Order through Reason

Kant's Transcendental Justification of Science

by Thomas E. Wartenberg, Durham/North Carolina

The standard account of Kant's philosophy is that it effects a reconciliation between the claims of empiricism and rationalism. Kant balances his criticism of the rationalist attempt to gain knowledge of objects completely independently of experience with an equally powerful critique of a simplistic empiricist account of the nature of empirical knowledge. Empirical knowledge, Kant argues, requires the mobilization of a whole set of concepts that do not themselves arise out of experience. To these concepts, Kant gives the title "Categories" and claims that they are, so to speak, the a priori furniture of the human mind. Even if one has reservations about the ultimate validity of Kant's argument, one must acknowledge the subtlety of his moves in effecting such a reconciliation.

On turning to the standard view of Kant's philosophy of science, one cannot but be surprised by the contrast. For here, rather than effecting a subtle balance between the claims of rationalism and empiricism, Kant is seen as standing squarely in the rationalist camp in virture of an a priori justification of Newtonian science. The Categories, which we previously noted were general concepts needed to account for the possibility of experience itself, turn out, on such a view, to be nothing more than disguised forms of Newton's Laws. Since the Categories are capable of an a priori justification, this results in the view that Newton's Laws are a priori true[1].

In the paper, I will attempt to show that Kant's philosophy of science is more sophisticated than the standard account would have us believe. Here too we might characterize Kant as, in some sense, forging a middle path between empiricism and rationalism. For although he holds that scientific theories are subject to confirmation and disconfirmation, he sees that the possibility of such testing requires certain assumptions that are themselves in need of a priori justification. What I wish to demarcate are precisely those principles that Kant takes to be necessary for the possibility of science, those principles that legitimize our belief in science as giving us insight into the nature of the phenomenal world.

This task will fall into three parts. In the first, I will present Kant's view of the methodology characteristic of the empirical sciences[2]. I will show that Kant conceives of

[1] For a typical example of a commentator who sees Kant as providing an a priori defense of Newton, see Körner, *Kant* (Penguin, 1955).

[2] Following Kant, I limit my discussion of "empirical sciences" to chemistry and physics. Kant sees all 'more complex' sciences, e. g. biology, as requiring a different justification. The *Critique of Teleological Judgment* is Kant's attempt to explain the status of biology.

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the scientist as searching for empirical theories under the guidance of a specific methodology and then attempting to confirm them by turning to nature via experimentation. What Kant finds problematic is the assumption that such a procedure is capable of telling us 'how things stand in the phenomenal world'. In the second part, I will investigate Kant's attempt to show that such procedures can be given an a priori justification if we assume the principle:

(SN) Nature is systematic.

I will show that Kant takes Reason to be capable of establishing a priori this truth about the nature of the phenomenal world. Finally, I will argue that this view, while doing justice to the regulative nature of Reason, does not violate Kant's own critique of traditional metaphysics. I hope to show, as a result, that the *Critique of Pure Reason* present an even more complex account of the presuppositions of experience than has been thought.

I

I would like to begin by considering Kant's specification of scientific method. For Kant, the two crucial features of science are that it employs theoretical concepts in the investigation of nature and that such concepts are introduced in accordance with the application of a specific methodology. Let us begin by considering Kant's discussion of the former aspect of scientific procedure.

Kant's initial characterization of scientific practice is as "the hypothetical use of Reason". This use of Reason in empirical investigation is intended as a contrast with the logical use of Reason, that is, deduction. What both uses of Reason have in common for Kant is the employment of the syllogism, the traditional form of argument. The difference, however, concerns the status of the major premise. In deductive inference, the *apodeictic* use of Reason as Kant calls it, the major premise, or universal, is accepted as true and is used to determine a more restricted class, the particular, in regard to a certain property. Thus, when we argue:

All persons are mortal
All warriors are persons
All warriors are mortal

the universal validity of the major premise is accepted and used to ground the validity of the conclusion by restricting the predicate "mortal" to a subclass of persons, i.e. warriors. Aside from terminological peculiarities, we can recognize a straight-forward account of the standard view of syllogistic inference.

But Kant points out that deduction is not the only form of reasoning that we employ. We often find ourselves compelled to establish the universal rules themselves.

> If, however, the universal is admitted as *problematic* only, and is a mere idea, the particular is certain, but the universality of the rule of which it is a consequence is still a problem. Several particular instances, which are one and all certain, are scrutinized in view of the rule, to see whether they follow from it. If it then appears that all particular instances which can be cited follow from the rule, we ar-

gue to its universality, and from this again to all paricular instances, even to those which are not themselves given. This I shall entitle the hypothetical employment of Reason (A646–7/B674–5)[3].

I think that it is fairly clear that what Kant intends to characterize here is the hypothetical-deductive model of theoretical science. Such procedure is characterized by the move from 'certain' particulars, be they observation sentences or more restricted laws, to a general law. What this procedure is intended to establish, according to Kant, is the universality of the law. By this, I take him to mean the validity of the universal, or major premise, as a law, that is, its status as a universal truth. He is focusing on the procedure used in scientific investigation whereby a law is established as a hypothesis, as a "mere idea" as he puts it, and is subsequently tested in terms of its ability to account for both the data that are given as well as new cases that are investigated via experimentation.

