what I called the Revelation model is the correct model of experience. In addition, there are assumptions involved about exactly what the propositions constituting the new certainties must be. None of these assumptions are demonstrated. Nor do I see how they could be.[22]

One example may perhaps suffice. Suppose I respond to my experience by accepting as (total) constraint that my posterior opinion must involve a probability of 90 per cent that it will rain tomorrow. Then perhaps, if I am very consciously introspective, I shall also be aware that this is so, i.e. that I'm accepting this constraint. Could it be that I am simply conditionalizing on the latter, autobiographical proposition? Well it is *possible*. But personally I am so aware of the unreliability of introspection that I would not take that proposition to be certainly true. Perhaps I would give it 90 per cent probability with 10 per cent for its opposite. And as a result my probability for rain tomorrow would become 81 per cent instead of 90 per cent. However, I doubt that such autobiographical commentary is normally involved. It may be postulated of course. More *recherché* evidence taking may also be postulated. This brings us back to the argument in section 1, that it is trivially possible to reconstruct everyone as a conditionalizer. But not fruitful.

We have now come to the end of this exploration of symmetry. It is rather gratifying to note that even this section and the last contain a number of disputed points and unsolved problems. In this Part as well as in the preceding there are, as far as our discussion is concerned, large uncharted areas that remain. To an empiricist it must necessarily be so, for whatever insight symmetry brings us for theory and model construction, scientific progress must always rest on contingent theoretical assumptions. Any a priori certainty it can enjoy is at best conditional.

NOTES

Chapter 1

1. J. E. Ruby, 'The Origins of Scientific "Law" ', *Journal of the History of Ideas*, 47 (1986), 341-59.
2. Ibid. 341.
3. My historical speculation will inevitably be biased by antirealist sympathies. For a different way of telling the story, compare E. McMullen's 1984 presidential address to the American Philosophical Association, 'The Goal of Natural Science', *Proc. APA* 58 (1984), 37-64. I want to thank Prof. McMullen for very helpful discussions and correspondence.
4. For 'occult' see E. J. Dijksterhuis, *The Mechanization of the World Picture* (Oxford, 1961), 157 (II-87). The locality involved is not spatial; teleological causes may operate over spatial distances (McMullen). The locality principle is rather that everything that happens must be due to the action of specific individual substances on each other.
5. *The Works of the Honourable Robert Boyle*, ed. T. Birch (London, 1672), iii. 13.
6. Sir R. Blackmore, *The Creation*, bk. ii, ll. 295-6; 313-18; 321-30, in J. Heath-Stubbs and P. Salman (eds.), *Poems of Science* (Harmondsworth, 1984), 120-1.
7. There is a famous argument by Einstein that (special) relativity entails that every conservation law is local (cf. R. Feynman, *The Character of Physical Law* (Cambridge, Mass., 1965), 63-4). In classical physics, as I shall note below, the conservation laws are global. It is possible to derive them for any mechanical system, subject to certain conditions; but that any given system, or even the whole world, satisfies these conditions, is not derivable, but needs to be assumed. In general relativity we find scope for global principles again, because the global geometry of space-time is not determined by its local geometry. Quantum mechanics, where the whole is notoriously richer than the sum of its parts, furnishes many examples.
8. *Dante's Inferno*, tr. T. Philips (London, 1985), 10.
9. Cf. Job 38: 25-9; Clement of Rome, First Epistle to the Corinthians, 20; the anonymous Epistle to Diognetus, 7. For the latter two, see

M. Staniforth (tr.), *Early Christian Writings* (New York, 1968), 33-4 and 178.

10. The view in question is generally known as occasionalism (sometimes, voluntarism). Although Aquinas's reaction is quite clear from the passages cited in the next note, the tension between the Aristotelian notion of nature and the occasionalist view did not become fully clear until the fourteenth century, during the realist-nominalist debates. See A. J. Freddoso, 'Medieval Aristotelianism and the Case against Secondary Causes in Nature', in T. Morr (ed.), *Divine and Human Action: Essays in the Metaphysics of Theism* (Ithaca, NY, 1988), and E. McMullen, 'The Development of Philosophy of Science 1600-1850' in J. Hodge *et al.* (eds.), *Encyclopedia of the History of Science*, forthcoming.
11. *Summa Contra Gentiles*, bk. 2, especially sects. 4-5, 7-9, 309.
12. Ibid., sect. 7.
13. For recent discussion of Descartes's concept of law, see G. Loeck, 'Auskunft über die Gesetzesartigkeit aus ihrer Konstruktion', *Osnabrücker Philosophische Schriften*, Universität Osnabrück, 1985, and *Der cartesische Materialismus: Maschine, Gesetz und Simulation* (Frankfurt, 1986).
14. Newton himself is not exactly clear on this. Discussing forces in *Principia*, bk. 1, def. viii, he characterizes them as the causes of the alteration of motion. But then he writes they are to be taken 'mathematically, not physically', and tells us 'not to imagine that by those words I anywhere take upon me to define the kind or the manner of any action, the causes or the physical reason thereof'. Cf. McMullen, 'The Development of Philosophy of Science 1600-1850', who comments on this vacillation (or equivocation?) that this separation of the mathematical and the physical enables Newton 'to bracket all questions about how forces function as *causes*, while retaining enough of a causal overtone to suggest he has somehow explained *why* the motion occurs'.
15. I want to thank R. Cooke for making this clear to me. Instructive also are the examples Earman gives in his *A Primer on Determinism* (Dordrecht, 1986), 32, 34, 36, and the possible reactions to them listed on 38. See also ch. 10, sect. 4 below.
16. The metaphor of 'source' should alert us to the danger that an empiricist may also presuppose some such metaphysics. Historical empiricism did indeed succumb to this and fell to Kant's critique as well. See I. Kant, *The Critique of Pure Reason*, tr. N. Kemp Smith (London, 1980), B127, B498-9, B792-6, B882.
17. R. Descartes, *Principles of Philosophy*, author's letter prefaced to the French translation; *Philosophical Works of Descartes*, tr. E. S. Haldane and R. T. Ross (Cambridge, 1985), i. 211.

18. A. Wickham, *The Contemplative Quarry* (New York, 1921), Envoi. This was quoted by H. Weyl in his seminal lecture 'Symmetry', *Journal of the Washington Academy of Sciences*, 28 (1938), 253-71; reprinted in his *Gesammelte Abhhandlungen* (Berlin, 1968), iii. 592-610.
19. Cf. the analysis of such reasoning by E. Mach, *The Science of Mechanics* (LaSalle, Ill. 1974), 516-20, 549 (see also 456-9).
20. See further my *An Introduction to the Philosophy of Time and Space*, 2nd edn. (New York, 1985).

