

# Bridging a Gap Between Naturalistic and Traditional Approaches in the Philosophy of Science

**Tetsuji Iseda**

Nagoya University

iseda@info.human.nagoya-u.ac.jp

## ABSTRACT

There seems to be a lack of mutual understanding between naturalistically oriented philosophers and more traditional philosophers in the philosophy of science. This paper attempts to bridge the gap between these two orientations by introducing a conceptual framework: a distinction between action-guiding rules and evaluative principles. To make the argument concrete, I examine exchanges between Frederick Suppe and Bayesians over Suppe's interpretation of W.J. Morgan's plate tectonics paper. Through a re-analysis of the Morgan paper, I conclude that Suppe's account through the special reasons requirement can be made compatible with a Bayesian account, though not quite in the way Bayesians themselves argue.

**1. Introduction.** There is a steady naturalistic trend in the contemporary philosophy of science. These naturalistic philosophers tend to focus on empirical studies about how science is actually done, rather than construct abstract theories about how science should be done. One of the recent advocates of this naturalistic trend is Ronald Giere, whose paper "Philosophy of science naturalized" nicely summarizes the basic ideas of the naturalists (Giere 1985). Naturalistic philosophers describe the way scientists make their decision in a certain case, without implying that that way is good (or bad, for that matter).

On the other hand, there are others, such as confirmation theorists, who keep working on the traditional issue of justification of scientific beliefs. Their arguments are usually supported by abstract arguments rather than empirical facts. They often use imaginary cases. Even though they also use concrete case studies from the history of science, the main focus is on abstract theories, and concrete examples are used merely for illustration.

The conflict between these two trends surfaces in the debates over the validity of Bayesianism as a theory of scientific method. In recent issues of the journal *Philosophy of Science*, we witnessed a few bitter exchanges between Bayesians and anti-Bayesians such as Mayo and Suppe, which did not seem to reach anywhere (Howson 1996; Mayo 1996; Suppe 1998a; Franklin and Howson 1998; Suppe 1998b). I think that a part of the problem is that their goals are too different to have a fruitful discussion. It seems to me that Bayesians, whose approach is fairly traditional, cannot see much sense in the naturalistic approach of anti-Bayesians, and vice versa.

Now, I see no problem with the existence of alternative approaches in the philosophy of science. However, I also think that it is possible to have a better cooperation which can benefit both naturalistic and traditional philosophers. This is the possibility I would like to pursue in this paper.

My arguments are centered around one specific case: the exchanges between Frederick Suppe and Bayesians over Suppe's interpretation of Morgan's plate tectonics paper. In the next section, I summarize Suppe's position and his interpretation of Morgan's paper in terms of the special reasons requirement. Then in section 3 I look at exchanges between Suppe and Bayesians as to whether Bayesianism can explain the structure of the Morgan paper. After that, in section 4, I offer my own Bayesian interpretation of the paper which, I think, is compatible with Suppe's analysis. In section 5, I try to generalize my arguments by introducing a contrast between action-guiding rules and evaluative principles. I argue that the relationship between rules which the naturalistic approaches look for and principles traditional approaches seek can be better understood in these terms.

**2. Suppe's naturalistic approach.** Frederick Suppe has written a series of papers which practice a version of naturalistic approach (Suppe 1993; Suppe 1997; Suppe 1998a). A common theme we can find in these papers is that philosophy should be able to account for actual practices in science including its seemingly social aspects. At the beginning of the paper we are going to look at, Suppe starts with the following claim: "an adequate philosophical account of testing ought to apply at the micro level and be able to take into account everything in the write-up. The only exception should be items for which specific reasons for exclusion can be given" (Suppe 1998a, 382). This requirement is based on his observation that scientific papers are under a strong pressure to be short, so we can expect that everything in a published paper has a reason to be there. Suppe allows that we explain away some aspects by showing that they have rhetorical or social functions which constitute a 'specific reason for exclusion'. However, if a philosophical theory regard a large part of a scientific paper as playing a non-epistemic role (such as a mere rhetoric), the advocates of the theory owe us an explanation why the author spent so much for something irrelevant to establishing the claim (402-403). Anyway, here we see Suppe's strong orientation toward accounting for, rather than evaluating, actual scientific practice in doing his philosophy of science.

