Paul Hoyningen-Huene

EPISTEMOLOGICAL REDUCTIONISM IN BIOLOGY: INTUITIONS, EXPLICATIONS, AND OBJECTIONS

The term 'reductionism' is, no doubt, equivocal. Let me therefore first state what I shall address in this paper and what I shall not. Using the well-known distinction between ontological, methodological, and epistemological questions of reductionism (e.g. Ayala 1974; Hull 1981; Mayr 1982, pp.60-63), I will dismiss the methodological domain. Many of the questions of reductionism which are of interest to the working biologist qua working biologist, will therefore not be treated here. Furthermore, I will not address questions of the ontological domain: I shall stick dogmatically to ontological reductionism without arguing for it. For the sake of argument, I shall presuppose that living beings are composed of the same kind of matter that is equipped with the same kinds of elementary interactions that are known to physics and chemistry of today. Thus the questions of epistemological reductionism remain.

In this paper, I want to discuss some of the arguments that have been put forward, both for and against epistemological reductionsm. I shall do this in a slightly different manner from the usual one in which each party articulates its theses and presents arguments for it. A complex discussion exists in the literature which has led to points of disagreement that, at first sight, seem almost unrelated to the original topic and almost 'ideological' in character, despite the fact that the original topic seems perfectly clear-cut. My methodologically basic conjecture in this paper is that most of the dynamics of this discussion are understandable by turning not only to the thesis articulated by the respective party, but by additional reference to the underlying intuition for which the thesis is an explication. The objections to a thesis do not necessarily undermine the respective underlying intuition; they may rather stimulate a more sophisticated reformulation of that thesis. To the opposing party this may appear to be a stubborn, dogmatic, and consequently frustrating way of answering the objections. It is this interplay of intuitions, explications, and objections to which I now turn.

P. Hoyningen-Huene and F. M. Wuketits (eds.), Reductionism and Systems Theory in the Life Sciences, 29–44.

I start with the position of the epistemological reductionist. His basic contention, as I understand it, is the following: Given the presupposition that living beings are made up of the same sorts of atoms and molecules as the physicists and chemists know them, and nothing else, then having a comprehensive knowledge of the particular atoms and molecules in a given organism, and their configuration, together with a knowledge of the laws of physics and chemistry, would in principle be sufficient to redefine all the properties of this organism in atomic and molecular terms, and to derive its behavior -- specifically, all the laws which it obeys. Generalizing this slightly, a knowledge of all the (relevant) properties of the elements of one level of the organizational hierarchy, together with a knowledge of how these elements are arranged at a particular higher level, would in principle be sufficient to redefine all the properties of, and derive the laws governing, the entities of this higher level. I will call this the **basic epistemological reductionistic intuition**, or more briefly, as we are in any case concerned only with epistemological questions of reductionsm, the 'basic reductionistic intuition'. On the same rather intuitive level of expressing things, and asked why this would be really a plausible intuition, the epistemological reductionist would answer that given the knowledge of the components and their configuration, everything to be known about the system is known. Since no other factors which could be causally relevant to the behavior of the system can possibly exist, all knowledge about the system must, in principle, be extractable from the complete knowledge of the lower level. (For a decomposition of this basic reductionistic intuition into three parts see Oppenheim/Putnam 1958, pp.23-27.)

The basic reductionistic intuition leads to the straightforward and well-known logical empiricist explication of epistemological reductionism (see e.g. Nagel 1961): the higher level is epistemologically reduced to the lower level if and only if: 1. the higher-level descriptive terms can be, extensionally equivalently, expressed by lower-level descriptive terms (the 'reduction functions', 'bridge principles', or 'correspondence rules'); 2. the higher-level laws can be logically deduced from the lower-level laws, including in the premises the reduction functions, and appropriate initial and boundary conditions describing the particular higher-level system (or class of systems) in lower-level terms.