Chapter 2

1. D. Lewis, 'New Work for a Theory of Universals', *Australasian Journal of Philosophy*, 61 (1983), 343-77; citations from 356-357, 364, 365.
2. For Peirce's discussion of the history of this idea, see his essay on Hume's 'Of Miracles'; *Collected Paper of Charles Sanders Peirce*, ed. C. Hartshorne and P. Weiss (Cambridge, Mass., 1931-5, vol. VI, bk. ii, ch. 5, especially pp. 364-6. See also the section 'Letters to Samuel P. Langley, and "Hume on Miracles and Laws of Nature"' in P. Wiener (ed.), *Values in a World of Chance: Selected Writings of C. S. Peirce* (New York, 1966).
3. *Collected Papers*, vol. V, sect. 5.93-5.119 (pp. 64-76). That first section bears the title 'Scholastic Realism'.
4. This may be compared with Reichenbach's conception of indeterminism which I discussed in my 'The Charybdis of Realism: Epistemological Implications of Bell's Inequalities', *Synthese*, 5 (1982), 25-38; see also ch. 5, sect. 6 below and *The Scientific Image*, ch. 2, sect. 5.
5. Reprinted Hartshorne and Weiss vi. 75-85; see esp. 76-9.
6. *A System of Logic* (London, 1846), vol. I, i; bk. iii, ch. 3; J. S. Mill, *Collected Works* (Toronto; 1963), 306.
7. D. Davidson, *Essays on Actions and Events* (Oxford, 1980), 213-14. I want to thank Mark Johnston for helpful conversations; I have also drawn on his 'Why Having a Mind Matters', in E. LePore and B. McLaughlin (eds.), *Actions and Events* (Oxford, 1985), 408-26.
8. The statement is general provided only that the universe satisfies the principle of identity of indicernibles: for in that case, whatever class of events we describe will have some uniquely identifying description.
9. The same can be said of Davidson's analysis of causation, relied on in this article. See 'Causal Relations' in his *Essays on Actions and Events* (Oxford, 1980), 149-62.
10. B. Willey, *The Eighteenth Century Background* (London, 1940), 126-7. This is not an accurate assessment if taken as a description of Hume's argument; as Peirce pointed out, that concerned probability

alone. But that Hume expressed this faith in uniformity is certainly evident.

11. Some writers do not balk; see e.g. N. Swartz, *The Concept of Physical Law* (Cambridge, 1985).
12. David Lewis tells me that such parallel pairs were discussed at UCLA in the 1960s, and I speculate this was the heritage of Reichenbach's discussion of such examples.
13. See my 'Essence and Existence', in *American Philosophical Quarterly* Monograph Series No. 12 (1978); J. Earman, 'The Universality of Laws', *Philosophy of Science*, 45 (1978), 173-81.
14. M. Tooley, 'The Nature of Law', *Canadian Journal of Philosophy*, 7 (1977), 667-98 (see esp. 686).
15. D. Armstrong, *What is a Law of Nature?* (Cambridge, 1983), 26.
16. See Earman, 'The Universality of Laws'; he does add that laws in the sense of Lewis are 'likely' to be universal.
17. My own attempts, utilizing formal pragmatics, are found in 'The only Necessity is Verbal Necessity', *Journal of Philosophy*, 74 (1977), 71-85, and 'Essences and Laws of Nature', in R. Healey (ed.), *Reduction, Time and Reality* (Cambridge, 1981).
18. Cf. Armstrong, *What is a Law of Nature?*, ch. 11; C. Swoyer, 'The Natural Laws', *Australasian Journal of Philosophy*, 60 (1982), 203-23.
19. For example, it is not in accord with my own, cf. *The Scientific Image*, ch. 5. An entirely different, but also contrary, point is made by McMullen, 1984: 'In the new sciences, lawlikeness is not an explainer, it is what has to be explained' (p. 51).
20. Perhaps the resemblance goes further: according to the *Posterior Analytics*, science deals only with the truly universal; and in the *Poetics*, Aristotle writes 'poetry is something more philosophic and of graver import than history, since its statements are of the nature of universals' (1451^7-9). The parallel can not be pushed too far, but remains striking nevertheless, if we are at all puzzled by our insistent craving for reasons why.
21. My own account of explanation, in *The Scientific Image*, implies that explanation is not a good in itself, and is worth pursuing only as a tactical aim, to bring us to empirical strength and adequacy if it can.
22. Armstrong, *What is a Law of Nature?*, 5; see P. Forrest, 'What Reasons do we have for Believing There are Laws of Nature?', *Philosophical Inquiry International Quarterly*, 7 (1985), 1-12.
23. N. Goodman, *Fact, Fiction, and Forecast* (Atlantic Highlands, NJ, 1954).
24. See *The Scientific Image*, p. 118, and 'Essences and Laws of Nature'.
25. R. Stalnaker, *Inquiry* (Cambridge, Mass., 1984), 149-50.
26. Cf. F. Dretske, 'Laws of Nature', *Philosophy of Science*, 44 (1977) 248-68; see esp. 251-2 and 254-5.

27. H. Margenau and R. B. Lindsay, *Foundations of Physics* (New York, 1957), 20.
28. That is certainly in accordance with one use of the term, which may indeed be quite common in some idiolects. Thus P. Abbot and H. Marshall, *National Certificate Mathematics*, 2nd edn. (London, 1960) designed 'for students taking mechanical or electrical engineering courses' has a chapter entitled 'Determination of Laws' which teaches how to find equations that fit given sets of data.
29. N. Cartwright, *How the Laws of Physics Lie* (Oxford, 1983).
30. Ed. M. Gardner (New York, 1966).

Chapter 3

1. I wish to thank D. Lewis for his comments on an early draft.
2. This is just the one metaphysical element which P. Duhem, in other respects an empiricist hero, introduced in his theory of science. In a later chapter we shall see attempts to elaborate much more extensive anti-nominalist theories into rival accounts of law and necessity.
3. For this chapter I am indebted to previous critical discussions by J. Earman, P. Forrest, and M. Tooley. Lewis has recently (1986) proposed an amendment to his account. I shall discuss it only in a note appended to this chapter.
4. *Counterfactuals* (Cambridge, Mass., 1973), 73-5.
5. J. Earman, 'Laws of Nature: The Empiricist Challenge', in R. J. Bogdan (ed.), *D. H. Armstrong* (Dordrecht, 1984), 191-224; quotes from 229-30.
6. For example, it may be that simplicity exists only in the eye of the beholder. If that is so, it may or may not matter to Lewis's analysis. After all he does *not* say that the best theories about a given world are those which are *there regarded* as optimally simple and strong. He assumes instead that the classification of theories as simple or strong is independent of questions of truth, and of the historical features of the world under consideration. I will leave this assumption unchallenged.
7. 'New Work for a Theory of Universals', *Australasian Journal of Philosophy*, 61 (1983), 343-77; see esp. 367-8.
8. See R. S. Woolhouse, *Locke's Philosophy of Language and Science* (New York, 1971) or ch. 3 of his *Locke* (Minneapolis, 1983); M. H. Carré, *Realists and Nominalists* (Oxford, 1950).
9. I do not mean that what we regard as virtuous simplicity is historically unconditioned; but given what we so regard, it is a feature that theories can have even in e.g. uninhabited worlds.

