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Social Values, Technical Norms and Scientific Criteria in the Development of Knowledge

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INTRODUCTION

The interrelations of science and technology as an object of study seem to have drawn the attention of a number of disciplines: the history of both science and technology, sociology, economics and economic history, and even the philosophy of science. The question that comes to mind is whether the phenomenon itself is new or if advances in the disciplines involved account for this novel interest, or, in fact, if both are interconnected. When the editors set out to plan this volume, their more or less explicit conviction was that the relationship of science and technology did reveal a new configuration and that the disciplines concerned with its analysis failed at least in part to deal with the change because of conceptual and methodological preconceptions.

To say this does not imply a verdict on the insufficiency of one and the superiority of any other one disciplinary approach. Rather, the situation is much more complex. In economics, for example, the interest in the relationship between science and technology is deeply influenced by the theoretical problem of accounting for the factors of economic growth. The primary concern is with technology and the problem is whether the market induces technological advances or whether they induce new demands that explain the subsequent diffusion of new technologies. Science is generally considered to be an exogenous factor not directly subject to market forces and, therefore, appears to be of no interest. As any case study in the history of technology that includes considerations of the cognitive preconditions of technological advances will show, such a picture is a drastic simplification. A careful analysis of the genesis of a new technology, such as those of the radio and the turbine included in this volume, reveals a complex interaction of a series of factors: preceding developments in the sciences, a specific configuration of economic possibilities and constraints, different orientations of scientists, engineers and entrepreneurs which make them recognize the practical potential of certain knowledge or prevent them from doing so, etc. Indeed, the number and variety of factors is so complex and their conceptualization so difficult that generalizations from case studies seem impossible at this point.

If we look at the history of technology we often find an over-emphasis on the autonomy of technological development and likewise, a stress on the ingenuity of individuals, on the 'heroism' of the lone inventor. Again, this view, although somewhat supported by detailed technical analysis, usually fails to convey the social context out of which the determinants arise that explain the behavior of the individual protagonists of technological development, and account for regularities as well as differences in time and geographical location.

If the historian of technology were to look to sociology for help, he may find clues in the general theory of society and social action, but he will be left with the task of having to 'translate' that theory in order to be able to apply it to the case of technology. Even though technology is one of the most important forces that shape modern societies, sociology has paid surprisingly little attention to it. And the little interest that is shown is focused primarily on the consequences of technological change, while a coherent explanation of the social forces that bring it about is lacking. The sociology of science has made a neat conceptual differentiation between science and technology relegating the latter to the sphere of the production and use of knowledge mediated by social interests. While in principle this is a fruitful perspective, it is limited to technology alone. Science, on the other hand, is perceived to be fundamentally different and the question posed is not which social forces shape its contents but which (functional) prerequisites have to exist in a society to make it possible as it is. Thus, the relationship between science and technology is an ephemeral problem, if any at all, just as technological development itself.

This negligence of the patterns of intellectual development on the part of sociology which, to some extent, can also be attributed to the sociologist's lack of detailed knowledge and his emphasis on social structures could well be expected to be compensated for by the history of science. The history of science and from an epistemological viewpoint, also the philosophy of science attempt to decribe and/or explain scientific development as a cumulative and progressive evolution of ideas where, at least in principle, each step in that process can be shown to be a consequence of the preceding one. Similarly, as in the history of technology, it provides 'internal' accounts of the evolution of scientific thought proceeding on the assumption that the social and economic context in which these ideas emerge plays at best a trivial role in that it makes science possible or impossible. Technology, on the other hand, is seen as a mere 'spin-off' from scientific ideas, alien to science exactly because it is subject to social needs and interests.

From this hopefully fair description of the respective foci and blind spots, analytical strengths and weaknesses of the disciplines that are concerned with the study of science and technology, it is evident that the problem of the relationship between science and technology falls between the lines of disciplinary demarcation. This is not, of course, a new insight, but little followed from it as is exemplified by the lack of response to Lewis Mumford's *Technics and Civilization.* Thus in 1959 Mumford who very early had tried to transgress the lines of disciplinary perspectives was invited by 'Daedalus' to review his own book. When this book originally appeared in 1934, Mumford recalled 'it stood alone in its field'. 25 years later, he felt compelled to comment "Whatever the original defects of 'Technics and Civilization', whatever shortcomings time has disclosed, it still unfortunately possesses its original distinction: It stands alone, an ironic monument if not an active influence".

