Her comments on my work cover a rich variety of topics. I won’t try to comment on everything she has to say, but she has focused on a number of issues that I have also thought about and think are important. Broadly speaking we do agree, I believe, about the role of models in the philosophy of science and also in various parts of science as well. I have several comments, so I will highlight each topic as I move from one to another.

Models of data. Schiaffonati mentions early in her discussion of models, the special topic of models of data a subject dear to my heart as reflected in my early publication on this topic (Suppes [1962]). I did not say as much about models of data as much as I would have liked in my recent book [2002], *Representation and Invariance of Scientific Structures*, even though this was part of my original plan. But what I have to say about models of data would have added too much. I say this to emphasize that my conviction about the importance of the explicit consideration of models of data has not waned. As I have remarked on several occasions in the past, those parts of mathematical statistics concerned with the consideration of data provide a rich assortment of models, for the language of modern mathematical statistics is entirely set theoretical in nature but statisticians do not really emphasize this, and philosophers of
science have neglected it more than they should. Of the most significance are the large steps of abstraction in going from the intricate details of experiments, or even of observational collection of data, to the restricted and formal representation of data used for testing scientific hypotheses in statistical form or estimating important physical parameters. It is a fact, not sufficiently emphasized in many quarters, that experimental methods in physics did not change in basic conception in the move, for example, from classical physics to the modifications required by special relativity. The methods of observation and measurement are essentially untouched. In fact, to be quite explicit about the matter, I do not know of a single axiom of measurement as such that was changed by the introduction of special relativity, or even for that matter, general relativity. This does not mean that new instruments of measurements were not introduced throughout the twentieth century, but rather that the fundamental theories of measurement of physical quantities and the detailed experimental methods of measuring those quantities were not changed.

In Chapter 10 of Krantz, Luce, Suppes, and Tversky [1971] a table that runs over six pages lists a great variety of physical quantities that are measured in experimental physics. The dimensional analysis and units of each of these quantities are given. In terms of their experimental measurements relative to a given frame of reference, I believe that not a single one has been changed by the theoretical introduction of relativistic ideas or quantum mechanics. There will, of course, be some change in invariance properties, for example, for the velocity of light, but the specific procedures of measurement relative to a fixed frame of reference have not. I linger on this point only to stress how the detailed consideration of experiments and the enormous carry over of methods of experimentation from one theoretical framework to another in physics is a way of challenging too radical a set of ideas concerning theory change. Again, this is not a topic I really discussed in my book [2002] but something that is a natural extension of the ideas developed there.
Isomorphism and similarity. Schiaffonati mentions the recent literature that suggests the notion of isomorphism that is used extensively in my book [2002] for relations between models of a theory be replaced by the weaker notion of similarity. This is a familiar move in Euclidean geometry. Two triangles of different size that are not congruent can still be similar because their angles are congruent. More generally in psychology a notion of similarity is widely used that violates in a strict sense transitivity, so familiar in standard notions of isomorphism or of congruence in geometry. Even with this violation, the intuitive idea of similarity as being a generalization of isomorphism still works well. I find no difficulties with this approach, in fact it is quite clear that it has been important to be more realistic about the finite precision of human or instrumental judgement to leave room for such non-transitivity. I scarcely discuss such a concept of similarity in the book [2002], but in the treatise on the foundations of measurement I co-authored, in volume 2 (Suppes, Krantz, Luce, and Tversky [1989], chapters 16 and 17) such ideas are explored in some detail and a very large number of references to the earlier and extensive literature are given.

Computational models. In many ways the most important concept that Schiaffonati introduces is that of a computational model. Again I find myself in agreement with what she has to say about such models and from my own standpoint I look at computational models as being not in opposition to set-theoretical ones, but a distinguished subset important in many parts of mathematics and science. Like her, I don’t think it is important to try to give an explicit formal definition of computational models but the intuitive idea is clear. We have in mind for such models that the resources are available in principle from a formal standpoint to compute functional values of a variety of sort. The standard computations that are made in statistics on data meant to test stochastic-process ideas constitute in many standard forms good examples of computational models. From a formal standpoint we could insist that in a computational model everything be computable in the standard mathematical sense but I don’t think this is going to take us very far, because what one
has in mind is not theoretical computations that might in fact not be possible to make, but actual models that are in a practical sense computational, is demanded in models that are simulations of some empirical phenomenon. I also like her emphasize that computational models are not just a kind of mirroring of phenomena but also a kind of rendering. This corresponds to classical ideas in geometry and perception of perspective from many viewpoints, which is often a feature of discussions of rendering.

It is also important to recognize that there are important problems that cannot be well simulated. A good example is given in chapter 7 of my book [2002], the example of a restricted form of the three-body problem in classical mechanics, which reduces to the motion of a single particle for study. This single particle, moving through the center of mass, and perpendicular to the plane of motion of the two other bodies, whose trajectories are closed ellipses, can have for selected values of the parameters, determinateness or uniqueness, in the mathematical sense of being a unique solution of the reduced ordinary differential equation, but can at the same time not be simulated well by any standard computational procedure, because of the rapid approach to randomness in the motion of this particle.

Multiagent systems. As a final concept introduced by Schiaffonati I consider her discussion of multiagent systems. This is an important topic in both contemporary computer science and also in several parts of the social sciences. In fact, markets with large numbers of agents operating in them are often studied as if there were a non-denumerable continuum of such agents, an abstraction that has no empirical justification whatsoever, but that is useful for computational purposes. For this kind of situation I find no natural extension needed of the concepts concerning models and theories introduced in my book [2002]. The consideration of multiagent activities is a natural one, and is natural already in physics in considering systems of many particles. When we want to have more complicated agents, I agree with her emphasis on agents interacting and coming to cooperative agreement about, for example, what actions are to be
taken. But again I find no problems with this and in fact many years ago I coauthored a book full of interactive models in primitive game settings but studied from a standpoint of stochastic models of learning (Suppes and Atkinson [1960]).