10. These assumptions are that the best theories, by the presently used criteria for good and better, are *also* the best by the criteria of (actual, historical) science, and *in addition* the best by the criteria for evaluating explanations.
11. This criticism is perhaps related to the charge in P. Forrest's challenging article 'What Reasons Do We Have for Believing That There Are Laws of Nature?', *Philosophical Inquiry*, 7 (1985), 1-12, that if laws are simply what is common to all the best 'summaries' of what actually happens, then the fact that something is a law, is just not the sort of fact which can explain anything. But this charge also carries the complaint that Lewis does not do justice to the connotation of necessity, I think.
12. I want to thank Mark Johnston for discussions of this subject.
13. See the incisive critique of natural kinds, in the context of similar issues, by P. Churchland, 'Conceptual Progress and World/Word Relations: In Search of the Essence of Natural Kinds', *Canadian Journal of Philosophy*, 15 (1985), 1-17. I recognize that this reasoning is a plausibility argument only. In a later chapter I will briefly examine A. Plantinga's argument that, given different factual premisses—e.g. that we are created in God's image—a different conclusion follows. I would contest that argument unless the premisses entail highly specific (and correspondingly less plausible) evaluations of our theoretical activity.
14. *Philosophy of Science*, 45 (1978), 173-81; quote from p. 180.
15. Kuhn, *Structure of Scientific Revolutions* 2nd edn. (Chicago, 1970), 107-8.
16. Since I am no friend of real necessity, as conceived in pre-Kantian metaphysics, this criticism may sound inappropriate on my tongue. But it seems to me that the misgivings of the metaphysician translate also into criticisms of Lewis's account, regarded as an analysis of modal discourse. For such as me, there is little relation between ontology and semantics—but not for Lewis.

Chapter 4

1. This is a large issue, which no analytic philosopher can safely ignore. My own view is that modal discourse is very strongly but tacitly context-dependent, and when we supply all the tacit relevant parameters, we are left with purely 'verbal' necessity and possibility only. This has of course been, in one form or other the traditional nominalist-empiricist line on modality. See my *The Scientific Image*,

ch. 6, and references therein; or, more closely connected with our present subject, my 'Essences and Laws of Nature'.
2. H. Reichenbach, *Elements of Symbolic Logic* (New York, 1947), see esp. 360, 368; F. B. Fitch, *Symbolic Logic* (New York, 1952), ch. 3, sect. 11.19; R. Montague, ch. 1 in R. H. Thomason (ed.), *Formal Philosophy: Papers by R. Montague* (New Haven, 1974).
3. W. Sellars, 'Concepts as Involving Laws and Inconceivable without Them', *Philosophy of Science*, 15 (1948), 287-315. At first sight, Sellars's account looks like a combined possible-worlds and universals account. But the relations between universals are eventually defined in terms between corresponding classes in possible worlds, exactly in the way that later became the standard modal account of properties.
4. 'Laws and Modal Realism', *Philosophical Studies*, 46 (1984), 335-47.
5. 'Time and the Physical Modalities', *Monist*, 53 (1969), 426-46; 'Counterfactuals Based on Real Possible Worlds', *Nous*, 18 (1984), 463-77; 'Laws of Nature and Nomic Necessity', MS, 1987.
6. 'Explicating Lawhood', to appear in *Philosophy of Science*, 55 (1988).
7. Walter Benjamin, 'Theses on the Philosophy of History, IX', in his *Illuminations* (New York, 1969), 267.
8. Compare Benjamin's notes on the angelic figure SPES of Andrea de Spisano: the *Florence Baptistry* entry in 'One-Way Street' in his *One-Way Street and Other Writings* (London, 1979). (Thanks to Glen Most.)
9. See articles cited above; the idea appeared already in Montague's early articles on the uses of possible-world semantics for philosophy. See further R. H. Thomason, 'Indeterminist Time and Truth-Value Gaps', *Theoria*, 36 (1970), 264-81; 'Combinations of Tense and Modality', in D. Gabbay and F. Guenther (eds.), *Handbook of Philosophical Logic* (Dordrecht, 1984).
10. The reader may have guessed at another difficulty: how should we distinguish a world which shares our history exactly until, say, 1900, from another world in which the initial history is exactly the same for equally long, but all events happen exactly four minutes later than in ours? This is not an insurmountable difficulty, but is better discussed in another context; see the section on determinism in Part III.
11. For details on the logic of branching time, see R. H. Thomason, op. cit. and my 'A Temporal Framework for Conditionals and Chance', *Philosophical Review*, 89 (1980), 91-108.
12. The main advocate of the views discussed here is P. Vallentyne, 'Explicating Lawhood'. In this section and the next, I am indebted to D. Lewis's lucid discussions of his own attempts to combine his conceptions of law and objective chance. See especially his *Philosophical Papers*, ii (Oxford, 1986), xiv-xvii and 126-31.

13. For ease of exposition I shall leave this and similar examples a little imprecise. The most stable isotope of radium has a half-life of 1620 years. The immediate disintegration product of radium is the radioactive gas radon; its most stable isotope has a half-life of 3.825 days.
14. This question is closely related to what Putnam calls 'Peirce's puzzle'. See H. Putnam, *The Many Faces of Realism* (LaSalle, Ill., 1987), 80–6.
15. A fact long emphasized by I. Hacking; see his *The Emergence of Probability* (London, 1975).
16. See D. Miller, 'A Paradox of Information', *British Journal for the Philosophy of Science*, 17 (1966); R. C. Jeffrey, Review of Miller *et al.*, *Journal of Symbolic Logic*, 35 (1970), 124–7; D. Lewis 'A Subjectivist's Guide to Objective Chance', in W. Harper *et al.* (eds), *Ifs* (Dordrecht, 1981); and my 'A Temporal Framework for Conditionals and Chance'.
17. See *The Scientific Image*, ch. 6. This chapter contains a modal frequency interpretation of physical probability; even a brief look, however, will show that it is not of a sort that could help the friends of laws here.
18. I say 'roughly': the precise form of the problem, and hence the solution, relates a probability measure on the possible states to a measure of duration, in a fashion which is theoretically contingent. It would be impossible to say therefore that the probabilities as such constrain or explain the proportional times of sojourn. Their connection is simply contained in the definition of ergodicity. In addition, it must be added that the agreement between probability and duration is deduced to occur in a class of trajectories of measure *one*. The measure is derived from the probability measure in question. Thus we have the same problem as for the Law of Large Numbers: even if ergodicity is postulated, we must ask why our subjective probability should follow suit upon *that* probability measure rather than some other one, or none at all.
19. The 'modal interpretation' of quantum mechanics has some *formal* similarity to the many-worlds interpretation, but without this realism about possible worlds. See my 'Semantic Analysis of Quantum Logic', in C. A. Hooker (ed.), *Contemporary Research in the Foundations and Philosophy of Quantum Theory* (Dordrecht, 1973), 80–113, and 'A Modal Interpretation of Quantum Mechanics', in E. G. Beltrametti and B. C. van Fraassen (eds.), *Current Issues in Quantum Logic* (New York, 1981).
20. L. E. Ballentine, 'Can the Statistical Postulate of Quantum Theory Be Derived? A Critique of the Many-Universe Interpretation', *Foundations of Physics*, 3 (1973), 229–40. See also S. J. Norman, *Subsystem States in Quantum Theory and Their Relation to the Measurement Problem*, Ph.D. Dissertation, Stanford University, 1981.

