Sheldrake's "Hypothesis"

Contribution to the Tarrytown Prize

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I.

It may be worthwhile and necessary to express in advance my conviction that books must not be burnt and thoughts must not be prohibited or the discussion of them hindered - as crazy, absurd or wrong, as they may seem to be.

Up to today books have always been burnt when those who have the power to burn books were afraid of the ideas that were brought up by these books. It does not matter whether these ideas really are "true" or actually are dangerous. It is enough if they are supposed to be.

I became quite interested in Sheldrake's book when I heard that "Nature" had suggested to burn it. I wondered why this single book could be so dangerous for science that a well-known and representative journal is interested in and wants to hinder the spreading of knowledge of this "hypothesis of formative causation" - and that made me read "A New Science of Life".

Particularly because I am going to show that these ideas do <u>not</u> at all represent a <u>scientific</u> hypothesis, I want to emphasize nevertheless that this book as well as all other books must not be burnt. It is the right of any author to publish his ideas, and it is a moral duty for all of us to defend this right.

Thanks to the "New Scientist" and the Tarrytown Group who did not join the proclamation for an autodafé, but in contrast helped to promote more rational and scientific standards for a more appropriate discussion of Sheldrake's ideas.

II.

> "The Tarrytown Prize will be awarded for the 'best test' that tends either to confirm or refute Sheldrake's hypothesis." (press release of the Tarrytown Group that was the announcement of the prize.)

The "best test" will primarily be understood as an experiment, carried out and/or analyzed with regard to the evaluation of the hypothesis. But an experiment that shall either tend to confirm or to refute Sheldrake's hypothesis presumes that this conception actually is a hypothesis that can be tested experimentally. And therefore only the outcome of the experiments will permit serious conclusions about the confirmation or refutation of the hypothesis. But what if this conception Sheldrake put forward is not a hypothesis at all?

Actually Sheldrake affirms throughout his book that this is a scientific hypothesis and that it can be tested. That means he states that his approach fulfils the criteria of scientific standards.

Sheldrake never did give a proof for this affirmation. Indeed he presents a lot of possible experiments. However, stating a list of examples is of course not a sufficient "proof" of the scientificallity and testability of a conception.

Whether Sheldrake's "hypothesis of formative causation" fulfils scientific standards or not is indeed a significant question that has to be answered before testing it experimentally. If it turns out that this conception does not satisfy scientific claims, all eventual experiments would become meaningless and would be in vain. Because it is of course contrary to the rules and not permissible to draw any conclusions from these experiments if they are derived from the wrong presuppositions.

Before conducting experimental tests it is necessary to examine with an "analytical test" whether the hypothesis really is a hypothesis and therefore suitable to be tested scientifically. This test will be carried out in this paper. The prize is not announced for the "best experiment" but for the "best test" - and that may include analytical tests as well as experimental.

Rupert Sheldrake is a scientist, and he claims to put forward nothing but a scientific hypothesis. He demands his conception of formative causation be treated with scientific standards and as a hypothesis, and he wants it to be tested experimentally - just as would be done with any other scientific theory. But that implies that it must be possible to test whether the hypothesis itself meets the expectations of a scientific hypothesis. Sheldrake states it does.

The following shall demonstrate that his assumption is wrong. Sheldrake's "hypothesis of formative causation" does not fulfil the criteria of a scientific theory, and the experiments Sheldrake designed and proposed do not permit any conclusion about the "hypothesis" - 'whatever the results of these experiments may be.

III.

No explanation or assumption represents a "scientific hypothesis". Before it can be considered as one it has to satisfy some essential conditions. One of the most important ones is the criterion of exact formulation of the hypothesis. If it is not exactly formulated and does not imply unequivocal definitions of its terms it is impossible to test this hypothesis experimentally in a scientific way.

The hypothesis has to include unmistakable definitions of its objects and of its field of validity. If it is inaccurately formulated, if for example the objects are not clearly named or if it is only vaguely stated which eases the hypothesis may be valid and which eases not, then it is not qualified for being tested experimentally, because it is impossible to get a clear impression of which experiments would be suitable to confirm or refute the hypothesis - and which would not.

So it would be impossible to test a hypothesis that says "...sometimes can be expected ... " or " ... in most of the cases …" without giving exact directions as to which cases that would be.