With this requirement in mind, Suppe starts a detailed analysis of the microstructure of a scientific paper using his own encoding system. He labels various parts of a paper with following letters: H stands for hypotheses, C stands for corollaries, M stands for methods used, D stands for data, I stands for interpretation of data, Q stands for specific questions on the interpretations, and R stands for rebuttals to the specific questions.

The paper he uses for the analysis is Morgan's seminal paper which laid down the basic ideas of the plate tectonics theory (Morgan 1968). In this paper, the hypothesis that the surface of the earth consists of rigid plates (along with related hypotheses) is examined by comparing its corollaries with data. In doing so, Morgan spends most of the paper presenting interpretations of data, raising specific questions on the data and their interpretations, and rebutting these questions; that is, the labels I, Q, and R account for a major part of the paper. Let us look at more detail. (Table 1 at the end of this paper shows the actual coding Suppe did with the Morgan paper.)

In Morgan's case, the main hypothesis is that "the surface of the earth is divided into about twenty units, or blocks" (Morgan 1968, p.1959). Morgan adds an assumption that these blocks are perfectly rigid, which yields predictions which can be compared with observations (Morgan 1968, pp. 1960-1961). Especially, this assumption of rigidity enables us to apply Euler's theorem to the analysis of faults between two blocks (Morgan 1968, pp. 1962-1963). Morgan realizes that there are distortions which do not meet the rigidity hypothesis, but he thinks that "it is of interest to see how far this simplifying [sic] concept of rigidity can be applied" (Morgan 1968, p. 1961). Suppe analyzes these passages into five sub-hypotheses H1~ H5 and three corollaries C1~C3 (Suppe 1998a, 386-388). The most important corollary is that "all the faults common to two blocks must lie on small circles concentric about the pole of rotation", which Suppe labels C1 (Suppe 1998a, 387). Most of the Morgan paper is spent analyzing data relevant to this corollary C1, dealing especially with questions raised by anomalous data and rebuttals of the questions.

Let us look at Morgan's arguments on the boundary between the Pacific block and the North American block as an example (I use this same example in my re-analysis later). Except for a few anomalous faults, most of the faults at this boundary can be seen as a segment of a concentric circle centered around somewhere near 53°N 53°W. This is interpretation I5 of data D7, in Suppe's notation. However, some anomalous data points cast doubt on this interpretation of the data (Q11 and Q12). With these data points, the fit between D7 and the prediction from C1 may not be that good. Morgan's answer to these 'specific questions' is to omit these anomalous data. Morgan justifies such an omission by referring to "the special nature" of these regions (Morgan 1968, p.1973). According to Suppe's analysis, what Morgan calls the special nature is that the idealizing assumption of the perfect rigidity is violated around these anomalous data (R13-R16). Because of this, these data points are out of the scope of the theory of plate tectonics. As a result, I5 itself is revised to exclude the anomalous data. Thus, according to Suppe, Morgan's argument here amounts to this: "since these regions violate the assumption of plate tectonics, they are irrelevant to the testing of plate tectonic theory" (Suppe 1998a, 389). Such a move seems illegitimate for philosophers, but since this Morgan paper is a thoroughly examined paper in the history of plate tectonics theory, philosophers "should

call into question conventional philosophical understanding of the relations between data and theory, suggesting that adequate philosophical analyses of testing and confirmation should accommodate such reasoning" (389-390). Here we see Suppe's naturalistic orientation again. A similar argumentative structure (interpretation of data, specific questions and rebuttals) can be found all around in the Morgan paper.

