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Theatricality and Performance in Science

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Traditionally, science has been compared with the notion of tragic fate in Greek dramatic literature, more recently with Brecht's notion of *epic theatre.* These two ways of describing science stand for entirely different conceptions of what science is and how it develops. This paper explores first the tragic (or *dramatic*, as I shall call it) conception of science, especially with respect to its epistemological implications. I argue that a dramatic view of science, as advocated by Alfred Whitehead, for instance, fails to do justice to the complexity of science as a collective cultural and epistemic enterprise that is constantly faced with questions of an unknown future. Paradoxically, however, it is nevertheless this dramatic view of science that we find institutionalised in our society. In search of a more appropriate way of describing the temporal and epistemic intricacies of scientific growth, I briefly introduce Yehuda Elkana's notion of science as *epic theatre* but then propose to describe the dynamics of knowledge-generation in terms of a haphazardly plotted spectacle rather than a play that follows a predetermined plot. The plot of the spectacle of the growth of knowledge is uncertain and contingent; scientists who are breaking new ground with their research are very much aware that the outcome of what they are doing is largely unpredictable. However, for their public performances (e.g. when applying for public funding) scientists have learned to cover up uncertainty and contingency, and to play the game of determinism.

I

One of the most fundamental premises for the development of modern science was, according to Alfred N. Whitehead (1861-1947), the assumption of the predetermined order of nature, the assumption that nature is governed by immutable laws which are (in principle at least) intelligible to human beings. Without this trust in the order and scrutability of the physical world, it is difficult to see how people could have found it worth their while to engage in natural philosophy. Historically, then, the idea of order cannot be a product of empirical science but must have existed before. In *Science and the Modern World,* Whitehead identifies two sources from which western science inherited this idea of a predetermined natural order. One source of this deterministic bias he sees, paradoxically perhaps, in the notion of tragic fate in Greek dramatic literature.[1] In the great tragedians of ancient Athens, Aeschylus, Sophocles, and Euripides, he sees the precursors of modern scientific spirit:

> Their vision of fate, remorseless and indifferent, urging a tragic incident to its inevitable issue, is the vision possessed by science. Fate in Greek Tragedy becomes the order of nature in modern thought. The absorbing interest in the particular heroic incidents, as an example and a verification of the working of fate, reappears in our epoch as concentration of interest on the crucial experiments. (12)

Whitehead compares the development of science with the unfolding of the merciless chronology of events in a predetermined world. Human interference with such a rigid order is impossible in principle; if it is attempted, it ends tragically and leads necessarily to human suffering:

> Let me here remind you that the essence of dramatic tragedy is not unhappiness. It resides in the solemnity of the remorseless working of things. This inevitableness of destiny can only be illustrated in terms of human life by incidents which in fact involve unhappiness. For it is only by them that the futility of escape can be made evident in the drama. This remorseless inevitableness is what pervades scientific thought. The laws of physics are the decrees of fate. (13)

He then illustrates this "remorseless inevitableness" of scientific inquiry with an episode from a meeting of the Royal Society in London at which he was present. Einstein had predicted that rays of light would bend as they pass in the neighbourhood of a powerful centre of gravity such as the sun, a phenomenon which could not be accounted for within the framework of Newtonian physics. Years later Einstein's speculations were verified by astronomers of the Greenwich Observatory. In the following passage Whitehead describes the moment when the Royal Society officially acknowledged Einstein's findings:

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[1] This is somewhat paradoxical because, following Whitehead's ensuing argument, one should think that the notoriously wayward and capricious gods of Greek mythology would have made it impossible for the scientific spirit to emerge.

> The whole atmosphere of tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny. . . . There was dramatic quality in the very staging: (the traditional ceremonial, and in the background the picture of Newton to remind us that the greatest of scientific generalisations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting: a great adventure in thought had at length come safe to shore. (13)[2]