Two remarks should be made here. Firstly, it is essential to include in the premises of this derivation initial and boundary conditions specifying the particular higher level system one is interested in. Omitting initial and boundary conditions would result in a completely uninteresting concept of reduction because it would be empty: no higher level, as a special configuration of lower level entities, could ever be epistemologically reduced to the lower level simply because the information of this special configuration would be missing, i.e. the system to be epistemologically reduced would not be specified. Although this point has been stressed many times (e.g. Nagel 1961, p.434; Putnam 1975, pp.138–139; Hull 1981, p.128; Friedmann 1982, p.38; Hoyningen-Huene 1985; but see also Hempel 1966 where initial and boundary conditions are not mentioned), an epistemologically anti-reductionistic position is sometimes argued for on the grounds that initial and boundary conditions are omitted from the premises of the deduction (see e.g. Ayala 1968, p.209; Ayala 2983, pp.284–285, 287; Dupré 1983).

Secondly, there has been some discussion about the status of the sentences that redefine higher-level terms, extensionally equivalently, in lower-level terms, the reduction functions. The problem consists of their status with respect to the analytic/synthetic distinction, with respect to the law/non-law distinction (e.g. Fodor 1974), and with respect to their alleged status as identities (e.g. Causey 1977). A minimalist position which seems appropriate here (but not in other contexts) simply leaves this question open: reduction functions have to be included in the premises in order to accomplish the deduction, whatever their status.

The basic reductionistic intuition together with its logical empiricist explication seemed for a while such a convincing line of thought that it was even used to explicate what ontological reductionism meant (e.g. Hempel 1966). In order to avoid ontological talk, one should express ontological reductionistic convictions in terms of epistemological reductionistic assumptions. This is highly plausible insofar as the way in which ontological convictions are gained in science, seems to consist of a comparison of the predictive and explanatory powers of theories with different ontological implications. The identification of ontological with epistemological reduction implied that it was impossible to articulate coherently a position that was both ontologically reductionistic (i.e. anti-vitalistic) and epistemologically anti-reductionistic.

But the logical empiricist explication of the basic reductionistic intuition soon came under attack, and with it the contention that ontological reductionism necessarily implies epistemological reductionism. There are a number of important objections to this account of epistemological reductionism, and I shall discuss some of them, especially with respect to their bearing on the basic reductionistic intuition.

1. With the advent mainly of Kuhn's and Feyerabend's work in the sixties it became clear that the logical empiricist explication was in trouble (Kuhn 1962; Feyerabend 1962). Firstly, even the simplest examples of accomplished epistemological reductions in physics demonstrate that the relation between reducing and reduced theory is not simply a logical deduction but involves

approximations (Schaffner 1967; also e.g. Wimsatt 1980, p.145). The reduced theory may, strictly speaking, logically contradict the reducing theory. Secondly, the same terms in the reduced and the reducing theory may have different meanings in the two theories, i.e. in the most obvious case, their extensions do not contain exactly the same elements.

It therefore became necessary to correct the explication of epistemological reduction: that is, the original, higher-level theory cannot be deduced from the lower-level theory together with the other premises, but only a `corrected` or `modified` version of it which is `strongly analogous` to the original theory (Schaffner 1967 and 1976).

How does this modification of the reduction model affect the underlying basic reductionistic intuition? It does not affect it at all; it shows only that reshuffling the information about the lower level, in order to yield knowledge of the higher level, is not as simple as the logical empiricists thought. The epistemological reductionist will not be intimidated by the prospect of rather complicated reduction functions in the epistemological reduction of classical genetics to molecular genetics, but he or she will not believe that reshuffling is impossible. Neither will he or she be intimidated by the relationship between molecular and classical genetics seeming more like a replacement than a reduction (whatever this difference is precisely), as long as it is believed that the ontological reduction holds (see especially Schaffner's 1976, pp.434-442 and Ruse's 1976, pp.447-454 reply to Hull).

2. A second objection concerns preconditions of scientific theories that have to be fulfilled in order to be able to decide whether the reduction relation between two theories holds or not.The theories in question must be sufficiently articulated to enable the (approximate) deductive relations to be proved or disproved. For this aim, theories have to be reconstructed by the philosopher, but this is not at all a unique procedure. Furthermore, since theories are historical entities they change continuously, and even at any particular point in time, many different versions of a theory may be held by different scientists. As a result, it is not even clear between which versions of theories a claim of epistemological reducibility holds, so that these claims are premature (e.g. Hull 1976 and 1981).