Suppe discusses three more or less traditional views on confirmation, namely the HD (hypothetico-deductive) method, the inference to the best explanation, and Bayesianism, and dismisses all of them because they cannot account well for these features of Morgan's paper. Suppe argues that the basic pattern of confirmation by the HD method and Bayesianism involve "making predictions then observing whether the predicted outcome obtains" (Suppe 1998, 396). However, such an argumentative structure is attributable only to about 30 percent of the Morgan paper (395). We come back to details of Suppe's criticism of Bayesianism later.

If traditional models of confirmation do not work, how should we interpret the structure of the Morgan paper? Even though Suppe does not offer an interpretation in that paper, his other papers provide an answer to this. Suppe thinks that what Christopher Cherniak calls "the special reasons requirement" provides the model for the argumentative strategy in science (Suppe 1993; 1997; see also Cherniak 1986). According to Cherniak, the special reasons requirement is a requirement that those who claim to have knowledge be responsible only for "specific counterpossibilities that there is a definite basis for thinking might now apply" (Cherniak 1986, 100). Suppe's version is slightly different: "knowledge claims *prima facie* are to be accepted unless one can raise specific doubts about the claim and give reasons for such specific doubts" (Suppe 1993, 162).

Adoption of such a weak requirement of justification is based on the awareness of the limitations of our cognitive capacity, which Cherniak emphasizes (Cherniak 1986; see also Suppe 1997, 393). Given those limitations, even asking for a total consistency in a belief system is asking too much; any of us would be irrational according to those traditional notions of rationality which willy-nilly assume the logical omnipotence of agents. Moreover, for such limited agents, even trying to be rational in the ideal sense may not be rational at all; there are important things to do other than trying to eliminate all inconsistencies. The special reasons requirement is an alternative way of thinking about scientific methodology suitable for finite agents. We cannot expect such finite agents to answer all possible doubts about their theory, so they are expected to answer only some of the questions.

It should be also noted that the special reasons requirement has a communal aspect in deciding which specific questions are to be answered (Suppe 1993, 163). The abstract thesis of the special reasons requirement does not tell us much about how to write a scientific paper. Relevant scientific community gives a concrete content to the requirement by (probably tacitly) setting a standard for a publishable paper.

To summarize, according to Suppe, the most prevalent argumentative strategy in Morgan's paper is a sequence of an interpretation of data, specific questions, and rebuttals of the questions (and/or a revision of the interpretation). Suppe thinks that traditional models of confirmation cannot account for this strategy, but Cherniak's special reasons requirement seems to account for it well.

**3. Exchanges between Suppe and Bayesians.** Before we look at exchanges between Suppe and Bayesians, let us begin with reminding us of what Bayesianism is. Bayesianism tells us that we should update our degree of belief in a hypothesis H given evidence e according to Bayes's theorem. Let me give you three versions of it.

$$P(H|e) = P(H)*P(e|H)/P(e) \quad \dots(1)$$

$$=P(H)*P(e|H)/\{P(H)*P(e|H)+ P(H_i)*P(e|H_i)\} \quad \dots(2)$$

H and H<sub>i</sub> are mutually incompatible and collectively exhaustive set of hypotheses, and e is evidence to take into account. P(H) is called the prior probability, the probability of the theory before we get the evidence; P(e) is the expectedness of the evidence, the probability that the evidence is likely to happen; P(e|H) is the likelihood of e, given that the hypothesis H is true; P(H|e) is called the posterior probability of H, the probability of H's truth given that e has happened. Another important claim of Bayesianism is that these probabilities should be regarded as subjective, though rational. We can assign a prior probability according to our intuitive, personal degree of belief, though, since it is a probability, it should obey the axioms of probability calculus.

