Coordination of Space and Unity of Science

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ABSTRACT

In this essay, I explore a metaphor in geometry for the debate between the unity and the disunity of science, namely, the possibility of putting a global coordinate system (or a chart) on a manifold. I explain why the former is a good metaphor that shows what it

means (and takes in principle) for science to be unified. I then go through some of the existing literature on the unity/disunity debate and show how the metaphor sheds light on some of the views and arguments.

1 Introduction: the unity of science

By way of introduction, I shall explain in this section, in the simplest terms, what I mean by a global unification in science. To obtain precision, it is best to begin by way of elimination. I shall not consider unity of science as a political achievement, e.g. the establishment of a global NASA; nor shall I consider it as a unification of a code of conduct for scientific research; nor as a unified ideology: e.g. strict testability by experiments of every hypothesis as the criterion for being scientific; nor as a purely methodological unification, e.g. the fact that Hamiltonian differential equations are used both in physics and economics does not imply that the two disciplines are unified; nor as a giant conjunction of all true scientific theories; and so on.

I take the core concept of unity of science to be derived mainly from two sources: the Carnapian (or logical positivist) reductionism: all scientific theories being reducible to a theory of the observables; and physicalism: one theory for one physical world. Surveying the literature of the unity/disunity debate, which will be selectively examined in section 4, one cannot fail to notice that the target notion of unity of science -- for a kind of explanatory unification of scientific theories -- contains the following elements (cf. Oppenheim & Putnam 1957).

[U1] A metaphysical postulate: the world -- at least the actual one -- is composed of a limited number of kinds of constituents (including one kind).

[U2] A unified fundamental theory: a single theory that accounts for the behavior of the fundamental constituents.

[U3] Reductionism: a set of reductional statements (or bridge lawlike statements) that connect statements of all other phenomena to the theory of the fundamental constituents.

An example of [U1] would be that nature ultimately consists of particles and fields (of a few kinds) and spacetime. An example of [U2] consistent with the metaphysical picture would be a set of terms describing all possible states of particles and fields in spacetime -- i.e. their behavior -- and a set of lawlike statements in those terms expressing laws that determine such behavior. And an example of [U3] consistent with the above two would be a finite set of statements that relate the theories for non-fundamental phenomena to the theory of the fundamental constituents. All phenomena are to be explained by the fundamental laws via such reductional relations. There are other possible images of unity of science, such as the Carnapian notion to which I will return in section 4. Suffice it to say that mine, which is essentially derived from Oppenheim & Putnam's, rests on the belief that if unification is to succeed, it will do so along or near the current trajectory of science.

It is also obvious -- judging from the current state of science -- that such a unification can only be defended as a goal that science may reach in the distant future. The debate is essentially on the question of whether or not the above should be regarded as a goal of science. But whether we should regard something as such-and-such depends on whether it can possibly be -- as a matter of metaphysics -- a such-and-such. This paper aims at providing an apt metaphor in geometry that illustrates the importance of the latter question and the difficulty of answering it.

2 The coordination of a space

Ever since Descartes discovered the algebraic method of studying geometry, we know that one needs to put a coordinate system on the geometric object one is studying before one can subject it to any algebraic analysis. Obviously, not all objects are suitable for coordination; and for the purpose of our metaphor, we begin with the topological spaces, which, among other things, are connected and have well-defined open subsets. A topological space is a pair, <X, T>, where X is a set of points and T a subset of the power set of X such that (i) the intersection of a finite number of members of T is also in T; (ii) the union of an arbitrary number of elements in T is also in T; and (iii) both the null set and X are in T. A coordinate system, or a chart, of any open subset of X is a map (or function) that sends every point in the subset to a unique tuple of numbers in R^n, where $R^n = R \times R \times ... \times R$, an n-fold Cartesian product of the real numbers. This is how properties of points -- i.e. geometric properties -- can be studied algebraically by relations among tuples of numbers. For instance, the chart for the 3-dimensional Euclidean space is the Cartesian coordinate system, R^3.

One may think that every manifold -- i.e. a topological space that can be covered by charts -- can be covered by a single chart. After all, if a manifold is coverable by a set of charts, it may also be seen as coverable by a single chart, which is produced by extending one chart to replace the rest. This, however, is not true. An illustrative example of the failure of a single global chart is given by the case of coordinating an n-dimensional sphere, S^n. It can be proven that it is impossible to map S^n onto R^n with a single mapping function. I shall give the usual illustration of why it is the case (see Figure 1). The easiest way to see this is to imagine an (n+1)-dimensional space in which S^n lives, where in Figure 1 the 2-dimensional surface of the page represents the (n+1)-dimensional space and the 1-dimensional circle S^n. We put R^n (R_1 in Figure 1) at one point on S^n -- call it S -- as its tangent space. Then we pick the point on S^n directly opposite to this point, call it N, as the vintage point from which we stereographically project every point on S^n to R^n. Every point of S^n can be so projected -- by ϕ -- onto a distinct point on R^n, except N, since the projection line of N, if we can call it that, is parallel to R^n.

