16th century despite various recognized inconsistencies and difficulties of interpretation casting doubt on Aristotle's physical explanations,[6] was based on an overall conception of the world which conformed to casual observation—a stationary Earth at the center of the universe, every move having a mover, change (including movement) as the result of seeking to obtain an end characteristic of the material, with heavy materials constrained to seek to move linearly toward the Earth's center, light ones linearly away from it, and heavenly ones in circles. Explanations were deductions from such unquestioned first principles, and although observations were by no means ignored and discoveries of new phenomena were made, the thought of overturning the overarching principles because of an awkward observation was inconceivable. And, in addition to the authority of Aristotle, there was the even greater authority of the Church, requiring explanations to conform to biblical dogma.

But such authorities did not go unquestioned, and by no means did everyone interested in understanding nature adhere to the Aristotelean methodology. Rather than being satisfied with the sufficiency of deductions from first principles, casual observation, efforts to come up with explanations of observations consistent with what the authorities took to be incontrovertible, and clearly unrealistic models designed to simulate the phenomena,[7] the new investigators sought understanding based on precise observation. Vesalius, with his detailed anatomical drawings published in the 1540s,[8] and Tycho Brahe, with his mapping of the stars and planets to the limit of accuracy possible with naked eye observation, first published in 1588, exemplify this trend. The idea that knowledge of nature was not derivable from abstract first principles but was to be had by systematic observation was taken up by Francis Bacon. In a comprehensive treatise published in 1620, Bacon argued that principles could be deduced inductively not only from observation but also from deliberate experiment carried out with the aid of instruments, intelligently designed and assessed. He gave elaborate instructions on cataloging and generalizing the raw results. Although his specific methodology (which he preached but did not practice himself) was unworkable, the underlying thrust of obtaining knowledge via exploratory research directed at confirming or refuting generalizations was significant—knowledge was contestable, nature itself was the judge, and experiments could be designed to force nature to render judgment.[9] The roughly contemporaneous discoveries of Harvey in anatomy, Gilbert in magnetism, and von Helmont in chemistry all followed such an approach.[10]

At the same time, another new scientific methodology, similar in that observation and experiment was determinative, but different in the central role played by mathematical reasoning, was being developed and demonstrated. Johannes Kepler, in striving to simplify Copernicus's hypothetical arrangement of a Sun-centered universe which maintained the ideal of circular planetary motion (but, like Ptolemy's Earth-centered scheme, required many epicycles to approximate observed planetary positions), recognized clues in his analysis of Brahe's observations that suggested that the orbits were in reality elliptical, deduced the exact mathematical form (specifically, of the orbit of Mars), and used the observations to confirm the much simpler arrangement. In addition, he was able to deduce and confirm (to the limits of precision the observations allowed) two further mathematical laws of planetary motion.[11] It was a demonstration that nature obeyed mathematical laws, laws which were confirmable by observation. Galileo Galilei, studying the motion of more local bodies and seeking to render it mathematically,[12] reasoned, in direct contradiction of Aristotle, that a body once put into motion would persist in that motion—with uniform velocity for horizontal motion, with uniform acceleration for vertical motion. Deviation from the mathematical ideal could be ascribed to such complications as air resistance. Galileo created an experimental setup—an inclined plane smoothed to minimize friction—to approach the mathematical ideal as much as possible in order to provide a test of his theory.[13] Here, as with Kepler, nature was conceived of as obeying, in ideal circumstances in which extraneous factors are abstracted away, simple mathematical laws which, with pertinent observations and carefully designed experiments, could be exposed to testing and shown to be true to nature. The stunning achievement of this mode of thinking was realized in the integration of Kepler's and Galileo's work by Isaac Newton, creating a mathematical edifice equally applicable to earthly and heavenly phenomena, and abundantly confirmed by probing experiment and precise observation.[14] The idea that aspects of nature could be precisely and reliably described by simple and elegant mathematical equations has been a cornerstone of science ever since.

The mathematically oriented methodology could be characterized as a revolutionary new way of looking at old phenomena, whereas the Baconian method was effective in producing unexpected discoveries in new scientific fields such as electricity and magnetism, and its application transformed chemistry from a craft into an experimental science.[15] But both have as their essential base the stipulations that knowledge is contestable and, whereas nature is the final arbiter of truth, it can and should be probed by suitably designed experiment to uncover phenomena which would not be observed under ordinary circumstances. Though different in origin, they have become merged, as can be seen in Newton's *Opticks*, in which the mathematical edifice of geometrical optics sits side by side with experimentally driven investigations into phenomena such as light polarization.[16]

A third conceptual innovation, developed concurrently with the other two, was the idea that the natural world could be understood in terms of the motions of various particles, varying in size and shape and invisible to the naked eye. The advance from Greek atomism centered on the attention paid to the motion of these particles, and specific mechanisms involving particle motion were proposed—for example that sound was caused by agitated particles of air. Descartes built this corpuscular idea into an all-encompassing theory of the world in which whirlpools of particles moved according to fixed laws.[17] On the one hand, this mode of thinking gave free rein for hypothesizing particle types and their motions to explain any phenomenon of interest; on the other, this arbitrariness was not a scientifically attractive feature. The problem was addressed in two different ways—by Huygens (and, later, Newton) insisting on mathematical descriptions of these motions, and by Boyle and Hooke (and, later, Newton) engaging in exploratory experiment. The corpuscular approach, inadequate on its own, was thereby integrated into the other two conceptual developments as a unifying agent.[18]

III. IDEOLOGY AND INSTITUTIONS

Traditionally, the social status of the scientist—understood as an independent thinker pursuing knowledge about nature as a vocation in its own right—was not particularly high. Even in those "golden ages" when the study of nature was actively pursued, science was generally subservient to moral philosophy, and the only widely valued scientific pursuits were those that could see direct application in satisfying immediate societal needs such as healing, building, astronomical prediction, and the provision of an overall world view. For example, in Greece, where there were some outstanding independent science specialists such as Aristarchus, Archimedes, and Apollonius, they were not at the center of societal interest or concern. However, many of these scientists were able to pursue their vocation due to the (sometimes unreliable) patronage of the Ptolemies in Alexandria, where they were isolated from the wider society and did not need their sanction or support.[19] The situation was not substantially different in medieval Europe, where scientific topics were subsumed under the umbrella of philosophy in the universities, and even into the early 17th century in Italy scientists such as Galileo relied on the patronage of powerful families and ran the risk of moral condemnation and religious persecution.