Let us consider the example that Kant uses to illustrate this hypothetical use of Reason.

> These concepts of Reason are not derived from nature; on the contrary, we interrogate nature in accordance with these ideas, and consider our knowledge as defective so long as it is not adequate to them. By general admission, *pure earth, pure water, pure air, etc.*, are not to be found. We require, however, the concepts of them ... in order to determine the share which each of these natural causes has in producing appearances (A645–6/B673–4).

Kant here presents the chemical theory in which all composite substances, all the substances of our ordinary world, are taken to be combinations of four elements. These elements are theoretical concepts introduced in order to unify our explanations ot the behavior of empirical substances. This is the first characteristic of the hypothetical use of Reason: its introduction of theoretical concepts.

Kant goes on to point to the use we make of such theories in experimentation. As he says in another context:

> When Galileo caused balls, the weights of which he had previously determined, to roll down an inclined plane; when Torricelli made the air carry weight which he had calculated beforehand to be equal to that of a definite volume of water; or in more recent times, when Stahl changed metals into oxides, and oxides back into metal ... a light broke upon all students of nature. They learned that Reason has insight only into that which it produces after a plan of its own ... (Bxiii).

The point is that the hypothetical use of Reason takes the theories which it produces to be guides for empirical testing. In each of the cases mentioned, a theory was proposed by the scientist and certain test results were predicted. With these test results in hand, the scientist was able to confirm the theory he proposed by performing an experiment. What Kant stresses is the role that Reason plays as a guide in empirical testing. Only if we form a theory will we know how to "approach nature", how to confirm a theory.

Thus, the hypothetical use of Reason is, for Kant, a major advance in the methodology of the natural sciences. It is a 'guide' for Reason in its theory building and experimentation, as Reason 'forces' nature to yield its secrets. But this procedure, revolutionary as it may be, is also problematic. For what, we might ask, is it that justifies us in turning to na-

[3] All quotations from the *Critique of Pure Reason* are taken, with some modification, from the Kemp Smith translation. I follow the standard method of citing the original pagination.

ture for confirmation of the existence of substances like pure air, substances that we introduced in order to unify our empirical knowledge? We can put this worry as the question of why the hypothetical-deductive method, in which we approach nature and ask it to yield examples of Reason's own creations, should be taken as rational. Why should the scientist expect nature to be capable of confirming his theories?

Kant's answer to the puzzle relies on the fact that scientific theorizing occurs within a complex methodological structure. The hypothetical use of Reason specifies the way in which an hypothesis is to be confirmed as a law. How Reason was led to produce a given hypothesis in the first place was, however, left unspecified. Kant's claim is that scientific theorizing will have a rational foundation only if we view hypothesis formation as one element in a methodology that seeks to unify empirical knowledge.

To see this, let us begin by considering Kant's specification of the three methodological principles which he takes as guides in hypothesis formation. The three laws are those of genera, species, and affinity. The methodological point of these laws is put quite succinctly by Kant in the following passage:

> The first law thus keeps us from resting satisfied with an excessive number of different original genera, and bids us pay due regard to homogeneity; the second, in turn, imposes a check upon this tendency towards unity, and insists that before we proceed to apply a universal concept to individuals we distinguish subspecies within it. The third law combines these two laws by prescribing that even amidst the utmost manifoldness we observe homogeneity in the graduated transition from one species to another, and thus recognize a relationship of the different branches, as all springing from the same stem (A660/B688).

We could symbolize the intent of these principles by claiming that they jointly specify a methodology *M*. We could represent this methodology as follows:

> (MG) All conceptual diversity should be reduced by the introduction of generic concepts.
> (MS) All conceptual unifications should be divided by the introduction of specific concepts.
> (MA) The differences among species under a single genus should be refined by introducing further division according to a principle.

What these three principles do is to provide the scientist with advice as to how he should proceed in attempting to refine scientific theories. They tell him never to rest content with the present state of science, but to try to gain a more complete science.

In order to see how this advice really functions as a guide to theory building, let us turn to a particular example that Kant introduces for this purpose. Although most of Kant's examples are drawn from the chemistry of his day, he also sees gravitational theory as established by such methodological directives.

> Thus, for instance, if at first our imperfect experience leads us to regard the orbits of the planets as circular, and if we subsequently detect deviations therefrom, we trace the deviations to that which can change the circle, in accordance with a fixed law, through all the infinite intermediate degreees, into one of these divergent orbits. That is to say, we assume that the movements of the planets which are not circular will more or less approximate to the properties of a circle; and thus we come upon the idea of an ellipse. Since the comets do not, so far as observation reaches, return in any such courses, their paths exhibit still greater deviations. What we then do is to suppose that they proceed in a parabolic course, which is akin to the ellipse, and which in all our observation is indistinguishable from an ellipse that has its major axis indefinately extended. Thus, under the guidance of these prin-

ciples, we discover a unity in the generic forms of the orbits, and thereby a unity in the cause of all the laws of planetary motion, namely gravitation (A662–3/B690–1).

