

# Future development of scientific structures closer to experiments: Response to F.A. Muller

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**Abstract** First of all, I agree with much of what F.A. Muller (Synthese, this issue, 2009) says in his article ‘Reflections on the revolution in Stanford’. And where I differ, the difference is on the decision of what direction of further development represents the best choice for the philosophy of science. I list my remarks as a sequence of topics.

**Keywords** Scientific structures · Measurement errors · Invariance · Computation · Ergodic

## 1 Actual beings

F.A. Muller (2009) is, I think, almost correct when he says in Sect. 4 of his article that the Informal Structural View (my view) says nothing about actual beings. Long ago, in my 1970 monograph, *A probabilistic theory of causality*, I noted that for a standard probability space, it was trivial to show by Padoa’s principle that we could not define the concept of an actual event in terms of Kalmogorov’s concepts and axioms. So I introduced an occurrence function  $\theta$ . Here is what I said:

It is obvious that the concept of the occurrence of an event is formally similar to the concept of a proposition’s being true. The four axioms given below assume the algebra of events as given in the usual set-theoretical fashion. The new additional concept of occurrence is expressed by a one-place predicate  $\theta$ . From the four axioms we can derive Huntington’s five axioms (1934) for formalizing the “informal” part of Whitehead and Russell’s *Principia mathematica* (1925).

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The predicate ‘ $\theta$ ’ corresponds to his predicate ‘ $C$ ’, where  $C(x)$  is interpreted to mean that proposition  $x$  is true.

### *Axioms of Occurrence*

Axiom 1. If  $\theta A$  then  $\theta(A \cup B)$ .

Axiom 2. If  $\theta A$  and  $\theta B$  then  $\theta(A \cap B)$

Axiom 3.  $\theta A$  or  $\theta \bar{A}$

Axiom 4. If  $\theta A$  then it is not the case  $\theta \bar{A}$ .

I shall only prove two theorems about these axioms.

**Theorem 1** *The Axioms of Occurrence imply Huntington’s five axioms.*

**Theorem 2** *The concept of occurrence satisfying Axioms 1–4 above is not definable in terms of standard probability concepts. Moreover, it is not definable in terms of causal concepts set forth in Definitions 1–8.”*

(Suppes 1970, pp. 37–39)

I am not claiming that these axioms are enough to deal satisfactorily with the problem of characterizing actual beings. The actual events I am referring to are also in some sense abstract. In fact, without trying to go deeply into the matter here, I think that much of our experience with actual objects and processes is indescribable in its full concreteness. Any description, in informal or formal language, is some kind of abstraction. Most of our ordinary talk, not just scientific talk, is saturated with concepts that always abstract from the full particularity of a given experience. This is why demonstratives are so necessary, and are available, in every natural language. This is an old problem in philosophy that I shall not consider further here.

## 2 Theory-talk vs. experiment-talk

In Sect. 6 Muller is correct that my view is not the semantic view of theories. My criticism of van Fraassen’s elegant coordinate-free or language-free view of theories is that this just reaches a further level of abstraction, far removed from what is going on in actual experiments. What is striking about the quote from him Muller gives, and also his own following remarks, is that they remain very much at the level of what I call theory-talk, not talk about experiments. In the other direction, with the emphasis on language of some sort, the semantic view moves away from the approach to representation and invariance of pure mathematics (I comment on this again later).

The most characteristic feature of experiment-talk, I would say, is that it is almost entirely removed from talk about models of any kind. The focus is on how things were done in a given experiment. Such talk varies widely from one subdiscipline of science to another. There is not a hint of any of this in van Fraassen. It is as if science were only about theories. In recent years I have written about experiments and how dependant they are on students and postdocs learning how to do them through apprenticeships. What is important here is that much of the instruction is nonverbal. You can never learn how to run a particle accelerator, or, for that matter, play tennis or soccer, by

listening to a lecture. Much nonverbal learning is required, but this essential aspect of science seems to be neglected by many, including both Muller and van Fraassen.