But considerable social changes were underway in Europe during the 16th century, and these were particularly evident in what became the Protestant regions of Northern Europe. This was a period of economic expansion and the opening-up of opportunities for entrepreneurship. The discovery of the New World, the expansion of trade routes over both land and sea, technological advances in machinery, printing, and navigation, and the fragmentation of traditional sources of political and religious authority produced a segment of non-aristocratic society in which the idea of progress and aspirations for personal betterment (in this world, not only in the next) took hold.[20] The new discoveries in science by the likes of Kepler, Galileo, and Gilbert were received positively in this social environment because (even if one's understanding of them was rather superficial) they promised progress in the ability to comprehend and control nature. It was clearly not the case that the scientific discoveries of the early 17th century actually produced useful technology, but the possibility was there, and that idea, written up and talked up, served to enhance the social status of science and scientists.[21] And the great popularizer of the notion that science was an important and socially valuable enterprise was none other than Francis Bacon, who envisioned extensive benefits that were to come from science that would enrich society.[22]

The new social status of science not only made science a respected vocation (which made patronage easier to come by) but also a field of interest to intelligent laymen. It opened the way for scientists together with individuals with some ability (and some means) to participate in discussions about scientific issues and even to engage in scientific experimentation itself.[23] Regular meetings involving such like-minded individuals took place in homes, business premises, coffeehouses, and college and university lodgings. For example, in England, an important (for later developments) group formed around 1645, centered at Gresham College London. Its members included the mathematician John Wallis, the physician William Harvey, and the astronomer Samuel Foster. Some members of this group relocated to Oxford around 1649, forming a group there, the Oxford Philosophical Club, while the Gresham group continued in London, its meetings often attended by London-visiting Oxonians. Members of the Oxford group later included the chemist Robert Boyle and the astronomer and architect Christopher Wren, and interacting with the group on occasion were the physicist Robert Hooke and the German diplomat Henry Oldenburg, who maintained a large network of correspondence with scientists across Europe.[24]

Perhaps to some extent inspired by Bacon's elaboration of an ideal scientific society,[25] a number of these informal groups, but most importantly the Gresham and Oxford groups, meeting at Gresham College in 1660, made plans to establish a more formal scientific society.[26] The new society sought and obtained royal approval from the newly restored Charles II, and in 1663 took the name "The Royal Society of London for Improving Natural Knowledge". The members chose the motto *Nullis in verba*, understood as "take nobody's word" and intended to emphasize the insufficiency of loose speculation and the need for experimental demonstration. Such demonstrations were conducted at the early meetings under the auspices of Hooke, appointed as Curator of Experiments. Oldenburg, one of the secretaries of the Society, at his own initiative and cost, established a regular periodical, which he named *Philosophical Transactions of the Royal Society*. This publication, which he described rather humbly in the Dedication in the first issue as "these Rude Collections, which are onely the Gleanings of my private diversions in broken hours", disseminated letters received, experiments reported and performed, and other items he expected to be of scientific interest to his subscribers (sometimes copied from other publications), not all of which was discussed at Society meetings.[27] The first issue appeared in 1665 and, although the monthly schedule was not reliably maintained and eventually abandoned, the *Transactions* has been in continuous publication to the present day. Following Oldenburg's death in 1677, it was financed by various secretaries of the Society and its editorial policy gradually became more oriented to publishing finished research which the Society Council thought fit to publish. But it was not until 1752 that it became the Society's official publication.

Besides being simply a vehicle for the dissemination of scientific news and research findings, Oldenburg's *Transactions* served to address an important problem faced by scientists in the new era of scientific experiment and discovery. If new worlds could be discovered by sailors, then, by analogy, new phenomena and new understandings of phenomena could be discovered by scientists, and, with respect and reputation to be had, the question of priority in discovery loomed large.[28] But the severity of the priority disputes that occurred during the 17th century, and the vigor with which associates of the disputants pursued them, is an indication that more than self-interest alone was at play, and that a code of scientific conduct had emerged—that although clearly multiple people could discover more or less the same thing, the recognized discoverer was the one who made public a convincing account first.[29] Oldenburg saw his journal as a vehicle for the public registration of discoveries by author and date and expected that such a service, offering evidence for priority, would encourage submissions.[30]

The *Transactions* inaugurated some other innovations, although their full significance was probably not recognized at the time. One involved the assessment of the quality of material submitted. Oldenburg had available to him more material than could be included, and so inevitably acted as referee, deciding what to publish and what to ignore. In this task, he probably had assistance from colleagues, and indeed his understanding with the Society was that the Society Council would review articles for approval for publication. And besides this gatekeeping function, the *Transactions* offered opportunities for scientists to comment on earlier papers, whether to criticize, to reinterpret, or to build on, and thus provided a widely read public forum where scientists could engage with each other. Finally, the accumulating issues of the *Transactions* were an accessible archive of scientific discovery, contention, and resolution and, as such, were a resource for future scientists to learn their craft and to build on an understanding of what had gone before.

The Royal Society and the *Transactions* were not the only scientific society and journal to be established in the 17th century—the *Journal des Sçavans*, whose content consisted largely of book reviews augmented by news articles that might be of interest to intellectuals in general (but did include some scientific papers), was published in Paris in the same year as the *Transactions* (but two months earlier)[31], and the Académie Royale des Sciences was established by the French government in 1666 as an advisory body for scientific matters. The Accademia de' Lincei operated between 1600 and 1630 in Rome, and the Accademia del Cimento, under the patronage of the Medici, supported scientific experimentation between 1657 and 1667. In German territory, the journal *Acta Eruditorum* began publishing in Leipzig in 1682 and was the outlet for the partisans of Leibniz in the priority dispute with Newton over the calculus, and the Akademie der Wissenschaften was formed in Berlin in 1700. By the end of the century there were at least eight publications containing reports of society meetings and discussions, and many of these societies, (including the Académie Royale) initially followed the policy of the Accademia del Cimento that discoveries made under the auspices of the society were represented as being made by the society, not the individual discoverer. This mode of reporting was eventually abandoned, however, and the policy of the Royal Society and the *Transactions* of registering individual credit became the norm.[32]

The upshot of these social and institutional innovations was that the 17th century saw the vocation of science achieve social acceptability and respect, and norms for interaction between scientists become institutionalized, both of which served to support and enhance the emerging idea that knowledge was contestable and that the only authority for assessing knowledge claims, even mathematically elegant ones, was verifiable observation and experiment.[33] It is at this point that the growth in scientific activity took off, slowly at first, but noticeably accelerating in the 18th century, at which point Bacon's promise of technological applications of scientific discoveries began to be realized, and the now familiar two-way interchange between science and technology established itself.

IV. A COALESCENCE OF INNOVATIONS

The Scientific Revolution is now well over 300 years old, and the growth both in scientific results—the ability to understand and manipulate nature—and in numbers of active participants in scientific endeavor shows no indication of tapering off.[34] The prestige of science in society is undiminished, and even those of the public who disagree with particular conclusions of some scientists cite other scientists for rebuttal.[35] Science has become an integral part of modern culture. It is this record of continuous growth and success and aura of indispensability to society that sets this scientific revolution quite apart from those that preceded it.[36] The question is: what was it about the developments in science and in the surrounding society in the 17th century that could account for such a remarkable phenomenon?