Again, all this may be conceded by the reductionist. Of course, earlier versions of a theory may not contain enough information about the lower level to accomplish the derivation: reduction functions may not be known, nor the initial and boundary conditions under which certain molecules perform certain function etc.. Also, it may be difficult to bring the theories to a degree of logical articulation that shows their connections explicitly. But all this does not damage the basic reductionistic intuition, it only involves more technical complications: if only atoms and molecules are involved, then the knowledge about them and their configuration is all that can be known about the system, however incomplete or insufficiently articulated for the logical connections, this knowledge and the knowledge of the upper level may be.

3. Further objections concern the status of the claim of epistemological reduction as an 'in principle' claim. Nowadays no one would contend that an epistemological reduction of any field of biology has been worked out in such complete detail that it could simply be pointed at, and the discussion brought to a close. Rather, it is a claim that the epistemological reduction is 'in principle' possible, given the ontological reduction.

Two main objections can be made. Firstly, the sense of 'in principle' is not clear (see e.g. Wimsatt 1979 and 1980; Dupré 1983). Would a procedure that involved some 10^{120} discrete steps be counted as 'in principle possible'? This is, of course, a good question. But again, it will not disturb the epistemological reductionist because there is no reason to believe, at least in the case of fairly simple biological systems, that such a large number of steps play a role in defining the reduction functions. (Things may be very different at the level of the human mind, but the problems of epistemological reduction of psychology should be distinguished from the problems of the epistemological reduction of biology). As long as such trans-astronomical numbers do not play a role, the fuzzy sense of 'in principle' will not be disturbing.

Secondly, it has been stated that the epistemological status of 'in principle' statements is problematic. They are believed to be "empirically meaningless" (Ayala 1967, p.210), or to be usually normative, as opposed to merely descriptive (Wimsatt 1979, p.358). But this does not really catch their epistemological character. I would suggest that the status of claims for the 'in principle' possibility of some epistemological reduction should be regarded as similar in status to a Gedankenexperiment: though not directly empirically testable, it is not considered normative or even senseless. Rather its function is, roughly speaking, conceptual clarification by spelling out the ultimate consequences inherent in a certain approach (compare Kuhn 1977).

4. A further possible objection can be formulated as follows. Suppose that the epistemological reduction of biology (or some particular part thereof) to physics and chemistry was feasible. This would amount to the fact that all biological explanations would ultimately be physico-chemical. But this is highly implausible since biology is distinguished from the other natural sciences by its particular modes of explanation -- in particular, teleological explanation (e.g. Ayala 1968).

Therefore, the premise of this argument must be wrong, i.e. biology cannot be epistemologically reduced to physics and chemistry.

In order to handle this objection, we do not need to decide whether teleological explanations (or any other type of alleged specifically biological explanations) are truly the decisive factor for the autonomy of biology with respect to physics and chemistry. The above argument is wrong for a different reason, namely that it equates an epistemological reduction with an explanation (i.e. the validity of the Hempel-Oppenheim scheme is presupposed without qualifying appropriate conditions of adequacy). But certainly not every deduction from laws and initial and boundary conditions of a lower level, together with reduction functions, of a proposition about something at a higher level is an explanation of this something (see e.g. Putnam 1975; also Hoyningen-Huene 1985 for an analysis of the anti-reductionistic arguments in Popper 1974), and, conversely, the possibility of an explanation of specifically biological phenomena by lower level theories does not by itself imply the possibility of the epistemological reduction (Friedmann 1982). Thus, explanations of higher-level events and laws in terms of lower levels should not be identified with the epistemological reduction of the higher level to the lower level. Using the distinction of three different domains of problems of reductionism I used in the beginning of this paper, this is simply the difference between methodological and epistemological questions of reduction. Thus, even if biology was epistemologically reducible to physics and chemistry, this would not ipso facto constitute a threat to the autonomy of biology (compare Hull 1981, p.125; Friedmann 1982, p.37).

5. The next two objections are mainly put forward by biologists (e.g. Lorenz 1977, ch.3 [but see also ch.2 which does not seem to be consistent in all respects with ch.3, and Lorenz 1981, ch.1]; Mayr 1982, pp.63-64; Wilson 1975, p.7; also Popper 1974), and they rest on the concept of emergence (for a treatment of this concept in the recent philosophical literature see Pluhar 1978 and Klee 1984).