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Arrighi has, in her variety of remarks and analyses of my work, caught very well the two most salient features, namely, on the one hand, my interest of long standing in formal axiomatic problems concerning scientific theories, and on the other hand, in sharp contrast my interest in experiments and the detailed analysis of experimental data. Among philosophers I am probably better known for my formal interests than my experimental ones. So in this response I will have a good deal to say about the experimental side of my own thinking and what I believe to be the proper role of the detailed consideration of experiments in the philosophy of science.

Arrighi has a number of different and interesting things to say about behaviorism, some general, some specific to my own views. I will begin with a number of comments about behaviorism. They will very much reflect the changes in my own views as I began to work on the brain in 1996. The first thing to note is how unsatisfactory from a formal standpoint the definitions of behaviorism are – this includes my own, of course. I don’t mean to suggest by this that we should not discuss what we think behaviorism is but rather, that we recognize from the beginning that we will not end up with some satisfactory detailed systematic idea. In this connection the conceptual importance of whether or not to include the brain in discussions of behaviorism is paramount. I am reminded of the contrast
between theories of matter before and after atomism was finally accepted as the correct theory of matter at the beginning of the twentieth century. When I say "correct theory of matter" I mean the correct theory in terms of substantial data available at that time. Moreover for the experiments supporting the existence of atoms, the correctness of the periodic table for the elements, etc. represent experiments that are not false. They stand for all time as a remarkable achievement in the same way as Ptolemy's astronomy did for 1500 years. They are approximately, correct as much of Ptolemy's astronomy still is. This doesn't mean further improvements don't take place. It's important, from my standpoint, to have a conception of science, including behavioristic varieties, that have a place for correctness at a certain level of approximation or coarseness, as well as obvious and continued improvements in what had gone before. This seems to be even more true, looking forward, of what we should expect in psychology than it is in physics, where so much has already been accomplished, even though the horizons of physics now seem unbounded in terms of what we can hope to learn over the next several hundred years. Anyway, the introduction in the twentieth century of substantial concentration on the brain has changed psychology for ever. Even though that change has been taking place, — to be conservative about the beginning —, since the excellent discussion of what was known at the time about the brain in the second chapter of William James' *Principles of Psychology* (1890) to the modern focus on brain imaging. This comes in four important varieties: electroencephalography (EEG), magnetoencephalography (MEG), positron emission tomography (PET), and functional nuclear magnetic resonance imagery (FMRI) — in the usual biological and medical literature the word nucleus, "nuclear" or an abbreviation for it are omitted because of the fear that medical patients may be disturbed by a form of imaging that refers to nuclear activity. That these methods all represent triumphs of the twentieth century are well known. The lateness of nuclear magnetic resonance is reflected in the fact that the fundamental physics was only fully worked out in the 1940s, for which Bloch and Purcell later received a Nobel Prize. Even MEG only came on the scene in the 1970s be-
cause of the availability of superconducting quantum interference devices to receive the extremely weak magnetic signals from the brain. So brain imaging is something new, and the current torrent of activity is something really new. This doesn’t mean that there weren’t excellent experiments on EEG at a much earlier time, beginning indeed with the early work of Hans Berger starting in 1910.

Well after this review of the growth of brain imaging, the natural question is this. Does the behavior of our brains count as a part of behaviorism? It is certainly true that the classical definitions do not include behavior of our brains, but this is only because at that time so little work had been done, even though some of it in the 1940s and the 1950s was superb. Moreover, various kinds of behavioristic experiments not concerned with the brain were focused on measuring nonobvious responses. Some examples are: Galvanic skin response, heart beat, and careful measurements of the latency of responses, that is, speed of responses, in given situations. So it is natural, it seems to me, now as we get to work in the twenty-first century, to extend behaviorism to include behavior of the brain. The real battle among philosophers will therefore be between those who think that mental activity is just a form of activity of our brains, a naturalistic thesis about the physical nature of the mind as opposed to those who find in the concept of mind something that goes beyond the possibility of a physical account. Obviously I squarely belong to the former, and not the latter, school of thought. Keeping these remarks about the brain in mind, it is clear that many of my own earlier writings about behaviorism need to be modified.

So let me turn to the contrast in the 1969 article on the vocabulary of behaviorism versus that of non-behaviorism. I put in this latter category, as Arrighi quotes, such words as “intention, belief, purpose of behavior, rule-following behavior”. Much more than in 1969, in fact I would say very much more so, I now want to include intention as a concept on the behavioral side. Moreover, I want to start by characterizing, in a way close to Aristotle in the De Anima but with a slightly different flavor, life as animate matter. It is then a feature of animals among living creatures, and therefore a feature of much animate matter, to have intentions. This is not the place to
go into a long argument about why the usual concerns about determinism are misplaced. I have decided views on this and have published them elsewhere (Suppes [1988], [1991], [1993]). Here I’ll just take it as a fact that I have an inclusive concept of behaviorism that’s happy with intentions and purposes, which, as Aristotle certainly held are just as much a part of the natural world as familiar properties of physical objects. A response to this might be said, well, such a catholic and all-inclusive behaviorism can hardly be rejected by those who believe in mental activity and mental concepts. But my answer is that I am excluding a variety of philosophical thought, namely, all that which is concerned to separate the mental from the physical. A good example would be mental representations. I don’t really know what a mental representation is unless what is meant by it is a brain representation, for example, what is a mental representation of the word “isomorphism”? (For more details on my conception of mental representation, see the last section of Chapter 3 of Suppes [2002]).