Suppe dismisses the possibility of giving a Bayesian interpretation of the Morgan paper for several reasons. First, what Suppe sees as the central claim of Bayesianism is Savage's convergence theorem (Suppe 1998a, 396).<sup>1</sup> This theorem states that, for a set of mutually incompatible and collectively exhaustive hypotheses, the posterior probability of the true hypothesis converges to 1 by an accumulation of certain kind of evidence (Savage 1954, 46-50). But we do not see anywhere in the Morgan paper an argument in this line. Second, some of the data Morgan used are quantitative, and could be used easily for a Bayesian calculation (Suppe 1998a, 397). For example, the percentage of boundaries which accord with Morgan's hypothesis could be used such a calculation, but what Morgan did was simply to impeach and then ignore those which do not fit. If Morgan was using Bayesian inference even implicitly, this is not quite understandable.

How do Bayesians reply to this? Franklin and Howson (1998) offer several replies. First, they argue that Bayesianism can easily accommodate the strategy of rebutting

---

<sup>1</sup> Probably Franklin and Howson are right that Suppe's own formulation of the convergence theorem is inaccurate (Franklin and Howson 1998, 414-415). However, the question still remains as to whether an accurate version of the convergence theorem can account for the structure of the Morgan paper.

alternative theories (412). In the above (2), if  $P(H_i) \cdot P(e|H_i)$  is small enough, the right hand side gets close to 1. This can be attained by finding an  $e$  which has a small  $P(e|H_i)$  for each  $H_i$  other than  $H$ . This amounts to finding evidence against each alternative hypothesis. Thus rebutting alternative hypotheses is important from a Bayesian point of view. Secondly, Franklin and Howson complain that Suppe conflates two questions; whether Bayesianism can account for the structure of Morgan paper, and whether Morgan himself used a Bayesian inference (413). Bayesianism is trying to do the former, while Suppe seems to charge Bayesianism with its incapability to answer the latter. Thirdly, Franklin and Howson questions the requirement that an adequate confirmation theory should be able to account for everything in a scientific paper.

Commenting on this reply, Suppe complains that Franklin and Howson does not actually show how their arguments apply to details of the Morgan paper (Suppe 1998b, 421). As for other points, Suppe simply repeats his original point. For example, Suppe repeats the claim that Bayesians owe us an explanation why Morgan did not use a Bayesian inference where it was easily applicable, if Morgan's method was implicitly Bayesian (422).

It seems to me that the reason why they do not communicate well with each other is that they do not make much effort to understand what the other party is trying to do.<sup>2</sup> Suppe is not much interested in confirmation theory as an abstract theory of justification. Franklin and Howson do not see much point in accounting for every detail of a scientific paper. This difference seems to have its root in their different images of the philosophy of science itself, namely Suppe's naturalistic approach vs. Bayesians' traditional approach.

If they have nothing to offer to each other, such a lack of mutual understanding is not problematic. However, I maintain that, once they recognize their difference and try to cooperate where they can, a more fruitful relationship will result. I will pursue this possibility for the rest of this paper.

**4. A re-analysis of the Morgan paper.** To begin with, I agree with Suppe that the Bayesians do not do a good job in accounting for details of the Morgan paper. They provide a schematic argument about why elimination of alternative hypotheses can increase the posterior probability. However, first, Morgan was dealing with alternative interpretations of data, rather than alternative theories. Second, the argument Franklin and Howson offer works only when we have a set of mutually exclusive and collectively exhaustive hypotheses, but such a set does not seem to have been available for Morgan. Finally, it is not clear whether all the specific questions Morgan dealt with were associated with some alternative interpretation.

---

<sup>2</sup> I think we can observe a similar pattern in the exchange between Mayo 1996 and Howson 1996.

However, I think that a different kind of Bayesian account of the argumentative strategy is possible. The baseline of my interpretation is this: *all the activities Suppe codes as I, Q, and R (interpreting data, questioning the interpretations and rebutting the questions) can be seen as negotiations on the likelihood  $P(e|H)$* . According to Bayes's theorem, the higher the likelihood, the higher the posterior probability of H. Often Bayesians assume that likelihoods are objectively determinable, but given the significant role played by an interpretation in a data analysis, these factors should be established through arguments. Such arguments will be particularly important when there is no obvious way of determining the prior probability of the tested hypothesis. If we can make the ratio  $P(e|H)/P(e)$  very high, a relatively low prior probability can be boosted up to a high probability.