Whitehead's second source from which modern science is said to have inherited its deterministic bias is medieval theology with its insistence on an entirely rational God. A belief in an arbitrary, irrational, or otherwise unreliable God would not have raised the possibility for modern science to emerge. Whitehead argues that in such a world, "Any definitive occurrence might be due to the fiat of an irrational despot, or might issue from some impersonal, inscrutable origin of things" (16). In such a world natural philosophy, the attempt to discover universal laws of nature, would be pointless. The assumption of order and scrutability, then, was logically necessary for the development of science. But the logician Whitehead hastens to add that this assumption was irrational: "I am not arguing that European trust in the scrutability of nature was logically justified even by its own theology." Whitehead, writing in the 1920s, is embarrassed that science seems never to have shaken off this irrational bias; science "has remained predominantly an antirationalistic movement, based upon a naive [religious] faith" (20).[3] Whitehead's worry is of course not that the assumption of order is wrong, but that it is logically not compelling and therefore wholly unsuitable as a rational basis for a logically consistent scientific rationale. Hence his urgent appeal to

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[2] To be sure, Whitehead's understanding of Greek tragedy owes more to his understanding of science than vice versa. However, Greek tragedy is not my concern here, neither Whitehead's reading of Greek tragedy nor the differences between the various Greek tragedians. What is at stake is the dramatic conception of science he so eloquently endorses.

[3] Whitehead alludes here to the physico-theologians of the seventeenth and eighteenth century, who conceived of scientific investigating largely as an uncovering of the pre-established divine order. For a comprehensive survey of the staggering scope of seventeenth and eighteenth-century physico-theological writings in England, Scotland, and the Continent, see Philipp. Many eminent scientists were physico-theologians of sorts, throughout the eighteenth and way into the nineteenth century. So for example the Swedish taxonomist Linnaeus, who writes in 1754: "If the Maker has furnished this globe, like a Museum, with most admirable proofs of his wisdom and power; if this splendid theatre would be adorned in vain without a spectator; and if Man the most perfect of all his works is alone capable of considering the wonderful economy of the whole; *it follows that Man is made for the purpose of studying the Creator's works that he may observe in them the evident marks of wisdom* . . ." (in Brooke 58).

subject the epistemological foundations of science to thorough philosophical criticism lest science would "deteriorate into a medley of ad hoc hypotheses" (21).[4]

Whitehead conceives of science as a series of inevitable discoveries that were made with "implacable destiny" after the true method of scientific investigation had been invented in the seventeenth century. Once the "soil, the climate, the seeds were there," he writes, "the forest [of science] grew" steadily (20). Such a conception of science may be called dramatic because it stipulates that, once the remorseless knowledge-generating machinery of science has been set in motion, there is no way we can control or shape its output; if science develops with "remorseless inevitableness," the scientists are mere performers on the stage of determinism, simply following the lines inscribed in the immutable laws of nature. This view of science is by no means idiosyncratic of Whitehead. For the historian of science George Sarton, the "history of science is the only history which can illustrate the progress of mankind"; for "progress has no definite and unquestionable meaning in other fields than the field of science" (5). Sarton, too, implies that every particular discovery is bound to be made sooner or later and that it would always be exactly the same discovery. Had, for example, gravitation not been discovered by Newton, the relativity of space and time not by Einstein, they would have been discovered by someone else, in exactly the same way. In the immensely influential tract *Science: The Endless Frontier* (1945), the physicist Vannevar Bush talks of science euphorically as an "edifice" whose form "is predestined by the laws of logic and the nature of human reasoning. It is almost as though it already existed" (in Horgan 22).[5] Bush's view of science, too, is dramatic because it implies that the temporal unfolding of our knowledge of the world is fixed and inevitable. Methodologically, this means that the logic of scientific research is predetermined, too; and that research is scientific to the degree researchers apply that predetermined logic. A conclusion that is scientifically sound is logically always compelling; it owes nothing to the scientist's personal deliberation but is guaranteed by the "predestined" laws of logic, which are applied to empirical data. In science, it seems, consensus is inevitable.

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[4] To establish such a rationalistic basis was the ambitious goal of analytic philosophy and logical positivism in the twenties and thirties of this century – a vain enterprise, as it turned out.

[5] Horgan argues that "Bush's essay served as a blueprint for the construction of the National Science Foundation [U.S.A.] and other federal organisations that thereafter supported basic research on an unparalleled scale" (22).

The dramatic notion of science has survived to the present day; it can be seen, for instance, in the way in which we have institutionalised scientific expertise. In political discourse especially, science is still that solid bastion of objectivity where scientists are engaged in the pursuit of pure facts, which are then handed over to the politicians to be used for the benefit of society as a whole. Practitioners of science and politicians, Elkana points out, "accept that science and its results are predetermined, that the road to truth is one, and that the discipline dictates its own problem choice" ("Epistemology of the Opposition to Science" 185). What this institutionalised idealisation of science presupposes is exactly what is implied in the dramatic notion of science à la Whitehead, Sarton, and Bush: that unanimity is a distinguishing feature of science; that scientific findings are always compelling; and that science develops along a given line. All these assumptions are in startling contrast with scientific practice.