The methodological, epistemological, ideological, and institutional innovations that emerged during that century are well known, and all have been cited as causes of the Revolution.[37] For both Immanuel Kant and William Whewell, the key figure was Bacon, whose elaboration of the experimental-inductive method marked the clear break from Aristotelean science. Ernst Mach points to Galileo's revolutionary method of forming an abstract mathematical model and submitting its consequences to experimental test. For Eduard Dijksterhuis, the Revolution was set in motion by Copernicus, whose inspired placing of the Sun at the center of the universe, despite the work being very much in the tradition of Greek mathematical astronomy, led to the decisive innovations of Kepler and Galileo in mathematizing natural phenomena. Alexandre Koyré saw the essence of the Revolution as the emergence in the work of Galileo and Descartes of a new conception of motion which sheared it of the purposefulness with which Aristotle had endowed it and replaced that with the idea of different states of bodies in a mathematically described space, which fitted together with a new emphasis on precise measurement. Edwin Burtt also pointed to the emergence of a new mathematically oriented world view which, in the work of Copernicus and Kepler, and finally Galileo, resulted in the replacement of Aristotle's formal cause with mathematical description. Rupert Hall downplayed the empirical innovations and emphasized the new ideas involving the embrace of mathematics in describing nature in terms of lawlike regularities, a method which had possibilities for application to sciences other than astronomy and physics. Thomas Kuhn divided the sciences into two groups: the "classical sciences" (such as astronomy and physics) had their mathematically oriented upheaval in the conception of how the world is to be described, while the "Baconian sciences" (such as magnetism and chemistry) had their empirically oriented upheaval in methods of investigation. And Richard Westfall saw as crucial the integration over time, culminating with Newton, of the two separate ideals of exact mathematical description and a world view in which the underlying causes of phenomena could be deduced from experiment in terms of the motions of particles. All these authors cite, with varying degrees of emphasis, causes involving radically new conceptions of knowledge and experiment.[38]

Another group of authors cite causes external to the content of science, causes involving ideological and institutional changes, as necessary for the Scientific Revolution to take place. Included here are those who highlight the emancipation from religious dogma, such as Andrew White, whose polemical account of the "warfare of science with theology" had a run of popularity in the early 1900s. In contrast, Reijer Hooykaas saw an ideological shift conducive to experimental science in the aftermath of the Reformation, when the idea that scripture could be studied individually independent of authorities led to the attitude that God's other book, nature, could be approached similarly and that the answers found therein had to be humbly respected. Hooykaas also pointed to the downstream effects of the voyages of discovery in changing social attitudes in favor of an investigatory approach to nature. Robert Merton's study of the relation between science and the society in which it was embedded in the 17th century led him to point to aspects of the "Puritan ethos"—its utilitarianism, its empiricism, and its antitraditionalism—which altered social attitudes in the direction of making the new empirical approach of science commendable. Although Merton did not make the claim explicitly, his thesis has been taken as describing a driver of the Baconian side of the Scientific Revolution. George Clark proposed that the experimental method had its origin in procedures used in the emerging industrial processes and in the quantitative thinking necessary in business accounting, as well as in the new utilitarian religious attitudes. Elizabeth Eisenstein saw the availability of the printing press as enabling the wide communication and follow-up, a crucial feature of modern science not possible in a scribal culture. And Joseph Ben-David asked how it came to be, in the 17th century, that a socially acceptable and respectable vocation of science emerged and found his answer in science's compatibility with a developing ideology of progress in rational political and educational reform, an attitude shared by the sorts of men who were instrumental in forming the informal, and later formal, scientific societies.[39]

Floris Cohen has invoked most of the aforementioned causes in a comprehensive story of the rise of modern science in the 17th century.[40] He stressed the revolutionary changes that the three innovations—mathematically precise laws, exploratory experimental methods, and corpuscular conceptions of reality—introduced, changes in not only the content of science but also in the conception of what constituted knowledge and how it was to be sought. As something of an afterthought, he noted the various aspects of the current societal environment which increased the chances that these developments would be pursued. When explaining the Revolution's survival, his major points were, first, that the Revolution was not snuffed out by religious and political forces thanks to the openness to innovation provided by the Reformation and the political stability following the Peace of Westphalia and the English Restoration, and second, that the potential for application (initially more hope than reality) resulted in both political endorsement in the chartering of scientific societies and societal acceptance in the growing industrial economy. If one regards, as Cohen does, the Revolution as a circumscribed episode that culminated with Newton, then that is possibly all there needs to be said, but what is not explained is the remarkable aftermath in which the science born in the 17th century has grown unabated into the cultural mainstay it is today.

In summary, with regard to the emergence of modern science, what we are left with are elaborations of necessary conditions; there is no compelling argument as to which set of them might be sufficient. And there is no viable explanation of why this particular upsurge in scientific activity, unlike all those that preceded it, has not lapsed into scientific stasis—instead, it shows continuous, even exponential, growth. So, rather than championing any particular necessary cause or set of necessary causes as providing sufficiency, the sufficient cause could be looked for in interactions between these causes. If the confluence of innovations of the 17th century resulted in the formation of a mutually supporting set of scientific processes, and if this system of processes was organized such that its interactions with its environment were adaptive in facilitating its own maintenance and growth, then the Scientific Revolution could be seen as the spontaneous formation of a radically new order of scientific endeavor and its remarkable continuance explained. This amounts to characterizing the scientific community that emerged in the 17th century as an autopoietic social system—in fact, more specifically, an anticipatory social system.

V. ANTICIPATORY SOCIAL SYSTEMS

An autopoietic system, as originally defined by the biologists Humberto Maturana and Francisco Varela, is a circumscribed physical domain in which the processes that maintain its operation are able to recreate themselves, using material and energy from the environment, but not requiring the assistance of any outside process. In other words, an autopoietic system is an operationally closed but thermodynamically open self-organizing and self-maintaining system. Maturana and Varela were building on the work of earlier systems biologists, particularly Ludwig Bertalanffy, to characterize the properties of living cells. Bertalanffy's approach to understanding self-maintaining biological systems, which was followed up and extended by Robert Rosen, was to emphasize the organization of the internal processes rather than the organization of the physical matter—in fact, he held that the former determined the latter.[41] This is because in autopoietic systems it is the functional processes that persist, whereas the physical components are continuously being reconstituted by the functional processes with the help of inputs from the environment.

Turning to the consideration of systems of social interaction, it is evident that, while there are suggestive commonalities at a very general level, the analogy with biological autopoiesis is a limited one. Thermodynamic openness is an obvious commonality. And the property of persistent functional processes operating on changing internal components can be seen in certain circumscribed social arrangements—specifically, in communities, where the norms of interaction that are understood and generally adhered to by the members of the community are symptomatic of the repeated exercise of specific types of functional processes.[42] But, whereas in biological systems the functional processes are implemented by the chemical action of some of the material components (enzymes, for example), in social systems the processes are implemented by the purposeful actions of the members of the system and the internal components produced and acted on can be intangible (expressed ideas, for example) as well as material.[43] And, to qualify for the designation "autopoietic", the functional processes in the social system would have to be a closed set, in which the inputs of any process were provided as outputs of at least one of the other processes.[44]