21. R. Jeffrey and B. Skyrms have discussed this in different ways; see further ch. 8.
22. This point was forcefully made by R. Foley in his commentary on my paper 'What Are Laws of Nature?' at the New Jersey Regional Philosophy Conference, Nov. 1985.
23. I do agree that common-cause models are pre-eminently good responses to prevalent sorts of explanation request—but that is all.
25. Pargetter, like D. Lewis, denies this, strictly speaking (personal communication).
26. I gave the example which follows, near enough, in my 'Essences and Laws of Nature'.
27. Thus, given such a crystal and its history, we can never be sure that it does *not* belong to the large 'randomizing' class. If it has responded differently in two tests, then of course we know that it does.

Chapter 5

1. Parts of this chapter appeared in earlier form as my 'Armstrong on laws and Probabilities', *Australasian Journal of Philosophy*, 65 (1987), 253–60. See also J. Carroll, 'Ontology and the Laws of Nature', which appeared in the same issue (261–76) and contains similar criticisms but suggests a different conclusion.
2. See my 'Theory Comparison and Relevant Evidence', in J. Earman (ed.), *Testing Scientific Theories, Minnesota Studies in the Philosophy of Science*, x (Minneapolis, 1984), and 'Glymour on Evidence and Explanation', ibid.
3. These problems are implicit also in D. Lewis's critique of Armstrong (Lewis, 'New Work for a Theory of Universals', *Australasian Journal of Philosophy*, 61 (1983), 366; *Philosophical Papers*, ii (Oxford, 1986), xii).
4. M. Tooley, 'The Nature of Law', *Canadian Journal of Philosophy*, 7 (1977), 673.
5. D. Armstrong, *What is a Law of Nature?* (Cambridge, 1983), 92.
6. F. Dretske 'Laws of Nature', *Philosophy of Science*, 44 (1977), 264.
7. The inference problem for laws conceived as relations among universals, is exactly parallel to that which Schlick urgued concerning objective values.
8. Dretske, 'Laws of Nature', 265.
9. Tooley, 'The Nature of Law', 678 ff.

10. Such examples must always, in an anti-nominalist context, have the caveat that the described properties may not exist.
11. My definition here is not exactly Tooley's of nomological relations, but very close if his definition incorporates the discussion that follows it ('The Nature of Law', 679-80); I have tried to do this by my choice of definitions for 'purely' and 'irreducibly'.
12. Sects. 3 and 4 are based on parts of my 'Armstrong on Laws and Probabilities', *Australasian Journal of Philosophy*, 65 (1987), 243-60. See also D. Armstrong, 'Reply to van Fraassen', *Australasian Journal of Philosophy*, 66 (1988), 224-9.
13. In ch. 6, sect. 5 of *What is a Law of Nature?* Armstrong explores the possibility of getting an 'independent fix' (p. 96) on necessitation *via* the notion of causality. I shall concentrate instead on sects. 3, 4, and 6 of that chapter.
14. Armstrong, *What is a Law of Nature?*, ch. 9.
15. By the generality of *now* and the continuity of time, three of the canvassed possiblities require an infinity of atoms of the power of the continuum, which is not even sensible, let alone possible.
16. A look back will also reveal the corollary, that remaining stable for interval *t*, rather than decaying within *t*, is the real universal. To my knowledge, this is the first deduction of the reality of a specific universal from a law of physics, and I respectfully offer it as a contribution to the theory.
17. 'Reply to van Fraassen', *Australasian Journal of Philosophy*, 66 (1988), 226.
18. The difficulty I raise here had been previously and independently raised by J. Collins in correspondence with Armstrong.
19. M. Tooley, *Causation: A Realist Approach* (Oxford, 1987), ch. 4.
20. There were certain other problems: the rules that could determine such probabilities were obviously sensitive to the structure of the language, and so the probabilities would change when the language was extended or translated into another language. Tooley correctly points out that the theory of universals—or anti-nominalism generally—can help here, in effect by determining which is the 'correct' language, in the weak sense of formulating a distinguishing difference in its own terms. But that does not help the main problem.
21. This refers to the early stage of Carnap's programme, in *The Logical Foundations of Probability*, 2nd edn. (Chicago, 1962). In later stages Carnap himself made no such claim of uniqueness.
22. University of Sydney, 1981. I must also thank Collins for his comments on my earlier correspondence with Armstrong and with Tooley.
23. The probability function which gives equal probabilities to all *TV*'s is Carnap's *m*† if restricted to the quantifier-free fragment. It has also feature that $P(Fn|Fa \& \ldots \& Fm) = P(Fn)$, and Carnap rejected it for that reason.
24. If we switch to non-standard numbers, the conclusion is only that at most countably many can receive non-infinitesimal probability. This helps with some technical difficulties, but it makes the non-uniqueness problem worse.
25. *The Works of the Honourable Robert Boyle*, ed. T. Birch (London, 1673), iii. 13; see R. Woolhouse, *Locke's Philosophy of Science and Language* (Oxford, 1971) for an excellent discussion.

Chapter 6

1. London, 1985, p. 92.
2. See G. Harman, 'The Inference to the Best Explanation', *Philosophical Review*, 74 (1965), 88-95. In this chapter I am much indebted to discussions of this issue by A. Fine in his 'The Natural Ontological Attitude', in J. Leplin (ed.), *Scientific Realism* (Berkeley, Calif., 1984).
3. For other discussions of these failures, see my 'Empiricism in the Philosophy of Science', in P. M. Churchland and C. A. Hooker (eds.), *Images of Science: Essays on Realism and Empiricism, with A Reply by Bas C. van Fraassen* (Chicago, 1985) and J. Watkins, *Science and Scepticism* (Princeton, NJ, 1984).
4. Compare Putnam's short discussion of Mill, Reichenbach, and Carnap in *The Many Faces of Realism* (LaSalle, Ill., 1987), 73-4.
5. This is essentially how the rule of induction is presented in Russell's *The Problems of Philosophy* (New York, 1959).
6. The point is not defeated by the stipulation that all inductive rules must converge to the straight rule as the sample size goes to infinity, even if that be accepted. The wide divergences will appear at every finite size, however large.
7. For another telling line of criticism, see H. Putnam, 'Reflexive Reflections', *Erkenntnis*, 22 (1985), 143-54. For still another one, see A. P. Dawid, 'The Impossibility of Inductive Evidence', *Journal of the American Statistical Association*, 80 (1985), 340-1 and sect. 7.1 of his 'Calibration-Based Empirical Probability', *Annals of Statistics*, 13 (1985), 1251-73, both of which refer to the result of D. Oakes, 'Self-calibrating Priors do not Exist', *Journal of the American Statistical Association*, 80 (1985), 339.
8. This is directed against certain Bayesian ideas about confirmation; see e.g. P. Horwich, *Probability and Evidence* (Cambridge, 1982).
9. See D. Armstrong *What is a Law of Nature?* (Cambridge, 1983),

ch. 5, sect. 4; my 'Armstrong on Laws and Probabilities', *Australasian Journal of Philosophy*, 65 (1987), 243–60 and D. Armstrong 'Reply to van Fraassen', ibid. 66 (1988), 224–9.