After that long remark, let me make a minor one about the nature/nurture controversy. It is inevitable, I suppose, that behaviorists are thought to always favor the nurture side of the debate. This is certainly wrong about me and even more so now that I have become much more absorbed in the behavior of the brain and in the transformations of perceptions as they move from the peripheral nervous system to the cortex. Anyone who thought wholly in terms of nurture is surely ignorant about the basic physics of what is going on. Perhaps the thing that is most important, and at the same time most neglected in the discussions of this controversy, is the fact of the enormously complicated physical transformations from every kind of perception essentially into an electrical signal going from the peripheral nervous system to the brain, especially to the cortex. To think for a moment that the intricate machinery of either the auditory or visual system, to take the two most important senses for perceiving language and much else about the world, are derived from scratch as a matter of nurture would be a piece of scientific lunacy. It took millions of years for these two systems to evolve. In fact, rightly put, hundreds of millions years. After this enormously long
period of evolution, it is scarcely surprising that we find it difficult to build robots that have a comparable sense of vision or hearing. On the other hand, one of the great marvels of evolution is how flexible this electrical and chemical system is. It makes itself available for fine tuning the job of nurture not only in the matter of language but the matter of almost all other aspects of interacting with the world. I guess I’ve come to see the nature/nurture controversy as tedious and uninteresting. The extreme view on either side seems hopelessly wrong.

Much of what Arrighi has to say about learning models in that section of her commentary I agree with. I just want to comment upon how restricted, on the one hand, the concepts of behaviorism are and, on the other hand, how naturally the theory extends into the estimation of parameters for unobservable processes. Let me clarify by two examples. The first is about the rhetoric aimed at stimulus-response theories. In fact, in the mathematical formulations of theories of the 1950s no claim was usually made about the observability of the stimuli taken in by an organism in the experiment. The experimental situation was described, not the stimuli actually perceived by the subjects of the experiment, be they gold fish or humans.

So how was the concept of stimuli dealt with? Well, there was no attempt to have even a theoretical identification of what the stimuli were in all the standard theories that were given formal expression. What was introduced as a parameter was the estimation of the number of stimuli. So the variability in behavior, for example, variation of responses under various partial schemes of reinforcement from one trial to another, would be fit better by the theory by detailed estimation of the number of stimuli.

In a similar vein it was standard to estimate a learning parameter, something that is still done, but that learning parameter was not in itself directly observable, but could only be estimated from theoretical machinery assumed in some particular version of behavioral learning theory given mathematical expression. The only two notions that were left for direct observation were that of response and the reinforcements given under a particular schedule by the experi-
menter. But in detailed theories, even this notion of reinforcement was made subjective in terms of what was perceived by the subjects, as opposed to what had been selected as reinforcement by the experimenter. So the notion of observability was never one that was given rigorous expression in this whole tradition. This is not to be negative about what was done, but just to be realistic from a broad philosophical standpoint about the intricacies of using the concept of observability.

One of the formal things that I concentrated on in learning theory was the proof that familiar linear models that did not explicitly use a concept of stimulus were the asymptotic limit of stimulus sampling models, where here “asymptotic” means asymptotic in the sense that the number of stimuli approaches infinity. The intricate and detailed proof of this result is to be found in Chapter 8 of Suppes [2002], a proof that is so tedious that my own view of the main interest is in showing how difficult it is to provide absolutely complete reductions of one theory to another even in closely related areas of science. Broader theses of reduction seem hopeless in most cases of serious realization, even though it may be important, as in my own view about the mental as a form of brain activity. This explicit focus represents a fundamental shift for many in the conception of what it means to have a mental life.

I turn now to the brain experiments which have been one of my principal interests since 1996. Let me concentrate on just a few remarks, and try to avoid in the process of doing so, giving an overweening amount of detail about the experiments. First I would like to reformulate what Arrighi has to say about the way I describe the temporal data in our EEG recordings, time-locked to the presentation of a verbal stimulus. From my standpoint the brain is computing first the identification of the transformed character of the stimulus as it moves through either the visual or auditory nervous system to reach the cortex. In this process, first it is transformed into electrical currents that in themselves generate, probably mainly in the synapses of the neurons in the cortex, the electrical field that we record. So our observable data consist of recordings of either an electrical or magnetic field reflecting the electrical activity and secondar-
ily chemical activity of the brain to the reception of such transformed stimuli (I'll speak in terms of electric, I could say electromagnetic or just magnetic but since the experiments are observing the electrical field I shall stick to electrical activity). The point to start with is to emphasize how transformed this electrical activity is in comparison with the physical nature of the stimulus that entered either the rod and cones of the eye or the sound pressure wave that entered the auditory system. Looked at simply from the outside it is utterly remarkable that so much detail in the original source of the stimulus is preserved as invariant. Of course, we are still in the process of discovering these invariants.

My second point is that I am looking then for what in this electrical field that we are recording represents, in the traditional sense of representation, the original verbal stimulus, which itself was a temporal activity. I refer in the case of reading not to the inert printed word on the page, but to the physical activity of observing this word and having electro magnetic phenomenon now at the level of light entering the rods and cones of the eyes. So we are looking for a standard representation, isomorphic in the sense of structure, but remarkably and wonderfully different in external appearance. The difference is almost as great as that between empirical procedures of measurement and isomorphic representations of those procedures to abstract numerical operations on numbers, to give us standard measurements of physical properties or processes.