Simple biological autopoietic systems can maintain their integrity by reacting to changes in internal state by negative feedback to maintain homeostasis. But openness to the environment allows for greater flexibility in the face of environmental changes via anticipation, where there is some ability to attempt to predict possible future states of the environment and their impact on the system itself. For example, some plants anticipate future cold weather and set in motion adaptive responses to it by sensing changes in day length—in effect, they have a simple internal model relating present day length to future temperature. In more complex systems with a facility for memory, the anticipatory responses can be modified by experience.[45] Perhaps the most complex anticipatory system known is the human brain, a system in which an internal model of the environment is maintained, is employed in the prediction of possible future situations and the preselection of suitable actions, and is modified by experiences of success and failure.[46] In general, a system is anticipatory if it implements within itself a model of itself and of its environment—a model which constitutes the system's knowledge—which allows it to change state on account of the model's predictions as to a future situation. The change of state may result in actions on the environment, or simply in dispositions to act. And the system's input from the environment may be processed within the system to confront, and perhaps modify, the model—for the model to be useful for anticipation, the system must be capable of learning, *i.e.*, adjusting its model to reflect experience of reactions from the environment.[47]

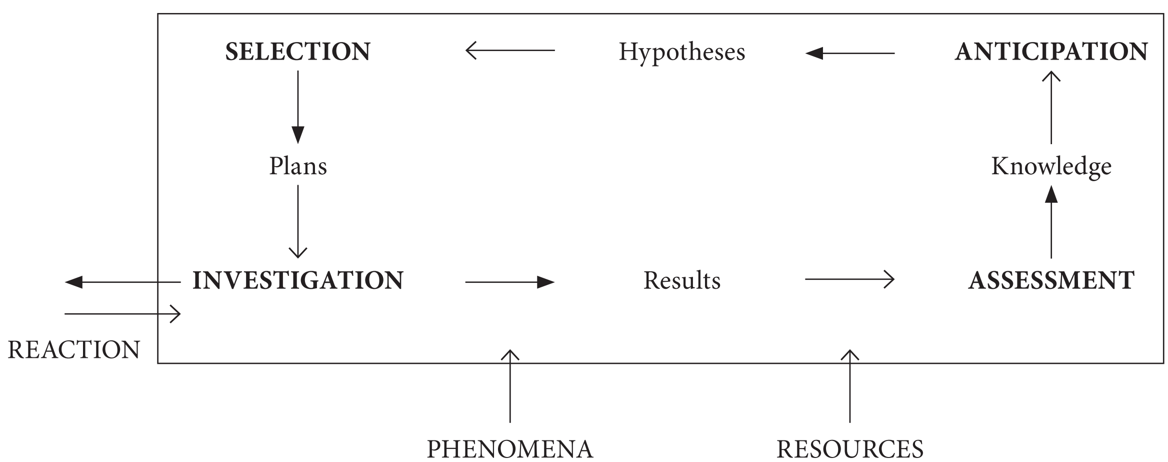
Arguing by analogy from these attributes of a biological anticipatory system, a social system would be autopoietic and anticipatory if it were materially open to its environment and contained the following sets of processes which form a functionally closed cycle:

- Investigative processes, which act on the environment and register and record the environment's reactions.

- Assessment processes, which assess these results for coherence and reliability and update the system's knowledge base.
- Anticipatory processes, which hypothesize future states and appropriate system actions with the aid of the system's knowledge base.
- Selection processes, which perform a culling operation on these hypotheses, selecting those considered actionable as plans forwarded to the investigative processes.

These processes are activated by the actions of the system's members who, like all individuals, "pursue happiness" according to their own subjective values, but are constrained (in large part, but not completely) by the norms that operate within the system and the available modes of interaction. The members are maintained by resources coming into the system from the environment. The system being open, phenomena in the environment can, in principle, be inputs to any of the system's processes. Outputs generated within the system are also, in principle, available as inputs to other processes and, depending on their implementation, parts or all of them may be visible to the environment. The processes of anticipation and selection, which bridge the system's knowledge and its plans for investigation, could, since their outputs are generated by assessments of an uncertain future, be termed "entrepreneurial".

The following is a schematic representation of the fundamental process organization of such an anticipatory social system, showing the self-sustaining cycle of social processes just described—investigation, assessment, anticipation, selection, investigation—together with their critical inputs and outputs. Processes are shown in bold within the system boundary; process outputs are indicated by closed-headed arrows and inputs by open-headed ones:



It is to be emphasized that this is a model of the organization of the processes within the system, not of the system's members. There is no necessary one-to-one association of members and processes—individuals may, and often do, participate in more than one of these processes. While the process organization is quite straightforward, the network of relationships and interactions between members can be a complex one of both competition and cooperation as members pursue their own interests under the constraints of the norms of behavior that emerge within the system and the modes of interaction provided by the system's processes.

It remains to be demonstrated that the confluence of scientific innovations in the 17th century provided the norms and functional processes necessary for the emergence of such an anticipatory social system and the built-in incentives for its growth.

VI. THE SYSTEM OF SCIENCE

Perhaps the most fundamental of the scientific innovations of the 17th century was the radical change in the conception of scientific knowledge. No longer was knowledge thought of in terms of deduction from an overarching authoritative scheme, and no longer could ingenious and elaborate constructions designed to "save the appearances" be acceptable as providing an understanding of phenomena. Instead, nature was to be actively investigated by probing experiment. The plans for such experimental action could be based on suggested mathematical generalizations or on inductions from previous observation, but whatever the source it was the reaction from nature that was to determine whether the suggested understandings were plausible or defective. A strong indication that the idea that active experiment was the final arbiter of truth had taken hold in the minds of the scientists of the time is evident in the Royal Society's motto, *Nullis in verba,* which was dismissive of speculation not backed up by experiment.

If knowledge were to be discovered and not simply deduced, then knowledge was mutable, and could be added to or corrected, depending on how the experimental results were understood and assessed. And here is where the genius of the Oldenburg innovations can be seen. There is an inevitable subjective component in the assessment of experiments—the results could be questioned, the apparatus judged unreliable, the experimenter considered incompetent, or complicating phenomena identified. Or the results could be ignored or dismissed out of hand because they did not conform to ingrained expectations. The resolution that Oldenburg provided for this pervasive problem was to enable the wide publication among scientists of experimental results and commentaries on them, so that assessment could take place in open discussion, and when (or if) a consensus emerged, it could be taken (at least provisionally) as an update to existing scientific knowledge. Without the availability of the printing press, this wide and timely distribution of purported facts and probing opinions would have been much more difficult, to the point of impracticability. This arrangement instantiated a decentralized process of systemic learning from experience in which there was no overriding personal authority, although reputational effects (which, in principle at least, reflected previous reliability) would mean that some assessments were more influential than others.

The access to widely distributed publication also played a large part in both fostering and constraining scientific entrepreneurship. Hypothesizing about scientific matters was certainly not a new activity in the 17th century, but its character changed with the new conception of knowledge and the new immediacy of exposure to criticism. Newton was famously dismissive of hypothesizing in the sense of loose speculation—"I feign no hypotheses" was the credo he added to the second edition of the *Principia*—and he insisted that scientific propositions be inferred from, and consonant with, the phenomena. Further constraining speculation was the acknowledged success of the use of mathematics for formulating coherent hypotheses, and this influenced the reception that published hypotheses met with in the scientific community. On the other hand, with the concepts of discovery and discoverer at play, there were rewards to be had for putting forward a viable hypothesis (and being the first to publish it), and this induced a competitive element into the process. For the first time in such an organized manner, scientific hypothesizing became the process of formulating anticipations of empirical phenomena based on an existing (and growing) base of knowledge,[48] an activity consistent with the age-old drive of curiosity but now hedged about with the possibility of both reputational costs and benefits.