10. I want to thank Y. Ben-Menachim for helpful discussion and correspondence.
11. This is not a subject without a history. Most of the arguments bandied about appeared already in the debates between Cartesian and Newtonian in the seventeenth century. These debates suffered however from a too easy equation of epistemology and methodology. In the terminology of Herschel's distinction, they tended to confuse the tactics of the context of discovery with rules proper to the context of justification. They also suffered from historical loyalties that kept the 'method of hypotheses' and 'method of induction' as the great and sole alternatives, allied to rival programmes in the natural sciences. We are therefore doomed to repeat this history, not through ignorance, but through loss of innocence. The opposition of induction and hypothesis reappeared in a new key at each subsequence stage, with Whewell's consilience, Peirce's abduction, Popper's bold conjecture, and so forth. See the illuminating essays by L. Laudan in his *Science and Hypothesis* (Dordrecht, 1981).
12. M. Friedman seemed reluctantly to come close to this position in 'Truth and Confirmation', *Journal of Philosophy*, 76 (1979), 361–82 (see esp. 370); R. Boyd appears to take it in P. Churchland and C. A. Hooker (eds.), *Images of Science* (Chicago, 1985); see also my reply to Boyd there.
13. For an effective critique of such evolutionary epistemology, from the vantage of current biology, see M. Piatelli-Palmarini, 'Not on Darwin's Shoulders; A Critique of Evolutionary Epistemology', Boston Colloquium for the Philosophy of Science, Jan. 1988.
14. Cf. the end of M. Tooley, 'The Nature of Law', *Canadian Journal of Philosophy*, 7 (1977), which suggests that IBE can furnish non-subjective prior probabilities for hypotheses.
15. 'Reply to van Fraassen', p. 228.
16. J. J. C. Smart, 'Laws of Nature and Cosmic Coincidences', *Philosophical Quarterly*, 35 (1985), 272–80; quote from p. 273.

Chapter 7

1. From R. Boyd, 'The Current Status of Scientific Realism', in J. Leplin (ed.), *Scientific Realism* (Berkeley, Calif., 1984), 67.
2. *The Port-Royal Logic*, 1662; cited R. C. Jeffrey, *The Logic of Decision*, 2nd edn. (Chicago, 1983), 1.
3. It is an avowal, not an autobiographical description. For discussion of this difference, and the various functions of first-person declarative sentences, see for example P. M. S. Hacker, *Insight and Illusion* (Oxford, 1986), 297 ff.

4. The point of view here adopted, with its insistence on a strict separation between expression of opinion and statement of biographical fact about opinion, I call Voluntarist, for reasons more specifically explained in 'Belief and the Will', *Journal of Philosophy*, 81 (1984), 235–56. Concerning vagueness in personal probability, see R. C. Jeffrey 'Bayesianism with a Human Face', in J. Earman (ed.), *Minnesota Studies in the Philosophy of Science*, ix (Minneapolis, 1983), 133–56.
5. See N. Rescher, *The Logic of Commands*; C. Hamblin, *Imperatives* (Oxford, 1987).
6. The idea needs to be refined. Consider for example the number of days on which he speaks; it must be finite. Therefore he'd have no chance of perfect calibration if he made the x an irrational number. But it is not a point of logic that a probability cannot be an irrational number. So we have to think about how the announced number relates to the proposition in some more complex fashion. See my 'Calibration: Frequency Justification for Personal Probability', in R. S. Cohen and L. Laudan (eds.), *Physics, Philosophy, and Psychoanalysis* (Dordrecht, 1983), 295–319.
7. For a general introduction to probability theory, see B. Skyrms, *Choice and Chance* (Belmont, Calif., 1986).
8. B. De Finetti, 'Probability: Beware of Falsifications', in H. Kyburg, Jun. and H. Smokler (eds.), *Studies in Subjective Probability* (Huntington, NY, 1980).
9. As reported in P. Teller, 'Conditionalization and Observation', *Synthese*, 26 (1973), 218–58.
10. More examples of such bets, as well as the general strategy, are described in my 'Belief and the Will'. There is a danger, when these coherence arguments are written in terms of bets, that they will be perceived as being essentially about betting behaviour. That is not so; they are about consistency in judgement. See B. Skyrms, *Pragmatics and Empiricism* (New Haven, Conn., 1984), ch. 2.
11. What I mean by this will be explained and demonstrated in ch. 13.
12. B. De Finetti, 'Methods for Discriminating Levels of Partial Knowledge concerning a Test Item', *British Journal of Mathematical and Scientific Psychology*, 18 (1965), 87–123; R. C. Pickhardt and J. B. Wallace, 'A Study of the Performance of Subjective Probability Assessors', *Decision Sciences*, 5 (1974), 347–63 and references therein.
13. I have attempted a longer sketch, though with a somewhat different focus, in 'Empiricism in the Philosophy of Science'.
14. (Oxford, 1912; New York, 1959) 7.

15. That we are of the opinion that our opinions are reliable must, however, be construed very delicately; see 'Belief and the Will'.
16. W. James, 'The Will to Believe'. Page references are to his *Essays in Pragmatism* (New York, 1948). Clifford's lecture is found in W. K. Clifford, *Lectures and Essays* (London, 1879).
17. Unlike e.g. Shimony, Friedman, and Seidenfeld, but like R. Jeffrey, and P. Williams, I allow here for different sorts of deliverances of experience, not always equivalent to simply taking propositions as evidence. See further ch. 13.
18. See further my article 'Rationality does not Require Conditionalization', forthcoming.
19. See, for related reflections, Lecture IV: 'Reasonableness as a Fact and as a Value', esp. pp. 77–80, in Hilary Putnam, *The Many Faces of Realism* (LaSalle, Ill., 1987). Merely to deny the relevant dichotomies does not remove the problem; but that is one way to begin the task of elucidating the parallel in relations between practices and standards, for all sorts of enterprises.
20. In the form of R. Jeffrey's Bayesianism 'with a human face'. If I still resist the name 'Bayesian', which has been stretched far beyond orthodoxy, it is to distance myself from certain ideas concerning scientific methodology, held by some Bayesians.
21. See R. Carnap, *The Continuum of Inductive Methods* (Chicago, 1952), sect. 18.
22. (Cambridge, Mass., 1967.)
23. This subject of 'calibration' is explored in my 'Calibration' and in A. Shimoney, 'An Adamite Derivation of the Principles of the Calculus of Probability' (forthcoming), which had been presented as a lecture already in 1982. That rightness, i.e. calibration, is not enough, since opinion is subject to other criteria as well, is clearly shown in T. Seidenfeld, 'Calibration, Coherence, and Scoring Rules', *Philosophy of Science*, 52 (1985), 274–94.
24. D. Armstrong, 'Reply to van Fraassen', *Australasian Journal of Philosophy*, 66 (1988), 224–9.
25. In more traditional perspective, this is to say that I opt for a voluntarist rather than an idealist refutation of scepticism. See the discussion of St Augustine's *Against the Academics*, in my 'The Peculiar Effects of Love and Desire', forthcoming in A. Rorty and B. McLaughlin (eds.), *Perspectives on Self-Deception*, (Los Angeles, Calif.).