This return to the image of the representation of concrete experimental procedures of measurement by abstract numerical operations suggests that we could, in principle, aim for the same thing in the case of the brain. So, though it is of course desirable to have a structural isomorphism between spoken speech, for example, and the brain representations of that speech when heard, and also for the even more complicated case when that speech is produced and is initially formulated in the cortex as something to be spoken, so we could go in a different direction and try to make a very detailed science out of the middle, so to speak. Namely, we would produce a detailed abstract structure for what we think of as the mental activities of ourselves as humans, or in simpler cases, of other animals. We
would then want to establish an isomorphism between the brain representation of a spoken sentence and the mental representation of that spoken sentence where the terminology of the brain representation would be physical. In particular it would be electromagnetic in character at the bottom level or at least at the level of EEG recordings. In contrast we would have an abstract formulation in terms of what we would like to think of as the purely mental. Such a program does not seem impossible to work on, but it is quite clear that thinking in philosophy of mind or in cognitive science has in no sense moved very far toward creating such a systematic theory of mental representations. Having such a separate theory of mental representations would in no sense imply we should move away from a completely naturalistic and physicalistic attitude toward the mental, namely, that animate matter can have mental properties, just as it can have electromagnetic properties, mechanical properties, and chemical properties. The viewpoint I am expressing is most certainly not a new one. It is very close to the attitude, expressed in different terminology by Aristotle in the *De Anima* and by Aquinas in his extensive commentary.

Arrighi mentions in several different places and from different angles my insistence on the continuing importance of Humes' central mechanism of the mind, association. I want to say something more here about what appears to be the universal role of association in the brain. Already in the eighteenth century Hume gives a number of good examples of association's role in learning about the world, as well as in the development of the passions, or what we would call now the emotions. Perhaps the cleverest and deepest example of association in the treatise is the analysis at the beginning of Book II of the passion of pride. In the heyday of cognitive science between 1965 and 1980 it was customary to shrug off the mechanism of association as old-fashioned and inadequate to handle the sophisticated concepts being developed by either cognitive psychologists or analytic philosophers. We have by now returned to a sounder view of these matters. The fundamental importance of association in the neurosciences is thoroughly appreciated, and the study of learning processes in terms of association is focused on in both
neuroscience and computer science. A common and mistaken complaint, during those heady days just mentioned, was the claim that, of course, association could not give an account of such complex concepts as those involving rule learning and following. But this is simply a mistake. We now know, as one of the great foundational clarifications of the twentieth century. Rules, or in more technical terminology, any computable function, can be constructed from very simple ingredients, be they a simple Universal Turing Machine's small number of states and unlimited tape, or an associative network active roles and links. Clear and definite mathematical proofs of these matters are widely available in the literature, so I will not say more. Some clear examples of association in artificial intelligence and machine learning are to be found in papers of my own with others on machine learning of robotic language (Suppes et al. [1995a]; Suppes et al. [1995b]; Suppes and Liang [1996]; Suppes et al. [1966]; and Suppes and Böttner [1998]). The scheme of learning in these machine learning papers is technical and in its own way rather complicated, but is, of course, simplicity itself compared to the much more complicated constructions taking place in our brains. On the other hand, there is much consensus among those working on these matters from a mathematical and a formal standpoint in neuroscience that the two main concepts that need to be understood in terms of how they can be realized in large assemblies of neurons are the concepts of association and of memory storage and retrieval. For a quite recent view of what a realistic neural model looks like to realize these two concepts, see Valiant [2005].

There is one point about Hume connected to a quotation from Bauer and Anderson given by Arrighi that I want to mention. Hume did emphasize that complex ideas were composed out of simple ones. I look upon this as a mistake just as William James did; in fact, it was James' main dissatisfaction with Hume's theory of association (James [1890], pp. 594-604). Fortunately, Hume did not give a lot of examples of the sort that would hang him out to dry for several centuries following. He just emphasized simple ideas too much. We could, of course, take another course of simplicity in defense of Hume, but not one that he had in mind, namely, the
development of conditioning in very simple animals, such as *Aplysia*, is inevitably much simpler than the associations that are the basis of learning in humans or other higher animals. But this is not a defense of Hume, just a way of indicating how there is a natural concept of greater and less simplicity, or, if you prefer the opposite, complexity in the learning of organisms.

My final comment on Arrighi concerns her closing remarks on the debate about connectionism and cognitive architecture. Going back to the well known article of Fodor and Pylyshyn [1988], she states the alternatives perhaps too simply, but I think she catches the main point right. My answer is clear. There is extraordinarily weak positive evidence and much negative evidence that the mental processing involved in perception or in cognition do not even begin to approximate the formal systems of inference available in “classical symbolic systems.” It’s nice to dream that organisms including humans, are so constructed, with symbols at the ready. There is really almost no evidence that it is correct or could possibly be correct. Now, this is not the place to begin the argument all over again. Let me just remind the reader, however, that the most universal classical symbolic system is that of classical logic, and it is well known how little of ordinary reasoning as expressed in ordinary language can in any direct way be reduced to this formal system. I do not think anyone who has studied the problem with any carefulness really believes that, beneath the enormous problems of expressing even the measured sentences of good lawyers, some simple logical system close to the classical one is doing the cognitive work. I cannot say positively exactly how that work is done. Trying to understand in serious detail the physical procedures of natural computation used by the brain is a worthy endeavor for the future.