II

Whitehead does not explain why rational unity or consistency should guarantee firmer ground for a steady progress of science than the "anti-rationalistic" assumption of an entirely rational God. What, if not the assumption of an entirely rational Creator, can guarantee that the world is created in a way that is amenable to the "laws of logic and the nature of human reasoning"? Nothing. Or, if there is no rational God (or no God at all), what reason is there to believe that the future form of the "edifice" of science "is predestined by laws of logic and the nature of human reasoning?" None. In spite of his dismissive gesture, the assumption of the predetermined order that underlies Whitehead's conception of science can ultimately only be justified theologically, with the notion of nature as a divine book. Historically, the importance of the notion of nature as "the book of God's work," which is epistemologically parallel to "the book of God's word" (i.e. the Bible),[6] can hardly be overestimated.[7] Here, I am interested in the metaphor for its illus-

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[6] This nice alliterative formula goes back to Francis Bacon's *The Advancement of Learning* (see Durel). The metaphor of the two books, however, is much older. It can be traced to medieval philosophers, in Montaigne, Galileo, Campanella, and many others. See Blumenberg; and Calvino, especially 683 note 1.

[7] Without a thorough understanding of the epistemological and methodological implications of this parallelism, the seventeenth and eighteenth-century debates about empirical science cannot be comprehended. Robert Markley talks in this context of an epistemological "isomorphism of nature and Scripture," "the texts of the two books – the discourse of theology and experimental philosophy – interpenetrate, inform, and elucidate each other" (40).

trative qualities, and because the idea has, as in the case of Whitehead, possessed people even in our century – tacitly at least. In one way or another the idea of nature as a text with a coherent rationale inscribed into it is behind all claims about the unity of science, irrespective of the emphasis of the kind of unity that is postulated: metaphysical, logical, methodological, etc. The most famous instance of this is when Galileo writes that God had written the book of nature in the language of mathematics. For Vannevar Bush, the book is, for no apparent reason, written in a language that can be deciphered by the "laws of logic and the nature of human reasoning"; and for Whitehead, it is the laws of a yet to be discovered rational basis that answer to the building plan of nature. These are all variations of the same assumption: one book, one language, and one Author. In the metaphor of the book of nature, unity is built in, even if the Author is absent. Ian Hacking suspects "that many atheistical admirers of metaphysical unity [of science] have, *au fond*, a thoroughly theological motivation." Hacking illustrates this point with the frequent claim by scientists that theories and laws are better if they are simple. On what grounds, if not on religious ones, Hacking asks, are we to assume that preferences founded upon aesthetics or ease of computation are more likely to be true? We would be hard put to come up with a straight answer to this question; but "Leibniz had a ready answer, that God preferred the simplest theory with the most diverse consequences; it was elegantly economical" (63).

