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Scientific Practice and the Disunity of Physics[1]

The last decade has seen a large amount of detailed case studies in the history, sociology, and philosophy of science. The episodes that are presented in these studies are often determined by specific features that seem not to be representative for all of science. It is this specificity and diversity of scientific practice that undermined the conviction that science or even physics is a unified enterprise.

It is my aim in this paper to look at some of the arguments that are brought forward for or against certain claims to unity/disunity (in particular to examine those arguments from science and from scientific practice) in order to evaluate whether they really show what they claim to. This presupposes that the concept or rather the concepts of the unity of physics are reasonably clear. I will therefore begin with the discussion of these concepts.

Three concepts of unity can be identified: (1) *ontological unity*, which refers to the objects physics is about; (2) *descriptive unity*, which addresses the descriptive devices physics employs in dealing with physical systems (3) *unity of practice*, which deals with what physicists actually do. There are various relations between these kinds of unity. For instance, if the theories in question are interpreted realistically, descriptive unity typically implies ontological unity.

1 Ontological Conceptions of Unity

(i) There is exactly one object physics deals with – one reality, one nature, one world, one universe. There is exactly one item that falls under all of these concepts – it is therefore one object in the sense of being a *single* object.[2] This is not a very substantial conception of unity. It is an expression of the fact that we are able to form all-inclusive concepts. Physics deals with the single object that falls under these all-inclusive concepts insofar as it deals with parts of this single world or single universe. It seems to me that hardly anyone takes it to be very controversial that there is just one universe, one world etc. that physics deals with in the sense mentioned.[3]

(ii) I. Hacking contrasts the above conception with the "harmony"-conception of unity. The latter concerns the connection of the parts of nature.

This unity has something to do with the integration or harmony of parts, a harmony that exists or does not exist after the item has already been individuated as one thing [...]. (Hacking 1996, p. 41)

This integration, interconnectedness or harmony of the parts of nature or of a physical system is subject to two different interpretations:

a) The interconnectedness of what physics is about might mean that the different entities which physics deals with are all of the same *kind*. Thus it is often claimed that electromagnetic, weak and strong forces as well as gravity are really only one kind of entity. Strictly speaking, "dreams of a final theory" or of a "theory of everything" concern the unification of *descriptions* of nature and we will deal with them below. However, it is often the conviction that the subject of the descriptional theory is unified which motivates people to look for such theories in the first place. Maudlin mentions the unification of electricity and magnetism in the Special Theory of Relativity as a case in point in a recent paper. It was a symmetry in electric and magnetic phenomena that convinced Einstein that electric and magnetic force are on a fundamental level one and the same. Maudlin concludes:

Thus the electric and magnetic fields are "unified" by being, in a way, eliminated entirely from the fundamental ontology, and by being replaced by a single frame-independent entity. (Maudlin, 1996, p. 133)

It was the conviction that there is an ontological unity that made Einstein look for a unified theory. This strategy leads us to the claim that physics is unified in the sense that all of the known forces are only specific appearances of the one entity which unifies them all.

(b) The interconnectedness of what physics is about might mean that all physical systems are related in such a way that it is, strictly speaking, impossible to causally isolate their behaviour without changing it. Interconnectedness in this sense amounts to *ontological* holism. This is one aspect of the kind of unity of physics Scheibe calls "coherence":

> A case in point is Newton's theory of gravitation. What this theory has to say about one body as being a gravitating body cannot be said other than by relating it to every other body in the universe. Moreover, if we were to find a system of bodies moving exactly according to Newton's theory this very same theory would permit us to conclude that the system is all-inclusive. No part of a Newtonian system being itself a Newtonian system, the part can only be understood by referring to the whole. (Scheibe, 1998, p. 52)

Even though our world is coherent or united due to gravity it is not *necessarily* so. A world with forces that have a finite range would be separable into parts that are not interconnected. (This, however, is not true in the case of quantum correlations, another example of ontological holism or coherence.)

If we accept the complete connectedness of all physical systems we end up with the following conclusion: Strictly speaking physics has to deal with a single object, *viz.* the whole world. It is dealing with the world not just in the sense of dealing with parts of it, as conception (1.i) has it, but with the world as an *integrated whole.*

(iii) One might also think of laws of nature as basic to our ontological inventory. In section (2) I consider laws or theories as descriptive devices that describe the behaviour of physical systems. If one takes the laws themselves to be part of our ontology then the nomological versions of the unity of physics that are considered in (2) have ontological counterparts.