Such a hypothesis is - in other words - not "falsifiable": any results of the experiments can be interpreted to be covered by the hypothesis - even those that are unexpected and were not predicted. All possible outcomes can be considered as a confirmation or at least not a refutation of the hypothesis, because the hypothesis only claims to predict most of the cases.

By pointing out the unexact formulations it is possible to refute a refutation of a "hypothesis" with any negative results. Conceptions that are not falsifiable therefore fail to qualify themselves as a scientific hypothesis.

Regarding these criterions, unequivocal formulation and the falsifiability, there is in other words no difference between a hypothesis and a theory. Since a hypothesis is tested in expectation of confirming it and when that is acknowledged and valid as a theory, it has to fulfil these criterions as a hypothesis that shall be tested first as well as a valid theory. The difference between these is one of acknowledgement but it must not be one of exactness.

Sheldrake is conscious about these criterions. He writes about vitalism:

> "If, to quote Sir Karl Popper, 'the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability', vitalism has so far failed to qualify." (p. 12) ([1])

He is willing to advance a hypothesis or theory that does not fail to qualify from a scientific point of view. And his statement that his hypothesis is "capable of being tested experimentally" (p. 118) aims obviously at its falsifiability and the fulfilment of scientific standards.

The question is whether that is right, and the method to examine that is to read his book carefully and to analyse its content: the hypothesis, Sheldrake's examples and his argumentations.

According to his "summary" (p. 115 - 118) he formulates his hypothesis for <u>all systems</u> or <u>all material morphic units</u>:

> "„… a further type of causation is responsible for the forms of all material morphic units …
>
> …. Each kind of morphic unit has its own characteristic morphogenetic field ….

> … All similar past systems act upon a subsequent similar system by morphic resonance. ... " (p. 115 - 117).
>
> (See also "… the morphogenetic fields of all past systems become present to any subsequent similar system…", p. 13).

Obviously he makes no restriction upon the validity within his summary. He does not name any systems or "material morphic units" 'r are not be affected by morphogenetic fields and morphic resonance.

Precisely that means that there is no exception and really that all systems are included. But that is a strange assumption because there is a lot of "material morphic units" whose "forms" are obviously caused without any "morphic resonance" or "morphogenetic fields". For example we can think of all the systems that are constructed and build by men. No doubt, all kinds of machines can be considered as systems and as "material morphic units", but not even Sheldrake would seriously state like "all past refrigerators act upon a subsequent refrigerator by morphic resonance", even though it is in strict accordance with his hypothesis.

And in fact, it is Sheldrake himself who stated once, that even in his opinion not all systems and their morphogenesis can be influenced by morphogenetic fields: "This is not to say that <u>all</u> form in living organisms is determined by formative causation" (p. 86, stressed by Sheldrake), and he explains:

> "Some patterns may come about through random processes. Others may be fully explicable in terms of minimum-energy configuarations: for instance, the spherical shape of free-floating egg cells (e.g. those of sea urchins) may be fully explicable in terms of the surface tension of the cell membrane. However, the very limited success of simple physical explanations of biological forms suggests that <u>most</u> aspects of biological morphogenesis are determined by morphogenetic fields." (p. 86, stressed by Sheldrake).

Now it is easy to see and to understand that Sheldrake in fact will not and cannot assert his theory for all systems. And even though he assumes that it is a great part of all systems, he does not assume that all systems are influenced by morphic resonance but only "most" of them.

But if actually he does not state that it is "all systems" or "all material morphic units" his hypothesis refers to - then which of "all systems" are supposed to be affected by morphogenetic fields, and which does he want to exclude? Where is the limit of the validity of that hypothesis? The attentive reader of Sheldrake's book cannot find any explanation or exact definition. He is capable to register that Sheldrake does not really want to refer to all systems - but he cannot know which systems he wants not to include. There is no exact definition and therefore no unequivocal way to understand this "hypothesis".

Consequently it is impossible to derive clear-cut experiments to test the hypothesis: ultimately only Sheldrake as the creator is able to distinguish which systems he wants to consider as being influenced by "formative causation". But that is not at all a scientific method of presenting a hypothesis.