However, we can put another R^n at N as its tangent space and then we have another chart -- R_2 in Figure 1 -- that coordinates every point of S^n except S; and if we could use these two charts together, we would have completely 'covered' S^n. That we can use the two charts together is guaranteed by their *compatibility*, namely, a transformation, say, from R_2 to R_1 with respect to the same open subset of S^n does not introduce distortion in the coordinate description of the subset. Mathematically, this property is reflected in the requirement that the two charts are C^∞-related, namely, for any non-empty intersection of two arbitrary open subsets of S^n, $\phi \circ \psi^{-1}$ and $\psi \circ \phi^{-1}$ are infinitely differentiable functions. The two charts, R_1 and R_2, are obviously C^∞-related because ϕ and ψ are C^∞ and since they are 1-1 onto maps, ϕ^{-1} and ψ^{-1} are also C^∞.[1]

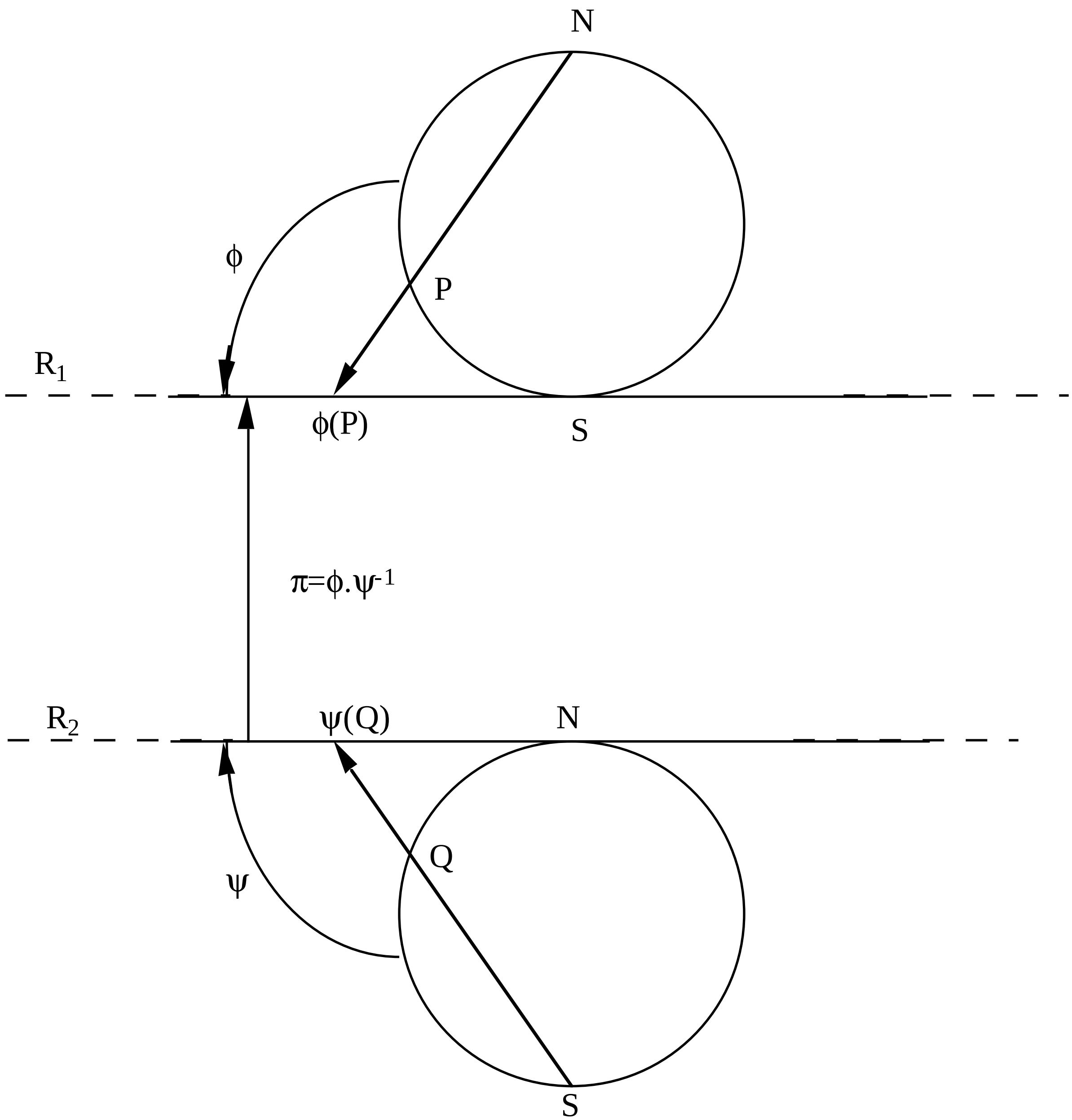

Figure 1: The coordination of an n-dimensional sphere in an (n+1)-dimensional space.