With the new focus on experiment, both informal groups and, later, formally organized ones such as the Royal Society provided facilities where scientific experiments could be carried out. This, again, was the implementation of an entrepreneurial process, motivated of course by curiosity but also by the reputational rewards of discovery. Hypotheses (whether published or developed personally by the experimenter) can provide the rationale for experimental plans, but experimentation is neither a cost-free activity nor one certain of success, and so, given such constraints and likely faced with more hypotheses than can reasonably be addressed, a process involving selective judgement in the face of uncertain future outcomes is required. And again, the new institution of publication within the scientific community of results and hypotheses and the commentary on them is a vital innovation, for it provides considered reasons why particular hypotheses might or might not be confirmable and thereby reduces the inherent uncertainty of the selection process. The practice of vetting submissions for approval for publication (a process which has grown into the now-familiar refereeing procedure) was also a helpful element in the culling process.

The final components for completing this system of science, for adding a driving force to the closed cycle of social processes just described—investigation, assessment, anticipation, selection, investigation—and for imbuing the system with the propensity to grow, are to be found, respectively, in Oldenburg's innovation of ensuring credit to authors of publications, and in the societal attitudes promoted by Bacon through which scientific work came to be seen as a respected and valuable activity. There may be several motivations for a person to adopt science as a vocation, and possibly the major one—certainly the one most often cited by scientists themselves—is curiosity and the promise of pleasure in its satisfaction.[49] But reputation amongst one's peers is also a natural motivation, whether openly acknowledged or not, and is the criterion of success in science (even if sometimes, unfortunately, it is only achieved posthumously), and the explicit recognition of personal contribution (an innovation which has grown into the norm of citation) is crucial for grounding that motivation in a tangible form.[50] The new system of science thereby presented strong incentives for intelligent people to enter a profession which was socially valued and supported, and which offered realistic opportunities for pursuing satisfying endeavor and obtaining recognition of success. And shortage of work for a growing number of participants was not likely to be a problem—in stark contrast to a system of knowledge based on deductions from an authoritative source, a system in which knowledge is based on experiment is open-ended, as technological innovations stemming in part from that experimental activity allow for more and more extensive and finely detailed investigation of what is an effectively (and may be actually) infinite nature.

VII. SUMMING UP

The 17th century in Western Europe saw a significant phase change in social space, out of which emerged a new social order—the system of modern science—that was stable, closed and self-maintaining in its process structure (but open to the environment for material resources obtainable from a growing and liberalizing economy), and possessing an inbuilt propensity to grow.[51] The methodological, epistemological, ideological, and institutional innovations that were developed and adopted during that century combined to enable the formation of such an order. None of them was sufficient in itself, but together they provided the building blocks for a social arena in which people with an exploratory attitude, executing the procedures that formed the new arrangement, could pursue their interests with reasonable hope of some positive return. The success of this social arrangement has been such that, today, any discipline with pretensions to be scientific organizes its community of practitioners along the lines pioneered in the 17th century and first fully realized in the sciences of physics and astronomy.[52]

The characteristics of this new system of science suggest that it can be modeled as an anticipatory social system, a closed system of processes whose outputs include a structure which can serve as the system's internal model of itself and its environment and which can be updated based on internally assessed experience of the environment. The body of scientific knowledge—a mutable complex of current consensus assessments of theories and methods is that structure which serves as that internal model.[53] The hypotheses and judgments of scientific entrepreneurs (which are the anticipatory element of the system affecting its research propensities), the environmental feedback from investigations, and the assessments of hypotheses in the light of that feedback are all transmitted within the system by the agency of repeated publication and citation transactions through which scientists engage with each other.

It is only by conceiving of science as a system—in particular, as an anticipatory system—that the reason why the scientific and societal innovations of the 17th century were able to combine with such spectacular effect can be understood. Individual contributions, brilliant as they were, were not sufficient to produce such a lasting effect, just as they had not been in the past. Thinking in terms of systemic properties, such as knowledge, anticipation, and assessment, and differentiating them from their counterparts as properties of individuals—certainly without denying the importance of the lower-level individual activities that produce these systemic properties—can have a very productive place in social theory. And the use of such "systems thinking" in understanding the phenomenon of the emergence and persistence of modern science is just such a case.[54]

NOTES

1 According to Cohen (1994, ch. 2), although the revolutionary character of the period was recognized as early as 1756 in d'Alembert's *Encyclopédie*, it was first Koyré (1939) and then Butterfield (1949) who gave the singular term currency with historians of science, even though the boundary dates, particularly the starting date, vary considerably from author to author. Wootton (2015), for example, picks Tycho Brahe's observation of a nova in 1572 as his starting date. But the idea has not been without controversy. Kuhn (1962) finds at least four separate revolutionary "paradigm shifts" in the 16th and 17th centuries, but in a later work Kuhn (1977, ch. 3) returns to the concept of a single scientific revolution as a paradigm shift but encompassing only the classical physical sciences. Shapin (1996, p. 1) cast scorn on the notion that any single event could initiate a decisive change in how natural knowledge was perceived or obtained. In the same skeptical vein, it has been questioned, for example by the historian Jan Golinski in 1988, as quoted by Cohen (1994, p. 499), "whether the notion of a coherent, European-wide, Scientific Revolution can survive continued historiographical scrutiny" as more detailed historical research has uncovered difficulties in explanations based on it. However, Cohen (2015), dealing explicitly with such difficulties, has developed a detailed and convincing account of the Scientific Revolution as a unique confluence of a specific series of developments during the 17th century.

2 An obvious omission from this list of "golden ages" is the European scientific Renaissance of the 16th century, triggered by the fall of Constantinople which enabled the release and subsequent translation of original Greek documents including, significantly, some works of Archimedes. Butterfield (1949) includes this period as part of the Scientific Revolution, dating its beginning at 1500. Maier (1949) considers the Renaissance (coupled with the earlier medieval European developments) as (p. 5) "the first stage of a grand development process of which the second and decisive stage falls in the 17th century". See also Maier (1982). For the purposes of this paper, it matters not whether the scientific developments of the 17th century constituted a separate revolution, marking a clear break from Renaissance thinking, as Cohen (2015) argues, or the second and final stage of a longer series of changes—what is important is that the 17th century developments were decisive in establishing a revolutionary new regime of scientific activity.

3 Cohen (2015, p. 26) dates the end of Greek science's golden age at the death of Hipparcus in about 150 BCE. He does not claim that there were no original Greek thinkers subsequent to this—Ptolemy and Galen in the 2nd century certainly stand out—but these were isolated cases. There were similar cases after the end of the other golden ages, but none of these represented a sustained revival in intellectual progress.

4 Cohen (2015, p. 263) is clear about the uniqueness of this revolution: "The enduring survival of the new nature-knowledge broke with all historical precedent. What we have become used to in our day, the unbroken growth of the scientific enterprise, is the big exception in world history and as such demands an explanation."