To show how it works, let us go back to Morgan's arguments on the boundary between the Pacific block and the North American block. The interpretation I5 of data D7 says that there is a regularity among fault lines in D7, which is predicted by C1. Thus,  $P(D7|C1)$  is pretty high under I5. Specific questions related to anomalous data points Q11 and Q12 cast doubt on this interpretation of the data. With these data points, the fit between D7 and the prediction from C1 may not be that good, which means that we have to lower the likelihood  $P(D7|C1)$ ; in fact, if the anomalous data are serious ones,  $P(D7|C1)$  should be close to zero.

Morgan's argument for an omission of anomalous data points (R13~R17) is more complex than Suppe suggests. In fact, Morgan offers an explanation of the anomalous data from the point of view of plate tectonics: "these faults suggest many small blocks moving independently of one another in this region" (Morgan 1968, p.1969). This move has the effect of making these data compatible with C1. Thus, their negative effect on  $P(D7|C1)$  is canceled. This is not even an ad hoc move, because if Morgan's conjecture is correct, then we should be able to find boundaries among these small blocks which obey the prediction of C1. Most philosophical accounts, including Bayesianism, recognize this move as completely admissible.

It should be noted that Bayes's theorem itself does not tell us how to determine the value of the likelihood. So we should not be surprised if an argument for that does not seem Bayesian. The special reasons requirement may well be the argumentative strategy here. However, Bayesianism tells us why such an arguments is important for us.

Thus, I think Suppe underestimated the applicability of Bayesianism to the Morgan paper. However, this does not undermine the usefulness of Suppe's own analysis of the paper. As Suppe notes correctly, Morgan does not appeal to Bayes's theorem or any other probability calculus. Thus, it is unlikely that Bayesianism is the rule which guides Morgan's arguments. We should not underestimate the fact that Morgan's arguments are not explicitly probabilistic. Let me turn to this issue next.

**5. Action-guiding rules and evaluative principles.** A part of the misunderstanding between Suppe and Bayesians is probably on the nature of a Bayesian account of a scientific practice. I think Suppe presupposes that for such an account to make sense, actual scientists should be at least implicitly Bayesian. Bayesians reject this presupposition. What we need here is a clear conceptual framework which shows the relationship between strategies scientists use and abstract theories like Bayesianism.

I think such a framework can be given by the distinction between two kinds of norms: evaluative principles and action-guiding rules.<sup>3</sup> Evaluative principles are basic standards of evaluation, which can be applied to various things --- actions, choices, intentions, judgments, objects, institutions, etc. Those principles may or may not be used by the actors in deciding the course of action. Action-guiding rules are those rules that are supposed to be used by the actors. However, not all evaluative principles are usable as action-guiding rules. For example, in ethics, the principle of utility calls for taking into account all possible consequences and maximizing the utility, and such a calculation is not feasible for limited beings like us. Furthermore, our attempts to conform to the evaluative principle may be undesirable from the very principle's point of view, if we have a natural psychological tendency to overestimate utilities of those close to us. In such a case, the evaluative principle may be used to pick up certain action-guiding rules which are easier to obey and tend to yield consequences which conform to the evaluative principle.

I maintain that Bayesianism is better conceived as an evaluative principle, not as an action-guiding rule. It is quite plausible that using Bayesianism consciously can lead to erroneous conclusions even from a Bayesian point of view. For example, to use Bayes's theorem, we need to assign reasonable values of  $P(H)$ ,  $P(e/H)$  and  $P(e)$ , which is not an easy task by any means. Moreover, if we know that people's assignments of probabilities tend to be biased in a certain direction (as Kahneman et al. 1982 show), Bayesians would not recommend to them to use Bayes's theorem; people may well be better off by using some other method, chosen from a Bayesian point of view. Thus, a completely non-probabilistic thinking may be justified from the Bayesian point of view, because of its usefulness in attaining approximately the same result as Bayesian conditionalization.