This passage elaborates two of the central features of methodology *M*. The first is that only if we are in possession of methodology *M*, is it possible to conceive of the search for a generic description of a range of phenomena as a rational procedure within scientific theorizing. That is, only because of *M* are we able to see the formation of the hypothesis that the orbits of the comets are parabolic as the result of a rational procedure. We arrive at this hypothesis by means of the higher level claim, "All the orbits of the heavenly bodies will be as similar, in their diversity, as possible". Since the planets move in elliptical orbits, we come up with the notion of a conic section as the generic concept governing the specific forms of heavenly motion, and thus, it is reasonable to suppose, the specific form of the orbits of the comets. Under the guidance of methodology *M*, we are enabled to provide a generic description that unifies the phenomena of cosmological motion.

But Kant points out that we are not able to stop there. (MA) tells us that we need to account for the specific variations that the generic concept manifests. And it is here that theorizing enters the picture. For only if we are able to develop a scientific theory that explains why the orbits of various types of celestial bodies take the form that they do, will we have developed the systematic relations among phenomena necessary for a scientific theory[4]. Kant's point is not that gravity *could* not have been discovered unless we had a generic description of all heavenly motion, but that only when we possess such a description do we in fact attempt to provide a single cause for all the phenomena so described.

Thus Kant has provided us with a model of scientific investigation in which the unification of empirical knowledge is our guiding aim. Through the mutual interests of finding appropriate descriptions of given knowledge and the positing of theoretical structures to account for the specific form that such knowledge takes, science provides us with a means to attain a systematic ordering of empirical knowledge. Methodology *M* represents the basic ways in which we proceed in articulating this ordering.

However, if one reflects on the broader context of scientific practice, one is met once again with a puzzle. For scientific theories are not regarded as simply convenient ways to package given empirical knowledge. We think that science extends our knowledge by telling us about the actual structure of nature and we *test* theories to see if, in fact, nature accords with them. But this is just what Kant sees as problematic. For unless we assume that the theoretical structures that we use to unify our knowledge have bearing on the empirical world, testing would not make sense as a means for confirming a scientific theory. Kant's query is why this is a rational belief about scientific practice. Why don't we simply view the hypothetical use of Reason as a "subjective drive for the orderly management or our knowledge"?

Put this way, we can see the force of Kant's inquiry. We take science to give us access to nature. But we employ a method in science that is guided by our own interests, that is the

[4] I owe my understanding of the function of (MA) to Annette Baier. Let me point out that nothing about methodology *M* tells us how to discover a scientific theory, but merely 'where to look for one'.

possession of orderly knowledge. How do we justify these two features of our beliefs about science?

II

What I have established so far is that one needs to make further presuppositions in order to justify the claim that scientific theories, the products of methodology *M*, despite the fact that they contain theoretical Ideas (concepts), are candidates for the ascription of truth, can be subject to empirical testing. Kant's claim is that, if we assume that nature is itself systematic, (SN), we can justify such assumptions.

Let us recall the exact nature of this problem. In the example of the pure elements, these substances were posited as the basic 'stuffs' out of which empirical substances were made. This was done because of the unity it provided in our representations of empirical substances and their interactions. But we also expected this theory, if valid, to account for all chemical interactions and 'demanded' that all our knowledge accord with it. It is precisely this aspect of our procedure in regard to scientific theories that Kant queries. He asks for an explanation of what justifies the treatment of such a theory as anything more than a logical construct, a convenient device we use to unify our representational scheme without claiming that it is rational to turn to nature to test it.

Kant's answer is that we are justified in so doing because nature has the sort of systematic order which particular scientific theories claim it to have. Although we do not have a priori knowledge as to which particular theories are the true ones, we do know that nature itself is systematically orderd. Although Kant on occasion confuses this claim with that of presupposing that a particular unification *must* be true of nature, a stronger claim that his theory does not justify, we can find him making precisely the point I have.

> But the only conclusion that we are justified in drawing from these considerations is that the systematic unity of the manifold knowledge of understanding, as prescribed by Reason, is a *logical* principle ... But to say that the constitution of the objects or the nature of the understanding which knows them as such, is in itself determined to systematic unity a priori, without reference to any such special interest of Reason ... would be to assert a *transcendental* principle of Reason, and make the systematic unity necessary, not only subjectively and logically, as method, but objectively also (A648/B676).

One might be tempted to read this passage as denying that the regulative use of Reason provides us with transcendental knowledge about nature. That is, one could see Kant as contrasting the logical principle of Reason with a transcendental one in order to affirm the former while denying the latter. Kemp Smith, for example, follows this lead. He claims that the passage is pre-Critical and contradicts the more mature, 'objective' strand of Kant's theory of Reason[5]. Although Bennett does not discuss this passage specifically, his

[5] *A Commentary to Kant's 'Critique of Pure Reason'* (Edinburgh, 1916), p. 544. As usual, Kemp Smith's willingness to acknowledge divergent strands in Kant's thought makes him too quick to attribute an inconsistency to Kant when there need be none.

claims about the subjective-objective distinction in regard to Reason, virtually parallel Kemp Smith's reading of this passage[6]. This view is, however, an error due to an insensitive reading of the text, for Kant goes on to argue that just such a transcendental principle as to the systematicity of nature is needed in taking *M* to yield theories that are capable of experimental confirmation[7].