The point here is not that in science assumptions of consistency and harmony are wrong, or bad, or that they have not proved fruitful. Historically, unification has been a success story, particularly in physics. But there is a caveat: diversification of science is a fact, too. The metaphysical assumption of one book, one language, and one Author has clearly not been vindicated. In the natural sciences especially, traditional fields of study splinter into ever more specialised fields. Despite what names like Molecular Medicine or Molecular Biology suggest, the process is clearly one of fragmentation and diversification, not of unification. Disciplines subsumed under the name Molecular Biology, for instance, do not unify chemistry and biology, nor do the different types of biologies or bio-sciences form a united superdiscipline, even though University faculties may for administrative or political reasons be structured in such a way. Yet it is not only science policy or the sheer bulk of (specialised) knowledge that brings new disciplines into being, or that makes it necessary for traditional disciplines to be broken up into subdisciplines. The diversification of science is also motivated by practical and methodological considerations. Research questions referring to different organisational levels of nature call for different methods, different equipment, and different research strategies – perhaps even different types or styles of reasoning. And the findings in one field cannot in any straightforward way be transferred to and integrated into the body of knowledge of other fields. Even worse for a proponent of a strong thesis of unity, there are fields which seem to be cut off hermetically from what would, in a reductionist sense, appear to be their neighbouring discipline. A case in point is high-energy physics and condensed matter physics. In a by now almost legendary essay on the "broken symmetry and the nature of the hierarchical structure of science," P.W. Anderson, one of the foremost condensed matter physicists, acknowledges considerations of symmetry to be of utmost importance to physics: "It is only slightly overstating the case to say that physics is the study of symmetry" (394).[8] But Anderson shows that in physics symmetry itself is discontinuous. There is no guarantee that symmetrical principles which hold on one level will hold true also on a higher or lower organisational level of matter: "a piece of matter need not be symmetrical even if the total state of it is"; or, "a really big system does not at all have to have symmetry of the laws which govern it" (395). With respect to condensed matter physicists, this means that they cannot rely on the findings of the elementary particle physicists. "In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science, much less to those of society" (393).[9] On different organisational levels, then, matter is organised differently; on each level new phenomena emerge which cannot be explained by reference to the properties of smaller units of matter. As a result, Schweber points out twenty years after Anderson, "High-energy physics and condensed matter physics have become essentially decoupled . . ." (38). Any strong reductionist hypothesis which reduces everything to simple fundamental laws (and then starts from these laws to reconstruct the universe) is therefore at least counter-factual; "the reductionist approach that has been the hallmark of theoretical physics in the 20th century is being superseded by the investigation of emergent phenomena, the study of the properties of complexes whose 'elementary' constituents and their interactions are [already] known" (34). When

8 In physics, considerations of symmetry have a strong reductionist thrust: "By symmetry we mean the existence of different viewpoints from which the system appears the same. . . . The first demonstration of the power of this idea may have been by Newton, who may have asked himself the question: What if the matter here in my hand obeys the same laws as that up in the sky; that is, what if space and matter are homogeneous and isotropic?" (Anderson 394).

9 The last twenty years of physics, Schweber holds, have changed this statement "from a folk theorem into an almost rigorously proved assertion" (37).

studying such emergent phenomena, scientists may for good reasons be guided by assumptions of symmetry, simplicity, or harmony; but there is no longer the hope that such considerations can ultimately bring together fields that appear to be epistemologically "decoupled."[10] Philosophically, the situation is messy, very messy; it is as if Whitehead's fear that science would "deteriorate into a medley of *ad hoc* hypotheses" had come true. For Ian Hacking, himself admittedly "an unabashed admirer of the great unifying physicists," "the scientific search for harmony [in physical nature] has been incredibly rewarding while the philosophical quest for [methodological and metaphysical] singleness [in science] has been in vain" (57). The kind of epistemological timidity implied in this statement is not untypical. Whereas Whitehead saw the task of philosophy in providing a universal, rational basis for all science, philosophers of science today usually resist such prescriptive attempts altogether and concentrate on the descriptive, on the analysis of how scientific arguments actually preceded.

Vis-à-vis the complexity and diversity of science, then, the notion of nature as a consistent text or book that can be read with one hermeneutic technique (the predetermined laws of logic and the nature of human reasoning that underlie scientific method) seems totally inadequate. But let us for a moment keep the notion of nature as a text (and the implied notion of a fixed reality) and turn to the question of consistency. Again, what reasons do we have to believe that the (divine) author has written the book of nature in accordance with the laws of logic and the nature of human reasoning? None. "When I read a volume," David Hume's Demea says, "I enter into the mind and intention of the author; I become him, in a manner, for the instant, and have an immediate feeling and conception of those ideas which revolved in his imagination while employed in that composition." But with a divine author such a re-enacting of authorial intention is not obviously possible for a human being. Demea:

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[10] This is not to say that no fruitful exchange between two such distinct fields can take place. What is exchanged, however, is not factual knowledge that allows one field to be deduced from the other in a reductionist sense, but research strategies or analytical methods. Schweber makes the point that today, "The commonalties of theoretical techniques used to address problems in what were different fields is a general phenomena [in science]. . . . The interdisciplinary nature of the new communities studying these [emergent] phenomena is . . . striking. The communities are held together by tools: renormalisation-group methods [a mathematical tool], nmr machines, lasers, neural networks, computers and so on" (38). Such "cross-fertilization" among different fields, as Schweber calls it, takes place in a largely unpredictable manner and must not be confused with methodological unity: "Tools and concepts are constantly being carried from one field to another in ways that are difficult to anticipate by any logical and structural analysis."