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Badino has divided his comments into four sections. The first is on a fundamental dualism with which I agree, but had not formulated the way he does in thinking about my own work. The dualism is that between invariance and pluralism. The contrast that Badino has in mind is the following. In axiomatizing a theory, for example probability as the main example he uses and one of the most important ones in my own work, we search for the invariant properties to be found in all, or almost all, applications of probability. It is then just the formal properties that have this invariance that are considered in the axiomatization. On the other hand, the applications of a theory of any power and usefulness are manifold and pluralistic in character. In spite of my emphasis in my book *Representation and Invariance of Scientific Structures* [2002] on the concepts of invariance and pluralism, I did not sufficiently emphasize this particular way of bringing the two together. I like what Badino has done. It clarifies the proper role of invariance and that of pluralism in thinking about scientific theories and their empirical content.

The important role that an axiomatic foundation has for the theory of probability and the possible clarification thereby of the notion of invariance, was well recognized by Kolmogorov is his short monograph on the foundations of probability [1933/1950], which was very effective in setting a modern standard for putting

probability in its proper place in the modern set theoretical formulations of mathematics. Here is a quotation from Kolmogorov's preface:

The purpose of this monograph is to give an axiomatic foundation for the theory of probability. The author set himself the task of putting in their natural place, among the general notions of modern mathematics, the basic concepts of probability theory—concepts which until recently were considered to be quite peculiar. This task would have been a rather hopeless one before the introduction of Lebesgue's theories of measure and integration. However, after Lebesgue's publication of his investigations, the analogies between measure of a set and probability of an event, and between integral of a function and mathematical expectation of a random variable, became apparent. These analogies allowed of further extensions; thus, for example, various properties of independent random variables were seen to be in complete analogy with the corresponding properties of orthogonal functions. But if probability theory was to be based on the above analogies, it still was necessary to make the theories of measure and integration independent of the geometric elements which were in the foreground with Lebesgue. This has been done by Fréchet.

While a conception of probability theory based on the above general viewpoints has been current for some time among certain mathematicians, there was lacking a complete exposition of the whole system, free of extraneous complications. (Kolmogorov [1933/35], p. v)

Pluralism, on the other hand, enters not only in the applications but in the various interpretations or representations of probability that have been advanced in the literature. I survey in some detail these various representations of probability, ranging from the logical to the subjective, in my book [2002]. In this case, the pluralism is not simply one just of application but also of more detailed thinking about probability. Exactly how is it being used in various contexts and by various authors. Because I very much agree with Badino on his main point, I only want to mention one other aspect of my own views. In the closing pages of the long Chapter 5 of my book on representations of probability, I emphasize, finally at the end, a pragmatic attitude. Here pragmatism means that we should accept without rancor the many different ways in which probability has been
interpreted over the last several centuries while it was being developed for applications in many different domains of thought and activity. The pragmatism comes from a realization that complex and deep applications often do not really work well if forced into one unique way of interpreting probability, for example, as a propensity or as a subjective belief. This idea of pragmatism is not based just on my own ruminations, but is illustrated by what has been said about probability by many physicists and also by one of the twentieth-century statisticians best known for a wide range of applications, namely, Frederick Mosteller (Mosteller and Wallace [1964/1984]).

In section two of Badino’s commentary he focuses on the axiomatization of Kolmogorov that I have just been discussing. I won’t say more here, for I almost entirely agree with what he has to say.

His third section concentrates on the propensity theory of probability, to which I devote a long section in Chapter 5 of my book. Here he and I agree on the necessity of having serious substantive mathematical analysis of the properties that any proper theory of propensity must have, in order to qualify as a genuine and important interpretation of probability. As I indicated, earlier in my book, I find that much of the discussion of propensity is lacking in the kind of technical detail needed to make it a serious competitor of relative-frequency or subjective theories of probability.

There is an important point that Badino comments on that I have stressed earlier, but think it needs stressing once again. It is easy to refer in casual conversations about the foundations of probability to “the” propensity interpretation as I did above. In fact, the propensity interpretation or representation is a grammatical mistake, literally, for we should be speaking of propensity representations. Because it seems certainly evident enough that different physical cases of propensity will lead to different structural axioms, not only to relate in a detailed way the physical structures and processes considered to the axioms of probability, but in even more detail to fix the exact nature of the particular probability distributions arising in a given case. So, for example, in the case of radioactive decay, we are led naturally by the physical phenomena to the exponential distribution, but in consideration of the standard theory of errors of meas-
urement in classical physics we are led to the normal distribution (usually called in physics the *Gaussian* distribution).

The fourth section of Badino's commentary is on the subjective interpretation of probability. Again, I agree with much that he has to say about subjective probability. I just want to emphasize one point that I have not stated in writing as much as I have in lectures and conversations. Badino points out my emphasis on having for the subjective view of probability a criterion of "pure rationality", which means as few purely structural axioms as possible. In my book I review various structural axioms and how they restrict the concept of rationality. Here I want to make a skeptical remark about pure theories of rationality. In almost every domain of experience where we have sought for something really simple and pure, the results have ended up being much more complicated. The necessity of structural axioms in the subjective theory of probability in order to have an exact numerical representation, has an analog in a more significant case, the search for simple foundations of mathematics. In the naive and glorious days of the beginning with Frege and Russell, it was thought that something rather straightforward could be found. If anything has turned out to be complicated, it is what is the right axiomatization for the foundations of mathematics from whatever viewpoint one starts: classical, intuitionistic, or even more restricted recursive fragments of arithmetic. Exactly what the axioms should be has no unique satisfactory answer. The philosophical importance of this absence of a completely unique satisfactory answer to almost every fundamental question of this kind that we examine is of much greater philosophical importance, in many ways, than the survey of what particular choices have been the popular ones.