The first aspect of the idea of emergence as a counter-argument to epistemological reductionism is that the synthesis of some elements to some 'whole' may lead to a behavior of that whole that is completely unexpected even on the basis of a complete knowledge of those elements, or their functioning in other combinations. As an example, Lorenz uses an electric circuit with a battery, a condenser, a coil, and a resistance connected in series. This circuit may exhibit damped oscillations which may indeed be unexpected if one knows the behavior of these elements only in connections of less than four elements.

But from this no argument against epistemological reduction follows. Though it is true that the oscillatory behavior of the circuit may be 'unexpected' -- even in principle, it is not derivable from the knowledge of the components and their characteristics alone -- it is precisely derivable from their characteristics and their connection, i.e. the relevant boundary conditions. Thus, if this example is thought of as representative for an aspect of emergence, it is far from being a counter-argument to epistemological reductionism; on the contrary, it is an excellent confirmation for the basic reductionistic intuition (see also Hoyningen-Huene 1985).

6. But there is a subtler aspect of emergence that seems incompatible with epistemological reductionism which is also present in the above-mentioned writings of biologists. This second aspect appears at first sight to be almost identical with the first, but it can be developed in a different direction. The core of this aspect of emergence is that the elements of some 'whole' may show a different behavior when integrated into that whole, from the behavior they display when examined separately. Thus the properties of these elements depend on the whole they are integrated into, and the knowledge of the whole is not derivable from knowledge of the elements studies in isolation. Conversely, if knowledge has to be gained about the elements, it must be derived from the knowledge of the whole, making epistemological reduction completely wrong-headed and impossible. This aspect of emergence stands somehow upon the same footing as the basic reductionistic intuition, and may therefore be called the basic anti-reductionistic intuition.

To this, the epistemological reductionist may, in order to save his intuition, respond with a distinction, namely the distinction between gaining knowledge about properties of things, and the properties of these things themselves. It may well be that the knowledge of the relevant properties of the elements of the whole, in order to accomplish the epistemological reduction of that whole, may be obtainable only by examining combinations of these elements, or even the very combination for which the epistemological reduction is intended. For example, the knowledge of a resistance relevant to its behavior in a circuit can only be attained by connecting it - really or ideally - with a battery. Similarly, the features of animals relevant to their social behavior can clearly be gained only by putting them into the appropriate social environment (or simulating it). Even so, the knowledge thus gained is knowledge about properties of the elements (appropriately called 'dispositions'), even if it is obtainable only by investigating the whole, and by virtue of which an epistemological reduction may, however, be carried out legitimately.

Clearly, this argument appears to be self-immunizing to the anti-reductionist, and it may be parried by another distinction that has been put forward by epistemological reductionists themselves (Oppenheim/Putnam 1958, p.10; also Ayala 1983, p.286). This distinction is absolutely essential in order to distinguish true epistemological reductions from pseudo-reductions. The distinction is between predicates that truly belong to one level, and predicates that are ad hoc constructs in order to accomplish the epistemological reduction of the next highest level. The example of Oppenheim and Putnam for a predicate of the second sort is the predicate `tran` which should apply to atoms of transparent substances, and by means of which the transparency of macroscopic bodies can be reduced to the atomic level. (Obviously the example is faulty since the transparency of objects does not depend on the sort of constitutive atoms alone, but the idea of the distinction is clear). Now the dispositions introduced by the reductionist seem to be exactly of the second sort: they are only introduced to accomplish a (possibly even useless) epistemological reduction. Beyond that, their explanatory power is exactly the same as the "dormative potency" property of opium, as put forward by Molière's doctor.