I think it is worth amplifying some of the differences between theoretical and experimental developments by looking at some problems and examples from the history of science. A good initial focus is the special theory of relativity. Einstein's original 1905 paper on special relativity is a wonderful example of "shock and awe" in science. It heralded one of the great scientific revolutions of modern science. On the other hand, from an experimental standpoint, no really new experimental procedures or new kinds of measurement were required to conduct the Michelson-Morley and other relevant experiments, although such procedures were changing, as they usually are. At that period of time in physics, there were no striking changes of methods of measurement associated with the confirmation of the theory. Michelson and Morley just used the kinds of interference measurements that had been thoroughly worked out in the 19th-century development of experimental physics. In fact, this was the first century of real experimental methods in physics. Such continuity of experimental procedures, including the main procedures of measurement, is a familiar fact about many important transitions in physics.

To take another example, there were no special new observational or experimental techniques directly associated with the Watson and Crick discovery of the double helix of DNA. They analyzed data from Rosalind Franklin and Maurice Wilkins. The X-ray crystallography that was used had been developed for some time, by von Laue, the Braggs, and later, for biological molecules, by Dorothy Crowfoot Hodgkin. A cascade of new experimental methods did follow the double-helix discovery.

Another kind of revolution takes place in a given area of science when a radically new kind of instrumentation is introduced. Classic examples are the telescope and, as another visual device going in the opposite direction, the microscope. With these new instruments, new discoveries were made that had a strong impact on subsequent theoretical developments, in the one case, in astronomy, and in the other, in biology. Certainly the most expensive experiments yet conceived in the history of science are the ones that are aimed at being the only approach practically available to settle such theoretical questions as the existence of the Higgs particle. The cost of the CERN accelerator sets the modern record, but the prior particle accelerators, including the long-serving Linear Accelerator Center at my home university, have also been extravagantly expensive by normal scientific standards. Yet without this new instrumentation, none of the new particles of modern physics would have been discovered, at least, not within the framework of earlier instrumentation. It is perhaps the best example of new physics being stymied without new methods of instrumentation.

Still another tale can be told to emphasize that too much stress just on scientific theory is out of place. Think of cases where new mathematical methods were important to progress. In ancient astronomy, the observational methods of the Babylonians, especially from the 8th to the 1st century BCE, were in many ways superior to those of the Greeks, and the methods of calculation were about as good. But in terms of what we now know, the great difference between Babylonian and Greek astronomy of ancient times lies in the sophistication of the mathematical developments of the Greek geometrical ideas, including spherical trigonometry, as opposed to the arithmetical-numerical methods and ideas of the Babylonians. As is well documented in

Jens Hoyrup's (2002) book, the Babylonians had a rich computational and algebraic tradition much earlier than the Greeks, but by modern standards it was not comparable to the Greek development of geometry with extensive application to astronomy.

Moreover, it seems extremely unlikely that we will find anything in the as yet large number of untranslated clay tablets of Babylon comparable to the *Almagest* of Ptolemy, which is a massive piece of mathematical and observational astronomy that in style and substance reads very much like a modern textbook of mathematical physics or astronomy, written with greater than usual attention to mathematical rigor. To give a quite concrete example, there is nothing as yet discovered in Babylonian astronomy comparable to Menelaus' theorem (about 100 CE) on the trigonometry of the right spherical triangle. Such spherical trigonometric results were used by Ptolemy extensively in the *Almagest*. But this development of spherical trigonometry did not begin with Menelaus, but rather at least as early as Autolycus of Pitane, about 300 BCE. Much of the well-known Euclidean work *Phenomena* is based on Autolycus. Another work is the *Sphaerica* of Theodosius, written about 100 BCE. Menelaus' work was particularly powerful and useful in astronomy, and was much read and used subsequently.