The fifth and final section of Badino's comments are on my probabilistic empiricism. Early in his comments he quotes a remark from my earlier book [1984] on not only the chaotic character of particular physical phenomenon but the general character of all our knowledge of the world. This remark amplifies the one I just made in the preceding paragraph about the problems of having simple axioms of rationality. For example, for subjective probability we also have a problem of finding really solid grounds anywhere but, and I
stress here as part of my firm convictions about science and philosophy, it is naive to expect anything else. In fact, I would hope that in the twenty-first century we will learn something from the enormous range of ideas that were proposed for settling all kinds of fundamental problems but in a naïve way in the twentieth century. What we should have learned is that settling fundamental problems in some fundamental way is a mistake. The right view is to recognize that we will continually be making adjustments and changes as particular situations arise in the future. There is no hope of having one final all-encompassing correct view of any major area of thought or activity. Driving home this point of intellectual modesty about what we can accomplish continues to be a main theme of my own philosophical viewpoint on almost any subject to be mentioned.

Finally, regarding my own general claims about probabilistic metaphysics and Badino’s skepticism that I have made always the right distinction, I remain firm in my convictions. To me probabilistic metaphysics is itself an empirical enterprise. It is what I call the genuine Aristotelian one. Aristotle’s metaphysics is a very different intellectual enterprise from, say, Kant’s *Critique of Pure Reason* or the fantasies of the philosophers whom Kant was criticizing. Aristotle’s metaphysics is a very general account of many things learned from experience. He makes no attempt to separate experience from metaphysics, and so it is with me. The only way in which metaphysics can be a serious subject is as one that reaches for principles to be found in much, if not all, of experience. Representations and invariances of various kinds are grounded within such a framework by there being partial mathematical results for various theories expressed formally and with the invariance of particular representations, embodied in appropriate theorems. These theorems will never be universal ones in the sense of something holding for experience of a great variety of kinds, but theorems dealing with particular parts of experience, as we expect especially in particular parts of mathematics or particular parts of a branch of science. So another tension, one that I mention as ever present in the kind of enterprise I have embarked on myself and encourage in others, is the tension between the solidity of particular formal results, as expressed in theorems on
representations or invariances, and on the other hand, the particular character of each of these theorems. They are never theorems of some universal metaphysic, but theorems about some particular aspect of the world or of our experience of it. Moreover, the foundational framework within which the theorems are expressed can also be regarded as something that is not grounded once and for all, and for eternity, as was once hoped would be the case with the foundations of mathematics. But will constitute an active and changing enterprise, with new results and new viewpoints as well, as is certainly characteristic of foundational work over the past several decades.

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RESPONSE TO ROBERTA FERRARIO

In the first two sections of her commentary on my work, Ferrario gives an excellent summary of the views expressed in my recent book [2002], especially on the three critical concepts of axiomatization, representation, and invariance. I also like her comments in the last part of section 2, namely, section 2.3, where she stresses two points that she states about my position that are fairly unusual. The first one is the heuristic value that in earlier publications I have assigned to the axiomatic method. She is right in this because it is a common position to compare heuristics to axiomatic methods, in the sense there is no claim that heuristic methods give a full formal account of the content of a subject, or that axiomatic methods are heuristic. I also want to emphasize another point that she does. This is that I think it is important to distinguish axioms that are heuristically valuable, or put another way, are intuitive in their presentation of content. Some axioms that seem necessary and are very useful in subjects are often described as technical. This usually means they are complicated, difficult to read, and do not have a straightforward intuitive meaning. It is a desirable distinction to be made about axioms that brings the concept of heuristics within the framework of the axiomatic method itself. In my 1983 article on this subject that she cites, the main example of axiomatization I criticized on this score is Mackey’s axiomatization of quantum mechanics ([1957],

Epistemologia XXIX (2006), pp. 385-388.)
The non-heuristic character of these axioms is I think the main reason they are seldom mentioned by physicists.

The second unusual point that Ferrario mentions is my stress on the use of axiomatic methods in the empirical sciences. I think she is still right about this point, even though by now the use of the axiomatic method, in such subjects as economics, is the natural way to treat a subject in such prominent journals as *Econometrica*. She mentions a point that I have not emphasized myself enough. This is the paradoxical fact that some of the most extensive axiomatizations in use in the social sciences are to be found in the area of measurement, because of the desire to make the procedures of measurement in these new sciences completely explicit. Here the axiomatic method is used to a fare-the-well, – to give an egocentric reference – as may be found in the three volume treatise *Foundations of Measurement* ([1971], [1989], [1990]) of which I am a co-author. Put another way, the axiomatic method has very much proved itself to be of use not only in empirical sciences but also in the general methodology of the empirical sciences.

The remainder of Ferrario’s paper is about formal ontologies with a use of axiomatic methods within the framework of first-order logic that has become increasingly popular in computer science. I generally agree with the comparisons she draws between the use of axiomatic methods in the sciences and in formal ontologies. The description of the use of axioms in formal ontologies in section 3.3 of Ferrario’s commentary has an outlook and a viewpoint that very naturally goes back to the first modern work on the foundations of the axiomatic method, namely, the 1882 treatise on geometry of Moritz Pasch, which made explicit the formal nature of modern axiomatics, namely, that no intuitive content of any particular interpretation is made a direct part of the axioms. This methodology, well-consolidated by Hilbert in his *Foundations of Geometry* [1899], is now very much the modern view of what one means by the axiomatic method in either mathematics or the sciences. What is said in formal ontologies has direct resonance with the interesting and simple examples given by Pasch in his early work. A point that is more modern than Pasch, and clearly is of great importance in for-
mal ontologies, is the non-categorical character of the axioms (a set of axioms is categorical if and only if any two models of the axioms are isomorphic). The many successful formal axiomatizations of geometry at the end of the nineteenth century and beginning of the twentieth century were mainly aimed at categorical axioms, for example, for Euclidian, hyperbolic, and elliptic geometry. Even though, it should be noticed, non-categorical axioms were familiar in projective geometry, for example, Fano's miniature projective plane of just seven points and seven lines, the report of which was published in 1892, was just one example of the many hundreds of finite geometries that have been considered since. Such finite geometries constitute, in many ways, developments parallel to those considered in formal ontologies.