Chapter 8

1. My views on explanation are mainly presented in *The Scientific Image*, ch. 5; in 'Salmon on Explanation', *Journal of Philosophy*, 82 (1985),

639–51; and in '*Was ist Aufklärung?*' in G. Schurz (ed.), *Erklären und Verstehen* (Munich, 1988). For critique, see e.g. K. Lambert and G. Brittan, *An Introduction to the Philosophy of Science*, 3rd edn. (Atascadero, Calif., 1987), and P. Kitcher and W. Salmon, 'Van Fraassen on Explanation', *Journal of Philosophy*, 84 (1987), 315–30.
2. 'Rationalism and Empiricism: An Inquiry into the Roots of Philosophical Error', in H. Reichenbach, *Modern Philosophy of Science* (New York, 1959).
3. H. Weyl, 'The Ghost of Modality', in M. Farber (ed.), *Philosophical Essays in Memory of Edmund Husserl* (Cambridge, Mass., 1940), 278–303.
4. MS, circulated spring 1987; forthcoming with the University of Illinois Press.
5. Urbana, Ill., 1974.
6. New York, 1979.
7. Chicago, 1988.
8. New York, 1970; 2nd edn. with new preface and postscript, 1985.
9. In S. Morgenbesser (ed.), *Philosophy of Science Today* (New York, 1967).
10. Westport, Conn., 1988.
11. Albany, NY, 1988.
12. J. Beattie, 'What's Wrong with the Received View of Evolutionary Theory', in P. Asquith and R. Giere (eds.), *PSA 1980*, ii (East Lansing, Mich., 1981), 397–426.
13. See W. Stegmüller, *The Structuralist View of Theories* (Berlin, 1979); C. U. Moulines, 'Approximate Application of Empirical Theories', *Erkenntnis*, 10 (1976), 201–27. For comparisons see A. R. Perez Ransanz, 'El concepto de teoría empirica según van Fraassen' (with English tr.), *Critica*, 17 (1985), 3–20 and my reply, 'On the Question of Identification of Scientific Theory', ibid. 21–30. In the actual analysis of scientific theories, the structuralist and semantic approach proceed in much the same way, except that the former tends to be more formal. An interesting approach that shares characteristics with both semantic and structuralist views has been developed by Erhard Scheibe; see e.g. his 'On the Structure of Physical Theories', *Acta Philosophica Fennica*, 30 (1978), 205–23.
14. See my 'Empiricism in the Philosophy of Science', sect. I. 6.
15. For an excellent study of this option see J. Hanna, 'Empirical Adequacy', *Philosophy of Science*, 50 (1983), 1–34.
16. This last sentence is not just provocative, but rejects the reality of objective chance. It is not an account of chance for it does not respect the logic of that notion.

17. For a review of the literature and a careful diagnosis of the fallacy involved in the paradox, see R. C. Jeffrey, *Journal of Symbolic Logic*, 35 (1970), 124–7. See also D. Lewis, 'A subjectivist's Guide to Objective Chance', in W. Harper *et al.* (eds.), *Ifs* (Boston, Mass., 1981), 267–98 where several of the following points are clearly made.
18. See my 'A Temporal Framework for Conditionals and Chance'.
19. By 'fully believe' I mean that I give it subjective probability 1; in this sense I fully believe that the mass of the moon in kilograms is not a rational number. Some further distinctions are clearly necessary, and can be made e.g. in terms of Popper or Renyi functions—I'll leave this aside here.
20. It is not even easy to apply it to our own actions. Could I not be rational, though certain that I will do something if circumstances allow, and yet believe that it is not a physically settled fact that I shall (i.e. believe that I can do otherwise)?
21. See R. C. Jeffrey, *The Logic of Decision*, section 12.7, and 'Choice, Chance, and Credence', in G. Floistad and G. H. von Wright (eds.), *Philosophy of Language/Philosophical Logic* (The Hague, 1981), 367–86. This reflection is at the heart of De Finetti's and cognate reconstructions of talk about objective chance.
22. See ch. 6 of *The Scientific Image*.
23. C. Gaifman, 'A theory of higher order probability', in B. Skyrms *et al.* (eds.), *Causality, Chance, and Choice* (Dordrecht, 1988).
24. See the discussion of calibration in my 'Belief and the Will'.
25. My 'voluntarist' resolution here of the problem of why the probabilities in accepted scientific theories constrain personal expectation in this way, seems to me at least similar, and perhaps the same, as that sketched by Putnam in the closing passages of *The Many Faces of Realism*.
26. For this topic and applications, see further B. Skyrms, 'Conditional chance', in J. Fetzer (ed.), *Probabilistic Causation: Essays in Honor of Wesley C. Salmon* (Dordrecht, 1988).
27. We can replace the simplifying assumption 2 by a much more general one, allowing mixtures to be made by integration instead of finite sum. There are still some limitations. In the case of a well-formulated physical theory, if not a human expert, we will have an exact mathematical description of the range of probability functions it allows.
28. See T. Seidenfeld, J. B. Kadane, M. J. Schervish, 'On the Shared Preferences of two Bayesian Decision Makers' MS, circulated 1987, and J. Broome, 'Bolker-Jeffrey Decision Theory and Axiomatic Utilitarianism', MS, 1988.
29. There is a voluminous literature by Skyrms, Cartwright, Lewis, Harper and Gibbard, and others; D. Lewis, Causal Decision Theory', *Australian Journal of Philosophy*, 59 (1981), 5–30, and B. Armendt, 'A Foundation for Causal Decision Theory', *Topoi*, 5 (1986), 3–19, contain references to the rest of the literature.

30. J. Worrall, 'An Unreal Image', Review of van Fraassen (1980), *British Journal for the Philosophy of Science*, 35 (1984), 65–80.
31. This answers a question posed in another review, the one by M. Friedman, *Journal of Philosophy*, 79 (1982), 274–83; see also P. M. Churchland and C. A. Hooker (eds.), *Images of Science* (Chicago, 1985), 302–3.
32. See R. A. Rynasiewicz, 'Falsifiability and Semantic Eliminability', *Brit. J. Phil. Sci.*, 34 (1983), 225–41, and his Ph.D. dissertation, University of Minnesota, 1981; see further J. Earman, *A Primer on Determinism* (Dordrecht, 1986), 105–6.
33. See my 'The World We Speak of and the Language We Live in', in *Philosophy and Culture: Proc. of the XVII World Congress of Philosophy at Montreal*, 1983 (Montreal, 1986), 213–21.
34. It should be added however that I soon found it much more advantageous to concentrate on the propositions expressible by elementary statements, rather than on the statements themselves. This is how my emphasis changed progressively in my articles on logical aspects of quantum mechanics, from 1968 onward. At later points there is not even a bow in the direction of syntactic description.
35. This is a quick sketch of my attempts to make sense of modality without metaphysics. See ch. 6 of *The Scientific Image*, and my articles 'The Only Necessity is Verbal Necessity', Essence and Existence', and 'Essences and Laws of Nature'. See also R. Stalnaker, 'Anti-Essentialism', in P. French *et al.* (eds.), *Midwest Studies in Philosophy*, iv (Minneapolis, 1979), 343–55.