Still another aspect of the relation between mathematics and science is that Ptolemy's extensive use of Menelaus' results required application of only the elementary parts. This seems to be a generalization that holds rather widely in the use of mathematics in physics. The more elementary parts turn out to be the parts that are really useful and pragmatically important, or at least are the ones that the physicists or astronomers take the time to learn. The tale I have been telling has more extreme versions in many parts of modern science. Scientists often have a rather poor command of both the mathematics and instrumentation used. A good example is the use of functional magnetic resonance imaging (fMRI) in contemporary neuroscience. The nuclear magnetic theory on which it is based was only discovered in physics in the 1940s, for which Felix Bloch and Edward Purcell got the Nobel prize. Its extensive application in fMRI is understood, in any serious detail, by an extraordinarily small number of neuroscientists, relative to the number that are using it for experimental work. This is not meant as a criticism of neuroscientists, but a comment on the complicated and highly specialized nature of modern science.

I should emphasize this last point more in terms of the philosophy of science itself. In this age of vast and necessary scientific specialization, it is impossible to enter into all the details in every direction in discussing recent scientific work, the ancient history of science, or the progress expected in the future. One can only be selective and hit-and-miss when trying to give a broad overview. My criticisms are based, not upon the absolute necessity for restricting the range of what is discussed, but on the point that too much emphasis can be placed just on scientific theory itself, whereas often it is at least as important to discuss experimental methods and how they have influenced the progress of science on the one side, and the developments of mathematics on the other. The glory of Ptolemy is his elaborate use of theory, observation, and mathematics. In this respect, it is easier to give a more thorough discussion of the older history of science. So, for example, there is a sense that in the future it will be recognized that the history of ancient astronomy, in spite of the restricted records, will be in many ways more complete than the history of modern science, particularly astrophysics and

cosmology, just because of the overwhelming wealth of data and the vast range of methodologies used in quantitative observations that in their full detail are theoretically immeasurable. And these matters are not restricted to astronomy and physics. They will also be characteristic in the future of biology, if they are not so already. In this respect I suppose my final point is to emphasize that, if we want anything like a realistic story of any of the major developments of science now and in the future, it will not be a simple and tidy one, theoretical or otherwise, but something of great complexity and nuance, more like a great novel than an admirably lucid textbook.

### 3 Invariance

I agree with Muller's further development of the Structural View in Sect. 6 of his article. It is an attractive program for some philosophers to follow. But I do want to make a remark about invariance and how it is treated as a central concept in modern mathematics. Representation and invariance theorems are proved in almost all parts of pure mathematics without reference to language or linguistic invariance. An invariance theorem is stated about mathematical objects with no explicit reference to the language describing these objects. This is the view of pure mathematics I carried over to scientific structures in my 2002 book *Representation and invariance of scientific structures*. I quite agree that this book of mine, as embodying my systematic views of scientific theories, does not deal at all with the problem of how to talk about actual beings or even experiments, but I have been under no illusion that it does. I say the same thing is true of Muller's extension of this program, but I am sure he is aware of this.

There is a problem about invariance that I now recognize more thoroughly than I did when writing the 2002 book. This is the tension between computation and invariance. It is sometimes asked, "Why don't physicists write their axioms and their formulation of physical laws in an invariant form instead of a covariant one?" Or a more drastic question: "Why are computations not done in computers in an invariant style, rather than satisfying some arbitrarily chosen basis, for example, the decimal base 10 (the customary one taught in school) or base 2?" I momentarily postpone my answer to note that it is already difficult to write the laws of physics in an invariant way. A general response might be, "Well, don't we already have a good example of invariance in the way synthetic geometry is axiomatized?" And the answer is "yes", but it is also a very good instance of computational power being weak. Computing much of anything in the notation of Euclid, or a modern axiomatization of Euclidean geometry in synthetic form, is nothing but a mess. This was well recognized long ago, and it was the great glory of Descartes to have discovered the computational power of analytical geometry. (None of the earlier anticipation by Apollonius and others went very far.) Physicists above all are opportunistic about notation. A transformation to a coordinate system that brings an important quantity of measurement, for example, velocity, to zero, is a good thing, because it simplifies the computations. In fact the effort to simplify computations tends to move in the opposite direction from invariance.