3 The analogy

Let us now explore what, as a metaphor, the above case in geometry may tell us about the unity of science. One must note first that this is strictly a metaphor; it is *not* a geometric analysis of the structure of scientific theories. The merit of such a metaphor depends of course on the strength of the analogy between the coordination of a space and the epistemic representation of the world. There is obviously a sense in which constructing scientific theories to describe the phenomenal world is like constructing charts to coordinate a manifold. However, one must be careful about how to pick out that sense under which the analogy holds. Most importantly, the world -- which is the counterpart of a manifold in this analogy -- should not be thought of as the physical world that contains nothing but the fundamental stuff in spacetime. Because that would be presupposing the truth of reductionism, about which the metaphor is supposed to shed light on rather than presuppose. It, instead, should be viewed as the totality of all the actual phenomena or sets of actual states of affairs. Thus, different open subsets on a manifold is analogous to different phenomena or different kinds of states of affairs; and, for instance, mechanical phenomena should, at least from the outset, be considered as a distinct area from thermodynamic ones. And the overlapping of open subsets on a manifold should correspond to the overlapping of areas of phenomena: mechanical phenomena overlap with biological or social ones in the sense that biological as well as social systems also exhibit mechanical properties (or are also mechanical systems).

Charts of open subsets on a manifold are then comparable to theories of different phenomena, and when phenomena overlap different theories give alternative descriptions of the same states of affairs, just like different charts give alternative coordinates to the same set of points on the manifold. By the overlapping of phenomena I mean nothing more than the co-instantiation of sets of different properties -- i.e. different phenomena -- on the same objects. Analogous to the geometric fact that the same points may belong to different open subsets and, therefore, be coordinated by different charts, the same objects may exhibit different phenomena and, therefore, be characterized by different theories. For example, let us suppose that biological phenomena obtain in systems belonging to B and economic phenomena in systems belonging to E, where B and E are respectively all and the only systems that exhibit biological and economic phenomena. B and E certainly overlap; but it is not true that one contains the other (I am assuming that robotic societies may have economies). The two kinds of phenomena overlap -- as I defined it above -- in at least human societies.

With such analogies in place, let us imagine that a group of people are given the task of coming up with charts to coordinate an unknown physical object that is larger than their sizes by many magnitudes. Suppose that these people are only interested in the purely geometric properties of this object; so what they are doing differs from a similar task in geometry as instanced in the previous section not in essence but only in degrees of approximation. They divide themselves into several teams and disperse into several equally distant locations on the object and begin charting or mapping. Suppose again that from where each team starts, the surface looks flat. So they all begin with the straightforward Cartesian coordinate system for the respective tangent spaces. But as they move towards one another's neighborhood regions, they discover that the Cartesian coordinate system which fits so well at their own home-bases can no longer be used. Their first instinct is probably to try out other types of charts and see if more territory can be covered by a single chart. Let us suppose again that they obtain some success in doing this and by keeping on inventing new charts -- i.e. using new mapping functions, they keep combining more and more previously incombinable regions into one under a single chart. (Let us remember that a chart is an arbitrary function from a geometric object to R^n, where n is the number of dimensions of the object. So with enough ingenuity one may be able to do a lot more than what the simple example given in the previous section shows.)

To make the long story short, if the object happens to be one that is geometrically possible to be coordinated by a global chart, these geometers have every reason to believe that a global chart for the whole object will one day be discovered. The task may be difficult or requires a time longer than any of these people could possibly survive, if the object is geometrically complex and/or of an immense size, but we would not say that their push for a global chart is mistaken or wrong-headed. But on the other hand, if the object is something like S^n in the previous section, the task is impossible even though most of these people's effort will be rewarded. *Prima facie*, the same is true, *mutatis mutandis*, with scientists looking for theories to describe and explain the phenomenal world. The metaphysical questions is: what is the 'geometry' of the phenomenal world?

What lessons if any can we draw from this metaphor?

I. It is obvious that if the analogy holds, whether or not science can be unified -- or whether or not it should be a goal of science -- is not up to scientists and philosophers. It is determined by some properties of the world *and* some general properties concerning our capacities of theory construction. The failure of a global chart for a manifold is caused both by the geometry of the manifold and the limitation of mapping functions. Something analogous should be true if unification is to be impossible. However, this does not mean that one has to be a realist in order to use this metaphor. The 'world' of which a unified theory is considered could be a world of 'experiences' or 'sense-data', if you will, and the question of unification may still be determined by the properties of this world and the limitation of our resources of theory construction. Therefore, being an empiricist cannot shield one from the above implications of this metaphor, nor can being an instrumentalist of scientific theories, because as long as theories are used as instruments to make predictions that have to answer to the world, whether or not they can be unified should still be a matter of the world and of the possibilities of the instruments. One has to be a conventionalist or a social constructivist in order to escape this conclusion. If the real world or the experiential world is like a sphere and we are like its geometers, our attempt to understand our world can never reach a global unification.

II. What happens if the geometric object is proven coordinatable by a global chart? It does not follow at all that the global chart should always be used in the algebraic study of the object, nor does it even follow that such a chart can always be found. The mapping function may be so complex that it is not practically discoverable or, even it is, not useful. The analogous case for science should be obvious. In fact, one may consider the following. Suppose that physicalism is established by some metaphysical arguments and, therefore, it is possible that the extended science of physics may have a single theory which accounts and explains, token by token, the entire set of states of affairs that comprise the phenomenal world. Should all scientists give up whatever they are doing and begin the pursuit and completion of such a unified theory? Would such a theory be useful, or necessarily superior to the current motley of special sciences, even if it can be formulated? Hence, the geometric metaphor is more helpful with the notion of the impossibility rather than that of the possibility of a unified science.