> In all such cases Reason presupposes the systematic unity of the various powers, on the ground that special natural laws fall under more general laws, and that parsimony in principles is not only an economical requirement of Reason, but *is one of nature's own laws* (A650/B678, my italics).

Human Reason, according to Kant, is no more capable of really deceiving us than God is, according to Descartes. Although there may be some 'inevitable pitfalls' along the way, Reason must be capable of showing us that Reason will eventually enable us to reach the truth. That is, we need to know that it is possible for scientific theories produced in accordance with *M* to eventually give us the truth about nature. Again, Kant is not trying to establish that particular scientific theories must be true a priori, but that we know on a priori grounds that science as a whole is a road capable of leading to truth. It is this function that (SN) performs[8].

There are a number of questions that one can ask regarding the assumption of the systematic unity of nature. The first is exactly what does it say about the structure of nature. That is, how would we specify the sort of order required of nature by such a principle. Secondly, we could ask how Kant goes about justifying such an assumption. And finally, we can ask how the presupposition of such order accounts for the possibility of scientific theories being empirically true or false[9].

Let us begin by considering what it means for nature to be systematic. Roughly, it says that nature must have the sort of order necessary to allow for the possibility of empirical theories being true of it. Kant takes such order to be specifiable by a transcendental interpretation of the three methodological principles previously mentioned. Corresponding to (MG), we need to conceive of all variety in the empirical world as unified. Empirical substances and their properties have enough similarities to allow us to form simple laws governing their behavior. The second feature of the systematicity of nature is that empirical objects differ from each other in such a way so as to allow for the formation of species concepts. The idea here is that, although things are similar enough to allow for the formation of genus-concepts, this won't preclude the formation of species concepts on the basis of differences. Finally, we have the principles of affinity, that the sorts of order

[6] *Kant's Dialectic* (Cambridge, 1974), p. 268.

[7] Heimsoeth acknowledges that one has to read the passage as affirming the objective validity of the systematic unity of nature. See his *Transzendentale Dialektik* (Berlin, 1969), Bd. III, p. 574. He fails to make clear, however, exactly what this commits Kant to.

[8] Whether this demand of Kant's for an a priori justification of the *possibility* of success is too strong a requirement is a question I consider in the third section of this paper.

[9] A full discussion of Kant's 'deduction' of the Ideas of Reason is beyond the scope of this paper. I consider here only the limited question of how Kant actually deduces the systematicity of nature. I discuss the larger problem in my unpublished doctoral dissertation, *Reason and Truth in Kant's Theory of Experience* (Pittsburgh, 1977).

that nature contains be such that the specific divisions of a genus that we arrive at through empirical science are capable of further division. This transcendental counterpart of (MA) completes our description of the a priori knowledge that we acquire about the world through (SN). It tells us that things in nature must resemble and differ from one another in a systematic way. This does not entail, Kant tells us, an actual infinity of subspecies in nature. Rather, "the *indeterminateness* of the logical sphere in respect to possible division" (A656/B684) is affirmed of nature.

What these three principles tell us about nature is that it must have enough order to allow for the possibility that scientific theories, *systematic* empirical descriptions, are true of any of its parts. The nature of this order is characterized analogically, through its allowing for theory formation. In this context, it is worth recalling Kant's claim in the first edition Deduction of the Categories that the manifold of possible experience must have affinity. We are now given a more precise specification of what such affinity amounts to.

Now, what I want to argue is that the principles of genera, specificity, and affinity, when taken as transcendental or metaphysical[10] specifications of the structure of nature, serve to justify the claim that scientific theories arrived at by methodology *M* are capable of empirical truth. That is, I see Kant as arguing that only on the presupposition of the systematic unity of nature will it be possible to treat scientific theories as capable of truth, as the sorts of things that might be true of nature. In talking about the principle of affinity, Kant remarks:

> This logical law of the continuity of species (formarum logicarum) presupposes, however, a transzendental law (the law of continuity in nature) without which the former law would only lead the understanding astray, causing it to follow a path which is perhaps quite contrary to that prescribed by nature itself (A 660/B 688).

What Kant refers to here as a logical law is what I have been calling a methodological principle. He claims that such a principle, something that seems to be due to the merely subjective drives of Reason, can be justified as valid of empirical objects only if we presuppose that it is based on a transcendental law, a law that applies continuity of forms to nature itself.

The question that we are now faced with is how Kant thinks that he can justify our acceptance of such a transcendental law. The argument that comes nearest to such a justification occurs in the following passage:

> If among the appearances which present themselves to us, there were so great a variety – I do not say in form, for in that respect the appearances might resemble one another; but in content, that is, in the manifoldness of existing entities – that even the acutest human understanding could never by comparison of them detect the slightest similarity (a possibility which is quite conceivable), the logical law of genera would have no standing; we should not even have the concept of a genus, or indeed any other universal concept; and the understanding itself, which has to do solely with such concepts, would be non-existent. If, therefore, the logical principle of genera is to be applied to nature ... it presupposes a transcendental principle (A653–4/B681–2).