Physicists like to put computation in its place by proclaiming the "axiom" that all computations are physical computations. This axiom seems to support the further

inference that computations require a particular representation to have a presence, for example, in the physical processing of a digital computer. But many mathematicians who dislike the tedious matrix and determinant calculations, which were drastically reduced in modern linear algebra, would seem to prefer the heuristic, “Don’t compute until you have to, stay with invariants as long as you can.” There is merit in both views. I introduce these clashing points of discord to note the tension between two of the most important concepts in modern science.

To put the matter more positively, these remarks about computation do not reduce the value of the concept of invariance in mathematics, physics, and other parts of science. For example, in the case of special relativity, one’s insight into the nature of the theory is, it seems to me, enhanced by knowing that the simple computation of the proper time between two point events is an invariant, and in fact, a complete invariant for special relativity. I mean by ‘complete’ that from only the assumption of this invariant for measurement frames of reference, one can derive the Lorentz transformations as the widest group preserving this quantity (Suppes 1959).

But it is also clear that knowing about this invariant does not mean that you want to use it for the basic formulation of axioms of relativistic mechanics, for example. This can be seen from an axiomatic analysis of the latter in Rubin and Suppes 1954. Also, in all kinds of structures in the theory of measurement, the representation theorems are proved in terms of representations that are not invariant, for example, the measurement of length, velocity, weight, any of the many kinds of quantities one can think of, or utility in psychology and economics. Of course, it is natural to then prove an invariance theorem, which is a theorem giving the widest group of transformations under which the properties of the quantity in question can be preserved. There is a great historical example that goes in the opposite direction. It is a virtue of the ratios of Euclid, used effectively all the way through to Newton’s *Principia*, to formulate relations in terms which avoid the problems of arbitrary units of measurement, but in doing so, complicate the calculations.

I now turn to Muller’s final section, Sect. 7 on completion. My own program for completion is of a very different character. What I see as important and much more fruitful as a direction for the Informal Structural View is to make the theoretical structures of science more closely match experimental data, and perhaps even more, experimental procedures. The rest of what I have to say is devoted to giving some extended examples of this program under four headings: measurement, computation, ergodic theory of observational error, and constructive mathematical foundations.

## 4 Measurement

In the theory of measurement, the program is to eliminate the classical structures that have an infinite domain and thereby match closely the structures of pure mathematics. The task remains to provide structures that are formally defined, but are much closer to actual methods of measurement. A recent paper of mine, ‘Transitive indistinguishability and approximate measurement with standard finite ratio-scale representations,’ exemplifies this approach (Suppes 2006). A representation theorem is proved in terms

of upper and lower measures to reflect the pre-statistical indeterminacy of an actual scale.

The psychological consideration of thresholds below which perceptual or other comparative judgments are difficult, if not impossible, was initiated by [Fechner \(1860/1966\)](#). An important early mathematical analysis was given by [Wiener \(1921\)](#). The probabilistic analysis of thresholds dates at least from works of [Thurstone \(1927a,b\)](#). [Falmagne \(1976, 1978\)](#) has also been a central contributor to this approach, with a number of other papers written with colleagues. Almost all of the work I have referred to assumes that the indistinguishability of similar events, objects, or stimuli is a nontransitive relation. The implicit assumption is that with many different discriminating observations, many initially indistinguishable events may be separated. Here the opposite is the starting point and the reason for the use of the word “transitive” in the title. It is a consequence of the axioms I introduce that indistinguishability is an equivalence relation, and so, transitive.

The basis for transitive indistinguishability is easy to explain. An object weighed is assigned to a unique minimal interval, for example, one between 1.9 and 2.0 g. The binary relation of two objects,  $a$  and  $b$ , being equivalent in weight,  $a \sim b$ , is that they be assigned to the same minimal interval. This relation is obviously an equivalence relation, i.e., reflexive, symmetric, and transitive, but in the system of approximation developed, these properties are not directly testable, but rather consequences of weighing operations with already “calibrated” sets of weights.