2 Unities of Description

Unities of description assume that there is a *single* law or theory that has to do all the work. Various conceptions can be distinguished according to the sort of work that they are supposed to do.

(i) There is a single theory that provides empirically adequate descriptions of every physical system, i. e. it is universally valid. This conception of a descriptive unity requires that all other theories or laws can be reduced to the one in question. One could take for example quantum mechanics as the fundamental theory. This would amount to the claim that all physical systems can be described empirically adequate by the Schrödinger equation. It is this conception that Mittelstaedt employs when he pleas for the unification of quantum mechanics with its interpretation.

> [T]here are good reasons to believe that quantum mechanics is universally valid and can be applied to all domains of reality, i. e. to atoms, molecules, macroscopic bodies and to the universe. (Mittelstaedt, 1998, p. 122)

Given its supposed universal validity the processes needed for testing and interpreting quantum mechanics should be part of what quantum mechanics is able to explain. On the other hand the measuring process – according to Mittelstaedt – does not fall into the domain of quantum mechanics because it is the measurements that provide the theoretical terms of the theory with meaning.[4] Mittelstaedt is forced into this dilemma, because he presupposes that physics ought to be unified in the above mentioned sense.

A more restrictive version of this conception would take the Hamiltonian to be part of the theory. It would then be the Schrödinger equation with *one particular* Hamiltonian that has to be able to provide empirically adequate descriptions of the behaviour of all physical systems. If one wishes to claim that a theory should take account of the fact that there is only one kind of force or potential in nature – a theory of everything in the sense of (1.ii.a) – one has to invoke this restrictive reading.

(ii) A less restrictive conception of the descriptive unity of science allows other theoretical or pragmatic virtues such as explanatory power or generality with respect to the domain of application to override empirical adequacy. The claim then is that there is one theory that is able to deal with all physical systems – even though the descriptions provided are not necessarily empirically adequate. If we take the Schrödinger equation as basic, this conception allows not only for idealization and modelling at the level of the Hamiltonian but even for a plurality of models for one and the same physical system. It thus reconciles unity at the level of theory or fundamental law with a possible plurality of modelling. In the literal sense of the expression even this kind of unified theory might be called a "theory of everything".

(iii) A third conception of unity points to the fact that our descriptions of the world as a whole cannot be split up into descriptions of parts of the world. In this sense our theory is holistic and thus unified. This is the descriptive aspect of Scheibe's conception of coherence.[5] Strictly speaking, there can only be one instance of application of this theory – as opposed to the above conceptions of unity of theories which allow for multiple instantiations.

A descriptive holism of this kind should not be confused with the epistemological holism Quine proposed. Whereas the latter is due to the fact that we cannot *verify* parts of the theory in isolation and thus concerns the *evidence* for the theory, the former kind of holism is entirely determined by an underlying ontological holism in the sense of (1.ii.b) and concerns thus the *content* of the theory.

Hitherto we have exclusively dealt with global conceptions of theoretical unity. One might, however, also argue for descriptive local forms of unity. Falkenburg shows that Bohr was striving for this kind of unity. She distinguishes two kinds of local unities: vertical and horizontal.[6]

(iv) *Vertical* unity provides a local unification of different theories, e.g. quantum mechanics and electrodynamics; it is an *inter*theoretical notion. A case in point is Bohr's analogy between the concept of frequency as used in electrodynamics with the one used in his atomic model. Later on Bohr uses the concept of "correspondence" to describe intertheoretical relations of this kind. The analogy or correspondence provides a local unification of concepts and thereby of theories. This conception is local in the sense that it is restricted to particular models and not to quantum theory and electrodynamics as a whole. It is a weakened version of (i) in the sense that it requires a local reduction of one theory to another (in the case of frequencies: for large quantum numbers quantum mechanics has to provide the same results as elctrodynamics)

(v) A *horizontal* unity provides a local unification of different phenomena that are treated by one and the same theory; it is an *intra*theoretical notion. An example is the use of analogy between the emission of light rays due to transitions of lightly bound electrons and the emission of X-rays due to transitions of firmly bound electrons. Old models are used to develop new models. The range of phenomena that is treated by quantum mechanics is locally unified through the use of these analogies. In Bohr's later writings it is the concept of complementarity that establishes a local unification of complementary phenomena such as the particle and wave aspects of, say, an electron. It is, however, necessary to go back to classical terminology to describe these aspects, i.e. it is necessary to employ intertheoretical relations. Complementarity establishes local unity within the domain of quantum phenomena, *relative* to the structure of the classical domain.[7] This conception is local in the sense that it unifies only certain groups of phenomena and not all of what quantum mechanics deals with.[8]