5 See, for example, Windelspecht (2001). Even contemporaries who were not scientists could see that a major revolution in scientific achievement was in progress—an example, quoted by Cohen (1994, p. 1), is the poet Dryden (1668), who (not without some exaggeration) asked "Is it not evident, in these last hundred years … that almost a new Nature has been reveal'd to us? that more errours of the School have been detected, more useful experiments in Philosophy have been made, more Noble Secrets in Opticks, Medicine, Anatomy, Astronomy, discover'd, than in all those credulous and doting ages from Aristotle to us?".

6 For example, to explain the fact that a pendulum bob does not stop at its lowest point, the notion of "impetus" acquired during the downswing was proposed, providing a mover for the upswing. Grant (1971) notes that, while problems like this were addressed, it was done piecemeal as disparate (and not necessarily consistent) amend-

ments within the Aristotelean system, and the system itself, with its top-down explanatory orientation, remained intact.

7 The classic case of such simulation was Ptolemy's ingenious astronomical model with its proliferation of epicycles, which provided a workable vehicle for prediction of planetary orbits. It maintained the ideal of circular motions in the heavenly sphere, while "saving the appearances" of irregular planetary motion.

8 In detailed and realistic anatomical rendering, Vesalius had an unpublished predecessor in Leonardo da Vinci, who made a series of drawings based on dissections in the early 1500s.

9 In Bacon's (1620, p. 28) words: "the real order of experience begins by setting up a light, and then shows the road by it, commencing with a regulated and digested, not a misplaced and vague, course of experiment, and thence deducing axioms, and from those axioms new experiments". Also (p. 36): "The true labor of philosophy … neither relies entirely or principally on the powers of the mind, nor yet lays up in the memory the matter afforded by the experiments of natural history and mechanics in its raw state, but changes and works it in the understanding." To get a feeling for the unworkability of his specific methodological recommendations, see his illustration (pp. 56-77) of the study of the various phenomena that he takes to be associated with heat (or the lack of it).

10 See Cohen (2015, pp. 129-135), who summarizes (p. 135): "In a gradual process around 1600, practically oriented, accurate observation is condensed into fact-finding experiment, where artifices are increasingly employed to force the manifestation of natural phenomena which would not have appeared of their own accord."

11 Kepler's Laws are: 1. The planets' paths are elliptical, with the Sun at a focus; 2. Each planet-Sun line moves over equal areas in equal times; 3. The ratio of the squares of the periods of any two planets is equal to the ratio of the cubes of their average distances from the Sun. The first two laws were published in Kepler (1609) and the third in Kepler (1619).

12 Galileo's desire to provide a mathematical description of motion arose from his study of the works of Archimedes. Although he failed in an attempt to apply Archimedean statics to the problem, he was able to surmount the difficulties with a more dynamic approach. See Rose (1975) and Cohen (1994, p. 284).

13 Galilei (1638) is a final summary by Galileo of his research. For extensive treatments of Galileo's work, see Koyré (1939) and (for an answer to Koyré's skepticism regarding the precision of Galileo's measurements) Drake (1978).

14 The *Principia Mathematica,* Newton (1687), contained the exposition of his "system of the world". His later work, Newton (1704), systematically followed the methodology of deducing mathematical generalizations from observation which were then subjected to experimental tests carefully designed both to confirm the generalizations and to exclude possible competing explanations. As he noted in his introduction (p. 7): "My Design in this Book is not to explain the Properties of Light by Hypotheses [i.e., loose speculation], but to propose and prove them by Reason and Experiments". For a comprehensive personal and scientific biography of Newton, see Westfall (1980).

15 See Kuhn (1977, pp. 41-46), who arranged the scientific disciplines of the 17th century into two clusters: the "classical physical sciences" such as astronomy and geometrical optics (to which he added the new study of motion), and the newly emerging "Baconian sciences".

16 Mokyr (2017, p. 105) puts this point forcefully: "Newton singlehandedly combined the deductive powers of mathematical modeling with Baconian stress on experimental data and observations, showing that the two were not only capable of coexisting in the same mind but could actually be complementary. The combination of his formidable mathematical and analytical skills with his continuous reliance on empirical and experimental data was regarded in his own day as a shining example that lesser scientists could only hope to mimic."

17 Ironically, Descartes' mode of theorizing—an overall conception based on unassailable principles from which local explanations can be deduced—is very reminiscent of that of Aristotle.

18 Cohen (2015, pp. 218-240) provides a compelling story of the activities of Huygens, Boyle, Hooke, and Newton in achieving an integration of the three different epistemological and methodological approaches to knowledge.

19 Ben-David (1971, pp. 40-41), a sociologist, placed much importance on the lack of societal legitimization as a reason for Greek scientific decline: "The newly differentiated role [of the independent scientist] was never given a dignity comparable to that of the moral philosopher. Independence from philosophy was a decline and not a rise in the status of the scientists. … [The] few astronomers, mathematicians, natural historians, and geographers who worked mainly in Alexandria were completely isolated from any general intellectual or educational move-

ment. [And so] specialized science lost its moral importance. … As a result, the role did not develop any further and, starting from the second century BC, scientific activity declined."

20 See Ben-David (1971, pp. 65-69) and Cohen (1994, pp. 367-374). For a quick summary of the economic, religious, and political changes that took place during the 16th century in Europe, see the article in Encyclopedia Britannica at https://www.britannica.com/topic/history-of-Europe/The-emergence-of-modern-Europe-1500-1648.

21 Ben-David (1971, pp. 65-66) emphasized the importance of these social developments, asserting that "fortunately for the development of science … there existed in Northern Europe a mobile class whose aspirations, beliefs, and interests—intellectually as well as economically and socially—were well served by the utopian claims made on behalf of science. … Thus, when the ebbing tide of science, which was receding from the scientific circles and academies of Italy, finally touched France and England, its direction was reversed." Merton (1938, pp. 453-454) emphasized the role of the English Protestant Puritans, asserting that "through the psychological sanction of certain modes of conduct [the Puritan ethos] made an empirically founded science commendable rather than, as in the medieval period, reprehensible or at best acceptable on sufferance. In short, Puritanism altered social orientations. It led to the setting up of a new vocational hierarchy, based on criteria which inevitably bestowed prestige upon the natural philosopher. … And one of the consequences of Puritanism was the reshaping of the social structure in such a fashion as to bring esteem to science." See also Ben-David (1985).

22 For a detailed discussion of Bacon as a "cultural entrepreneur", see Mokyr (2017, pp. 70-98). Bacon (1592, p. 216) was an impassioned and effective promoter of the promise of science: "Is not knowledge a true and only natural pleasure, whereof there is no satiety? Is it not knowledge that doth alone clear the mind of all perturbation? … But is this a vein only of delight, and not of discovery? of contentment, and not of benefit? Shall he not as well discern the riches of nature's warehouse, as the benefit of her shop? Is truth ever barren? Shall he not be able thereby to produce worthy effects, and to endow the life of man with infinite commodities?"

23 According to Ornstein (1913, p. 67): "Enthusiasm for experimentation and the widespread interest it aroused apparently led those devoted to science to enter into more or less formal affiliations. The rich and noble amateur devoted some of his wealth to gathering about him men who would jointly experiment and benefit by this collaboration. The professional scientist would become the center of people who joined him for instruction and whom he needed for assistance."