[10] I use "metaphysical" here to emphasize my claim that such principles give us a priori knowledge of the phenomenal world. It should be contrasted with "ontological" which I use to denote 'traditional' metaphysics.

Kant intends this argument only to establish the need for a metaphysical counterpart of (MG) and not the full order that is demanded by (SN). For our purposes, however, I will ignore this and consider the adequacy of the argument in establishing (SN) itself[11].

Kant's claim is that, in a world ordered by the categorial principles alone, scientific knowledge would be impossible. This is because all that those principles prescribe is that empirical objects must be interacting substances with intensive and extensive magnitude. The particular causal interactions governing these substances and the resemblances between individual objects are not specified. Kant's claim is that unless we presuppose a principle of order for such interactions and resemblances, science would be impossible.

Now I think that this claim is justified. For science – and I would also argue, though this is a more contentious claim, empirical knowledge itself – requires for its possibility a world possessing a higher degree of order than that specified by the categorial principles. Although these principles entail the fact that the objects of our knowledge must stand in causal interactions with other objects, the nature of the laws governing such interactions is left open. Further, the ways in which the objects resemble and differ from one another are left unspecified. But this means that the phenomenal world, in so far as it is specified by the categorial principles, might contain objects with no detectable similarities. But if this were actually the case, science would be impossible, for, as we have seen, a crucial aspect of science involves the discovery of laws. But such discovery requires the existence of relevantly similar cases and this is just what, on our presupposition, the categorial principles can not guarantee[12].

Thus, Kant argues that the Categorial Principles, though constitutive of the phenomenal world, are not enough to guarantee that this world be knowable by us. In order to

[11] I present this argument as establishing a *proof* on (SN) as a transcendental principle of Reason. In both introductions to the *Critique of Judgment*, however, Kant employs this very argument to show that we need to *assume* a principle of reflective judgment. As a result, it might be thought that this provides evidence against my interpretation of Kant's theory in the first *Critique*. That this is not the case will be seen once one reflects on the nature of the distinction between reflective and determinative judgments, a distinction upon which Kant's discussion in the third *Critique* is based. I see this distinction as contradicting many claims that Kant makes in the first *Critique*. Once one realizes that all empirical judgments are partly reflective, the distinction that Kant is seeking to draw breaks down. I have tried to argue that the introduction of theoretical concepts on 'reflective' grounds does not entail that they have no 'determinative' role. And the same 'mixed' view applies to empirical concepts (see A225/B273 ff.). As a result, the distinction between ordinary experience and a thoroughly-ordered experience that results from the application of the principle of reflective judgment will not hold up against the theory presented in the first *Critique*.
A fully satisfying explanation of this inconsistency would take us into a consideration of the relation between the first and third *Critiques* (as well as that of both of these to the second). Let me just say that I see the Introductions to the third *Critique* as intended to fulfill two purposes. First, Kant feels the need to introduce the concept of purpose as presupposed by the mechanical explanations in order to justify our more specific use of it in biology. Second, Kant wants to account for aesthetic judgments as based on a feeling of pleasure occasioned by our *surprise* that nature has an order conducive to our faculties. Both these purposes cuase him to present the principle of judgment as having no grounds in the structure of the phenomenal world, but as being a *mere* assumption. Such a view is simply not Kant's view in the first *Critique*.

guarantee the latter claim, we need to show that nature has systematic order, a claim I have formulated as (SN). The argument just quoted for the necessity of this claim as a basis for rational scientific inquiry is what I would characterize as Kant's transcendental proof of the systematicity of nature.

The question arises as to why I view this as a *transcendental proof* of (SN). I think this can best be illuminated by considering the Second Analogy. There, Kant argues that if we are to be justified in talking about events, such events must be caused. The argument is transcendental not because it moves from our beliefs to truths about the world, but because it shows what complex things must be true of the world for even our simplest beliefs to be true of it. I claim that a similar strategy is at work here. For Kant is arguing that if we are to be justified in using theoretical concepts to describe the world, the world must have systematic structure. (SN) is not a 'mere assumption' we make to ground *M*, but first constitutes our reason for taking theories to be true of the phenomenal world[13].

There is still, however, a final aspect of Kant's justification of scientific practice that remains to be considered: how (SN) grounds scientific theorizing. In this regard, we must remember that it was the role of experimentation that I claimed to be the focus of Kant's concerns over scientific procedure. With what justification, Kant asked, does Reason turn to nature to confirm the existence of substances denoted by theoretical Ideas (concepts)? So let us assume that nature has the sort of order that is specified by the three principles of Reason considered as transcendental or metaphysical principles. The question, once again, is why a scientific theory arrived through procedures involving *M* is the sort of thing that can be true of nature.