3 Unities of Scientific Practice

(i) Methodological conceptions of unity claim that there is one method that characterizes all of science or physics. Popper's falsificationist strategy is a case in point. Similarly Poser thinks that the unity of science consists in a shared regulative ideal of truth. Even though such a regulative ideal does not allow for definitive criteria of theory-choice it explains why science is intersubjectively controllable.[9]

Another example is the *Principle of Action* Graßhoff describes in his paper:

> The following principle holds for any person A, action H and G: *Principle of Action*: If (i) A has the goal G, (ii) A believes that doing H under the given circumstances is a way to reach G, (iii) There is no other Action besides H, which in A's opinion has a higher preference for reaching G, (iv) A has no other goal which diverts him from G under the given circumstances, (v) A knows how to do H, (vi) A is able to do H *then A concretizes H.* (Graßhoff, 1998, p. 186)

As it stands this principle is very general; it becomes a *scientific* principle when applied to model construction. The aim that scientists generally hope to achieve is according to Graßhoff empirical adequacy.[10] The actions that are envoked to achieve this goal are modifications of models (model expansion and model construction).[11] Graßhoff suggests that all or most of scientific actions can be reconstructed in this terminology.

(ii) In a recent article Cat, Cartwright and Chang describe Neurath's conception of the unity of science as a unity of action:

> By unity he emphatically did not mean a single system of science, which he deemed impossible; instead, what he sought was the linking of different bits of science at various points of prediction and action. To make this possible he urged an "orchestration" of the sciences that was partly to be realized in the form of the Encyclopaedia. (Cat et al. 1996, p. 369)

This kind of unification is not really a unity of science or scientific practice as it allows for all kinds of ontological and descriptive disunities of science. It rather locates unification of certain bits of scientific knowledge and experimental and technological procedures *outside science* in its application to specific problems. As far as I understood Cartwright in the discussion, this is not only the position she takes to be Neurath's but also her own.

4 Possible Claims

Concerning the above conceptions of unity, various claims are possible. We can look for evidence from science or scientific practice only with respect to one of the following claims. One could argue that either

(a) a unity of a certain kind *is* actually realized or that

(b) physicists *aim* at realizing a certain kind of unity or that

(c) physicists *should aim* at realizing a certain kind of unity.

Since ontological kinds of unity do not depend on the actions of physicists options (b) and (c) only apply to descriptive and methodological kinds of unity. I will now examine what the evidence for claims (a), (b) and (c) looks like.

5 Realized Unities

With respect to claims of kind (a) defenders of descriptive kinds of unity readily agree that such unities are not actually realized. It thus remains to consider whether there is evidence for ontological and methodological kinds of unity.

The ontological singleness-conception (1.i) does not confront us with a very substantial conception of unity. It is an expression of the fact that we are able to form all-inclusive concepts. It therefore seems not to be a debatable point.

As to Scheibe's ontological coherence claim (1.ii.b) – the parts of nature are united in such a way that they cannot be separated – the evidence seems to be as good as it can be. For the theories we hold to be true tell us that there are quantum-correlations and they tell us about infinite-range forces. So the evidence for ontological coherence is as good as the evidence for these theories. Ontological coherence is not invalidated by the fact that the relevant interactions or correlations are small. It might help us to *describe* parts of the world *as* isolated. But that does not mean that they *are* isolated.

It remains to consider the unity of forces (1.ii.a). Is there any evidence for an entity that replaces all the known forces in the sense described above? This is a question one should leave for physicists to answer. I guess that science does not yet provide a generally accepted answer to this question.[12]

So what about methodological unity? Poser's claim that truth is instantiated as a regulative idea is a reaction to the fact that it is difficult to isolate generally accepted criteria for theory-choice within scientific practice. The argument for truth as a regulative idea rests entirely on the fact that scientists *argue* for or against certain claims. Other features of scientific practice cannot be used as evidence either for or against this conception unless one is able to establish inevitable connections between these features – eg. criteria of theory-choice – and truth. Poser's conception of methodological unity seems thus to be instantiated in science; however, it is so weak a conception that it seems to be instantiated in politics as well.

The claim that Graßhoff's action-principle with respect to the goal of empirical adequacy is *realized* in scientific practice amounts to the claim that scientists *aim* at empirical adequacy. I will therefore discuss it in the next section.