24 A fuller list of some of the major participants in these groups, and a good picture of the range of topics discussed, is contained in John Wallis's autobiography, in Scriba (1970, pp. 39-40).

25 Bacon (1626) set out his vision of a societal institution expressly dedicated to scientific documentation and research which he called "Saloman's House".

26 According to the Royal Society's website at https://royalsociety.org/about-us/history: "The very first 'learned society' meeting on 28 November 1660 followed a lecture at Gresham College by Christopher Wren. Joined by other leading polymaths including Robert Boyle and John Wilkins, the group soon received royal approval". Wren's lecture and the first meeting were in fact three years apart, as the following extract from the history of Gresham College, Chartres & Vermont (1998, pp. 30-32), makes clear: "In 1657, … Christopher Wren … gave his inaugural lecture as Professor of Astronomy. … Wren's inaugural lecture was also a kind of manifesto of the new science … Three years after this seminal lecture, the monarchy was restored and the scientific network which centred on Gresham College played a crucial part in the meetings which led to the formation of the Royal Society. The entry in the first journal book of the Society dated 28 November 1660 reads thus: 'These persons according to the usual custom of most of them met together at Gresham College to hear Mr. Wren's lecture. Robert Boyle, William Petty and others were there. After the lecture was ended, they did according to the usual manner withdraw for mutual converse. Where among other things that were discoursed of, something was offered about the design of founding a college for the promoting of physico-mathematico experimental learning.' There follows a discussion on the constitution and a great deal of time was spent on this subject."

27 Moxham (2016, p. 466) notes that the *Philosophical Transactions* "though widely acknowledged as the first scientific periodical, was intended by its founder as a commercial enterprise predicated on his privileged access to the latest natural-philosophical goings-on rather than an editorially neutral vehicle for presenting research in finished form". Nonetheless, Oldenburg did not himself make appraisals of any knowledge claims other than to accept

them for publication, and so (p. 478), by "bringing disparate communications on related subjects into one place Oldenburg helped to create, for highly interested commercial reasons, an ideal of disinterested communication".

28 For a survey of many priority disputes occurring during the 16th and 17th centuries, see Wootton (2015, pp. 112-119).

29 According to Merton (1957, p. 639): "To say that these frequent conflicts over priority are rooted in the egotism of human nature, then, explains next to nothing; to say that they are rooted in the contentious personalities of those recruited by science may explain part, but not enough; to say, however, that these conflicts are largely a consequence of the institutional norms of science comes closer, I think, to the truth."

30 In letters to Boyle in late 1664 discussing plans for the new journal, Oldenburg observed that "We must be very careful as well of regist'ring the person and time of any new matter, as the matter itselfe, whereby the honor of the invention will be reliably preserved to all posterity" and, further, that "all ingenious men will thereby be incouraged to impact their knowledge and discoverys". These letters are in the RS Archives—see https://en.wikipedia.org/wiki/Philosophical_Transactions_of_the_Royal_Society.

31 To get a good picture of the differences in content between these two journals, see Banks (2009).

32 For a detailed discussion of scientific societies, journals, and other publications in the 17th century, see Kronick (1962), and in particular (pp. 113-117) for a citing of the policies of non-attribution in society proceedings. The notion that discoveries should not be individually credited was probably a reflection of the influence of Bacon's urging that science should be an endeavor pursued for the common good of society. Prior (1954, p. 362), discussing Bacon's ideal conception of a man of science, notes that, for Bacon, "If the fatal sin against the canons of true science is pride, the all-embracing virtue is charity." But this ideal did not work well in practice—according to Middleton (1971, pp. 327-328), Borelli, a prominent member of the Accademia del Cimento, grew disenchanted with the sharing of credit and left the society, soon after which it disbanded. Ornstein (1913, p. 91) also refers to the "intense jealousies" which festered as a result of the credit-sharing policy.

33 It should be understood that the absolute authority of empirical data is an ideal. Scientists, for good reasons, will summarily reject apparently confirming observations and will decline to repeat them if the theory in question seems in their judgment to be highly implausible. Michael Polanyi (1967) explains this aspect of scientific behavior very clearly, with examples, and sums up as follows (p. 536): "A vital judgement practised in science is the assessment of *plausibility*. Only plausible ideas are taken up, discussed and tested by scientists. Such a decision may later be proved right, but at the time that it is made, the assessment of plausibility is based on a broad exercise of intuition guided by many subtle indications, and *thus it is altogether undemonstrable. It is tacit.*"

34 One measure of the growth of science is provided by Bornmann & Mutz (2014, p. 2215), who report: "We have looked at the rate at which science has grown since the mid-1600s. In our analysis of cited references we identified three growth phases in the development of science, which each led to growth rates tripling in comparison with the previous phase: from less than 1% up to the middle of the 18th century, to 2 to 3% up to the period between the two world wars and 8 to 9% to 2012. For a survey of various ways in which measurement of the growth of science has been attempted, see Gilbert (1978).

35 The prestige of science in modern society is such that the *ad hominem* tagging of an intellectual opponent as a "science denier" packs significant emotional punch. Shapin (1995, p. 390) affirms this underlying trust in science itself: "The homage paid to science is best evident in the very existence of a public stock of formal natural knowledge. All those who believe that the earth goes around the sun, that DNA is the genetic substance, that there are such things as electrons, and that light travels at 186,000 miles per second are, by so believing, doing scientists honor. Nor is that honor restricted to blind acceptance. … Legitimate concerns over the 'use' and 'consequences' of scientific knowledge do not affect the honor paid to science: the very problems that science is said to generate flow from the recognition of its potency."

36 Cohen (1994) discusses in some detail the lack of longevity in the periods of scientific activity in Greece (pp. 241-260), in medieval Europe (pp. 260-267), in the Islamic world (pp. 384-417), and in China (pp. 439-482).

37 The following one-sentence descriptions of the views of various historians writing on the subject of the causes of the Scientific Revolution are obviously totally inadequate at capturing the detail, the nuance, and even the con-

tradictions of these views, but they are an attempt to capture what seems, admittedly subjectively, to be their most important point.

38 For a summary and comparative discussion of the contributions of Kant, Whewell, Mach, Dijksterhuis, Koyré, Burtt, Hall, Kuhn, and Westfall, with references to and quotations from their original works, see Cohen (1994, pp. 21-150). Also discussed are the contributions of Pierre Duhem, who located the birth of modern science in a 13th century proclamation by the Bishop of Paris allowing for some freedom in theorizing about the nature of the world, and who found in the impetus theory of Buridan and others in the 14th century a direct forerunner of the concept of inertia; Anneliese Maier, who contested Duhem's conflation of the ideas of impetus and inertia and regarded the Scientific Revolution as a gradual repudiation of Aristoteleanism occurring in two phases, a preliminary one in the 14th century and the decisive break coming in the 17th century; Herbert Butterfield, whose historical survey gave intellectual heft to the concept of "the Scientific Revolution" which he also, like Maier, regarded as a long process with a decisive phase initiated by Kepler and Galileo; and Auguste Comte, who proposed a stage theory of scientific development and identified the 17th century as the period in which the combination of Galileo and Bacon moved astronomy and physics into the "positive" stage in which quantitative mathematical laws were discovered.