Finally, I am happy to remark that the approach to formal ontologies shares a pluralism I much advocate in the analysis of structures and theories in the empirical sciences.

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Mackey G.W. [1957], ”Quantum Mechanics and Hilbert Space”, American Mathematical Monthly 64S2: 45-57.
I appreciate and find very suggestive Bramè’s defense of Kant’s *apriori* approach to geometry. I do not think it is necessary to cite texts to agree in these remarks that Kant makes Euclidian geometry pure *apriori* synthetic, as the pure form of spatial intuition – this is not exactly the Kantian terminology but states clearly enough what he claims to have established. Unfortunately, as Bramè admits in both of the main directions of systematic application of geometry, namely, to physics and the physical world, on the one hand, and to perception and the detailed nature of perception on the other hand. There is very good evidence that the Euclidian model is not correct. Of course in the latest twists and turns of astrophysics and galaxy astronomy, the flatness of astronomical space is once again being approximated to a reasonable degree. I have something more to say on this later, but the real attack on Euclidian space in physics is at a more fundamental level of the nature of microscopic space. This includes the very space around us as we move about on earth. Again, it is not appropriate to go into the technical details, but the evidence is certainly, at the present time, overwhelming that the fiction of strong smoothness that is not only continuity but strong differentialability of ordinary Euclidian space does not hold in physics. As we approach ever smaller regimes, we do not reach zero-di­mensional points but an ever more complicated environment of
particles, generated in all likelihood, at least partly, by our efforts to make ever smaller observations.

The same point is to be made about perceptual space. The detailed experimental data, which I review in my book [2002], argues from many different directions that the space of visual perception is not in any conclusive sense Euclidian in nature. One of the arguments not given as often in the consideration of perceptual space is that the modern mathematical formulations of Euclid do not match the data. In fact, there is a conceptually clear gap between the standard geometries developed in modern mathematics since the beginning of the nineteenth century, and the kind of spaces on the other hand that are natural to modern physics or psychological models of perception. These models of perception by the way apply not only to visual space but to the other senses as well. The evidence in the case of perception is for a threshold and inside the threshold, where continuity would need to be assumed for classical geometry, the axioms required to produce such continuity, weak or strong, have no serious empirical support.

What I said about thresholds in perceptual space apply as well to thresholds in physical space. But the thresholds are on a different scale. For example, the thresholds found in physical space are orders of magnitude smaller than those in perceptual space for the instrumentation applied to the observation of physical space in experiments of a great variety are very much more refined than the thresholds so widely observed for perceptual space.

*Kant’s Great Merit: The Antinomies.* What I’ve said so far sounds as if I am very much opposed to Brame’s view and the entire Kantian enterprise, but this is not how I think about the matter. I am in fact an admirer of the depth of the difficulties in modern science, or in the philosophical foundations of modern science, Kant [1781/1997] so clearly and carefully pointed out. The four antinomies: the first on the beginning of time, the second on the continuity of physical quantities or matter, the third on freedom, and the fourth on the existence of a necessary being, all present challenges, whether the thesis or antithesis is supported. Kant also recognized
that the arguments on each side were strong and that therefore there was no simple proof that one of the two sides of each antinomy was clearly the correct one. His own organization of the antinomies in such a way that he affirmed for working purposes of science, and of empirical thought more generally, the antithesis of each antinomy did not mean that he claimed to give any absolute proof of their correctness. In fact, it was exactly in the nature of his thought to see that no absolute proof was possible. His broader thinking about these matters is very clearly explained in the pages of the critique following the presentation of the four antinomies, especially in section III (A462/B490–A476/B504) of Chapter 2 of the antinomy of pure reason, which follows the presentation in section II of the four antinomies.

I do want to note that Kant affirms that the antitheses represent the position of pure empiricism, because the proofs of the theses must necessarily go beyond experience. On the other hand this doesn’t mean that from experience alone the antitheses can be themselves proved. Here is his well-known assertion on this matter at A466:

In the assertions of the antithesis, one notes a perfect uniformity in their manner of thought and complete unity in their maxims, namely a principle of pure empiricism, not only in the explanation of appearances in the world, but also in the dissolution of the transcendental ideas of the world-whole itself. Against this the assertions of the thesis are grounded not only on empiricism within the series of appearances but also on intellectualistic starting points, and their maxim is to that extent not simple. On the basis of their essential distinguishing mark, however, I will call them the dogmatism of pure reason.