> Such near an approach we never surely can make to the Deity. His ways are not our ways. His attributes are perfect but incomprehensible, and this volume of nature contains a great and inexplicable riddle, more than any intelligible discourse or reasoning. (26)

Still, the "volume of nature" seems also to contain some lucid passages. We have indeed acquired knowledge in certain areas: we have, for example, learned enough about gravity to build aeroplanes, or enough about the function and composition of blood to make blood transfusions. Yet our knowledge about the physical world has remained fragmented and incomplete; and there is no reason to assume that these islands of knowledge can one day be brought together to form a whole. Scientists in different fields, it appears, are reading different pages of the same book. But what if the book contains a collection of short stories rather than just one tragedy with a consistent plot? Or perhaps it is not a book at all but a magazine with independent articles, as the physicist James Clerk Maxwell once suggested! Musing on the epistemological implications of the image of nature as a book, Maxwell writes to the bishop of Gloucester and Bristol in 1876:

> Perhaps the book, as it has been called, of nature is regularly paged; if so, no doubt the introductory parts will explain those that follow, and the methods taught in the first chapters will be taken for granted and used as illustration on the more advanced parts of the course; but if it is not a book at all, but a magazine, nothing is more foolish than to suppose that one part can throw light on another. (in Hacking 61)

Even if we accept a divine author and the assumption of the predetermined order of nature, then, "nothing is more foolish than to suppose" an internal consistency that answers to human rationality. But in the same vein one could also ask: Why one language? Might the different episodes in the volume of nature not be written in different languages? Of course they might, some perhaps even in languages not intended for human understanding. It would therefore be equally "foolish to suppose," as for example Rudolf Carnap did, that one could find a unified language of science, for that would mean to look for the best of all languages, God's.

Now there is an aspect of the development of science that the image of nature as a multilingual magazine cannot capture: contingency. The really knotty problems start when we give up the notion of nature as a given text altogether. For then we give up the assumption of the predetermined order of nature and with it the notion of a reality that is fixed. As a consequence, contingency comes into the picture. The sticky point here is inevitability. Could science have developed differently? To what extent is the development of science a socially or culturally determined process? For Whitehead it was clear; inevitability is what distinguishes science from other human endeavours, "remorseless inevitableness is what pervades scientific thought. The laws of physics are the decrees of fate." Also for Bush, the "edifice" of science "is predestined by the laws of logic and the nature of human reasoning. It is almost as though it already existed." Could science really have developed differently? When we think of scientific findings that have become part of our every-day assumptions about the world (e.g. Newtonian mechanics) such claims seem absurd. Yet the case is less clear when it comes to the social sciences or to conceptual entities postulated by modern physics. Two scientific communities may be at odds with each other over the same set of phenomena without one of them being manifestly wrong; unanimity and consensus are not distinctive features of science, but must be sought for in science as in other realms of human culture. New knowledge is accepted only within a (scientific) community that is willing to sanction that kind of knowledge. As Andrew Pickering has shown in a case from high-energy physics, in practice conceptual clashes cannot be said to be resolved simply on the basis of "scientific" merit; sociological moments pertaining to the demands of a larger scientific community play an equally important role. In *Constructing Quarks* (1984) Pickering claims that the theory of quarks won against its rival theory not because it could muster more compelling data, but because it met with less opposition from within the scientific community. The theory of quarks prevailed over its opponent because it offered a theoretical context which could be accepted by other scientists more readily, for it did not radically challenge more traditional assumptions about the field. In the end, it was a collective concession to continuity (rather than radical novelty) that settled the dispute. What is at stake is not the question of whether there are quarks or not, or whether continuity is a good thing, but whether the theory of quarks was inevitable. Pickering's answer is that it was not; for him quarks are contingent in the sense that high-energy physics could have developed differently, in a "non-quarky" way, had the scientific community (as a social group) decided differently.[11]

[11] The development of science is sensitive to social influence in various ways and on different levels. Pickering's book describes social mechanisms at work within the communities of scientists themselves, in his particular case the extremely small sub-community of high-energy physicists. In a similar vein, Bruno Latour and Steven Woolgar show how in scientific laboratories facts are constructed socially, rather than in any straightforward sense inferred deductively from data. But many sociologists and historians of science insist that social factors from out-