III. The difference in the geometric case between a chart and an atlas may dispel some of the confusion in the debate on unification. In geometry, there are spaces that are not coverable by charts. If a space is not a topological space, there is no guarantee that it can be covered by a set of compatible charts. Moreover, being covered by charts that are not all compatible with one another is another possibility, but a mere possibility only, for it borders on an abuse of the predicate, 'being covered'. Using charts to cover a space is to allow us to study every point and its neighborhood of the space by algebraic means; but if there are points that are coordinated by two incompatible charts, such a study of them would be impossible. And finally, the difference between being covered by a set of compatible charts -- an atlas -- and being covered by a single chart is precisely defined in geometry. The latter is the case when a single mapping function suffices to coordinate the entire space, while for the former more than one function are needed, where the transformations among the functions must be smooth, which means that they are compatible. The analogy with the case of science should lead us to three scenarios:

(a) a state of science in which one theory explains all (two senses of which will be discussed later);

(b) a state of science in which different theories explain different domains of phenomena while the theories are compatible (to be made precise later); and

(c) a state of science in which different theories explain different domains of phenomena while not all theories are compatible.

If the geometric metaphor stands, the controversy between unity and disunity of science should be analogous to a choice between (a) and (b), while ruling out (c).

IV. In Figure 1, the two charts, ϕ and ψ, are almost global whether we use ϕ or ψ, only one point in each, N or S respectively, being missed. There presumably are many such cases in geometry where a manifold is practically covered by a single chart except for a few 'pathological' points or regions; and the different charts we use are usually those each of which makes one of such pathological points or regions appears normal -- as S is normal in ϕ and N in ψ. The moral of this consideration is that even if a manifold is known to be uncoverable by a global chart, it does not follow that we must give up constructing and using a single chart for most of what we need to do. When confronted with the case in Figure 1, we may still have the freedom to choose either (i) to adopt one of the charts and extend it all the way until it reaches the neighborhood of this singular point, N or S; or (ii) to insist on using two distinct charts and do the transformations for the overlapping region, which by the way is the entire sphere except the points N and S.

However, one crucial difference remains between choice (i) -- or rather its analogue in science -- and choice **III** (a) above: a search for an 'almost' or 'approximately' unified theory while a genuine one is impossible can only be justified on purely pragmatic grounds. If such a theory is neither simple nor useful, we then have no reason to make it a goal of science. Whereas the metaphysical possibility of a unified theory is itself the justification for unity; the burden of justification is then on the shoulder of the anti-unificationists. Unless it can be shown that it is practically impossible to reach that unified theory, the unificationists do not have to be moved by any other arguments.

V. If a manifold is coverable by a global chart, it by no means follow that only one such chart exist for that manifold. Therefore, we only talk about the existence of 'a' global chart, not 'the' global one. Similarly, if science is unifiable, it does not follow that there is only one theory which unifies science. So, we should only talk about the possibility of 'a' unified theory of science, not 'the' unified one.

4 The unity vs. the disunity

Now let us make some contact with the existing philosophical literature on the unity of science.

For Carnap and the logical positivists (Carnap 1934, 1938; see also Neurath 1946; Morris 1946), the notion of unification should not carry any metaphysical commitment, namely, anything similar to [U1] would not make any sense. Carnap considers the notion of unity to consist of a unity of language and a unity of laws, but maintains that the former is more pertinent to science and more likely to be realized than the latter. Scientific theories are roughly divided into those of the physical and those of the biological, in the broadest senses of the two terms. The unification of language is to be achieved via reduction, though not of the biological to the physical (or vice versa), but rather of both to what he calls the (physical) 'thing-language'. The vision is an operationalist one in which all terms used in either the physical or the biological sciences are to be rendered meaningful by a specification of the conditions under which observations are obtainable that either confirm or refute those sentences in which the terms figure. Whatever is needed for describing the conditions and the outcomes comprises Carnap's thing-language.

There is a clear and simple sense in which the Carnapian conception of unity is very similar to our case in geometry. Is not the thing-language similar to a global chart? With a set of more or less homogeneous terms and a few rather simple rules of application, the thing-language, if capable of reducing all other terms in science, can in principle descriptively 'covers' the entire phenomenal world. Even the reduction from other languages to the thing-language finds its analogy in the geometric case, where different local charts can be transformed to the global chart so that all geometric properties are preserved under the transformations. One might say that for Carnap and the logical positivists, the 'manifold' -- the object of science -- is not the world or phenomena but the set of all possible experiences, which they claim is 'coverable' by a single thing-language. As for unity of laws (or of theories), it should be viewed as 'an aim for the future development of science. This aim cannot be shown to be unattainable. But we do not, of course, know whether it will ever be reached. (Carnap 1938, 61)'

The difference between the language and the laws with respect to the certainty of unification (or the lack of it) seems to be one of whether or not a plausible general scheme is available by which reductions can in principle be carried out. In the case of the thing-language, a scheme was thought to be in hand while in the case of laws, no scheme of such specificity was nearly in sight. If the analogy with the geometric case works, it should be clear that whether or not the thing-language can unify science, in terms of having a single language for science, depends almost entirely on the nature and structure of our experience, about which nothing less than a full-fledged metaphysical investigation can help us to find out. The positivist approach which shuns metaphysics and champions the logic of science -- the syntax and the semantics of scientific languages -- is clearly not an adequate approach. This version of the unity of science was justifiably given up, together with the whole doctrine of logical positivism/empiricism. I have no intention to rehearse the reasons, but it is gratifying to see that the geometric metaphor actually points to the right reasons for which the Carnapian notion of unification must fail. The possibility of a unified description of our experience is not merely a matter of language, just as it is not a matter of choosing the right mapping function that determines whether a manifold is globally coverable.