Franklin and Howson seem to have something like this distinction in mind. However, when they account for an argumentative strategy from a Bayesian point of view, they simply derive the strategy from Bayes's theorem without paying much attention to our cognitive limitation. One result of this is that their derivation is sensitive to assignment of various probability. I do not think that such an account can sufficiently deal with a non-

---

<sup>3</sup> A similar contrast is widely used in ethics, especially in debates over the validity of utilitarianism. We do not find many example of this distinction in epistemology or the philosophy of science; Goldman's distinction between 'regulative' and 'non-regulative' rules (Goldman 1986, 25) is close to this distinction, though he does not contrast them in the way I do here.

probabilistic argumentative strategy, which is insensitive to any kind of probability. What we need is a more indirect argument which takes into account our psychological capacities: the (in)capacity to assign reasonable probabilities, the tendency to be influenced by biases, and so on.

In the case of the argumentative strategy in the Morgan paper, if my interpretation is correct, Morgan was trying to increase the likelihood  $P(e/H)$  through rebutting alternative interpretations and specific doubts. Still, he did not mention anything like the probability  $P(e/H)$ . In fact, if he had mentioned anything like that, that could have caused further specific doubts about the validity of the assigned probability. Given our cognitive limitation and personal biases, I do not think we can agree on the assignment of such a probability. *Thus, for such an argument to work, a non-probabilistic argumentative strategy which has roughly the same effect had to be developed.* This is the strategy we see in Morgan's (and many other) paper. Even though this is a sketch of a possible explanation, I think an argument of this line can bridge the gap between the special reasons requirement and Bayesianism.

**6. Concluding Remarks.** To summarize, I think that both Cherniak's special reasons requirement and Bayesianism can deal with the argumentative strategy shown in the Morgan paper. Even though they offer quite different accounts, they can be made compatible when we introduce a suitable conceptual scheme: namely the distinction between action-guiding rules and evaluative principles. Bayesians seem to be aware of the distinction between two levels, but they do not seem to pay enough attention to the role to be played by our cognitive limitation in distinguishing the two levels.

Since, in a sense, my proposal is an extension of Bayesians' project, probably it is less acceptable for naturalistic philosophers. One possible reaction from naturalistic philosophers is that I am trying to reintroduce a rational reconstructionist view of science. Lakatos's idea of rational reconstruction has been heavily attacked by historians, sociologists and empirically-oriented philosophers of science. Am I making the same mistake as Lakatos?

What I would say as a reply is that abstract confirmation theories can be seen as general theories which reveals a hidden pattern rather than rational reconstruction.<sup>4</sup> Even if Bayesianism is proposed as a normative theory which reconstruct actual scientific practice, it may also be used as a descriptive general hypothesis. To be sure, we find many activities of scientists which diverge from Bayesianism. However, as we all know, there are various ways to save such a 'falsified' hypothesis: limiting the scope, adding

---

<sup>4</sup> Think of the theory of natural selection as an example. We do not see natural selection anywhere in behaviors of real organisms, but still the theory reveals the hidden pattern behind all such behaviors. Probably we can see Bayesianism as a descriptive theory of this sort. If Bayesian conditionalization tends to lead us to true beliefs, it is possible that a selection process preserves those rules which approximate Bayesian inference in a non-probabilistic way.

auxiliary hypotheses, revising the theory itself slightly, etc. My own argument in this paper can be seen as an example of this. Anyway, I think that a naturalistic philosophy of science can make use of such general hypotheses to deepen understanding of individual cases, and I see no reason why Bayesianism should be purged from their tool kit. Of course this does not mean that they should accept Bayesianism; it may well be a wrong descriptive theory (though this is not as obvious as some seem to think). All I am asking is to be cooperative and open-minded to those who pursue different approaches, and to listen to what they have to say.