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The development of science, then, is potentially discontinuous, unpredictable, and contingent. In what terms are we to talk about such a process? Clearly, no straightforward narrative assuming steady progress along a predetermined line of development will do. We need to reconceptualise the story of the growth of scientific knowledge in such a way that it can do justice to the epistemic and temporal intricacies of that process. To be sure, the new story of science also can only be told in narrative terms; but it is going to be a different narrative, one that is less dramatic, a narrative that no longer evolves with "implacable necessity," as Whitehead would have had it. Since the 1960s scholars of the history,[12] sociology[13] and philosophy[14] of science

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side science, too, must be taken into account. David Bloor, to mention an extreme proponent of this line of thinking, describes how political, ideological, and even religious assumptions shaped (unconsciously) the mechanical natural philosophy of Robert Boyle and his Royal Society friends; and how that philosophy was used by these people for their own political ends. Problem choice in science is also something that is to a large extent socially determined; science does not tell us what kinds of questions scientists ought to pursue. Whether research funds should go into the new bio-sciences rather than into more traditional fields of research is a political decision. For instance, by cancelling the SSC Project (against the will of the scientist involved), politicians spelled the end of an era for high-energy physics in the US. (To what extent all these instances of social influence on science also involve contingency is a question I cannot pursue here.)

[12] The history of science as a scholarly discipline was virtually non-existent before World War II. For a pre-war account of the history of science that exemplifies an extremely naive position with respect to the epistemological status of science, see Sarton (5). A helpful introduction and a collection of essays representing the various approaches to the field up to the early 1970s is in Teich and Young, esp. introduction iv-xxi; see also Crosland. For a more recent contextualist approach to the rise of natural philosophy in England, see Larry Stewart.

[13] For a general account of new developments in the field up to about the mid-seventies, see MacLeod.

[14] The line between the sociology and history of science has become increasingly difficult to draw. In the wake of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), historians, sociologists, and philosophers of science have become increasingly concerned with the social relations of science; many now study its content, methodology and progress collectively as part of a social system. Prior to Kuhn, in the work of the sociologists Robert K. Merton and Karl Mannheim for instance, the (mathematical) sciences enjoyed a privileged position in terms of their epistemological status; scientific methodology and truth claims were thought to be immune to social influences and were therefore deemed to be beyond the pale of the sociologist. With David Bloor and the "Strong Programme" of the Edinburgh School, however, the natural sciences too have become an object of sociological investigation; see also Shapin, "The Social Uses of Science." Today, Shapin is arguably one of the most prominent representatives of the contextualist approach to early science and knowledge; see also Shapin and Shaffer, *Leviathan and the Air-Pump*; and more recently, Shapin, *A Social History of Truth*. The interdisciplinary character of academic interest in science as a cultural rather than purely cognitive activity has

have gradually come up with such different narratives. An original and interesting case is Yehuda Elkana, who keeps the imagery of the theatre, though not that of the Greek tragedy. In *Anthropologie der Erkenntnis* Elkana describes (according to his subtitle) "die Entwicklung des Wissens als episches Theater einer listigen Vernunft." He argues that science and its development could more accurately be described by the concept of *epic theatre* as developed by Bertold Brecht and Walter Benjamin.[15] The main thesis of epic theatre is in sharp contrast with the tragic view of history as evoked by Whitehead. In epic theatre, Benjamin maintains, the order of events is not predetermined; "it can happen this way, but it can also happen quite a different way" (in Elkana, "Of Cunning Reason" 34). In epic theatre "history's outcomes are never inevitable, always amenable to political intervention and transformation." The narrative in Brecht's epic theatre "does not imply any straightforward, unproblematic unfolding of chronology or other linear sequences . . . the emphasis of Brecht's work is upon discontinuity" (Hollington 77). Thus the question to be asked in the history of science cannot be (as with Whitehead, Sarton and their ilk) "what were the sufficient and necessary conditions for an event to take place" or a discovery to be made; in epic theatre the historically meaningful question can only be, "what were the necessary conditions for the way things happened, although they could have happened otherwise" (in Elkana, "Of Cunning Reason" 34). It could always have happened otherwise, had the context been different, had people taken other decisions, choosing other options at their disposal.

Thus the metaphor of epic theatre captures important and often neglected aspects of the history of science. It can show for example that the "edifice" of science has not, as Bush believed, always been there simply waiting to be

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triggered a new discipline: Science and Technology Studies; for a useful and comprehensive introduction to the field, see Felt, Nowotny, and Taschwer.