39 For a summary and comparative discussion of the contributions of White, Hooykaas, Merton, Eisenstein, and Ben-David, with references to and quotations from their original works, see Cohen (1994, pp. 308-377).

40 See Cohen (2015, pp. 262-269).

41 See Maturana & Varela (1972), Bertalanffy (1928; 1968), and Rosen (1975; 1991). In Bertalanffy's (1968, p. 27) own words: "In the last resort, structure (i.e., order of parts) and function (order of processes) may be the very same thing: in the physical world matter dissolves into a play of energies, and in the biological world structures are the expression of a flow of processes." Rosen's (1991, pp. 119-120) short statement of his approach to understanding biological systems was "throw away the matter and keep the underlying organization … The organization of a natural system … is at least as much a part of its material reality as the specific particles that constitute it at a given time, perhaps indeed more so." For a good historical overview of the development of the role of process organization in biology, with references to the historical literature, see Mossio *et al.* (2016).

42 For detailed discussions of social norms, their contexts, their emergence, and their theoretical treatments in different disciplines, see Hechter & Opp (2001). A distinct, but very much related line of thought goes under the rubric of "spontaneous order", in which social arrangements are characterized as emerging and sustaining themselves, in the words of the Scottish Enlightenment philosopher Adam Ferguson (1767, p. 187), by "the result of human action, but not the execution of any human design". Adam Smith (1776, pp. 24-32, 484-485) deployed the idea in the context of markets, most famously with his metaphor of the "invisible hand", and Carl Menger (1870, pp. 257-285; 1883, pp. 139-159, 223-234) revived it with a number of applications, including the origin of money. The great exponents of spontaneous order theory in the 20th century were Michael Polanyi (1962) with reference to science, and Friedrich Hayek (1960; 1973) with reference to markets, the law, and liberal society in general. For detailed treatments of the development of spontaneous order theory see Barry (1982) and Hamowy (1987), and for an analysis and criticism of both Polanyi's and Hayek's treatments see Butos & McQuade (2017).

43 The concept of autopoiesis has been transplanted to several social disciplines, including sociology. See Mingers (1995) for summaries and critiques of these applications. An influential sociological application is Luhmann's (1984) theory of society as a whole as autopoietic communication, but this is pitched at such a high level of abstraction that it is difficult to see its usefulness in specific cases, although it has found some application in the area of law. For valiant attempts to clarify Luhmann's work, see Seidl (2004) and Mingers (1995). The social application of the concept in this paper stays clear of the difficulties that bedevil other attempts by limiting the analogy and clearly defining both the domain and the set of functional processes of the specific system involved.

44 In Rosen's (1991) terminology, the thermodynamic openness and functional closure of systems are referred to as "openness to material causation" and "closure to efficient causation" respectively, where "material" and "efficient" are two of the four Aristotelian causes. See also McQuade (2019).

45 Rosen (1975, pp. 53-61) sets out a general classification of types of adaptive—and anticipatory—systems in order of growing complexity.

46 The economist Hayek, who had originally trained as a theoretical psychologist, was one of the first to describe the functional aspects of the brain in these terms, showing how a mutable model of the environment could be maintained and updated within a complex neuronal structure and used to create dispositions for action in particular circumstances based in part on past experience. See Hayek (1952).

47 Rosen (1974) defined "anticipatory modes of behaviour of organisms … [as those] in which an organism's present behaviour is determined by: (a) sensory information about the present state of the environment; and (b) an 'internal model' of the world, which makes predictions about future states on the basis of the present data and the organism's possible reactions to it." See Rosen (1985) for a detailed mathematical treatment of anticipatory systems.

48 Polanyi (1967, pp. 540-541): "This is what the existing body of scientific thought offers to the productive scientist: he sees in it an aspect of reality which as such is an inexhaustible source of new and promising problems."

49 A Pew Research Center survey, posted online at https://www.pewresearch.org/fact-tank/2016/10/24/as-the-need-for-highly-trained-scientists-grows-a-look-at-why-people-choose-these-careers/ dated October 24, 2016, shows clearly that curiosity as to how the world works and the expectation that it can be satisfied (often planted in childhood by parents, teachers, and books) is given by a large majority of the scientist respondents as a motivating factor for their entering a scientific career.

50 Evidence that the desire for reputation is a strong driver motivating scientific activity is seen in the competitive behavior of scientists. In his study of scientific competition which focuses on the danger of being anticipated in a discovery, Hagstrom (1974, p. 3) notes that "competition is severe when being anticipated means losing all or nearly all recognition for one's work" and that (p. 8) "these results demonstrate the sensitivity of scientists to proper recognition of their work".

51 Stability and growth are not guaranteed, however, for science is an open system and not only is it dependent on the surrounding society for resources but also it requires that the surrounding society respect the integrity of its processes, for these are the geese that have laid the golden eggs of scientific knowledge. See Butos & McQuade (2006; 2012).

52 In fact, all modern academic communities are organized with a similar process structure. Where they differ is in the investigative process. For the sciences, this process operates as a probe of the environment from which the environmental responses are taken as strong evidence in assessments of hypotheses about the environment. It is the system's anchor to reality, and for the physical sciences in particular it holds tight. For sciences whose objects of study are complex systems—climate science and (even more so) economics, for example—the bottom is much sandier, and the interpretation of experiments and observations subject to correspondingly greater latitude. For the humanities, the grip may be even more tenuous, and the possibilities for restraint on speculation much more attenuated. Mathematics is a hybrid case in which investigations are often conditioned by findings in the well-anchored sciences, but in which internal conceptions of what is interesting are also operative. The differences in the perceived "progressive" nature of the various academic disciplines seem to be strongly correlated with the extent of the attachment of the system's anchor—its mode of probing its environment—to real phenomena and to the definitiveness of the results obtained. In the case of the social sciences, this observation would support Hayek's (1967, pp. 22-42) analysis of the difference between natural and social sciences in terms of "the degree of falsifiability" of proposed facts as opposed to a fundamental difference in the structure and procedures of the two groups of sciences. But an alternative viewpoint, one stressed by Mises (1949, pp. 39-41) and bolstered by Hayek (1948, pp. 57-76), is that the problem with the social sciences lies in the widespread application of the method of the natural sciences to the social sciences, due to a lack of recognition of fundamental differences between the nature of the relevant facts in the two domains.

53 There is no implication here that the body of scientific knowledge will always grow in a uniform way as more and more of the environment is probed. The very nature of its construction—based on consensus assessments of sensed phenomena and their posited explanations in the light of previous consensus positions—is that of a layered structure in which additions and changes tend to be concentrated in the outer layers. Questioning deeper consensus positions risks reputational costs, but if successful not only are reputational gains realized but outer layers of knowledge which were built on the now-discarded position must be reassessed and a new overall consensus formed. This is consistent with the alternation of periods of "crisis" with long periods of growth and con-

solidation described by Kuhn (1962) and others. For a discussion of this and other issues in the philosophy of science—including constrained relativism, antifoundationalism, underdetermination, theory-ladenness, and incommensurability—from the point of view of science as an adaptive system, see McQuade (2010).

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