## REFERENCES

- Cherniak, Christopher (1986), Minimal Rationality. Cambridge: The MIT Press.
- Franklin, Allan and Colin Howson (1988), "It Probably Is a Valid Experimental Result: A Bayesian Approach to the Epistemology of Experiment", Studies in the History and Philosophy of Science 19, 419-427.
- . (1998), "Comments on 'The Structure of a Scientific Paper' by Frederick Suppe", Philosophy of Science 65, 411-416.
- Giere, Ronald N. (1985), "Philosophy of Science Naturalized", Philosophy of Science 52, 331-356.
- Goldman, Alvin I. (1986), Epistemology and Cognition. Cambridge: Harvard University Press.
- Howson, Colin (1997), "Error Probabilities in Error", Philosophy of Science 64 (supplement), S185-S194.
- Howson, Colin and Peter Urbach (1993), Scientific Reasoning: The Bayesian Approach, 2nd edition. La Salle: Open Court.
- Kahneman, D., P. Slovic and A. Tversky eds. (1982), Judgment under Uncertainty: Heuristics and Biases. Cambridge: Cambridge University Press.
- Mayo, Deborah (1996), Error and the Growth of Experimental Knowledge. Chicago: The University of Chicago Press.
- . (1997), "Error Statistics and Learning from Error: Making a Virtue of Necessity", Philosophy of Science 64 (supplement), S195-S212.
- Morgan, W. Jason (1968), "Rises, Trenches, Great Faults, and Crustal Blocks" in Journal of Geophysical Research 73, 1959-1982.
- Savage, L.J. (1954), Foundations of Statistics. New York: Wiley.
- Suppe, Frederick (1993), "Credentialing Scientific Claims", Perspectives on Science 1, 153-203.
- . (1997), "Science Without Induction" in J. Earman and J. D. Norton (eds.) The Cosmos of Science: Essays of Exploration. Pittsburgh: University of Pittsburgh Press; 386-429.
- . (1998a), "The Structure of a Scientific Paper", Philosophy of Science 65, 381-405.
- . (1998b), "Reply to Commentators", Philosophy of Science 65, 417-424.

Hypothesis	Data	Associated Interpretation	Associated Doubts	Rejoinders
H			Q18	<i>R28</i>
H1, H2	D1		Q1	R1, R2, R3
H3-H5			Q3	R4
C1: Am-Af	D2	I1	I2	R6
			Q6	I4
	D3	I2	Q4	R6
			Q5	R7
	D2, D3	I4	Q8	<i>R9, R10</i>
D6		Q9	R11	
C2: Am-Af	D4	I3	Q6, Q7	I4, M3
	D5		Q7, Q8	<i>R8, R9, R10, M3</i>
C1: Pac-Am			Q10	R12
	D7	I5	Q12	R13
			Q11	<i>R14, R15, R16, R17, I5'</i>
		I5'	Q13	R18
D8	I6	Q14	R19	
C2: Pac-Am	D9	I7	Q15	<i>R20, M3</i>
C1: Ant-Pac	D9	I8	Q16	R21, R22
C2: Ant-Pac	D10	I9	Q16	<i>R21, R22, R23, R24, M3</i>
C1: Ant-Af	I3, I7, I9	I10	Q17	R25
C2 Ant-Af	I3, I7, I9	I10	Q17	R25, M4
C3	I3, I7, I9	I10	Q2	<i>R25, R26, R27</i>

**Table 1:** Suppe's analysis of the structure of Morgan's paper. The table is reproduced from Suppe 1998a, 393, with the author's permission. Basic keys are given in the text of this paper. Rejoinders in italics are those which leave unrebutted residual doubts. Colloraries C1 and C2 are subdivided by plate boundaries to which these colloraries are applied (American, African, Pacific and Antarctic). To learn more about this table, please consult the original Suppe paper and, desirably, the Morgan paper too.