Here is his firm statement two pages later on the attractions of empiricism as reflected in the antitheses:

On the contrary, however, empiricism offers advantages to the speculative interests of reason, which are very attractive and far surpass any that the dogmatic teacher of the ideas of reason might promise. For with empiricism the understanding is at every time on its own proper ground, namely the field solely of possible experiences, whose laws it traces, and by means of which it can endlessly extend its secure and
comprehensible cognition. Here it can and should exhibit its object, in itself as well as in its relations, to intuition, or at least in concepts an image for which can be clearly and distinctly laid before it in similar given intuitions. Not only is it unnecessary for the understanding to abandon this chain of natural order so as to hang onto ideas with whose objects it has no acquaintance because, as thought-entities, they can never be given; but it is not even permitted to abandon its business, and, under the pretext that this has been brought to an end, to pass over into the territory of idealizing reason and transcendent concepts, where there is no further need to make observations and to inquire according to the laws of nature, but rather only to think and invent, certain that it can never be refuted by facts of nature because it is not bound by their testimony but may go right past them, or even subordinate them to a higher viewpoint, namely that of pure reason.

Hence the empiricist will never allow any epoch of nature to be assumed to be the absolutely first, or any boundary of his prospect to be regarded as the uttermost in its extent, or that among the objects of nature that he can resolve through observation and mathematics and determine synthetically in intuition (the extended) there can be a transition to those which can never be exhibited *in concreto* either in sense or imagination (the simple); nor will he admit that one can take as fundamental in nature itself, a faculty (freedom) that operates independently of the laws of nature, and thereby restrict the business of the understanding, which is to trace the origin of appearances guided by necessary rules; nor, finally, will he concede that the cause of anything should be sought outside nature (an original being), for we are acquainted with nothing beyond nature, since it is nature alone that provides us with objects and instructs us as to their laws. (Kant, A468/B496–A470/B498)

It is I think absolutely important to be clear that Kant does not support empiricism as providing any absolute foundation. The virtue of the antitheses and of pure empiricism in the form that he states it is in not permitting any outlandish ideas of pure reason, unsupported by experience, to be adopted as true. It is not that on the basis of antitheses one can give a proof that the world did not have a beginning, etc. Here is a final passage I’ll quote that states this very well:

Human reason is by nature architectonic, i.e., it considers all
cognitions as belonging to a possible system, and hence it permits only such principles as at least do not render an intended cognition incapable of standing together with others in some system or other. But the propositions of the antithesis are of a kind that they do render the completion of an edifice of cognitions entirely impossible. According to them, beyond every state of the world there is another still older one; within every part there are always still more that are divisible; before every occurrence there was always another which was in turn generated by others; and in existence in general everything is always only conditioned, and no unconditioned or first existence is to be recognized. (Kant, A474–A475/B503)

What I think can be added to Kant's wise words are the results of modern investigations, both conceptual and empirical, showing why it is a mistake to attempt to reach some absolute position and why we can give a more definitive argument, more definitive than the one given by Kant it seems to me, as to how the antinomies are to be resolved.

Modern resolution of the antinomies. Let us begin with the first antinomy to show the kind of argument I think we can now be aware of and consider, not the big bang and what may have preceded it, but the second part of the antinomy as to whether space is finite or infinite. The central point is easy to make. The measurements on which the inference that space is flat, now current in astrophysics, all involve errors in measurement and approximations. Take a closed finite space, but one that is thousands of orders of magnitude greater in diameter than the range of any of our current observations. There is no distinguishing empirically between that enormous finite space and one of infinite extent. The inference to be made from this inability to distinguish between infinite flat space and extraordinarily large finite curved space is that on the basis of experience we cannot definitely choose one over the other. Kant comes close to saying something like this, but does not quite do it because he is also concerned about the necessity of the laws of nature. This is more evident in discussing the next antinomy.

In the case of the second antinomy this same problem arises but now with the arbitrary small but discrete nature of physical quanti-
ties or matter, as opposed to its continuity. Again if we have a small enough scale, orders of magnitude below that of any equipment currently known, no distinction can be made between a very fine grid and continuous space. The measurement experiments I mentioned earlier show well enough that any program to hope for a series of ever finer experiments that asymptotically converge on the continuity of physical space is certainly a hopeless enterprise. So again, the right empirical attitude, very much in the spirit of the quotations I've given above from Kant, is that no choice between thesis and antithesis in the case of the second antinomy can be given and in each of the remaining antinomies this is the way out.

For example, the same kind of argument holds for the third antinomy, but it is worth remarking on some of the supporting argument in some detail. In Suppes [1993], I cite the beautiful results in ergodic theory that once errors of measurement, or, put another way, inevitable variability in the context of any given physical experiment is considered, it is impossible to make arbitrarily accurate measurements. Consequently, it can be shown there is no way of choosing between the purely classical deterministic theory of the idealized motion of a ball in a Sinai billiard table, that is, a billiard table with a convex obstacle in the middle, and a stochastic process version of the motion of the ball. I have outlined this on several occasions. I refer especially to the fundamental work of Ornstein and Weiss [1991]. It is worth noting that to deal with the errors, a more complicated concept of geometrical congruence is introduced to show that under this probabilistic notion of congruence, no matter how many observations are made, there is no distinguishing between a classical physics model of the motion of the billiard ball with bounded accuracy of measurement and to a Markov stochastic model.

So in the case of the third antinomy, spontaneous motion, the spontaneity of activity so brilliantly postulated in the thesis by Kant is defended, without any appeal to concepts of human or any other living activity, but to the nature of physical processes themselves. What is the story? Kant was right in pinpointing the antinomy. The modern ergodic result is that we cannot decide on the basis of expe-
rience whether the thesis or antithesis is correct. I shall omit consideration of the fourth antinomy, but the same line of argument I have just given can be followed through.

So, where I come out is that the revised neokantian position can be made quite sound on these matters, but—and this is an important but, what remains from my perspective is indeed the pure empiricism without any necessity of natural laws and without any apriori synthetic, but a methodological position that wisely requires no extension of reason beyond experience in making claims about the nature of the world (Suppes [1995]).

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