15 The point is made (in German) in Elkana, *Anthropologie der Erkenntnis* (118-122) and (in English) in "Of Cunning Reason." The problem with Elkana's reading of Brecht and Benjamin is similar to Whitehead's reading of the Greek tragedians: it tells us more about him than about them. As Fredric Jameson has pointed out, Brecht is an author who can easily be rewritten in terms of the concerns of the present. Brecht would have had very little to do with Elkana's conception of science; Brecht's science is not that of "Koyré, Bachelard and Kuhn." "For Brecht . . . 'science' is far less a matter of knowledge and epistemology than it is of sheer experiment and of practical, well-nigh manual activity. His is more an ideal of popular mechanics, technology, the home chemical set of the tinkering of a Galileo, than one of 'epistemes' of 'paradigms' in scientific discourse. Brecht's particular vision of science was for him the means of annulling the separation between physical and mental activity and the fundamental division of labour (not least that between worker and intellectual) that resulted from it. . . ." See Jameson's conclusion in *Aesthetics and Politics* (204). I am interested here in the illustrative capacity of Elkana's comparison of science and theatre, not in Brecht's or Benjamin's conception of history and science.

discovered. Science as we know it today has been constructed and shaped by people who responded to the political, social, and cognitive environment they happened to live in. If we look at the world in the spirit of epic theatre, history is no longer the unfolding of the inevitable. With respect to science this means that we are to some extent relieved from the burden of inevitability that determinist historians and other commentators of science have put upon us; and science is (in part at least) restored to human initiative and, most important perhaps, to human responsibility.

But it seems to me that the image of science as epic theatre breaks down as soon as we step down from the lofty pinnacle of hindsight and look at science-in-the-making. The metaphor works fine as long as we refer to the past, to the history of science. It works because the story of science is, like epic theatre, deterministic and non-deterministic at the same time: The history of science is non-deterministic in that its outcome is always contingent; all could have happened otherwise, had circumstances been different, had people taken other decisions at their disposal. Yet any historical narrative is in an important sense also deterministic, for by the time it is told, history's outcome is determined and therefore no longer amenable to human intervention. Both these aspects are also enacted in epic theatre: the non-deterministic element is comprised in the epic narrative of the play, which is discontinuous and contingent; the deterministic (and with it the dramatic) is enacted in the theatrical setting, which does not allow for the audience to interfere with the given plot. No matter whether a play is epic or tragic, its plot is always predetermined by an author.[16]

What the notion of science as epic theatre cannot capture are the moments in which a new episode of the story of science is being plotted. In order to illustrate the temporal and epistemic intricacies of science-in-the-making we need a more radical image of scientific growth. I suggest the notion of a *spectacle*. The story of the growth of scientific knowledge bears features of the spectacular in that it evolves in a largely unpredictable, often counter-intuitive fashion. It is a spectacle that knows no author and no clear line of demarcation between audience and characters. The plot of this spectacle is the outcome of a complex dynamics between the attempts of the peo-

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[16] Brecht's audience was quick to realise this. If measured against its own claims, Brechtian theory of theatre must be said to have failed. This may count towards an explanation why, in spite of its anarchistic and revolutionary potential, Brecht's technique could be so easily domesticated; why Brechtian theatre, in other words, could offer so little resistance to world-wide dissemination and painless consumption by the masses. It may also explain why Brecht in his later years abandoned the concept of epic theatre in favour of dialectical theatre, in which the traditional distinction of stage and audience became blurred; see Hollington (77).

ple (scientists and non-scientists alike) involved in the "making" of science to shape it (conceptualise) it and the constraints imposed on these attempts from the physical world "out there." Not anything goes in the spectacle of the growth of knowledge; our predictions and our attempts to shape it may very well be frustrated. The plot of this spectacle is multiple and has many loose ends. Even if we acknowledge that social factors from within the scientific community and from society at large influence the course of science in a lasting way, we have to accept that there is a realm which seems to be immune to such influences; nature may lend itself to different attempts at conceptualisation, though not to all we can dream up. "In the physical sciences [especially], nature places strong constraints on our experiments and means of observation and plays the role of an arbiter" (Schweber 40).