But this move does not leave the reductionist defenceless; he or she may claim that the criterion by which the anti-reductionist classifies, for example, the social dispositions of animals as predicates of the second sort, as purely ad hoc in order to accomplish a reduction, is fundamentally mistaken. This can be shown by reductio ad absurdum, namely by using the same criterion for a class of predicates that are surely of the first sort, but which the criterion used by the anti-reductionist classifies as of the second sort. Take the case of any interaction of particles which is a fundamental situation for physics. Any sort of interaction between particles is explained in physics by the charges these particles bear, e.g. electromagnetic interaction by electrical charges of particles, gravitational interaction by (gravitational) mass of particles, etc. Charges as a fundamental property of particles are first introduced only to account for these interactions, that is a particle having a certain sort of charge, and being completely isolated from particles having the same sort of charge, may show no sign whatsoever of its charge without rendering the concept of its charge illegitimate. This is analogous to what biologists favoring emergence and hence epistemological anti-reductionism keep stressing: that their objects, e.g. some social animals, do not show certain properties when studied in isolation. But the analogy demonstrates that the introduction of certain predicates merely for the sake of the accomplishing a reduction is, by itself, insufficient reason to render these predicates purely ad hoc constructs, and thus illegitimate.

But why does the introduction of charges in physics seem legitimate even though they are at first introduced only in order to break down an apparently emergent property of an assembly of particles, and although these mysterious charges of a particle may never show up when the particle is separated from the respective whole? The answer is roughly this. First of all, these alleged properties of particles are very few, and, together with the laws of interaction, they account quantitatively for an infinite number of different wholes, i.e. they have a strong predictive power. Secondly, they obey certain conservation laws, i.e. they have a well-defined sort of stability. Thirdly, and largely as a consequence of the two foregoing items, they have explanatory power, i.e. it is believed that these charges are causally relevant and that means that they really exist.

Let us summarize the previous discussion about emergence and epistemological reduction. 1. The emergence of new properties of elements when integrated into some `whole` is, by itself, no argument against epistemological reductionism. 2. The attribution of properties to these elements that are potential in character when the elements are removed from the whole, for the sake of epistemological reduction, is not by itself unscientific even if it is in principle impossible to have evidence for the existence of these properties when the elements are removed from the whole. 3. The conditions for the legitimacy of the postulate of some properties that make epistemological reduction possible are, without the concrete case being specified, probably only vaguely determinable: the hypothesis of the existence of these properties should be credible, fruitful, independently testable etc., whatever that ma mean in a particular situation.

Thus the almost omnipresent fact of emergent properties as properties that show up only when the respective elements are integrated into some `whole` is, as such, completely neutral with respect to the epistemological reductionist`s and the anti-reductionist`s case. In order to argue their respective cases, the reductionist has to show that some emergent property is also potentially existent when the element is separated from the respective whole, and he or she has to do so, roughly speaking, by constructing an interesting and credible theory around the postulate of these dispositions. In the above-mentioned case, the disposition of members of a species to some social behavior, this might be the discovery of the genetic basis of this behavior, together with the discovery of the trigger mechanism which would make the lack of this behavior outside a specific environment intelligible, without making the disposition to it nonexistent.

The anti-reductionist must either show that the reductionist`s task is not realizable, or construct a theory which makes the emergence of the emergent properties intelligible without exclusive recourse to properties and laws of the lower level.

Does all this affect the basic reductionist`s intuition? Yes, it does so crucially because it makes explicit an assumption that went unnoticed as an integral part of that intuition, and that may be false. It is the assumption that indeed the behavior of an element of some `whole` can always credibly be traced back to some property this element also has in separation from this whole, if only as a disposition. Physics has been extremely successful with this, but this by itself is no argument that the success will continue forever.

7. The next objection can be formulated in a weaker and a stronger form. In the weaker form, it attacks the "tacit assumption of nomological determinism" that is asserted to be presupposed in the basic reductionistic intuition (Friedmann 1982, p.34). The thesis of nomological determinism claims that, given the set of laws governing the lower level, the set of laws governings the higher level is uniquely determined. However, in the case of the epistemological reduction of biology to physics and chemistry this thesis may not hold. Since one of the relevant theories is non-linear thermodynamics, it cannot be ruled out that the laws valid on the biological level may essentially depend on some microscopic fluctuation that took place during prebiotic or biotic evolution. This implies that the thesis of nomological determinism may be invalid in this case.