Neurath and Cartwright locate the unity of scientific knowledge and action outside science. So there is no evidence for or against this conception within science or scientific practice. What we are interested in is whether there are other kinds of unity that characterize science over and above this conception.

6 What Scientists Aim at

What scientists aim at can be found out by looking at what counts as success in scientific practice. It is here that another notion that figures in the title of the conference comes in to play, namely the notion of a model. This concept is used in various senses within the philosophy of science. There is for example the notion of a *semantical model*. Models in this sense are what theories are true of. The conception of semantical models is important in physics insofar as every solution to the Schrödinger equation is a model of quantum theory. It is, however, not a useful concept for the analysis of how real physical systems are represented in scientific practice. This is due to the fact that semantical models have no force in explaining certain important and constitutive features of scientific practice, *viz* idealizations. Idealizations usually come into play when the force function or the Hamiltonian is specified. A certain physical system is described *as* a harmonic oscillator. Cubic and higher terms in the potential are neglected, certain features are simplified for the sake of mathematical simplicity etc. Harmonic oscillators, infinite potential wells, etc. are examples of *idealized models*. Morrison,[13] Cartwright[14] and Graßhoff[15] work with idealized models. The very notion of an idealization entails that the relation between an idealized model and what it is an idealized model of cannot be described with the help of a semantical model, because to describe something *as* something implies that it is not true of it (in the strict sense). A third kind of model that plays a role in the philosophy of science is the *analogical model*. In this case the way a theory deals with a certain kind of physical system is used as a model (or paradigm) for other kinds of physical systems. Thus at the end of the 19th century mechanical models were used as analogical models for electrodynamics. This use of model is described in Falkenburg's paper on Bohr.

"Model" is a relational term. Each of the three senses makes use of a different relation. A semantical model is a model *with respect to a theory*, an idealized model is a model *with respect to a real physical system* and an anological model is a model *with respect to the theoretical treatment* of a physical system. The problem with these different concepts of a model is that one and the same kind of model can serve as a model in all three senses. Take for example the case of an oscillating rotator. It can be used as an idealized model for carbon monoxide. The treatment of carbon monoxide with the help of the oscillating rotator is an analogical model for other systems such as ethylen. If one furthermore specifies the value eg. for mass etc. the idealized model is a semantical model for quantum theory.

With respect to the unity/disunity debate it is *idealized* models that occupy centre stage. This is so because idealization works against empirical adequacy and thus against the claim that scientists aim at descriptive unity in the sense of one overarching theory that is empirically adequate in the sense of (2i). A further argument against descriptive unity in the sense of (2i) is the plurality of models and theories that is used even in established physics as Morrison and Cartwright point out. The crucial question, however, is whether plurality and idealization are purely *heuristic* features or rather characterize the state of physics we aim at. Graßhoff, for example, presents a case study where the plurality of models is eventually eliminated and idealizational features are eventually removed. So what do scientists aim at? The crucial test would be the analysis of paradigmatical cases where scientists have the option to opt for different treatments of physical systems that instantiate different theoretical virtues. It is in cases like these that one can see whether empirical adequacy is all that scientists aim at or whether other virtues may override it. Elsewhere I have tried to argue that this can be done and that empirical adequacy is not the only virtue that scientists aim at. [16]

If it were possible to establish idealization and the proliferation of model-construction as *persistent* features of physics this would work against (2i) – i.e. the claim that the aim is a universally applicable and empirically adequate theory – but not necessarily against (2ii) which only asked for universal applicability. The fact that we use a plurality of Hamiltonians to model a certain physical system – even if the choice is determined by technological considerations, as the paper by Morrison indicates – does not undermine the claim that quantum mechanics is the universal theory that is able to deal with all phenomena. The construction of Hamiltonians allows for idealization and plurality *within* quantum-mechanics. However, if Cartwright's claim is true, that theories as opposed to models peacefully coexist (eg. quantum mechanics and classical mechanics), then we have a different case.[17] A plurality of fundamental theories obviously speaks against (2ii) as well.

What we are left with then are local kinds of unity in the sense of (2iv) and (2v) such as Falkenburg described. Even though there are no overarching theories in the sense of (2i) or (2ii) local unity is established through the use of analogical models. The use of models in this sense is very likely motivated by the wish to employ the few Hamiltonians we can deal with in as many cases as possible. This, however, does not yet require a *semantic* or *linguistic unity*[18] as Bohr envisaged it: Even though the same mathematical structures are used in various areas they need not be interpreted with the help of the same vocabulary. Aiming at local unities with the help of analogical models does not imply that scientists aim at a semantical unity.