An object assigned to the minimal interval (1.9–2.0 g) is said to have, as an approximation, an upper measure (of weight)  $w^*(a) = 2.0$  g and a lower measure  $w_*(a) = 1.9$  g. In practice, for all but the most refined procedures of measurement, no statistical analysis of having weight in such a minimal interval is given. In the cases when the minimal interval is just on the borderline of instrument performance, a statistical analysis can be given for repeated measurements. It is conceptually important here to retain both upper and lower measures, for the foundational view formalized in the axioms is that no finer measurement than that of a minimal interval is available in the given circumstances. No theoretical construction of a probability distribution for location within the minimal interval makes much scientific sense. The point being emphasized is that the formalization given is meant to be a step closer to much, but certainly not all, actual practice of measurement when a fixed standard scale representation is available.

## 5 Computations

The same approach should be applied to the computational side of science, especially now that so much of current science is nonlinear and equations cannot be solved in closed form, but only numerically approximated. As an example, the classical axioms of arithmetic need to be replaced for computational purposes by axioms of floating-point arithmetic. The point is to show that with full rigor we can match new constructive foundations to what is going on computationally.

In fact, it is quite a complex and subtle matter to give satisfactory axioms for floating-point computations. It can even be argued that there is no completely satisfactory set of axioms available at the present time. In this discussion, I would just like to hit a

few of the high points, to indicate why it is useful but often difficult to implement the kind of foundational program that gets closer to the actual practice of computation.

There is something much simpler that is widely taught already in elementary-school mathematics, namely, standard scientific notation. So, if we know a number, say, to 10 significant digits, and the units are, for example, miles, and the distance is the distance from Earth to some other planet at a given moment, to express this in standard scientific notation, we first write the first digit as a number strictly between 0 and 10, for example 1.567897. Then we have as the second part the expression of power as a power of ten. So to express something simple, like a debt of \$1,600,000, we would write that debt as  $\$1.6 \times 10^6$ . Well, this is simple enough, and it does not have any implications that go against the standard axioms of arithmetic. Floating-point and actual extensive computations are another matter. Here, the scheme must be not only finite, but bounded. We cannot compute, in any actual computer, finite numbers that are too large. It is very easy to write down two numbers that we would like to multiply, which cannot be multiplied in any existing computer, and if we take what some physicists claim seriously, we can even assert that, in principle, such multiplication could never occur, in any computer in our universe, in the amount of time, for example, that the Earth has already existed.

So the introduction of a finite bound in floating-point arithmetic is the real problem. In the ordinary mathematical system, we have the neat and clean closure of operations saying that any two numbers can be added to obtain another number, any two can be so multiplied, and with a restriction on division by zero, we say the same of division. There is no restriction in the case of subtraction if we are willing to introduce negative numbers, which we always do, of course. So with the full range of real numbers, the operations of addition, multiplication, division, and subtraction are closed, meaning we can perform these operations on any two numbers, and we obtain another number. Division by zero presents special problems I will ignore here, but which much can be said about. In computations that are limited finitely, something more complicated has to go on. It is these complications that produce all the difficulty for floating-point arithmetic.

Historically, there was not much attention paid to the problems of floating-point arithmetic; it all began with the serious development of computers. But then it was evident immediately that something of a highly specific and technical nature was needed in order to have proper control of computation. Otherwise, results that would be difficult to interpret could easily be produced. So, starting around the middle of the 20th century, much attention has been paid to this problem, and the literature is now extensive. The point of emphasis here is not the extensive character of this literature, but rather the lack of almost any developed foundational literature on the subject. There are some axiomatizations, and at least one or two dissertations written on the matter in modern computer science, but compared to the kind of foundational attention that has been devoted to a great variety of problems in elementary number theory, little exists. It is part of the program I advocate to make this a foundational issue, and to discuss in the constructive spirit, which I think should be applied to mathematics as used in science, the idea of having finitistic constraints on floating point to match the necessary constraints on the measurements and computations actually used in scientific practice.