The obvious counter-move to this is to assert that the thesis of nomological determinism is not implied in the basic reductionistic intuition: the laws of the higher level are not simply determined by the laws of the lower level alone, but may depend on the relevant initial and boundary conditions as well. This counter-move may be parried in two ways.

The first possible parry is not very convincing: it contends that the above counter-move would deprive the biological laws of their character as laws, since their validity would then depend on some matter of fact (Friedmann 1982, p.24). But nobody I think would hold that biological laws as we know them, or are trying to know them, have to be completely independent of all events in the history of evolution. Take the 'fact' (or 'law') of the chirality of biologically produced sugars, or some biological law depending on that fact. Nobody seems to believe that the chirality could not be opposite for life on a different planet, but this does not transform a generalization that is valid strictly for life on earth, and that depends on the chirality of biologically produced sugars, into a non-law. The concept of a biological law is simply not so strong.

The second possible parry seems cogent at first sight since it not only contends that the thesis of nomological determinism may be invalid, but also that even the laws of the lower level, plus the initial and boundary conditions, may be insufficient to determine the laws of the higher level (Friedmann 1982, p.35 fn.2 and p.38). This stronger thesis can be argued for on the basis of the indeterminacy of quantum mechanics. According to quantum mechanics, two identical systems, i.e. having exactly the same quantum mechanical initial state, may nevertheless develop into systems obeying different macroscopic, e.g.

biological. laws. A particular event, say the decay of a particle, may, for instance, take place at different times in the two systems leading to different microscopic fluctuations, which, in turn, may finally amplify to different macroscopic laws. (A variation of this argument not involving quantum mechanics may be obtained using identical thermodynamic initial conditions).

This presents a serious difficulty to the epistemological reductionist who may try to overcome it in the following way, but it is arguable whether this adjustment is justifiable, or purely ad hoc in a negative sense. In the above-mentioned case, the initial and boundary conditions as they must be specified according to the laws of the lower level, together with other necessary premises, are not strong enough to determine the (macroscopic) behavior of the system, and consequently, epistemological reduction is impossible. But this does not really run counter to the basic intuition itself. Microscopically speaking, if one cannot, in principle, predict what will happen, one cannot expect that, from all the available microscopic information (and the reduction functions), macroscopic behavior will be derivable in every case. This being the case, the missing microscopic information may be treated in the same way as other necessary factual information, namely the initial and boundary conditions in the technical sense. On the one hand, it may be argued that an unpredictable momentous microscopic fluctuation plays in its consequences the same role as the initial conditions in the technical sense, and must therefore be added as supplementary information to the premises of the intended derivation. On the other hand, it may be argued that this move only proves the anti-reductionist's case, namely that from the physics and chemistry laws, together with all the information on initial and boundary conditions these sciences specify as such, and all necessary reduction functions, the biological laws will probably in principle not be derivable.

Which of these possible points of view one accepts as legitimate depends on the position one is defending. To the reductionist, the first argument seems fair, and the second unfair, although he will acknowledge the necessary correction of his or her position (unless some other way out is found). This is because indeterministic systems were not really covered by the basic reductionistic intuition, and some modification is therefore in order. To the anti-reductionist, it may seem quite the contrary, and the reductionist only adds one more self-immunizing move by which he or she is dogmatically trying to protect the position from devastating criticism. This situation is, in its circularity, very similar to situations which, according to Kuhn, occur in debates about paradigm choice: "Each group uses its own paradigm to argue in that's paradigm defense." (Kuhn 1970, p.94).

8. The last objection is different from the preceding ones in quality. It states that the whole approach to reductionism involving the concept of epistemological

reduction is at best uninformative, and at worst grossly misleading, about what is really going on in the life sciences when ties to physics and chemistry are made. As a consequence, philosophers of science should ask different questions, and should not remain stuck on a question that is in almost no contact with scientific life (see references in Ruse 1976, pp.454–460; Maull 1977; Wimsatt 1979 and 1980; Mayr 1982, p.63). This objection may be summarized in Lakatosian terms by saying that epistemological reductionism is the hard core of a strongly degenerating meta-scientific research program.

