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THE ART OF KNOWING

An essay on epistemology in practice

By Jens Høyrup

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Hvad enten man mere empirisk bestemmer Sandhed som Tænkens Overeensstemmelse med Væren, eller mere idealistisk som Værens Overeensstemmelse med Tænken, saa gjelder det i ethvert Tilfælde om, at man nøje passer paa, hvad der forstaaes ved Væren [...].

Søren Kierkegaard, ***Afsluttende uvidenskabelig Efterskrift****, 2. Afsnit, Cap. 2.*

THE ART OF KNOWING
An essay on epistemology in practice

By Jens Høyrup

LECTURE NOTES
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Roskilde University

Dedicated to Tom Webb, Ib Martin Jarvad,
Arne Thing Mortensen, and Poul Lübcke
Appreciated colleagues and friends,
and in memory of Koba Ziro

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CONTENTS

I. INTRODUCTORY OBSERVATIONS

The following pages are lecture notes for the second part of a university course on the philosophy of human science. The first part of the course dealt with the *distinctive characteristics* of the humanities—those features which constitute their particular identity. Citations in the following of [Høyrup 1993] refer to the notes for this first part. The present, second part concentrates on the complementary aspect: Those features which the humanities share with other sciences, the features which characterize the humanities as well as biology, physics, economics and sociology as *sciences*[1]. But even *scientific knowledge and cognition* share a number of characteristics with other kinds of human knowledge and cognition—not least the quality of being a less direct rendition of that reality which we know about than we tend to think in naïve everyday life[2]. Critical understanding of the general properties and categories of knowledge,

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[1] I remind of the definition of the concept of a »science« which was set forth in [Høyrup 1993: 4]: A *socially organized and systematic search for and transmission of coherent knowledge* in any domain. Speaking in the following of the humanities as *sciences* is thus not meant as an implicit claim that they should emulate the natural sciences—but rather that *all* sciences share a number of qualities of which some have mostly been discussed in relation with the natural sciences and others in the context of the humanities or the social sciences. Illustrations will, accordingly, be taken from all three domains.

[2] It should be noticed that this distinction between scientific and other kinds of knowledge presupposes that *knowledge* is more than explicitly formulated theory. *Knowledge*, we may say briefly, is *any kind of conscious or subconscious acquaintance of the surrounding world allowing some kind of adequate action*.

moreover, will throw light on certain problems belonging more or less specifically to the humanities.

In brief survey, the notes are built up as follows: Chapter II introduces to the general problem of what knowledge is via a presentation and discussion of Piaget's »genetic epistemology«. Chapters III to V develops what could be called a »philosophy of science« in the habitual sense, starting in Chapter III with a general presentation of some basic categories and some classical points of view—Platonic and Aristotelian realism, empiricism and Popperian falsificationism, instrumentalism, and the »demarcation problem«. Chapter III also comprises a first confrontation with what I have chosen in accordance with the philosophical tradition to designate a »materialist« notion of truth (but which many contemporary philosophers would call instead a »realist« concept). Chapter IV embraces a presentation and critical discussion of two main approaches to the problem of scientific development: Lakatos's concept of dynamic »research programmes«, and Kuhn's theory of progress through a sequence of »normal science« phases separated by »scientific revolutions«. Chapter V, footing on the discussions of Chapter III and IV, confronts and connects three classical core problems of the epistemology of science (and of epistemology in general): the questions of truth and objectivity, and the notion of causation.

Already Chapter IV considers the acquisition of scientific knowledge as the product of a scientific community. Chapter VI expands this approach, in particular under the perspective of norm or value systems (»morality«) as regulators of the functioning of social communities. In the process of doing so, it develops a general view on what norm systems are.

Chapter VII takes up the historically-concrete making of scientific knowledge under the conditions of the »scientific-technological revolution«. Starting out from a discussion of the relation between »theoretical« and »applied« knowledge, it returns to the Kuhnian cycle, viewing it now specifically as a description of the development of scientific disciplines, and contrasts it with the inherent interdisciplinarity of applied knowledge.

Chapter VIII appears to abandon the philosophy of science altogether. Its central problem is indeed whether, and in which sense, *art* constitutes a way of knowing, drawing for this on the epistemology which was developed in previous chapters. The conclusion, however, is that art plays a central role as training of the skill in synthetical judgment without which analytical thought is useless, in science as in any other domain.

It goes by itself that the exposition owes much to many precursors, from Aristotle and Kant onwards. Some of those by whom I am inspired I have read in original, from others I borrow indirectly (and certainly often without knowing so). It must be emphasized, however, that the essay is not meant as a survey of the views of select philosophers and philosophically minded psychologists and sociologists. Instead I have attempted to formulate a personal synthesis, while keeping it so open that readers will still be allowed to get a broader view of influential opinions and important problems and to agree or disagree with the single strands of the argument.

The argument is indeed a complex network containing many threads and open suggestions. Even though the underlying thought is certainly rationalist, the ideal of rationality which forms its basis is that of dialogue and not the absolutist ideal of the strict proof. As a hint to the reading it may therefore be added that the footnotes are just as important as the main text. They contain further reflections, objections, qualifications, or they function as a device which allows a branching of the argument. Some of them contain material which is essential in subsequent parts of the essay. They should *not* be skipped.

All translations into English are mine if nothing else is stated.

Philosophy and the problem of knowledge

At least since the pre-Socratic philosophers Parmenides and Zeno, the »problem of knowledge« has haunted philosophy; Zeno, in particular, is famous for a number of paradoxes meant to show that our naïve everyday »knowledge« cannot correspond to genuine reality—we »see« the arrow reach its target and »know« that Achilles takes over the tortoise. But the intellect demonstrates clearly, according to Zeno, that this *cannot possibly be true*. Plato, probably following Socrates on this account, argued that we cannot come to learn what we do not know already, and developed his doctrine of ideas on this foundation. Aristotle tried to put things straight by distinguishing different kinds of reality (»particulars« and »universals«, in the language of his Medieval followers—to be explained in more detail below) and different kinds of knowledge. Thereby Aristotle set the stage for the discussion as it took place until Kant, in his »critical« approach, introduced a distinction between the *conditions which delimit our possibilities of knowing* and the properties of *that reality which we strive to know about*, the famous »thing in itself« as distinct from »the thing as it appears to us«: *We* cannot know (material) reality without categorizing it into objects, time, space, and causality. Whether *reality in itself* is structured that way will forever remain undecided and undecidable.

Epistemology (the theory of knowledge, of *episteme;* alias *gnoseology*, theory of *gnosis*) does not end with Kant; nor does Aristotle's »setting the stage« imply that philosophers followed his doctrines until the late eighteenth century. But some way or other all philosophical discourse in the field has been concerned with the relevance or irrelevance of Plato's, Aristotle's and Kant's categories, concepts and doctrines in the field[3]. Mostly, it has also been »philosophical«—i.e., it has been highly sophisticated in its relation to earlier contributions to the philosophical tradition but at the same time commonsensical and often naïve[4] in its appeals to

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[3] An introduction to epistemology based on the historical discussion is Losee [1972]. One critical observation should be made in connection with this otherwise recommendable work: Hegel, as every approach somehow derived from Hegel, is absent from Losee's universe.

[4] Here as in the following, I use the term »naïve« in opposition to a generalization of Kant's notion of a »critical« approach, and not broadly as »gullible«. The »naïve« attitude is the one which accepts things for what they seem; the »critical« approach is the one which investigates whether, why and in which sense the naïve attitude is justified.

Evidently, most of our practical life has to build on »naïve« foundations and does so without problems. As formulated by Ogden Nash (quoted from Thorkild Jacobsen [1988: 123]): »O, Things are frequently what they seem/ And this is Wisdom's crown:/ Only the game fish swim upstream./ But the sensible fish swim down.« Often, critique does not tell us that we were *wrong* when being naïve but only that we were right or as right as could be without knowing why; this is, e.g., the outcome of Kant's »critique of pure reason«.

No critique is ever definitive. What seemed at one moment to be an absolute underpinning (be it Euclid's proofs that the methods of practical geometers were right, Kant's critique of Newtonian physics or Marx's critique of Smith-Ricardian political economy) turns out with historical hindsight to make other »naïve«

empirically established *knowledge about the processes of knowing*. Aristotle himself, it is true, based his doctrines upon profound understanding of the best standards of scientific knowledge of his times; even philosophers like Locke and Hume, however, were far from understanding the real intricacies in Galileo's and Newton's scientific methods and standards, notwithstanding their claim and belief that *they* expounded the true sense of the feats of these heroes.

Practical scientists, on their part, however sophisticated the methods they apply in the acquisition of knowledge about the field they investigate, are often highly naïve when it comes to understanding the philosophical implications of these »critically«. As formulated by Imre Lakatos [1974a: 148 n.1], »scientists tend to understand little more *about* science than fish about hydrodynamics«.

For this double reason, a direct and immediate dialogue between classical epistemology (or, more specifically, classical »philosophy of science«) and actual scientific practice is not likely to be very fruitful. An introduction to the problems of epistemology intended to further critical reflection among »practitioners of knowing« should therefore rather take its starting point in approaches developed during the twentieth century, based on thorough empirical observation of the processes of knowing and requiring that their philosophical framework should fit these empirical observations[5].

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presuppositions which in their turn can be »criticized«. We choose a misleading metaphor when we speak about establishing »a firm foundation« on which we can build safely. We always build on swampy ground; what criticism does is to hammer the piles on which we build through so many layers of mud and clay that they are not likely to move significantly.

[5] This preliminary description, »naïve« as anything could be, should not be read as a claim that these approaches are *nothing but* empirical. One of the main points of the following (derived, in fact, from the empirically oriented studies, and agreeing in this respect with Kant) will be that no knowledge is based on empirical observation and experience alone.

II. A PIAGETIAN INTRODUCTION TO THE GENERAL PROBLEM OF KNOWLEDGE

One approach to the problem of knowledge is through *individual cognition*. This is even, one might reasonably claim, the most obvious approach, since knowledge is always knowledge known by somebody, however socially conditioned and organized it may be. Even knowledge embodied in books or databases has been put down by somebody and, more decisively, is only transformed from black dots or magnetic traces into *knowledge* when interpreted by a mind.

A number of twentieth century psychological schools have set forth doctrines or theories about the nature and construction of human cognition. Some, like the claims of behaviorist school[6], are in the main postulates about how cognition *should be* explained. Of interest in the present connection are in particular the Soviet Vygotsky-Luria-Leontieff-school, and the Swiss Piaget (1896-1980) with collaborators, on whose work I shall base the present chapter (while being to some extent inspired by the former school in my own reading of Piaget).

Piaget is most widely known as a »child psychologist«, which is true in the sense that he contributed decisively to the understanding of child development (and has been amply used/misused in the planning of curriculum reforms) but is otherwise a misleading statement. He started out as a biologist specializing in mollusks but with a strong interest in metatheoretical questions, not least in the logic and philosophy of science. This led him to accept in c. 1920 the task to standardize Cyril Burt's IQ

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[6] According to which, briefly spoken, all human knowledge is (like behaviour in general) to be explained as a web of conditioned reflexes—cf. [Høyrup 1993: 183ff].

test for French children, which again led him to discover his own approach[7].

The principle of the IQ test can be described as *kinematics*, »movement-description«. A number of problems are presented to the experimental subject, and correct and wrong answers are taken note of. At the same time it is known (from the »standardization«) which tasks are solved by average children at a given age. In this way, the »mental age« of the subject can be determined, or the »intelligence quotient« understood as »percentage problem-solving capacity« in comparison with the average subject of the same age. The central concept is thus the dichotomy »correct«/»wrong« answer, and the central tool the time-table telling at which age various types of problems will normally be solved correctly. Questions concerning the driving forces behind the process of intellectual development, and even the very idea of a *process*, do not occur.

Piaget soon noticed that the »wrong« answers were not only »wrong« but also systematic. We may illustrate this by an example borrowed from his later research. A girl of five[8], asked whether there are more girls or more children in her kindergarten may answer »more girls«, »more boys«, »equally many« [*viz*, girls and boys] or »I do not know«. The one answer you never get is the »obviously correct« answer »more children«.

I shall return to this experiment below. For the moment we shall only observe that the consistent deviation from adult thought must correspond to a different way to conceptualize and think about quantity, not to mere absence of conceptualization and thought. This was also one of conclusions drawn by Piaget, who set out to find the *dynamics*, the active forces and processes, of the development of cognition. The other conclusion was that things like the Kantian categories (object, number, space, time, causality)

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[7] See the introduction to Rotman [1977].

[8] A »normal« girl, that is; here and everywhere in this chapter, age indications are only approximate and subject to large variations. But if your own five-year old niece gives the »grown-up« answer you can be sure that she would have given the »five-year« answer at ages three or four. In the language of technical statistics, ages constitute an »ordinal scale«, and are only numerically true as averages (and even that only for that geographical and social environment where they were established—whence the need for re-standardization of British IQ-tests in France).

cannot be inborn, and thus that cognition has a genesis and results from a development process.

The titles of some of the books which Piaget published over the following two decades read like as many empirical tests of Kant's categories: *Judgement and Reasoning in the Child—The Child's Conception of Physical Causality—The Moral Judgment of the Child—The Child's Conception of Number*. Part of this research was based on the »typical answers« of many children to »revelatory questions«, e.g., of the type current in IQ testing. As his own children were born, Piaget took advantage of the possibility to observe and interact with the same children over several years, thus interpreting their mind »from within« in a more hermeneutic manner.

During the following decades, Piaget reemerged from his submersion into empirical child psychology and formulated his general »genetic epistemology«. i.e., a general theory of human cognition as the outcome of a process. It is this mature theory (which, it should be noted, remained a living research programme and was never a fully finished doctrine[9]) on which I draw in the following, and which is the starting point for certain further reflections[10].

Schemes and dialectic

Let us start from an example. A child of (say) 1½ years is familiar with a variety of balls: White, blue, red, variegated; made from cloth, from rubber, from leather; with diameters ranging from 3 to 6 cm. It knows that you may push them, make them roll, throw them from the chair where you are sitting to make your patient big sister pick them up for you. If a new ball of some colour and of familiar weight and diameter gets into its hands, the child will demonstrate in practice that it knows how to behave with balls. We may say that the child possesses a *practical concept*

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[9] Although, so rumours tell, Piaget was so dominating a personality that the group of researchers at his Institute in Geneva never developed into a genuine »school« but remained a circle of »Piaget's collaborators«. Theoretical innovation seemed to have been the privilege of the master.

[10] It should be pointed out that some of the illustrative examples and experiments referred to in the following are borrowed from Piaget; others are my own observations, mainly made on my own daughters.

or (in Piaget's language) a *scheme* for balls. This scheme is not present in conscious thought but is *sensori-motor*, i.e., derives adequate movements/ actions from sensual perception.

If now some fully unfamiliar ball is presented to the child—say, a football or, even better, a 5 kg leaden ball, certain familiar acts are impossible. It is too heavy to be thrown (and if you roll in from the table onto your sisters feet her patience will certainly be gone). But accident or deliberate experimentation may produce the experience that even this object can be pushed and rolled along the floor. In this way, the leaden ball is *assimilated* to the scheme. The scheme, on the other hand, is changed and made more flexible by encompassing even this unfamiliar ball: It *accommodates* to the larger field of experience.

This process introduces a number of key concepts from the Piagetian theory. Firstly, the pair assimilation/accommodation. Secondly, the concept of *equilibrium*, which according to Piaget is the central aim of this twin mechanism. *Equilibrium*, in this terminology, is no static condition, but to be conceived in the likeness (and according to Piaget indeed as a special case of) the dynamic equilibrium of living organisms. Irrespective of the surrounding temperature and the precise nature of its food, a dog will conserve approximately the same body temperature and the same organic structure; to keep up this equilibrium is the task of its metabolic processes. Extreme conditions which destroy the equilibrium will, at the same account, kill the dog. A new-born puppy, on the other hand, can stand fewer variations of its living conditions. In this sense, the equilibrium of the mature dog is more stable than that of the puppy. And in the same sense, the accommodated scheme for balls constitutes a more stable equilibrium than its predecessor, since it is able to grasp without difficulty a wider range of different balls.

Thirdly, the inescapable duality between assimilation and accommodation makes Piaget's theory of knowledge a *dialectical theory*. It makes sense of Plato's claim that you cannot come to know what you do not know already, and gives substance to Aristotle's statement that you may, in different interpretations of the word, know and not know something at a time: you will only discover that the leaden ball is a ball if you already know (in the case of the infant, »know« practically) what balls are. But (and this is the key point where Piaget's dialectic really goes beyond the classics): by discovering the leaden ball as a ball you also come to *know more about what balls are*. In this way, every act of knowing is at the same time an assimilative interpretation with regard to an existing *cognitive structure* and an accommodation of this structure to new domains of reality; although they are strictly distinct, none of the two processes can take place in the absence of the other.

Two further observations can be made on the above example. Firstly: If nothing of what the child usually does when playing with balls fits the unfamiliar object, it will not be assimilated to the ball scheme; nor will it of course be assimilated to any other scheme if it does not match in some way. In most cases, this will result in a practical rejection of the experience in question—what makes no »sense« is not noticed or quickly forgotten[11]. Secondly: Assimilation presupposes attentiveness; it is therefore more likely to result from deliberate experimentation than from accidental events (behaviourist pedagogical theory notwithstanding, cf. [Høyrup 1993: 185]).

Let us then look at another example. During the first months after birth, the baby's world can be described as

> an object-less universe, composed of perceptual tableaux which appear and disappear by a process of resorption, and an object is not sought when it is hidden by a screen—the baby, for example, withdraws his hand if he is about to grasp the object and the latter is covered by a handkerchief. At a later stage the child begins to look for the object, lifting the handkerchief at A where it has just been covered; and when the object is displaced to a position beneath B (for example to the right, whereas A was on his left), the child, although he has seen the object being placed at B, often looks for it at A when it disappears again; that is, he looks in the place where his action has been successful on an earlier occasion. He therefore does not take account of successive displacements of the object which he has nevertheless observed and followed atten-

[11] This is no absolute rule. Human beings, not least children, are curious, and a puzzling object or class of objects *may* provoke intense experimentation/investigation (inquisitive »play« in the case of the child), and in this way provide the basis for the construction of a new scheme; but if not even the single sensual outcomes of experiments »make sense« with regard to the existing cognitive organization of sensory experience, this constructive process is not likely to take place.

tively. It is not until towards the end of the first year that he looks for the object unhesitatingly in the place where it has vanished.

([Piaget 1972: 11f]; cf. [1950: 108ff]).

At this age the child will thus remove a blanket under which a coveted toy has been placed. Still, if a Basque beret and not the toy turns up below the handkerchief, it abandons the pursuit. Only in a following phase will it remove the beret and—triumphantly perhaps—find the toy, whose permanency as an object firmly located in space is no longer subject to doubt [Piaget 1973: 11ff].

Balls are no necessary ingredients of the universe, and the construction and stepwise accommodation of a scheme for balls can thus not wonder. But permanent objects seem to us to be unavoidable, one of the very fundaments for knowledge of the world. Piaget's investigations show, however, that even this as well as all the other Kantian categories without which no knowledge of the physical world is supposed to be possible is the product of a development, going through a sequence of accommodative extensions and equilibria.

In one of his publications, Piaget [1973: 66] defines the *scheme* of an action as »the general structure of this action, conserving itself during [...] repetitions, consolidating itself by exercise, and applying itself to situations which vary because of modifications of milieu«. On p. 114 of the same work, a scheme is defined more briefly but in the same vein as »what is generalizable in a given action«. Both variants of the definition may call forth the legitimate question, whether the *scheme* is really part of the mind of the knowing and acting child or only a construct, made by the observing psychologist for his convenience[12]. If the scheme is nothing but a psychologist's construct, the whole idea of accommodation becomes dubious, and the question of the status of the schemes must therefore be addressed before we go on.

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[12] This question is indeed the normal positivist reaction (see below) to any idea of *general structure* or *universal* encompassing and determining particulars—is it THE HORSE in general which determines the characteristics of individual horses, or is the universal concept (the species) nothing but a shorthand in which zoologists sum up their knowledge of similar individuals?

Normally questions of this kind are undecidable and hence, one may claim, meaningless pseudo-problems. In the case of *schemes*, however, a decision can be reached—not, it is true, in the precise case of the scheme for balls, but if we look at *linguistic schemes*.

The past tense of »hunt« is »hunted«, the past tense of »reach« is »reached«, etc. These forms, too, are formed according to a scheme, which can even be seen to apply »itself to situations which vary because of modifications of milieu« (the »e« is pronounced when coming between »t« and »d« but not between »ch« and »d«; and even in this case we may ask whether the scheme is a grammarian's shorthand or really present in the mind of speakers before they have been taught grammar.

The answer is »really present«, and is provided by young children below the age where they can understand any grammatical explanation. They may have learned the forms »hunted« and »reached« by hearing them spoken by grown-ups, and might in principle just store these forms individually. But they have never heard the form »goed«, since all mature speakers say »went«. None the less, their first past tense of »go« is »goed«—and when they discover that this does not agree with adult usage, they opt for a while for the compromise »wented«[13]. These forms can only result from a sub-conscious general scheme which the child has constructed on the basis of forms like »hunted« and »searched«. This scheme, which deviates (in extension) from the grammarians' scheme, must be present in the child's mind; it can be no mere observing psychologist's construct. If the existence of schemes is established, on the other hand, their accommodation (in the case of verbal conjugation in the steps »goed« —> »wented« —> »went«) is also real.

The periods

»Every act of knowing is at the same time an assimilative interpretation with regard to an existing *cognitive structure* and an accommodation of this structure to new domains of reality«, as stated above. But as a scheme tends toward equilibrium, the accommodative aspect of the process becomes less

[13] »Gåede« and »gikkede«, respectively, in my own observations. But English linguists confirm the English forms given here.

prominent. The different schemes available at a given moment also tend to form a coherent system and to share a number of basic features and limitations (to be exemplified below). This is precisely what justifies the idea of an over-all cognitive structure[14]. Most of the time, the gradual accommodation of schemes brings about a maturation of this structure, increasing its coherence, »stability« and functionality. At certain moments, however, new mental abilities (one of them being the emergence of language and conscious thought) destabilize the structure: within a relatively brief period, new schemes arise, old schemes accommodate thoroughly, and schemes are integrated in new ways. This lays the foundation for a new cognitive structure, organized at a higher level, which is going to assimilate increasingly large ranges of *earlier* as well as *new* experience.

These changes of the over-all cognitive structure demarcate the *developmental periods*, of which Piaget counts four (less thorough transformations of the structure—e.g., the one allowing the child to remove the beret to find the toy—delimit *stages* within the period):

1. The *sensori-motor* period, extending from birth until the age of 1½ to 2 years (with the usual *caveat* concerning these precise ages).
2. The *pre-operatory* period, extending from 1½/2 until c. 7 years.
3. The period of *concrete operations*, from c. 7 to c. 12 years.
4. The period of *formal thought*, from c. 12 years onwards[15]

[14] Piaget, who loved mathematical metaphors and believed them to be more than metaphors, would often bolster up this explanation by referring to a number of mathematical and (apparently self-invented) pseudo-mathematical concepts in this connection. This need not bother us here.

[15] I remind of the observation made above that the age indications correspond to averages as established in the (urban European) environment where Piaget established the sequence. Others have made similar investigations in others surroundings, finding (with a proviso concerning formal thought in certain societies, to which I shall return) the same relative sequence but a considerable time-lag, e.g., in an Iranian rural district where children were »not only without schools but also without any toys other than pebbles and bits of wood« (Piaget [1973: 154], citing Mohseni's research); it is important to note that this delay only concerned the onset of the operatory stage, while tests based on preoperatory thought showed no difference between these children and children from Teheran.

During the sensorimotor period, the development of intelligence is characterized by increasingly effective coordination between sensory perception and motor activity (practical action). Starting off from a number of separate and unconnected »sensory spaces« (a sucking space, a visual space, a tactile space, an auditory space), a unitary picture of the surrounding world is gradually achieved, where a noise will provoke the child to look around for the source, and an interesting object coming within the visual space may make the child move towards it and grasp it[16]. The categories of *space, time, permanent object,* and *cause* are developed as *practical* (but certainly not conscious) *categories*. What this means in the case of the permanent object was already elucidated above. Having a *practical category of space* means possessing (among other things) the ability to plan a composite trajectory through a room, picking up a toy at point A and going on directly (and without new spatial planning) to point B where the toy is to be used for some purpose. Possessing a practical category of causation implies, e.g., to get the idea to draw a table cloth upon which a coveted object is placed outside your range.

Implicit in the formation of the unified sensory space and of the practical categories is a gradual *de-centralization* (or »miniature Copernican revolution«, as Piaget calls it [1967: 79]). The child itself is the only possible centre of the sucking space, of the tactile space, etc.—these spaces are »egocentric«[17]. In the integrated space, on the other hand, the child only

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[16] These acts of intelligence should be held strictly apart from certain reflexes which seem to mimic them. The grasping reflex, e.g., makes the new-born infant grasp whatever comes within its hand and cling to it; intelligent grasping is an intentional act which the child may choose or not choose to perform.

[17] If the concept of an egocentric space seems queer one may think of those sensory spaces which are never really integrated into the unified space: The gustatory »space«, the olfactory »space«, the pain »space«, the heat and cold »spaces«. All of these are immediately perceived where *you are yourself,* and they are often felt as *moods* rather than as information about the world around you. Sounds, in contrast, are not perceived as heard in the ears or anywhere else in the body but at their presumed source (barring exceptional cases), and light as a rule not at all as light but as objects seen at a distance.

This does not (or not alone) depend on the nature of the stimulus: dogs, so it appears, integrate their olfactory space into the unified sensorial space, and perceive smells as connected to objects, not as mere smells.

occupies one position among many possible and equally valid positions (which means that it can plan a trajectory passing through a sequence of positions).

The sensorimotor cognitive structure reaches maturity around the middle of the second year of life, when integrated space, practical categories and decentralization are attained. At this moment the child behaves as adequately as it is possible without the intervention of conscious thought, given its physical and sensory equipment. At that moment it has reached the maximum intelligence of chimpanzees. It also stands at the threshold to a new period, characterized by the first emergence of language and conscious thought[18].

One element of the new cognitive structure is created through the maturation of sensorimotor thought. Being able to *plan* a trajectory or stopping at a problem, reflecting and suddenly knowing how to go on after an »*aha*-Erlebnis«[19] are indications that the schemes for action have been »interiorized«, have become an element of thought, which can be anticipated before (or without) becoming actual action (cf. [Piaget 1973: 57]).

Another element, and a symptom that thought is emerging, is *symbolic play*. Sensorimotor children »play with« (e.g., with a ball, or with their toes), while older children »play that« (e.g., that they are parents while the doll is the child). Such symbolic play presupposes the symbolic function, but it also provides this function with substance through internalization of the play.

The most conspicuous element, of course, is language, which has been prepared through extensive sensorimotor play with speech sounds (»babbling«) but only becomes language and a tool for thought in the moment where a string of speech sounds functioning as phonemes is used

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[18] Chimpanzees seem indeed to stand at the same threshold—so precisely there, in fact, that the same experiments can be interpreted by some scholars as proof that they possess the ability for language and by others as proof that they lack this ability.

[19] This term was first coined by Wolfgang Köhler (also known as one of the founding fathers of gestalt psychology) as a description of the same moment in chimpanzee behaviour.

symbolically for something beyond itself and is interiorized with this function[20].

The (embryonic) emergence of a new cognitive structure does not abolish the achievements of the preceding period. Children of three are no less sensorimotor effective than they were a year before. Still, the immediately interesting point in a discussion of the cognition of children between two and seven is of course the *new* level, the characteristics of *thinking*.

The thought of »pre-operatory« children (a term to be explained in a moment) is characterized by Piaget as follows:

— it is egocentric;
— it is centring;
— and it is irreversible.

That thought is egocentric (not the same as egoistic!) means that the child does not perceive of its thoughts as *its own thoughts* which others may not share; in other words, it comprehends its point of view as the only possible point of view, which makes it difficult to entertain a genuine dialogue with the child unless you already know what it thinks. This may be illustrated by means of a conversation with a girl of two who had been to a zoo with her creche:

Adult: »What did you see in the zoo?«
Girl (with enthusiastic emphasis): »That one!«
Adult: »What is 'that one'?«
Girl (with increased emphasis): *»That one!«*
Adult: »Did you see monkeys?«
Girl (happily): »Yes«.

[20] This may be made more explicit by an example. At one moment in life, you may use a sound like »mama« in order to make your mother take you in her arms; this can be interiorized in the same way as any other sensorimotor scheme, and will allow you to plan how to be taken into your mother's arms; but only when the sound is used as a more general symbol for this all-important permanent object (normally the first object of whose permanence we are sure) will it allow you to think about your mother and not only about the specific act.

The linguistic distinction between sounds and phonemes (which is not very important in the present context) is explained briefly in note 37.

The answers given by young children when confronted with questions of »why« are characteristic of a generalized egocentric attitude. They are not answered as concerned with *causes* but as regarding an anthropomorphic *aim:* »If Lake Geneva does not go as far as Berne, it's because each town has its own lake«[21].

It has been objected repeatedly to the cognitive interpretation of such statements that children answer in such unexpected ways because they understand words differently. This is certainly correct in itself. Children understand words in a way which corresponds to their answers. But they do so consistently (and in spite of the way the words are used by their adult surroundings from which they learned them) because they are unable to grasp the usage of the grown-ups, which cannot be assimilated to their own cognitive structure. The »cognitive interpretation«, the interpretation of children's sayings as evidence of their basic pattern of thought, is not only permissible but mandatory.

The »centring« character of thought can be explained through another example (in fact one of Piaget's key experiments). A child of five is shown a ball of soft clay and looks at the experimenter as he rolls it into a sausage. Asked whether the sausage contains more or less clay than the ball, three out of four will respond that there is more, since »the snake is longer«; the rest will claim that there is less, since »it is thinner«. In both cases, the child concentrates interest on one conspicuous feature of the situation and does not take other features into account.

The irreversible character of thought can be elucidated by one of my own observations. A girl who had learned rudimentary counting by means of the number jingle (and who was thus at the threshold to the next period, cf. below) was asked as follows: »Seven birds are sitting in a tree. Then two more birds join them, but soon two birds fly away. How many are

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[21] [Piaget 1967: 25]. Twentieth-century adult thought, of course, may still answer *some* questions of »why« as concerned with purpose. The motor-road around Roskilde is there »because cars should be led around the centre of the town« and not »because workers levelled the surface and spread concrete«. But the Alps are there because of geological forces and not because they constitute such a nice skiing resort. At least in the era of secularization, adult thought tends to de-anthropomorphize processes not performed by human beings.

left?«. The answer was simple and characteristic: »You would have to count them again«. Some months later, in contrast, she was asked the same question, but dealing with »a hundred thousand million birds« (i.e., a number which she could not remember but knew was a number), eight birds joining and eight leaving. She was not encumbered by the impossibly large number but simply answered »equally many«. At this moment she could grasp the process »from above«, and administer the mental process from a meta-level from where the mutual cancellation of the two changes (in other words, the reversibility of the process) could be perceived.

The absence of a meta-level in the thought of pre-operatory children is seen in another one of Piaget's question types—the one exemplified above by the question whether there were »more girls or more children in the kindergarten«. The point in the answers given by pre-operatory children is precisely the absence of a meta-level on which the total category of children can be comprehended *together with* the two distinct subcategories. In the absence of this level, separating the girls automatically transforms the idea of »children« into »the remaining children«, i.e., »boys«. The mental process is really an ongoing process, a *chain* where each step supersedes its predecessor step and makes it inaccessible to renewed treatment (cf. [Piaget 1950: 113])[22].

[22] It is sometimes claimed that the young children produce their »absurd« answer not because of any inability to understand »correctly« but simply because they assume the psychologist to have something sensible in mind. That this is no adequate explanation follows from an observation which I made on my other daughter when she was close to the end of the pre-operatory period (having already learned rudimentary counting). When asked whether there were more girls or more children in her kindergarten she replied »I don't know«. I then produced a model, telling that her fingers were the children, those on the left hand the girls and those on the right hand the boys; when asked, she was able to identify both girls, boys, and all children correctly and without difficulty.

Next I asked for the number of girls, and she counted until five on her left hand. When asked about »all the children« she counted until five on her right hand. I took her hand and continued with »six« on her left—and before I reached »seven« she burst into violent crying, screaming »one can't do that«—without my knowing it, my seemingly innocuous experiment had violated her world order.

As one may imagine I did everything I could in order to comfort her, admitting that it could not be done in the way she thought, but that it could in the way her big sister and I thought, ... In the end, I asked whether she understood, which

The radical egocentricity exemplified by the use of »that one« as a valid terms for monkeys retreats during the pre-operatory phase, and from the age of four the child is able to participate in what the adult interlocutor perceives as sensible dialogue. This demonstrates that mature pre-operatory thought is a functioning and relatively adequate structure—relatively adequate functioning is indeed the very condition that a cognitive structure can be stable and thus mature. It may therefore seem misleading to characterize this thinking by *what it is not (yet)*, i.e., not based on *operations* (to be explained in a moment). But in spite of relatively adequate functioning of the thought of the period, its irreversibility and centring character as well as a less radical egocentricity remain for years, and a meta-level does not develop. As an alternative to the negative characterization »pre-operatory«, Piaget therefore uses the term »intuitive thought« ([1950: 129]; [1967: 29]) to describe the typical way of thinking of children between four and six as *what it is:* Stating opinions without argument or support from facts; how should, in fact, a chain of arguments be constructible if you cannot step outside the chain of your own thinking? And what would be the use of arguments if you do not see your own thinking as only one possible way to think which your interlocutor does not necessarily share?

The negative characterization »pre-operatory« is a characterization of that which typical pre-school thinking *is not yet* but is on its way to prepare. Around the age of seven, children will know that there are more children than girls in a mixed school class, and more flowers than primroses in the garden. Similarly, they will now tell that the sausage and the clay ball contain the same quantity of clay. In the latter case, the reason normally given is typical of the acquisition of reversibility: You could roll the sausage back into its old shape. The idea of »same« is, in fact, quite empty or at least unspecific; it will take years before the child is able to foretell that the *weight* of the sausage is not changed, and still more before it will predict

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she denied—but at least she had stopped crying. Two hours later, however, she could understand *nothing but* the operatory structure, and for days she went on enthusiastically posing me analogous questions about mackerel and fish, and other categories as bizarre as possible.

Neither crying nor enthusiasm could certainly have been produced if only a linguistic misunderstanding had been involved.

that the volume (i.e., the raise of the level of water in a glass into which the clay is immersed) is conserved. Similarly, the child who knows that flowers outnumber primroses is his garden (and who tells that not all animals are birds, since »there are also snails, horses ...«, while all birds »certainly« are animals) will not be able to decide whether there are more birds or more animals outside the window[23].

In Piaget's words [1973: 24], the reason for the latter failure is probably that »flowers can be gathered in bouquets. This is an easy concrete operation, whereas to go and make a bouquet of swallows becomes more complicated; it is not manipulable«. Initially, the intellectual operations of the next cognitive structure are thus strictly bound up with concrete imagination. But they are what Piaget calls »operations«, i.e.

> »interiorized (or interiorizable), reversible actions (in the sense of being capable of developing in both directions and, consequently, of including the possibility of a reverse action which cancels the result of the first), and coordinated in structures [...] which present laws of composition characterizing the structure in its totality as a system«.
>
> ([Piaget 1973: 76]; cf. [1967: 78])

This concept is beautifully illustrated by the »hundred thousand million birds« plus and minus eight[24]. Only at this level can a concept of counting be constructed (younger children see no problem in having mislearned the number jingle in such a way that it ends in a loop, e.g., 1-2-3-4-5-6-7-8-9-10-11-7-8-9-10-11-7-8-...); only at this level is conceptual analysis possible; etc. In general, operatory thought is felt by adults to be »logical«; actually, Piaget used the term »pre-logical« as late as 1940 instead of the technical term »pre-operatory« which he was to coin later (see [Piaget 1967: 30]).

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[23] This delay of the unfamiliar as compared to the familiar is obviously contradicted by the observation reported in note 22. The difference is to be explained by the blow to which I had unknowingly exposed my daughter's world order, and illustrates the importance of affective factors even in the domain of seemingly neutral cognition.

[24] With the proviso that the girl had been subjected to so many Piagetian problems that she was able to manipulate fairly abstract entities like the fancy number in question.

Operatory thought starts in the most concrete domain, as we have seen. Gradually, larger areas are assimilated—at the typical age of ten, no doubt will remain that there are more animals than birds outside the window, nor that the weight of the clay ball does not change when it is transformed into a sausage; at the age of thirteen, most children will admit that even the volume cannot have changed. Still, the logic of operatory thought remains a *logic of the concrete*. Instead of seeing assimilation as an extension of operatory structures so as to cover increasingly abstract domains we should indeed rather see it as an integration of increasingly wider ranges of experience into the realm of the familiar and thus concretely imaginable[25]. Purely formal operations remain inaccessible. This is illustrated by the inability of typical children below twelve to answer correctly one of the old Burt problems, an example to which Piaget returns time and again (e.g., [1950: 149]): »Edith is fairer than Susan; Edith is darker than Lily; who is the darkest of the three?«. The child of ten will mostly argue that both Edith and Susan are fair, while both Edith and Lily are dark. Edith must thus be in between, Lily must be darkest, and Susan is fairest. Only at (typically) twelve, thus Piaget, will the child be able to argue correctly from the purely formal sequence, e.g., by inverting the first statement into »Susan is darker than Edith«. From now on, the child (now, normally, an adolescent) will be able to manipulate symbols in agreement with abstract rules and without consideration of their actual meaning.

The ability to handle such purely formal problems should then demarcate the emergence of the fourth (and definitive) cognitive structure. My own investigations and observations of mathematical (i.e., supposedly purely formal) reasoning as well as the research of the last 30 years regarding the importance of writing (and, by implication, material representation) for the mastery of abstract thought[26] make me doubt the

[25] This follows clearly from my experience as a teacher of mathematics and physics to grown-up students of engineering or mathematics. Even many of these would make typical pre-operative (centring and irreversible) errors as soon as problems dealt with entities with which they had no familiarity, and which they could not imagine concretely (e.g., sound energy density).

[26] Summarized in [Ong 1982]. If, as it seems, supposedly formal thought can only develop on the basis of writing (or equivalent material representation), there is

validity of the absolute distinction between concrete and formal thinking. The typical mathematician will immediately transform Burt's problem into writing: »E>S, E<L«, turn the first statement around into »S<E«, and combine the two into »S<E<L«. This is no longer a sequence of formal operations but a progression of quite manifest manipulations of visually familiar and thus »concrete« entities, facilitated by an iconic symbol[27]—concreteness being, in fact, no immanent characteristic of the object but a characterization of the attitude of the knowing person to the object (in themselves, birds are no less concrete than primroses).

Regardless of the absolute or only relative character of the transition from concrete to supposedly formal thought, the acquisition of the ability to handle mentally the endless range of problems outside the realm of direct concrete experience (be it by means of formal thought or through the construction of pseudo-concrete representations) is of course an important step; without this step, in particular, scientific knowledge could never be achieved.

Supplementary observations

The characterization of the periods of cognitive development is a skeleton; in order to dress it in flesh and skin one has to make a number of supplementary observations, of which I shall introduce a few—some from within the Piagetian perspective and some from the outside.

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nothing strange in the apparent absence of formal thought from cultures without writing. Writing cultures tend to consider thought based on interiorized writing as »formal« but to see internalization of other material representations as »concrete«. The claim that »primitive man is unable to think formally« (which was current in this or similar forms in the psycho-ethnography of the earlier twentieth century) is thus nothing but a self-promoting reformulation of the quasi-tautological observation that »cultures without writing do not base their thought on writing«.

[27] Not only is the symbol < smaller in the end pointing to the smaller entity; the relation between < and > also corresponds to the spatial reversal in the written line which transforms one relation into the other. Most important perhaps: one < can be located within the other, as ≪, in a way which appeals directly to the sensorimotor experience of putting a smaller box into a larger one and this into a still larger one.

The first observation to be made extends the remark that »children of three are no less sensorimotor effective than they were a year before«. Nor are they at seven, nor at 25. Sensorimotor learning continues, when you learn to ride a bicycle, when you begin driving a car, etc. Certain skills, furthermore, are learned at the level of conscious thought and then absorbed as subconscious sensorimotor skills (e.g., changing gears in a car, or binding the bow knot of your shoes). To some extent, and increasingly, the basic or primitive cognitive structures are integrated with the higher (conscious) structures and made subservient to these (as we shall see in a moment, this integration is far from complete and no unproblematic process).

Cognitive structures were totalities integrating many schemes characterized by shared basic features and limitations. Even when broadened so as to encompass bicycle and car riding, sensorimotor cognition is thus different, and distinct, from operatory cognition. None the less, schemes may in some sense be transferred from one level or structure to another—or, perhaps with a better metaphor, serve as models for the construction of analogous schemes at new levels. Such a suggestion is made occasionally by Piaget (e.g., [1967: 25f, 48]). It is indeed a characteristic of operatory thought that it achieves at the level of thought the same decentration, reversibility and composability which was achieved at the sensorimotor level during the second year of life. The process could be formulated as an assimilation of verbal and nonverbal thought to sensorimotor schemes[28]. Correspondingly, the characteristic schemes of formal thought could be seen as an assimilation of sentences to schemes used to deal with concrete objects (if it is at all justified to distinguish these two levels, cf. above).

[28] The schemes of which Piaget speaks in this connection can be characterized in a broad sense as »logical schemes«: the fact that there are more flowers than primroses does not depend on the specific nature of the two categories but only on the fact that the first category encompasses the second without being itself exhausted. It might be worthwhile investigating (via the »spontaneous grammar« of young children, see e.g. [Bickerton 1983]) whether the development of grammatical schemes can also be correlated with the preexisting structure of sensorimotor schemes.

A different type of observation concerns the status of Piaget's favourite discriminative experiment: *conservation* (the clay sausage and its kin). The understanding of conservation is more or less taken by Piaget to be *the essential content* of operatory thought. Cross-cultural studies makes this doubtful. Australian aboriginal children are indeed unable to master conservation at an age where European children do so; use of maps, however, which also requires operatory thought and only comes later to European children, comes within their range at the age when European children grasp conservation[29]. It seems as if at least operatory thought is a rather open-ended potentiality, the precise actualization of which depends very much on that cultural practice which brings one or the other kind of mental operation to the fore.