So the hypothesis is not falsifiable either. And an experiment with obvious unpredicted outcome will nevertheless not provide a basis for Sheldrake to consider his hypothesis as refuted - as bad as the outcome may be from his point of view. Since by pointing out the inaccurate formulation it is always possible to hint that this special case is not evidence of one of the "most" systems the hypothesis is talking about. And of course - due to the vague formulation it will be impossible to know (and exclude) these cases in advance.

A quite impressive example of how such an "unfalsifiable hypothesis" can be treated and used to discuss and interprete even negative results as not-refuting for the "hypothesis" is given by Sheldrake himself on pages 132 - 133. There he designs an experiment and derives a prediction from the hypothesis. He concludes:

> "If this result were actually obtained, it would provide positive evidence for the hypothesis of formative causation, und would be inexplicable in terms of the mechanistic theory. A negative result would be inconclusive for two reasons: first, the direct affects of environment X on morphogenetic processes might be so strong that they always channelled morphogenesis into X-type chreodes in spite of the relatively small stabilizing effect of morphic resonance on these pathways. (2) And second, plants of other varieties of the same species would influence development by morphic resonance, although less specifically;" (p. 133).

In that way Sheldrake demonstrates excellently what an unscientific and unfalsifiable "hypothesis" is and how it can be used to argue about results of experiments that are derived from such a conception that does not satisfy scientific standards.

So far in this test and examination of Sheldrake's hypothesis it is easy to see that it is not a scientific hypothesis at all. It is possible to conduct experiments - but even if they give the expected results it is not possible to draw any conclusion about the "hypothesis".

Sheldrake put forward a conception that cannot be considered as a scientific hypothesis after all. It is formulated in a completely unclear and vague manner - it does not define exactly the "systems" it is referring to (or those it is <u>not</u> referring to) or give an accurate idea of <u>when</u> <u>which</u> kind of causation is to be expected the "morphogenesis" of a system.

Therefore there may not be a cogent reason to refute this "hypothesis" for a certain case: either one can conclude afterwards that the kind of systems that were tested in a single experiment are evidently not referred to by the "hypothesis", or one can argue that because the other "types of energetic causation known to physics…" (p. 115) must be so dominant that it was impossible to prove the nevertheless existing and believed influence of the morphogenetic fields in this case. And this applies to all of them who <u>believe</u> in the truth of this idea: there always is an expedient to "save" their "hypothesis" and to give explanations and excuses for failed "proofs" by hinting to the vague formulation of Sheldrake's conception -

> "It should be re-emphasized that these (morphogenetic, J.H.-L.) fields do not act alone, but together with the energetic and chemical causes studied by biophysicists and biochemists." (p. 86).

But both accuracy and falsifiability are essential prerequisites to be capable to design unequivocal experiments and to make definite predictions according to the hypothesis which is to be tested. These basic conditions are not fulfiled.

Regardless of the results of the eventual experiments it is not permissible to draw any conclusion from these about a confirmation or refutation of this "hypothesis" as long as one uses scientific standards. Any experiments carried out to test this hypothesis and their results are in fact meaningless and in vain. This does not imply that the conviction about the existence of morphogenetic fields is wrong, but that it is impossible to test this conception with adequate scientific methods.

IV.

In addition to the fact that Sheldrake's conception does not represent a scientific hypothesis, it may be worthwhile to analyse whether it could be developed into one.

In other words the question is: is it at all conceivable that a scientific theory can be constructed which, on the one hand, refers to "systems" and their "forms", and on the other hand combines this with valid scientific theories - when these do not refer to "systems" and "forms", but to respectively different concrete (defined and named) systems.

Sheldrake definitely does not refer to concrete, defined systems but systems in general. For his theory he presupposes the existence of systems per se, and he wants to explain these systems and their at times special forms by supposing the existence of "morphogenetic fields" influencing "systems". And again, he does not wish to understand his approach as an alternative but rather as a supplement and "in addition" to traditional scientific theories:

> "In addition to the types of energetic causation known to physics, and in addition to the causation due to the structures of known physical fields, a further type of causation is responsible for the forms of all material morphic units" (p. 115/6).

> "It proposes that specific morphogenetic fields are responsible for the characteristic form and organization of systems at all levels of complexity, not only in the realm of biology, but also in the realms of chemistry and physics" (p. 13).