My earlier scheme of unification, [U1] - [U3], is obviously modeled after the Oppenheim and Putnam proposal (Oppenheim & Putnam 1958) (see also Margenau 1941), which hypothesizes a six level hierarchy for science and emphasizes successive microreduction from higher to the lower levels, which ultimately ends with the physics of elementary particles. Unlike Carnap, the notion of reduction carries both metaphysical and epistemic obligations: not only theories at a higher level are reduced to those of a lower level in accordance with the standard account of Kemeny and Oppenheim (1956), but microreductions of one level of phenomena to another also imply compositional and structural (asymmetrical) relations between the reduced and the reducing. By such an image, the explanatory function of scientific theories -- which goes in the opposite direction of reduction -- becomes one of the key attributes of a unified science (which is exactly what scientists are after in a unification), and the reductional scheme roughly matches the one that the actual practice of science displays. Causey's version (Causey 1977) of unification is similar to Oppenheim & Putnam's, except it is a far more elaborate theory that goes beyond a simple and general hypothesis. It stipulates a logical structure for scientific theories, namely, it consists of fundamental laws, derivative laws, and identity relations (apart from logical terms), where the last item is mostly responsible for the microreductional relations; and because of this structure, it is able to offer a set of explicit conditions that a unified theory must satisfy (Causey 1977, 114-121). Such a vision of the unity of science was further enhanced by Friedman's theory of scientific explanation (or understanding) (Friedman 1974), in which convincing arguments are made at least for the case of *local unifications*. It also put in an extra dimension into the argument for unification, namely, scientific understanding of phenomena is mostly achieved only through reductive unification: we would not truly understand a set of unrelated phenomena until we see how they share the same underlying compositional elements and causal mechanism.

The geometric metaphor obviously works for this notion of unification. Just as in the case of coordinating a space, whether or not science can in principle be unified must be ultimately determined by the properties of the world and the limitation of our resources of theory construction. Oppenheim and Putnam dismissed a trivial version of unification --

science as a single conjunction of all theories -- and avoided addressing the issue of a unified fundamental discipline (Oppenheim & Putnam 1958, 4). The geometric metaphor may help us see what exact their position is, given these two moves. I shall discuss them in turn.

1. If the single conjunction of all theories accounts for everything and is true, it means that the theories are consistent with one another, especially when they deal with overlapping phenomena (whose meaning is given in the first paragraph of section 3). By dismissing this possibility, Oppenheim and Putnam is rejecting options **III** (b) and (c) in section 3. Hence, they are committed to **III** (a) which resembles the demand for a global chart. This is a significant point about the Oppenheim-Putnam position which may not be realized without the geometric metaphor. A successful reduction of two theories which cover two overlapping sets of phenomena, either in the form of one reducing to the other or the two being reduced to a third, is quite similar to the merging of two maps of two overlapping open subsets of a manifold, which can also happen in either of two ways: one map may be completely replaced by the other -- if one function can map the union of the two subsets in question -- or a third map is introduced to replace the two. And the break-down of a reduction is therefore also similar to the break-down of a single chart in Figure 1: when neither the extension of the stereographic chart from N can cover point N (or S *mutatis mutandis*) nor a third chart can cover both S and N, we know that an atlas is required.

Luckily, there is a simple case of an *apparent* break-down of reduction in science that I may mention briefly for an illustration (see Liu 1999 for more details). Thermodynamics of equilibrium had been considered quite successfully reduced to statistical mechanics in that all thermodynamical phenomena in equilibrium are accounted for by the theory of statistical mechanics via a general reductional scheme that also reflects the compositional structures of thermo-systems. However, it was then realized that certain phenomena, such as phase transitions (a transition, for instance, of water from liquid to steam when boiling) and critical phenomena (e.g. opalescence), cannot be accounted for by the statistical mechanics of finite systems. Eventually, it was realized that one has to go the length of modeling the systems in which such phenomena occur as *infinite systems* before this 'hole' in the reductional scheme is plugged. When one expands to infinity -- with certain restrictions -- the size and number of particles of the system which is undergoing a phrase transition, what is said in thermodynamics about such a transition is rigorously reduced to what is said about it in statistical mechanics. Otherwise, a simple 'no-go' theorem proves the impossibility of such a reduction. This case is so similar to the case in Figure 1, it is almost uncanny. The stereographic chart from N, ϕ, is capable of covering the entire sphere except for the point N. One may be tempted to think that we may extend R^n to include the 'point of infinity' so that N can be mapped onto that point. After all, is it not true that two parallel line on a plane meets at the point of infinity?[2]

This case also presents an acute difficulty for the realists. Whenever, as in the above case, a reductional relation can only be rescued with certain maneuvres that appears to be purely formalistic, what should we believe about our world? Are the properties of phase transitions and critical phenomena as described by thermodynamics genuine properties or mere mathematical fluff? Either reduction fails -- so fails unity of science -- if they are mathematical fluff or it holds but we have to admit that some ordinary systems, such a kettle of boiling water, are systems of infinite physical dimension.