Chapter 9

1. C. B. Daniels and S. Todes, 'Beyond the Doubt of a Shadow: A Phenomenological and Linguistic Analysis of Shadows', in D. Ihde and R. M. Zaner (eds.), *Selected Studies in Phenomenology and Existential Philosophy* (The Hague, 1975), 203–16.
2. A model consists, formally speaking, of entities and relations among those entities. Not all parts are intended in this description of empirical adequacy. For example, a non-Euclidean space might be ismorphic to some part of a Euclidean space, if we allowed the introduction of new relations to single out this 'substructure'. That is not meant.
3. I use the word deliberately: it was a tragedy for philosophers of science

to go off on these logico-linguistic tangles, which contributed nothing to the understanding of either science or logic or language. It is still unfortunately necessary to speak polemically about this, because so much philosophy of science is still couched in terminology based on a mistake.

4. The impact of Suppes's innovation is lost if models are defined, as in many standard logic texts, to be partially linguistic entities, each yoked to a particular syntax. In my terminology here the models are mathematical structures, called models of a given theory only by virtue of belonging to the class defined to be the models of that theory.
5. Unlike perhaps Giere, I take it that normally the asserted relation of real systems to members of the defined class is not identity but some embedding or approximate embedding. See the Postscript to the 2nd edn. of my *An Introduction to the Philosophy of Time and Space* (New York, 1985).
6. For this topic, and for a sensitive analysis of the relations between state-spaces, parameters, and laws see E. Lloyd, *The Structure and Confirmation of Evolutionary Theory* (Westport, Conn., 1988).
7. See B. Ellis, 'The Origin and Nature of Newton's Laws of Motion', in R. Colodny, *Beyond the Edge of Certainty* (Englewood Cliffs, NJ, 1965), 29-68, and J. Earman and M. Friedman, 'The Nature and Status of Newton's Laws of Inertia', *Philosophy of Science*, 40 (1973), 329-59.
8. See M. Przelewski, *The Logic of Empirical Theories* (London, 1969); R. Wojcicki, 'Set Theoretic Representations of Empirical Phenomena', *Journal of Philosophical Logic*, 3 (1974), 337–43; M. L. Dalla Chiara, and G. Toraldo di Francia, 'A Logical Analysis of Physical Theories', *Rivista de Nuovo Cimento*, Serie 2, 3 (1973), 1-20; and 'Formal Analysis of Physical Theories', in G. Toraldo di Francia (ed.), *Problems in the Foundations of Physics* (Amsterdam, 1979); F. Suppe, 'Theories, The Formulations and the Operational Imperative', *Synthese*, 25 (1972), 129-59; P. Suppes, 'What is a Scientific Theory?', S. Morgenbesser (ed.) in *Philosophy of Science Today* (New York, 1967), 55-67; and 'The Structure of Theories and the Analysis of Data', in F. Suppe (ed.), *The Structure of Scientific Theories* (Urbana, Ill. 1974) 266-83.
9. I have discussed this further, with examples, in 'Theory Construction and Experiment: an Empiricist View', in P. Asquith and R. Giere (eds.), *PSA 1980*, ii (East Lansing, Mich., 1981), 663-78.
10. These reflections clearly bear e.g. on Glymour's theory of testing and relevant evidence, and his use of this important and original theory in arguments concerning scientific realism; see C. Glymour, *Theory and Evidence* (Princeton, NJ, 1980) and J. Earman (ed.), *Testing Scientific Theories, Minnesota Studies in the Philosophy of Science*, x (Minneapolis, 1983). See also D. Baird, 'Tests of Significance Violate the Role of Implication', in P. Kitcher and P. Asquith (eds.), *PSA 1984*, (East Lansing, Mich., 1985), 81-92, esp. sect. 4.

Chapter 10

1. Taken from G. E. Martin, *Transformation Geometry* (New York, 1982), ch. 4. It is related to the argument, already given in ancient times, for the optical law of reflection; see ch. 1 sect. 4. The two alternative ways of solving the problem, by differentiation and by symmetry, and also its relation to Fermat's reasoning about optical reflection, are presented fully in A. Ostrowski, *Differential and Integral Calculus*, i. 318-19 (Glenview, Ill., 1968).
2. For general discussions see H. Weyl, *Symmetry* (Princeton, NJ, 1952) and J. Rosen, *Symmetry Discovered* (Cambridge, 1975).
3. See my *An Introduction to the Philosophy of Time and Space*, ch. 4 sect. 4b.
4. *Collected Works of Charles Sanders Peirce*, v, (Cambridge, Mass., 1964), 45-6.
5. See my *An Introduction to the Philosophy of Time and Space*, ch. 3 sect. 3.
6. Celsius originally made 100° the freezing point and 0° the boiling point; see P. van der Star (ed.), *Fahrenheit's Letters to Leibniz and van Boerhaave* (Amsterdam, 1983), p. 28 n. 1. I want to thank James Lenard for this reference.
7. Lucretius, *On the Nature of the Universe*, tr. R. E. Latham (New York, 1985), bk. ii, p. 66.
8. B. Russell, 'On the Notion of Cause with Applications to the Free Will Problem', in H. Feigl and M. Brodbeck, *Readings in the Philosophy of Science* (New York, 1953).
9. 'Deterministic Theories', in R. H. Thomason (ed.), *Formal Philosophy: Papers by R. Montague* (New York, 1974).
10. Dordrecht, 1986. I want to thank Roger Cooke for a helpful discussion.
11. Is the imagined world deterministic or indeterministic by our account? The question is elliptical: it applies only to the world classified as a certain kind of system. The kind of system described by classical physics minus conservation of mass and energy, this example shows us, is indeterministic.
12. The example also violates what Poincaré called the hypothesis of central forces, that is, the eighteenth-century idea that every force can be regarded as being exerted by some body or bodies. Here the

deceleration corresponds to a force not apparently covered by that hypothesis.