Nature is systematic. This means that nature specifies its genera in a manner conducive to the human understanding. Although we don't know a priori exactly how and to what extent such systematicity is instantiated, we know that nature must have such systematic order. It is a piece of "objective, but indeterminate" a priori knowledge. But this means that particular empirical genus-species systems, in which given types of objects are seen to be related to a more fundamental genus or in which a given type of object is seen to be divisible into two or more species – empirical systems of this sort are recognizable as partial specifications of the sort of order that nature has. As such, they are certainly the sorts of things that could be true of nature. Nature has the sort of unity that could be instantiated

[12] This argument shows that Kant does not, in the Second Analogy, justify our employment of 'nice' causal laws in describing nature. His view in regard to empirical laws is, as I have shown, more complex. He is not justified, however, in moving from the claim that nature must exhibit an order over and above that posited through the Categories in order for science to be possible, to the more specific claim that nature must be systematic. Only if we assume that science is necessarily a practice governed by *M* can we see how Kant assumes that (SN) is precisely the sort of order that nature must have in order for science to be possible. What we might expect is a methodologically independent argument for the structure of nature and then a further claim that *M* is the best method for descovering such an order. Such is not, however, Kant's procedure.

[13] Since I claim that all empirical judgments have a regulative content (cf. footnote 11), (SN) is actually needed to justify the claim that *any* empirical concept is true of nature. Some of Kant's remarks about "particular experience" in the published Introduction to the third *Critique* suggest this view.

by empirical systems of the genus-species variety. So if we consider a particular empirical theory of that type, it might be true of nature. But such systems are the type of thing that methodology *M* tells us to look for. So if T is a scientific theory arrived at by *M*, it must have the logical form that empirical systems in nature do have. As a result, testing can tell us if T is true of nature.

Let us consider what this proof actually establishes. It shows that, since nature has certain basic structure in common with theories produced in accordance with *M*, such theories can be put to the test of experimentation in order to establish their actual veracity. As a result the systematic unity of nature functions as a metaphysical truth that grounds empirical testing.

There is, however, a further use of (SN) that can be deduced from this proof. For two theories may both have equally strong empirical confirmation and yet differ in regard to their "systematic properties". We can then use these properties as grounds for theory choice. Thus, in the example quoted above, Kant claims that the Copernican theory is superior to the Ptolemaic in that it provides a unitary description and causal explanation of cosmological phenomena. As a result, we accept the more comprehensive and systematic theory, since it more closely approximates the systematic order that we know nature has.

But this is not to say that I believe Kant has provided an adequate account of theory acceptance. For example, empirical confirmation of a theory is a more complex matter that Kant recognized. Also, there is no reason to believe that scientific methodology cannot be subject to revision. Nevertheless, I think it fair to say that Kant's account of scientific activity is a more careful balance between the demands of empiricism and rationalism than commonly thought. The view that science progresses by the refinement and unification of empirical theories is a far cry from the belief that some one scientific theory is a priori true, the view that the standard interpretation attributes to Kant.

III

Let us pause to review the course of my argument. I began by considering Kant's description of scientific practice. We saw that science was characterized by Kant as involving hypothesis formation based upon a specified methodology and the testing of such hypotheses. To justify the rationality of such a procedure, Kant argues that we need to presuppose nature's conformity to Reason. That is, we must presuppose that nature has systematic unity. On such a presupposition, scientific experimentation in accord with hypothesis formation is rational because we can, to some extent, guarantee the possibility of its success. Nature contains empirical systems; testing helps us determine which ones.

My interpretation of the regulative use of Reason, thus, sees Reason as having a dual role. On the one hand, Reason establishes a scientific methodology in accordance with which empirical scientific theories are to be searched for and accepted. But, as I have argued, Reason also justifies the rationality of such a procedure by establishing an item of metaphysical knowledge, i. e. (SN).

At this point, I would like to consider an objection to my view that Reason, in its regulative use, establishes the systematic unity of nature as a metaphysical truth. To get at this objection, I want to consider some of Bennett's remarks on Kant's theory of Reason.

Bennett's view is that the principles of Reason in their regulative use embody "good, safe advice" for the scientist. However, he also notes that Kant, being the sloppy thinker that he was, fails to keep this position distinct from that in which the principles are viewed as constitutive.

> Perhaps we can explain this strange departure of Kant's from his basic position [that the regulative principles simply embody advice]. He thinks that regulative principles, construed as advice, urge us always to seek further unity in our corpus of knowledge: so they reflect Reason's supposedly relentless drive towards the unconditioned: so they amount to more than mere advice. [Kant] drifts into saying that they are not imperatives at all, but indicatives which describe the world. If this is not the slide which I have described, then it is something even worse[14].

What Bennett takes to be a "slide" on Kant's part from a view of regulative principles as imperatives which function as advice to the scientist to a view of these principles as actual descriptions of the world is nothing of the sort. Rather, the latter sorts of principles are precisely what is needed to ground the "advice" embodied in the principles of scientific methodology in order that scientific theories arrived at by following this methodology be capable of empirical truth. It is Bennett who fails to see the difference between these two types of principles, both of which function in Kant's theory.

But what may be bothering Bennett is the notion that, if we treat the regulative use of Reason as presupposing that nature is systematic, we will fall into the error of treating the regulative principles as constitutive.

Like many commentators before him, Bennett assimilates "constitutive" to "having insight into the nature of reality" and "regulative" to "due to our speculative interest"[15]. Therefore, to treat the regulative principles as giving us any insight into the structure of reality would be to deny their status as regulative, and not constitutive, principles. Further, this would violate Kant's stricture against such attempts to know reality.