> "… these fields do not act alone, but together with the energetic and chemical causes studied by biophysicists and biochemists" (p. 86).

Physics, biology and chemistry do not refer to "systems". Theories advanced by these sciences do not talk about "systems", but about different and concrete systems. But what are the "systems" Sheldrake refers to?

Contrary to men, rats, flowers, cells and molecules, "systems" do not exist. One may well consider such things as systems, but it is not possible to consider one object as more than one system at a time. Likewise one may consider a certain object as apart of another system or also as a union of systems. Yet a system does not "exist" in any of the cases, it is nothing more than a product of analytical work by a human being.

A "system" does not exist objectively (which means independent of the observing subject), it becomes visible and observable only by the observant who actually is defining and considering a certain object <u>as system</u>.

Ashby gave a quite remarkable clear and useful definition of "system":

> "… The <u>system</u> now means, not a thing, but a list of variables. This list can be varied, and the experimenter's commonest task is that of varying the list ("taking other variables in account") until he finds a set of variables that gives the required singleness." (3)

The combining of variables itself is done by the experimenter, and what is considered to be "the required singleness" will differ from one experimenter to the next, and therefore also the respective systems. Biologists define other systems than chemists or physicists. But the "systems" exists only "in the head" of experimenters, not objectively: systems are combined, defined and finally also experienced and "discovered" by subjects, human beings.

It is now also possible to understand what the "form" of any system will be: it is completely dependent and a consequence of the <u>definition</u> of the individual system. The "form" of a system is accurately represented by the combination of the variables which form the system.

All that escapes Sheldrake. One reason is that he does not even try to comment on what he means by "system". Indeed he tries to handle "the problem of form" (p. 55 - 59), but finally he does not succeed satisfactorily. He himself obviously has no concrete idea of what he means when he talks about "form": are forms "recognised directly" (p. 55), can they "only be represented visually" (p. 56), or is the description of "forms" yet more than a topological problem? Because form - in the sense of Sheldrake's use - "includes not only the shape of the outer surface of the morphic unit but also its internal structure" (p. 116)? (On the other hand, he also distinguishes between "form" and "organization": " … specific morphogenetic fields are responsible for the characteristic form and organization of systems at all levels of complexity", p. 13). Because he does not have an accurate idea of "system" he cannot succed in obtaining an accurate idea of "form".

But on the other hand he presents a lot of concrete examples where he in fact is able to give an evident representation of what he wants to say in every case. He does not need to represent the forms he talks about "only visually", mostly he uses just a few words or sentences. Everyone is able to understand what the special systems are he is referring to in every example and what is to be considered as their "forms".

And this is only because he is neither referring to "real objects" as such nor to any "systems existing by themselves". He talks about systems he creates himself in every case: it is he who reduces the "real thing per se" to a system by only taking into consideration the "characteristics" he considers as characteristic.

He does not refer to the characteristic form of objects in any of his examples - but the characteristic form of systems, also in accordance with the formulation of his hypothesis that does not talk about things, but systems and morphic units.

Obviously Sheldrake is not conscious of this difference, and so he mixes up the "real things" with the systems he is talking about. That is a typical confusion of logical types,(4) caused by using the same terms for different meanings.

Sheldrake forgets finally that it is he who defines the systems he is talking about in his examples. And by neglecting his own contribution to the creation of these systems he tries to explain them by supposing the existence of morphogenetic fields.

Now it is easy to see that the "similarity" between two systems can only surprise one if one has forgotten that systems are defined, whereas similarity can not directly be found between two objects. Actually there are infinite ways of describing a thing or object, and it is always an observing subject that decides which way this object shall be perceived: which attributes ,are to be considered as the characteristics (and with that the characteristic form) of this thing. And by describing an object’s characteristics and referring to these characteristics as representing the object itself one is actually not referring to the object itself but to a system that is formed and defined by the observer/experimenter.

Only after a reduction of objects to systems (and that means to the characteristics a subject wants at times to be considered as characteristic) is it possible to compare them and judge "similarity" or difference".

Similarity and difference are not attributes of things as such but results of an analytical process advanced by an observer. Two things can never be similar "as such" but only with regard to some defined criterions. These criterions are choosen by the one who compares the two things. And it depends only on him and the measure he is applying to determine whether similarity "exists" or not.