This is a very strong objection indeed to a philosophy of science that tries to analyze fairly heterogeneous reductionistic tendencies and research strategies in the life sciences just in the light of the one basic reductionistic intuition that leads to the doctrine of epistemological reductionism. Many other motives may underlie reductionistically inclined research, and because epistemological reduction and explanation are not coextensive, one may well mistakenly make epistemological reductionism the centre-piece of philosophy of biology.

How does this claim affect the basic reductionistic intuition? It does not, even if it is true, for two reasons. Firstly, one has to distinguish between this intuition itself, and the strategic place one alleges for it in the real process of research in the life sciences. For reasons that are easily understandable from the recent history of philosophy of science, epistemological reductionism may have played an unjustifiably pre-eminent role in the analysis of science (but see Schaffner 1981, p. 95). But removing it from this place does not imply that the underlying intuition itself is mistaken.

Secondly, it may be that it is worthwhile to keep an eye on the basic reductionistic intuition, and its possible role in the life sciences. Let me explain this by means of an analogy. Let us suppose that Kuhn is right and that something like 'normal science' exists not just as a pitiable form of science. Does this falsify Popper's basic intuition that the spirit of science as a collective enterprise is, or at least should be, a critical one? I don't think it does, because all it demonstrates is that even in good science the idea of criticism is not at work as straightforwardly and directly as Popper thought. It may turn out that in order to really put into practise the idea of criticism, it may be necessary to refrain from it somehow, and that the idea of fundamental criticism cannot therefore be found in scientific everyday life, without making science basically uncritical. I am not suggesting that the situation is exactly analogous to the case of epistemological reductionism in the life sciences; I am only stressing the possibility that after a period of exaggerating the immediate importance of a certain idea, namely her the idea of epistemological reductionism, the danger of exaggerating the unimportance of it may be a real one in the period that follows.

So, what is the net result of this lengthy discussion? On the one hand, the basic epistemological reductionistic intuition has not survived the battle unscathed, and the most serious wounds come from quantum mechanics (objection 7), and the uncovering of the risky ontological conjecture implicit in it that properties of elements that are manifest only when these elements are integrated into some whole, may be treated as resulting from dispositions of these elements themselves (objection 6). Whether it bleeds to death from these wounds is difficult to say, but even if it does, it is not clear how many and which people will mourn, since its possible function as a Gedankenexperiment (objection 3), both in biology itself and the philosophy of biology, is not sufficiently clarified (objections 4 and 8). On the other hand, what I have called the basic anti-reductionistic intuition has come through the battle without a scratch only because it has been disqualified from it for not being relevant here (objections 5 and 6). And thirdly, both parties get into trouble when it comes to a precise articulation of what they defend or attack, respectively (objections 1 and 2), because each party may well survive the demolition of some explication of its respective intuition without being forced to surrender. Thus, if somebody pressed me to respond to the question: Who is right, the epistemological reductionist or the anti-reductionist? I would probably answer: yes and no.

Acknowledgement. Critical comments by Gerhard Vollmer on an earlier draft of this paper are gratefully acknowledged.

Bibliography

Asquith, P.D., Kyburg, H.E. (eds.): Current Research in Philosophy of Science (Edwards, Ann Arbor, 1979).

Asquith, P.d., Nickles, T. (eds.): PSA 1982. Proceedings of the 1982 Biennial Meeting of the Philosophy of Science Association (Philosophy of Science Association, East Lansing, 1983).

Ayala, F.J.: "Biology as an Autonomous Science", American Scientist 56 207-221 (1968).

--: "Introduction", in Ayala/Dobzhansky 1974, pp.VII-XVI.

--: "Beyond Darwinism? The Challenge of Macroevolution to the Synthetic Theory of Evolution", in Asquith/Nickles 1983, vol. 2 pp.275-291.

Ayala, F.J., Dobzhansky, T. (eds.): Studies in the Philosophy of Biology. Reduction and Related Problems (Macmillan, London, 1974).

Block, N. (ed.): Readings in Philosophy of Psychology, vol.1 (Harvard UP, Cambridge, 1980).

Causey, R.L.: Unity of Science (Reidel, Dordrecht, 1977).

Cohen, R.S., Hooker, C.A., Michalos, A.C., van Evra, J. (eds.): PSA 1974 (Reidel, Dordrecht, 1976).