2. Shelving the question of whether physics -- or the fundamental science -- is itself unified is in fact not as harmless as it seems. If one thinks that science is unified as long as everything else is proven reducible to physics, while physics itself may comprise different theories for different phenomena, one is quite mistaken. It might be thought that if everything else in the world is made of particles and every other theory is reducible to some or all of the theories of their behavior, science is adequately unified even if those theories of the particles are not. However, such a picture of physics is too simple-minded, which does not do justice to what physics does and could do. Physics is not just a science for elementary particles and fields; it is also a science for ordinary objects, such as plants and people, and for collections or groups of such. If we have a unified theory of physics, we then may have a clear idea -- a set of criteria, perhaps -- of which properties of such objects or groups of them belong to physics and which to other special sciences, such as biology and psychology. But if we don't -- which means all kinds phenomena which are not currently included in physics may eventually be included -- then science may be unified quite trivially: work out all the reductions to physics that can be worked out among theories for different types of phenomena, and then take the remainder -- which includes the non-reducible theories or theory parts -- to be the domain of physics (or the fundamental science). Science is therefore unified. For instance, if there is a part of the mental whose theory is apparently not reducible to the science of physics as we now understand, we can just extend physics to include the theory. And if that is the only part in the whole enterprise of science that is not apparently reducible to the current physics, science is thus unified. It is only when we say that the physical must also be accounted for by a single theory, that such trivial cases of unification is ruled out. And this is why the drive within the community of physics for a unified theory is not a superfluous effort, (and also why I put [U2] in my scheme above). For what it means for physics to be unified and why it should be unified, see Erhard Scheibe's two-volume work on the 'Einheit der Physik' (Scheibe 1997/9).

In connection to the unification within physics, Morrison (1994) points out an interesting possibility that the theory seems unified while the things -- particles and/or fields -- are still 'disparate'. The actual unifications -- such as the unification of the electric and the weak interactions -- seem to give us nothing more than some formal or mathematical uniformities and/or similarities among the 'unified' entities; they do not tell us anything about their causal or substantival unity. It looks as if, to put this line of argument to its extreme, what I have dismissed in the beginning as one of the non-starters for unification, i.e. finding both physical and economical phenomena describable by Hamiltonian differential equations shows nothing about their unification, applies to physics as well. In sharp contrast, Einstein's original dream of reducing all natural forces to the 'geometry' of spacetime is a true unification in every ounce of its meaning.

Between the article of Oppenheim and Putnam and Causey's book, many discussions of the unity of science and, especially, of the possibility of reducing the mental or the social to the physical (which in principle affects the issue of unity of science) have appeared in the literature (some of which are cited and discussed in Causey's book), but none seems more significant than Fodor 1974, which threatens to shatter the dream of a reductional unification. It opened the floor gate for anti-reductionism arguments, especially regarding the reduction of biology (cf. Kitcher 1984, Rosenberg 1985, Kincaid 1990). One must acknowledge, after Fodor, that for any upper-level property in science, if it is multiply realizable at the level it is supposed to be reduced, reduction is seriously threatened[3]. With the realization that token-token identity does not imply type-type identity, one must see that metaphysical unity of the world no longer implies the unity of science via reduction. It might be true that all phenomena supervene on processes of the fundamental stuff of the world *and* that there exists a theory of that stuff that is capable of explaining the rest; but by these alone we cannot conclude that theories of upper-level phenomena will eventually be reduced to that theory.

It is important to note that Fodor's arguments are not necessarily against unification, but they are against *reductional* unification. Fodor alluded to his version of non-reductional physicalism as a revised or alternative version of unity of science (cf. Fodor 1974, 97ff) and Causey (1977, 142ff) -- calling it 'Tokenism' -- emphasized that it is such. The difference may be illustrated by an actual example in science. In the case of 'reducing' thermodynamics to statistical mechanics I discussed above, it is not only possible but almost certain that all thermo-phenomena in equilibrium -- including phase transitions and critical phenomena -- supervene on states of molecules, which are accounted for by statistical mechanics of finite systems. And yet thermodynamics as a upper-level theory of thermo-phenomena is not reducible to statistical mechanics of finite systems for the aforementioned reasons. Phase transitions and critical phenomena as described in thermodynamic terms are only reducible to statistical mechanics of infinite systems, which does not seem to be reduction at all.

The lesson from the geometric metaphor for Fodor's token version of unity of science, if the metaphor is apt, is simply this. Physicalism without reduction still does not imply the unity of science. (Therefore, it is perhaps too quick for Causey to grant Fodor's Tokenism as a genuine alternative conception of unity of science.) It is quite possible that everything else is token identical with the physical and yet the physical cannot be accounted for by a unified theory. Physics is not yet unified and may never be unified. Let me take some time to explain this point in connection with our geometric metaphor. The rationale for proposing reductive unification is that some things are obviously composed of other things as their parts. In the same way, the rationale for wanting to find a single theory that explain all fundamental physical processes is that they clearly show signs of being the instantiations of properties that belong to a single kind of stuff. But, again, if the metaphor is apt, we must say that even if they are such instantiations, there is no guarantee that a single theory exists to account for all of them. The fact that all points belong to a single geometric object -- all topological spaces are connected -- and each point and its neighborhood can be mapped with a single chart onto R^n evidently does not guarantee that the whole object can be mapped onto R^n with a global chart. Similarly, the inference from everything is physical and each token physical even is nothing but a set of happenings at certain spatial and temporal locations, which seems to have already a definite description in physics, to there exists a globally unified science of physics is not valid. Whether physicalism, even if it is true, leads to the unity of science still depends on the 'contour' of the physical, not just on the describability of parts of it.