Neither objects nor their forms are recognised directly. Perception of similarity always implies a preceding classification and systematization carried out by the observer. Even if he is unconscious of and neglects that - it is he who defines and classifies, compares and "sees" the similarities and differences and the next one, using different measures and classifying in a different way, will find different similarities and differences.

That leads to the conclusion that there is no sense at all to construct a theory that is based on the presupposition that "systems" exist and that explains the form of systems in general and in combination with acknowledged theories of natural sciences when each of them refers to concretely defined systems.

In fact the terms "form" and "system" are words that belong to a language talking about theories of natural sciences. These terms do not refer to existing things but to talking, thinking and researching about existing things.

A theory about "systems" and "form" in general can not be a theory similar to other theories of the natural sciences or be combined in a rather logical-methodologically clean way. It can merely be a meta-theory, a philosophical theory or theory of cognition, dealing with questions like "why do we define concrete systems the way we do - and why do we not define them in one of the infinite other ways that also were possible?"

A theory about "systems" and "form" has therefore always to take into consideration the fact that it is human beings who "create" and "perceive" certain systems as systems. Regarding this it is obvious that quest ions and explanations referring to the existence of "systems" in general and their forms is a paradox.

V.

Applying scientific standards, Sheldrake's conception is not a testable hypothesis nor can it ever be. It is formulated highly inaccurately and is mistakable so that it is impossible to derive clear-cut experiments and testable predictions which can falsify the hypothesis. Furthermore it is evident that there is no need and no facilities to explain the objective existence of systems and their form by morphogenetic fields (which are to be regarded "as analogous to the known fields of physics in that they are capable of ordering physical changes", p. 72) - systems as such are not existent but defined.

Without any agitation it can be stated that Sheldrake's conception did not qualify at all as a scientific approach - according the valid scientific standards. Quod erat demonstrandum.

But this is only possible to discover if one actually is willing to prove whether this conception really is scientific and if one refers to a certain idea of "science".

Nobody can be forced to prove Sheldrake's conception in this way. Therefore there is also another, quite different way to deal with this "hypothesis": it can just be taken for granted that it really is scientific. Presupposing this - as Sheldrake actually does - as an apriori it makes no sense nor is it necessary to analyse it. Assuming that the hypothesis satisfies scientific standards as a conditio sine qua non, it becomes impossible to detect the contrary. One would rather be very astonished and surprised about all the new "scientific" discoveries and perceptions one gains by applying that 'hypothesis", than to find its unrespectability.

In fact this implies a change of the rules of science, an alteration of scientific standards. And that is of course possible as well: under the condition that we formulate new definitions for "science" that fit Sheldrake's hypothesis, we can talk about the· "hypothesis of formative causation" as a scientific hypothesis, and with that special meaning of "scientific" it is also possible to test it in experiments and to draw conclusions about confirmation or refutation of this hypothesis.

But we should always remind ourselves that this would be based on completely different ideas of "science", "test", "hypothesis" etc.

The question of confirmation or refutation of Sheldrake's approach as a valid scientific theory depends, as we can see, not mainly on the results of the experiments proposed by himself, but it will be determined by choosing a definition of "scientific" and by altering with that scientific standards. This means one has to decide about the importance one is furthermore willing to attribute to the exactness of terms, falsifiability of a theory and unequivocal logical conclusions. And again, this decision has to be made before eventual experiments.

Annotations

(1) All page numbers are referring to: Rupert Sheldrake, A New Science of Life, London 1984 (Paladin Books)

(2) With this interpretation Sheldrake is once more not following his own hypothesis that states that morphic resonance is "provisionally assumed not to be attenuated by space or time, and to continue indefinite1y" (p. 117)!

(3) Ref. to: W. Ross Ashby, An Introduction to Cybernetics, London 1958 (Chapman & Hall), p. 40

(4) Ref. to: Gregory Bateson, Mind and Nature, New York 1979 (E.P. Dutton), p. 114 - 126

[This paper was sent to the Tarrytown Prize in 1986 – but I never did even get an answer. Prof. Dr. Johannes Herwig-Lempp, Große Ulrichstr. 51, D-06108 Halle, Germany, www.herwig-lempp.de, johannes@herwig-lempp.de, August 2007]