In considering the notion of "reality" in Kant's philosophy, one must remember that both the noumenal and the phenomenal worlds have a claim to being called "reality". The crudity of Bennett's contrast between constitutive and regulative principles is due to his failure to specify the nature of the "reality" that he takes constitutive principles to give us insight into. I hold that Kant's attribution of the predicate "regulative" to the principles of Reason is meant to contrast with two different types of constitutive principles. On the one hand, the principles of Reason in their regulative employment are not themselves constitutive of the noumenal objects of the Self, the World, and God. Traditional metaphysics is characterized by Kant as the search for just such constitutive principles. On the other hand, the principles of Reason are not constitutive of phenomenal objects either. The categorial principles of the Analytic are jointly constitutive of the notion of an

[14] Bennett, *Kant's Dialectic*, p. 276.

[15] *Kant's Dialectic*, p. 278. But see Lother Schäfer, *Kants Metaphysik der Natur* (Berlin, 1966), p. 130ff. for a recognition of the inadequacy of such a view.

object of experience. The point of my discussion is that to deny that the principle of Reason is constitutive even in this sense is *not* to deny the status of that principle as a member of the framework of principles necessary for the possibility of experience. What I wish to argue is that we must recognize that framework principles which are not constitutive of the objects of a practice are necessary for the specification of that practice.

In order to make the claim more plausible, I would like to consider chess and the rules by means of which we can specify that activity. The rules of chess can be divided into at least two types. On the one hand, we have those rules that determine the nature of the pieces. For example, we might have one like:

(K) The King occupies a single space and can move to any contiguous, unoccupied and unthreatened space.

Following traditional terminology, I will call such a rule a constitutive rule in that it constitutes the possibility of a marker's being a King. The entire set of constitutive rules is necessary for playing chess in that it specifies the domain of objects, the pieces, that are needed for engaging in that activity. The claim that the constitutive rules specify the game of chess is to show that they specify allowable transformations of the board specified by (K).

There is, however, a second type of rule that is no less necessary to playing chess than the constitutive rules that we have just considered. We shall call these rules "procedural rules". Rather than specifying what it is to be a particular piece, these rules concern procedures that specify what it is to engage in playing the game of chess. For example, the following are procedural rules of chess.

(RA) White and Black must alternate moves.
(RC) The game ends when a checkmate or a stalemate is reached.

It is easy to see that such rules are of a different type than those that specify the pieces of chess. In calling them "procedural", I mean to call attention to the fact that they specify the nature of a procedure in accordance with which the game of chess must be played. We could put this difference between the two types of rules as one concerning the *function* of the rules: the constitutive rules specify the nature of the pieces that can be moved in accordance with the procedures outlined by the procedural rules[16].

Now someone might claim that the procedural rules, though important for chess, differ from the constitutive rules in that they do not give us information about possible transformations of the board that can count as moves in a game of chess. Chess is, after all, a game and one needs to know the basic procedures that will count as "playing the game". But this does not mean that we should assume that the procedural rules tell us facts about chess positions. One could claim that expecting such rules to have a descriptive content would be to make a mistake akin to the one Bennett sees in Kant's claim that the regulative principle of Reason tells us something about nature.

[16] I am not treating these different types of rules as a syntactic, but a functional or intentional classification. I leave open the question of whether one could revise the rules of chess so that these types of rules were extensionally equivalent.

What I wish to argue is that the procedural rules, though they function to explicate the procedures that go to make up the game of chess, tell us something about chess situations, something about possible transformations of the board that can count as moves of the game. To see this, consider the following specification of a transformation in chess. The board is in the standard initial position with the exception that both of White's rook pawns have been advanced one rank (to Rook 3). The move is specified as Black moves the King's pawn to KP3. On first glance, this seems to be a perfectly legitimate move in a game of chess. After all, the transformation is in accord with the rule for moving pawns. But once one begins to consider how the board could have gotten into its current state, it will become apparent that it must be White's move. Since White begins play and, according to the specified situation, must have moved an even number of times, the specified transformation cannot take place in a game of chess. But it is only in light of the procedural rules of chess, such as (RA), that we are able to reach this conclusion. Only through the mediation of the procedural rules, over and above the contribution of the constitutive rules, is this particular item of knowledge about possible transformations of the board available to us.

What I take this to show is that the rules of chess, though describable in terms of their specification of possible transformations of the board, fall into two classes both of which are necessary to the specification of the game. These two types of rules are distinct in that they fulfill different functions[17]. Not only is it necessary for the rules of chess to specify the pieces with which the game is played, but it is also necessary to specify the procedures in accordance with which the pieces are to be moved in a game of chess. As a result, we see that the procedural rules are necessary to the game of chess. For only with them will it be possible to characterize an arrangement of 32 pieces of wood on a checker board as occupying a possible position in a game of chess.

The trouble with empiricist models of knowledge is that they tend to seduce us into believing that all that is really necessary for knowledge is a specification of its objects. In regard to chess, this would amount to the claim that only the constitutive rules are necessary for playing the game. What I have been trying to show is that the situation is more complicated, that we need not only to know what the pieces are, but how their movement is to be *regulated.* What is not always recognized is that this latter knowledge, though embedded in procedural rules, is also a necessary part of the specification of the activity of chess playing. Only if we think in terms of the mutual contributions of procedural and constitutive rules, will we arrive at a satisfactory view of the nature of chess and, to pick up the analogy, empirical knowledge.