A final cluster of observations concerns the mutual relation between the different co-existing cognitive structures. Firstly there is the phenomenon of *cognitive regression*. It was explained above how operatory structures only assimilate unfamiliar domains gradually, as they become familiar. Familiarity, however, is only one factor. Engineering students are more likely to fall back on intuitive thought at exams than during daily teaching. This recourse to basic cognitive structures is a fairly common consequence of emotional stress, and can be observed on many occasions—in heated discussions, when you try to find by systematic trial-and-error the code of an unknown bicycle lock and get nervous, etc. In the history of science, repeated examples of elementary blunders committed by eminent minds approaching the borders of their understanding can be listed.

The choice of cognitive structure to apply in a given situation is indeed no conscious decision. Nor are, in fact, the schemes themselves. This is of course true in the case of the infant applying the ball scheme or constructing a grammatical form »wented«; but it is equally true of the daily thinking of the professional philosopher. He, of course, may notice that his conclusions follow from a particular syllogistic scheme, like the classical »All men are mortal; Socrates is a man; thus Socrates is mortal«. But he needs no scheme to know it, and he knows it immediately before

[29] See [Cole and Scribner 1974: 152f].

correlating it with the scheme. The schemes inside which our conscious thinking takes place are themselves unconscious (cf. [Piaget 1973: 31-48]).

This holds in general, and normally we do not think about it. At times, however, the complex interplay between that which we are aware of and that which does not come to awareness may produce paradoxical errors and failure to grasp correctly what one is able to do correctly in practice. As an example of this »cognitive repression« I quote Piaget:

> A child is given a sling in its simplest form: a ball attached to a string which is whirled, then aimed at a goal. At first, there is no goal whatever and the child enjoys whirling the ball at the end of the string and then letting it go, noting that it flies off from his side (and in general even seeing that it flies off in the extension of the rotary direction). Next a box is placed thirty to fifty centimeters away and the child, often as early as five years old, quickly manages to reach the box by whirling the ball from his side (about nine o'clock, if we consider as clock dial the rotation surface, the box itself being placed at noon). Having done so, the child is complimented; he begins again several times and is asked where he has released the ball.
>
> A strange reaction then occurs. The youngest children claim that they released the balls exactly in front of them (about six o'clock) and that the ball left in a straight line, from six o'clock to noon (the diameter of the rotary circle) into the box. Others (children aged seven to eight) claim that they released the ball at noon, that is, facing the box. About the ages of nine to ten, there are often compromises: The ball is released about eleven or ten-thirty, and it is only about the age of eleven or twelve that the child replies at once that the ball left at nine o'clock, that is, tangentially and no longer facing the goal. In other words, the child soon knows how to accomplish a successful action, but years are needed before he becomes aware of this, as if some factor were opposed to this knowledge and retained in the unconscious certain movements or even certain intentional parts of successful behavior.
>
> The factor behind inhibition is easy to discover. The child represents his own action as divided into two periods: spinning the ball, then throwing it into the box; whereas without this goal he throws the moving object anywhere. But, for him, throwing to the goal supposes a perpendicular trajectory to the box, thus a release facing it. When asked to describe his action, he thus reconstructs it logically as a function of this preconceived idea and hence does not wish to see that actually he proceeded differently. Therefore he distorts and even dismisses an observation contrary to the idea he has and which alone seems right to him.
>
> [Piaget 1973: 36ff]

At the sensorimotor level, the child thus knows correctly what to do. But this correct knowledge is not brought to awareness because it disagrees with a pre-existing conceptual scheme which reconstructs the process wrongly; one might add that even the correct awareness of the oldest children is probably the outcome of a reconstruction—albeit a better one—and not of immediate observation. The path leading from sensory perception and even action to recorded observation is far from direct[30].

Most often, indeed, awareness only results from challenge or conflict. If a wrong reconstruction is used as the basis for the planning of further action one may be forced to recognize that something is wrong (because the sling ends up in a wrong place), and then be led to better understanding. When the activity in question is unproblematic and everything functions as expected, awareness is superfluous and need not arise. We are normally quite unaware of the precise mechanisms of walking, and we have no reasons to produce awareness: walking can be integrated without that in consciously planned movement, e.g. according to a map. It is only when the terrain is utterly difficult, if one of our feet is severely hurt, or in similar situations, that we are forced to focus awareness on the actions involved in walking.

The moral of this observation is of extreme importance for any theory of knowledge. Since theories are themselves products of awareness and addressed to conscious awareness, they tend quite naturally to identify knowledge with conscious knowledge. But the larger parts of our know-

[30] This does not hold for children alone. Once Piaget's collaborator A. Papert had investigated whether children are able to tell afterwards what they did when walking on all four. The result was that the youngest provided a physically rather impossible explanation (movement »in Z«, first the arms are moved, then the legs, ...). Somewhat older children would provide a physically possible explanation which did not agree with that they had actually done (»in N«, both left limbs, both right limbs, ...; obviously copied from ordinary walking). Only the oldest children tended to produce a correct description of what they had actually done (movement »in X«) [Piaget 1976: 1-11].

Before they presented this result to an interdisciplinary symposium, Piaget and Papert had the participants walk on all four and then asked them to describe in writing what they had done. According to Piaget's account, physicists and psychologists tended to give the correct description—while logicians and mathematicians gave the physically possible but actually wrong description [Piaget 1973: 41].

ledge—firstly the basic schemes, but secondly even many of the actions performed within these schemes—are unconscious, and as much as possible remains so as long as unawareness gives rise to no problems. Many activities, moreover, which are learnt at the conscious level are removed from consciousness by repetition and training (training is indeed *intentional* removal from consciousness)—changing gears and shoe-binding were mentioned above as examples. This is a question of simple economy. Conscious awareness can keep track of only a few processes at a time (and it does so relatively slowly); it therefore has to be reserved for these aspects of our actions which can *not* be accomplished automatically; it is also easily distracted and thus more likely to commit errors than automatically performed routines.

A theory of scientific knowledge which does not take that into account can be reproached of neglecting the important fact that even scientific knowledge is human knowledge. It is inadequate, *either* by being unable to understand the economy and ease by which even scientific cognition works through the automatization of sub-procedures; *or* by copying the incomplete awareness of the acting individuals, not recognizing the pertinence (or the very existence) of those procedures which are automated. In both cases, it also fails to explain the errors to which automatization gives rise on certain critical occasions.

The status of schemes and categories

These are questions to which we shall return (p. 76 onwards). At first, however, we shall have to close the discussion of individual cognition.

Kant, we remember, held certain categories to be inescapable frameworks without which we cannot know; Piaget, on his part, investigated how these categories arise, and demonstrated that at one moment in life these categories and the schemes in which they are organized were not yet part of our cognitive equipment; they were thus not only *not* inescapable but in fact inaccessible. At this moment in life we gathered and organized our experience in other ways. The Kantian equipment is only a preliminary outcome and not the starting point of our knowing about the world.

But why do we end up with precisely these categories? Might suitably planned education have produced a cognitive structure not based (among other things) on the category of *permanent objects*, on the expectation that a toy which has been concealed must still *be* somewhere, and that a rabbit drawn out of the magician's sleeves must have been hidden somewhere? Might our world have become one possessing no fixed boundaries between any *this* and any *that*, a world ever fluid and elusive?

This other world is difficult to describe, and for good reasons: it is not our world and not the world to which our concepts and our language are adapted. Are then the separation of our world into objects, our logical schemes and categories simply an implicit message we could not avoid when learning our language and learning to describe our world in language?

This is certainly not completely false, as illustrated by a cross-linguistic example. Chinese does not allow the enunciation of counterfactual statements like »if printing had not been invented, then the industrial revolution could not have occurred« but only approximate equivalents translatable into »Printing has not been invented, and therefore the industrial revolution has not occurred«. In a test where English-speaking subjects are asked for a conclusion from the first formulation combined with the statement »The industrial revolution has occurred«, they will have little difficulty in deciding that printing has been invented. Chinese, confronted with their equivalent, are likely to protest that »Printing has been invented« and thus not to accept the game. Bilingual Taiwanese Chinese, moreover, tend to react »in English« when asked the test in English and »in Chinese« when asked in Chinese [Bloom 1979]. Without adequate support in language (or other symbolizations), higher logical schemes are not easily accessible.

But most of the fundamental categories develop as practical categories before language is acquired, and others (like the girls-children scheme) may be present in language but systematically misunderstood until the moment where the cognitive structure is ready to use the linguistic structure as mature speakers do. Furthermore, basic categories like the permanent and separate *object* are common to all languages, which would hardly be the case if language itself was not constrained in this domain.

As far as the basic categories are concerned, language can hardly be more than a secondary regulatory factor, a support which stabilizes the incipient formation of individual categories and structures by lending them adequate symbolic expression.

This leaves us with two possibilities. The categories may be determined by *our* perceptual and nervous apparatus; that they develop may then reflect that this apparatus is not fully evolved when we are born but needs to go through a *process of maturation*. Or they may really correspond to the structure of the world in which we live; their development then reflects a *process of discovery*.

None of the two explanations taken alone is satisfactory. If nothing but maturation is involved, why should children in the Iranian countryside acquire operatory structures at a later age than urban children from the same country while being no less intelligent according to tests which do not require operatory thought (see note 15)? And why should Australian aboriginals develop operatory structures in an order which differs from that of Swiss children? On the other hand, whatever the structures in which the world around us is ordered, *we* would not be able to adopt them into our cognitive structure if we were not in possession of an adequate nervous system.

This can be illustrated by another reference to the category of permanent objects (I rely for this on [Jerison 1973] and [*id.* 1976]). Frogs and other more primitive vertebrates possess no scheme for the permanent object; frogs jump at the direct perception of visual signals (e.g., representing flies). A frog whose eyes have been turned around in a surgical operation will jump in a wrong direction for the rest of its life. Early mammals, however, who hunted their prey at night and had to rely on the integration of sound impressions over time in order to construct the path of the prey, developed a larger brain and a scheme for permanent objects—more precisely: a larger brain which allowed them to organize their sensual impressions as representations of permanent objects.

The larger brain with which mammals (and birds) are born has large biological costs. Birds and mammals are born immature and defenseless, and would never survive if their parents left them to themselves as crocodiles do with their offspring (mammals indeed *became* mammals as a way to take care of their litter). If the possibility to experience the outer world as consisting of permanent objects had not implied definite advantages which could balance this cost, selection pressure would soon have eliminated the larger mammalian brain. We cannot conclude that the world *consists of* permanent objects. In fact, it does not in any absolute sense: the fox pursued by the hounds exhales and transpires, and matter which in one moment is fox is not a moment later; physics tells us that we may analyze the fox into atoms and into still smaller particles. What we *can* conclude is that the material world is constituted in a way that allows an adequate practice if we order our perceptions as representations of permanent objects. Only in this sense can we say that the world in which we live »is« itself structured in permanent objects.

Mutatis mutandis, the same will hold for other fundamental cognitive categories and schemes. Evidently, the evolutionary level at which they have evolved will be different, and operatory thought appears only to have arisen during the process of hominization.

III. THE NATURE AND DEMARCATION OF SCIENTIFIC KNOWLEDGE

Knowledge is always known by somebody, if not actually then at least before it was stored in books or other receptacles; and stored knowledge remains knowledge only in so far as it can reemerge as the knowledge of somebody. Therefore, all knowledge partakes somehow in the characteristics of individual knowledge.

But part of what we know is *only individual* knowledge; part of what we know is *only accidentally shared* by groups of people who happen, e.g., to be witnesses of the same events; and part of our knowledge is *produced so as to be communicated and shared*.

Part of what we know, furthermore, concerns *particular occurrences*—e.g., the rainy weather in Copenhagen in this moment; another part is of a *more general character:* describing, e.g., the Danish climate or the dynamics of cloud formation and movement. Some of it, finally, consists of *isolated bits*, while other parts are *built up as wholes* (»theories« and the like) whose single parts only obtain their full meaning as parts of these wholes.

Scientific knowledge is produced so as to be communicated and shared in stored form; it is generalizing in character; and it consists of wholes. It is, furthermore, produced by communities with a strong internal interaction by means (*inter alia*) of stored knowledge (books, scientific journals, letters). For all these reasons, scientific knowledge possesses particular qualities.

Some of these concern the relations between producers, users and (they should not be forgotten) victims of the knowledge in question, and the links between its production and its social and technical uses. Others (and they are the theme of the present chapter) concern more purely epistemological questions. For instance: Is scientific knowledge true? If it is, in what sense and under which conditions? If not, why do we then rely upon it, so often with considerable technical success? Etc.

Since scientific knowledge normally ends up as stored knowledge, traditional philosophy of science has approached these epistemological questions with regard to the stored form of knowledge. In later decennia, however, certain workers in the field have insisted (against strong opposition from traditionalists) that genuine understanding of scientific knowledge (including understanding of the stored final phase) can only be achieved if we understand its original *emergence in process* as individual knowledge generated within a field of specific social interaction.

Chapters IV and V will take up the latter approach. In the present chapter I shall introduce some fundamental concepts and terms and look at some of the established approaches and their problems.

A pseudo-historical introduction to some key concepts

An Ancient anecdote[31] reported in [Høyrup 1993: 5] may be repeated with profit together with some of the commentaries made there. It runs as follows:

> One day Plato the philosopher met his fellow philosopher Diogenes, who, as so often, made a teasing comment on Plato's philosophy. »Good Friend«, he said, »Table and cup I see; but your tablehood and cuphood, Plato, I can nowhere see.« »That's readily accounted for, Dear Diogenes«, replied the other. »You have that which is needed to see the table and the cup: that's the eyes. But you lack what is required to grasp tablehood and cuphood: namely the intellect«.

This story locates the two philosophers with regard to a number of the essential concepts and problems of established philosophy of science:

Plato is an *idealist*. That is, to him things exist primarily *as knowledge or thought*, in a mind, as concepts or *ideas* (in Greek: that which is seen), whence the term. The single material representatives of the concepts—the tables as representatives of the tablehood, the tragedy *Medea* as a representative of *THE TRAGEDY* as a genre—are precisely that: representatives *depicting* genuine reality[32].

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[31] Told by Diogenes Laërtios in his *Lives of Eminent Philosophers* VI, Chapter 53; slightly paraphrased here from [Hicks 1980: II,55].

The characterization of the present section as »pseudo-historical« should be emphasized. It does not present the points of view of Ancient philosophers in the context of their total thinking (and still less in the general context of their times); nor is it faithful to the real complexity of their ideas. It is rather a rash exploitation of the historical material, intended to procure a pedagogical introduction to a number of themes which have stayed important in the philosophy of science ever since Antiquity.

Expositions of the views of the Ancient (and later) philosophers which are somewhat more faithful to complexities and context can be found in [Losee 1972].

[32] In order to do Plato justice it should be said that the »tablehood« and »cuphood« of the anecdote are parodic distortions of Plato's real doctrine, which is concerned with »ideas« like *Courage* and *the Good*, modelled on that *Triangle* which is the real object of mathematical proofs even when a particular triangle has to be drawn for the proof.

Nevertheless, the parody has a point in itself, calling attention to an inner problem or even inconsistency in the Platonic and similar doctrines. Some geometrical proofs concern *the Triangle*, others only *the Right Triangle*, still others

Diogenes on the other hand is a *materialist:* only tables and tragedies exist; Tablehood and Tragedy—the ideas—are *our* inventions. He is also a *positivist:* we must distinguish that *positive* knowledge which we get from observation of the real world from those figments of the mind which we add ourselves—interpretive frameworks, concepts, generalizations and metaphysical entities (nobody ever saw the *force of gravitation*, an *animal* which was not a particular individual belonging to a particular species, *justice*, or *the tragic dimension of human existence).*

With regard to another dichotomy, Plato can be characterized as a *realist*[33]: according to him, the universals (general concepts, ideas) possess the status of something really existing. Plato is indeed an *objective* idealist. The ideas of tablehood etc. do not have their fundamental existence as images in our individual intellects—these are merely our only access to that higher Universal or Divine Mind where they have their real abode. Diogenes, on the other hand, is a *nominalist,* and regards the universals as nothing but *names* (puffs of the voice, as the nominalists of the Late Medieval universities used to say) which *we* invent and use as shorthands to sum up collections of particular experiences or objects.

As one may perhaps guess from the expression »objective idealism«, another, »subjective« idealism exists. Much modern positivism is of this breed. According to subjective idealism, every reference to an external reality is ultimately nonsense: our mind has access to and registers nothing but our *sense impressions.* These form a forever impenetrable screen between our mind and anything which may lay

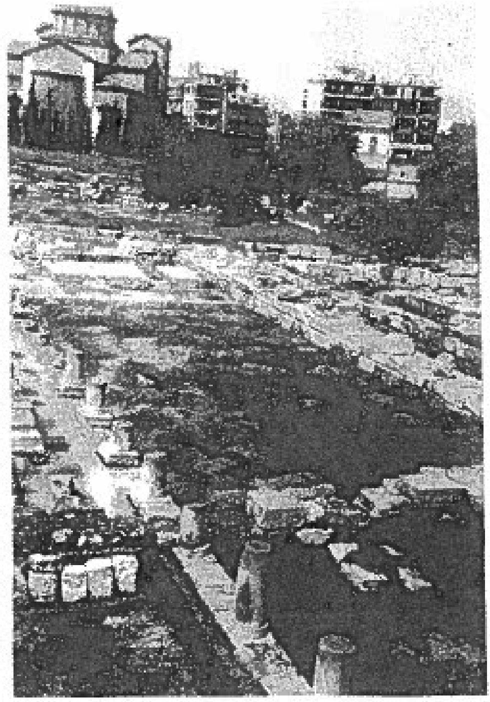
all regular polygons. Does this mean that there exists a particular idea of the Right Triangle, and which are then its relations to the idea of the Triangle. Does, in a similar way, the idea of the Artefact (or Material Object) split up into sub-ideas for tables, cups, ...? When taken to this consequence, the simple structure of the universe of ideas dissolves into an indefinite and virtually infinite number of nested and intertwined sub-, super- and interwoven ideas.

[33] In later years, it is true, the traditional term has been taken in a very different sense, grossly corresponding to materialism: The outer world exists, and our knowledge is about this. To avoid ambiguities we may then speak of Plato's attitude as an instance of *concept realism,* the doctrine according to which concepts correspond to really existing entities.

The reason for this rather confusing linguistic innovation appears to be political: Since the term »materialism« has mainly been used by Marxists since the late nineteenth century, you'd better avoid it!

behind.[34]

At this point, we have probably exhausted the philosophical contents of the anecdote. In order to complete the list of essential concepts we will have to involve a third Ancient philosopher: Plato's rebellious follower Aristotle.



If the dialogue between Plato and Diogenes ever took place (which in itself is not very likely), then this is the most likely location: In the left of the picture we seen the remains of a portico where Diogenes spent much of his time. To the right the road from Athens to Plato's *Academy*.

Aristotle was an *empiricist*. If Aristotle is to be believed, all knowledge derives from experience and thus comes through our senses. The empiricist attitude has been summed up in the *dictum* that »nothing is in the intellect which was not first in the senses«. But Aristotle was no positivist, and no nominalist. He held that *both* the particulars (the single tables, the single tragedies) and the universals (the Tablehood and the Tragedy) existed—the latter as *essential* characteristics objectively shared by the directly observable tables and tragedies, respectively. The uni-

[34] The ultimate consequence of this principle is *solipsism* (from *solus ipse*, »oneself alone«), the tenet that one's own mind is the only thing of whose existence one may be sure—everything external, including other persons and their minds, may be nothing but dreams and imagination (however these are to be understood without being contrasted with perception of the real world). No philosopher likes being considered a solipsist, for which reason a variety of devices have been invented by subjective idealists. But if the same strict logic is applied to these inventions that made their inventors accept the sense impressions as an impenetrable screen, they evaporate immediately.

versals constitute the *essence* which lays behind and determines the *phenomena*, that which *appears to* and can thus be *observed by* our senses.

But Aristotle was no idealist of the Platonic ilk. He did not regard the *forms* (a term which he came to prefer to Plato's *ideas*) as the only really existing entities, and the phenomena as merely fleeting and ephemeral, imprecise and ultimately non-existing representatives. The Tablehood is not something existing besides and above the particular tables; *Tablehood* only exists *as tables*—the essence is only there *in* phenomena. Aristotle is thus a materialist—but he is no positivist. With regard to the dichotomy between *realism* and *nominalism*, he has been labelled a *moderate realist*. Diogenes represents a flat rejection of Plato's point of view—an *antithesis*; Aristotle, on the other hand, can be regarded as integrating and going beyond both positions, producing a genuine *synthesis*.

According to Aristotelian empiricism, it is the aim of scientific investigation to determine the essence of things through observation of phenomena. The method is *induction:* Examination of many horses allows us to find that which by necessity will hold for anything that is *horse*—i.e., to find the *essence* of the horse; examination of the constitutional history of many city states will lead us forward to knowledge of the essence of the state; etc.

When induction has thus provided us with the essential truths concerning a field, we may deduce through the applications of logic alone what must necessarily hold for particular phenomena without further empirical observation. If it has been shown to be part of the essence of cows to have four stomachs, we need only establish that Karoline is a cow in order to know that she got four stomachs.

Once again, *mathematics*, and in particular geometry, supplies the model for this epistemology, as it is made amply clear by the choice of illustrative examples in Aristotle's main work on the philosophy of science, the *Posterior Analytics*. From thorough investigation of the properties of geometrical figures we establish a number of fundamental truths (»axioms«) concerning ideal points, lines and figures in the geometrical plane; from these we may deduct, e.g., that the sum of the angles of a triangle equals two right angles (i.e., 180°). When that is done, we only need to establish that a given figure is a triangle, i.e., contained by three straight lines, in order to know the sum of its angles; empirical measurement is no longer necessary.

In Antiquity already it was objected to Aristotle's methodology and epistemology that induction is a logical impossibility. Even if we have dissected 10 000 cows, we can never be absolutely sure that N° 10 001 possesses the same number of stomachs as the others. Couldn't Karoline belong to that fraction of a percent which has only three stomachs? Or perhaps be *the* exception? To accept four stomachs as an aspect of the essence of the cow can be nothing but an inspired guess, the validity of which is never absolutely guaranteed[35].

This point of view is classified as *scepticism*. According to the sceptical point of view, science can never establish necessary truths concerning the essence of phenomena, only plausible truths[36]. It will be seen that scepticism, nominalism and positivism are related philosophies: if we cannot *know* whether we have penetrated to the essence of things (scepticism), then is it close at hand to claim that our preliminary concepts about the structure of the world are mere abbreviations and names which *we* have invented for convenience (nominalism)—and it is tempting to regard all discussion of a not directly observable essence behind observable

[35] Alternatively, we may of course take four stomachs as part of the *definition* of a cow. But then we shall only know whether Karoline is a cow when cutting her up. If we want to know about empirical reality, definitions do not solve our problem.

This is the point of another Plato-Diogenes anecdote: When Plato had defined *Man* as a biped and featherless animal, Diogenes plucked a fowl and brought it into the lecture room with the words, »Here is Plato's man«. In consequence of which it was added, »having broad nails« (Diogenes Laërtios, *Lives* ... VI.53, ed., trans. [Hicks 1980: II, 55]).

[36] One may also speak of »probable truths«, as often done. But if so, then only in a loose sense: logically seen, observation is just as unable to establish that it is »more than 90% sure that all cows have four stomachs« as it is to establish a *100% necessary* truth. Strictly speaking, talking of quantified probabilities in such cases is pure nonsense, since it tells that »in nine out of ten worlds, all cows have 4 stomachs«—there *is* only one world, as far as we can know.

This observation may seem trivial. None the less, it is often overlooked in practice. The conclusion of, e.g., a medical double blind experiment is frequently explained to be that »it is 95% sure that this new drug works«; but what *should* be said is that there is only a 5% chance that anything as suggestive as our actual outcome would occur accidentally if the drug has no effect.

phenomena as metaphysical rubbish contributing nothing to the real process of knowing (positivism).

According to the Ancients, scientific knowledge consists (roughly speaking) in finding the *objects* which exist within this world, and in listing their properties and characteristics (in technical language: it consists in establishing an *ontology*). To them, the essences of things could thus be listed as *objects and properties*. Modern sciences, on the other hand, also look for relations, structures, and dynamical interactions. In the perspective of modern sciences, *essences* should thus involve these categories (even though they rarely use the term *essence*, which smacks too much of pre-Modern thinking)[37]. None the less, and irrespective of terminology and the exact character of general/universal features, the old questions remain whether universals have any status within reality. *Mutatis mutandis*, the concepts of objective and subjective idealism, materialism, empiricism, positivism, nominalism and scepticism apply as much in the modern as in the Ancient world.

Empiricism and falsificationism

These pages are not written with the intention of summing up however superficially the history of philosophy. The preceding section—let it be stressed once more—was only meant to serve as a pedagogical framework for the presentation of certain important concepts and views. The present section will therefore leave pseudo-history (and step a bit outside the confines of professional philosophy) and look at certain attitudes to the

[37] Evidently, modern sciences *also* encounter the question of existing objects or entities—linguistics, e.g., what kind of existence to ascribe to the phoneme /t/. The status of sounds may be left to physicists, and so may also the specific *t*-sounds in *ten* and *steam* (the first of which is aspirated and the second not—cf. [R. H. Robins 1971: 122]). Linguists, however, who notice that the first of these sounds [t^h] cannot occur *in English* in places where the second is possible, and vice versa, speak of the two sounds as representing the same phoneme /t/. No physical analysis (whether a frequency analysis of the sound or a description of the sound production process) can reduce the two to one, since phonemic identity is language-specific—the Danish *r*-sounds in *arbejde* and *rede* (one of which is vowelized and the other not) represent the same phoneme; in Arabic, the former would be heard as representing the initial phoneme /ʿ/ of *ʿAlī*, and the second the phoneme /ġ/ used in *Baġdād*. No other science can decide for linguistics whether, and in what sense, phonemes exist—in the speakers' minds or expectations, in some objective structure of language, or as linguists' shorthand or abstractions.

production of scientific knowledge which are widespread today even among those who take part in this production.

One of these attitudes is nothing but Good Old Empiricism, coined as a rule of conduct rather than as a stringent philosophy. Formulated in maxims it runs as follows:

> *Scientific explanations are only allowed to make use of concepts and to postulate relations and structures which can be rooted in experience, observation or experiment. Mythological explanations referring to entities with no such empirical underpinning are inadmissible: they only obstruct genuine scientific insight.*

This programme contains some essential points—first of all that science should deal with reality and not be spun out of pure imagination. It also corresponds to the immediate impression which working scientists get from most of their work: we simply describe (so we feel) what we find, including the relations and structures which turn up in our material. But it does not avoid fundamental problems.

Traditionally, the whole complex of logic and mathematics is regarded as falling outside the domain regulated by the empiricist rule. The statement

> *if I know that rain makes the street humid, and that it is raining, then I can conclude that the street will be humid*

appears to be unproblematically and unconditionally true—expressed in the appropriate language it will have to be accepted by a Touareg in Sahara knowing neither streets nor rain. It is a *tautology*, a statement which is true because of its logical structure.

That difficulty is normally solved by means of a distinction (going back to Kant) between two kinds of scientific statements: *Synthetic* statements which deal with reality, and for which the empiricist claim must be upheld if they are to be meaningful; and *analytic* statements (the theorems of logic and mathematics), the truth of which follows from their own structure, but which on the other hand tell us nothing about the structure and properties of *reality*[38].

According to this distinction, the truth of analytic statements is given *a priori*, i.e., *in advance* (*viz*, in advance of experience). The truth of synthetic statements only follows *a posteriori*, i.e., *after* experience.

Kantian philosophy, it should be noted, accepts the analytic and *a priori* character of logical tautologies, the truth of which follows from definitions—»all husbands are male«, if we define a »husband« as »the male part of a married couple«. Likewise, it accepts the synthetic and *a posteriori* character of normal descriptive statements. But »2+2=4« is, according to Kant, a *synthetic a priori:* It cannot be false, whence it is *a priori;* but none the less it tells us something about reality (e.g., that two married couples are exactly what you need for playing bridge). The same holds for all those categories which are *a priori* necessary for making experience: Space, time, causality, etc. Precisely because we cannot know reality without making use of these frameworks, knowledge about the frameworks by itself tells us something about *our* reality,—namely, *the only way in which we can deal with reality.*

As it will be remembered from the previous chapter, Piaget started out to find the roots of the Kantian *a priori* categories, demonstrating that they are in fact the outcome of a genetic process, and hence not really *a priori*. Since they result from practical experience in the world (experience which is made in interaction between our biologically determined equipment *and* the outer world), they also seem to be *synthetic* though dealing with the most general properties of reality, properties which reveal themselves in any environment in which children are brought up. Piaget's results thus appear to imply a reduction of the classification of statements into a new dichotomy: Analytic *a priori*, which only encompass tautologies by definition; and synthetic *a posteriori*, which embrace not only normal descriptive sentences but even the theorems of logic and mathematics.

Certain empiricist philosophers (e.g., Willard Quine [1963/1951]) make the point that words can never be fully reduced to simple definitions (what, e.g., happens to the concept of »husbands« in recent Danish matrimonial legislation?). This would

[38] Einstein once summed up this distinction and its consequences in a nice aphorism: In so far as mathematics consists of certain truths it tells us nothing about reality; in so far as it informs us about reality it is not certain truth (approximate quotation from memory).

A remark on terminology may be useful: the English language allows a distinction between »analytic« and »analytical«. The former adjective characterizes statements whose truth follows from their structure; the second means »using analysis«. »Analytic« *may* also be used in the second sense, but the present essay takes advantage of the possibility to distinguish, and observes a similar distinction between »synthetic« and »synthetical«).

abolish the category of analytic statements definitively. Ultimately *all* statements would have to be judged in the light of the »empiricist imperative« formulated above; the apparent exceptions will have resulted from a too naïve understanding of logic and mathematics.

Closer investigation of the empiricist program reveals more severe difficulties that those which can be circumvented by the segregation of logic and mathematics or by arguments that after all they constitute only relative exceptions.

Firstly, experience is never completely *pure*, i.e., made before and independently of every theory; if this was not clear before the invention of the telescope and the microscope, it should at least be glaringly obvious to anybody looking at the technical equipment of a modern scientific laboratory. The cell biologist presupposes the functioning of contrast media and microscopes and thus theories belonging within chemistry and optics; the literary scholar and theatre critic watching *Medea* knows beforehand that what goes on is a play (in any other case he would be morally obliged to save Medea's children); etc. Experience and theories are elements of a network, in which certain elements can be regarded as more elementary than others and less subject to doubt; they can therefore be regarded as legitimate underpinning and background. But it is impossible to make a sharp cut between *empirical knowledge* (constituting a theory-free background) and *theories* derived from this background knowledge, as required by the empiricist ideal.

Secondly, the scepticist argument remains valid. You can never, however great the amount of your experience and the number of your experiments and observations, derive theoretical generalizations with absolute logical necessity. All your acquaintance with and examination of winged feathered beings will not force you to bring forth the concept of *birds;* reading all the books written to date and listening to all the monologues and dialogues of history will not by necessity make you discover a *deep structure* in language. Even if we forget for a moment about the impossibility to obtain purely empirical knowledge, we are forced to conclude that »science« created in agreement with the empiricist letter will be restricted to listings and descriptions of singular cases, and perhaps to tables and statistics—»natural history« instead of biology. *Theory* will never emerge.

Ringleader in the statement of twentieth century empiricism was the so-called »Vienna circle« and a number of associates, who in the 1920s formulated the programme of »logical empiricism« (by others often labelled neo-positivism)[39]. Ringleader in the destruction of the programme was the same group, astonishing as that may seem: Over decennia this school tried off strategies for a complete empirical underpinning of scientific statements, searching for a *verification criterion* by means of which precisely those statements might be singled out which can be founded upon and proved from experience, i.e., *verified*, from all those which could not be verified and which were therefore scientifically meaningless. Their work never produced a verification criterion which could demarcate science from non-science precisely, but instead a triple negative conclusion. Firstly, *existing science* cannot be reconstructed in this way, i.e., reduced to a system of single statements which directly or indirectly have their complete reference to experience; secondly, *no science* embracing theoretical statements can be constructed in agreement with the prescriptions; thirdly, *no* verification criterion can be found which distinguishes sharply between empirically meaningful and empirically empty statements[40].

As an alternative to the untenable demarcation by verification, the Austro-English philosopher Karl Popper has proposed his own recipe. In »naïve« formulation, and thus corresponding to the above »empiricist imperative«, it can be summed up as follows:

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[39] »Logical« because of the way in which it tried to get behind some of the vagueness of classical empiricism, including the »empiricist imperative« formulated in these notes: Science does not build directly on experience—perceiving something and enunciating a theory belong at different levels of existence. But experience has to be formulated in simple sentences which state what has been experienced (»protocol statements«, since these are to be entered into the experimental protocol of the scientist). In agreement with the rules of logic, these statements are then to be combined into higher level statements (generalizations, »laws«, »theories«, etc.).

[40] The whole argument, together with the arguments against the existence of genuinely analytic statements, is given in Quine [1963/1951].

We are allowed to use in our explanations whatever self-invented concepts and hypotheses we like; but we should be aware that our hypotheses are indeed nothing but hypotheses, preliminary explanatory models, and not the truth. We should therefore constantly check our hypotheses as thoroughly as we can, and we must reject them as useless as soon as they enter into conflict with our observations of reality—i.e., as soon as they are »falsified«.

Popper did not invent this canon. In the main, it coincides with the idea of the »hypothetical-deductive method«, which has been practiced for centuries[41]: Instead of working our way inductively from particular observations in order to find the inner regularities and laws behind phenomena (their »essence« in the classical language), we guess at a set of laws or relations and try to deduce predictions about how things will behave if our guess is correct. If the prediction turns out to be correct, the guess is regarded as strengthened or »corroborated«; if not, we try another guess. Nor is Popper's stress on the forever preliminary character even of corroborated hypotheses original; already the American philosopher Ch. S. Peirce emphasized that new counter-evidence may always turn up, and labelled this idea *fallibilism*[42].

Popper's fundamental idea is thus less original than his own writings try to tell. Nevertheless he must be credited with spreading the Gospel, and indeed with *making it a gospel*, to such an extent indeed that Peirce's original term is at times used as a synonym for Popperianism[43]. Writing

[41] E.g., by Newton in his analysis of the planetary system, although he tried to present his results in empiricist garb.

[42] Even Peirce of course has his forerunners. In the *Malleus maleficarum*, a handbook in witch-hunting from 1484, it is taught that the inquisitor should never pronounce any accused innocent however much she might seem so, only declare that no evidence had so far proved her guilt (III.20, ed., trans. [Summers 1971: 241]). Even then, new evidence might always turn up. Nobody should be acquitted, cases should be postponed for want of proof.

[43] But not always! In his discussion of Popper's methodology and aims, Lakatos [1974a: 93-132, in particular 112 and 114] makes Peirce's term cover the sceptical position that any knowledge, including knowledge of presumed facts which are supposed to falsify a theory, may equally well be mistaken. This »sceptical fallibilism« is almost as far from Popper's philosophical inclinations as can be.

in the wake of the Vienna circle (to which he was close in the late 1920s without being a genuine member), whose use of *verification* as the criterion of demarcation between meaningful and empty statements was presented above, he was also the first to use *falsification* as a criterion of demarcation: Statements and theories which are compatible with every imaginable situation (and which can therefore never be falsified) have no place within science[44].

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[44] As examples of such not genuinely scientific theories Popper [1972: 34] refers to »Marx's theory of history, Freud's psycho-analysis, and Alfred Adler's so-called "individual psychology"«. He illustrates this (p. 35)

> by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child).

In contrast, Einstein's General Theory of Relativity would have been »simply refuted« (p. 36) if starlight had not been seen to be bent around the solar disk during the solar eclipse of 1919. According to Popper, this »risk« taken by the theory is what makes physics *scientific*, in contrast to psycho-analysis and historical materialism.

Rhetorical zeal makes Popper forget that his drowning episode is not of the same kind as the eclipse observation. When it comes to describing single aspects of events taken out of context, physics is no different from Popper's aversions. Physics too may explain that a piece of lead flies upwards (it has just been shot out from a gun) and that it falls downward (it was shot upwards 50 seconds ago and is now falling back), or that water evaporates (the kettle stands on an electric boiler) or freezes to ice (somebody cut out the current, the window is open and the weather frosty). This oversight is rather typical of Popper's ways, and may provoke the question why so sloppy a thinker is worth mentioning. The reason is three-fold. Firstly, Popper is not always rhetorical and therefore not always sloppy; secondly, precisely his sloppy thinking has become extremely popular; thirdly and finally, coming to grips with Popper is a useful step in the present argument.

Instrumentalism and truth

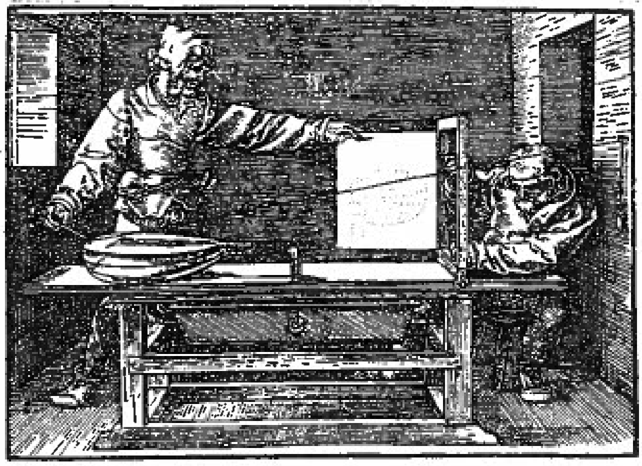
If our explanations are built on arbitrary constructions and remain forever preliminary and subject to rejection at failure, they cannot be »true« in the classical naïve sense—things which may be false tomorrow (because they may then have been falsified) cannot be true today. If theories cannot be claimed to be *true*, however, the best explanation of their role seems to be that they are *tools* for practical action. There is thus a close connection between Popper's ideas and *instrumentalism:* Scientific theories have no truth value, are neither true nor false. Since they *cannot* be judged on the scale of true and false, we should not try to do so. Scientific theories should be evaluated in the way you evaluate instruments, according to effectiveness, e.g. for prediction.

According to instrumentalist tenets, Copernicus' theory, which claims the Earth and all planets to circle around the Sun, is thus neither more nor less true that the Ancient Ptolemaic notion of a fixed Earth around which Sun, Moon, planets and stars move. Both are applicable as *models*, and our only reason to prefer Copernicus' model is that it is simpler and therefore gives rise to less complex calculations if both models are built up with orbits corresponding to empirical observations[45]. Being no more *true* than the alternative, Copernicus' model is to be preferred for a reason that will convince any artisan: It feels better in the hand.

A fundamental objection against the instrumentalist interpretation of scientific statements is this: Instruments can be used for precisely that for which they have been designed; they can be used for other purposes only if their constitution reflects, or corresponds to, *features which are shared* between the intended use and the other possible uses. A screwdriver can be used for many different screws, but only because they all carry a notch; and it can only be used at all because its edge fits the notches and its rotation symmetry corresponds to the rotation by which its target is screwed in. Similarly with theories. We may claim that we judge them according to instrumental value; but *we* cannot invent that value freely, it is revealed (or denied them) when they are applied—»the proof of the pudding is the

[45] Strictly speaking, the Copernican model is only decisively simpler than the Ptolemaic system if we refer to Kepler's revision of the Copernican theory.

eating«. The *applicability* of a tool is thus a consequence of its *correspondence with certain features of reality to which it is applied*—features that themselves are not brought about by the tool. Similarly for theories regarded as tools: their *truth value* can be explained precisely as a *structural agreement* or *correspondence with features* of reality. Further on, this conception will be spoken of as a *materialist notion of truth.* Evidently, as also demonstrated by the example of the screw-driver, *correspondence* is something quite different from *similarity*, not to speak of *identity*: a screwdriver provided with a notch instead of an edge would be worth nothing. In general, theories as well as screwdrivers belong to other categories than the reality described by the theories and the screw to be put in by the screwdrivers. Reality consists, among other things, of atoms, birds, emotional states, and poems (according to physics, biology, psychology, and literary scholarship, respectively). Theories, on the other hand, consist of words and other symbols. Only from an idealist point of view do reality and theory belong to the same realm, both being in some way idea or concept; but then Plato's idealism postulates an absolute



Perspective drawing of the principles of perspective drawing. From Dürer, *Unterweysung der Messung* categorical rift between *real* reality, i.e. the realm of ideas, and apparent, material everyday reality[46].

Instrumentalism is thus right in seeing family likeness between a screwdriver and a theory, and has a good point in its subversion of the metaphysical concept of truth which ultimately presupposes an idealist stance; but it is mistaken in believing that the screwdriver and the theory are alike because they are equally arbitrary with regard to reality. Provocatively speaking we may say that the reason for the usability of the screwdriver is that it possesses a structure which corresponds to certain essential features of the structure and function of screws—it embodies, in materialized form, part of the *truth* about screws.

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[46] That theories »consist of words and other symbols« points to another characteristic which they must possess beyond »structural agreement or correspondence with features of reality« if they are to be considered »true«: logical consistency (or, put differently: words put together without consistency can correspond to nothing beyond themselves, they are meaningless). Much work has been done in twentieth-century formal logic to render precision to this requirement, which practical scientific workers tend to treat no less commonsensically than the idea of »correspondence with facts«. The Polish-American logician Alfred Tarski, in particular, is known for having formulated a »theory of truth« determining the conditions which must be fulfilled by a formal sentence system if it is to possess this logical consistency; he is also known for having shown that attempts to determine the truth or falseness of the sentences of such a system from within the system itself lead to self-referential paradoxes of the type »this statement is false«. Truth has to be ascribed from without, by a metalanguage.

Sciences are not written in formal but in technical languages which ultimately derive from common daily language. None the less, Tarski's latter observation is important for understanding the difficulty with which we are presented when we try to understand the nature of the »correspondence« between sentences (or theories) and reality. If correspondence is revealed through interaction with reality (»praxis« in a Marxist sense) functioning in the role of the metalanguage, then it can not be discussed within the quasi-formal discourse of logical theory but only in a (genuine) metalanguage which steps outside: a metaphorical language which evokes for us *our experience* of such interaction—e.g. the above screw-driver. In another metaphor: Perspective drawing is a way to render three-dimensional reality in a two-dimensional plane. How you make a perspective drawing can be shown in a perspective drawing (see the Figure)—but it is only because we know about three-dimensional reality and move around in it that we give the right interpretation to the Dürer's woodcut, and that we see the present drawing as a cube and not as a plane jigsaw: 

One decisive difference remains between the screwdriver and scientific theory. The instrumental validity of the screwdriver is static and limited; science, on the contrary, is in continual development, constantly searching for new correlations and ever extending so as to grasp new phenomena. Kepler's Copernican cosmology is more true than Ptolemy's planetary system because it allows a unified treatment of celestial and terrestrial physics (until Kepler, the heavenly bodies were supposed to move by necessity according to other laws than those which governed movement below the sphere of the Moon)[47]. By saying that the *reason for* the usability of theories is that they reflect features of reality, we also claim that *reality carries objective features* which can be reflected by theory—»objective« in the sense that they are contained *in the object*, in that reality which is studied. The assertion that theories are better (»more true«, as just said concerning Kepler) if their range can be extended implies that the objective features carried by reality are of general validity, that *reality is coherent*, i.e., *potentially one*[48].