The answer to the question formulated at the outset as to the causes of the growing interest in the problem is that in all likelihood both the relationship of science to technology is changing and the awareness of it is growing. Indications of the former are new types of institutions such as problem orientated research installations in which the activity of knowledge production transcends the borderlines between basic research and technology, an increasing convergence of the character of scientific and technical training and of the professional organization emerging from a similar convergence of methods, procedures and bases of knowledge. All of these used to be accepted criteria of differentiation between science and technology, but they do no longer seem to be fit for that purpose.

The volume we have compiled cannot pretend to trace all these changes and/or to do this by approaching the phenomenon with an integrated inter-disciplinary analysis that avoids all the shortcomings of the established fields. To formulate programmatic standards is one thing, to realize them in concrete research is quite another and if the problematic of research on science and technology did not exist, obviously there would be no need for a new programme. It is not even clear if it is realistic to expect that the disciplines in question will eventually converge in an integrated approach in the study of the relationship of science and technology as the complexity of such an enterprise would be immense and the advantage of controlling different variables for different goals of analysis would be lost.

Aside from such methodological caveats the compilation of articles that are supposed to be focused on and in consonance with a new and ambitious programme is to a considerable extent a matter of risk and chance. As often happens, the final product looks somewhat different from what was originally planned. It would demand continuous research collaboration to achieve consensus in outlook and thus homogeneity in their writings among a group of authors when attacking a complex issue such as the one presented here. This is what this volume may contribute to, it was not the context from which it emerged. This collection of essays may be said to represent a state of research, however, which from various disciplinary viewpoints, provides the starting point for the kind of analyses we have in mind. Thus, although the individual contributions need the reader to relate them to the research programme, we hope that as a collection they are compelling enough to elicit discussions among historians of science and technology, economic and social historians as well as sociologists.

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PART I

SCIENCE AND TECHNOLOGY
The Conceptual Distinction Reconsidered

PHYSICS – NATURAL SCIENCE OR TECHNOLOGY?

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Modern physicists understand themselves in a *naturalistic* way. Critical questions which philosophers of science ask about reasons for the development of long term research, about the theoretical structure of the results of such research or about the relation between fundamental research and application, usually end with remarks about constraints called, for short, 'natural laws'. These answers are given despite some facts which relate to such different matters as what physicists really do when they engage in reasearch, what conditions on the institutional level physical research must conform to, and what traditions of investigation into nature they follow.

This paper criticizes this understanding of physics by physicists. It proposes an alternative classification of science and technology, and tries to exhibit methodological and political consequences of such an alternative understanding. Thus, in proposing a *critique of science* the paper makes use of a philosophy of science which does not remain stuck at the metalevel of description as if it were to satisfy an abstract need for philosophical classification (which itself seems to require a freedom from concrete consequences). This critique of the physicists' self-understanding, therefore, does not share with such a self-understanding of science the view that critical reflection is part and parcel of an ideology divorced from science's every day practice. What characterizes the claim of this paper is this: beginning with a 'theory of science' which since the Vienna Circle has become an independent discipline, the analysis here should lead to a kind of philosophy of science where the word 'philosophy' stands for the claim of ancient philosophers to be engaged in the search for that kind of knowledge which leads to better action.

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1. What Does it Mean to Say that Physicists Understand Physics in a Naturalistic Way?