Furthermore, even if the physical is accounted for by a unified theory -- i.e. we have physicalism with the possibility of a single theory for everything, it may still be a long way from having the grand unification as a goal of science. Unlike a reductionist scheme, where the actual local higher-level explanations can still be done by the special sciences -- knowing that such explanations can in principle be delivered from the bottom, the physicalist theory which has to reach higher-levels token by token for each actual explanation may have no practical plausibility. It may well take the whole machinery of science an unreasonable amount of time and resources to explain a 'simple' higher-level phenomenon, so that the theory is virtually of no use. One of the lessons from Fodor's article is that even if science is already proven unifiable via the 'token route', it would not help at all, because it would still be much better to go about one's business with the special sciences -- i.e. to explain phenomena of different levels with theories at, or close to, those levels. In this sense, Quine (1969) is probably wrong in arguing that as science develops, more and more natural kind terms -- we may substitute 'natural kind terms' with 'kind terms suitable and used by special sciences' -- will be rendered useless and given up and replaced by explanations of a more homogeneous kind, such as by an account of the structures and functions of the ultimate constituents.

The geometric metaphor may also be useful in analyzing other proposals for the unity or the disunity of science. At the same year when Causey's book appeared in print, two articles, one by N. Maull (1977) and the other by L. Darden and N. Maull (1977), came out, proposing a notion of unification 'without reduction', namely, unification via what they call 'interfield theories'. The idea is roughly this: between any two levels of phenomena, there are always phenomena which do not belong to either. The theory at each level however does reach down (or up) into them and can provide descriptions or explanations which, though compatible, are not the same. In such situations, an interfield theory -- they regard levels of special sciences as consisting of 'fields' rather than 'theories'

-- is usually required and constructable. These interfield theories, rather than reductions, are the cement that join different fields of science and make it a unified whole.

Suppose, now, that we have two overlapping open subsets on a manifold, each of which has its own chart. Since the manifold is covered by an atlas, the two charts are compatible at the overlapping regions. This is analogous to the claim that in science all theories among traditional levels are at least consistent -- i.e. if two claims from theories at two distinct levels are made about the same phenomenon at an overlapping region between levels, they should not contradict each other. None of the charts may well coordinate the overlapping region, while a third chart exists which best coordinates it. If the manifold is not singlely chartable, it is quite possible that none of the three charts are replaceable by another. This, I believe, is an exact analogy of the situation that Darden and Maull considered. So, the kind of unification they propose is a unification of an 'atlas'. Note that their 'unity by interfield-theory connection without reduction' is analogous to a complete covering by an atlas without the possibility of a global chart.

From our previous discussion, I believe that such a unification of science is a unification in name only. No one can prevent me from calling an atlas of a manifold a 'global chart', but .... Moreover, an unpleasant possibility appears for this notion of unity via interfield theories. Given reductionism or the physicalist unity via the 'token route', there is no need to be concerned about unification within a level of phenomena -- except at the fundamental level. Even if biology or psychology is not unified within itself, science can still be unified when every piece in biology and psychology is reduced to a unified theory of physics (if reductionism is true) or when they can be discarded (if the token route works). But if the unity must be achieve by linking up the peripherals of each level of phenomena, it is no good if pieces inside some level are not 'connected' -- i.e. unified in some sense. Whatever a unified science may turn out to be, a single piece of rag would not do.

The most recent advocates of the disunity of science, here I mention Dupré and Cartwright among others, are champions of metaphysical pluralism. Since their target is actually much broader than unificationism -- fundamentalism (or foundationalism) (Cartwright 1999) and imperialism (Dupré 1994), *inter alia*, are also within the range of their aim, I can only lightly touch on the parts to which our metaphor may be of some use. Dupré (1983, 1993) explicitly complained about the ineffectiveness of many previous anti-reductionist arguments because they only argue for epistemic or pragmatic impossibility of unification. For Dupré, if reality is metaphysically disordered or pluralistic, those arguments become superfluous. Here the geometric metaphor is useful again: if a manifold turns out to be one that is not coverable by a global chart, whatever other arguments concerning the power (or the lack of it) of some type of mapping functions would be superfluous. Many people, including the ones whose works we have so far examined, believe that it is ultimately an empirical question as to whether reality can be captured by a unified science, which is fundamentally different from the case in geometry, where the possibility of a global chart is determined *a priori*. Dupré, if I understood him correctly, apparently thinks otherwise. Metaphysical pluralism is certainly not a thesis to be verified or falsified by empirical investigations. There are good reasons to think that if knowability of the world is a matter of metaphysics -- how knowledge of any kind is possible -- it is also a matter of metaphysics whether the world is knowable ultimately through a unified theory. Of course I do not suggest that there is any direct analogy between geometrically determining whether a manifold is coverable by a global chart and metaphysically determining whether the phenomenal world is describable and explainable by a single unified theory.