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[47] This extendibility is essential if we want to formulate a truth theory which is relevant for the humanities. Claiming that your interpretation of a Platonic text is *true* because it coincides with Plato's own understanding makes no empirical sense—how do we know that it does? But interpretation of a Platonic text (or any other past text) makes use of techniques which are *also* used in the present—some of them in everyday dialogues with people with whom we share a material practice, some of them in the court-room, where textual evidence is combined with material evidence. If the interpretive techniques which we use on Plato do not function in the communication and together with the material practice and evidence of our own age, we will have to reject them as general tools.

It may be relevant to remember in this connections that the textual criticism of Renaissance humanists, from Petrarch to Casaubon, consisted precisely in the application of techniques used to expose forged juridical documents.

[48] This is another way to approach a question dealt with in the end of Chapter II. Here it was concluded that »the material world is constituted in a way that allows an adequate practice if we order our perceptions as representations of permanent objects«, and it was suggested that other fundamental cognitive categories and schemes had a similar foundation. The same kind of argument applies in the case of scientific knowledge: if the requirement of logical consistency and extensibility works (as it normally does), then this must tell something about the reality that our theories deal with.

But we might continue: the permanency of the fox was not absolute, although we might discuss its shortcomings in term of the same principle (exhalation of air, The affirmation of instrumentalism, that there is no truth, and that we should choose our theories as it fits our aim, ends up by being inverted: No, our choice is not arbitrary and not subjective: The aim of science must be to capture (as far as it is possible in the given moment) as many of the objective features[49] in as general a form as can be done, and thus to be—*in this sense*—as true as possible. Only *then* will our knowledge be instrumental.

This is the real crux of the empiricist imperative as formulated above; this is Aristotle's old programme, to find the essence behind phenomena, but stripped of the belief that any definitive essence can be found once and for all; and it is, for that very reason, the core of the dialectical-materialist understanding of the aims and possibilities of scientific insight.

But still it tells nothing about the ways to attain this scientific insight; let us therefore return from this excursion into the theory of truth to Popper's recommendations in the matter.

Instruments or models?

Occasionally, Popper formulates himself as if he were an instrumentalist[50]. But his fundamental attitude is certainly different. This is made clear by his judgment of people who do not reject a theory when it is (in Popper's opinion) falsified. His rhetoric is that of a preacher denouncing a liar, not of the carpenter censuring a bungler who reaches out for the

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etc.). If other fundamental schemes (including the requirement of logical consistency) are also »biologically *a posteriori*«, we have no guarantee of their absolute validity; we only know them to be *a priori* and hence inescapable in our actual life.

[49] As we shall discuss later on, however, *our* questions to reality determine the *kind* of features that will be reflected. Only our aim of driving screws into the wall makes the edge and the symmetry of the screwdriver relevant—if we wish to use our screws as weight units, a pair of scales would be the relevant instrument.

[50] »The tentative solutions which animals and plants incorporate into their anatomy and their behaviour are biological analogues of theories; and vice versa: theories correspond (as do [...] honeycombs [...] and spiders' webs) to endosomatic organs and their way of functioning. Just like theories, organs and their functions are tentative adaptations to the world we live in« [Popper 1973: 145].

screwdriver when he is to knock in a nail. Even if truth is only preliminary, maximal truth is set forth as a moral obligation.

Nor is Popper's real point of view, however, identical with the italicized imperative formulated above; or rather, this imperative he only uses for polemical purposes—the point of view he is willing to defend in a serious discussion is more sophisticated.

In order to see why we may look at the difficulties presented by the »naïve-dogmatic Popperian imperative«.

Two objections were already raised against empiricism, where they were equally relevant. Firstly, observation and theory belong on different categorical levels. Therefore, *facts* cannot contradict theories; only statements (e.g., about facts) can contradict other statements (e.g., predictions made by theories). This was the problem which logical empiricism tried to overcome by concentrating upon the connection between »protocol statements« and theories, leaving to practicing scientists the translation of observations into protocol statements. The same could of course be done in the Popperian perspective. But this leads to the second objection. No observation is *pure*, every observation presupposes a number of general cognitive structures or theories—increasingly so in contemporary experimental science. But what precisely are we then to do when (a statement about) an observational fact contradicts our predictions? If, e.g., a telescope observation of the planet Mars finds the planet in another place that predicted by the Theory of Relativity? Should we regard the Theory of Relativity as falsified and reject it? The theory of the telescope? Both? Or none?[51]

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[51] This is a somewhat simplified version of a real dilemma which once presented itself to Newton and the Royal astronomer Flamsteed. Flamsteed did not find the Moon where Newton had predicted it to be; Newton, however, was able to convince Flamsteed that he had made wrong corrections for the refraction of light in the atmosphere, and that the real position of the Moon was where (Newton's) theory would have it to be. In coming years he went on to correct Flamsteed's supposed »facts« time and again. See [Lakatos 1974a: 130 n.5].

The example may also be correlated with the discussion between Galileo and his opponents when he published his new telescopic observations of hitherto unknown celestial phenomena (the Lunar mountains, the satellites of Jupiter). Among the objections to Galileo was the question, how he could be so sure that the effects were not artificial creations of the telescope.

A tentative solution might be gained from the observation that the functioning of the telescope has been confirmed through many other uses, including observations of terrestrial phenomena, and that it is thus more likely than the Theory of Relativity to hold water. But »confirmation« belongs with empiricism, being in fact nothing but that »verification« which falsificationism tries to replace! The solution thus ends up with the same conclusion as that which came out of the analysis of the logical empiricists: Science cannot be analyzed into single statements which are confirmed or rejected one for one: to some extent, scientific truth depends upon the totality of the scientific explanation.

As already told, these objections hit naïve falsificationism on a par with empiricism. A final objection, on the other hand, turns one of the objections against empiricism upside down. Empiricism could not explain the origin of theoretical concepts since they could not be derived directly from experience. It is precisely the aim of falsificationism to make space for these unsubstantiated yet indispensable entities. But the cost is as large as the gain: falsificationism makes possible the existence of theoretical concepts by disconnecting them completely from experience. In this way, theories end up by being nothing but *computational models*, which bear no more similarity to the reality they describe than the gears of a watch bear to the movements of the Solar system—the only connection being that the pointers of the watch can be used to predict the position of the Sun in the firmament. If the precision of the watch is insufficient, we scrap it and replace it by a different model: a digital watch containing no gears but only a quartz crystal and a printed circuit.

This is not the way real theories change. When one theory is replaced by another one dealing with the same field, the new theory will contain some new concepts and certain new relationships. But the concepts are rarely *quite* new, nor are the relationships. As classical mechanics was replaced by the Theory of Relativity, e.g., a number of fundamental entities like time, space, and mass (the first two being Kantian *a priori* categories, we observe) had to be understood in new, more sophisticated ways than believed till then; they also turned out to be mutually connected in ways which Newton had not imagined. But they were not abolished. The pattern of which they were elements was restructured, which changed their meaning—much in the same way as cognitive schemes accommodate when they are integrated into new cognitive structures during individual development. As phonemes (cf. note 37) replaced letters as elements of linguistic analysis, this was more than a mere change of names. Yet even though there is no one-to-one correspondence between letters and phonemes, the agreement is large enough to permit us to name most phonemes after letters. It was indeed the introduction of the phoneme concept that allowed linguistics to change its focus from written to spoken language with much less abrupt changes than an unmediated reference to speech sounds would have required[52].

Theories are thus *not* mere computational models, and predecessor theory and successor theory are more intimately related than the two watches. This falls outside the comprehension of falsificationism, which is at best able to explain the continuity between classical mechanics and the Theory of Relativity as a consequence of Einstein's lack of imagination.

Curiously enough, this problem is solved by empiricism with the same elegance as falsificationism solves the problem which empiricism creates concerning the justification of general concepts: *if* theoretical concepts are after all founded in experience, then there is no reason to wonder why they undergo only relative change instead of disappearing. The two approaches to the problem of knowledge solve each other's difficulties—but in mutually unacceptable ways. They stand as thesis and anti-thesis, in a way which in the first instance is as barren as a marriage between Plato and Diogenes.

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[52] We may also note that the decision to spell »ten« and »steam« with the same letter *t* shows the generations who introduced and adapted the writing of English (long before the emergence of any science of language) to have had an understanding of sounds as elements of language not too different from that of modern phonemic linguistics—a striking case of continuity of theoretical concepts in spite of theory change.

IV. A NEW APPROACH: THEORIES ABOUT THE SCIENTIFIC PROCESS

It was already stated that Popper is only a »naïve falsificationist« for polemical purposes. But it is the naïve Popper who is generally known; the naïve Popper is the real counterpart of empiricism; and the »philosophical« Popper is, after all, an attempt to keep together with string and tape the naïve Popper where he falls into too obvious pieces. For all these reasons, the naïve Popper is the more interesting of the two. The philosophical Popper is (in the present context) mainly of interest as a step toward that »realistic« Popper which his one-time follower Lakatos has created through a reinterpretation of the key concepts of his theories, and toward the understanding of *scientific knowledge* as resulting from a *scientific process*.

Popper and Lakatos: theories or research programmes?

The philosophical Popper (whom I shall call Popper_1 in the following, while Popper_0 is the naïve Popper and Popper_2 is Lakatos's construction) differs from Popper_0 on three essential points[53].

Firstly, Popper_1 does not take theories to be falsified by conflicts with experience, i.e., by »facts«. As stated above, facts and theories belong to different domains. Theories consist of statements expressed in words or other symbols, and therefore they can only enter into logical conflict with other statements. Theories are therefore not falsified by facts but, according to Popper_1, by *statements dealing with facts*—basic statements, in Popper's

[53] The exposition of these differences owes much to [Lakatos 1974a]. This article is also the source for the labels Popper_0, Popper_1 and Popper_2.

idiom, evidently a concept which is closely related to the »protocol statements« of logical empiricism[54]. The »theory« »all swans are white« cannot be in logical conflict with a bird in flesh and blood; what falsifies it is the basic statement »here is a black swan«.

Superficially seen this is only a shift of the problem which does not solve it—»basic statements« and facts still belong to different categories—and a specification—how should a theory be in conflict with empirical facts if it was not contradicted by the enunciation of these facts in statements? But in connection with »improvement« nº 3 the shift will turn out to have important consequences within all ideologically sensitive scientific domains (cf. below).

The next innovation replaces precipitate dogmatism with philosophical and historical common sense. The infant mortality of theories would be exceedingly high if every theory in conflict with (statements of) facts were to be rejected. Grossly speaking, *every* theory is contradicted by lots of facts at birth. But are we to reject Galileo's law of falling bodies because it is not obeyed by a falling withered leaf? Or a theory of prices referring to costs of production because it does not fit rare stamps?

Such rash rejections are not usual. In both cases you will have a definite feeling that the deviations from theory are due to specific circumstances, even though you may not yet be able to specify and quantify them. But you would evidently be dismayed if the speed of heavy leaden balls and the price of eggs went equally astray.

Popper_1 attempts to formalize this consideration by restricting the range of inconsistencies with regard to reality which will count as falsification. Evidently such a restriction cannot be specified in general. But when working on a theory you should point out (yourself and beforehand) the specific domains where the theory should in any case hold good[55]; if it

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[54] »What I call a 'basic statement' or a 'basic proposition' is a statement which can serve as a premise in an empirical falsification; in brief, a statement of a singular fact« [Popper 1972a: 43]. Further on in the same book Popper tries to construct a fence between his own concept and that of the logical empiricists, which he finds too »psychologistic«.

[55] »... criteria of *refutation* have to be laid down beforehand; it must be agreed which observable situations, if actually observed, mean that the theory is refuted« [Popper

does not, you should reject it *without mercy* (Popper's rhetorical style)—if you don't, you are dishonest (*ditto*). According to Popper, Galileo should thus »at his peril«[56] determine beforehand that »if my law of falling bodies does not fit within 98% the speed of a leaden ball of 20 kg falling 50 m, I will burn my manuscripts and turn to literary criticism«. Theories should »stick out their neck«: they should be unable to escape the hangman of experience, should he happen to pass by.

The third difference between $Popper_1$ and $Popper_0$ relates to the problem that scientific »facts« are obtained by means of methods themselves presupposing theories, as in the case of the telescope observations of the Moon[57]. Even this problem Popper gets around by making it a moral obligation to choose in advance. Before testing your theories you should also stick out your neck by deciding beforehand which theoretical fundament you accept as unproblematic and hence above criticism. Woe to the scientist who *post festum*, when his theory *has* got into trouble, starts doubting his telescope. He is, according to Popper, dishonest[58].

1972: 38 n.3].

[56] [Popper 1972: 42 n.8].

[57] Popper himself will rather point to human weakness, but the conclusion is the same: »... we may point out that every statement involves *interpretation in the light of theories*, and that it is therefore uncertain. This does not affect the fundamental asymmetry [between possibly falsifying observation and falsifiable theory—JH], but it is important: most dissectors of the heart before Harvey [who discovered the blood circulation—JH] observed the wrong things—those, which they expected to see« [Popper 1972: 41].

[58] One is tempted to ask how to characterize Popper's own attitude. At the age of 17 he engendered his marvellous falsificationist epistemology, though in the naïve »$Popper_0$« version. In coming years, when he discovered the shortcomings of this programme, he did not give it up. Instead he repaired it by means the distinction between facts and »basic statements« and all the other subtleties belonging to »$Popper_1$«.

But since, as we shall argue below, the prohibition of *a posteriori* criticism of the theoretical foundations of observations is ill founded, there is no serious reason to censure Popper for his failure to submit to his own rule.

Lakatos has composed an ultra-short-story demonstrating the divergence between Popper's methodological requirements and the real behaviour of the scientific community[59]:

> A physicist of the pre-Einsteinian era takes Newton's mechanics and his law of gravitation, (*N*), the accepted initial conditions, *I*, and calculates, with their help, the path of a newly discovered small planet, *p*. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory *N*? No. He suggests that there must be a hitherto unknown planet *p′* which perturbs the path of *p*. He calculates the mass, orbit etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet *p′* is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet a bigger one.[60] In three years' time the new telescope is ready. Were the unknown planet *p′* to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research grant to send up a satellite to test his calculations. Were the satellites instruments (possibly new ones, based on little-tested theory) to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it? No. He suggests that there is some magnetic field in that region of the universe which disturbed the instruments of the satellite. A new satellite is sent up. Were the magnetic field to be found, Newtonians would celebrate the sensational victory. But it is not. Is this regarded as a refutation of Newtonian science? No. Either yet another ingenious auxiliary hypothesis is proposed or ... the whole story is buried in the dusty volumes of periodicals

[59] [Lakatos 1974a: 100f]. As in the case of my above Mars/telescope example, Lakatos's story refers to somewhat more complex but similar real-life events.

[60] If the tiny conjectural planet were out of reach even of the biggest *possible* optical telescopes, he might try some quite novel instrument (like a radiotelescope) in order to enable him to 'observe' it, that is, to ask Nature about it, even if only indirectly. (The new 'observational' theory may itself not be properly articulated, let alone severely tested, but he would care no more than Galileo did). [Lakatos's footnote]

and the story never mentioned again.[61]

This story agrees well with what goes on within even the most exact sciences. Within the realms of social sciences and the humanities, where precise predictions are rare, and where the distinction between facts, theoretical notions and ideological conventional wisdom are not easily established—there, as one might guess, the rules are even more rarely observed.

A follower of Popper might reply that he knows: Popper does not describe what scientists actually do—he proposes a programme which would make science advance more rapidly and with fewer wasted efforts than it actually does *precisely because Lakatos's story is correct*[62]. Popper's rules would not ensure that no mistakes were made; but they would reduce the number of mistakes and, especially, the time that is wasted on mistakes.

Our follower of Popper would be wrong. Scientific work according to Popper's rules would, like most »work according to the rules«, be a synonym for a strike in disguise.

There are several reasons for that. Firstly one may ask what happens when a theory has been falsified and therefore rejected. When planet *p'* does not show up in the telescope, should we then reject Newton's understanding of the planetary system and of mechanical physics in general? Should we stop calculating the dimensions of steel beams for bridge buildings and make them at random instead? Having rejected Newton's mechanics we have no theory at all, since all predecessor theories

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[61] At least until a new research programme supersedes Newton's programme which happens to explain this previously recalcitrant phenomenon. In this case, the phenomenon will be unearthed and enthroned as a 'crucial experiment'. [Lakatos's footnote; in the next note he refers to Popper's polemics against the Freudian and Adlerian psychologies which can be made agree with any state of the actual world, pointing out, as done in the present pages on a simpler example, that the same holds for Newtonian physics].

[62] This answer is not always given by Popper himself, in particular not when he lapses into Popper$_0$. The point in his discussion of psycho-analysis and Marxism versus the testing of the General Theory of Relativity in his [1972: 34ff] is precisely that the latter, representing *science*, behaves differently from the former, representing *pseudo-science*. Elsewhere, however, Popper is more clear about presenting a set of prescriptions and no description.

have *also* been falsified. The absurdity of the claim is blatant, and shows the Popperian notion of »merciless rejection« to be empty jargon.

One may also—which is Lakatos's main point—observe that Popper's understanding of the nature of a *theory* is much too static and formal. A theory which is to fit Popper's prescriptions is complete and fully finished—a Tarskian formal language (cf. note 46); it consists of a set of formulae (verbal or symbolic) which definitively state the mutual relations between the concepts of the theory, and a set of rules which allow the translation between theory and observation, i.e., allow observation to function as a metalanguage telling which statements are true and which false.

Few theories, if any, have been born in that form. It is debatable how many attained it before they died. Theories are born instead, as we shall see in more detail below, as open structures. Only through the collective work of the scientific community with the theory does one fully discover its implications and possibilities and its relations to other theories[63]. Already for this reason it is impossible to indicate when a theory is conceived at which points decisive testing should be performed.

In social and human sciences, Popper's methodology would give rise to yet another problem, which has to do with the *conventionalism* of Popper_1.

In general, conventionalism belongs to the same family as instrumentalism. Like instrumentalism it holds that one theory is no more *true* than another, competing theory—ascribing »truth values« to theories is as nonsensical as ascribing colours. Whether we use one or the other

[63] This was in fact pointed out at one moment by Popper in an article from 1940: »The dogmatic attitude of sticking to a theory as long as possible is of considerable significance. Without it we would never find out what is in a theory—we should give the theory up before we had a real opportunity of finding out its strength; and in consequence no theory would ever be able to play its role of bringing order into the world, of preparing us for future events, of drawing our attention to events we should otherwise never observe« (reprint [Popper 1972: 312]). And similarly: »... this dogmatism allows us to approach a good theory in stages, by way of approximations: if we accept defeat too easily, we may prevent ourselves from finding that we were very nearly right« [1972: 49]. Yet, as pointed out by Lakatos, this glimpse of insight does not influence his general thinking and his magisterial preaching.

theory for our description of reality is decided *by convention*, no more compulsory than the convention which makes us speak of »cigarettes« and »pamphlets« and not of »cigamphlets« and »parettes«; empirical evidence may at worst force us to change the way we interpret of our theory, the »rules of translation« between observation and theoretical prediction[64]. *Scientific objectivity* is thus nothing but agreement about a shared convention—and if you disagree with the majority of your scientific colleagues, *you* are automatically the sinner against objectivity.

This breed of conventionalism is treated with as intensive scorn by Popper as are Marxism and psychoanalysis. Conventionalism, indeed, denies the falsifiability of theories, eschewing it through a reinterpretation of the rules of translation between theory and observation. Popper sees clearly and correctly that conventionalism can function as a cloak for scientific opportunism and for facile thinking in grooves. But his own philosophy contains obvious conventionalist elements: convention and nothing but convention points out which kinds of conflict should be regarded as falsification; and convention decides which theories should be ranked as unassailable and which should be submitted to continuous attempts at falsification[65].

In ideologically sensitive areas, i.e., in particular within the social and the human sciences, even this brand of conventionalism will easily entail stagnation in ideological opportunism. What is more easily agreed upon by the majority than the set of already familiar, stereotype ideas? Once more the objective scientist will be the one who accepts the received opinions of respectable people, and the dishonest worker the one who

[64] All this may be more clear from an example. If you sit in a train, you will normally state that it starts moving after the doors have been closed; but you might equally well state (and at times get the momentary expression) that you and the wagon stay at rest, and the rest of the universe starts moving. In the first case, the observation that empty bottles start rolling along the floor is explained as a consequence of the law of inertia; in the second by a changing gravitational field.

The standard example is in larger scale but built on precisely the same principle: the question whether the Earth or the Sun is at rest.

[65] This is not kept secret by Popper, who speaks explicitly of his methodology as »methodological conventionalism«.

challenges conventional wisdom and sticks to his own scientific convictions[66].

As stated above, Popper's one-time follower Lakatos has formulated that more accurate epistemology which in his opinion might grow out of Popper_1, and has baptized it Popper_2[67]. The central point in Lakatos's epistemology is reflected in this labelling: »Popper«, in fact, does not refer to the person but to what Lakatos calls a *research programme*, evidently inspired by the person; Popper_0, Popper_1 and Popper_2 are nothing but single stages within the development of this programme.

Precisely this example may provide the most easy explanation of the concept. A research programme is not, as with Popper, a static and solid entity; it is a progression through a number of static theories superseding each other. A research programme thus roughly coincides with the more loose parlance of a »theory« as something which is in continuous development.

The feature which unites a sequence of theories into a research programme is the existence of a shared *hard core*, a set of notions about the entities which exist within the field dealt with in the research programme. In the Popper programme thus *theories* which cannot be derived from empirical observation; *falsifications* which kill theories; and some kind of facts or representatives of facts taking care of the killing. In the Newton programme, material particles and forces. These entities are the tools which the theories belonging to the programme apply in order to describe/explain empirical reality. In addition to this ontology, the hard core prescribes the kinds of explanation which should be aimed at, the methods which are to be preferred, etc.

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[66] Popper's requirement thus stands in curious contrast to Merton's norm of »organized scepticism« (see p. 137), which Merton illustrates by the German *dictum* »ein Professor ist ein Mensch, der anderer Meinung ist«, i.e., one who does *not* automatically submit to received opinions.

[67] Rumour has that Popper_2 is already in manuscripts written by the real Popper but kept unpublished—maybe because their publication might reduce the famous $\text{Popper}_{0/2}$ to ashes.

The hard core is »hard« in the sense that it does not bow to conflict with observational reality—it is, in another word, *irrefutable*. If we use the Popper programme as an example, we see that empirical observations similar to Lakatos's short story may demonstrate that falsification *à la* Popper_0 and Popper_1 does not describe the real process of scientific development; but they cannot prove that the rejection of unsatisfactory theories may not in some way or other be understood as »falsifications«. Experiments might show that the gravitational force does not depend on distance in the way Newton believed; but they could hardly prove that *forces* are *in general* untenable explanations.

The protection of the concepts (etc.) belonging to the core at »any price« is called by Lakatos a *negative heuristic*—a guide as to what one should *avoid finding*. The core also contains a *positive heuristic*—a guidance prescribing how increasingly extended ranges of reality are to be explained, and in which order »anomalies« are to be solved. The existence and efficiency of this positive heuristic is of course the reason for the cohesion and continuation of the research programme—if Popper_0 and Popper_1 had produced no interesting points there would have been no reason to stick to the programme and to a fundamental idea like falsification.

The theories which make up a research programme are not only gathered into a common heap because they are characterized by certain shared features constituting a shared hard core. As already intimated, and in the likeness of Popper_0, Popper_1 and Popper_2, they are ordered in a progressing sequence, T_1, T_2, T_3, ... T_N, ...

In order to introduce more fully the relations between the members of such a progression of theories it might prove useful to look at a more substantial example than the Popper sequence. Such an example is provided by the progression of economic theories built upon the labour theory of value.

The first step, the one which originated the hard core and the programme, was Adam Smith's *Wealth of Nations* from 1776. In this work, Smith formulated a theory of the price of a commodity as proportional to the working time required to produce it[68]. This doctrine was no loose postulate but argued from the competition between workers as well as between manufacturers and thus connected with the concept of three social classes[69]. This was a radical innovation with regard to the preceding physiocratic conception, according to which only work in agriculture was productive, while all kinds of industrial transformation (e.g., grain into flour and bread) were unproductive. (Cf. [Høyrup 1993:125-131, *passim*]).

In his more systematic *Principles of Political Economy and Taxation* from 1817, David Ricardo took over the labour theory of value, and used it among other things to explain the mutual advantage of foreign exchange in a market system. By using the concepts of competition and scarcity he also managed to explain from the theory how the mere possession of agricultural land would allow landlords to earn money (the rent)[70]. Ricardo, however, wrote in a situation where some industries were significantly more »capital intensive« than others; this difference had been less conspicuous 40 years before, when Smith wrote his book. Ricardo knew that the difference had to influence prices, without being able to integrate this knowledge into the theory of value, and therefore restricted himself to the statement that the working time required to produce a given

[68] Evidently, my exposition of Smith's and other economic doctrines is cut to the bare bones, and simplified to that extreme where only the features which are essential for the epistemological argument stand back.

[69] »Those who live by profits«, i.e., capitalists; »those who live by wages«, i.e., workers; and »those who live by rent«, i.e., landlords. In Smith's England, it should be remembered, landowners would normally lease their land to farmers. The latter would thus be counted as capitalists, while the landowners (who owned only land but no means of production) were a separate class.

[70] In brief: If a country needs N tons of grain per year, this requires that the best A acres are cultivated. Some of these acres yield more than others at the same expense of labour, but the price of the grain will of course be the same. Competition will fix the price at the level corresponding to the labour costs of the poorest land, which is the highest level (if the price were lower, nobody would care to cultivate this land, which would result in shortage, famine and raising prices). The landlords possessing the best land will thus get more from their grain than the labour costs—or they will lease to capitalist farmers who will be willing to pay the difference between their selling price and their labour costs as rent.

Keine Hexerei, nur Behändigkeit!

commodity would determine its price until perhaps 90%—one has spoken of Ricardo's theory as a »90% labour theory of value«. If we speak of Smith's theory as T_1, Ricardo's will be T_2.

Both T_3 and T_4 are due to Marx. T_3 solves the difficulty which arises if the labour theory of value is applied to the price of labour itself, i.e., to the wages. It would seem that a labour cost determination of wages should lead to a payment for 8 hours of work which would be able to buy precisely the product of 8 working hours. This would leave no space for profits—in flagrant contrast with the normal state of affairs.

In Marx's writings from the late 1850s onwards (T_3) this problem is solved. Prices, according to this theory, are still determined by the working time needed to produce commodities; but the wage is not the payment for the *working time* but for the *working power*. The working time needed to produce 8 hours of working power is the time normally required to *produce* that working power, i.e., to produce the commodities which the worker consumes in order to keep going for another day[71]. If the working class of a country only consumes half of its social product, we see that the time used to produce what an average worker needs to go on for another average day is produced in 4 hours. The price of *8 hours working power* equals *4 hours working time*. This leaves space for profit.

Another problem is still unsettled—*viz* Ricardo's problem of varying capital intensities. This difficulty is only resolved in volume III of *Das Kapital*, which was published posthumously by Engels in 1894 (T_4). The breakthrough consists in a separation of the concepts of »value« and »price«. The value of a commodity is now *defined* as the working time normally required for its production under given technological, societal and historical conditions. *If* prices were equal to values (after a suitable conversion of time into monetary units), capital would be invested in those sectors where a maximum of work was done (and hence a maximal profit earned) per unit of invested capital[72]. These sectors would soon be

[71] Of course averaged over life, so that the costs of procreating and feeding children is included. In the present simple version, a two-class model for society (capitalist and working classes and nothing else) is presupposed.

[72] Equality of prices and values would mean that the profits per working hour would be same in all sectors; the more capital you need in order to employ one

overproducing compared to actual demand, while those depending on larger investments per working hand would be underproducing. The imbalance between demand and offer would make prices fall in the overproducing sectors and make them rise in the underproducing ones. This continues as long as profit rates differed; in the end, prices will be stabilized precisely so far from values that the profit rates of all sectors are equal[73]. Ricardo's problem is solved—indeed by means of theoretical considerations borrowed from his own theory of rent. At the same occasion another difficulty dissolves: How to explain *within the labour theory of value* the incomes of banks, wholesale merchants and *rentiers*.

After the death of Marx, only Marxist economists continued work within the framework of the labour theory of value (grossly speaking). The reason was obviously the political consequences of the doctrine, as they had been uncovered by Marx[74]. Further development was branched, as was the labour movement itself. One further development (T_5', T_6', etc.) consists of inconsistent crisis theories (Rosa Luxemburg, Ernest Mandel and others). Another branch contains a better theory of the dynamics of economic crises (T_5, Kalecki) and the solution of remaining problems concerning the relation between value and price (T_6, Sraffa).

What is proved by this whole story? First of all that Lakatos's research programmes are no description of real history but »rational reconstructions«, in Lakatos's own words. Marxism is certainly more than a further elaboration of Adam Smith's research programme, and one gets no genuine description of Marxism through isolated exposition of Marx's economic analyses. Worst of all, Marxist analyses of the increasing monopolization

worker, the less you will find your profits per invested £ to be.

[73] In the actual world they will of course never be completely stabilized—continuous technological development is one of several factors which cause the point of equilibrium itself to be moving.

[74] Gustafsson [1968: 14-16] lists a variety of sources which document this explicit concern. The main problem was the separation of working time and working power (T_3), which automatically entailed a concept of exploitation.

of capitalist economies after 1870 are not easily fitted into a rational reconstruction relating everything to the Smithean starting point[75].

At the same time, however, the story shows that Lakatos is far from being totally wrong—the rational reconstruction reflects central aspects of the historical development, and can thus be claimed to be a *true* theory for the structure of scientific development in the sense explained above. Finally, the process exhibits some of the characteristics which according to Lakatos distinguish the development of research programmes.

Firstly it shows that research programmes may easily live with »anomalies«—points where they disagree with observation without being able to explain precisely why. Ricardo's 90% theory is in fact nothing but an attempt to talk away an acknowledged anomaly, the influence of capital intensity on prices. The labour theory of value was not dismissed by Ricardo because he got stuck in a problem which could not be solved for the moment. Nor did the difficulty paralyze Marx, though only two theoretical breakthroughs allowed him to solve it.

Still, a research programme cannot live with all its difficulties without doing *something* about some of them. If Ricardo had only been able to introduce his 90%-restriction and had not increased the explanatory power of the programme on other points (of which foreign exchange and rent were mentioned), the programme would have *degenerated* (Lakatos's term), and it would have been abandoned by active research as soon as an alternative approach had been found.

Changes which are not degenerative are called *progressive problemshifts* by Lakatos. A progressive problemshift occurs when a new theory is both empirically and theoretically more powerful—if it predicts *more* than the predecessor (greater theoretical content), and if it predicts *better* (greater empirical content). If we forget about the 90%-restriction, the whole sequence T_2-T_3-T_4-T_5-T_6 consists of progressive problemshifts. T_5', however, which aimed at increasing the theoretical content of the theory, was no progressive shift: on one hand, it did not increase the empirical content of the theory; on the other it was ridden by inner inconsistency. The same

[75] In justice it should be said that Lakatos did not propose the application of the research programme idea to the development of the labour theory of value; but similar features would turn up in many other instances.

holds for T_6', Mandel's attempt to show how the spread of fully automatic industry would entail the collapse of capitalism.

Theories falsified by theories

An important feature of Lakatos's conception is his notion of falsification. A theory, as we have seen, is not falsified by an anomaly, however serious. According to Lakatos, a theory is falsified *by another theory*—by a theory with greater theoretical and empirical contents, by a theory which so to speak *explains why its predecessor failed to explain* specific anomalies.

This is in itself a progressive problemshift, solving or dissolving no less than four of the central dilemmas presented by $Popper_1$:

- Firstly, the question what to do in the interlude between the falsification and resulting merciless rejection of one theory and the devising of a replacement. There *is* no such interlude, since falsification only follows from the development of a new and better theory.
- Secondly, a difficulty which, though not formulated explicitly above, follows from the lack of continuity of theoretical concepts through the cycle of falsification and ensuing free invention of a new theory. At best, Popper's methodology might bring forth a sequence of increasingly precise *models* of reality; but even in the best of cases the falsification cycle will never procure us with *increasingly deep theoretical insight:* every time a theory is falsified and thus rejected we replace it by something which in principle is *totally new* (like the digital watch replacing the mechanical watch). We cannot raise the question *what* was wrong in the rejected theory: It is the model as a totality that is wrong and thus rejected. Within the framework of the materialist notion of truth (see p. 45) we may say that if the key concepts of the hard core reflect essential aspects of reality, then the research programme allows an increasingly exhaustive investigation of these features, and thus an increasingly objective reflection.
- Thirdly, falsification *à la* Lakatos does not invite to ideological opportunism as does Popper's methodological conventionalism. On the contrary: If a theory is only regarded as falsified by another theory which offers deeper and more precise explanations, then disagreement

with superficial ideology and conventional thinking will be a less threatening argument against it[76]. Theories get greater opportunity to confront reality directly, bypassing the censorship of received opinions.

— Fourthly and finally, the paradox evaporates that every observation is polluted by theoretical or proto-theoretical presuppositions. If theories are falsified by theories this is no longer a source of logical trouble but only another expression of the tangled character of scientific (and other) knowledge: a totality which cannot be fully analyzed into mutually distinct elements, be it into the verified and thus meaningful statements of empiricism or into Popperian basic statements.

At one point, Lakatos's conception (Popper$_2$, as we remember) can be claimed to constitute a degeneration with respect to Popper$_1$ (the sophisticated real Popper). Popper's aim is to formulate a *logic of research*[77], a formalized system which can be set before the scientific community as a set of rules which it ought to obey. Lakatos's rational reconstructions preserve this aim to some extent. But Lakatos has given up the conviction that the falsification processes of the rational reconstruction (not to speak of that real history which it rationalizes) can be formulated in a way which complies in full with the requirement of formalization[78]: Who is able to

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[76] Once again we may correlate with Merton's »organized scepticism«: while Popper's prescriptions would tend to undermine this norm, Lakatos explains it and makes it a methodological necessity.

[77] This is the best English translation of the original German title of [Popper 1972], and describes Popper's intentions adequately. The corresponding English title (*The Logic of Scientific Discovery*) may be better for advertisement purposes but misses the point completely: The only part of the research process which according to Popper should be completely free and subjectively creative and not subject to any logic is precisely the phase of *discovery*, the invention of a new theory. Formalization and strict rules belong with the *control process*, the compulsory stubborn attempts at the life of the assumed discovery.

[78] Conversely, of course, Lakatos also gives up the belief in the completely unfettered process of invention: as long as innovation takes place within the same research programme, it is guided by the positive heuristic and restricted by the negative heuristic, both of which are constituents of the hard core of the programme. But even if guided and restricted and thus no act of pure subjectivity, innovation has not become formalized.

balance the degenerative versus the progressive elements in the shift from Smith to Ricardo? Everybody, of course—but very precisely to balance, not to state the definitive and indubitable decision. In many instances it is also only through extensive and meticulous research that one is able to decide whether a theory possesses greater empirical content than a competitor; it may even be that agreement with observation improves on some points and decreases on others. The decision can no longer be reached by an impersonal and objective judge, it is attained instead in an informal arena, *viz* the scientific community »voting with its feet«. The choice between theories is the sum of individual choices made by individual workers deciding which theory they are going to make the foundation for their own further work. Paraphrasing the jibe against Ricardo, Lakatos's theory can be characterized as a »90% logic«.

However, rather than speaking against Lakatos's epistemology this observation tells something about the concept of degeneration: Degeneration need not be a defect, even though this is the obvious moralistic implication of the term. It may just as well be a rejection of empirically degenerative aberrations contained in earlier theories. In this vein we may look at Popper_0 and Popper_1 as aberrations within a research programme starting informally from a hypothetical-deductive understanding of scientific method, and at Lakatos/Popper_2 as an alternative and more fruitful development from the same roots. In any case, theoretical degeneration need not be a development for the worse—as well known by military planners, a tactical retreat from unwisely occupied positions may be the only way to avoid imminent defeat and the best preparation for further offensives.

Two final points should be addressed before we leave the presentation of Lakatos's ideas. Both may, like the discussion of degeneration, be approached through the example offered by the theory itself.

According to Lakatos, the normal situation within a scientific discipline is the existence of several competing research programmes. Evidently, the philosophy of science offers a striking example of this. During an extended period, e.g., logical empiricism and Popperianism were both pursued (along with other programmes). Similar examples can be found in many other disciplines; still others, on the other hand, seem to be dominated by one programme at a specific moment.

Here it is important to remember that falsification takes place *within* research programmes. One research programme cannot (if we follow Lakatos) be falsified by another programme, because its hard core is irrefutable. Research programmes are not falsified, they are given up when they degenerate and when alternative choices are at hand[79].

To some extent, the process by which one research programme displaces another thus constitutes a parallel to the falsification within a research programme. Yet the parallel is imperfect: a new research programme does not necessarily get the upper hand because its empirical content is larger than that of the programme which it supersedes; it may be preferred because it explains a specific anomaly which has resisted the predecessor so stubbornly that the whole validity of this programme has come to be doubted. This is what happened within chemistry in the late eighteenth century, when a programme explaining combustion as the absorption of a new chemical element *oxygen* replaced a predecessor theory which explained combustion as the liberation of an igneous substance called *phlogiston*. Among other things, the phlogiston theory had explained the colours of many chemical substances. Within the framework of the oxygen theory, these colours became inexplicable. None the less, the oxygen theory was preferred, because the areas where it possessed increased empirical content were considered more important[80].

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[79] In rare cases, programmes have even been considered degenerating beyond hope and have been given up by practitioners notwithstanding the absence of alternative programmes. In such cases the whole discipline has been abandoned by the scientific world and considered a pseudo-science, and an earlier belief in the results of the programme is declared superstitious or at least illusive. One example of this process is *phrenology*, a nineteenth century science about the alleged relation between people's character and the form of their skull (cf. [Høyrup 1993: 151]). The rejection of astrology by astronomers in the seventeenth century may be looked at in the same perspective.

[80] One of these areas was the specific weights of chemical substances—if weight was to be conserved in chemical processes, phlogiston had to have changing and sometimes negative specific weight.

From Lakatos's point of view, this is a decisive difference. All things considered, however, it seems to amount to no more than a difference of degree. True enough, the abandonment of one programme for another cannot be described as a formalized process—it results from a process of balancing and »voting with the feet«. But precisely the same was, though to a lesser extent, the case when we considered the falsification process within a programme.

The last feature of Lakatos's epistemology is that it is *reflexive*, i.e., that it is able to describe itself. Although it is nowhere said, exactly this must be the coquettish point in Lakatos's uses of the term Popper_2 as a label for his own approach. In so far as it claims to be a description of the actual process of knowing and of its conditions, i.e., to be itself *knowledge* about that process, reflexivity must of course be required from any epistemology[81]. Yet far from all epistemologies are in fact reflexive. As already hinted at, Popper's own rules would have forced him to give up his ideas as exhaustively falsified if he had followed them. Empiricist philosophy is no better off—logical empiricism, in particular, would probably be forced to see its own statements as ultimately meaningless if it applied its own verification standard. Similar auto-destructive conclusions will be reached in the cases of instrumentalism and conventionalism.

Still, Lakatos's theory is not the only reflexive offer on epistemology market. The epistemology of dialectical materialism has the same characteristic[82]. Moreover, full reflexivity is only achieved by Lakatos if he is given

[81] Evidently, reflexivity is in itself no proof of the adequacy of an epistemology. As pointed out in note 46, the truth of a system cannot be proved by self-reference. But as a minimum it must be required that the self-references contained in or implied by a system which pretends to be true are of the type »this statement is true« and not variants of the so-called *liar's paradox*, »this statement is false«.

[82] But not the kind of vulgar Marxism that claims consciousness to be nothing but reflection of the socio-economic circumstances under which it is produced, and rejects the relevance of any discussion of it in terms of truth value. If thinking in general should be understood on a par with Pavlov's conditioned reflexes, why should then the status of vulgar Marxism itself be different? Similar conclusions will be reached in the case of other unrestricted sociologisms, as also when we look at Skinner's behaviourist epistemology or other deterministic theories—indeed for all epistemologies which deprive knowledge of the possibility of being a *true*

a materialist interpretation, through which a truth value can be ascribed to a research programme and its appurtenant theories *in spite of* the metaphysical and irrefutable character of its hard core.

The limits of formalization

Lakatos's epistemology is able to grasp essential features of the development of scientific knowledge. But it is not able to grasp *all* essential features—no theory is. And it has not solved all the problems to which it directs attention.

Like Popper and the logical empiricists, Lakatos still regards science as a formalized system: A theory consists of unambiguous statements dealing with concepts and their mutual relations, and of »rules of translation« telling how the predictions of theory and empirical observation may be compared;—and science, on its part, consists of theories.

This conception Lakatos shares with Popper, and for that matter with logical empiricism. To be sure, their formal understanding of the system of scientific knowledge does not imply that Popper and the logical empiricists (nor, *a fortiori*, Lakatos) have not discovered the importance of ideas without formalizable foundation for the development of knowledge. Quite the contrary, the logical empiricists distinguished sharply between the »context of discovery« and the »context of justification«: they were fully aware of the possibility that the context of discovery of ideas may be far removed from empirical verification, involving intuition, religious and metaphysical ideas, etc. What they asked for was that an idea, once proposed, in the context of justification could be »verified« empirically. Popper, on his part, made a cardinal virtue of what logical empiricism had felt forced to accept: new theories should be freely devised, any attempt to make rules for this process would be inimical to science. Only in the moment when the theory *has* been formulated does the merciless effort to falsify set in. The whole model looks as if inspired by traditional

description/reflection of the real world (cf. [Høyrup 1993: 185]).