## 6 Ergodic theory of observational error

The next big step is to use the ergodic theory of chaos to get general results about errors. The philosophical importance of these results has not, in my view, been much appreciated by philosophers of science. Roughly speaking, they go like this. Let us recognize that all, or at least almost all, measurements of continuous quantities have errors bounded away from zero. This is contrary to the mistaken idealism of 19th-century philosophy of physics that errors could be reduced ever closer to zero in a continuing sequence of improvements. Given such errors, we also recognize as impossible the exact confirmation of continuous deterministic physical systems. I am not saying this in a precise way; I think the intuition is clear enough. In any case, the details are clarified by the remarkable results of [Ornstein and Weiss \(1991\)](#) about a variety of chaotic systems, including as perhaps the most vivid one, “Sinai billiards,” which means the following. A convex obstacle is added to the center of the table, which the billiard ball rebounds off of, according to the same symmetrical laws of physics that govern rebounds off the sides of the table. Sinai proved that, for such billiards, one has an ergodic measure, i.e. over a long enough time the ball will pass arbitrarily close to all points on the surface of the table, except for a set of measure zero. What is important is that Ornstein and Weiss also add a concept of  $\alpha$ -congruence. Such congruence is of the following sort. Two models, for example, one deterministic and the other stochastic, can be defined for Sinai billiards, such that they are not only measure-theoretic isomorphic but are  $\alpha$ -congruent, i.e., all distance measurements for the same pairs of points are within  $\alpha$  (the bound on error) except for a set of points of measure  $\alpha$ . Take as the deterministic model the simple classical physics one for idealized billiards with no loss of energy, but with errors of measurement as noted above, and let the second be a stochastic model which is a Markov process, suitably defined to be provably  $\alpha$ -congruent to the deterministic one. The fundamental theorem is that, no matter how many observations are made, as long as the number is finite, we cannot distinguish between the goodness of fit of the deterministic model and the stochastic one. This establishes a new kind of invariance that pushes hard against many conventional doctrines of long standing in the philosophy of science. Note that the two models are mathematically inconsistent. But empirically one cannot distinguish between the two.

These ideas about error are not just methodological ones, but important limitations on our knowledge of the world. They constitute one kind of excellent answer to Kant’s conflicts of the antinomy of pure reason. Consider, for example, the second conflict, concerning whether matter is discrete or continuous. It is obvious how what I have just said could apply to this antinomy. The same thing can be said about the third conflict on causality. I have written about these matters in an informal way in several places ([Suppes and Chuaqui 1993](#); [Suppes 1995, 1999](#); [Suppes and de Barros 1996](#)). I am sorry to say that I did not include these topics in my 2002 book.

## 7 Constructive mathematical foundations

Finally, the implementation of the program, in terms of the first three steps I have outlined, suggests a fourth one to make the picture fully finitistic in spirit. This is to

provide a computational foundation of analysis that uses the same ideas about the nature of errors. Here the basic move is to have a non-standard constructive form of analysis. By *non-standard* I mean that we have real infinitesimals, and by *constructive*, I have in mind a very strong form, namely that the axioms are quantifier-free, so that it is a free-variable system. Such a system is of course much weaker mathematically than classical analysis, but my claim is that once the presence of errors that I have been talking about is taken seriously, as a foundational necessity, then everything that one can in fact do that is significant scientifically can be done within such a restricted, constructive mathematical system. Now of course I may be wrong in saying that *everything* can be done. The point of the program, which is a very concrete one, is to see how far one can go and moreover, where there are counterexamples requiring stronger methods to lead to new scientific results that can not be formulated in such a framework, if there are any.