The roots of the words *physics* and *naturalistic*, one Greek and one Latin, refer to 'being born', that is, to organic, living parts of *nature*. Physics, however, owes its enormous 'success', to the accomplishments of seventeenth-century physicists in abandoning the Aristotelian approach. The core of this approach was an analysis of motion in the guise of a theory of causes. This analysis, on the one hand, could specify the motions of free fall and projection as a special class of motions explainable by exterior causes as opposed to another class of motions, organic ones, which called for a different explanation. But on the other hand, this analysis could not reach the successful idea of classical mechanics, namely the principle of inertia. Nature, in the specific sense of classical physics and despite the literal sense of the words 'physics' and 'nature', is the inanimate. Consequently from its beginnings in the seventeenth century until today physics is better referred to as *mechanics*. For, more or less burdened with theological ideas, the world as the object of the physicist's research is to be seen as a large machine the functions of which require investigation, description and exploration.

The statement that physicists understand their discipline in a naturalistic way then always refers to a terminological shift of 'nature' from the organic or living to the mechanical or technical.

General statements such as 'physicists understand their discipline in a naturalistic way' are difficult to argue for by means of physicists' explicit declarations. Only rarely do physicists make judgements about their discipline as a whole. But sometimes in physics textbooks belonging to an older tradition remarks can be found on the purpose, the object and the nature of physics. Statements occur here about the need to investigate 'nature' or 'natural phenomena' especially 'an immense multiplicity of natural phenomena'. In every day life situations regularities would have already imposed themselves on the man in the street. Observations of those regularities moreover would have already lead, especially since the introduction of mathematical methods, to the successful discovery of natural regularities, in short 'natural laws'. Despite the fact that a highly developed set of instruments, both of a conceptual and nonlinguistic kind, has been introduced into physical research, it is said that the physicist's curiosity is directed to natural phenomena by technical means which are at best isolated in a pure form or demonstrated as dependent on completely manageable conditions. There is also much talk about the seeking and finding of natural laws, a metaphor which suggests that natural phenomena are searched for like rare plants or minerals. A list of such metaphors would include among others: that the book of nature is written in mathematical symbols, that the universe shows itself to be ordered or to be a cosmos in the original Greek sense of this word, that a deeper penetration of the secrets of nature is the just reward for patient search after the laws of nature, and so on. All such metaphors show that 'nature' defined as the object of inquiry is what it is because there exists an order in nature which becomes all the more manifest the more cleverness is invested in research.

Thus put, physics remain part of a *mythical tradition.* No modern physicists would like to personalize nature. Nature is *objectified.* It is defined as a given object about which empirical research can teach us that certain laws are valid in it. Even the major crisis in the foundations of physics, which lead to the relativity theories on the one hand and to quantum physics on the other hand, could not overcome this conviction. Despite the often repeated opinion that relativistic theorems about space and time are not to be interpreted in a realistic way as H. A. Lorentz did but in a conventionalistic way as A. Einstein did, the relativistic theory of space and time still holds way among physicists as a set of natural laws. And despite Bohr's insight into the complementarity of the given and the produced in microphysical observation, i.e., despite the insight that only the whole complex of the given and the disturbance of the given by observation is open to physical description, this basic problem of microphysics is interpreted as a particular difficulty about the *access* to a certain range of magnitudes corresponding to analogous problems in macrophysics. But the reasons for these difficulties of access are looked for once again in the field of natural laws.

One could, of course, suppose (and support it with good reasons, e.g., that the self-understanding of physicists are always something *post factum*) that the opinions of physicists about physics is part of a dilettante philosophy in the articles of older prominent physicists, or an ideological phenomenon of the 'Überbau' presented in the superficial introductions to textbooks. In either case such opinions could be judged as not seriously defendable and also as practically irrelevant, lacking any real consequences. These meta-statements do not, one could suppose, say anything about relevant features of the self-understanding of physicists and do not have any bearing for physics at all. Such a supposition, however, would be wrong. There are methodological and political reasons to assume that the physicists' naturalistic misinterpretation of their discipline entails *concrete consequences*.

Although the acknowledged theories of modern physics stem from such different sources as astronomy, caloric theory, theories of machines and of the magnetism of the earth, optics, and so on, physicists nevertheless assume that they are dealing with *one nature* which, therefore, has to be described by *one theory*. If historically there occur partial theories which are logically inconsistent with each other, at least one of these partial theories must be wrong. The unification of historically developed theories into one big theory which covers all natural phenomena (and, in the ideal case, is an axiomatic theory) has been, since the beginning of the nineteenth century at least, a naturalistically based aim of physics.