Certain kinds of idealization also work against the claim that descriptive coherence in the sense of (2iii) is what scientists aim at. Even though our best theories tell us that there actually are quantum-correlations and that there are infinite-range forces, physical systems are often treated *as though* they were completely isolated. Physicists employ experimental and theoretical strategies to treat physical systems as isolated. They try to shield off causal influences and subtract theoretically the influence of other causal factors. Since physicists employ these procedures they seem to aim at describing physical systems in isolation even though strictly speaking they never are.[19] So descriptive coherence is certainly not what is exclusively aimed at.

7 What Scientists Should Aim at

Treating this question we change topics: from description of science to prescription for science. Therefore we have to look for reasons rather than evidence as to why scientists should or should not aim at the unity of science. It is, however, difficult to establish epistemic or other values that can be taken as a basis from which one can reason to certain prescriptions. Probably the most plausible line of reasoning is to appeal to empiricist values in the spirit of van Fraassen: Experience is our one and only source of information. Scientific theories should not go beyond available information.[20] Therefore scientists should aim at constructing empirically adequate (and maybe universally valid) theories in the sense of (2i). However, the persistent and deliberate use of idealization in modelling even in paradigmatic cases of scientific practice implies that even science at its best does not *exclusively* aim at empirically adequate theories. Shall we then say that all of science has to be reformed?

It seems to be difficult to argue from scientific practice to what scientists *should* aim at. What one has instead is that the suggested aims of science (unity or disunity) often appear to be connected to certain political contexts. This is very explicit in J. Dupré's recent book. There he claims that scientists should aim for the disunity of physics because unity presupposes an undesirable society:

> On the picture I am presenting, only a society with absolutely homogeneous, or at least hegemonic, political commitments and shared assumptions could expect a unified science. Unified science, we might conclude, would require Utopia or totalitarianism. (Dupré 1993, p. 261)

On the other hand there are also political contexts that encourage aiming for the unity of science. Appeals to truth and the universal validity of physical theories seem to encourage opposition in totalitarian systems in their appeal to universally valid human rights. Galison describes how the connection between political contexts and the unity/disunity debate vary in history. [21] This suggests that even given that we share the relevant political convictions it is not clear how to establish arguments on this basis for the unity/disunity debate. As a result I do not see that there are any convincing arguments for what scientists should aim for.

In conclusion it seems to me that there are no arguments from science or scientific practice to any kind of unity or disunity that scientists *should* aim for. The case is different, however, with respect to what they *do* aim for. The practice of constructing idealized models indicates that local unities in the sense of Bohr and maybe universal applicability – pace Cartwright – is what is sought for. Theories that are *both* universally applicable and empirically adequate are certainly not what scientific practice points to. Concerning ontological unity: Besides the trivial singleness-conception the only claim for which we find convincing evidence within science is the claim to ontological coherence or holism.

Notes

1 Thanks to M. Carrier for helpful comments on an earlier draft and to J. Kraai for correcting the English.

2 Hacking calls this the "singleness"-conception of unity. See (Hacking, 1996, p. 40 f).
3 See Hacking, however. His counterexamples refer to to some additional features rather than to the pure singleness-conception; (Hacking, 1996, p. 45).
4 See (Mittelstaedt, 1998, p. 131)
5 See (Scheibe, 1998).
6 See (Falkenburg, 1998, p. 98 f.)
7 See (Falkenburg, 1998, p. 113 f.)
8 Unities of description presuppose a unity of language at least to the extent that reductionist claims are entertained. I will not discuss this point in more detail.
9 See (Poser, 1998, p. 18 f.).
10 See (Graßhoff, 1998, p. 193 f.).
11 See (Graßhoff, 1998, p. 187 f.).
12 Maudlin comes to the conclusion that there isn't much evidence for a unity of forces: see (Maudlin, 1996) "At this point, there is little hard evidence for the kind of structure postulated by the GUTs and even less for the TOEs."
13 See (Morrison, 1998).
14 See (Cartwright, 1998).
15 See (Graßhoff, 1998).
16 See (Hüttemann, 1997, chap. 2).
17 See (Cartwright, 1998, p. 28–34).
18 See (Falkenburg, 1998, p. 116).
19 See (Hüttemann, 1998, chap. 3).
20 See (v. Fraassen, 1985, p. 286 f.).
21 See (Galison, 1996, p. 6).

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