Also relevant is the difference between a non-coverable space -- in the sense of no cover by atlas -- and a coverable space which is not coverable by a global chart. Dupré does not always seem to be clear which counterparts of these alternatives he is arguing for. A metaphysically plural world could be either, but his arguments against Determinism --

which move towards an image of the world as causally incomplete -- seems to indicate that he is inclined towards the former -- the analogue of an uncoverable space.

For Cartwright (1994, 1999), whatever resembles a global chart for a manifold is out of the question for science. She clearly supports anti-reductionism and equal rights for special sciences, and her arguments mostly derive from a deep appreciation of the central role of models in science. From the apparently impossibility of constructing a mechanical -- more accurately, a compositional-mechanical model -- for a thousand dollar bills blown apart by a gust of wind, she concludes that mechanical laws do not apply to the situation; but since a hydrodynamic model is still possible for it, the laws of hydrodynamics may apply. From that, she infers that hydrodynamics is not reducible to mechanics -- more accurately, it is not micro-reducible to the mechanics of the constituents of the hydrodynamic systems in question. (Note, there is no metaphysical question about whether the system of the wind-blown bills is composed of a thousand dollar bills.) The same move also seems to block a unification via the token route, for according to Cartwright, no models, no laws, and hence no explanations by laws. A more urgent question is whether Cartwright, like Dupré, also does not care for the analogue of an atlas for science. Sometimes, her image of Nature -- '[who] has a rich, and diverse, tolerant imagination (1994, 361)' -- seems to indicate that Nature resembles more of a space not coverable by an atlas.

However, Cartwright has a move, especially in her discussion of the relationship between classical and quantum mechanics, that seems to deprive the geometric metaphor of its relevance. Whenever there seems to be an inconsistency in assigning a quantum and a classical state to the same physical state of a system, given we are fully justified in doing so, we can deny that they are about the same state, and the contradiction disappears. 'There are both quantum and classical states and the same system can have both without contradiction. (Cartwright 1994, 362)' There is no analogue of this in our case of coordinating a space: if there are two open subsets on a manifold whose charts are not compatible at the overlapping regions, we cannot appeal to any relevant differences of the two subsets to make the charts compatible again. There must be tough cases in which making such a move may cause difficulties. For instance, what if according to a 'true' classical description the age of the universe is A, and according to a 'true' quantum description it is B, and A ≠ B. Whether the answer is positive or negative determines whether or not Cartwright subscribes to the analogue of an atlas in science.

5 Conclusion

If the geometric metaphor is adequate, physicalism does not imply the possibility of unification; nor does it imply reductionism. For the latter one does not need the metaphor (because of Fodor 1974) but for the former one does. If the world is unifiable, the geometric metaphor further tells us the following. Metaphysically speaking, there are two possible alternatives for unifying science: one is to have a horizontally unified fundamental theory -- e.g. physics -- and then to have every other phenomenon explained token-by-token; the other is to have special sciences for different levels of phenomena and achieve the unity by complete reductions to the (self-unified) theory at the fundamental level. The latter implies the former but not vice versa. However, epistemically speaking, the former is almost impossible while the latter is not. Unless our current conception of physics of elementary particles and fields are fundamentally mistaken, it is almost ridiculous to think of a direct explanation of, say, French Revolution, by laws and initial/boundary conditions of the elementary particles and fields involved.

But on the other hand, what constitutes a break-down of reduction? The toughest kind of questions that philosophers have to face regarding judgments on reduction is the kind instanced in the reduction of thermodynamics to statistical mechanics that we examined earlier. When in the end mathematical physicists proved that thermodynamics is only reducible to statistical mechanics of infinite systems, what should we say? *Ceteris paribus*, is reality unifiable or not? In our geometric case, if one can find a systematic way of extending the map of a manifold to its pathological points or regions, there seems to be no reason why the extended map cannot be taken as a global chart for the manifold. The same, *mutatis mutandis*, seems applicable to the scientific cases, at least to the case in question, where finite statistico-mechanical systems (and laws and types of conditions that go with them) are extended to infinite such systems so that the whole realm of thermo-phenomena, at least, is adequately 'covered' -- via reduction -- by statistical mechanics. But if no such extension is available, we probably should call it quits. For a fascinating discussion on how techniques of taking thermodynamic systems to infinity suggests a solution to the problem of multiple realizability, see Batterman 2000.

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[1] The actual proof that $\phi \circ \psi^{-1}$ and $\psi \circ \phi^{-1}$ are infinitely differentiable goes something as follows. First we inbed the stereographic maps into the higher dimensional space so that a normal Cartesian coordinate

system can be given to them. Then we can prove that ϕ and ψ and their inverses as functions in such Cartesian coordinates are infinitely differentiable functions. For an example of such proofs, see Isham 1989, 3-4.

[2] The difficulty of such extension is that the result is underdetermined. Should the N in Figure 1 be mapped by the extended ϕ to +∞ or -∞, since both 'points' equally qualify, but not both together (for then ϕ is no longer a function). We may choose one of the two, but on what grounds?

[3] One may be tempted to say that reduction then fails, but this may be too strong, see Batterman 2000.