A naturalistic understanding of the *history of physics* corresponds to the naturalistic understanding of theories like those mentioned above. Thus, the history of physics is written as a chronology of successive discovery. As far as theories are concerned, history leads to a growth in the unification of knowledge by *imbedding* partial theories into more comprehensive ones. For example consider the imbedding of acoustics into mechanics or of optics into electrodynamics. Naturalistically, the possibility of arranging theories in this way is not just simply stated, but it is assumed that physicists are forced to act this way by natural laws. Acoustic phenomena are said, for example, *to be* mechanical ones, optical phenomena *to be* electrodynamical ones. But the methodological decision to explain acoustic phenomena by means of mechanics or optical phenomena by means of electrodynamics is not seen.

Part cf this idea of imbedding partial theories into more comprehensive ones is the belief that older theories in physics are not falsified but defined more precisely with respect to the domain for which they hold. For instance, the relation between the Galileo-invariant mechanics and the

Lorentz-invariant electrodynamics is interpreted in such a way that classical mechanics still holds for the domain of small velocities (relative to the velocity of light).

Nature and its laws are still held to be the most important basis for the historical development of theories. Even in the extreme case (which has already occurred in connection with relativistic physics) that a theory, interpreted as a true or corroborated picture of nature or its laws, makes statements about instruments of measurement which differ from those statements which led to the construction and the use of these instruments, physicists prefer to classify these instruments as natural objects and interpret the principle of construction as a kind of preliminary error on the every day level. Although they owe their knowledge about nature to the artificial properties of their measuring instruments, they interpret these instruments as approximations of natural laws (e.g., the euclidean properties of instruments used to measure length as an approximation to the non-euclidean 'structure of space'). As long as the principles guiding the construction of instruments can be interpreted consistently with the empirical theorems based on measurements made by these instruments, no further problems are raised.

Finally the *scientific* character of physics, despite the discussions among philosophers of science over three generations, is construed as the *objectivity* of physics. And objectivity again is interpreted as independence from space and time. It is, therefore, located in the domain of natural laws since independence from space and time is a special mathematical property of certain physical theorems held to be empirical. As a consequence, problems which arise between natural scientists and humanities specialists in a university's every day practice were usually explained among physicists with the argument that the object, 'nature', of their own discipline implies objectivity, whereas the object of the social sciences and the humanities, namely human actions and their results, must lead to endless debates and unresolvable conflicts between schools. Here the methodological characteristics of naturalistic self-understanding merge into political ones. It is held to be a question of scientific empirical knowledge only as to whether a comprehensive theory about nature is possible, research which is independent of any political or moral discussion about its own goals is suggestive. Therefore it is not surprising that those who defend the principle of research as *value-free* ('wertfrei') are mainly natural scientists. Here the value-free character of physics is commonly understood as neutrality with respect to application. The ancient philosophical view that any kind of knowledge is valuable just because it finally enables men to act better is reduced to a mere defensive strategy against questions asked of physicists concerning the goal or the purpose of fundamental research in physics. A largely subjectivistic and hedonistic idea of being a scientific investigator, of making important discoveries, and of being free of all responsibilities for bad applications of their own results corresponds to the wide-spread distinction between fundamental and applied research.

Although it might be easy to defend this distinction as a reasonable application of the principle of the division of labour, the distinction is more usually confirmed by naturalistic arguments: whereas applied research in the defined sense of research whose results are addressed to producers and consumers, is directed by needs or wishes of these clients, basic research addresses other scientists and serves to reveal the truth about nature. Only this program makes a belief in the inner laws and independent destiny of basic research plausible.