In general, *any* epistemology claiming possession of a complete and exhaustive explanation of the nature of knowledge can only be reflexive if it is able to explain the existence of complete and exhaustive knowledge. No middle road appears to exist between Platonism and epistemologies which are as open as that of Lakatos.

liberalistic ideology: anybody should be allowed to settle down as a shoemaker or as a railway baron, free of state regulation and control; but in the moment when he *has* started his business, he should be subjected to the objective verdict of the market, which kills unsound undertakings without mercy. Like attempts to keep a falsified theory alive, efforts to keep unsound businesses afloat through public intervention will only do damage to the common good.

Lakatos is less of a liberalist than Popper in his epistemology. The concept of a »research programme« and of progression within the programme, and particularly the idea of a positive heuristic, describe the process of invention as less than fully arbitrary and as somewhat open to theoretical comprehension—as taking place within a certain pattern. But the origin of research programmes and of their hard cores is still left outside the area considered by the philosophy of science as inaccessible to formal analysis. This does not invalidate the rest of his analysis, but it remains a serious restriction that the theory leaves out of consideration the life-giving moment of the development process as unexplainable; what would we think of a theory of ecological metabolism which explains that animals live from each other and ultimately from plants but disregards the photosynthetic process through which plants secure the energy supply for the total ecosystem?

In other respects too, Lakatos's search for formalized structure (and his desistence from describing what cannot be sufficiently formalized) creates more problems than it solves. The distinction between falsifiable theories and the irrefutable hard core of a research programme is surely meaningful. Yet the two levels can not be regarded as absolutely distinct; in the case of the labour theory of value, for instance, we must probably see the separation of price and value (T_4), and perhaps even more the separation of working time and working power (T_3) as so radical reinterpretations of the foundations of the programme that its hard core is changed. If we look at the shift from $Popper_1$ to $Popper_2$ we must also acknowledge that the new concept of falsification is so far removed from its predecessor that even here the hard core is affected—no wonder that Popper rejects $Popper_2$.

Finally, the sharp distinction between research programmes is dubious. It is true that different research programmes build on different sets of cardinal concepts, and no complete translation from one programme into the other is possible—cf. the relation between empiricism and naïve falsificationism. But they are still connected via their reflection of features of the same reality (if we presuppose a materialist view), and often of the same features (even though the problem for empiricism and naïve falsificationism seemed to be that each of them was formulated with regard to features which were inaccessible to the competing programme). Thereby the possibility emerges that the concepts of one programme may be explained at least with some approximation in terms of the core and theories of the other; perhaps one programme may even develop to the point where it is able to explain the accomplishments and the failures of the other—which was the criterion for falsification *within* a programme.

Apart from being built upon the basic premiss of a materialist view (*viz* that knowledge reflects features of a reality existing independently of the knower and the knowledge in question), this conclusion corresponds to experience borrowed from the history of a variety of sciences.

If we look at the confrontation between the phlogiston- and oxygen-theories, the former was mere nonsense as seen from the stance of early oxygen theory, and its triumphs nothing but lucky accidents. In some instances, in fact, the »substance« phlogiston »turned out« (so oxygen theory) to be identical with carbon or hydrogen; in others it represented the absence of oxygen. But the development of the concept of »degrees of oxidation« in the later nineteenth century provided an explanation of what was common for carbon and the absence of oxygen. It thus became clear which features of reality (as seen by the mature oxygen theory) were reflected in the doctrine of phlogiston. Phlogiston theory, which had originally been *abandoned*, could now be seen as *falsified* in Lakatos's sense.

Corresponding examples can be found everywhere. Most obvious is perhaps the relation between Ptolemaic and Copernican planetary astronomy. If we accept the Copernican system (or one of its later variants) it is easy to calculate how planetary orbits behave as seen from the Earth, and hence to see how the Ptolemaic model manages to account with relative precision for the position of planets on the celestial vault. But even the more ambiguous field of social sciences offers some instances—as many, indeed, as can be expected in a domain where woolly conceptualizations and cross-fertilizations often make it difficult to speak of distinct research programmes.

A striking example is provided by the relation between the labour theory programme and that »neo-classical« or »marginalist« programme which replaced in within academic economics after c. 1870, when the former programme had become politically unacceptable (cf. above, n. 74). The neo-classicists started out from concepts and problems which were explicitly incompatible with the labour theory of value. Asking for a theory which was equally valid for rare stamps and for eggs, it had to take its starting point in consumers' preferences and not in the costs of the producer[83]. But gradually the neo-classicists were forced to approach the questions that had occupied Smith and Ricardo, *viz* the global economic process of society. At that moment they had to develop a concept of the *price of production* which determined the long-term price level of products which (like eggs) could be produced in any quantity[84]. This price of production turns out to be explained by arguments that follow the fundamental structure of the discussion in *Das Kapital* III of the relation between value and price (only published some years later). Marx is hence able to explain Marshall, just as Copernicus/Newton is able to explain Ptolemy[85].

Lakatos, we may say, sees the development of a scientific discipline as consisting of a number of parallel lines (each representing a research programme) competing for the favour of the scholars of the field and terminated at the moment when favours fail. A more realistic view, on the other hand, would have to look at the lines as partly interconnected. Lakatos's idealization is correct in so far as the connections between research programmes are weaker than the connections between theories belonging within the same programme; but an understanding which aims at getting beyond Lakatos's formalization should start by recognizing the existence of interconnections[86].

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[83] Jevons, *The Theory of Political Economy*, published 1871.

[84] Marshall, *Principles of Economics*, 1890.

[85] In both cases, on the other hand, the reverse explanation (Ptolemy of Newton, Marshall of Marx) turns out to be impossible, for the simple reason that Newton's and Marx's theories include dynamic explanations which fall outside the scope of their competitors.

Marx, on the other hand, is not able to explain Keynes's theory of the economic cycle; this is only done by Kalecki (T_5)—see [Robinson & Eatwell 1973: 48ff].

[86] This is no point of pure philosophy but carries an important message for practical scientific work: You should never dismiss the reflections and theoretical results achieved by another school with the argument that they belong within another research programme and that they are therefore irrelevant for you. *Dialogue is possible* and often the crucial condition that you may progress along your own road—not least within the human and social sciences.

We may add that even the lines themselves (the single programmes) possess an inner structure. Branchings are common, and it is not always possible (as it was in the case of the labour theory of value) to distinguish between a sound trunk and vain aberrations. The solution of single problems (concerned, e.g., with specific anomalies) may be the occasion for the emergence of specific sub-programmes within the same global research programme. At times such sub-programmes may be absorbed into the main programme when a satisfactory solution to their specific problems has been found; at times they may provide the starting point for a new discipline or sub-discipline.

Kuhn: Paradigms and finger exercises

Chronologically and historically, Lakatos's concept of »research programmes« is an attempt to describe from a Popperian point of view an approach to the problem of scientific knowledge which in many respects constituted a radical break with established ideas. Making Kuhn—the originator of the new approach—appear as a commentary to and an elaboration of Lakatos's ideas is thus a pedagogical trick and no reflection of historical connections. None the less, the trick may be useful.

Thomas Kuhn is a former physicist turned historian of science and no philosopher. This is clearly reflected in his book *The Structure of Scientific Revolutions* ([1970]; 1st ed. 1962), in which his ideas were first presented in print. It does not, like Popper's presumed »logic« of research, attempt to prescribe rules which are supposed to guarantee more steady scientific progress; instead, Kuhn's first aspiration is to find structure and coherence in the baffling imbroglio of the history of the sciences; his second aim (which need not be secondary) is to understand why this structure is able to produce *knowledge*, and to show how it may indeed be adequate and perhaps even necessary for the production of scientific knowledge, regardless of its conflict with time-honoured ideas about the nature of good science[87].

[87] This second question was formulated in the title of Kuhn's contribution to a symposium on »Scientific Change« held in Oxford in 1961 (published as [Kuhn 1963]): "The Function of Dogma in Scientific Research". Some years later, Kuhn

The central concepts in Kuhn's understanding of scientific development are *the paradigm; normal science;* and the *scientific revolution. Normal science* is science whose development is governed by a paradigm, and a *scientific revolution* is the replacement of one paradigm by another. The *paradigm* itself is thus an adequate starting point.

The term is borrowed from traditional language teaching, and is another name for the *exemplar*.

An exemplar or paradigm is a word which is used to train a conjugation scheme—as in Latin *amo, amas, amat, amamus, amatis, amant*, or in German *ich liebe, du liebst, er liebt, ...* . Other words belonging to the same category (*a*-stem verbs and weakly conjugated verbs, respectively) will then have to be conjugated »in the same way«, in a way which is understood quite as much through subconscious training as from explicit rules. The point of using the paradigm instead of the abstract sequence of endings *-o, -as, -at, -amus, -atis, -ant* is precisely this subconscious way of functioning. If you had only learned the latter system you would have to switch from speaking to analytical thinking each time you were to use a verbal form. The paradigm, on the other hand, functions much in the same way as the subconscious sensorimotor schemes described by Piaget[88]—or it may serve at least as the starting point for the construction of a subconscious scheme[89].

[1974: 237] formulated his double approach as follows: »The structure of my argument is simple and, I think, unexceptionable: scientists behave in the following ways; those modes of behaviour have (here theory enters) the following essential functions; in the absence of an alternate mode *that would serve similar functions*, scientists should behave essentially as they do if their concern is to improve scientific knowledge«.

[88] Cf. also what is said on p. 12 regarding linguistic schemes.

[89] It should be observed that in structuralist linguistics the term *paradigm* is used in a way which differs fundamentally from Kuhn's : In the rudimentary sentence structure »subject—verb«, the phrases »a dog«, »the bird«, and »Susan« are part of *one* paradigm, the set of words or all phrases which may serve as subject, and from which precisely *one* element is to be chosen; the phrases »runs«, »dies«, and »is born« belong to another paradigm.

This use of the term is derived from its meaning in traditional grammar, too; even from the sequence *ich liebe, du liebst, ..., one* element is to be chosen when a sequence is to be constructed.

The key point in Kuhn's approach to scientific activity is that it is a creative and active practice in the same way as the use of language. You learn to use German verbs through reading and speaking German and through the training of paradigms, not from the mere reading of grammatical rules; you learn to ride a bicycle by trying and not merely through explanations of the role of the handlebars for maintaining balance (I still remember the explanations I got at the age of five; they would have sent me headlong into the pavement, had I ever tried to follow them); you learn to play the piano through finger exercises, transposition exercises and training, not from mere explication of the musical notation of the correspondence between single notes and keys, and of the major/minor system. In a similar way you learn to work inside, e.g., the Newtonian research programme by using its theories and by observing their use, not from a mere abstract exposition of »Newton's three laws« and the law of gravitation or of the »hard core« of the programme with its appurtenant negative and positive heuristic; you learn to perform a structuralist analysis of a literary work by doing and by following analytical work, not from mere exegesis of the principles of structuralism.

—not *merely* from theoretical and abstract exposition, though evidently *also* in this way. Scientific work does not stop at being skill and knack, it is *also* a conscious activity. Researchers are not sensorimotor infants but analytically minded adults that integrate the schemes of their cognitive unconscious as tools for conscious operatory thought. The essential point—and a point which is neglected by both Popper and Lakatos and indeed by almost all philosophers of science—is that scientific activity *also* contains an essential element of skill.

From where, then, do scientific workers get their skill? In former times, before the systematic training of future research workers in universities, by reading THE BOOK—that decisive book which had moulded the basic understanding of their discipline. Astronomers read Newton's *Principia;* before this seminal work was published they read Kepler or Copernicus[90];

[90] With one historically important exception: After the Galileo trial, Jesuit astronomers read Tycho Brahe, who allowed the Earth to remain quiet. Even after the publication of the *Principia* they were supposed to do so. However, since they undertook to translate and publish explanations of the Newtonian system, we may

and before Copernicus was accepted they had read Ptolemy's *Almagest*. The *Principia*, Kepler's *Astronomia nova*, Copernicus' *De revolutionibus* and the *Almagest* functioned, each in their time, as those exemplars through which astronomers were trained to see the planetary movements, consciously and subconsciously, as astronomers could be expected to see them, and to analyze the problems of their field as currently done.

These books thus functioned in a way analogous to that of the paradigms of language training, which explains the origin of Kuhn's central term. As it often happens, however, the actual meaning of the term came to differ from the etymological origin—in fact already in Kuhn's own book. The paradigm concept, if it had only referred to the role of such books, would only have described an earlier stage in the development of the sciences. In modern times, natural scientists are trained by means of *textbooks* and prepared exercises, not by following immediately in the footsteps of the founding fathers of their field; they will only be confronted with original research papers at a relatively late stage of their education, and rarely at all with the classics[91]. In the humanities, early confrontation with research literature is customary; but one will seldom find (neither at present nor in the past) a field to be defined by *one* book to the same extent as physics was once defined by Newton's *Principia* and economics by *The Wealth of Nations*.

Even though Kuhn *does* use the term »paradigm« to denote the pivotal books which once defined their respective fields, he therefore mostly uses the term in as somewhat different sense, *viz* about that *collective attitude*, that *collective intuition*, those *shared techniques* and that »tacit knowledge«[92]

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presume that they used it just as much as other astronomers at least for training purposes, irrespective of conceivable metaphysical reservations.

[91] Quite a few biologists, of course, will read (passages from) Darwin's *Origin of Species*, some physicists may take a look at Galileo's *Discorsi*, and many economists may study some chapters from Smith's *Wealth of Nations*. But these classics are so removed from what goes on now in the respective fields that they can have no genuine training function; having them on your bookshelves and having looked into them is rather a way to affirm your professional identity.

[92] The concept of »tacit knowledge«, which has been amply used in explanations of the Kuhnian view, was created by the philosopher-chemist Michael Polanyi. The insights which (even then with a considerable delay) gained wide currency with

which natural scientists once got by working their way meticulously through THE BOOK, and which is now acquired in other ways. In a postscript to the second edition of his book, Kuhn even proposes to reserve the term *paradigm* for the shared »constellation of beliefs, values, techniques and so on« [1970: 175], and to label the »exemplary past achievements« instead *exemplars* for the sake of clarity.

The paradigm in the sense of a »constellation of beliefs ...« is a totality, and those constituents which can be brought forth through analysis will only direct and govern scientific work *because* they are parts of an integrated whole. Recognizing this restricted value of analysis, however, should not prevent us from having a look at the constitution of the whole.

One element of the paradigm may be familiarity with an exemplar, a fundamental work or group of central works. In certain cases this exemplar need not belong to the discipline itself—thus, the works of the anthropologist Claude Lévi-Strauss (*La Pensée sauvage* etc.) and the linguist Ferdinand de Saussure (*Cours de linguistique générale*) have played the role of exemplars for structuralist currents within many disciplines.

More important than the exemplar itself, however, is what you learn from it. The contribution of the exemplar to the paradigm may be found on several different levels. From Newton's *Principia*, e.g., you may learn about the actual movement of physical bodies influenced by forces. More generally, you learn that the forces acting upon one body originate with another body, and that the acceleration of the body multiplied by its mass equals the total force acting upon it; you learn mathematical techniques, and you learn that these techniques are the means by which you compute the movement of bodies. You learn a precise, »Euclidean« deductive construction of your line of argument, and thus that physics *may* be (and, implicitly, *should* be) constructed as a rigorous deductive progression of propositions and calculations. You learn that physical theory should relate to and explain phenomena, and you learn how to relate theory to phenomena.

From Adam Smith's *Wealth of Nations* (which functioned as an exemplar in classical British political economy) you also learn on several levels at

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Kuhn were thus not totally unprecedented—Polanyi is not the only precursor.

a time. You learn to divide the population of a country into social classes according to the source (not the quantity) of their income. You learn that the relevant sources are *wages* derived from work; *profits* derived from the possession of means of production (capital); and *rent*, derived from the possession of land); for which reason the classes are working class, capitalist class, and landed proprietors (cf. note 69). You learn about competition and its effects, and about the formation of monopolies and about their consequences. You learn about quantity of work as the factor which determines prices within a market economy. You learn that economic analysis presupposes social statistics and historical considerations. You learn a specific way to analyse and to argue.

What you learn from an exemplar may thus be summed up as follows:

— You learn about the kinds of entities which constitute the world of the discipline: Physical bodies, forces, ... / kinds of income, social classes, ... (in philosophical jargon: an *ontology*).

— You learn which types of explanations belong legitimately within the discipline—which explanations *should* be used by a physicist and an economist, respectively. Implicitly, you also learn which kinds of explanation should be avoided (the *moving intelligences of celestial bodies* and *just prices*, respectively, to mention kinds of explanations used before Newton and Smith).

— You learn about a number of techniques which can be used to attack the problems occurring within the discipline, and you learn how to use them.

— and you are provided with a total idea of what the world (of the discipline) looks like, a global perspective on (the pertinent) reality.[93]

The paradigm is thus related to the »hard core« of a Lakatosian research programme (no wonder, since the hard core is just Lakatos's explanation of the paradigm concept from a Popperian perspective). But there are important differences. The Kuhnian paradigm is not as precise and as precisely formalizable as Lakatos presumes his hard core to be. A »total

[93] In the postscript to the second edition of *The Structure* ... [1970: 187ff], Kuhn introduces some more precisely defined constituents of the paradigm; but since these are geared specifically to the paradigms of physical sciences they need not concern us here.

idea« and a »global perspective« cannot be summed up exhaustively in well-defined propositions. Learning »how to use« the techniques of a discipline is the acquisition of a skill; skills one may *speak about,* but a skill in itself is not something which can be enunciated (as can a theory or an ontological presupposition). Presupposing his hard core to be clearly expressible, Lakatos can imagine that a scientist may reject one research programme and start working upon another by a fully conscious choice. The idea of the paradigm as containing an prominent factor of training and skill, on the contrary, implies this shift to involve more of a new learning process and less of a free instantaneous choice (the choice actually involved is the choice to start learning anew, to assimilate a new perspective which *is not yet yours* and thus not fully understood in the moment you choose). Ultimately, the paradigm shift is not an individual affair but rather a process affecting the whole disciplinary community—»a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it«, as Kuhn [1970: 151] quotes Max Planck (the physicist who took the first step in the development of quantum mechanics in 1900). The paradigm involves elements of collective intuition, and intuition, as we all know, cannot be changed by deliberate choice or majority vote.

Becoming familiar with an exemplar is not the only way you learn to work within a paradigm. It is even questionable whether you learn it in full in that way. The kind of knowledge which is contained in the exemplar may contribute to the collective intuition; yet it is mainly through *working as* a physicist or an economist while using the exemplar as a navigation mark that you make the exemplar paradigmatically productive. This is why the contemporary training of natural scientists (and, to a large extent, economists, sociologists, linguists, etc.) can be successfully effected without exposition to exemplars but by means of textbooks and appurtenant exercises, the gist of which is that the exercise is to be performed as presupposed within the paradigm; and this is why many fields of human science can transmit their paradigms through exposing students to select pieces of current research literature combined with independent work.

The structure of scientific development

Kuhn's primary aim was never to describe the socialization of future research workers. It was to understand how scientific fields develop. But once the insight was gained that the dynamics of scientific development could not be understood unless the moment of *production* of this knowledge was taken into account (cf. the initial passage of Chapter III, p. 30), the socialization of workers turned out to be pivotal: The distinctive character of *scientific* knowledge must then depend, among other things, on the particular way workers within a field see and deal with this field, and hence on the process that makes them see it thus.

When a new field becomes the object of systematic (»scientific«) investigation for the first time, there *is* as yet no such particular way to see it and deal with it. Those who approach it do so from common sense understanding of its character and common sense definition of its contents[94], *and* from a general intention to understand it »scientifically«. As examples of such »pre-paradigmatic« (proto-)sciences one may take »women's studies« from around 1970 and the study of electrical phenomena between 1600 and 1750.

In a pre-paradigmatic science the approaches are multiple and uncoordinated. The results obtained by one worker will normally build on presuppositions and refer to concepts which are not shared by others, and others will therefore have difficulties in assimilating them. Instead they will tend to be neglected and eventually forgotten, maybe to be rediscovered 20 or 40 years later[95]. Borrowing the Piagetian idiom we may

[94] With the proviso that they will often have been trained as scholars within other fields. Their »common sense« is thus the common sense of the general scholarly community as tainted by their specific training within particular fields. As a friend of mine once asserted about a former physicist who had gone into peace research and from there into sociology, where she had met him as a teacher: »A. is a physicist; he will never be anything but a physicist«.

[95] This is, for instance, what happened to Vladimir Propp's analysis of the invariable morphology of (Russian) fairy tales from 1928: It only became influential in the 1950s, when Lévi-Strauss and others had established the structuralist paradigm—within which, by the way, the implications of Propp's findings were interpreted in a way which differed decisively from Propp's original »diffusionist« understanding of the matter. Cf. the prefaces and introductions to [Propp 1968] and [1984].

say that the workers in the field possess no common cognitive structure which is fit to integrate unexpected results and to keep them available for further use and elaboration by the community as a whole. Pre-paradigmatic sciences are not *cumulative*—at best, single schools with a certain inner coherence (as found, e.g., in women's studies from the mid-seventies onward) exhibit cumulative characteristics.

It may happen, however, that a particular contribution or a specific school obtains so convincing results that other workers of the field accept its approach as an exemplar, trying to emulate it (the precise nature of the contribution is irrelevant, as long as it only convinces and is able to transmit *some* relatively coherent approach to the field). This breakthrough may start the development of a genuine paradigm, and as long as this paradigm serves, the field is in a phase of *normal science*.

During such a phase, work is directed at *expanding the paradigm:* to understand more and more of reality through the paradigm. One may speak of applying the theory to new areas, or of developing new theory for new areas on the basis of the paradigm; the latter formulation may be preferable, because the expansion to new areas may require addition of new concepts and presuppositions, and an *articulation* of the paradigm with regard to its original content by which it is made more precise, explicit and conceptually clear.

This articulation is the other aspect of what goes on during the normal science phase. Clarification of concepts and increasing adaptation of the paradigm to that reality which is studied may result as secondary effects of the expansion of the paradigm—if you apply the outlook generated within women's studies to the situation of sexual minorities or suppressed racial groups, then you get a new perspective even on the original core of your field (to the point where certain feminists have come speak of women as a *minority*, regardless of actual numbers), and you get new skill in dealing with it. Similarly if you apply Newton's laws to the flow of water through pipes, or the principles of structural phonology to analysis of kinship structures. But articulation may also follow from conscious efforts to get better insight into the foundation of earlier results.

Much work within normal science is concerned with the solution of »puzzles« (Kuhn's term). The metaphor refers to such everyday phenomena as riddles, crossword puzzles and chess problems. In all of these we know that a solution exists; it is only up to our ingenuity to find it. The same thing characterizes normal science: since the paradigm »knows« what the world (of the discipline) consists of and which types of relations hold good between its constituents, all problems within this world *must* (if we are to believe the implicit claim of our paradigm) be solvable; the question which remains open is whether *you* are smart enough to find it. If a problem resists your efforts, at least the colleagues in your field will conclude that you were not—after all, they know from proper experience that the paradigm is fully efficient for all relevant purposes. Only if others fail like you did will the problem cease to be a mere puzzle and become an *anomaly* which challenges the paradigm.

The appearance of an anomaly may lead to focusing of work on precisely this stubborn problem (cf. Lakatos story as told above), and then perhaps to a solution; or it may remain unsolved, and if the paradigm remains effective in other respects it may then be encapsulated, while work goes on under the paradigm without being disturbed[96].

The puzzles which are taken up during a phase of normal science are not selected at random. In combination, the global view which the paradigm gives of the constitution of its object and the array of results obtained until a given moment will suggest which problems are *now* most urgent and most likely to be solved[97]. This explains the recurrent phenomenon of *simultaneous discovery:* extremely often, the same essential discovery is made by workers who have no direct connection and indeed nothing in common beyond a shared paradigm; this, however, is enough

[96] This happened to the discovery that the perihelion of the planet Mercury (the point where it comes closest to the Sun) rotates (»precesses«) in a way which cannot be explained by Newtonian mechanics. The anomalous precession was discovered in the early nineteenth century, and everything suggested that it *should be* explainable. It was not, and was thus shelved—and was ultimately solved by the General Theory of Relativity a full century later.

[97] This process is seen *en miniature* each time a professor allots thesis topics to doctoral students. The teacher is expected to know in advance which questions are now solvable: it would be irresponsible to make students run on a track leading nowhere, but equally irresponsible to make them repeat what has already been done. The teacher is, so to speak, supposed to be the paradigm in person.

to make them take up the same problem, and provides them with sufficient knowledge about what should be expected to make them see an actual outcome as epoch-making.

Pre-paradigmatic science was not cumulative. Normal science is. Stubbornly, it sweeps up everything which the paradigm is able to interpret, shelving anomalies encountered in one direction if it is still successful in others. Eventually, however, anomalies accumulate and tend to turn up at all essential points. The efficiency of the paradigm for puzzle solution shrinks, and during the effort to explain one or another anomaly, the paradigm is articulated in increasingly divergent fashions. Eventually, without having been given up the paradigm may exist in almost as many versions as there are active workers within the field; ultimately, this will of course undermine its credibility (and obliterate its character of *shared* beliefs etc.). The field will end up in a state of *crisis*, where doubt about the efficiency of the paradigm grows into general distrust of its previous accomplishments: If phlogiston theory runs into paradoxes when trying to account for specific weights, are we then really entitled to believe in its explanations of colours? Are these not likely to be spurious and only accidentally in agreement with observations? The willingness to engage in quite new approaches spreads, varied proposals come up, for a while different schools may exist alongside each other. In many ways, the situation is similar to that of the pre-paradigmatic phase. Only when one approach has proved its ability to solve precisely those problems which had become central during the crisis period (and only if competitors are unable to do it as satisfactorily) will this approach come to serve as the starting point for a new paradigm, inaugurating a new phase of cumulative normal science.

The shift from one paradigm to the next constitutes a *scientific revolution*, which is characterized by sharp rupture. Taking the Copernican Revolution as an example, Kuhn [1970: 149f] suggests that we consider

> the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they meant by »earth« was fixed position. Their earth, at least, could not be moved. Correspondingly, Copernicus' innovation was not simply to move the earth. Rather, it was a whole new way of regarding the problems of physics and

astronomy, one that necessarily changed the meaning of both »earth« and »motion«. Without those changes the concept of a moving earth was mad.

The content of a concept is only partially to be derived from empirical observations (cf. what was said above in the discussion of the problems of empiricism); in part it depends on the total theoretical structure in which it partakes and the practice inside which it serves[98]. *Mutatis mutandis*, the observations on the changing meaning of terms must therefore hold for all paradigm shifts. The discourses before and after a change of paradigm (or across a paradigmatic border) are »incommensurable«. A conference may »bring people to talk to each others who would never read each other's papers«[99]; but it was my definite impression on the occasion where this was formulated that they did not understand each other too well.

Many of Kuhn's early critics (and quite a few superficial followers in later years) take the claim for incompatibility to imply that no communication and no rational argumentation is possible across the paradigmatic border. This is evidently a wrong conclusion, built among other things on an absolutistic concept of rationality, and it was never intended by Kuhn[100]. Breakdown of communication is *partial*. This suffices to exclude unambiguous *proofs* that one part is right and the other is wrong; but it does not prevent critical, rational discussion, where appeal can be made to

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[98] In the sciences, this practice is in part constituted by the research process, in part by teaching and applications. For the astronomers of the later sixteenth century, astronomy teaching in universities (which *had* to be traditional) and the computation of planetary positions to be used court astrology (which by necessity asked for these positions as seen from the Earth) were no less weighty than astronomical research (see [Westman 1980]).

[99] Mogens Trolle Larsen, formulated at the dinner table the last evening of the symposium »Relations between the Near East, the Mediterranean World and Europe—3rd to 1st millennium BC«, Århus 1980. The participants were mostly archaeologists falling in two groups: those oriented toward social anthropology and the use of statistical analysis on the distribution of finds—and those for whom »the only facts are artefacts«, i.e., for whom archaeology should make no theorizing about societies and their structure and interaction but simply dig and describe the finds and their stratification meticulously.

During the symposium, an exasperated member of the former group commented upon the attitude of the latter with the phrase »Oh yes, the only acts are artefacts!«; the immediate answer was a simple »Yes, of course«.

[100] In his postscript to the second edition of his *Structure ...*, Kuhn [1974: 198ff] takes up in some detail the problem of incommensurability and the misunderstandings to which his original statements had led.

those cognitive structures which are shared across the border[101]. The situation bears some similarity to the description of the same situation in two different languages possessing non-isomorphic conceptual structures[102].

The analogies between Kuhn's and Lakatos's formulations are evident. As already stated, the *paradigm* corresponds to the *research programme*,

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[101] The *only partial* breakdown of communication distinguishes Kuhn's analysis of the paradigm shift from two apparently related lines of thought: Wittgenstein's notion of »language games«, and Foucault's »archaeology of knowledge«. Wittgenstein's analysis [1968: §11 onwards] comes close to Kuhn's in pointing out that a language game is connected to and rooted in a *particular* practice; but it leaves no space for description of the process by which one »paradigmatic language game« develops into another (for good reasons, since this is only a characteristic of certain »games«, like the paradigms of scientific disciplines and—with some modification—artistic »schools«), and it leaves aside how the practice underlying the language game is itself to some extent a result of the game.

Foucault's analysis produces a much coarser grid than Wittgenstein's multiple coexisting language games. He speaks [1966: 13f] about two prominent discontinuities in the Western *episteme*, one around the mid-seventeenth century (which we may connect to the reception of Descartes and Galilei) and one in the early nineteenth century (the epoch, e.g., of Comtean positivism). But he is even more explicit than Wittgenstein in his statement that seeming continuities within single sciences over one of these watersheds (e.g., between the »general grammar« of the mid-seventeenth century and modern linguistics) are nothing but surface effects; in Foucault's view, Linné's biology has much more in common with seventeenth-century *Grammaire générale* than with Cuvier's comparative anatomy or Darwin's theory of evolution. This may be quite true, but only in a perspective which concentrates on other aspects of the disciplines in question than their relation to their object, and which *eo ipso* (as also stated by Foucault) excludes any idea of cognitive progress through the shift, and indeed any critical communication. Maliciously one might maintain that Foucault is only right (in this respect) under the perspective where it does not matter whether what he says (in this and any other respect) is right, only that the way in which he says it reflects a particular French intellectual style and the make-up of the French book market; one need not be a follower of Foucault to find this to be a distorting and reductive perspective.

[102] Cf. the relation between the conceptual clusters »knowledge/cognition« and »Wissen/Erkenntnis/Erkenntnisvermögen«. »Cognition« encompasses only little of what is covered by »Erkenntnis« and most (all?) of what is meant by »Erkenntnisvermögen«, and »knowledge« correspondingly more than »Wissen«. This is one among several linguistic reasons (non-linguistic reasons can be found) that epistemology looks different in English and German; still, translations *can* be made which convey most of a German message to an English-speaking public.

normal science to *work within a research programme*. Still, differences are no less conspicuous[103]. One of them turns up if we look for the analogue of Lakatos's »hard core«. A paradigm possesses no hard core, no sharp distinction between the absolutely inviolable and that which can be freely reinterpreted and changed in order to obtain better agreement with observations. All levels of a paradigm may be affected by articulation.

In spite of articulation, however, the main task which normal science sets itself is the solution of puzzles, where the paradigm not only »ensures« (i.e., assures) that solution is possible but also mostly tells what the approximate outcome will be (in experimental or other empirical research), or how an explanation will have to look (in theoretical investigations). If things turn out in a totally unexpected way, and if they cannot be explained even with hindsight to agree with what could be expected (that is, if they constitute an anomaly), the results will often be neglected (as told above). Rebellious thought is rare, »dogmatism« prevails.

This agrees badly with common sense ideals concerning the character of science and the behaviour of scientists (not to speak of Popper's rhetoric). It is also at variance with the way scientists experience their own work: the efforts to grasp things in new ways, the struggle to get around apparently impossible obstacles and the eventual success by means of a sudden deep insight—these are predominant features. How comes?

The latter problem may be postponed for a while. But the first, »how science can make progress if it is so rigid and dogmatic«, should be approached now.

For one thing, the »dogmatism« of normal science does *not* imply that the exemplar (or the textbook) is regarded as sacred scripture which *cannot* be wrong. Firstly, the very principle of cumulative science is to use preceding knowledge (including the exemplar) in order to succeed where predecessors (including again the exemplar) have failed. The attitude is

[103] Analogies and differences taken together illustrate the partial yet only partial breakdown of communication between incommensurable paradigms. Where Lakatos will see a research programme and look for its hard core, its positive and negative heuristic, etc., all of which can be put on paper, the Kuhnian will look for interpretations and collective understandings and intuitions, which can only be described with approximation, and which together constitute a seamless whole.

nicely summed up in the statement that we are like »dwarfs perched on the shoulders of giants [seeing] more and farther than our predecessors, not because we have keener vision or greater height, but because we are lifted up and borne aloft on their gigantic stature«[104]. Secondly, what is learned from the exemplar is not specific sacrosanct results but a general and open-ended way of thinking (in Aristotelian jargon: not the content but the *form* of the exemplar is important); nothing prevents workers from using this thinking to correct concrete mistakes committed in the exemplar.

But normal science is not only functional because it allows the errors of an original accomplishment to be corrected; it ensures that the carrying capacity of the paradigm is tried out to the full, and that it is not rejected at the encounter of the first apparent anomaly or the first change of fashion. It regulates and structures systematic examination of the field which it covers (as opposed to what takes place in the pre-paradigmatic phase, where work is unsystematic, unstructured, and largely ineffective); finally, and for the same reasons, the paradigm is a most efficient instrument for bringing forth and establishing the anomalies that eventually make it break down.

The latter point is contained in an aphorism which Engels formulated long before Kuhn: »In chemistry, only a century's work according to the phlogistic theory supplied the material which allowed Lavoisier to discover in the oxygen that Priestley had produced the real counterpart of the imaginary phlogiston substance, and thus to throw over the whole phlogiston theory«[105]. Approbation of the »dogmatism« of normal science is thus no conservative attitude, and no endorsement of static thinking; it is connected to a view according to which scientific progress comes from that »essential tension« of which Kuhn speaks in the title of another book, and not from gratuitous rhetoric à la Popper (cf. the quotations in note 87 and on p. 119).

[104] Bernard of Chartres (c. 1120), quoted by John of Salisbury in *Metalogicon* III, 4 (transl. [McGarry 1971: 167]).

[105] *Dialektik der Natur*, MEW 20, 335f. Another »Kuhnian« point is also contained in the passage: Priestley produced oxygen, but understood it within the framework of the phlogiston theory as »phlogiston-free air«; only Lavoisier »invented« oxygen, thus engendering the new paradigm.

Collective and individual knowledge

The question may be shifted somewhat. A theory of scientific development is a theory about the production of collective knowledge. There is thus nothing strange if scientific development exhibits features which are similar to the characteristics of other types of cognition. And even here, stability appears as a prerequisite for development and change.

Let us first have a brief look at *art*—postponing to a later chapter the intricate question which (if any) kind of knowing is involved in art. A comparison between scientists working within a paradigm and artists belonging to the same »school« or tendency (»impressionists«, »serial composers«, »absurd drama«) is close at hand. Even here it is obvious that working out the possibilities of one school is one of the factors which make innovation and even rupture possible[106].

Another parallel may be followed further and more precisely. If we replace Kuhn's »paradigm« with »scheme«, his »expansion of the paradigm« with »assimilation to the scheme«, and the »articulation of the paradigm« with »accommodation«, we shall get the gross structure of Piaget's epistemology. Even here, as we know, the child is only able to search for and gain knowledge because it possesses a cognitive structure organizing the search and the transformation of sense impressions into comprehensible *experience*. Only because this structure exists and is relatively stable can it create the conditions for its own replacement by a higher structure.

In individual cognitive development, the cognitive structure is evidently individual, even though, e.g., the over-all character of one individual's pre-operatory cognitive structure is very similar to that of another individual. But in that process of knowing collectively which is the essence of the scientific endeavour, the cognitive structure must by necessity be shared. At the same time, its development is not regulated and monitored by a pre-existing language and set of concepts, a pre-established stock of relevant

[106] Looking with historical hindsight at Cézanne's or Marie Krøyer's paintings, one will easily see *cubism* working itself toward the surface; mindful listening to Richard Strauß's early operas or even to Schönberg's still Late Romantic *Gurre-Lieder* may make one understand why Schönberg came to feel the need for his dodecaphonic technique.

everyday experience, and an already unfolded life-world (although, evidently, all of these are there and contribute to the formation of the scientist's mind). The collective cognitive structure must be brought forth by the scientific community itself—and this is precisely what is done through the establishment of the paradigm, through the common reading of and work from exemplars, educational »finger exercises«, etc.

It is an important feature of Piaget's epistemology that the cognitive structure is not constituted by conscious and explicit knowledge; it belongs at the level of the cognitive unconscious. That this is so is an *empirical fact* as good as any that can be established by psychology. If we oppose Kuhn's and Lakatos's approaches, we will remember one of the important contrasts to be that Kuhn supposes the paradigm to consist much less of formulated theory and explicit statements than Lakatos assumes regarding his »hard core«. According to all we know about general human cognition, Kuhn's view is thus more empirically plausible than the alternative; similarly, the parallel suggests that those structures which both see as mandatory for scientific development result to a considerable extent from socialization and training, and not exclusively from conscious choice. On the other hand, scientific knowledge in stored form presents itself in explicit and relatively unambiguous statements[107]. Important elements of the paradigm/the hard core must therefore consist of clearly expressible statements. Part—but only part—of the paradigm consists of »tacit knowledge«.

From the discussion of the relation between sensorimotor schemes and operatory thought (Chapter II) it will be remembered that whole structures (e.g., »Riding a bicycle«) may be made subservient to conscious (e.g., operatory) thought (»in order to get to Roskilde I shall have to get at the

[107] Some ambiguity remains. Reading research papers from an unfamiliar discipline is difficult, not only because you have not read the textbooks but also because you have not been brought up in a way which makes you understand the hints, connotations and implicit arguments contained in the texts. Reversely, writing out from your own discipline may be difficult not only because you have to present simplified versions of that textbook knowledge which your readers do not possess but also because you have to bring your own implicit knowledge to awareness; if you do not, you will neither be able to explain it explicitly or nor to communicate with the readers' implicit knowledge by means of hints, connotations and metaphors.

train at Østerport; to Østerport I may ride on bicycle«) without being themselves brought to awareness, i.e., without requiring conscious reflection regarding details (e.g., the problem of balance, coordination of the feet, ...). The way in which scientific work integrates sub-functions assimilated during professional upbringing (laboratory technique; the way a literary scholar reads a novel) appears to be more than a vague analogue of this aspect of general human cognition and problem-solving.

Above, we touched at the conflict between Kuhn's description of normal science and the participants' own experience of the situation: even within normal science the worker will often feel his activity to be a continuous struggle with the material, which is only brought to success by means of new ideas. The apparent paradox may be elucidated through this parallel between scientific and general cognition and the displacement of routines from the focus of awareness. According to Piaget's epistemology, we remember, *every* act of knowing is at the same time assimilative and accommodative. Every new item of knowledge which is assimilated to a scheme alters this scheme, at least by extending its scope—cf. *Aha-Erlebnisse* of the kind »My God! This thing that has puzzled me for so long is really nothing but ...« and »Oh, *that* is how it is to be understood«.

Scientific processes of knowing carry the same Janus face. The historian of science who looks at the first applications of functionalist explanations within the sociology of science will tend to see the assimilative aspect of the event: »here, the sociology of science simply expands the familiar anthropological paradigm so as to cover a new range of phenomena—typical normal science«. As a practising sociologist you will see things differently. The application of the paradigm, of everything which is self-evident, of all your tacit knowledge, will not be in focus. You may be aware of this aspect of the matter, precisely as you know that you use your typewriter and your typing skill when putting your results in writing. But why bother about such peripheral and trivial matters if you are to tell the crux of your endeavour. The crux is clearly the thing which was *difficult*, that which did not go by itself, the *new* ideas and the reinterpretations of familiar concepts which were necessary before a theory explaining the functioning of religious rituals and kinship structures in tribal societies could be used to explain patterns of scientific communication[108].

Such extensions of our scientific knowledge which are *only* assimilative, which require no cognitive initiative whatsoever but only trite routine, will normally be regarded as mere *application* of science (to be distinguished from »applied science«, cf. below, n. 162) and not as genuine scientific activity. One reason that many ideologues of science (not least Popperians) reacted so strongly against the concept of normal science will probably have been that Kuhn, through his emphasis on assimilation as an important aspect of the process, appeared to equate most scientific activity with what they see as a rather boring and unimaginative routine[109].

Two kinds of »logic«

The parallel between Kuhn's specific and Piaget's general epistemology may carry universal implications. The similarity between the development of collective scientific knowledge and individual knowledge suggests the double (assimilative/accommodative) constitution of both to correspond to a necessary characteristic of human knowledge. Seen in this light, Kuhn's theory turns out to be a »logic« for the development of scientific knowledge as a *social, productive* process[110]. What Lakatos has formulated is (in

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[108] Another aphorism may highlight the matter: »In normal science, 95% of everything is routine; during a scientific revolution, only 90% is routine« (Donald T. Campbell, at the »Symposium on Evolutionary Epistemology«, Ghent 1984).

[109] Thus John Watkins, in an insipid panegyric of Popper's genius [1974: 32]: »The careful drawing up of a horoscope, or of an astrological calendar, fits Kuhn's idea of Normal Research rather nicely«. Or Popper, in an otherwise much more interesting essay [1974: 53]: »The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He [...] has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*« (Popper's emphasis).

[110] And still, in fact, a »rational reconstruction« and no faithful rendition of actual historical processes. A dozen of years ago, much fun was made of the fact that the word »paradigm« did not turn up at all in Kuhn's book [1978] about Max Planck's first steps toward quantum theory. Without reason, I would say: it is quite legitimate to derive an overall structural »logic« from the shimmering of real historical processes—but this logic should not necessarily be used to redraw in black lines the contours of the single historical process, thus eliminating the

agreement with his Popperian starting point) rather a logic for the development of (stored) knowledge viewed as an abstract process, in which the productive mediating role of the working scientific community between one stage of stored knowledge and the next is regarded as immaterial.

At the same time (and in the same moment), Lakatos understands »knowledge« in a way which excludes creativity and fantasy from the domain which epistemological theory can legitimately investigate. Kuhn, on the other hand, who regards the production of knowledge as carried out by actual human beings, opens up the possibility that the creative and the systematic aspects of the process of knowing may be regarded together—even though he makes no remarkable suggestions himself in this direction, and does not want to do so.