Over the past fifteen years, I have written with two collaborators several long articles on constructive, non-standard foundations of analysis (Chuaqui and Suppes 1990, 1995; Sommer and Suppes 1996, 1997; Suppes and Chuaqui 1993). I remark that absorbing the theory of error in a sharp way into these foundations has been the solution to giving a much more robust and simplified system that in some sense matches very well almost everything that is actually done in scientific practice mathematically, with some possible exceptions here and there.

Without giving a detailed description of the system, it is still possible to convey a sense of its main features, especially those of some philosophical interest. I list three.

1. In Suppes and Chuaqui (1993), we introduced the concept of a *geometric subdivision* of a closed interval  $[a, b]$  of order  $v$ , where  $a$  and  $b$  are finite real numbers and  $v$  is an infinite natural number. We used this concept to define the differential  $du = (b - a)/v$  and  $u_i = a + u_i du$ , for  $0 \leq i \leq v$ . So the  $u_i$  form a partition of  $[a, b]$ , and thereby mark the divisions of the geometric subdivisions depending on  $a$ ,  $b$ , and  $v$ . This concept was anticipated in the work of Cavalieri (1635), a student of Galileo's, on the geometry of indivisibles.

All of this has complications that are evident in the article mentioned. More generally, use of the infinitesimals of modern nonstandard analysis is a natural computation approach, but brings with it more subtle complexity than initially expected, as the details are worked out. In the system I am now developing with Ted Alper we simplify, and thereby eliminate, much of this onerous detail, by using a strictly finite approach as the basic model. We replace the geometric subdivision by an extremely fine but finite equally spaced grid, with the spacing many orders of magnitude smaller than any current physical constants or limitations on measurement.

2. The second feature concerns the very large. If the universe is of fixed finite size, but not too large, we may be able to measure it. On the other hand, if it is finite but very large, or infinite, there may be no way to empirically distinguish the hypothesis of being very large but finite from that of being infinite.

To make this idea more concrete, consider how an approximately spherical universe with a diameter greater than  $1000^{1000^{1000}}$  kilometers could be distinguished from a flat infinite universe. So the second heuristic principle Alper and I are

using is the indistinguishability in empirical quantitative terms of a very large finite space from an infinite one.

3. The third heuristic principle is that our finite models, with a very small physical distance corresponding to an infinitesimal, should be the basis of establishing, as a weak form of isomorphism, an indistinguishable reflexive and symmetric relation between our very large finite models and standard models using classified analysis, as applied to quantitative empirical tests or simulations of empirical scientific results in any domain of science, but especially physics.

Of course, this program is quite different from the completion Muller has outlined. It does not mean one of us is right and the other wrong; it just means there are fundamentally different ways to extend the Informal Structural View to more details of scientific interest. My own attitude, starting with Suppes (1962), is to go as deeply as possible into the actual practices of science at the level of measurement, observation, and computation, and how they should be reflected back into theory when the limitations imposed by errors or environmental variations are taken seriously. I also look upon this approach to error as really the proper answer to Kant's antinomies. As Kant himself hints in various passages, the difficulty, as in the case of Russell's paradox for Frege's system, is the unbounded or unconditioned character of the results aimed at. It is conditioning or restricting that is necessary to have a consistent theory of nature. If unrestricted and unbounded operations are permitted, then we get some form of antinomies, even in pure mathematics. This was the kind of thing that was wrong with Frege's system, and also leads to the standard paradoxes of the same sort in axiomatic set theory.

As a final point, the program of constructive mathematical foundations outlined above also challenges the pure a priori synthetic status of arithmetic and geometry at the heart of Kant's and much later work in the foundations of mathematics. Actually, the challenge here is not so much about the a priori, but whether arithmetic and geometry in their classical axiomatizations are scientifically the correct choice. The question is not according to the often mentioned slogan about the certainty of pure geometry versus the uncertainty of empirical geometry, but rather what axiomatization is best for each major scientific discipline. New proposals for both arithmetic and geometry are part of the program I have outlined here. The constructive thrust is not to limit available mathematical methods, as such, but rather to build mathematical structures that more closely match scientific practice.

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