I introduced the analogy between the rules of chess and those rules necessary for the possibility of science in order to justify my claim that the systematicity of nature, though a regulative principle of Reason, is not merely a convenient guide for the scientist. I have argued that we need to interpret (SN) metaphysically in order to justify science as a means

[17] It should be noted that there are certain rules that might be called constellational and which fall between the two types I have mentioned. An example of such a rule would be one that specified that a checkmate occurred when a player could not parry a check.

of finding out about the nature of the phenomenal world. The chess analogy is meant to show that Kant's attempt to specify a regulative principle of Reason is a particular application of a general distinction between two types of rules needed in specifying an activity. Just as chess requires the procedural rules to enable us to perceive the pieces as forming a possible position in a game of chess, science requires that we presuppose a system of nature in order that we able to treat scientific theories as telling us the truth about the phenomenal world. And just as the procedural rules tell us something about which transformations can count as moves in a game, the regulative principle of Reason tells us something about the structure of the phenomenal world.

There are two features of a rational activity that make it necessary that it be specified by both procedural and constitutive rules. On the one hand, such activity requires the adoption of a basic strategy for reaching its goals. In chess, this amounts to claiming that to play chess one must intend to win. To try to win, one must make moves intended to establish one's superiority over an opponent. A particular strategy for winning makes sense only in the context of such a goal directed activity. But the establishment of such a context, one in which a player is trying to win, presupposes the specification of the rules of the game, the constitutive and regulative rules, in order that we know what would even count as winning.

But even this is not enough to guarantee the *rationality* of engaging in the activity. Checkers modified only in that one player began on white and one on black is an easy example of a rule governed, but irrational activity in that the aim of such a game could not be achieved through following its rules. In order therefore to guarantee the rationality of engaging in an activity, one needs to know the possibility of succeeding in one's goals. We know that playing chess is rational because we know that it is possible, through the application of the rules of the game, to reach its aim, i. e. checkmating one's opponent.

Kant's theory of the regulative use of Reason is an attempt to show that science is a rational activity in just this sense. Science is rational in that it is possible via the adoption of methodology *M* to reach our goal, the establishment of true empirical theories. But such rationality is established only when we have proved that nature itself has the sort of unity that science will enable us to uncover. For only with this item of metaphysical knowledge can we guarantee the possibility of reaching our aim by engaging in scientific research.

Thus we see Kant's justification of scientific procedure involves two distinct parts. On the one hand, he shows that, in order for science to be possible, nature must be systematic. It is such metaphysical knowledge that I have claimed is embodied in (SN). On the other hand, if we are to come to know that order which we know on a priori grounds nature must have, we need to proceed in accord with methodology *M*. This basic methodology grounds our acceptance of more particular maxims in accordance with which hypotheses can be formed for empirical testing. By considering the nature of the game of chess, I hope to have shown the plausibility of Kant's strategy.

IV

In this paper, I have tried to present Kant's justification of natural science. Science, for Kant, is Reason's attempt to gain access to the specific nature of the phenomenal world. As such, its rationality requires that we be able to show that the methodological procedures that it employs will lay bare the actual nature of the phenomenal world.

Throughout this paper, I have been stressing Kant's conception of science as an empirical discipline whose theories receive justification via Reason's a priori contribution to the framework of metaphysical principles which it is Kant's purpose to establish in the first *Critique.* In so doing, I have been attempting to highlight a feature of Kant's thought that has not received sufficient attention, thus leading to a simplistic view of his philosophy of science. There is, however, another side to Kant's theory of science. Scientific theories have, according to Kant, an a priori element[18]. This is, after all, not surprising. Since Kant holds that *all* objects of experience need to satisfy certain a priori constraints, this claim necessarily holds of those objects of outer sense that compose the subject matter of the physical sciences. What I have tried to show is that, despite his acceptance of such a priori principles, Kant does allow experience itself a crucial role in the justification of scientific theories.

As a result, we can see that Kant's justification of scientific procedure is a sophisticated critique of both empiricism and rationalism. For Reason, although it cannot succed in the rationalist dream of establishing particular truths about the Self, the World, or God, justifies scientific procedure, according to Kant, by demonstrating a priori the systematicity of nature. Kant's claim is that, only in so far as this is possible will we be able to see science as rational, as a procedure that will, in the end, enable us to comprehend the true nature of our phenomenal environment[19].

[18] This theory is presented by Kant in the *Metaphysical Foundations of Natural Science.* Although Kant does give an a priori justification of some Newton's Laws, the inverse square law is conspicuous in its absence. My claim is that Kant argues that certain a priori principles must apply to matter as the subject of the physical sciences if science as such is to be even possible. That the possibility of a science of matter rules out certain theories a priori does not tell us how we ought to proceed to justify a particular theory. It is this latter function that I have been attempting to attribute to the regulative use of the Ideas of Reason.

[19] An earlier version of this paper was read at the University of Pittsburgh, Duke University, and Columbia University. The present version has benefited greatly from comments received at each of these places as well as from those of the referee of Kant-Studien.