Objections and further meditations

The primary purpose of the present pages is neither to present a survey of the opinions of Piaget, Popper, Kuhn and others, nor to investigate systematically what may be the insufficiencies of their theories. It is to present a general (though neither complete nor encyclopedic) view of the characteristics of scientific knowledge and of the social process in which it is established, and presentation as well as critical discussion of the theories are subservient to this aim.

Even for the purpose of establishing a general view, however, it is worthwhile to consider some of the problems left open by Kuhn or even called into existence by his work.

A problem of the latter type presents itself when Kuhn's arguments about the efficiency of normal science are used for science policy purposes (in a way Kuhn would never do himself, it must be stressed). It it be mentioned first because it justifies some of the dismay called forth by Kuhn's *Revolution*.

The scientific understanding of a problem area (say, the failures of education) does not really progress as long as it stays in the pre-paradigmatic phase where many approaches compete—thus certain policy-makers' (so far sensible) reading of Kuhn. If, however, this area has become

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shimmering from view.

socially important or politically hot, then we'd better know something scientifically about it in order to implement a sensible policy—thus managerial rationality since 1945 (more about this in Chapter VII). Alas, research about the area (educational studies) is so obviously pre- or at least non-paradigmatic that we cannot expect genuine progress to take place within a reasonable time horizon; then *let us do something about it*, and declare one of the approaches to be the paradigm, and channel all research monies accordingly—thus the conclusion.

This line of argument may seem attractive to bureaucrats, whether professional officials or academic members of advisory bodies. It should be obvious from the above, however, that the reasoning is highly fallacious: paradigms indeed acquire their status by *deserving it*, by convincing workers in the field of their efficiency; and they loose it (and *should* loose it, irrespective of the preferences of grant-giving authorities) when no longer convincing. Better perhaps: the underlying epistemology is conventionalism and not Kuhnian dialectic—if all theoretical approaches are equally valid, then bureaucrats can be allowed without risk to choose, and there can be no serious objection to their choosing the one which seems most immediately promising or which agrees best with their preconceived ideas.

Updated versions of conventionalism in Kuhnian disguise are not too rare, but not of urgent interest in the present context. We shall thus leave further discussion of this matter aside, and turn to issues deriving from what Kuhn actually says.

Firstly to a question which has been emphasized much more in the discussion than it really deserves, but which has the merit to suggest further reflection: during phases of Kuhnian normal science, disciplines are as a rule dominated by a single paradigm; Lakatosian disciplines, on their part, are normally split between discordant research programmes. The view appears to depend heavily on the eyes?

In any case, the view depends critically on the direction in which you look, and the way you describe it on the sense you give to your words—*in casu* of the ambiguous term »discipline«. The hard core of a research programme, we remember, encompasses among other things a distinctive view of reality and hence a specific demarcation of the discipline. Competing research programmes within (what university administrations and

Lakatos see as) the same discipline may therefore define themselves so divergently that it makes better sense to speak about *competing disciplines* dominated each by its own paradigm[111]. Much depends on the question whether disciplines are to be defined institutionally (e.g., from appurtenance to specific university departments) or cognitively.

But much also depend on the choice of prototype disciplines. Kuhn tends to choose his from the physical sciences: Astronomy, physics, chemistry. Lakatos, on his part, looks more often to softer sciences (cf. my illustrations through economics or philosophy of science)—or to phases in the development of physical sciences where Kuhn would see a crisis or a pre-paradigmatic field of research.

All in all, the paradox dissolves into disagreement about concepts (a typical case of »incompatibility«) and about the delimitation of the »typical«. It points, however, to a much more fundamental question: How much, and how many, fall under a specific paradigm?

As an example I shall take my own situation in 1969, at the moment where I finished my master thesis in high energy physics. I was associated with a subdiscipline comprising at most around 100 publishing participants. Everybody within this circle followed closely[112] everything that was done under a paradigm which had been born in 1967 and was still under articulation (it never matured before it was superseded), and we had our own quite distinct methods and our own argot. Evidently, other methods and techniques we shared with other high energy physicists, under what can be described as an open-ended paradigm ten to fifteen years old. A common trunk we shared with physicists in general, e.g. quantum

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[111] Cf. the two kinds of archaeologists described in note 99. People who »would never read each other's papers« are not really members of the same discipline, even though they may have their positions in the same university department.

That two disciplines compete about understanding the same section of real-life reality does not imply that they have to be understood as two variants of the same discipline—cf. the unending discussion between psychiatrists and psychologists about who has the better understanding and who dispenses the correct treatment of the same patients. As discussed below (Chapter VII), disciplines only form through specific approaches.

[112] At one moment the head of my working group, the late Koba Ziro, presented a publication as »several weeks old«.

mechanics (brought to maturity between 1926 and 1935) and the theory of relativity (1905-1912).

Kuhn speaks about *the* paradigm. If so, how much belonged to the paradigm under which I worked, and how many were my companions?

One may observe (Kuhn does) that quantum mechanics *when seen as a paradigm* (i.e. as something to which you are socialized, not as a body of formulae or theory) is not the same thing in chemistry, solid state physics and high energy physics. The way you have *learned to work* with quantum mechanics differs from one field to the other. Quantum mechanics is a constituent of all three paradigms, but not the same quantum mechanics[113].

The consequence of this point of view is that the paradigm characterizes the small unit with its 100–200 publishing participants. Normal science phases hence become relatively short, and revolutions rather frequent—but also rather moderate. A revolution in the small unit to which you belong does not imply that you have to learn everything anew: those who replaced »my« paradigm with the first version of string theory could continue to use quantum mechanics much as they had done before.

Since even much of that knowledge and many of those professional skills which are conserved through a revolution of the »local« paradigm share the characteristics of a paradigm (resulting as they do from socialization, training and practice), it may be reasonable to modify the idea that the scientists is submitted to a single paradigm, and to look at him as a member of several circles: some broader and others narrower, some intersecting. Each circle shares a cognitive pattern of paradigmatic character, and single workers may well experience a revolution in their local circle without being for that reason forced to rethink and relearn everything in their professional practice. Such things only happen when the larger circles are struck by a revolution: no scientifically living branch of physics (if we stick to that example) was practiced in the same way after the maturation of quantum physics and relativity theory as before 1900; even the structure

[113] This I can confirm from personal experience: the quantum mechanics I taught to students of chemical engineering dealt mainly with concepts and techniques I had never heard about when studying physics. To be able to teach it adequately I had to work hard.

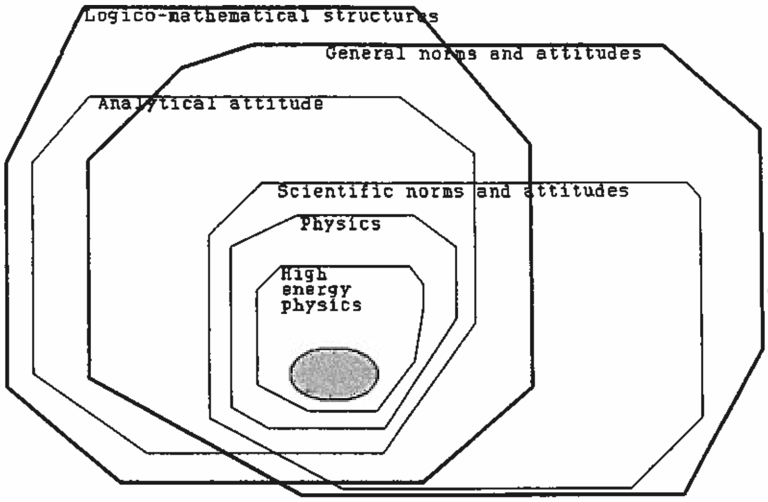
of subdisciplines was thoroughly revised, many specialties disappeared and many more emerged.

Whether one wants to reserve the term »paradigm« for that which is shared by the small unit or one accepts the notion of paradigmatic circles or levels may be a matter of taste. Whatever our choice we should remember, however, that the complete cognitive structure of the individual scientist is, precisely, *a structure* and not exhaustively described as a paradigm cast in a single piece (cf. the diagram on the following page, which may seem complex but is actually utterly simplified and only meant to be suggestive). We have to acknowledge that some of its constituents are shared with everybody else who has attained fully mature operatory thought; some–not least an analytical attitude–are shared by scientists from other disciplines; some may be shared by members of certain disciplines but not by the members of others (we remember how physicists and psychologists fell into one group in a Piagetian experiment and mathematicians and logicians into another, see note 30). Some elements of a physicist's paradigm he will share with other physicists since Galileo: I recall my own awe when reading Galileo's *Discorsi* as a young student—here spoke an eminent *physicist*, however much his theories have been buried under repeated paradigm replacement. Many, of course, he will only have in common with contemporaries or with other members of his sub-discipline (the Kuhnian paradigm *stricto sensu*).

Beyond these elements, the diagram refers to »general« and »scientific norms and attitudes«[114]. As will argued below (Chapter VI), it is impossible to separate completely normative or moral attitudes from the cognitive structure—as a matter of fact, much of the paradigm or the hard core *is* of normative/quasi-moral type, prescribing what *should* be done and looked for.

The composite nature of the single scientist's cognitive structure provides us with a scheme inside which *creativity* can be accounted for. If you encounter a problem within your discipline—be it one requiring

[114] These, we may note in passing, encompass (but are not exhausted by) what makes it possible for Foucault to see the multitude of sciences of a certain epoch as representatives of a single *episteme*—cf. note 101.



The embedding of a high energy physicist's paradigm. The shaded area corresponds to the paradigm of his small unit.

the »5% creativity« of normal science or the »10% creativity« of a scientific revolution—this discipline does not constitute your sole cognitive resource. You will indeed have been trained in many practices beyond your scientific specialty and have acquired a wide array of skills and patterns of thought through these processes; in many cases, these provide you with models which you may transfer to the solution of your scientific problem and use in combination with what you know from you paradigm[115].

[115] Since scientists rarely tell the sources for their ideas except when these sources seem »honourable«, i.e., scientifically relevant, I shall have recourse once again to an example from my own experience:

In the early 1980s I began working with the corpus of mathematical cuneiform »algebra« texts from the early second millennium BC. I was soon led to a complete reinterpretation of almost every term and technique. *If* more than a dozen of people around the world had been active in the field, this could have been characterized as a (local) revolution; in the wider contexts of the history of mathematics or assyriology it looks more like the assimilation of the field to an anti-anachronistic paradigm which looks at Greek geometry or Babylonian laws not as incomplete forerunners of *our* thinking but as expressions of the culture within which they were created.

But neither assyriology nor the anti-anachronistic ideal provided me with the tools which allowed me to understand the Babylonian texts. One aspect of my method was instead *structural semantics;* that, however, I did not know at the time, and my improvised method was instead inspired from my supervision of student projects analyzing literature structurally (even this I did not think about in the process, but it could be seen with hindsight). Another aspect was *hermeneutic reading*—yet not taken over from what I knew about hermeneutics but an application of the way I had once used to read wrong answers to mathematical exercises closely in order to discover the underlying reasoning and thus be able to make their errors pedagogically fruitful. At least one visualization of a procedure I might have taken from many places, but I happened to borrow it from my half-forgotten particle physics via a different use I had made of it in an analysis of ancient Egyptian mathematics.

However frivolous these inspirations may seem, they can all came be traced unambiguously to various kinds of professional experience of paradigmatic character. The anti-anachronistic drive, on its part, did not come from any allegiance to a historicist programme, but rather (as far as I can see—but at such points introspection becomes suspect, as any psychoanalyst can tell) from deep-rooted personal norms and attitudes *also* reflected in the way I once read my students' mathematics exercises.

Innovation and creativity cannot be reduced to mere heaping of such accidental extra-disciplinary inspirations, and extra-disciplinary inspirations change when they are brought together and applied in a new context (my hermeneutic readings of Babylonian texts were certainly more analytical than those I made intuitively as a mathematics teacher). But the anecdote will hopefully show that the fine structure of the paradigm provides us with a framework within which the creative process can be discussed and thus, to some extent, *understood*.

V. TRUTH, CAUSALITY AND OBJECTIVITY

Enlightened by the discussion of the making of scientific knowledge we shall now return to three classical issues: the problem of *truth* or correspondence between theoretical statements and reality (where we shall have to connect a number of discussions and reflections from the previous chapters); the nature of *causality;* and the question of *objectivity* versus *subjectivity*.

Truth

As all empiricists, the logical empiricists worked from the implicit premiss that the correspondence between the single observed fact and the statement of that fact was straightforward[116]. For them, as for the whole philosophical current descending from them, the »theory of truth« is concerned with how truth values can be ascribed consistently to the sentences of a formal language (cf. note 46), and not with the question of »agreement with facts«.

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[116] That any philosopher would do so after Kant may seem astonishing: whatever one thinks of Kant's solution, he should recognize the existence of a problem. If we can only know by imposing *our* categories on the world, then simple correspondence appears to be excluded.

Yet the explanation need not be neglect of Kant's insight (even though the logical empiricists wanted to render earlier philosophy superfluous rather than continuing it). It could also be that they took Kant fully to the letter: categories which we cannot help applying in an invariable form which we cannot influence may be counted as a part of that reality which it is the aim of science to describe—»observed facts« are procisely *observed* facts. Such a solution had already been proposed by d'Alembert in the preface to the *Encyclopédie*, decades before Kant formulated the problem (see [Høyrup 1993: 207]).

The position of Popper$_0$ is similar: the difference between the two approaches hinges not on any disagreement concerning the »naïve« correspondence theory concerning elementary facts but on the explanation of the way from single statements of facts to theory. Popper$_1$ as well as the later phases of logical empiricism came to admit that »naïve« correspondence does not hold water, but none of them succeeded in creating a credible »critical« substitute. The consequence drawn by the logical empiricists instead verged toward scepticism, while Popper's later writings lean toward an unacknowledged instrumentalism (cf. note 50).

In classical philosophical terms, the consequences drawn by Kuhn from *his* view of scientific development would also have to be characterized as sceptical. He argues strongly that scientific progress is real, but in the sense that »later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied« [1970: 206]. But scientific theories do not come closer and closer to truth through successive revolutions, as common sense would have it. Scientific progress, like Darwinian evolution, is instead to be understood as a steady movement »*from* primitive beginnings but *toward* no goal« [1970: 172].

At closer inspection, the truth which Kuhn cannot discover as the goal toward which scientific development moves turns out to be *an ontology*, a »match, that is, between the entities with which the theory populates nature and what is 'really there'« [1970: 206]. So far Kuhn is indubitably right. Newton introduced *forces* into nature, and Einstein's general theory of relativity abolished them again; if Newton's move was one toward greater ontological truth, and forces are »really there«, then Einstein's move was in the wrong direction. If Lavoisier's explanation of combustion as absorption of oxygen constituted ontological progress, then the acceptance of the phlogiston paradigm had been a mere error, irrespective of its actual successes (including its role in Priestley's production of oxygen).

In the case of competing paradigms approaching the same subject-matter in different ways we are no better off. Word classes, e.g., may be defined from morphology and meaning or from syntactical function. If we choose to interpret, e.g., the form *gone* in »Peter is gone«, as a conjugated form of the verb *go*, and if we follow this principle throughout, we get one type of insight into the structure of language; if we choose to interpret it as an adjective because it is parallel to *clever* in »Peter is clever«, we get other insights[117]. If one of the two approaches is ontologically correct, then the insights gained from the alternative approach are spurious and not to be relied upon.

Certainly, situations exist where one approach turns out to be mistaken and the other not (or not clearly) so. In other cases, however, later developments show that none of the two was quite mistaken. Above (p. 72), the interpretation of the phlogiston theory in terms of »degrees of oxidation« was mentioned. In linguistics, we may refer to the relation between the neogrammarian theory of language development (which referred to laws of phonetic change) and Saussure's early structuralist description of language as it looks at one particular moment. In Saussure's view [1972: 193-195], the two approaches are incompatible; when discussing development he works within the neogrammarian paradigm (when needed correcting specific laws—thus [1972: 201]). A couple of decades later, however, structural linguists of a new generation (in particular Roman Jakobson) were able to reinterpret the sound shift laws as resulting from structural constraints. In both (and many similar) cases we may conclude that the later integration of the two approaches approaches into a more mature theory has shown both of them to be in some way true—which requires that we formulate a concept of truth where this *can* be said meaningfully.

As a matter of fact this concept was already formulated on p. 45, where the truth value of a theory was interpreted as a »*structural agreement or correspondence* with features of reality«; on the same occasion it was observed that »*correspondence* is something quite different from *similarity*, not to speak of *identity*«.

This materialist notion of truth is not ontological—or at least not necessarily ontological. Certainly, if we suppose that some entity does exist (be it with provisos, like the fox on p. 30), then the best theories will be those which contain it. In this case we may speak of »ontological existence«.

[117] To be sure, the example is simplified into the extreme—but not significantly more than the references to planetary systems and combustion theories. See [Diderichsen 1971: 20ff] for an exposition of the problem in the context of a descriptive grammar.

We may surmise the element *oxygen* to be such an entity. During the two centuries that have passed since its discovery, we have come to know increasingly more *about* oxygen: that it consists of atoms composed in a specific way; that three different isotopes of oxygen exist; that it has a particular spectrum; etc.—and even how oxygen can change into other elements through nuclear processes. We have come to know answers about oxygen to which neither Priestley nor Lavoisier suspected the questions; we have not, however, succeeded in getting rid of oxygen as a particular way in which matter can be organized.

Phlogiston had no similar ontological existence, however real the regularities of chemical processes which were explained through the assumed existence of this substance. When taken to be a substance it turned out to resemble the elf maiden: one sees her, one hears her enticing proposals; but when he tries to grasp her, her back is hollow, and his arm catches nothing but thin air. Newtonian forces are similar, they only manifest themselves through the regularities which they formalize, and one can get into no other contact with them. Word classes may fall somewhere between: a delimitation made on the basis of declination turns out to have semantic and syntactical implications (if only approximate). Social classes defined from income sources as done by Adam Smith may also be located at an intermediate position, since individuals who belong to the same class are likely also to share much of their general experience and their material and spiritual culture. The labour value of goods (or the customer preferences of the competing marginalist economic theories) or the Gothic style in architecture, on the other hand, are probably no be better off than the Newtonian forces; few of the basic entities to which social and human sciences refer seem to be. If we expect truth to be ontological truth we do not need Kuhn to tell us that few of the theories about human social and cultural life are *true*.

But why should truth be ontological? No sensible scholar would claim that the most adequate (or »true«) description of Shakespeare's works is a dictionary listing his vocabulary, and no biologist would be satisfied by taxonomy alone. The »truth« of the screwdriver did not consist in the isolated correspondence between its edge and the notch of the screw, but in that combination of edge and rotational symmetry which corresponded to the entire make-up of the screw and allowed us practical interaction with it (namely to put it into the wall). Taken in isolation, the »ontological« truth of the edge is even meaningless, since only the practical interaction turns the edge into something which corresponds to a notch—screws have no edges, they are constituted in a way that makes the application of an edge appropriate. The truth of a theory, we may repeat, consists in its *»structural agreement or correspondence* with features of reality« as revealed in practical interaction with the object (be it in interpretation, cf. note 47)[118].

Then, however, the basic categories which form the framework of our cognition become *true*—cf. the discussion of the practice allowed by the category of the permanent object on p. 30. The progress within a paradigm also becomes a progress toward greater truth, not a mere accumulation of solved puzzles: expansion of the paradigm means that more features of reality are accounted for coherently, while articulation implies greater precision in the structural agreement. Even the replacement of one paradigm by another is a progress toward greater truth, at least if the replacement follows the Kuhnian pattern *expansion —> accumulation of anomalies —> crisis —> convincing new interpretation achieving paradigmatic*

[118] Experimental science, it is true, may aim at controlling whether a certain theoretical entity can be interacted with in new ways, so to speak testing whether the elf maid whom you see and hear and whose hair you smell can also impress the sense of touch. It was precisely because they could be contacted through several channels (semantics, declination, syntax, or economy, living conditions and culture) that word classes and social classes were held above to be more ontologically real than phlogiston. But since physical science provides the scale on which degrees of real existence is conventionally measured it is worthwhile remembering that quantum mechanics (though ridden with paradoxes as it still is when used to describe more than isolated experiments) dissolves the ontological existence of physical matter: if an electron has to be something specific, it is a particle in some experimental settings and a wave in others—maiden to the eyes and thin air to the sense of touch. A consistent description (relative as its consistency is) can only be reached at the cost of giving up its ontological separateness.

status[119]—cf. the discussion (in terms of »research programmes«) on p. 72.

In formal as well as Aristotelian logic, a meaningful statement is either *true* or *false;* the everyday idiom of »not quite true« and »almost true« has no place here. Of two conflicting statements, furthermore, at most one can be true. Neither seems to apply any longer if we allow (for instance) both the phlogiston and the oxygen theory to be materially true, however much one is held to be »more true« than the other. As long as we move within the same conceptual framework (within which the problem of non-compatibility does not present itself), it is no more difficult to speak of the sentence which is *less true* than the other as *false* than it is to say in traditional logic that the statement »Peter is a boy, John is a boy, and Joan is a girl« is *true* while the statement »Peter is a boy, John is a boy, and Joan is a boy« is *false* and not just »67% true«. If the two statements belong within incompatible conceptual frameworks things are less simple—even if we regard the early oxygen theory as more true than the phlogiston theory, the statement »carbon and hydrogen have nothing in common beyond being elements« cannot be declared »so true« that the phlogiston theory identification of the two is completely false; only when a common framework (in this case the developed oxygen theory) ripens is it possible to decide—in the present case that the phlogiston theory had a point.

In the end, the Lakatosian-Kuhnian criticism (or even rejection) of the naïve correspondence theory of truth thus unfolds (in materialist interpretation) as a genuine *critique.* While sceptical postures in the manner of Pilate (»What is truth«—John 18:38) are often meant as a mere way to wash one's hand (cf. Matthew 27:24), the critique tells us that truth, though never final

[119] The situation is different if an ideologically inconvenient paradigm is replaced by something more convenient—as was the case in the neo-classical revolution in economics, cf. note 74 and p. 73. In such cases, progress toward greater truth is evidently not assured. Nor is, however, the Kuhnian progress »*from* primitive beginnings«—Jevons is certainly more primitive than Ricardo.

nor absolute, is *not arbitrary* nor to be decided from fancy[120]. Not every point of view is as good as any other.

Causality

The concept of causality goes back to two types of immediate or daily-life experience. One is acquired already in the sensori-motor period, in the form of that »practical category of causation« which makes you draw a table cloth in order to get hold of a flower vase which your mother has tried to put outside your range (cf. p. 14). When your cognitive structure develops, the category enters awareness. In this mature form it is behind any planned action aiming toward a specified end, and thus the foundation of every technology—physical, chemical, medical, psychological, or social. Actions according to this scheme fall into separate phases: first you conceive a strategy; then you start drawing the table cloth; after a short while, the vase falls over the edge of the table onto the floor, and you get hold of it (maybe in some unforeseen state, but this concrete problem is common to all strategic planning and not our present concern).

The other kind of proto-causal immediate experience only enters our life with pre-operatory thought: it is the question »why?«. To the question »Why is the floor wet and covered with broken glass?« you may answer »because I wanted to get hold of the flowers«; alternatively you may tell that the vase fell over the edge of the table (and your brother may sneak on you and tell who was responsible for pulling it). The latter explanations are what we are used to call *causal*, dealing with various aspects of the process behind the broken glass; the former is *teleological*, an explanation of the purpose which you had in mind when pulling.

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[120] Francis Bacon, in the opening passage of his essay "On Truth", also drew on St. John for a comment on those who claim truth to be a mere whim:

> What *is Truth;* said jesting *Pilate;* And would not stay for an Answer. Certainly there be, that delight in Giddinesse; And count it a Bondage, to fix a Beleefe; Affecting Free-will in Thinking, as well as in Acting. And though the sects of Philosophers of that Kinde be gone, yet there remaine certain discoursing Wits, which are of the same veines, though there be not so much Bloud in them, as was in those of the Ancients.
>
> [Bacon 1937: 5].

The sciences also ask and answer why's; in general we feel that theoretical sciences are characterized precisely by posing and trying to settle such questions for their own sake, while applied sciences translate practical aims into questions and translate the answers back into strategies. In the humanities (»theoretical« as well as »applied«), the following question types will be familiar:

— Why will so many people use time and money on reading *Bild Zeitung?*
— Why will so few people read Thomas Mann?
— Why did jazz develop among the Black in New Orleans?
— Why is the normal form of the Oedipus complex absent from the Melanesians of the Trobriand Islands?
— What made the Roman Empire collapse?
— What were the reasons that made research a central activity for the nineteenth century university?

All six questions may be understood causally; the first two (which ask about the actions of people) may also be given teleological answers (e.g., »because the majority sees no point in playing high brow«, or »because *Bild Zeitung* arouses one's feeling of being alive, while Thomas Mann's prose is so complex that you fall asleep«).

In sciences which do not deal with the conscious decisions of human actors, only causal answers to the question »why« are normally accepted nowadays—biologists do not believe that the giraffe got its long neck »in order to« be able to eat leaves from trees; instead, what eighteenth century theologians would see as examples of God's design is explained as the adaption to a specific ecological niche through Darwinian selection pressure.

The different meaning of the »why« has been used by some philosophers to delimit the humanities—in particular by the Dilthey school. Causal answers to the above questions may be quite legitimate, according to this view; but the sciences which provide them (the sociology of literature and art, psychoanalytically oriented anthropology, economic history, etc.) belong outside the humanities (more precisely: the *Geisteswissenschaften*). *Explanation* is causal; *understanding*, the purpose of the humanities, may well investigate how reading *Bild Zeitung* or Thomas Mann affects one's cultural status. But this knowledge is only relevant inasmuch as it is also known by the potential readers—what *we* know is only relevant for understanding the actions and opinions of people who *also* know it.

Others have claimed that the only way the humanities can pretend to the status of *sciences* is if they allow causal explanations. Explaining reading habits through the delusive motives people give for their actions (or, still worse, by inventing motives and claiming that these are the motives of the actors) is no better that referring to the intense desire of the giraffe for green leaves.

Often this latter stance is coupled to a professed positivist view and to the claim that causation has to be understood according to Hume's definition and not through the multiple causation proposed by Aristotle. Since the positivist understanding of scientific knowledge is not without problems, and in view of the importance of the why's for every scientific practice, an investigation of the characteristics of scientific knowledge will have to probe this claim.

Hume's view is set forth in his *Enquiries Concerning Human Understanding*. Causation is no necessary connection between one event (»the cause«) and another (»the effect«) (section VII,ii,59, ed. [Selby-Bigge 1975: 74f]). It is nothing but *an expectation on the part of the observer* produced by habit. When we have seen innumerable times that a billiard ball starts rolling when hit by another, then we *expect* that billiard ball *A*, when hit another time by billiard ball *B*, will start rolling as usual. This, and nothing more, is meant when we say that being hit by *B causes A* to start roll. Moreover, thus Hume, the concept of causation requires that the effect comes *after* the cause.

To this a modern Aristotelian will object that there are many answers to the question why *A* moves as it does. Being hit is evidently one; but if the balls had consisted of soft clay the outcome would have been different; so it would if *A* and *B* had not been spherical, or if *A* had been located at the very edge of a table not provided with a cushion. A complete answer to the question *why* will thus involve *efficient causes* (the hitting); *material causes* (ivory, not clay; the surface of the cloth); and *formal causes* (the laws of semi-elastic impact and of sliding/rolling, as well as the geometrical forms involved). If we want to understand what goes on we will also have to notice that somebody plays billiards and wants *B* to move (perhaps as it does, perhaps otherwise), ultimately wanting to win the game and to gain the stake; both of these are *final causes*[121].

The modern »positivist« view of causation may presuppose the naïve correspondence theory of truth for the observation of single events and hence replace Hume's habit of mind by a regularity of nature; or it may accept the criticism of naïve correspondence and stick to the subjective expectation. In both cases it identifies *the cause* as an *efficient* cause, an event which is invariably followed by another event. It is generally held that this kind of causal thinking is the one about which the physical sciences speak[122], and therefore the one which should be emulated by social and human sciences.

The premiss is blatantly wrong (and the conclusion thus no conclusion but an assertion which must be investigated independently). Firstly, a physical description (in case, by Newtonian mechanics) of the billiard game will involve all the aspects listed under the Aristotelian explanation: physical configuration and shape, masses, friction, elasticity. The events »hitting« and »starting to roll« are at best *aspects* of *moments* of a complex process without any privileged status. Actually they are even less: they do not exist as events. When *B* touches *A* both will be compressed, and increasing compression will be accompanied (not followed in time) by increasing mutual repulsion. This repulsion will accelerate *A* and decelerate *B*, and after a short while *A* runs faster than *B*. After another short while the two balls separate, and we see *A* first sliding and then rolling along alone.

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[121] The Medieval scholastic tradition and later anti-Aristotelianism have spoken in the singular of *the* efficient, *the* material, *the* formal and *the* final cause. Yet according to Aristotle's point of view, »the modes of causation are many«, even though they can be grouped in classes according to their character (*Physica* 195^{a}28; trans. [Hardie & Gaye 1930]).

[122] Many of those who do not accept the relevance of Humean causality in social and human science, on their part, claim that it was the one which prevailed in Newtonian physics, and that it has been left behind by modern physics. Why—they ask—should social and human sciences imitate a model which has shown itself to be erroneous in the physical sciences?

In this description of the physical process, as we see, there *is* no e »hitting« preceding another event »starting to move«. Both are proce and indeed *the same process*. In the idealization where both balls absolutely hard the processes contract to a momentary event, it is but then to *the same* moment. This is nothing specific for billiard balls; e description of classical physics has the same property. So has also e description according to relativistic physics and quantum theory. reason that Newtonian forces have to be given up is precisely the unacc ability of delayed causation. Hence relativistic physics will not spea one electric charge q_1 acting at another q_2, which it could only do wi delay corresponding to the velocity of light; instead, q_1 is told to proc a local electromagnetic field; this fields propagates, and acts on q_2.

Hume was indeed quite right when connecting his »events« to me habit: the »hitting« and »rolling« are not moments of the process descri by physical science; they are moments of the awareness of the obser *He* sees and hears ***B*** approaching and hitting ***A***, and afterwards he not that ***A*** has started rolling.

Hume's scheme thus does not lay bare the underlying structure of physical description, and is certainly no critique of Newtonian ca tion[123]. It is a formalization of the sensori-motor scheme of pract causation and of strategic action in general: *First* I get the idea of hov get hold of the vase, and start drawing the table cloth; *afterwards*, the v falls to the floor. Here as in the Humean explanation, everything in configuration is taken for granted: the table, the table cloth on which vase stands, etc. Formally, of course, Hume has left aside the anthropon phic notion of decision and planning. Fundamentally, however, emphasis on temporal separation shows that the »effect« is an *end re* and thus *an aim*—no step in an ongoing process is ever the end unless define it to be because you are not interested in what comes afterwa Similarly, the privilege of *the hitting* over the other aspects of the proc is that it corresponds to *an action which you may undertake intentionally*. table and the elasticity of the balls are given; as a player you push.

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[123] That Hume attempted such a critique but did not produce one was alre pointed by Kant in the *Critik der reinen Vernunft* [B19; *in Werke* II, 59].

Yet Humean causality, even if a formalization of strategic action, is not relevant for technological thinking. What I need to know (intuitively, in this case) in order to win the billiard game is the degree of elasticity of the impact, the way friction transforms sliding into rolling movement, etc. The knowledge which *serves* technological planning is indeed »Newtonian«, not »Humean«: what you do is to interfere with or determine specific features of a process (acting as an »efficient cause«), but where you can only determine the ultimate outcome if you understand how the features on which you act interact with and are conditioned by other (formal, material, and structural) features.

Humean causality is hence neither relevant for theoretical natural science nor for technological thinking. Remains the question whether it can be given any meaning within the humanities.

The answer is easily seen to be negative. We cannot notice it to be a regularity that all Roman Empires collapse, since there was only one. Nor can we test the hypothesis that relatively few people read Thomas Mann because he is difficult by seeing if more people will read authors who are in all respects like Thomas Mann except that they are more readable. As everybody knows who has enjoyed *Joseph und seine Brüder*, an author whose prose is easily read would be different from Mann on almost all accounts.

What one *can* do is to look in general at *The Collapse of Complex Societies* (the title of a book published a few years ago [Tainter 1988]), or to investigate the public of a variety of authors, thereby finding similarities and divergences in possible »cause« and possible »effect«. This is often believed to be the closest one can get to (Humean) causality in the humanities.

It may well be the closest on can get—but still it has nothing at all to do with the Humean concept. In the moment we single out a specific class of societies as »complex« or single out an array of features by which authors can be characterized, we have already introduced a screen of theoretical or at least pre-theoretical thinking between the events and the way in which we interpret them—in a more radical sense than that in which even simple observations are by necessity tainted by theoretical presuppositions.

The only kind of causality which is meaningful within the humanities (and the social sciences, for that matter) is the one which also makes sense in the physical sciences: *correlation with, or explanation in terms of a theoretical framework*—though evidently a theoretical framework which is not the one of the physical sciences, and more often an open framework than a finite and formalized set of statements.[124] A less shocking formulation of the claim is that *causality is never an extra component of the scientific explanation of phenomena beyond theory, neither in the natural nor in the social or human sciences.* It is (at best) *a way to formulate the theoretical explanation which singles out one element* as the most interesting and considers other elements and features as a background with which the element in focus interacts.

That causality tells us nothing beyond theory may be a dismal conclusion for those who accept that theories are fallible and therefore want to base their science or their technology on more firm foundations. As we have seen, however, this aim is not attained by attempting to adopt a Humean idea of causality, and giving up illusions is no real loss. Moreover, as we have also seen, the inescapable »fallibility« of theories only means that no theory can pose as absolute truth. Inasmuch as a theory can be claimed to be *true* in a materialist sense, as argued above, causal explanations in the sense of correlation of phenomena with theory are *also true*.

When one element of a theoretical explanation is singled out as a cause it may be so because we want to use our knowledge in some kind of technology: that which it is of interest to consider as a cause is what *we* can influence or determine, that which is technologically manipulable; those features of the situation which we can do nothing about are then understood as a background, the conditions to which our action is subject. But even purely theoretical investigations may speak of and discuss causation. In such cases, the choice of the cause is obviously rather free, and may depend on which features of the situation we find most interesting, or which aspects of the process we want to scrutinize more closely while relegating others to the background.

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[124] An attempt to formulate such a structure explicitly is Braudel's separation of long-, medium- and short-term determination in history—cf. [Kinser 1981].

One of the more grotesque (or, if you are in that mood, tragic) facets of scientific life is the jealous and hostile passion with which scholars discuss in such situations which is *the* cause. Was the tuberculosis which killed several of my grand-uncles *caused* by bacteria or by their living conditions? These competing causalities are evidently complementary aspects of *the same* theoretical explanation: the disease cannot occur unless one is infected; but infection is much more likely to result in disease when he is badly fed and his lodging never dry. This example may be outdated except when one or the other sanitary policy is advertized (i.e., when it comes to technological choices), precisely because a single theory explains how the two explanations concur. If the two derive from different theoretical approaches (which, like the early structural and the neo-grammarian approach to linguistics may *both* be »parts of the truth«, even though their mutual relation is not, or not yet, elucidated), similar discussions are still seriously meant. In the humanities, where theories *stricto sensu* are rare and open-ended frameworks the rule, discussion may change into calumny or non-discussion because both parts regard the other as mistaken beyond hope of salvation by argument[125].

Sticking stubbornly to your own paradigm may well be an efficient way to make the sciences progress, as held by Kuhn: as an advocate in court, you should do what can be done to find arguments in favour of your client. But like the advocate you should also listen to the other part, and acknowledge that the final verdict may not be totally in your favour. Dismissing that possibility, and rejecting *a priori* that approaches which differ from one's own may be legitimate, is no more productive in science than in court, and no less preposterous.

[125] This was indeed the situation in my own discipline, the history of science, until a few of decades ago. As late as the early 1980s the discussion between »externalism« (the explanation of scientific development from general cultural, social, technological and economic factors) and »internalism« (the explanation from the inner dynamics of the sciences) was still regarded by most members of the field as meaningful.

Objectivity, subjectivity, and particularism

If one's choice of the element to be singled out as a *cause* is »rather free«, does that mean that causal descriptions are subjective and not objective? In general, is *scientific knowledge* subjective or objective? This question is more trendy than the question of *truth*, maybe because it is easier to assert that the other part in a discussion is subjective than to prove that he is wrong.

Is science objective? Certainly not if we understand objectivity as *coincidence* with the object. Nobody can carry an objective picture of a house in the brain or the mind, since the house is larger than the skull and the mind contains no bricks. Or, as formulated by a student of mine when I objected to an explanation he had given at an examination that it was incomplete[126]: *Any complete model of reality is—by definition—reality.*

Criticizing a scientific description for failing objectivity in this sense is thus either absurd or foul game. Science, like any other knowledge, is *by necessity subjective.* Even the way a house is depicted on a photograph depends on the optics of the lens and on whether the film is black-and-white or in colours. What science sees also depends on the conceptual structure through which it looks: Priestley made oxygen but saw phlogiston-free air.

But science, as knowledge in general, is also subjective in the sense which corresponds to the dependency of the picture on the place from where it was taken and the dependency of an answer upon the question. Knowledge never comes from passive reception of whatever passes before one's indifferent eyes; it is always the result of an active practice—as a minimum, of selective attention, but often of much more intentional operation. In science, knowledge comes from experiment, investigation, critical reflection, etc. As our practice vis-à-vis the reality we want to know about is not in itself part of that reality, whatever knowledge we earn is by necessity *subjective* and not determined by the object alone.

This much is common sense. It should have been evident at least since Kant, and does not illuminate the relation between objectivity and

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[126] Mark Madden, Spring 1990.

subjectivity very much. An interesting quality of the Piagetian and Kuhnian epistemology is that it allows us to discern a sense in which the subjective aspect of knowing may in itself be more or less objective.

In order to see how we shall first return to the discussion of the status of the schemes and categories of our general cognitive structure (see p. 27ff). When we conclude, for instance, that »the material world is constituted in a way that allows a more adequate practice if we order our experience as representations of permanent objects«, then we have concluded something about the material world—namely about its responses to a particular practice. This predictable response is a property of the material reality in which we live, i.e., of *the object* of our knowing; the category of the permanent object, however much it is a constituent of our subjective cognitive equipment, is *also objective.*

The basic categories and logical schemes, however, though arising through cognitive development, are end points. Once my daughter had acquired the scheme according to which there are more children than girls in a mixed kindergarten (see note 22) she could understand *nothing but* that, and once we have organized our way to experience in permanent objects, we cannot avoid doing so. It is left to further empirical studies to find the degree of permanency of actual objects, and to construct for instance a theory which allows us to discuss in which sense salt is conserved when dissolved in water. Irrespective of their genesis through biological evolution and individual development (which make them *synthetic*, i.e., informative about reality), the basic categories and schemes remain *synthetic a priori* within the ongoing process of knowing; they thus belong to another cognitive species than the *synthetic a posteriori*, the actual outcome of our observations. Once the synthetic a priori have arisen, it is no longer possible to distinguish in their domain between *more* and *less objective* subjectivity.

This distinction is only pertinent when it comes to discussing the synthetic a posteriori, in particular scientific knowledge. To see this we may compare the Kuhnian cycle with the *hermeneutic circle*—I quote the explanation given in [Høyrup 1993: 167f]:

> At our first approach to a foreign text (in the wide sense, i.e., to any spoken, written, sculptured, painted or similar expression of meaning) we interpret

> it in agreement with our own presuppositions and prejudices, which are in fact our only access to the universe of meanings. But if the foreign text does not fit our interpretive expectations on all points (which it rarely does), and if we investigate the points of non-compatibility seriously, we will be led to revise our prejudices. The revision will enable us to understand the presuppositions of the foreign mind (or understand them better) and hence even to understand ourselves from the foreign point of view. Understanding the other leads us to better insight into our own expectations to universes of meaning, and hence allows us to approach the foreign text (or other texts) with better prejudices.

Some features of this structure are certainly different from what we see in the Kuhnian cycle of normal and revolutionary phases. The relation between the scientist and the object of a science is less symmetric than that between the interpreting mind and the interpreted mind[127]. The »better prejudices« with which we approach the object after a change of paradigm do not come from understanding ourselves from the point of view of the object. But they remain *objectively better*, i.e., they reflect the *features of the object* more precisely or at a deeper level, according to the arguments on pp. 72 and 104. The progress of a scientific discipline through scientific revolutions (when these follow the Kuhnian ideal scheme), which was spoken of above as a *progress toward greater truth*, can also be understood as *progress toward a greater objectivity of its subjectivity*.

So far so good. Yet »subjectivity« is not only used about the inescapability of knowing only in response to specific questions and in terms of a particular conceptual framework. These two kinds of subjectivity were assimilated to the making of a photograph on a particular film by means

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[127] Anthony Giddens [1976: 146ff], when discussing the similarities between the two circles, characterizes the process of interpreting a foreign text or social world as a *double hermeneutic*:

> Sociology, unlike natural science, stands in a subject-subject relation to its 'field of study', not a subject-object relation; it deals with a pre-interpreted world, in which the meanings developed by active subjects actually enter into the actual constitution or production of that world; the construction of social theory thus involves a double hermeneutic that has no parallel elsewhere [...].

What this hermeneutics looks for is thus an understanding of (e.g.) Rousseau's *Émile* and its impact which builds on *how Rousseau and his contemporaries understood* the world.

of a camera with a particular lens and from a specific perspective. Given these conditions (and the shutter speed etc.), what will appear on the picture is determined by the object.

Or it should at least be. If it is not, the picture has been retouched. Retouching corresponds to those kinds of scientific knowing which are affected by other factors than the object, the kind of question (purportedly) asked, the instruments used, and the conceptual framework which is referred to or which is shared by the community of workers in the field[128]. This kind of subjectivity is better spoken of as *particularism*. To some degree it cannot be avoided—you always have motives beyond those which you confess to yourself or reveal to your psychoanalyst, even when it comes to knowing. But since (by definition) private distortions cannot be controlled by others, they detract from the value of knowledge to the same extent as they are present. Scientific knowledge is shared, or should at least be shareable; but knowledge expressed in a code which cannot be deciphered by others can not be shared. *Particularism*, though to some degree inescapable, should hence be minimized through critical discussion.