The story of the growth of knowledge is a haphazardly plotted spectacle in which nobody can claim an epistemological vantage point; there are no predestined laws of logic that determine its course. No science "has ever begun its history with a *tractatus de methodo* which has proceeded on the basis of rules previously established" (Rossi 6). Logical consistency is something that is imposed on the course of science only retrospectively, once the outcome of an episode is known; only with the benefit of hindsight is it possible to conceive of the development of science as inevitable. Logical consistency in the history of science, in other words, is always anachronistic; as long as we move with time and resist the temptation of hindsight, we are faced with uncertainty, discontinuity, and paradox.

In practice, the spectacular inherent in the dynamics of knowledge-generation clashes with the dramatic conception of science as institutionalised in our society. Such clashes are usually either misinterpreted or ignored; or they are covered up cunningly. Take for example the case of scientific expertise mentioned above, applied to the recent debates over the greenhouse effect, say. The institutionalised role of science is first to analyse the problem in its complexity, then to work out possible solutions, and finally to hand over the results to the policy makers and politicians, who in turn can use these results as the scientific bases of their political decisions. The system allows for disagreement on the level of political decision-making but not on the level of scientific analysis. That different competent scientists working in good faith may disagree on the assessment of the same evidence is not conceivable. "Yet," as Steven Shapin points out in "Why the Public Ought to Understand Science-in-the-Making," "scientists publicly *disagree* in their assessments of what the evidence means and, indeed, of what the evidence is" (28). For the historian of science, however, there is no need for alarm;

such scientific controversies are "entirely normal displays of contingency and uncertainty" (29). Yet for those who believe in the "methodological fairy-tales" ("a universal efficacious scientific method which sorts out good from bad data and confirms or disconfirms scientific theories"), such publicly displayed disagreement among the scientific community causes a threat not only to their idealised view of science but also to the integrity of scientists involved. Shapin rightly says that people who, for whatever reason, hang on to the fairy-tale notion of science have little choice when faced with a lack of consensus in science; they will "pick among these conclusions: (a) that one lot or another of scientists is incompetent, or lying, or in the pay of special interest groups like the nuclear power industry . . . or (b) that the area concerned is not science at all" (29). I recall an interview on Swiss Radio in which a politician, a specialist in environmental policy, was asked how she could possibly claim to base her decisions on scientific evidence when the reports of the experts contradict each other in such an obvious manner. After a long sigh she answered: "This is really a problem, you see. At times I think that the scientists do it on purpose, just to vex us, the politicians."

Very often, however, scientists try to cover up moments of uncertainty and contingency in order to live up to the demands of the institutionalised ideal of science. For their public performances, scientists have in fact developed techniques of deception that border on fraud. A telling example in this context is how research projects are presented to committees that decide over public funding. How can a "good" project be distinguished from a "bad" one? The distinction, as Feyerabend pointed out, does not lie in the fact that good scientists suggest what is plausible and promises success, whereas bad scientists suggest what is implausible, absurd, and bound to fail. "It *cannot* lie in this because we never know in advance which theory will be successful and which will fail" (305). How, then, can a scientist convince a committee of his or her project? If he or she really ventures onto new ground, the outcome is unpredictable in principle. Of course, scientists start from a set of questions for which they seek answers; yet these questions may easily turn out to be irrelevant, and the project as such may go in a direction not anticipated initially. Hence scientists can, if they are honest, only *speculate* about the outcome of a research project; and there is always the possibility that the project may fail. It is not by such timidity, however, that research committees can be convinced of a project. Scientists learn to keep their epistemological cards close to their chests. In practice, many resolve the dilemma simply by postulating results they can, strictly speaking, not yet know – the very results, that is, which the research project in question is actually designed to find out. Others have developed somewhat more sophisticated techniques, for example Eörs Szathmary. On the occasion of a symposium about genetic engineering at the ETH in Zurich, Szathmary gave away what is apparently common practice among scientists:[17] The secret is to be always one step ahead. Instead of risking failure by promising findings that are purely speculative, he predicts what he already knows! In his applications for research funds, Szathmary promises findings which he has already discovered in one of his previous research projects, but has not yet published. The trick is successful because it covers up precisely the two aspects of scientific development which the institutionalised, ideal view of science cannot cope with – uncertainty and contingency.

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[17] Symposium on "The Controversial Gene and How We Got There," 4 May 1998. Eörs Szathmary is Professor at the Department of Plant Taxonomy and Ecology, Eötvös University and the Collegium Budapest, in Budapest.

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