2. Nature and Technology

The naturalistic understanding of physics can be criticized from several different points of view. One of the best developed approaches is centered on theories of scientific languages. This approach points out that on the one hand any science is necessarily bound to represent its results linguistically, and on the other hand any terminology, that is to say any system of linguistically communicable distinctions, has a conventional component which is not determined by nature or its laws. Another well known criticism is based on the weakness of the naturalistic interpretation of the history of physics. The concept of nature can be proven to have changed so basically during the history of culture – and to have changed much because of science – that it would be absurd to speak of nature as an historically enduring and identical object of science. In particular, the relation between man and nature in the areas of food production and manufacturing of goods disallows any assumption that there is a nature which can be investigated independent of all historical presuppositions.

Here, however, we shall follow another approach. An unbiased consideration of physics shows that physicists have a great deal to do with *instruments* – an observation which does justice to the unquestionable fact that physicists as empirical scientists gain their experience only by the use of particular instruments, apparatuses and tools. The experience of modern natural science is *apparative* experience, and experience necessarily structured by instrumentation. The instruments used for the collection of empirical results in the form of protocols of observations or measurements are themselves constitutive of these results. Closer consideration allows us to distinguish at least *three kinds of physical instruments* according either to the intentions which are pursued with each, or dependent on the presuppositions of their construction or use.

One class of instruments is used for the *observation of natural phenomena.* These are, in the first place, visual observation of astronomical phenomena, the naturality of which consists in their being independent of men. This independence of planets and stars and indeed of any meteorological or geological phenomenon means that it 'is there', that it can be seen without instruments in at least a diffuse form, and above all that it is not an artefact, i.e., not produced by man. To be sure, the time has passed when it could be a scientific discovery of first rank to explain by means of a telescope the spots on the surface of the moon as shadows of craters. Today physicists are also accustomed to say that natural phenomena are 'described in a quantitative way'. In other words, what shows itself, shows itself as a 'numeric datum' relevant for a theory and relevant only in connection with a particular measuring tool. But this view no longer distinguishes between results of measurements and results of observations, although any use of a measuring tool always makes a 'factum' out of a 'datum' merely by the conventional choice of scales or units.

Here *measuring tools* count as a second class of physical instruments. It does not make sense to say that they are used for the observation of natural phenomena. Although it is indisputable that measurements say something about what is measured, measuring instruments serve to produce artificial phenomena. A clock very well exemplifies this, and it is important to realize the difference between a clock and, say, a microscope. A microscope allows one to observe the inner structure of an organic cell.

This cell is an object which exists independent of the fact of its observation. Clocks however do not show a 'natural object' time or the passage of time as if this object were something 'natural' like the flow of the river Rhine. Time, or more precisely, the domain of temporal statements relevant to physics, does not naturally exist. It is, rather, a *cultural product* which allows communication about comparisons of events with respect to their duration or succession. These comparisons are then generalizable by being referred to a standard motion which is artificially produced as a uniform motion of the clock's hand. Since the days of classical mechanics, uniform motion has been an imitation of the earth's rotations, a fact which led the technology of previous times to divide the day for purposes of organization or for special problems of medicine (pulsilogium), astronomy or navigation. Only after Kant's arguments against the earth's rotation as an ideal standard for the measurement of time is it possible to interpret the history and the improvement of clock-making as guided by a norm, even if not explicitly formulated, which prescribes a uniform motion independent of a natural example.

Therefore, the *duration* of an event which a clock measures is an *artificial phenomenon*. And the situation of other measuring tools is analogous. Only if one were to forget all the actions necessary for the non-verbal production of a measuring rod and all the verbal conventions as well could one say that a natural object such as a broken branch 'has a natural length'. 'Length' can be spoken of in the sense of a length ratio between two objects, and a length ratio can be defined operationally only for straight connections between pairs of points. Being *straight* is a property of the edge of a stick or of a ruler and therefore has to be achieved artificially, that is, by a technology. In other words, the production of straight edges precedes the possibility of stating that a particular natural object has a particular length. This is especially so in a scientific context where explicitness is required. In such a context even a system of *prescriptions* about how to produce straight edges without the use of other straight edges or measurements of length is methodologically prior to the measurements themselves (1).

In summary, whereas natural phenomena like clouds, stars and lakes, can be observed because they exist independent of any human action, lengths, durations, velocities, accelerations, masses, forces, frictions,