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[128] It may be a profitable aside to point out that the seemingly innocuous phrase »which is referred to or which is shared« hides a serious dilemma. Many social scientists, noticing that social thought is always involved in conflict and cannot avoid taking sides at least by asking the questions of one party, have held that this inescapable subjectivity should be brought under control by each social scientist telling his side, his employer and his sympathies explicitly and honestly. This stance (which is in particular associated with Gunnar Myrdal) may seem attractive, and certainly has a point. But apart from the naïveté of the expectation that people (and social scientists are also people) should tell honestly when their sympathies and aims contradict those of their employer, Myrdal's cure against unbridled subjectivity suffers from the same weakness as Lakatos's formalized conception of his »hard core« (cf. p. 90): too much of the framework that is shared by a scientific community consists of tacit knowledge, and will not be revealed if one asks workers for their political position—the paradigm under which they have been trained may well by built around questions and concepts asked and formulated from a quite different position.

Participating in a community which shares a paradigm, including the tacit knowledge which goes with it, is therefore just as important as honesty if subjectivities are to be kept as objective as possible.

VI. THE ROLE OF NORMS

In 1965, a symposium was held in Oxford, at which among others Popper, Lakatos and Kuhn were present, and the topic of which was a discussion of Kuhn's *Structure of Scientific Revolutions*[129]. As Popper and Lakatos saw things, the question was whether Kuhn was right in replacing the(ir) logic of research with a description of the social psychology of scientists; Kuhn, on the other hand, asked whether Popper and Lakatos had really brought forth a *logic*, and answered that their actual output was an *ideology*.

Much may speak in favour of Kuhn's reply. Still, both Popper_1 (and even Popper_0) and Lakatos/Popper^2 are too close to aspects of the scientific production process to be dismissed as *nothing but* ideologues in the vulgar sense. Also Kuhn's own work, as we have seen above (p. 92), results in a kind of logic for the social production of scientific knowledge. Even though there may be many similarities between *science* and *organized crime* (to paraphrase a discussion running through the same conference proceedings; both are indeed social activities perpetrated by relatively closed and highly specialized communities), there are also noteworthy differences. We can therefore only come to understand the nature of scientific knowledge if (so to speak) we grasp how the process of science, *in spite of its similarities with organized crime*, can be described approximately in terms of a »logic« through which it manages to produce some kind of *reliable knowledge*.

[129] The revised contributions from the symposium were published in [Lakatos & Musgrave 1974]. Many of them were cited above.

Traditional hagiography explained the specific character of science by the exemplary character of scientists. Science is reliable because scientists are eminently reliable; science is objective because scientists are heroically objective and dedicated to their cause; etc.

Bankers will probably not agree that scientists are significantly more reliable than average people when it comes to their use of a cheque account. If they are *within their professional work*, it must be explained in other terms than through general moral perfection.

Logic and norms

A »logic« is a fixed pattern. If the development of scientific knowledge in social process follows a specific logic (or just follows it to some extent), we must presume that this social process is itself governed by a set of general »laws« or regularities.

Two questions can then be asked. Firstly, whether the logic of scientific development is exact and formalizable, or rather to be described as a »dialectical logic of development«. Secondly, which *kinds* of social regularities are involved, and how they succeed in bringing about a logic.

Kuhn's offer is not formalizable, and is in fact dialectical. In the closing passage of his article on "The Function of Dogma in Scientific Research" he points out that

> scientists are *trained* to operate as puzzle-solvers from established rules, but they are also *taught* to regard themselves as explorers and inventors who know no rules except those dictated by nature itself. The result is an acquired tension, partly within the individual and partly within the community, between professional skills on the one hand and professional ideology on the other. Almost certainly that tension and the ability to sustain it are important to science's success.
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> [Kuhn 1963: 368f].

Elsewhere he points out that the whole process only functions because what he speaks about here as »established rules« are indeed not explicit and unambiguous rules but shared *norms* or *values* which individual workers interpret differently:

> [...] individual variability in the application of shared values may serve functions essential to science. The points at which values must be applied are

> invariably also those at which risks must be taken. Most anomalies are solved by normal means; most proposals for new theories do prove to be wrong. If all members of a community responded to each anomaly as a source of crisis or embraced each new theory advanced by a colleague, science would cease. If, on the other hand, no one reacted to anomalies or to brand-new theories in high-risk ways, there would be few or no revolutions. In matters like these the resort to shared values may be the community's way of distributing risk and assuring the long-term success of its enterprise.
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> [Kuhn 1970: 186[130]]

The Kuhnian framework, however, is not the only place where we have encountered the need for a dialectical understanding and for application of the concept of norms. A manifest dialectical tension is seen in the contrast between the empiricist imperative:

> *Scientific explanations are only allowed to make use of concepts and to postulate relations and structures which can be rooted in experience, observation or experiment. Mythological explanations referring to entities with no such empirical underpinning are inadmissible: they only obstruct genuine scientific insight.*

and its falsificationist counterpart:

> *We are allowed to use in our explanations whatever self-invented concepts and hypotheses we like; but we should be aware that our hypotheses are indeed nothing but hypotheses, preliminary explanatory models, and not the truth. We should therefore constantly check our hypotheses as thoroughly as we can, and we must reject them as useless as soon as they enter into conflict with our observations of reality—i.e., as soon as they are »falsified«.*

As they stand, the two rules of scientific conduct solved some of each other's problems, as we remember—but in mutually unacceptable ways. None of them, however, could be rejected as plainly irrelevant. They stand in much the same relation as these two passages from Deuteronomy:

> *Thou shalt not kill.* (5:17)

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[130] When this was formulated, the idea that a system may function better if its components are allowed a margin of unpredictability (and the notion that real systems function that way) was highly untraditional (for which reason Kuhn resorts to the metaphor of »risk distribution«). During the last decade, of course, the metaphor of »chaos theory« has popularized the idea.

and

> *But of the cities of these people, which the Lord thy God doth give thee for an inheritance, thou shall save alive nothing that breatheth. Namely, the Hittites, the Amorites, the Canaanites [...].* (20:16f)

These rules, like the empiricist and falsificationist maxims, express a *moral dilemma*, the admissibility of which is perhaps the most important distinctive characteristic of a system of (moral) norms or values as opposed to a set of binding juridical rules or a theory.

One of the things we demand from a *theory* is that it should be free of inner contradiction (to be sure, the existence of a recognized contradiction is no reason for automatic and immediate rejection, as exemplified in the history of quantum physics; but it is at least an anomaly which one should try to solve): if the same theoretical system predicts on one hand that a bridge which we try to build will stand and on the other that it will fall down, we should obviously try to find out what is actually going to happen, or at least to find out why our theories cannot tell. *One aim* of theory construction (though not necessarily its actual scope) is that it should be fit to serve *strategically rational* action (Weber's *Zweckrationalität*), which it cannot do if producing contradictory predictions.

The same demand for internal consistency we make to juridical laws—for this reason, the existence of capital punishment presupposes a clear distinction between that sort of homicide which is *murder* and hence to be punished, and the executioner's work for which he gets his salary. No juridical system could live undisturbed by a clear contradiction like the one exemplified by Mosaic law; when a contradiction occurs in real life (i.e., not only as the outcome of a thought experiment), the judge or some other instance of authority has to decide which norm is primary; if the contradiction presents itself recurrently, the legal system has to be adjusted.

Explanations of morality

Norm systems do live with contradictions, whether we like it or not; that they do so is a main reason why different social actors choose differently (cf. Kuhn as quoted on p. 119 regarding the »individual variability in the application of shared values«). To understand *why* norm systems have to live with contradictions we may take a brief look at the nature and origin of norms and morality[131].

One very influential explanation (which is historically coupled to empiricist philosophy, and which shares its matter-of-fact attitude and its bent toward atomistic analysis) is *utilitarianism:* Behaviour is *morally good* if it is *useful*, i.e., if it promotes general human happiness.

Three problems at least inhere in this understanding of morality. Firstly, it reduces morality to strategic rationality with an undefined aim: what, indeed, is *general human happiness* if not itself a moral issue? Secondly, it makes no sense of the experience of the moral dilemma: if two alternative actions are both prescribed, that one is obviously best which according to a cost-benefit analysis is *most* useful. Thirdly, it presupposes that the consequences of a course of action are finite[132]—if not, cost-benefit analysis is impossible.

Equally influential is Kant's approach through the distinction between the *hypothetical* and the *categorical* imperative (cf. [Høyrup 1993: 133]): My present action is morally good if, when generalized into a rule, it is of *absolute* (»categorical«) validity[133]. Strategically rational action (»under

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[131] It will be seen that I treat morality (or ethics) and norms as one thing. This is not done by everybody. Certain authors would see *norms* as *that which people think is right*, while *morality* is concerned with *what is right* in itself. Others would reserve the term *morality* for serious matters and use only *norms* to denote, e.g., norms concerning good manners. (What the latter would do to describe cultures where unwillingness to apologize for pushing somebody involuntarily is a sufficient reason to kill him in a duel I do not know; the distinction seems not to be cross-culturally valid).

[132] Or rather that the actual value of their *sum* is finite, in the same way as the present value of £1 to be paid each year from now to eternity is £21 at an interest rate of 5% per year—i.e., if consequences which our children and neighbours have to bear are less important than those which hit ourselves, and those which hit our grandchildren and our neighbours' neighbours count even less.

This is, of course, precisely the presupposition of every cost-benefit analysis: consequences which are beyond my horizon *do not exist;* consequences within my horizon are only counted to the extent that those who suffer them count for me.

[133] Such a generalization can of course be performed at different levels. For most of Kant's contemporaries it would be obvious that the execution of certain criminals was morally right—not because of the generalization »you should kill other people« but, e.g., because »you should protect society against the damage which could be

the hypothesis that you want to achieve X you should do Y«—Kant's hypothetical imperative), on the other hand, is thereby neither morally right nor wrong. From a Kantian point of view, it is hence not the action in itself which is judged morally right or wrong: the moral judgment can only deal with the action in its context of justification and intention.

In any case, it will be seen, there is no more place for the moral dilemma here than inside the framework of utilitarianism (with a proviso to which we shall return). One, at least, of two rules in mutual conflict cannot be of general validity; one, at least, is hence no moral rule.

Under the impact of Kantianism, utilitarianism has been split conceptually into *act utilitarianism* (the classical stance, which judges acts individually, from their particular consequences) and *rule utilitarianism*, which does not ask whether single acts, but only whether *rules of conduct* are useful. The difference between the two positions is larger than it may seem at first. Firstly, they may judge the same action differently: if I need the money and you do not really need it, act utilitarianism may find it justified if I omit to pay my debt to you. But rule utilitarianism would see that the rule »you need not pay your debts« would be the end of lending, and thus damage those in temporary economic distress (a real-life instance of the same divergence is cited below in note 152).

Secondly, rule utilitarianism, while fitting our immediate feeling that morality is concerned with rules and not with the expediency of single acts, misses the »positivist« simplicity which constitutes the merit of act utilitarianism to such an extent that it is dubious whether it still deserves the utilitarian label. It can never be given within a particular action under *which* general rule it should fall (cf. note 133): is »hanging X« an instance of »killing a fellow human being«, of »annihilating an enemy«, of »executing a war criminal«, or of »celebrating our victory by liquidating a war criminal belonging to the enemy's side«? Classification of acts under rules already presupposes some kind of moral theory or proto-theory telling the pattern of possible rules. No more than in descriptive science is there any smooth road from »positive fact« to generalization.

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wrought by incorrigible criminals«.

Rule utilitarianism, if it is to be believed, has no more space for moral dilemmas between rules than Kantianism. *If* it is at all possible to calculate the utility of a rule (which is certainly no simpler than calculating the utility of an action), then one can also calculate which of two conflicting rules is »more useful« and thus primary. It is only through its theoretical shortcoming—namely because the ascription of an action to a rule is itself ambiguous—that a specific action may present a dilemma. This weakness (or force through weakness) it shares with Kantianism.

It is not possible to derive actual morality from utilitarianism of either one or the other kind; but from a complex of socially accepted core values (inner solidarity within the Twelve Tribes of Israel, combined with the right to conquer and subdue, if we think of the Deuteronomy example), we may derive a set of norms which looks so similar to the standards of real morality that the utilitarian idea can hardly be *totally* wrong[134]. Kant, on the other hand, is certainly right when emphasizing that moral norms are characterized by claiming *general* validity. The kind of utilitarianism which offers the best prospects as a »rational reconstruction« (or retracing) of the emergence of morality is thus indubitably rule utilitarianism, its inherent difficulties notwithstanding.

[134] Evidently, the »socially accepted core values« also belong to the category of morals. The immediate scope of utilitarianism is thus only to reproduce the process by which a few core values (in the traditional formulation summed up as »general human happiness«) unfold as a complete moral system.

If a similar argument was to be used in order to explain the emergence of the core values, it would probably have to involve something like Habermas' »universal pragmatics« (see below, note 135) as moulded into a less universal shape by the necessarily restricted horizon within which any human society lives, together with other aspects of human biological nature (»happiness« is certainly a cultural variable, but only in exceptional cases reconcilable with starvation or extreme physical distress). A complete philosophy of morality would then, furthermore, have to take into account that the core values, *qua* participants in the general system of moral norms, are themselves reshaped by the processes through which this system unfolds.

Morality, language and social practice

The phenomenon of the dilemma may advance our understanding by another step. Norms should be seen as *social phenomena*. Human beings living together in a society socialize each other (in particular, of course, their children) and make sense of their existence by describing their situation and actions in language[135]. Patterns of action which serve shared interests will be formulated in general terms, *as norms*—and patterns which go against shared interests will be forbidden, again in general normative terms[136]. These normative prescriptions and proscriptions will be *practical knowledge*, knowledge about *how one should act*. To the extent that norms are made explicit there will be a certain amount of bargaining between them at higher communicative levels, for instance through their integration into religious systems or through the »thought experiments« of myth, drama and literature (cf. p. 179). The outcome will be total structures which are categorically different from the structures of practical action and which possess a certain degree of autonomy.

In these structures, as already more modestly in the enunciation of rules of conduct in language, the general formulations of norms will be *more general* than those patterns of action which they formulated; conflicts which

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[135] Habermas, in his »universal pragmatics«, derives certain basic aspects of morality not from the contents but from the very presence of this communicative situation: If communicative messages could not be *presumed* to be true, they would not be messages (even the lies of commercial advertisement only function because their character of communicative messages make us accept their overt or tacit implications at one level of consciousness, in spite of what we may know at other levels). Similarly, the mutual character of communication entails at least rudimentary human equality.

The pattern of the argument is obviously (though tacitly) borrowed from Merton's notion of »institutional imperatives«, to which we shall return below (p. 134ff). At present, however, we are concerned with other aspects of the communicative practice.

[136] Indeed, formulation in language *cannot avoid* being in general terms: general terms constitute the backbone of any language, proper names are by necessity peripheral and could never serve to tell anything on their own.

Already our use of language (e.g., the concept of »lending«) thus provides us with an a priori pattern of possible rules with regard to which we interpret actions, i.e. with a first version of the proto-theoretical foundation that rule utilitarianism presupposes without recognizing it.

never occur in the living existence of society will arise if the norms are taken *to the letter* and not in agreement with that social practice which they formulated in the first place (even though bargaining and restructuration tend to clear the most obvious conflicts away). In this way, dilemmas may arise, at least as virtual or philosophical problems—as Wittgenstein [1968: §38] tells, »philosophical problems arise when *language goes on holiday*«. In its real life, a culture will possess a large amount of »tacit knowledge« concerning the range within which norms should be applied (the Ancient Israelites will have had no difficulty in recognizing *whom* »thou shalt not kill« and whom to exterminate; nor have the fighters of countless Christian armies, nor the host of eminently Christian judges). None the less, because conflicting interests may interpret the range of shared norms differently and tacit knowledge is necessarily blurred, the theoretical dilemmas may also turn up in practice.

No social situation, moreover, is completely stable, while norms, once they have come into being, become embedded in language, religious ritual, myths, literature etc., and are thereby provided with a fair amount of temporal inertia. For both reasons, and in particular through the two in combination, norms may end up governing behaviour far beyond the range of experience from which they grew originally—and what was once merely *potential* conflicts between norms may thereby suddenly be actuated in possible behaviour[137].

Change has been a condition of human existence as long as human society has been human and communicative. The distinction between the two sources for dilemmas is thus merely analytical. It is not possible to point to a stage of innocence where only theoretical dilemmas existed. Human societies, as long as they have been in possession of moral norms, have always been troubled by moral dilemmas.

But not only total societies: Shared experience, specific patterns of discourse, conflicting interests, and change over time also affect single

[137] The actuation of conflicts because of changing social (here technological) realities is exemplified by one of the central norms in traditional medical ethics: the patient should be kept alive as long as possible. New medical technologies make »as long as possible« indefinitely long, while transforming the concept of »alive« in a way which changes the meaning of the norm through and through.

institutions and communities *within* society at large. These will therefore develop a specific ethos of their own, which will share many of the characteristics of the normative structure of general culture.

This brings us back to the creation of scientific knowledge. The empiricist and falsificationist rules of conduct are, in fact, nothing but community-specific norms, generalizing the experience and pragmatic rules of conduct of working scientists, and brought into mutual conflict when formulated and taken to the letter by philosophers. The shared anomaly of the two imperatives—the absence of purely observational knowledge which can serve verification or falsification—on its part, is of the same kind as the dilemma presented by infinitely life-saving medical technologies. If not created, it has come to the fore through the development of sophisticated instrumentation, from the invention of the telescope and the microscope onward.

It is thus not only in Kuhn's interpretation that the »logic« of the scientific process will have to be non-formalizable and dialectical. The underlying pattern (or »logic«) of any social process governed to an appreciable extent by norms or values has to be so.

Knowledge, norms and ideology

Should we then conclude (as Kuhn tends to do) that Popper's and Lakatos's attempts at formalized non-dialectical methodologies are descriptively irrelevant ideologies? Or, more brutally, that they have to be descriptively irrelevant *qua* ideologies?

The question may be approached via a discussion of the concept of ideology in the light of the theory of morality. *Ideologies* may be understood as systems which possess both normative and descriptive aspects but which cannot be fully analyzed into normative and descriptive elements—systems which, in the same breath, tell how and what the world *is* and how one should (therefore) behave, which merge theoretical and practical knowledge. Whether the Popperian and Lakatosian methodologies can be descriptively relevant is thus a specific case of a general question: whether the normative aspect of an ideology by necessity invalidates its descriptive aspect (or by necessity invalidates it completely)—and then, in the second instance, since the normative aspect was implied by the description, nullifies even this?

Utilitarianism of either kind explains norms as strategically rational prescriptions (with a tacitly presupposed aim of undefined »happiness«); since strategic rationality cannot exist without knowledge about the connection between our actions and their ensuing consequences, utilitarianism presupposes that its norms translate knowledge. Quite as much cannot be said if norms are generalizing reconstructions of patterns of action which are adequate within a specific and more restricted socio-cultural horizon. None the less, they generalize from what was adequate action within the restricted domain, given the shared interests of the carriers of the socio-cultural system as understood by these. In terms of the materialist notion of truth introduced in Chapters III (p. 45) and V they therefore contain a core of *truth* reflecting features of this social situation—a core which may be larger or smaller, depending upon the degree of generalization and on the kind of reconstruction which has taken place[138]. Norm systems are thus *already ideologies*, possessing an aspect of implicit descriptive knowledge; they are »proto-knowledge«[139]. Even presumed descriptive knowledge, on the other hand, presupposes a framework, a cognitive structure prescribing implicitly the way in which the subject-matter *should* be understood; since the selection of *interesting* features corresponds to a *particular* practice (remember Australian aboriginal children developing map understanding well before conservation, in contrast to European children—see above, p. 24), a prescription of *how to know* also involves an implicit delineation of a range of *possible actions*. Both from this point of

[138] Reconstructions aiming at inner consistency and utilitarian explainability may, like the search for coherence of scientific knowledge, increase the truth value of a norm system. Reconstructions aiming at agreement with mythical frameworks need not do so.

[139] An implication of this that cannot be pursued systematically in the present context is the possibility that the cognitive aspect of a norm system may then, like any piece of knowledge, be *wrong*, *mistaken* or (if we refer to the materialist notion of truth that is presupposed in the argument) *less true* than possible.

So much remains of utilitarianism, even when norm systems are seen as reconstructions of generalizations, that erroneous cognitive presuppositions (inasmuch as they can be traced) can be used to judge the norm system.

view and through its prescription of how to know it involves *practical knowledge*. Knowledge systems, like norm systems, are *already ideologies* and thus »proto-morality«. No *absolute* distinction between the two (or three) domains can be made; differences are *of degree* although—it must be stressed—the span from one extreme of the spectrum to the other is immense[140].

[140] The non-separability of description and prescription, it is true, contradicts the implications which Hume drew from a famous observation made in the *Treatise of Human Understanding* (III(i)1, ed. [Mossner 1969: 521]), and which have been widely accepted within philosophy since then: there can be no logical derivation leading from sentences built around »is« or »is not« to sentences built around »ought« or »ought not«. Since the former sentences are descriptive and the latter prescriptive, Hume's observation seems (and is generally taken) to imply that knowledge and norms are not only separable but actually separate.

The arguments that knowledge is proto-normative and norm systems proto-cognitive does not invalidate Hume's logical observation; but they do go against what he concludes from it. What they say is that both norm systems and seemingly neutral descriptions share the character of the question »When have you stopped beating your wife?«: they make presuppositions which are not stated in the sentence itself, but without which the formulation of the sentence becomes meaningless.

It can be argued that the very distinction between »is-« and »ought-sentences« is of the same character. The understanding of the descriptive statement presupposes our familiar »naïve« correspondence theory of truth; that of the prescriptive statement builds tacitly on a no less naïve understanding of the freewill problem: A prescriptive sentence »You ought to do X« presupposes, at least as understood in Hume's argument, that »you« are in possession of a Free Will and that you are hence able to decide sovereignly to do or not do X. But the relation between free will and determination is more complex than this. The naïve conception of the Free Will renders the momentary feeling of deciding freely (e.g.) to shout at your neighbour because the gangster deserves it. But thinking back tomorrow at your present rage you may think that you overreacted on Mr. Jones's jesting provocations *because* you had slept badly; if it is your husband who shouts you may think so already in the moment when it happens. What one decides to do is thus not as independent of what *is* as he feels in the moment. But further, reversely: what *is* and influences one's way to act encompasses not only lack of sleep and actions performed by his neighbour but also norm systems—norms which Mr. Jones has transgressed as well as norms which allow one to scold the scoundrel or which constrain one more than he can bear.

From the statement that »Mr. Jones has stopped beating his wife« we may conclude that Mr. Jones is or has been married, since the statement would be meaningless without this presupposition. From statements building upon an indefinite array of presuppositions we cannot decide a priori which conclusions can and which cannot be made; in particular, if an »is-statement« has normative

It is, no doubt, legitimate to characterize Popper's and Lakatos's views as ideologies for scientists; but that does not prove in itself that they have not got a descriptive point. In Lakatos's case this was argued quite extensively above. As far as Popper is concerned, the question is rather, *which* point? Is he right when declaring his methodological prescriptions to be purely utilitarian norms, a guide to how science will progress most rapidly? Or are they misleading in this respect, which would mean that the purported cognitive content of the ideology is *wrong* in the sense suggested in note 139? That the latter possibility is to be preferred was also argued in some depth above. But Popperianism also has another level of cognitive content with normative implications—*viz* that science progresses because it follows Popper's precepts (cf. note 44), which implies that scientists have to be respected as »objective beasts« by the surrounding society, and in particular have to be more highly respected than Marxists and psychoanalysts and their kin. This norm certainly *is* useful for the scientific community—but mainly so with regard to its similarities with the world of organized crime[141].

Value relativism and value nihilism

Another look at the categories of the general philosophy of morality will permit us supplementary insights into the discussions about scientific development.

It was asserted above that morality is able to live with dilemmas. This is only partly true. Firstly, of course, the reconstruction of incoherent rule systems through bargaining and various kinds of thought experiments have the function (and, as far as many literary thought experiments are concerned, the deliberate aim[142]) of exposing and thereby to solve, to

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presuppositions (as »Mr. Jones is a criminal«, where the notion of a criminal is defined legally and may encompass norms about what should be done to criminals), then Hume's argument fails.

[141] Members of the Sicilian Mafia speak of themselves, and want to be spoken of, as *uomini d'onore*, as *men of honour*.

[142] Think, e.g., of Sophocles' *Antigone*, which confronts the norms of the city state and of political society (represented by Creon) with those of ancestral morality and

surmount or to reconcile them. Secondly, awareness of the existence of inconsistencies in the moral system may (in particular in secularized and enlightenment periods, when the Solomonic wisdom of religious authorities is questioned) lead to *value relativism:* norms there must be, but we choose them freely—*man* is the measure of all things, as once formulated by Protagoras (and held by utilitarianism). Or the conclusion may be *value nihilism:* »If God does not exist, then everything is permitted« (Ivan Karamassov) or »*Good* is only what is good for the strongest«, as Plato expressed the ultimate consequence of Protagoras' relativism in *Gorgias* and the *Republic*.

Both attitudes can be found within the philosophy of science. Lakatos's whole methodology of »research programmes« is, indeed, a relativist reaction to the breakdown of Popper's claim that an absolute methodology *could*, and *should*, be applied if we want to know in spite of the fallible character of all knowledge: The community of experienced scientists is, in fact, the »measure of all things« within their science, those who decide which methods are to be accepted. Another runaway Popperian has taken the nihilist standpoint: Paul Feyerabend, whose discovery that Popper's proclaimed »rational method« does not work made him publish a book with the title *Against method*, in which it is claimed that »there is only *one* principle that can be defended under *all* circumstances and in *all* stages of human development. It is the principle: *anything goes*«[143].

Value nihilism is a tempting inference from the discovery that absolute norms have a hollow ring. It looks like another version of the Socratic principle that »the only thing which I know is that I know nothing«. But the probable outcome of practiced value nihilism is not Ivan Karamassov's

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human love (represented by Antigone and her *fiancé* Haimon).

[143] [Feyerabend 1975: 28]. Feyerabend himself declares the philosophy to be *anarchist* in his subtitle. Many currents, it is true, can be found within anarchism. Still, Feyerabend's principle reminds most of all of that which later anarchists have held to be malicious lies about Bakunin. The political quotation in the chapter leading forward to the principle is actually from Robespierre: »Virtue without terror is ineffective«. Feyerabend is an anarchist because it sounds so nice, and in spite of his confession (p. 20) to detest most anarchism and most anarchists past and present because of their seriousness and their lack of respect for human lives. In language and in the interest of philosophical provocation, too, *anything goes*.

gentle desperation but Rodion Raskolnikoff. Outside literature, and in a somewhat less drastic illustration: In local democracy it is unavoidable that those who decide know some of those whom they decide about. An absolute prevention of local favouritism and similar corrupt behaviours is therefore only possible if local democracy (and local government altogether) is abolished. The nihilist supporter of local autonomy will therefore have to drop the prohibition of favouritism—any norm which cannot be upheld absolutely cannot be upheld at all. The result, of course, will be that vaguely endemic corrupt manners become epidemic.

Norm systems, indeed, are not only reconstructed reflections of adequate patterns of behaviour. They are also what we usually take them for: *Regulators* of behaviour. Conflicting norms—in this case the norm of democratic government as close to those concerned as possible, and the norm of decent behaviour and of equal opportunities irrespective of kin, friendship and protection—are so too. They cannot be absolute prescriptions[144], but through acts of balancing (affected, among other things, by the socialization and tacit knowledge of the range to be given to each norm) they may still put certain limits to behaviour in a situation which is strained by contradictory claims and interests. Irrespective of the anomalies which were discussed above, this also holds for methodological norms like the empiricist and the falsificationist imperatives.

Institutional imperatives

Terms like »prescription«, »methodology« and »rule« are often used in the vicinity of Popper, Lakatos and Kuhn (and in many other quarters of the philosophy of science). Principles from the philosophy of morality and norms should therefore be applicable to the process of scientific development and work—as they were indeed applied above. As a matter of fact, however, this approach is far from traditional. The aspect of the sciences which is traditionally discussed through the concept of norms is the *sociology of science*.

[144] For this we do not need norms in conflict. The validity of rules in practice can never be more absolute than the linking of single acts to particular rules.

The seminal (indeed paradigmatic) work which launched this norm-based sociology of science was an article by Robert K. Merton from 1942 on »science and democratic structure«[145]. Concerning the »ethos of science«, Merton explains that

> The institutional goal of science is the extension of certified knowledge. The technical methods employed toward this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). The institutional imperatives (mores) derive from the goal and the methods. The entire structure of technical and moral norms implements the final objective. The technical norms of empirical evidence, adequate, valid and reliable, is a prerequisite for sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. The mores of science possess a methodological rationale but they are binding, not only because they are procedurally efficient,

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[145] This was the title given to it when it appeared as a chapter in [Merton 1968]. Originally it was entitled "A Note on Science and Democracy". Both versions of the title point to the origin of the essay in the antifascist debates of the late 1930s about the role of science. The setting was explained thus by Merton:

> [...] A tower of ivory becomes untenable when its walls are under prolonged assault. After a long period of relative security, during which the pursuit and diffusion of knowledge had risen to a leading place if indeed not to the first rank in the scale of cultural values, scientists are compelled to vindicate the ways of science to man. Thus they have come full circle to the point of the reemergence of science in the modern world. Three centuries ago, when the institution of science could claim little independent warrant for social support, natural philosophers were likewise led to justify science as a means to the culturally validated ends of economic utility and the glorification of God. With the unending flow of achievement, however, the instrumental was transformed into the terminal, the means into the end. Thus fortified, the scientist came to regard himself as independent of society and to consider science as a self-validating enterprise which was in society but not of it. A frontal assault on the autonomy of science was required to convert this sanguine isolationism into realistic participation in the revolutionary conflict of cultures. The joining of the issue has led to a clarification of the ethos of modern science.

The reasons for public distrust of science have evidently changed since 1942. The ecological crisis, for instance, was still below the horizon, and the involvement of social science in the management of minds through scientifically designed advertisement and propaganda were not yet conspicuous (although it *had* begun as early as the 1920s). But the phenomenon of public distrust remains, for which reason clarification of the actual ethos of science is still important—not primarily for purposes of self-defence but rather as a basis for self-critical reflection.

but because they are believed right and good. They are moral as well as technical prescriptions[146]

The final sentence has a clearly Kantian ring. »Moral prescriptions« are those which (are held to) have absolute character, while »technical prescriptions« are merely tools for strategic rationality. But the actual understanding of the nature of norms comes close to the one proposed above. »Institutional imperatives«, in fact, are understood as *norms which at least to a certain degree must be respected if the institution is going to fulfill its presumed role—in casu*, the production of »certified« (as opposed, e.g., to *revealed*) knowledge.

These imperatives are not codified explicitly in any catechism for future scientists; they become visible, as Merton points out, in the »moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contravention of the ethos«[147].

Merton himself, however, codified the system of institutional imperatives, finding four of them:

1. *Communism*, »in the non-technical and extended sense of common ownership of goods«. Apart from eponymity expressing recognition (»Boyle's law«, »Rorschach test«), nobody has property rights to scientific knowledge. Scientific results should be made public, firstly so that others may use them, secondly in order to be subjected to criticism (prerequisites for cumulativeness and certification, respectively). And further: »The communism of the scientific ethos is incompatible with the definition of technology as 'private property' in a capitalist economy. Current writings

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[146] [Merton 1968/1942: 606f]. Two terminological details should be taken note of: Merton speaks of *certified*, not *certain* knowledge. And he speaks about empirical *confirmation*, not *verification*. The quotation can hence not be used to classify Merton as a *positivist* (and hence to dismiss him as irrelevant—cf. [Kjørup 1985: 135]). It is nothing but a concise common sense description of the aims and methods of scientific work.

[147] [1968/1942: 605f]. As an aside on Merton's own »context of discovery« it can be told that Merton had intensive first-hand knowledge of these moral attitudes. He wrote his PhD dissertation under the guidance of the historian of science George Sarton, who continually taught him about what and what not to do—as Merton told in a lecture at the George Sarton Centennial Conference, Ghent 1984.

on the 'frustration of science' reflect this conflict. Patents proclaim exclusive rights for use and, often, nonuse. The suppression of invention denies the rationale of scientific production and diffusion«.

2. *Universalism*, which »finds immediate expression in the canon that truth-claims, whatever their source, are to be subjected to *preestablished impersonal criteria:* consonant with observation and with previously confirmed knowledge. The acceptance or rejection of claims entering the lists of science« is not to depend on the personal or social attributes of their protagonist: his race, nationality, religion, class and personal qualities are as such irrelevant«. Universalism thus deals with knowledge, but no less with *persons*. The optimal progress of knowledge requires that nobody who is competent is excluded from the scientific institution[148].

3. *Disinterestedness* is the norm which explains those features of scientific activity which traditional hagiography derives from the particular moral qualities of scientists (altruism, honesty, »objectivity«), or from their personal motives: curiosity, thirst for knowledge. Scientific disinterestedness requires that the scientist should not distort his science or his results in order to gain personal advantage or in the service of particular interests (in the terminology introduced on p. 117, disinterestedness thus imposes the elimination of particularism). Transgressions in this field are probably the ones which are most severely punished. As a rule, the scientist who has been caught in deliberate fraud can start looking around for a different career[149]. The same thing, of course, will happen to an accountant who

[148] This was evidently, when written in 1942, a reference to the expulsion of Jews and Social Democrats from German universities. The effect had been described bitingly by the old David Hilbert (too much of a Nestor of mathematics to be maltreated by the Nazis at that date), as NS-Reichsminister Rust asked him in 1934 whether it was really true that the Mathematical Institute in Göttingen had suffered from the expulsion of Jews and their friends: »Jelitten? Dat has nicht jelitten, herr Minister. Dat jibt es doch janich mehr!« (quoted from [Neuenschwander & Burmann 1987:25]).

[149] In one Danish case from the 1950s, the Rector Magnificus of Copenhagen University resigned from his office, not because he had committed fraud himself, but because he had been unwilling to believe evidence that his son-in-law had done so. The son-in-law when discovered gave up his scientific career, changed his too characteristic name, and settled down as a practicing physician. A number of more

has betrayed his employer. Other professions, in contrast, have quite different norms. The strictness with which the accountant is treated can be compared with the lenience with which his employer is handled when caught in insider trade at the stock market; the bad luck of the cheating scientist can be compared to the praise bestowed upon the fraudulently imaginative journalist[150].

Disinterestedness does not prohibit (and no norm can prevent) misunderstandings of experiments, blindness to adverse results, and overly dogmatic trust in established theories. What it proscribes are cases like the physics professor from Copenhagen going to Thule in January 1968 »in order to prove« that the crash of a B 52 carrying a number of H-bombs had produced no radioactive pollution. Jørgen Koch's slip (occurring during an interview in the radio news) demonstrates that the norm is not universally respected. But the start one feels when hearing statements like this demonstrates the existence of the norm *as a norm*—and overt admis-

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recent cases from the US are described in [Broad and Wade 1982].

[150] One example: Some years ago the Danish journalist Jan Stage was forced to admit in court that he had invented himself an interview with Bület Ecevit, endangering thus this Turkish social democratic politician whom the military government had forbidden to make any public announcement. Shortly afterwards, Stage's employer *Politiken* ran an advertisement campaign featuring precisely Jan Stage—much better known and much better suited for advertisement purposes than his honest colleagues, it seems.

There are good reasons that the scientist and the accountant are more strictly regimented than most other professions: In both cases, controls are almost automatically applied; and in both cases, the fraud undermines the very *raison-d'être* of the profession. The journalist, on his part, is rarely paid solely for telling the truth; entertainment value is quite as important for newspapers getting an appreciable part of their income from advertisement and the rest from customers paying rather for entertainment and relaxation and for the subjective impression of being informed than for information itself (in the case of papers which get support from political parties, employers' or trade unions, etc., other reasons ask at least for a specific perspective on truth).

Professional honesty thus depends on the situation and the rationale of the profession. So does the particular character of that honesty. The accountant has to be honest about money. The scientist who mixes up private and institutional money may be rebuked, fined, or perhaps dismissed—but may in even the worst of cases hope to get another job within the profession. Public trust in science, and the confidence with which others may use his scientific results, are not undermined.

sions of the kind can only undermine the public trust in the scientific institution. The rule thus *is* an institutional imperative: rampant disrespect endangers the institution.

4. *Organized scepticism* is the claim of the scientific institution that it should not be bound by the interests of other institutions in its work, and not be bound by prevailing opinion and prejudice[151]. This norm is certainly useful for the cognitive functioning of science by offering moral support to scientists who risk conflict with those in power by staying loyal to what they (suppose to) know, and by censuring opportunism; but the attitude is one which will easily bring scientist into conflict with their surrounding society[152]:

> [...] Most institutions demand unqualified faith; but the institution of science makes scepticism a virtue. Every institution involves, in this sense, a sacred area that is resistant to profane examination in terms of scientific observation and logic. The institution of science itself involves emotional adherence to certain values. But whether it be the sacred sphere of political convictions or religious faith or economic rights, the scientific investigator does not conduct himself in the prescribed uncritical and ritualistic fashion. He does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed.

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[151] It will be remembered from Chapter IV that this question was central to one of the important differences between Popper_1 and Popper_2/Lakatos: Popper_1's methodological conventionalism tends to make scientists bend to conventional wisdom; Lakatos's methodology of research programmes, on the contrary, will protect research challenging accepted opinions as long as it remains fruitful.

[152] Maintaining this ideal in spite of pressure is thus, in the isolated instance, contrary to act utilitarianism. It would be much more remunerative to agree with government officials, newspaper magnates, etc. But it may be prescribed by rule utilitarianism: a science which has bent too obviously to the desires or requests of authority tends to be decried at the next turn of the road (in Spring 1990, the whole concept of »social science« was abolished in the late German Democratic Republic, as a consequence of the too apologetic behaviour of too many social scientists!).

Much hagiographic history of science probably serves the purpose of mediating between act and rule utilitarianism at precisely this point: If science can be shown to have been right in resisting heroically the now defamed authorities of former times, it might be right in continuing to defy authority.

—or, to be more precise: His professional ethos tells him that he does not need to preserve it. As we know, scientists are not *only* scientists but also members of society, and many of them split their allegiance between the norms of their profession and those of society (or their social group) at large. Obviously, the Atlantic allegiance of the physics professor mentioned above outweighed not only his allegiance to the norms of his profession but even his awareness that there might be a problem.

Organized scepticism, it should be noted, has nothing to do with the customary concept of scepticism. It does not imply scepticism toward the possibility of obtaining reliable scientific knowledge—on the contrary, the latter kind of scepticism is often promoted by those who wish to domesticate the provocative self-assurance of science encroaching on the sacred domains of other institutions or to wash their hands when convicted of having acted in bad faith[153]. It is therefore totally mistaken to cite as evidence against the norm of organized scepticism five scientists out of seventeen who would not accept reports of flying-saucers »no matter who made the observations« ([Sklair 1973: 154], quoting a study from 1960); precisely these five, indeed, illustrate the norm, being so sure about the assertions of their science regarding the impossibility of interstellar travel and being so knowledgeable about the susceptibility of even fellow scientists to mass illusion that they felt entitled to contradict every report, be it published in *New York Times* or made by the president of the National Association for the Advancement of Science. »Organized scepticism« does not contradict Kuhn's findings about »dogmatism«; the two are, in fact, sides of the same coin.

Theoretical versus applied science

Merton's article became a Kuhnian paradigm for a generation of American sociology of science, though mostly in a sadly trivialized reading. As a reaction, a later generation has been eager to show that it is all wrong.

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[153] Thus the psychologist H. J. Eysenck, when it turned out that the research on which he drew for proving that intelligence was determined by inheritance alone was one immense fraud, started a campaign to prove (Popperian methodology at hand) that astrology was as good a science as any other.

Many of the objections are irrelevant in the present context, but one point of the discussion is not.

If scientific activity is regulated by the Mertonian norms, then scientists in general, or at least the majority, should subscribe to these norms when interviewed by sociologists—thus an alluring start of the argument. Most of those whom the sociologists regard as scientists, however, are active in industrial and other applied sciences; a survey which is meant to be representative will hence be dominated by applied scientists; some surveys have indeed looked solely at industrial scientists. The result of such studies has been that many industrial and similar scientists do not subscribe to the Mertonian norm system, in particular not to the communist imperative.

Though often represented as counter-evidence (e.g., in [Sklair 1973:154]), this is actually an essential underpinning of Merton's argument, which connects it to the above discussion. Industrial and other applied scientists do *not* work inside an institution whose primary »institutional goal [...] is the extension of certified knowledge«. The aim of their work is the adaption and application of new or (often) old knowledge (cf. chapter VII); they are paid for producing knowledge which can end up as privately owned technology[154]. That they do not follow all Mertonian norms (or do so only in a restrictive interpretation, cf. below) merely illustrates that these are *institutional* imperatives: they have little to do with the personal character and history of scientists, and they do not belong to the corpus of already existing scientific knowledge as an inseparable attribute; they crystallize within an institution, i.e., a network of social interactions organized around a particular core value (as it was called above, see note 134): »the extension of certified knowledge« in a collective process.

It can still be objected that even many scientists working within the institutions of theoretical science (as well as whole institutions) do not obey the norms. The Ancient Testament, however, also abounds with stories about members of the Twelve Tribes killing each other, which does not

[154] These formulations only fit industrial scientists precisely. Applied social scientists, for instance, rarely produce artefacts that may be bought and sold; their employer, however (be it the state or a private institution), will control their work and use its results in much the same way as an industrial corporation controls and uses the work of an industrial scientist.

invalidate the norm »Thou shalt not kill« (those who cannot afford a horror movie may take a look at Judges 19-21). An analogue of the Popperian argument from p. 56 will be more justified in the present context than was the original version: The more often and the more strongly the norms are broken, the less efficient is the work of the institution, and the more likely is it to run into blind alleys. Keeping *all* knowledge as private property (i.e., secret or encoded in a way which impedes other from building on it) would mean the end of science; prohibiting the teaching of »Jewish« physics in Germany (1933 to 45) or (supposedly »bourgeois«) genetics in the Soviet Union (1948 to ca. 1955) delayed research significantly in both countries; etc.

Further norms, contradictions, contradictory interpretations

In another respect, the Mertonian norms seem to agree less well with the above analysis of the nature of norm systems: the scheme looks too simple, clear-cut and free of inner contradictions. It looks more like a set of »ethical rules« for the profession laid down by a supervisory committee than as a piece of real-life morality[155].