Dupré, J.: "The Disunity of Science", Mind 92 321-346 (1983).

Feyerabend, P.: "Explanation, Reduction, and Empiricism", Minnesota Studies in the Philosophy of Science III 28-97 (1962).

Friedmann, K.: "Is Intertheoretic Reduction Feasible?", British Journal for the Philosophy of Science 33 17-40 (1982).

Fodor, J.A.: "Special Sciences, or the Disunity of Science as a Working Hypothesis", in Block 1980, pp.120-133; originally in synthese 28 97-115 (1974).

Hempel, C.G.: Philosophy of Natural Science (Prentice Hall, Englewood Cliffs, 1966).

Hoyningen-Huene, P.: "Probleme des Reduktionismus der Biologie", Philosophia Naturalis, ~~forthcoming.~~ 22, 271–286 (1985)

Hull, D.: "Informal Aspects of Theory Reduction", in **Sober 1984**, pp.462–476; originally in Cohen et al. 1976, pp.653–670.

--: "Reduction and Genetics", **The Journal of Medicine and Philosophy** 6 125–143 (1981).

Klee, R.L.: "Micro-Determinism and Concepts of Emergence", **Philosophy of Science** 51 44–63 (1984).

Kuhn, T.S.: **The Structure of Scientic Revolutions** (Chicago UP, Chicago, 1962, 2nd. ed., enlarged, 1970).

--: "A Function for Thought Experiments", reprinted in Kuhn, T.S.: **The Essential Tension. Selected Studies in Scientific Tradition and Change** (Univ. of Chicago Press, Chicago, 1977), pp.240–265.

Lorenz, K.Z.: **Behind the Mirror. A Search for a Natural History of Human Knowledge**, transl. by R.Taylor (Methuen, London, 1977).

--: **The Foundations of Ethology**, transl. by K.Z.Lorenz and R.W.Kickert (Springer, New York, 1981).

Maull, N.: "Unifying Science without Reduction", in Sober 1984, pp.509–527; originally in **Studies in History and Philosophy of Science** 9 143–162 (1977).

Mayr, E.: **The Growth of Biological Thought. Diversity, Evolution, and Inheritance** (Belnap, Cambridge, 1982).

Nagel, E.: **The Structure of Science. Problems in the Logic of Scientific Explanation** (Harcourt, New York, 1961).

Nickles, T. (ed.): **Scientific Discovery** (Reidel, Boston, 1980).

Oppenheim, P., Putnam, H.: "Unity of Science as a Working Hypothesis, **Minnesota Studies in the Philosophy of Science** II 3–36 (1958).

Pluhar, E.B.: "Emergence and Reduction", **Studies in History and Philosophy of Science** 9 279–289 (1978).

Popper, K.R.: "Scientific Reduction and the Essential Incompleteness of All Science", in Ayala/Dobzhansky 1974, pp.259–284.

Putnam, H.: "Philosophy and Our Mental Life", in Block 1980, pp.134–143; originally in H. Putnam, **Mind, Language, and Reality. Philosophical papers** vol.2 (Cambridge UP, London, 1975) pp.291–303.

Ruse, M.: "Reduction in Genetics", in Sober 1984, pp.446–461; originally in Cohen et al. 1976, pp.653–670.

Schaffner, K.F.: "Approaches to Reduction", **Philosophy of Science 34** 137–147 (1967).

--: "Reduction in Biology: Prospects and Problems", in Sober 1984, pp.428–445; originally in Cohen et al. 1976, pp.613–632.

--: "Introduction [to] Reductionism and Holism in Medicine", **The Journal of Medicine and Philosophy 6** 93–100 (1981).

Sober, E. (ed.): **Conceptual Issues in Evolutionary Biology. An Anthology** (MIT Press, Cambridge, 1984).

Wilson, E.O.: **Sociobiology. The New Synthesis** (Belnap, Cambridge, 1975).

Wimsatt, W.C.: "Reduction and Reductionism", in Asquith/Kyburg 1979, pp.352–377.

--: "Reductionistic Research Strategies and Their Biases in the Unit of Selection Controversy", in Sober 1984, pp.142–183; originally in Nickles 1980, pp.213–259.