That the normative regulation of scientific practice is indeed much more equivocal was already pointed out by Robert Merton in a humorous and less widely read paper on »the ambiguities of scientists«. He lists nine pairs of mutually contradictory norms, beginning as follows [Merton 1963: 78f]:

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[155] The difference between such precisely stated »ethical rules« (for journalists, advertisement firms, etc.; *not* to be confounded with the general concept of ethics) and morality is that morality tells (more or less unambiguously) *how one should behave*. »Ethical rules« tend—in particular in cases where they are not derived from the aim of the profession but go against it, as in the case of e.g. advertisement ethics—to state (like law) the limits of the permissible, ultimately thus telling *how far one may deviate from decent behaviour without risk of condemnation or penalty*—or, if »positive« and non-committal, they tend like Popper's methodology to communicate the honesty and altruism of the profession.

(The trivialized reading of) Merton's scheme has indeed inspired the ethical rules for scientific behaviour which are administered by academic authorities in the US.

1. The scientist must be ready to make his new-found knowledge available to his peers as soon as possible, BUT he must avoid an undue tendency to rush into print. [...].
2. The scientist should not allow himself to be victimized by intellectual fads, those modish ideas that rise for a while and are doomed to disappear, BUT he must remain flexible, receptive to the promising new idea and avoid becoming ossified under the guise of responsibly maintaining intellectual traditions.
3. New scientific knowledge should be greatly esteemed, BUT the scientist should work without regard for the esteem of others.
4. The scientist must not advance claims to new knowledge until they are beyond reasonable dispute, BUT he should defend his new ideas and findings, no matter how great the opposition. [...].
5. The scientist should make every effort to know the work of predecessors and contemporaries in the field, BUT too much reading and erudition will only stultify creative work. [...].

[...]

and so, on and on

as stated at the end of the list of »moral dilemmas«. That a normative system may function even though ridden with such contradictions (and all practicing scientists know both the dilemmas and how to deal with them in single cases) hinges on the tacit knowledge of the participants in the social pattern that is regulated by the norms. Much of this tacit knowledge is part of the paradigm which governs work within a discipline; familiarity with this paradigm (and awareness of the number, the character and the severity of the anomalies with which it is confronted) allows the worker to decide whether a new suggestion should, e.g, be dismissed as a mere »intellectual fad« or hailed as a »promising new idea«[156]. The rest (inasmuch as two segments of tacit knowledge can be distinguished) structures the »merely social« interactions within the profession (e.g., whom to honour, and how).

Even the four »institutional imperatives« are ambiguous, and not interpreted in the same way by everybody. To some, for instance, the »communist« norm only means that final results should be made publicly known at some adequate moment without becoming thereby public

[156] That not all workers make the same choice is one of the points in an activity governed by norms and not by rules—cf. Kuhn [1970: 186] as quoted on p. 119.

property which can be used freely by everybody[157]. To others, the »communism of the scientific ethos« *is* indeed »incompatible with the definition of technology as 'private property' in a capitalist economy«, as claimed by Merton[158].

How the institutional imperatives are interpreted also changes—and *has* to change— from one discipline to the other. To see why we may look at the implications of »universalism« and »disinterestedness«. Both are rejections (in somewhat different terms) of *particularism*, and thus *objectivity norms*. Yet how science achieves actual objectivity (and thus implements the two imperatives) depends on the problems and methods of the single discipline. In those branches of medical research which test individual cures, double-blind testing functions excellently; but to claim that this is the only way to guarantee scientific objectivity is evidently preposterous (already in medical disciplines like epidemiology or preventive medicine, but *a fortiori* in sciences like astronomy, sociology and historical research). In sociology, where the value system of scientific workers may overlap with the value system that regulates the social unit under investigation, it may give sense to claim that sociological science should be *value-free*; as demonstrated by numerous hilariously absurd discussions taking place in the late 1960s, claims that science in general (and not only sociology and sociology-like human and social sciences) is/is not/should be/should not be value-free give little meaning.

Even though discussions about the responsibility and societal involvement of the sciences are often expressed in different terms nowadays, the theme can be pursued with profit. The idea that sociology should be value-free was formulated by Max Weber; already in *his* writings the use of the

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[157] In recent years, this attitude has been demonstrated in glaring and dismal dimensions by the behaviour of the molecular biologist Robert Gallo, purportedly co-discoverer of the HIV-virus. Within the humanities it is widespread in all fields where a monopoly on the right to publish findings can be upheld—e.g., in archaeology and Assyriology.

[158] As the General Union of Danish Students fought its battle against contract research at universities in the early 1970s, it took care to appeal to both versions of the norm. Arguments that research paid by contracts could not be published freely was correlated with the »narrow« interpretation; the slogan »Research for the people, not for profit« was an appeal to the broad version.

concept is ambiguous, and later interpretations are no less divergent. The sociologist Alvin Gouldner [1973: 11-13], in a lucid and stylistically sparkling essay on the meaning and function of »the myth of a value-free sociology«, starts by presenting the opportunities offered by the concept:

> [The value-free doctrine] enhanced a freedom from moral compulsiveness; it permitted a partial escape from the parochial prescriptions of the sociologist's local or native culture. Above all, effective internalization of the value-free principle has always encouraged at least a temporary suspension of the moralizing reflexes built into the sociologist by his own society. From one perspective, this of course has its dangers—a disorienting normlessness and moral indifference. From another standpoint, however, the value-free principle might also have provided a *moral* as well as an intellectual *opportunity*. [...].
>
> The value-free doctrine thus had a paradoxical potentiality; it might enable men to make *better* value judgements rather than *none*. [...].
>
> The value-free doctrine could have meant an opportunity for a more authentic morality. It could and sometimes did aid men in transcending the morality of their »tribe«, [...], and to see themselves and others from the standpoint of a wider range of significant cultures.

The value-free doctrine could thus, we see, push sociologists from particularism toward universalism in their value judgements, and enhance the organized scepticism of the discipline. But Gouldner goes on with harsher words:

> But the value-free doctrine also had other, less fortunate results as well.
>
> [...] many [...] used the value-free postulate as an excuse for pursuing their private impulses to the neglect of their public responsibilities [...]. Insofar as the value-free doctrine failed to realize its potentialities it did so because its deepest impulses were [...] dualistic. [...].
>
> On the negative side, it may be noted that the value-free doctrine is useful both to those who want to escape *from* the world and those who want to escape *into* it. It is useful to those [...] who live off sociology rather than for it, and who think of sociology as a way of getting ahead in the world by providing them with neutral techniques that may be sold on the open market to any buyer. The belief that it is not the business of a sociologist to make value judgements is taken, by some, to mean that the market on which they can vend their skills is unlimited. From such a standpoint, there is no reason why one cannot sell his knowledge to spread a disease just as freely as he can fight it. [...].
>
> In still other cases, the image of the value-free sociology is the armour of the alienated sociologist's self. [...]. Self-doubt finds its anodyne in the image

> of a value-free sociology because this transforms [the sociologist's] alienation into an intellectual principle. [...].
>
> There is on way in which those who desert the world and those who sell out to it have something in common. Neither group can adopt an openly critical stance toward society. [...].

When the value-free principle is understood in these ways—not as a statement that sociology need not repeat or apply prevalent value judgements but as a claim that the activity of the sociologist is itself above moral judgement—it has thus become a disguise for breaches of the norms of disinterestedness, organized scepticism and communism (in the broad interpretation which emphasizes the general social responsibility of the sciences). It is only in this variant (when no longer a norm of objectivity but only of marketability) that the value-free postulate can be generalized to all sciences.

Other fields possess their own more or less idiosyncratic specifications of the »objectivity norms«, which we may pass over without further discussion. The examples of the double-blind technique and the value-free doctrine should suffice to make a general point: that norms at the level of Merton's »institutional imperatives« function not only directly as regulators of scientific behaviour but also as »core values« around which more specific norms crystallize during the practice of the single scientific field. The conflicting interpretations of the value-free principle also illustrate that the norms regulating scientific work (occasionally down to the specific norms of the paradigm—cf. note 74) are susceptible to interaction with the norms of general social life.

VII. THE THEORY OF INTERDISCIPLINARY AND APPLIED SCIENCE

The discussion of Chapters IV and V presupposed (numerous examples at hand) that scientific work is made within separate disciplines; Chapter VI assumed that science may be either »theoretical« or »applied«. Both suppositions refer to common sense knowledge; none the less, both are contradicted by many traditional philosophies of science, not least empiricism[159]. At best, they regard the division of science into disciplines and the split between theoretical and applied knowledge as purely pragmatic divisions of labour with no epistemological or philosophical basis—»Knowledge is one. Its division into subjects is a concession to human weakness« (H. J. Mackinder, quoted from [Mackay 1977: 99]).

The aim of the present chapter is to show that the divisions *are* epistemologically founded, and to explore the relations between disciplinary and interdisciplinary science and between these and the pair »basic«/»applied« science. After having looked in the preceding chapters at the nature of (scientific and general) knowledge and at that specific practice which aims at the production of scientific knowledge we shall thereby be brought to consider the production of scientific knowledge as an aspect of general social practice.

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[159] Even Habermas, while setting aside the humanities and emancipatory social science, follows Peirce in his identification of natural science as a mere means to produce technology—cf. [Høyrup 1993:167].

Know-how and know-why

That scientific insight (»know-why«) can be used in strategically rational practice as »know-how« is both a commonplace and part of our daily experience—as illustrated by an advertisement slogan painted on a van belonging to a pharmaceutics firm which I noticed in the street some years ago: »Today's theory—tomorrow's therapy«. The commonplace is as old as the scientific revolution—Thomas More as well as Francis Bacon claimed that natural philosophy had the double aim of producing useful technology and honouring God by studying his creation[160].

The commonplace was translated into an analytical tool for research statistics around 1960. At that moment, the OECD had come to consider the development of adequate theory a crucial fundament for technological and social progress (and thus for the supremacy of the »West« over the Soviet Union[161]). As a consequence, the OECD embarked upon a massive

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[160] On the other hand, the idea that technical practice is or should be derived from scientific theory is not much older in Europe than the late Renaissance (in the Islamic world, it can be traced back to the eight or ninth century C.E.). Aristotle, in particular, tells in the *Metaphysics* that *first* the practical arts were invented in order to provide us with the necessities of life. Later, theoretical science developed, which was honoured more highly because it had *no* technical purpose (cf. [Høyrup 1993: 22f]). Aristotle's slogan would thus rather have been »yesterday's therapy, today's theory«—or, even more radically, and denying that the two have any inner connection, »Yesterday therapy. Today's progress: theory«.

We may find Aristotle's point of view outdated, and perhaps be repelled by the philosopher's disdain for those who provided him with food and shelter. Yet what he tells was historically true in his times, and corresponds exactly to an institutional and epistemological split between practitioner's knowledge and theoretical »know-why« which only broke down definitively in the nineteenth century. Here, the typical Renaissance legitimation of science as adoration of the creator *and* civically useful should not mislead us: It refers to the use by practitioners of the *elementary* items from the theoretician's tool-kit.

[161] As we now know, this proved right, in the sense that the socialist countries were unable to transform their massive investments in research and scientific education into productive and institutional innovation. In the capitalist world, on the contrary, the outcome of the first oil crisis, where the need for technological change had imposed itself, was accelerated development on all levels.

Technological development, it is true, does not in itself create or destroy social systems. But unequal economic development provided the basis for what came to happen on the political and institutional levels.

patronage of »science policy« programs in the member countries, and in order to monitor the programs and the distribution of research funds, a handbook containing prescriptions for the production of research statistics (the »Frascati Manual«) was prepared. The manual contains the following definitions of »basic« and »applied« research [*Measurement of Scientific and Technical Activities*, 19f]:

— Basic research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundations of phenomena and observable facts, without any particular application in view.
  Basic research analyses properties, structures and relationships with a view to formulating and testing hypotheses, theories or laws.
— Applied research is also original investigation undertaken in order to acquire new knowledge. It is, however, directed primarily towards a specific practical aim or objective.
  Applied research is undertaken either to determine possible uses for the findings of basic research or to determine new methods or ways of achieving some specific and pre-determined objectives [...].[162]

In agreement with the commonplace, the OECD thus sees the realms of »theory« and »therapy«, i.e., basic research and practical technology, as belonging together, linked through applied research (and, in fact, through a further bond constituted by »experimental development«, which makes science-policy makers speak of R&D, »research and development«)—but still as clearly different activities. Why this happened is a question to which we shall return below (p. 155 onwards).

The acquisition of theoretical knowledge

From a common-sense point of view, OECD's strategy may seem awkward. If technological progress and know-how is the aim, why then finance activities aiming at the acquisition of »new knowledge of the underlying foundations of phenomena and observable facts, without any particular application in view« (the kind of knowledge of which I speak as »theoretical«) and not merely research looking for *the relevant kinds* of

[162] It should be clear already from this definition that »applied research« may be a much more creative process than the trivial »application of science« assumed by Popper and others (cf. p. 92 and note 109).

knowledge? Or, if basic research *is* relevant, why is there any need particular stage of »applied science«?

The answer is connected to the dynamics of the Kuhnian cyc described above, but may also be approached from the common dyna of student projects as made at Roskilde University.

Such a project normally takes its starting point in an intere *phenomenon* (Greek for »that which is seen«) providing a first formul of the problem and a first idea about the relevant research stra However, work guided by this formulation will normally set the ori phenomenon in a new light, *through concepts which only appear t participants in the project due to the first phase of the systematic investig* Then they redefine the problem (at least that is what they *should* do), continue work according to the new definition of the problem anc strategy. This spiral process may continue; in the end the treatment o problem (and indeed the problem itself) may have very little d connection to the phenomenon which originally inspired the work, much more with the internal but hidden structures which were reve by the systematic investigation (»the underlying foundations of phenor and observable facts« of the *Frascati Manual*).

The Kuhnian cycle of successive paradigms repeats the same struc in larger scale. In the pre-paradigmatic phase, scholars start investig phenomena which present themselves directly, and which are ei intriguing or in some sense important—maybe the curious fact that g when rubbed by a woollen cloth, attracts small pieces of paper, mayb effect of taxation principles on the productive activity and the distribu of wealth and poverty within the country. The initial investigation re in the discovery of new connections between phenomena giving ris new questions. In the ensuing phase, phenomena from seemingly c different domains (lightning!) may enter the field, while others from original area of investigation are discarded from view as after all irrele with regard to the underlying regularities which have come in focu when Mesmerism changed from a supposedly magnetic phenomenon hypnosis); in this phase (and more radically in later repetitions of the c questions will come to the fore which could not even have been imagi when the process started—as we have seen, the basic problems of

Kapital could only be asked when Adam Smith had split society into classes according to kind of income (cf. p. 61). But even though the questions could not be imagined directly from common sense observation of the phenomena which constituted the starting point, the answers remain relevant for explaining these phenomena—the Marxian analysis of the economic process tells better than both physiocrats and Adam Smith were able to do the dynamics of English economic life in the decades preceding the appearance of *The Wealth of Nations*.

What begins as an investigation of a particular range of phenomena belonging concretely together thus ends up as an investigation of the underlying regularities or laws for everything which can be explained (e.g.) from competition and monopolization of resources, or from Maxwell's equations for the electromagnetic field; what has begun as scrutiny of an arbitrary section of the world (whether delimited spatially or by association with a particular practice) is stepwise transformed into a general examination of reality under the particular *perspective* of the techniques and concepts developed by the discipline. In this way we are led to much more fundamental insights not only into the phenomena which set us on the track but also into others—but only into particular *aspects* of these phenomena[163].

This contradicts the general way of speaking according to which the different sciences deal with different *sections* of reality: biology deals with living beings (zoology with animals, botany with plants); physics with non-living matter; economics deals with the economy; sociology with *our* society and anthropology with the functioning of primitive societies[164]; electromagnetic theory of course deals with electricity and magnetism.

Yet electricity and magnetism are not specific sections of the physical world—they are all-pervasive. Similarly, no single human action is *solely*

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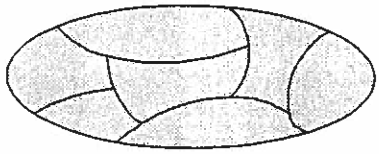
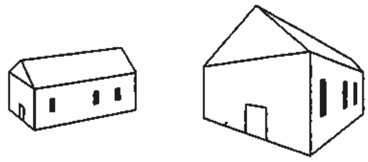
[163] That empiricist philosophy of science is blind to the epistemological foundation for the splitting of knowledge into disciplines is thus a specific instance of its general blindness to the role of *theory*.

[164] The latter point, alas, is no parody in bad taste; it renders the objection actually raised by a fellow member of the profession when the anthropologist A. Shiloh wondered [1975] that his discipline had done nothing to understand how the National Socialist system of extermination camps had been possible.

economic. When I buy a piece of cheese in the supermarket, my action can of course be analyzed as a step in the economic cycle. But how I move around when searching and paying may also be described by mechanical physics and anatomy; that I feel a need to eat is a matter for the physiologist, and that I wish to eat cheese could give an cultural anthropologist the occasion for an explanation along quite different lines; my choice of a particular brand instead of another would certainly be of interest for the advertisement psychologist; etc.



Separate sections of reality?



Or different perspectives on the same reality?

That we tend none the less to accept the common way of speaking illustrates to which extent we have come to see the world in which we live through the perspectives of the established sciences and to think in terms of their fundamental concepts. We have come to accept as self-evident, for instance, that society consists of an economic, a juridical and a political sphere; only second thoughts allow us to rediscover that these spheres are wholly abstract[165].

The simple Kuhnian cycle (as translated here into the metaphor of *perspectives in dynamic development*[166]) is not the whole truth about how disciplines develop. Above (p. 73), the »neo-classical« or »marginalist«

[165] It may be objected that the three spheres are embodied in separate institutions: The stock exchange (etc.), the judicial system, and the parliament-and-party complex. But the transactions going on at the stock exchange are undertaken on legal conditions established by political authorities (and their very meaning indeed defined in commercial and corporation law); similarly in the case of other seemingly sphere-bound institutions. The spheres remain abstractions and analytical tools.

[166] Where, if we continue the metaphor, each picture reveals where the camera should be moved next in order to take an even more detailed picture—a feature of the development of a discipline which was formulated on p. 116 as »progress toward greater objectivity of its subjectivity«.

revolution in academic economics was mentioned as a change of perspective which was inspired by political convenience rather than the inner dynamics of classical political economy as based on the labour theory of value. Similar examples are numerous[167]. More cognitively productive, however, are two other processes: branching and integration.

Branching of disciplines can be explained at several levels. Since the later seventeenth century, the number of active scientists has doubled every fifteen years[168]; since it is impossible to follow in even modest detail the technical work of more than a few hundred colleagues, larger disciplines tend to split into specialties which loose contact (cf. p. 96 on the magnitude of the group sharing a paradigm in the narrow sense). But the process can also be explained at the epistemological level: Every time new conceptualizations or new techniques emerge within a discipline (i.e., at least when a new paradigm takes over), new open problems turn up, and specializations materialize when groups pursue different questions (and tend to loose contact quickly if further conceptualizations and techniques developed around one question are not obviously relevant for other question types[169].

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[167] Without having researched the matter directly it is for instance my definite feeling that the shift from Mertonian to anti-Mertonian sociology of science (see p. 138) is better characterized as an »academic patricide« than as a Kuhnian revolution. In disciplines without a firm inner structure (similar in many ways to Kuhn's pre-paradigmatic sciences), such patricides may be an easy way for the young wolves of a new generation (in the present case *my* generation) to show their academic valour.

[168] This rule of thumb was stated by D. Price [1963]. It is certainly only a rough approximation, and the process cannot go on in this pace for much longer (it seems indeed to have decelerated somewhat over the last 20 years), but it is not without value. A similar growth rate turns out to hold for the number of scientific publications appearing each year and for the population of scientific journals.

[169] Explanations at still other levels can also be given, e.g. through the specific recruitment structure of a scientific institution. One such explanation has been given of the explosive development of new disciplines in nineteenth-century Germany: Since there could be only one (ordinary) professor for each discipline at a university, the only way to argue for a new professorial position was the invention of a new sub-discipline.

The various explanations do not contradict each other. They relate to each other in much the same way as do the pushing, the material and shape of the billiard

Integration or convergence take place when the methods and concepts developed within one field turn out to be relevant for the understanding of another, and when two fields provide compatible explanations of a common object; they are furthered by the norm that scientific explanations should be consistent[170]. At times they lead to a genuine integration of separate disciplines into one, at times the process is better described as an assimilation (in a Piagetian sense) of a whole domain to the paradigm of another discipline (as when the neogrammarian sound shift laws were explained by structuralist linguistics—cf. p. 102); at times a new discipline emerges from the process alongside those which entered it; mostly (since the delimitation of disciplines is always blurred) the outcome is somewhere in between. In any case (as a colleague once summed up my view in a metaphor), the interplay between branching and integration assures that »the tree of science is no spruce but a banyan tree«.

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ball, and the configuration of the table.

[170] The ultimate consequence of this norm goes further than the integration of single disciplines. Since *reality* (or *physical reality* or *the reality of human life*) is one, the descriptions provided by the different sciences ought to be compatible and in the final instance to be put together in a large coherent system of *unified science*. This was the dream of the Romanticists, and again of the logical empiricists (Kuhn's *Structure of Scientific Revolutions* was actually published in the "International Encyclopedia of Unified Science", a series directed by the most important logical empiricists). The neo-humanist movement in early nineteenth-century Germany dreamt of unified *Geisteswissenschaft*. A rather recent formulation (concerning the social sciences alone) runs as follows

> Economics and anthropology, and for that matter sociology and political science as well, are all—insofar as they are scientific—*ultimately the same science*. As economics broadens its horizons, it will increasingly seem to be invading realms of behavior that have in the past been reserved to other social disciplines. As other social sciences become more rigorous, they will increasingly grow to resemble economics (and, indeed, to *be* economics).
>
> [Hirschleifer 1977: 133]

The formulations are rather asymmetric, and social scientists outside economics will probably find them outright imperialist. But the principle remains the same.

The »Scientific-Technological Revolution«

The spiral development of scientific disciplines lead, thus it was stated above, »to much more fundamental insights not only into the phenomena which set us on the track but also into others—but only into particular *aspects* of these phenomena«. But whoever wants to build a bridge or to improve the educational standard of the young generation is interested in the bridge or education *as functioning wholes*, not only in aspects. A bridge should be able to carry its load when built; but it should also be stable toward wind and (depending on its location) earthquakes, it should not corrode, and at best it should also correspond to certain aesthetic norms; an educational policy should involve *what* to teach to whom, but it does not function if teaching is badly made, if those who should be taught cannot afford participation, or if teachers cannot be recruited. Practice is concerned with many-sided *sections* of reality (by necessity, if a »section of reality« is understood as what belongs together within a particular practice).

This difference between the orientation of »know-why« and »know-how« is the reason that they were not only carried by separate professions but to a large extent by non-communicating professions until the early nineteenth century: The higher levels of theory were carried by university scholars, members of scientific academies, etc. Practitioners, on their part, were taught as apprentices by other practitioners. Most of their knowledge had originated in direct practice; what was borrowed from theoreticians consisted as a rule of specific techniques (e.g., the use of logarithms in navigation) and not of connected insights; normally these techniques would belong on the basic and not on the advanced levels of contemporary science[171].

The first major change of this pattern was the appearance of the scientifically taught engineer in the early nineteenth century (cf. [Høyrup 1993: 139]). Around the engineering schools, a particular »engineering

[171] It goes by itself that this is only a rough approximation. It is, however, significantly less distorted than the opposite simplification: that the Technological Revolution taking place since the late Renaissance was derived from the Scientific Revolution.

science« developed (cf. [Channell 1982]), the aim of which was so to speak to translate and combine the knowledge of the »aspect-oriented« sciences into practically relevant information.

Efforts to integrate *theory developed with an eye on application* and *practice making use of actual research results* (and not just of the results that the engineer had been taught in his youth by a teacher who had learned about them in *his* youth in the university) began around c. 1850, first in organic chemistry and soon in electrotechnology (Siemens) and metallurgy (Krupp etc.). This step has been spoken of as the beginning of the »Scientific-Technological Revolution«. It was contemporary with parallel attempts to develop behavioural sciences for use in »social engineering«, the most important examples being probably Galton's eugenics and Lombroso's physiognomic criminology (cf. [Høyrup 1993:152]).

World War I, along with sonar, poison gas and other new technologies developed by physical scientists on the basis of their theoretical insights and their best research techniques, gave rise (in alliance with contemporary industrial needs) to the development of »engineering psychology«, scientific investigation of how to design machinery in agreement with what psychology could known about the perception, discrimination and reaction capabilities of the human operator (cf. [Chapanis 1968])[172]. Alfred Binet's and Cyril Burt's creation of the IQ-test-technique (cf. p. 7) occurred in the same period and exemplifies the integration of psychological science with other divisions of general practice (*in casu* the educational system). It was followed in the inter-war period (if we restrict the list to human, social and organizational sciences) by the creation of industrial sociology, by mass media studies aiming at advertisement efficiency, and welfare economics (John Maynard Keynes, Gunnar Myrdal); further, during World War II, by operations research, enhanced propaganda studies, and by studies of the »cultural anthropology of the enemy« (undertaken in particular by Ruth

[172] The establishment of engineering psychology thus exemplifies that direct application of science in industry and warfare which provoked the Cambridge mathematician Hardy to formulate in 1915 that »a science is said to be useful if its development tends to accentuate the existing inequalities in the distribution of wealth, or more directly promotes the destruction of human life« (quoted in [Hardy 1967: 120]).

Benedict; see [Ember & Ember 1977: 42f]). After 1950, the OEEC and its successor organization OECD promoted the generalization of science-based »policies« (the very use of the word in this sense is indeed a post-war phenomenon): economic policy, educational policy, criminal policy, population policy, technology policy, science policy, etc. The postwar era can hence be regarded as the inauguration the mature phase of the Scientific-Technological Revolution[173]. This situation, and its problems, is what produced the *Frascati manual;* »science policy« is indeed a meta-policy meant to produce the scientific knowledge needed in the other policies.

Some of the sciences created during the scientific-technological revolution started from a low level of theoretical knowledge and developed the necessary knowledge directly for the purpose of application. The whole field of mathematical statistics (which has had an splendid career since then) was founded by Galton in this way as a tool for eugenics; Binet's IQ tests represent a similar instance. Both cases are characterized by the absence of developed theoretical disciplines which could have served. The general experience has been, however, that the theoretical sciences were in possession of relevant knowledge of importance. If we refer to the questions formulated on p. 148 it is therefore clear why the science policy experts of the OECD would find it appropriate to invest in basic research.

It had also been common experience (since the development of »engineering science«) that the knowledge possessed by the theoretical disciplines could not be used directly. If practice regards a *section* of reality, and theoretical disciplines only provide a particular perspective on this and other sections, no single theoretical discipline can do the job (whether

[173] Evidently, speaking of this phase as »mature« does not imply that nothing new is going to happen. As a matter of fact, another phase shift is already taking place. The early integration of scientific knowledge into machinery and practical processes was, so to speak, put in once and for all: Burt constructed a number of tasks and put them together and then standardized the test; afterwards, the test was ready for use and would not be changed (only replaced by a new version when it proved inadequate). Increasingly since the 1960s, scientific knowledge is put into the machinery and the process itself so as to allow servo-control and other kinds of autoregulation (certainly with better success in car construction than in medical service and other social processes).

we think of bridge building or the planning of an educational policy); as an engineers' saying tells, »the difference between theory and practice consists in condensed water«[174]. Moreover, if communication between successive paradigms within the same field is only partial, the same holds by necessity in stronger form for the paradigms of different fields which must be combined in order to give a sufficiently complete understanding of the practical problem to be dealt with. The combination of several theoretical disciplines is therefore no easy process but one requiring active transformation of the conceptual frameworks involved, and active analysis of *how* the different frameworks relate to each other. This is the task of applied science. Often, applied science may have the further task to investigate questions left open by all theories—in the terms of the saying quoted above, when other disciplines have provided the knowledge needed to build a house which is stable and thermally and acoustically as well isolated as required, then the applied scientist has to find out how to modify their application in order to eliminate the unforeseen problem of condensed water.

A special task of applied science comes from its direct coupling to strategically rational action. Applied science should tell how to achieve certain effects by deliberate action (cf. the discussion on p. 111 of Humean causality as a formalization of strategic action), and it should thus single out those factors which can be influenced by a human agent (identical with, or acting on behalf of, those who want the effect to be achieved): an applied educational science which tells pedagogical skill to be a natural gift that cannot be taught is only of interest for educational authorities if it also tells how *they* may find the pedagogically gifted candidates; a science which

[174] A »real-life« example is the statement that anthropological investigation of societies that base their agriculture on artificial irrigation may well contribute to improvement of the social efficiency of irrigation systems—but only on the condition »that some anthropologists, some of the time, take their problems not from theories of social organization and social evolution but from the concerns of the bulk of mankind—problems of food production, productivity, income distribution, and employment—and work backwards from there« (R. Wade, in [Hunt & Hunt 1976: 405]). Basic science can only function as applied science if it borrows the characteristics of applied science.

tells the skill to be teachable is only of interest if it also tells *how* to teach it.

An applied science, it should be clear, is no trivial collection of results from the theoretical sciences. It is no less of an active process than theoretical science. It may also to some extent run through transformations similar to the Kuhnian cycle; but it cannot move from »section-« to »perspective-orientation« as long as it remains an applied science; its problems belong, so to speak, not to the science and its scientists but to those authorities, organizations or corporations that want to apply their knowledge[175].

Interdisciplinarity

Integration and convergence of theoretical disciplines is a familiar phenomenon since long, and might well have been spoken of as »interdisciplinarity«. As a matter of fact, however, nothing in that direction occurred—maybe because it was rarely clear whether genuine integration or cross-disciplinary inspiration was involved, maybe because the traditional normative ideal was »unified science«, not the unification of a few disciplines.

What goes nowadays under the name of interdisciplinarity evolved instead (without yet carrying the label) around the engineering and similar schools and the emergence of applied sciences. In a general sense, an »engineer« is a practitioner who has been taught, and makes use of, the results of actual science in his work (with some imprecision, an engineer is thus somebody who practices an applied science; actually, there is no one-to-one correspondence between separate engineering professions and actual applied sciences). The »engineers« of the nineteenth century were

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[175] Cf. note 154 and the surrounding text. An informative discussion of the implications of this ownership is [Schmid 1973], where it is argued that those who want to make »science for the people« should accept that »the people« as organized in unions or other organizations really formulate the problems (instead of devising their own »people« and determining what should be its problems), and the author's own objection [1981] that existing social science is not fit for that model—whether in a popular movement or in a firm or an organization, all the sociologist can honestly do is to offer participation as a critical intellectual.

mostly engineers in the received, specific sense, and they were taught as engineers were still taught at the Technical High School a few decades ago (the actual changes since then are modest, but they are authentic): The curriculum consists of a number of »basic disciplines«—mathematics, physics, chemistry, more or less adapted to the particular needs of the profession[176]—and a number of »application disciplines«.

During the twentieth century, many other professions have become »engineering-like« (e.g., nursing), and others have taken shape as new, still »engineering-like« professions (e.g., social work). Derek Price, alongside with his doubling of the number of *scientists* every fifteen years since the later seventeenth century, suggests [1963: 6f] that the number of people who *apply scientific knowledge in their daily practice* (»engineers« in the present pages) has doubled every ten years since 1900. Until the 1960s, they were trained in much the same way as the classical engineers. Similarly, the new applied sciences that arose (e.g., communication studies, industrial sociology) followed the pattern of »engineering science«: combining a fixed set of theoretical disciplines, adjusting and correcting their perspectives until condensed water and similar problems have been minimized. In neither context was any need felt to give the system a distinctive name. Instead we may speak of this particular kind of unacknowledged interdisciplinarity as an »engineering model«, which is characterized by finite and well-defined multi-dimensionality.

The name »interdisciplinarity« only surfaced (soon to become high fashion) in the 1960s, as the finitist presupposition failed. It did so, on one hand, in the training of the new »engineering« professions (and, for that matter, even in the training of engineers *stricto sensu*). It was no longer possible to teach young people the disciplines and the particular knowledge from these disciplines they would need in the ensuing forty years of professional activity.

[176] Adapted at least through the selection of pertinent topics. When it comes to the adaptation of perspectives, the teachers of the basic disciplines often have difficulties. From my own experience at an engineering school I remember two colleagues (*B*, a nuclear physicist, and *H*, trained as an engineer) producing a common course in electromagnetic theory for future building engineers. After a couple of years *H* observed that »*B* eliminates one Maxwell equation per year, but the result remains the same«.

Two of the strategies which were invented to circumvent the problem are irrelevant to the epistemological issue: the production of human »dispensable items«, trained for a very specific activity and dismissed when that activity was superseded (customary in the lower ranks of computer operation), and »lifelong education«, primarily of members of the traditional professions by means of supplementary courses. A third strategy, championed by the policy makers of the OECD, was the creation of more flexible educational institutions encompassing both traditional university subjects and training of members of the practical professions, and based on »interdisciplinarity« and involving the students in »project work«[177]. Such projects should simulate or exemplify the confrontation with practical problems whose elucidation requires the integration of an array of disciplines that cannot be specified in advance and once for all.

But the finitist assumption also broke down in the applied sciences themselves. Classical engineering science had been concerned with a particular and rather well-defined part of reality, and the formation of the early applied sciences was based on the assumption that practical reality *could* be cut up in pretty well-defined slices. A field like »educational studies«, however, is not at all well-defined; as insights grow (and as unforeseen condensed water turns up), new approaches are included in the field, and old ones perhaps discarded as unsuccessful, at a pace which had not been known in the earlier phases of the scientific-technological revolution.

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[177] A number of such teaching institutions connecting active basic research and the education of practitioners were indeed created in the late sixties and the early seventies. In Great Britain, in the wake of a reform plan "Education for the Future", a large number of teacher-training colleges were upgraded as »polytechnics« (they have now been reclassified as »universities«, but the substance remains the same). In West Germany, a number of such institutions were erected anew as *Gesamthochschulen*, while others of the same character were given the name of universities (e.g., Bremen and Bielefeld). (Outside the OECD, but with similar aims, the GDR had its *Dritte Hochschulreform*). In Denmark, the new universities in Roskilde (1972) and Aalborg (1974) represented the new idea.

Not all institutions were equally organic in their interdisciplinarity. In many cases, the slogan covered realities not wholly different from those of the traditional engineering schools.

Worse perhaps than the indefinite number of disciplines that may be involved is their character. The classical engineering sciences drew on disciplines whose mutual relation was relatively clear—the Kuhnian incompatibility between the paradigms of (say) mechanical physics and metallurgical chemistry (both involved in our bridge) only becomes serious at their most advanced levels; on lower levels, it is normally not too difficult to establish who is responsible for condensed water. Even as complex a project as »Manhattan«, the project which created the first atomic bombs and which involved in total some 250,000 collaborators, followed that model: mathematicians, physicists and chemists made the research and the fundamental design, military people took care of secrecy, and large industrial corporations built and ran the factories.

The mature phase of the scientific-technological revolution, however, asked for scientific answers in realms where disciplines with much less well-defined perspectives were involved[178]. The many OECD-sponsored policies, *if* they were to build on scientific knowledge, would all involve economics, legal studies, organizational theory and sociology, together with the sciences involved in their particular objective[179]. »Global« problems

[178] It is therefore somewhat paradoxical that participants in a research project on the *Lebensbedingungen der modernen Welt* directed by Habermas formulated the thesis in 1973 that science was entering a »post-paradigmatic« phase of »finalization«, where fundamental sciences could be oriented toward any practical problem where they were needed ([Böhme, van den Daele & Krohn 1973]; cf. [Schäfer (ed.) 1983] and [Pfetsch 1979]). The examples which were set forth in the argument were precisely basic sciences like classical mechanics whose perspective was well understood (because active research had stopped), and applied sciences like agricultural science which drew on such basic sciences.

[179] The communication difficulties arising in these situations can be illustrated by a dialogue which took place in 1960 at one of the OECD-sponsored conferences which prepared the »new math« reform in mathematics education:

Hans Freudenthal [mathematician, but primarily a main authority on mathematics education]: *We could teach anything, drive the children in any direction. But there exist other things at school. We must see the whole together.*

Jean Dieudonné [mathematician, and leading figure in the formalist »Bourbaki« transformation of mid-twentieth-century mathematics]: *Non, nous parlons de mathématiques. Le reste je m'en fous.* [No, we speak about mathematics. Fuck the rest].

[Grubb & Kristensen 1960: 12f]

of population growth, resource conservation, climate and ecological balance also became urgent during the 1960s, and since neither traditional techniques nor common sense and *laissez-faire* had much to promise, scientific insight seemed to be necessary if anything was to be done about them. Even here, however, the mutual relation between the perspectives of the relevant disciplines was not clear—how much had to be presupposed, e.g., about social, sexual and nutritional habits, and about ploughing techniques in their interaction with the quality of the soil, etc., if Esther Boserup's optimistic research results about the tolerability of population growth in Java were to be transferred to other contexts? In these applied social and human sciences[180] and in the scientific approach to the global problems, *interdisciplinarity* thus turned out to be a problem which had to have a name if it was to be discussed; very soon, the name of the problem got the status of a slogan which was in itself believed to procure the solution.

Interdisciplinarity in basic research

Very soon, too, the concept spread to »non-applied science«, and it may be here rather than in the genuine applied sciences that it really gained its spurs as a slogan. One current leading in this direction was that kind of »science for the people« which, in polemical formulation, tended to devise its own »people« and determining what should be its problems (cf. note 175). In less polemic words: That widespread current in the radical scientific environment of the late sixties and the seventies that tried to develop knowledge of direct relevance for actual societal and political

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No wonder that the OECD was forced to discover the problem of interdisciplinarity.

[180] Speaking of educational studies etc. as »applied social and human science« is in itself an illustration of the problem. Many of the policies erred because of a belief that everything could be planned from organizational theory, economics and sociology. But any strategy to regulate our behaviour (and the policies *are* such strategies) must take into account *both* our social existence and our existence as cultural beings, producing and reacting on meaningful communication in a historical context. On the level of applied science, the distinction between human and social science is an absurdity.

issues: peace research, women's studies, black studies, critical studies of education[181], critical science policy studies, etc.

The *Frascati Manual* has a term for this kind of fundamental research [*Measurement of Scientific and Technical Activities*, 19f]:

> [...] in some instances basic research may be primarily oriented or directed toward some broad fields of general interest. Such research is sometimes called "oriented basic research".

Not all research belonging to this class is and was of course politically radical. Fields like »Soviet studies« and »China studies«, whose aim was to know *anything of relevance* for understanding the Soviet Union or China, were often sophisticated espionage in disguise[182], and some of the scholars and institutions making peace and conflict research were more interested in how the US might establish their own peace in Vietnam than in avoiding wars, or they were paid by institutions with such interests. Research laboratories financed by particular industries (Philips, Carlsberg, etc.) tend to ask for research connected to possible products (»research dealing with beer is in favour«, as a colleague summed up what he had learned when examining the purportedly »basic« research of the Carlsberg Laboratory[183]).

There are strong institutional and financial reasons that China studies and Beer studies retain their »orientation«; in this respect they show themselves to be more closely related to the applied sciences than to basic research. Fields defined by the personal engagement of the workers, on their part, have turned out to exhibit much of the dynamics of scientific disciplines in the pre-paradigmatic phase; if the vocabulary had been at hand, it would indeed have been possible for the early economists of the

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[181] Not only my knowledge of the debates surrounding the new math reform but also my familiarity with (and my particular interpretation of) Piagetian genetic epistemology goes back to engagement in critical studies of mathematics education.

[182] The Danish Institute of Eastern European Studies, which was no cover for such intelligence work, had great difficulties in overcoming the suspicion of East European authorities [Andreas Jørgensen, personal communication].

[183] »Det må gerne handle om øl« [Uffe Sæbye, private communication]. That was in 1977. Since then, the official aim of the laboratory has been redefined as applied research.

eighteenth century to speak about their science as »interdisciplinary studies of the problem of wealth«, involving social statistics, political and social philosophy, and history. In some cases, e.g. in women's studies, it has also been possible to observe something like a Kuhnian circle—not only in a sense that certain books (at one moment Germaine Greer's *Female Eunuch*) have acquired a paradigmatic status, only to be replaced by another exemplar after a while, but also through assimilation and accommodation. Thus, at a certain moment approaches which had been used in women's studies turned out to be relevant in various kinds of minority studies; thereby features of the female situation turned out to be specific instances of something more general, and many workers in women's studies began speaking about women as »a minority«. In a commonsensical statistical interpretation, this is evidently absurd. Epistemologically, it is not: from that perspective it is simply a proof that deeper work has shown the fundamental structure of the social minority situation not to be statistical. As it happened when Newton took the common sense term »force« and gave it a specific interpretation (at odds with daily usage) and when Freud did the same to the »sexual drive«, the concept of a »minority« was reinterpreted so as to reveal the deeper structures of reality—those which only come to the fore through the dynamics of a theoretical discipline.

In the end, the interdisciplinary interests of the 1970s have resulted in that kind of processes which were spoken of above (p. 152) as »integration or convergence«. Instead of being a universal solution, »interdisciplinarity« in the theoretical sciences (whether spoken of as such or not) has turned out to be one moment in the global dynamics of scientific knowledge, a complement of the Kuhnian cycle and no alternative—the mediation which takes care that the deeper knowledge which is built up in the development of theoretical science is never totally cut off from general practice. Dialectic, a fundamental feature of the individual acquisition of knowledge and in the development of a discipline, is also to be found at this level.

VIII. ART AND COGNITION

Further investigations of the sociology of the scientific-technological revolution might be fruitfully contrasted with the deliberations of Chapter VI concerning the role of norms in the regulation of the scientific process. It is clear, for instance, that »big science«—the activity of large, hierarchically organized research laboratories and organizations—leaves little space for individual, value-based decision on the part of most participants. Decisions of importance are taken by the managers of the projects, and their primary loyalty is not necessarily directed toward the scientific value system. It is also clear that scientists who depend critically for the funding of their research (and ultimately for their living) on research councils or similar bodies may tend to let their research interests be determined not from the »prescriptions« of the paradigm as to what is important but from what they suppose the granting authority will appreciate. Quite often this authority can be safely supposed to favour some kind of »social« utility—relevance for export industry, not too critical understanding of social and cultural change, etc.; scientists may therefore be driven toward presenting a more »finalized« picture of their scientific insights than warranted (cf. note 178), and work accordingly, i.e., on levels where theoretical development has stopped[184].

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[184] As *Danmarks Grundforskningsfond* (Danish Fund for Basic Research) distributed its first 800,000,000 DKr to 23 research projects, critics pointed out that most projects were applied rather than basic; the chairman found no better defence than a claim that all »contained important elements of basic research«, which seems to imply that all were at most *oriented basic research* (*Weekendavisen*, 21.5.1993 p. 5). The selection was also strongly biased toward »mainstream research«. This might in itself seem a justifiable choice if we believe Kuhn's arguments in favour of normal science; but since oriented basic research tends either to be similar to pre-

Instead of pursuing such issues, however, we shall let these suggestions suffice and return to philosophical questions, examining what the epistemology developed so far has to tell in relation with aesthetic theory—in other words, we shall address the relation between *art* and *cognition*, which is one of the central questions of aesthetics, though only one among several.

Knowing about art

Let us imagine that we open the radio and hear the beginning of *Die schöne Magelone*. When encountering this or any other piece of art, we *know that it is there*. In the present case we perceive the sounds, we notice that they constitute music, we distinguish a piano and a voice; we may discover that the words are German, follow the words, perhaps we even recognize the work or at least the composer.

If instead the music is an ethnographic recording from Burundi in Central Africa, we may have greater difficulty in bringing the work from perception to classification. We may be unable to identify the instruments, and we may feel puzzled by its complex rhythm. But we still recognize that *it is there*.

In this sense, the problem of art and cognition is relatively trivial. We may also take Saussure's *Cours de linguistique générale* in our hands and notice that it is a book, that it is written in French, etc. This is *not* what we mean by the cognitive dimension of a piece of scientific theory. What epistemology investigates is the relation between *the theory set forth in the book* and *the purported object of this theory* (language, in the actual case).

However, that a piece of art, if we are to understand it as such, has to »be there«, is not quite as trivial as it would seem. This may be seen if we ponder the relation between two traditional conceptual pairs.

There is widespread agreement that the concepts of »beauty« and »art« are closely linked. Some define one from the other, others do not go so far but claim that »beauty« in some sense (at least through conspicuous, intentional and provocative absence, which we may characterize as a

paradigmatic science (where »normal science« does not exist) or to be finalized, the cocktail is dubious.

»negative aesthetic«) distinguishes the work of art[185]. The explanation of »beauty« with reference to (sensual and/or intellectual) »pleasure« is also conventional. It is clear, however, that the pleasure of senses which have not been integrated in the »unified space« (gustation and olfaction, and the senses of pain, heat and cold—cf. note 17) is never referred to as »beautiful« in what we feel to be the genuine sense. Sensations that cannot be apprehended as *sensation of something* cannot be »beautiful« however pleasant they are. But art, if connected to the category of beauty, must then be something which can be apprehended by the senses that give access to unified space.

However, since the beauty of a poem, whether apprehended through reading or through the ear, does not depend on its actual location, »being there« cannot itself be at stake. But then the necessary condition for something to be art must instead be that we grasp it by *that kind of intellect* which makes use of the senses of unified space, i.e., that kind of intellect which sets itself apart from what it perceives, from the things that »are there« physically or conceptually[186].

Knowing in art

Though not fully trivial, this remains a modest conclusion, and we may still ask whether the work of art stands in a similar relation to something else as Saussure's book (*theory*) stands to language (*reality*). May we claim that a work of art encompasses or transmits knowledge about something?

At times the answer is an obvious *yes*. Many works have a clearly descriptive dimension (for which reason all aesthetic theory from Aristotle until the eighteenth century spoke of *mimesis* or »imitation« as an essential

[185] Useful general surveys are [Dieckmann 1973] and [Beardsley 1973].

[186] It could be added that a work of art, *qua* work, i.e., something which has been *produced* by somebody (cf. [Heidegger 1977/1936]), must necessarily *be there*. But it may non the less be perceived without being grasped by the senses which locate in a *there*—as illustrated by the work of the perfumer.

aspect of art). For instance Malinowski's poem "Kritik af kulden" (»Critique of frost« [1980: 5])[187]:

Tidligt i marts vender vinteren tilbage
Og havens nybeskårne æbletræer
Svæver som pelsklædte spøgelser
I streng frost og fuldmåne. Herinde
Blomstrer en gren.

In early March winter returns
And the newly pruned apple trees
Float like furred ghosts
In austere frost and full moon. Inside
A twig stands in bloom.

As it stands, this seems to be a naked description of a situation. But this situation is *of no interest in itself*. This *contents* cannot be the reason that the poem is printed and sold (nor that Malinowski would spend his all too short life on writing poems).

In certain cases, analysis along these lines would even lead us to characterize the work as *a lie*; no wonder that the era which considered *mimesis* a central characteristic of art repeated time and again that Homer was the greatest of liars[188]. An illustration of this point is provided by Cecil Bødker's poem "Generationer" (»Generations« [1964: 103])

Faderen stod på den bare jord
og gjorde et hus
med egne hænder.

The father stood on naked ground
making a house
with his own hands.

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[187] My translation, as usually (with apologies to Malinowski, and to all the poets whom I disfigure in the following).

[188] Thus Aristotle, in *De poetica* 1460^{a}19f (trans. Bywater 1924): »Homer, more than any other has taught the rest of us the art of framing lies in the right way. I mean the use of paralogism«.

That the mimesis of the poet does not have to depict an actual (or historical) state of affairs is what (etymologically) makes him *a poet*, somebody who *produces* something; we might translate him literally into »a maker«. As the term came to designate the maker of verse, it acquired connotations not too far from our idea of »creativity«. However great the changes in the character, function and understanding of art during the last 2500 years, it is hence also possible to point to continuities that make it meaningful to speak as generally about the artistic sphere as done in these pages.

Sønnen steg op på faderens skuldre
og satte nye etager på
med andres hænder.

Sønnesønnen lå på ryggen
på tagterrassen
og tog solbad.

The son climbed his father's shoulders
adding new stores
with the hands of others.

The grandson lay on his back
on the roof terrace
sunbathing.

Evidently, the situation described has never existed. The poem, none the less, seems utterly meaningful—but this must be in an unfamiliar sense (»metaphorical«, we would normally say, and believe that a term solves the problem; it doesn't really before we have made clear(er) its relations to other terms and concepts).

At times, the contents of the work is not a description but a message, a (moral or similar) *opinion* about the state of the world, directly expressed or implicit. Let us consider Gelsted's "Døden" (»Death« [1957: 78]):

Aldrig mere skal jeg se et træ
—hvile i det svale bøgelæ.

Forårsskyer går i himlens blå,
aldrig mere skal jeg se derpå.

Timen kommer, som jeg ikke ved,
hvor i glemsel alting synker ned.

Dækket til af jord som nu af nat
ligger jeg af lys og liv forladt.

Glemt er hver en drøm og hver en sang,
intet er jeg som jeg var engang.

Intet er den jord jeg dækkes af,
ingen hvile er i ingen grav.

Vilde, røde hjerte, alt du led,
intet er det i al evighed.

Nevermore shall I see a tree
—repose in the beeches' cool shelter.

Spring clouds drift in the blue of heaven,
nevermore shall I watch them.

The hour arrives that I do not know,
where everything slides into oblivion.

Covered by soil as now by night
I lie, deserted by light and life.

Forgotten every dream and every song,
nothing I am which once I was.

Nothing the soil that covers me,
no rest is found in no grave.

Fierce red heart, all you suffered
is nothing for ever and ever.

The poem may be read as a commentary to a famous Epicurean maxim (ed., trans. [Solovine 1965:139]:

> Death is nothing to us: since that which is dissolved is deprived of the ability to feel, and that which is deprived of the ability to feel is nothing for us.

Since Gelsted was well versed in Greek literature, and familiar with ancient philosophy, the poem probably *is* a commentary, and an objection. A work of art may thus also speak at the same level as a philosophical argument. This allows us to formulate with more precision the question which also follows from the observations that Malinowski's »facts« are uninteresting and Bødker's wrong: why is Gelsted's objection expressed in a poem, and not in another philosophical statement that »what I cannot accept about death is that my sufferings shall be forgotten«? Why will Malinowski tell in verse instead of prose that defeat is never complete and definitive? Why should Bødker express her critique of liberalist ideology as poetry? This question will be pivotal for the argument below.

At times the obvious answer to the question whether a work of art transmits knowledge about something is *no*. A generally accepted instance is Bach's *Musical Offering*, where neither description of things (not even in a metaphorical sense, whatever that is) nor moral or similar opinion is to be found[189]. Vivaldi's concert *The Spring* from *The four seasons*, however, is no less adequate in spite of its title and programme. Only because we *know* that it should correspond to typical situations belonging to a particular season are we able to recognize them—they do not

[189] Except, of course, in the sense that a work of art which follows a certain stylistic canon »convincingly«, can be taken as an indirect argument in favour of this canon, and hence also—inasmuch as the canon reflects norms and attitudes belonging in the domain of general culture—to express a view with general implications. In this sense, Bach's *Offering* to Friedrich II of Prussia can be seen as a justification of courtly formalism.

But as when we spoke of the »metaphorical description«: this statement opens up a problem, and does not settle it. Namely: how does a stylistic canon reflect norms and attitudes? So far we may only conclude that the »obvious answer« is not necessarily the final word.

correspond to them in accordance with any code of more general validity. Words, of course, are also different from what they describe (cf. the screwdriver and the screw of p. 45); but in this case correspondence follows from a more general code.

What makes Vivaldi differ from Bach is thus not presence versus absence of descriptive contents. It is rather that Vivaldi's style allows us to organize what we perceive with less mental effort, and that we are less disquieted by it. The *Musical Offering* forces us to concentrate. Anton Webern's reinstrumentation of the "Ricercare"-movement asks even more from us. Vivaldi's concert does not *force* us to concentrate but has sufficient content and density (again: whatever these metaphors may cover) to make attention and repetition rewarding.

Fresh eyes

The issue of concentration and attention brings us to one of the central twentieth century views on the role of art in relation to the problem of cognition: The role of art is to bring us beyond cognitive routine, to make the familiar unfamiliar in order to let us see it with fresh eyes. A radical yet quite plain example taken from the world of music is John Cage's *Credo in US*, which starts by flattering the listener's conventional musical understanding, quoting the beginning of Dvořák's Ninth Symphony *From the New World*, and then suddenly jumps to something which in this conventional understanding is totally cacophonous (and continues to jump back and forth between the two). But the point of view exists in many variants.

Very important is Brecht's theory of *Verfremdung* (»estrangement«, »making unfamiliar«, *not* »alienation«, which translates *Entfremdung*). When Brecht undertakes to shatter the illusions and the identification produced by naturalist stage play, his intention is that we shall not be allowed to indulge in trivially sentimental pity with the poor Mutter Courage as she looses all her children in the 30 Years' War[190]. He wants us to recognize her role as co-responsible for what happens to herself as well as to her

[190] *Mutter Courage und ihre Kinder* [*Gesammelte Werke*, vol. 4].

children—we should judge and hence learn and not only empathize. Similarly, when Peter Weiss lets the contradictory forces of the French Revolution be embodied by lunatics[191], he prevents us from identifying with one of the parties and forces us to accept the dilemma—and having de Sade embody moral disengagement he prevents us from dodging the dilemma by taking *this* facile position.

Another exponent for the position that the role of art is to liquidate easy routine is *modernism*. While Brecht and Weiss want to make us see *a state of affairs* of general importance with fresh eyes, much main-stream modernism (not all! and not all modernism is mainstream![192]), when it has to state a theoretical position (and in particular when it is explained by its academic advocates), asserts that the aim of art is to force upon us the fresh eyesight *abstractly*, without any engagement in morally important real life issues which might lure us into believing that this real life is what the work of art is about (we might say that they see Bach's *Musical Offering* as a model for art in general).

A theoretical formulation of the view was provided by the »Russian formalists« (c. 1915-1930), who influenced both Brecht and Eisenstein's montage technique. René Wellek [1973: 173b] sums up their views as follows:

> They had grown up in a revolutionary atmosphere which radically rejected the past, even in the arts. Their allies were the futurist poets. In contemporary Marxist art criticism art had lost all autonomy and was reduced to a passive reflection of social and economic change. The Formalists rejected this reduction

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[191] Peter Weiss [1964], *Die Verfolgung und Ermordung Jean Paul Marats, dargestellt durch die Schauspielgruppe des Hospizes zu Charenton unter Anleitung des Herrn de Sade.*

[192] One strong exception, borrowed from Paul Celan's *Schneepart*, which I shall abstain from translating:

> Ich höre, die Axt hat geblüht,
> ich höre, der Ort ist nicht nennbar,
>
> ich höre, das Brot, das ihn ansieht,
> heilt dem Erhängten,
> das Brot, das ihm die Frau buk,
>
> ich höre, sie nennen das Leben
> die einzige Zuflucht.

[Celan 1975: II,342].

> of literature. But they could accept the Hegelian view of evolution: its basic principle of an immanent dialectical alteration of old into new and back again. They interpreted this for literature largely as a wearing-out or »automatization« of poetic conventions and then the »actualization« of such conventions by a new school using radically new and opposite procedures. Novelty became the only criterion of value. [...].
>
> Jan Mukařovský (born in 1891), a follower of the Russian formalists in Czechoslovakia who developed their theories more coherently [...] formulated the theory very clearly: »A work of art will appear as positive value when it regroups the structure of the preceding period, it will appear as a negative value if it takes over the structure without changing it«. [...] In literary history there is only one criterion of interest: the degree of novelty.

Two things may be added. Firstly, that the view of the formalists emphasizes *the intellect* and the role of sober-minded reflection. Secondly, that it gives an explanation why artistic styles are worn out and have to be replaced when they themselves have become routine: If it is the style itself and not an irreconcilably contradictory tension between form and contents in the particular work (Bach!) that is supposed to crush standard expectations, a work cannot fulfil this role once its style has itself developed into a standard expectation.

Another example from the world of music may illustrate the point (and, it the same time, show its shortcomings). In the 1950s and early 1960s, Karlheinz Stockhausen and Pierre Boulez were recognized on a par as leading members of the Darmstadt School of serial music. As long as this extrapolation and automatization of Schönberg's dodecaphonic principles was unfamiliar, one (I!) would indeed find that the compositions of the two were equally forceful as sources for »fresh hearing«. As time went on and the »conventions« became familiar, Boulez's music stopped triggering intense attention, and it came to be somewhat boring; Stockhausen conserved his interest though now, one might say, as *music* and not as an enhancement of acoustical awareness[193].

[193] These observations and impressions are mine and of course subjective; but they seem not to be quite private, since Boulez concentrated on a conductor's career while Stockhausen has remained a productive composer. The young Danish serial composer Thomas Koppel, to whom the serial technique was so much of an automatic and calculated technique that he made his compositions while listening to rock music, quit serial music altogether and became a rock musician.

The formalist theory may also be applied to kinds of art (and quasi-art) which do *not* fulfil its requirements: trivial art, but perhaps in particular sub-genres like pornography, horror and violence (in the following I shall speak in a generalized sense of these and their kin as »pornographic« genre's). They draw on *what is already there* in the receiver's mind, preferably on strong dreams, drives and prejudices, in ways which prevent rational deliberation and even the mere establishment of qualitatively new cognitive relations. The customary in customary amalgamation, in particularly that which is already so strongly felt that it is easily drawn upon and thus relatively resistent to reason and to integration in higher synthesis, is enhanced and stabilized[194].

Form versus contents

In order to better understand the implications of the formalist view we may return to Chapter II—more precisely to Piaget's concept of schemes and to the dialectical relation between assimilation and accommodation.

The point of this dialectic was that actual knowledge about something (the *contents* of our cognition) is always organized in a specific *form*, the scheme and that cognitive structure in which the scheme participates. *Formalism* carries its name because it sees the *form* of cognition as the aim of art[195], not the creation of supplementary contents (who, in fact, is

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[194] Drawing preferentially on *what is already there* as strong dreams, drives and phobias, often to the exclusion of rational deliberation, also characterizes expressionist currents. But expressionist art (at its best) aims at creating new cognitive relations, and therefore draws less on preestablished prejudice and on the customary and the customary blend than the pornographic genres.

But the difference may be subtle. The leap is short from certain kinds of expressionism to very efficient propaganda (another »pornographic« genre)—for instance from the use of the rats in Murnau's film *Nosferatu* to certain scenes in the Nazi movie version of Feuchtwanger's *Jud Süss*.

[195] Of course, one need not have read Piaget to use these terms, which do not even correspond to Piaget's own way to express himself.

One may also notice that the term »form« is used in aesthetics in various senses, not all of which are relevant for a discussion of »formalism«. A useful discussion is [Tatarkiewicz 1973], which however tends to distinguish mechanically (between »Form [concept] A«, »Form B«, ..., »Form G«) rather than seeing the connections between the different meanings, and which also includes notions which *might* have

interested in knowing about the temperature in Malinowski's garden :
forgotten day in March?).

Yet even if the Piagetian framework makes the central princip
formalism more clear, it also highlights its failings and problematic feat

A *form*, firstly, can no more be separated from contents than con
from form. You cannot, as Jean-Luc Godard parodically makes Ferdi
propose in the movie *Pierrot le Fou*, write a novel on »la vie simplem
Novels telling about *life* must necessarily deal with »la vie des homn
i.e., must necessarily be that novel which Ferdinand refused to w
Piagetian schemes, in the same way, are only built up as generaliza
of actions or interiorized actions, i.e., as the form of something. If pc
is not allowed to deal with »morally important real life issues« it will l
to treat of *unimportant* issues. Or, as stated by Kandinsky, innovat
artistic form if anybody ever was, in a strong attack on marketable *a*
art's sake: »Form without content is not a hand but an empty glove f
with air. An artist loves form passionately just as he loves his tools o
smell of turpentine, because they are powerful means in the servic
content«[196].

Specifically, formalism possesses no instrument allowing us
understand the function of Gelsted's and Bødker's poems. These poe
indeed, do not force upon us a *new way* in which to see the use of w
and phrases—their artistic form is quite conventional. Instead they cor
a new understanding of *the world* (the nature of death, the reality beh
the myth of »free initiative«). Formalism is unable to explain why th
insights are better formulated as (traditionalist) poems than in thesis fc

More generally, formalism can be claimed to identify innovatio
artistic and in *cognitive* form. Even though this may be warranted in m
cases, it is no universal truth. In fact, becoming familiar with the rap
changing artistic forms of the twentieth century is largely an assimila
process: »Oh, music may also sound like this« / »Indeed, this is also
impressive painting«. As any assimilation, this involves accommodat
but mainly of our concepts of musical or visual beauty. Our interes

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been called »form« but which are actually labelled differently.

[196] *Über das Geistige in der Kunst*, trans. [Tatarkiewicz 1973: 221].

music, however, is not explainable through the observation that music changes our understanding of music.

How are we then to understand the interest of art which does not live up to the formalist requirements? And how are we to understand the *actual* effect of trivial literature, pornography, etc.? We need an understanding of art which do not merely discard them but on the contrary understands the (empirically) different effects of »real art« and »trivial art«.

Gelsted and Epicurus

The Epicurean maxim expresses a chain of purely analytical discourse. A familiar phenomenon is investigated stepwise: What does it mean to fear death? What does it mean to fear any*thing?* Since this peculiar »thing« is something which I cannot perceive, I cannot fear it.

It is easy to loose the thread of a complex analytical argument, which deprives it of its power to convince. Epicurus' argument has therefore been summed up in an aphorism, »Why fear death? When I am here, death is absent. And when death is here, I am absent«. This pulls together the main lines of the argument in a way which allows you to apprehend them in one glance. One reason that the aphorism can be grasped in this way is the doubly symmetric structure of the argument (presence/absence, I/death), which on one hand binds the two clauses constituting each of the last two periods together, and on the other joins these two periods; another reason that the aphorism is grasped and remembered is the artistic-humorous form. None the less, even the aphorism does not convince if one *is* scared of death.

Gelsted's poem is a protest against Epicurus' acceptance of death. Stated analytically, the argument might run something like this: What I cannot concede to death is that it reduces me to pure absence. I can only bear my existence if I am able to ascribe to it a meaning, if beauty is real, if suffering is real, etc. The fact that I shall die, that everything which in this moment is beautiful or bitter matters nothing under the perspective of Eternity, is unacceptable to me; what I fear is this fact of ultimate meaninglessness of *the present* and not an abstract future »thing« called death.

Yet instead of presenting this analytical string, which will easily be as existentially non-committal as Epicurus' argument (although, I discover, I actually put it into words and phrases which are more heavily loaded than a purely analytical argument *should* be), the poem *reminds* the readers of what *existence* is, and thus *suggests* how *absence* can be grasped. It draws upon the connotations of words and upon what readers can be supposed to know about the experience of spring under newly green beeches, upon their knowledge that »spring clouds drifting in the blue of heaven« imply sunshine, fragrance and breeze but no storm; as a climax it draws upon the readers' own experience of suffering, and on the extra pain added to suffering if it is recognized to be meaningless.

All this is not put into any logical or analytical framework. The coherence of the poem is rhetorical and rhythmic, using the contrasts »nevermore/the beeches' cool shelter« and »spring clouds/nevermore« (a double contrast which keeps the two first stanzas together, enhancing their weight in the argument); then comes a middle part, strung together by »oblivion«/»forgotten« (*glemsel/glemt*) and leading to a final climax produced (at the rhetorical level) by a triple *nothing* (evidently, this structural analysis could be expanded). Just as the text draws upon the readers' total understanding of what it is *to live* in order to make it clear what it is *to die*, it is left to the readers to take bearing of the rhythmic and rhetorical structure in order to build up an ordered totality, an implicit synthetical argument. And it is, indeed, an ordered totality which is built up.

The words of the text do not serve in a sharply defined sense, as they would (ideally) do in a technical manual, where the role of the reader is reduced to understanding (or, perhaps, *not* understanding) the terms correctly. The words carry their whole, open-ended load of connotations, and should do so. By means of its rhythmic and rhetorical structure, however, the text puts these bundles of connotations into mutual relations which readers would not automatically produce on their own. Assimilating these relations to their own understanding, readers will accomplish *an accommodation*, while the technical manual only gives information about familiar entities, i.e., assimilation relatively free of accommodation. The manual, if well written, can be *used*. A work of art cannot: »using« it involves one in *co*-producing.

This is a general characteristic of the artistic product, and explains why a work of art is not exhausted by one reading (or whatever kind of reception is involved)[197]. On one hand, assimilation normally presupposes repeated experience. On the other, one and the same work will be seen differently from different readers' perspectives, and by the same reader in different moments. This is a simple consequence of the open-ended and non-overlapping ranges of connotations (not only of words but also of rhythmic and other structures) produced in different readers and in different situations. Although one interpretation of a work can often be argued to be »better« than another, i.e., to make better sense of more features of the work[198], it is not possible to translate a work into a single definitive interpretation—even the artist will not be able to do so, since even the artist's range of connotations is open-ended.

On this account, the difference between *trivial art* and what we might call *complex art* (which is not the same as *complicated* art—complication may follow from confusion, but complexity not) can be seen in a new perspective. Trivial art does not put things and concepts into unexpected relations,

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[197] Since even the forms of artistic expression carry a load of implied meanings and connotations derived from their use it also explains why it is impossible to resurrect the styles and genres of former times. As expressed by Kandinsky [1911]:

> We cannot re-create the sensibility and the internal life of the ancient Greek; therefore, even if we try for instance to apply the Greek principles in sculpture, we shall create only shapes which are similar to the Greek ones, but the work will forever stay without spirit. Such imitation is similar to that of a monkey. Externally, the movement of the monkey are fully similar to those of a human.The monkey sits down, takes a book to its nose, turns over the leaves, seems to be fully absorbed; yet the internal meaning of these movements is completely absent.

[198] This *may* mean that the better interpretation is also in better agreement with the intentions of the artist. But this need not be the case. Erich Maria Remarque intended to write a patriotic novel, and believed to have done so. Only when the manuscript of *Im Westen nichts Neues*, after having been rejected by some ten patriotic publishing houses, was accepted by a left-wing publisher and became an immense success, did he discover that *his* patriotism was not that of the patriots but somehow pacifist [Nils Rickelt, personal communication].

and therefore it does not produce accommodation. Yet what is utterly familiar for one person may be an unexpected and inciting discovery seen from another's perspective. Accommodation of the *totally* unfamiliar, furthermore, is not likely to occur (cf. the leaden ball of p. 9). Whether a particular work is »trivial art« or not is hence not merely to be determined from the work itself; it also depends on reception and on the capacities and preceding experience of the receiving mind.

On the same account, we may understand the frequent weakness of didactic art. »Leftist detective stories«, »progressive fairy tales«, and »morally edifying versions« will all too often be reduced to one level of meaning. In order to make sure that the reader gets the »right« associations, the challenge of open-ended connotations is reduced to a minimum. Yet guiding the mind of the reader so that it performs no »wrong« movements, and thus barring co-production, prevents it from performing that autonomous activity which is a presupposition for accommodation. Preventing the occurrence of »wrong« new thoughts is tantamount to preventing the occurrence of new understanding[199].

[199] Evidently, this is not the only reason that art is claimed to be banal or of bad quality because it is morally or politically engaged. Often it simply means that the critic does not share and does not appreciate the values which are expressed.

A classical example of politically engaged art which is *not* banal art is provided by Eisenstein's *Potemkin*. There is no doubt about the political message. This, however, does not make the movie banal. The political message, indeed, is not presented in homiletic one-dimensionality, but through a highly complex use of the pictorial medium and the temporal organization.

As an (unusually transparent) example of a critic disguising political disagreement as art criticism one may cite a commentary to Shostakovitch's *Leningrad Symphony* written by Clive Bennett for a record edition (Decca D213D 2). If, as the composer's notes tell, the symphony is inspired by the war during which it was written, and if it is meant as a requiem for the victims of the Nazi atrocities, it is obviously nothing but »a film score without a film«. But »the symphony, if we believe [the] interpretation [that the inspiration is rather Stalin's purges], becomes transformed from a propaganda work into a canvas of universal significance«.

Art as thought experiments

For the next step in our argument we may return to the difference of opinion between Kuhn and Lakatos. For the latter, the hard core of a research programme was precisely definable. For Kuhn, the paradigm was constituted in part by tacit knowledge, skills, and context-defined concepts. It is not possible to *define* what, e.g., a *text structure* is, nor to prescribe exactly a universal method for finding it. What you acquire when learning textual analysis is unsharply defined knowledge and skill in analyzing. Your experience within the field will be associatively connected, like Gelsted's »tree«, »spring clouds« and »suffering«.

As it was argued, this difference is what makes Kuhn's approach a more adequate description of the actual scientific process: Scientific practice is to a considerable extent based upon intuitive knowledge, knowledge organized in totality and in analogies.

This is the reason for the importance for the *thought experiment*. A prototype is the argument against the rotation of the Earth which was raised by the Paris philosopher Buridan in the fourteenth century: as we all know, the stars of heaven seem to circle around the Earth once every 24 hours. Wouldn't it be more economical if the Creator had made the Earth rotate and had left the immense sphere of stars at rest? Perhaps, but hardly the case, Buridan explains. Reflect upon what happens if you shoot an arrow vertically upwards. It will fall down upon you own head. But if the Earth rotated, you would have moved a considerable distance (some 1500 feet per second) while the arrow was in the air, and it would fall to the ground far from where you are. This is obviously not the case.

According to later physics, Buridan's intuition is wrong. He is unaware of the law of inertia, according to which an arrow shot from a horizontally moving bow will receive a horizontal component of movement. This, however, is only important in so far as it shows that intuitive knowledge need not be correct. More central is the observation that scientific argumentation presupposes a certain measure of global knowledge (correct or incorrect) about the behaviour of its object.

Works of art can also be regarded as thought experiments. In some cases this is obvious. If we consider a novel like George Orwell's *1984*, what makes the book influence us is the fact that it is psychologically plausible: The world which is depicted *could be* a world, with all the complexities of a real world, for all we know about social life and human beings. Evidently, »all we know« is a historical product (as was Buridan's knowledge); in the moment when our intuitive knowledge makes the world of the novel seem implausible, the novel itself will loose its actuality[200].

Other works of art are only thought experiments in a transferred sense. A piece of pure music is neither plausible nor implausible with regard to our experienced daily world. Its »plausibility« depends on its inner coherence. But even a piece of pure music is a testing of possibilities and consequences within a space of plausible solutions (a form or style); the »transferred sense« thus *is* a sense.

The reason that Buridan needed his thought experiment (and that Niels Bohr and Einstein needed theirs!) is that analytical thought does not exhaust everything we know. Thought experiments allow us to gauge what we can formulate analytically against what we know tacitly (i.e., to check those pieces of knowledge which we can isolate against the totality of the world as we know it). Similarly, the genuine thought experiments of art allow us to gauge specifiable moral (etc.) convictions, to see whether they will work acceptably in a specific situation created (fictionally) for that purpose. In this way, Shakespeare demonstrates in *Romeo and Juliet* that the morality of the family feud and honour are unacceptable. The thought experiment of the tragedy leads to better knowledge about how we should live, to superior practical knowledge. That the questions explored by art are (as all questions about practical knowledge) normally much less accessible to explicit analysis than those investigated by the sciences explains why

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[200] When explaining in *De poetica* (1451^{a}36f: trans. [Bywater 1924]) the difference between the mimesis of the artist and that of the descriptive historian, Aristotle states that »the poet's function is to describe, not the thing that has happened, but a kind of thing that might happen, i.e. what is possible as being probable or necessary«. And later (1454^{a}35ff), »whenever such-and-such a personage says or does such-and-such a thing, it shall be the necessary or probable outcome of his character; and whenever this incident follows on that, it shall be either the necessary or the probable outcome of it«. Again (1460^{a}26), »a likely impossibility is always preferable to an unconvincing possibility«—*viz* because a work of art does not function if its world appears to us as an implausible postulate.

thought experiments play a much more central role in art than in scientific discourse.

Realism

In mechanical interpretation, what was just said about *Romeo and Juliet* might be taken as an argument in favour of *realism*. Before we go on this term has to be explained. »Realism« as I use it here is not the same as »naturalism«—it may be its opposite. The pair realism/naturalism is rather a transposition into aesthetics of the epistemological pair realism/nominalism. Nor is »realism« meant here as the antithesis of »embellishment« (which it sometimes is)—actually, the two concepts are close neighbours[201].

Positively stated, *realism* (or *aesthetic realism* as I shall call it in the following in order to avoid misunderstandings, and since we are dealing with art) is the view that a work of art should lead to understanding of the *essence* of things. It may do so by depicting phenomena naturalistically, but whether it does so or not, the important thing is that it should lead to insight in something more fundamental than these phenomena; in this sense, aesthetic »realism« is akin to philosophical realism—whether »strong«, objectively idealist as Plato's variant, or »moderate« as Aristotle's.

An example of definitely non-naturalist realism is *Futurism*. Futurism did not use the realist label; but the label »Futurism« is itself a claim that art should tell the essence of the new world: speed, aggressiveness and fight, breakdown of classical harmony. *Surrealism* (sometimes non-naturalist as with Max Ernst, sometimes deceitfully naturalist as with Dali), often

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[201] The reason that these conceptual fences have to be constructed is of course that the word is used in so many different senses. Taken as a synonym of naturalism, however, the term is superfluous; and taken as a token for the view that art should show the world as ugly or as cruel as it is instead of postulating a harmony which isn't to be found in reality, the term becomes epistemologically uninteresting (although it may be highly relevant in the political discussion of the responsibility of art and artists in a specific historical context).

What I try to do here is to specify a sense which underlies *some* of the views on art which proclaim themselves »realist« (not least »Socialist Realism«), and a number of others which use different banners.

inspired by psychoanalysis, aimed at showing the higher reality of the mind as uncensured by reason.

Within a philosophical context which itself is clearly different from classical realism and its concepts of »truth« and »reality«[202], a suggestion of aesthetic realism can still be found in Heidegger's "Ursprung des Kunstwerkes" [1977/1936]. »Instating itself in work [of art], truth [which is 'the truth of being'] appears« (p. 69). Van Gogh's painting of a pair of peasant's shoes uncovers not only the shoes as things but the whole of the peasant's lived experience: the hard toil of monotonous ploughing, the hostile wind, the loneliness of the work in the fields, the anxiety for the daily bread, ... (p. 19).

What was said above regarding *Romeo and Juliet* is a similarly »realist« interpretation. The tragedy does not tell merely about a particular sequence of events arousing fear and pity; it also tells what are *the real* consequences of the prevailing code of honour and the practice of family feuds—no less than van Gogh's shoes as *lived experience*. Even Aristotle moves on a comparable level (though with reference to a different overall philosophy) when he asserts (*De poetica* 1451^{b}5-7; trans. Bywater 1924) that »poetry is something more philosophic and of graver import than history, since its statements are of the nature rather of universals, whereas those of history are singulars«. In spite of what it is tempting to read into the term *mimesis*, Aristotle's ideal for art is thus no naturalistic imitation of phenomena (cf. also the quotations in note 200).

Both Heidegger's and Aristotle's aesthetic realism is cautious and unpretentious in the sense that none of them is coupled to a statement of what the underlying truth revealed or displayed by the work of art should be. What one might call extreme aesthetic realisms do not share this restraint—certainly not that Socialist Realism which was proclaimed as

[202] Heidegger [1977/1936: 14f] points out that the dichotomy matter/form is derived from tools created with a certain purpose; and concludes that seeing everything *that is* as the result of an imposition of *form* on *matter* presupposes that it is the outcome of an act of creation similar to the one by which men create their tools—and thus in the final instance, and however much theologians and philosophers try to deny the parallel—*faith*;

a programme at the Congress of Soviet Writers in 1936, and which may be the most outstanding example of a declared aesthetic realism from this century[203]. It was coupled to a version of Marxism which already knew not only the past and the present essential conditions of class struggle but also the certain outcome[204]; art was therefore to reflect the movement of (this) history past and future and lay bare how particular situations were explainable in terms of the general laws of history.

The way this programme was coupled for a while to political power falls outside the scope of a discussion of the relation between art and cognition. What falls inside is the observation that the programme, if followed to the letter, makes the expression in artistic form superfluous[205]. If the essence of things is already known so precisely that it can be translated into prescriptions, then it can no less easily be explained as theory, and art which follows the prescriptions *becomes* theory (and ultimately ineffective as art, cf. the above observations on didactic art); what can, and what needs to be expressed as art is open-ended knowledge.

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[203] The programme of Socialist Realism is certainly not the only example of political control of art from the twentieth century. Other instances of this phenomenon, however, have not been supported by a similarly elaborate aesthetic philosophy.

[204] This (Stalinist) version of Marxism was thus in itself (like all the brands of Marxism which subscribe to strict economic determinism or to a closed Hegelian dialectic where »history has been but is no more«) a brand of extreme philosophical realism or objective idealism—as caustically pointed out in Sartre's *Questions de méthode*—cf. [Høyrup 1993: 200].

[205] This »letter«, it should be said to do justice to the better theoreticians of the movement (e.g., Georg Lukács), was not theirs explicitly—they had too much pragmatic sense and artistic feeling to reject everything which was valuable art, and could avoid doing so by moving imperceptibly between »realism« as here understood and »naturalism«. What I do in these pages is to draw some consequences which the fathers of Socialist Realism had too much insight to draw, but which are none the less inherent in their ideas, as demonstrated in not a few of the works that came out of the programme. Lukács himself [1969/1948: 78] notices their »monotony [...]. One has barely begun the reading of most of these novels before he knows everything that is going to happen: in a factory, vermin is at work; everything is chaotic, but finally the Party cell or the GPU discovers the nest of wrongdoers, and production flourishes [...]«. As far as predictability is concerned no worse than much trivial art—but certainly not what a philosopher of art whose ideals were Tolstoy and Balzac would like to sponsor.

Aesthetic realism (not least *this* realism) is often, and justly, seen as antithetical to formalism. The reason is not that realist art does not care about form. On the contrary: that Madame Bovary kills herself by swallowing arsenic does not in itself tell anything about the obtuseness of provincial bourgeois society; if Flaubert's novel manages to relate this »essential truth« about bourgeois life it is through the way it selects, orders and tells its material—i.e., by means of *the form* of the novel. Aesthetic realisms (and not just this realism) are passionately absorbed in putting art into *the correct* form, that which corresponds to their assumed underlying order of reality. What distinguishes realism from formalism is not the degree of absorption in the question of form; it is that formalism rejects the idea that any particular form should be correct and hence definitive, arguing instead that *every* artistic form is wrong when it has become customary or trivialized.

Synthetical understanding and practical knowledge

The experience of artistic thought experiments and the analysis of Gelsted's poem suggested that the formalist understanding was insufficient, however much its »realist« counterpart exaggerates its own point. It remains, however, that a large class of artistic works refer to nothing outside themselves and the stylistic canon to which they relate, neither as thought experiments creating a world in agreement with our tacit knowledge, nor with connotations derived from our »lived experience« of cool shadow and suffering. If we are not satisfied by the formalist explanation that they allow us to see with fresh eyes (and as we have seen, there are reasons not to stop at that point), what are we then to do about works that (like Bach's *Musical Offering*) are only thought experiments in a transferred sense?

Clearly they cannot gauge the validity of moral convictions etc., since such convictions have neither presence nor representative in the work. What they do to us when we put their »plausibility« on trial—in terms of inner coherence and of the tension between form and material, stylistic canon and actual use—is rather to sharpen our ability to comprehend totalities, to perceive intuitively[206]. For instance: If we have become able to grasp (consciously or, just as well, subconsciously) the structure of Beethoven's piano sonatas, we shall have no difficulty in following the implicit prescriptions given by the rhythmic and rhetorical structure of Gelsted's poem; and we shall also have enhanced our chances to grasp a structure in complex real situations.

—Certainly not *the* structure: real-life situations are, no less than works of art with their indefinite range of connotations and resonances, infinitely complex, and we shall find no bottom if we dive into them; realist aesthetic theory fails on both accounts. But precisely therefore training in grasping *as much and as essentially as possible* and concluding *from that* is crucial. In Brecht's words[207]:

»Was hilft zweifeln können dem der sich nicht entschließen kann! Falsch mag handeln wer sich mit zu wenigen Gründen begnügt aber untätig bleibt in der Gefahr wer zu viele braucht	What help is doubting for the one who is unable to conclude! Incorrectly may act the one who is satisfied with too few argu- ments but unfit in danger remains the one who needs too many

Because no description of reality (no scientific description and, *a fortiori*, no other) can be transcendentally and exhaustively true (i.e., identical with what it describes), translating one's apprehension of a situation into a formal system from which a conclusion can be drawn always involves a moment of *synthetical judgment* which integrates our analytical and tacit knowledge but cannot itself be argued exhaustively in analytical argument. Under which moral rule a certain act is to be counted, for instance, is itself not to be derived from rules alone (cf. above, p. 123). Alf Ross, the Danish legal philosopher, gave the strong formulation to this observation that the

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[206] The following line of reasoning owes much to [Feinberg 1977].

[207] "Lob des Zweifels", [*Gesammelte Werke*, vol. 9, 626-628]. The poem as a whole is a recommendable treatise on epistemology in practice.

leap from legal premisses to action is *irrational*[208]. Knowing how to this leap (and all the analogous leaps) is *practical knowledge.*

We may conclude that: *if skill in logical inference and analytical t is of any use in human life, then only if coupled to practical knowledg corresponding skill in synthetical or intuitive judgment*. We may then co an argument begun on p. 30. There, the fundamental cognitive cate were suggested—like the conserved object—to allow more adequate and to have developed biologically for that reason. This, we now see could only do if they developed *together* with a faculty for syntheti intuitive judgment.

Art, we have also seen, trains that faculty, and allows its develop into a genuine skill[209]. Apart from its function in concrete int reasoning and as a source for better practical knowledge, as discusse *alia* in connection with Gelsted's poem and the role of art as th experiment, art (and here, abstract music no less than figurative pai or text with a meaning) may also have a fundamental cognitive fur as *training of that integrative or synthesizing competence without which th*

[208] Cf. [Jarvad 1993: 94]. Elsewhere (p. 43), Jarvad sums up Ross's view in the that »theories and analyses do not lead to decisions, do not designate c decisions as correct and others as wrong, at most they pinpoint errors in the for the decision. Science does not lead to decision and action«.

[209] We may also recall that scientific practice is largely governed by paradig knowledge, as social life by norm systems. Both contain, and presuppose for application, the wielding of intuitive and integrative knowledge. Art, by tra the faculty for integrative thought, is thereby also a training of the very ba scientific practice and social life. By being less bound than real-life moral refle and scientific work to a specific content, it may provide a more thorough comprehensive training than the two activities provide occasion for thems

Paraphrasing what Gouldner says about the value-free doctrine (cf. p. 143, the free activity of art may enable us »to make *better* value judgements rather none«—»better« in the sense of less parochial, better since based on a br understanding of our total situation and the implications of our actions. This be a first step in a justification (and, at the same time, a critique) of the idea understanding of art leads to moral improvement, and that aesthetic educ is the best moral education.

analytical abilities are empty—as training of the very *faculty* for practical knowledge[210].

This sounds like a conclusion to the question concerning art and cognition, and in a way it is. Yet it is important to remember that these functions of art as a way to practical knowledge and as training of practical cognition *per se* do not answer (or at least do not exhaust) the question why we engage in art, as producers or as co-producers. The phenomenology of *beauty*—as told by two illustrious witnesses—may serve once more.

In his *Confessions* (X.xxx-xxxv—ed. trans. [Trabucco 1960: 123-147]), St. Augustine aligns indulgence in the pleasures of the senses with erotic concupiscence (and with interest in natural philosophy characterized as »vain curiosity«). Pleasant smells are no problem for him (they produce no beauty, we might say), but all the more is music. Though he has improved, he still cannot ignore the musical beauty of a psalm melody sung by a beautiful voice or avoid feeling it as a caress—and no better is the vision of the sweetness of the world. Aristotle, when locating the essential beauty of the tragedy, points to its arousal of »pity and fear« (*De poetica* $1453^{b}12$). Less immediately than the »pornographic« genres but no less truly, *all* impressions of beauty—and hence all engagement in artistic production or co-production—appear to be rooted in our affections. *How* it relates to them is a major question if we wish to understand what art is, and one might postulate that the experience of »beauty« results from some kind of unity of affection and integrative insight; but the problem does not belong within the present line of argumentation, and we shall leave the argument here.

With respect to its root in affections, of course, engagement in art only differs by *degree* and *degree of immediacy* and perhaps in the *kind of affections involved* from other kinds of human conscious activity. No such activity is undertaken without a motive, and motives are by definition rooted in the affective. The sphere of art may therefore be less absolutely separable from other spheres of life than presupposed for convenience in the

[210] This fits an observation made on p. 166, *viz* that art must be grasped »by *that kind of intellect* which makes use of the senses of unified space«. This is the kind of intellect which needs to be trained if analysis and synthesis shall work together.

preceding pages. Still, if large enough, differences in degree remain decisive, and art, if no absolutely separable sphere, remains a sphere of its own no less than science and morality.

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