Chapter 11

1. I want to thank my student James Lenard for helpful comments on this chapter.
2. See further G. E. Martin, op. cit.
3. See the exposition and criticism by Ernst Mach, *The Science of Mechanics*, tr. T. McCormack (LaSalle, Ill. 1942), ch. 1 sect. 3.
4. G. D. Birkhoff, *Collected Mathematical Papers* (New York, 1950) ii. 890-9; iii. 788-804.
5. See E. Mach, *The Science of Mechanics*, ch. 3 sect. 3 for a full account.
6. See also J. C. C. McKinsey and P. Suppes, 'On the Notion of Invariance in Classical Mechanics', *British Journal for the Philosophy of Science*, 5 (1955), 290-302.
7. *Theory of Groups and Quantum Mechanics* (New York, 1931), ch. 3 sect. 14.
8. *Concepts of Mass* (New York, 1961), ch. 12.
9. In retrospect it is easy to see that some of Carnap's attempts to reformulate logical theory were inspired by the use of these notions in physics. His *logically determinate* corresponds to our *covariance*; and he had two explications for it. The first and main one was semantic: such a statement is either true for all interpretations or true for none. The second was partly syntactic: the result of syntactically transforming the statement by a uniform substitution (i.e. not just x for y but simultaneously also y for x) has always the same truth-value of the original. A third criterion which is much closer to our present usage would be obtained if we characterized transformations of interpretations (logical models) and defined the character in question as preservation of the truth value under all such transformations. The notion of covariance is certainly essentially a logical one, if applied to propositions; it betokens a certain kind of generality which amounts, in the extreme case, to the character of being either tautologous or self-contradictory. However, this extreme case is reached only if the group of transformations is so large as to preserve only logical structure.
10. See J. C. C. McKinsey, A. C. Sugar, and P. Suppes, 'Axiomatic Foundations of Classical Particle Mechanics', *Journal of Rational Mechanics and Analysis*, 2 (1953), 253-72.
11. Compare J. Aharoni, *Lectures on Mechanics* (Oxford, 1972), 290-304.

12. $\delta H/\delta t$ is also discarded at this point. See Aharoni, *Lectures on Mechanics*, 295.
13. *Symmetry* (Princeton, NJ, 1952), 26-7.
14. The concept of generality has a logical fascination all its own; in 'Essence and Existence' I have attempted to show how permutation symmetry helps to explicate it (and its contrary, the relation of being peculiarly about something specific), in modal semantics.

Chapter 12

1. I want to thank Mr Moore for allowing use of this example, and Dorothy Edgington for telling me about it.
2. See I. Todhunter, *A History of the Mathematical Theory of Probability* (London, 1865), 222-3.
3. See L. E. Maistrov, *Probability Theory: A Historical Sketch* (New York, 1974), 118-19.
4. See I. Todhunter, op. cit., 491-4.
5. In the Euclidean plane, a hyperbola is described by an equation of form $(x^2/a^2) - (y^2/b^2) = 1$, an ellipse by $(x^2/a^2) + (y^2/b^2) = 1$, and a parabola by $y^2 = 2px$.
6. I. Hacking, 'Equipossibility Theories of Probability', *British Journal for the Philosophy of Science*, 22 (1971), 339-55; K.-R. Bierman and M. Falk, 'G. W. Leibniz' *De incerti aestimatione*', *Forschungen und Fortschritte*, 31 (1957), 168-73; Leibniz, *Opuscules et fragments inédits*, ed. L. Couturat (Paris, 1903), 569-71.
7. Buffon's needle problem is discussed in many probability texts (e.g. J. V. Uspensky, *Introduction to Mathematical Probability* (New York, 1937) and in the standard histories of probability. It appeared in G. Buffon's supplement to his Natural History, *Essai d'arithmétique morale*. See further E. F. Schuster, 'Buffon's Needle Experiment', *American Mathematical Monthly*, 81 (1974), 26-9 and for a survey, H. Solomon, *Geometric Probability* (Philadelphia, 1978), ch. 1.
8. Results of the experiment are described in M. G. Kendall and P. A. P. Moran, *Geometrical Probability* (London, 1963). For serious doubts as to the reliability of the actual experiments, see N. T. Gridgeman, 'Geometric Probability and the Number π', *Scripta Mathematica*, 25 (1960), 183-95. The number of trials required according to Gridgeman is of the order of 90.10^{2n} for precision to n decimal places.
9. *Calcul des probabilités* (Paris, 1889), 4-5; 2nd edn. 1907, 4-7 (reprinted as 3rd edn., New York, 1972). See further the discussion of Bertrand's book in section 12.6 below.

10. Bertrand himself stated the problem in roughly this form: the problem of choosing a number at random from [0, 100] is the same as that of choosing its square (*Calcul des probabilités*, 2nd edn., 4). He adds that these contradictions can be multiplied to infinity. His own conclusion is that when the sample space is infinite, the notion of choosing at random '*n'est pas une indication suffisante*'—presumably not sufficient to create a well-posed problem.
11. See E. T. Jaynes, 'The Well-Posed Problem', *Foundations of Physics*, 3 (1973), 477–92, which has references to preceding discussions.
12. The concept of measure will be discussed more formally in the next chapter. Note here that a measure assigns non-negative numbers and is additive.
13. See ch. 13 sect. 3.
14. The probabilities must be the same for the events ($a \leqslant x \leqslant b$) and ($ka \leqslant y \leqslant kb$), so we deduce:

$$\int_0^b f(x)\ \mathrm{d}x = \int_0^{kb} f(kx)\ \mathrm{d}(kx) \text{ for all } b$$

hence $f(x) = kf(kx)$, for any positive constant k. This equation has a unique solution up to a constant multiplier:

$$f(x) = (1/x)$$

This gives us the basic measure:

$$M(a \leqslant x \leqslant b) = \mathrm{K}(\log b - \log a)$$

because $(1/x)$ is the derivative of $\log x$ (natural logarithm).
15. R. D. Rosenkranz, *Inference, Method and Decision* (Dordrecht, 1977), 63–8. See also R. D. Rosencrantz, *Foundations and Applications of Inductive Probability* (Atascadero, Calif., 1981), sects. 4.2 and 4.1.
16. A discussion of Buffon's needle along these lines is provided by M. Kac, E. R. van Kampen, and A. Winter, 'On Buffon's Problem and its Generalizations', *American Journal of Mathematics*, 61 (1939), 672–4.
17. P. Milne, 'A Note on Scale Invariance', *British Journal for the Philosophy of Science*, 34 (1983), 49–55.
18. Despite some rhetoric that seems to express the wish it were not so, Jaynes's article really agrees. Specifically, he implies that to treat a problem as solvable by symmetry considerations is to assume—what might be empirically false—that all relevant factors have been indicated in the statement of the problem ('The Well-Posed Problem', 489). Thus to treat a specific problem that way can itself not be justified a priori; the solution is correct for reality only conditional on that substantial assumption.
19. Ibid. 477–92.

20. E. T. Jaynes, *Papers on Probability, Statistics and Statistical Physics*, ed. R. Rosencrantz (Dordrecht, 1983), 128.
21. See the concise, perspicuous exposition in A. P. Dawid, 'Invariant Prior Distributions', in S. Kotz and N. L. Johnson, *Encyclopedia of Statistical Sciences* (New York, 1983), 228–36. The main figures in the search for 'invariant priors' besides Jaynes were H. Jeffreys's classic text *Theory of Probability* (Oxford, 1939), and D. Fraser (see e.g. his 'The Fiducial Method and Invariance', *Biometrica*, 48 (1961), 261–80).
22. See E. T. Jaynes, 'Prior Probabilities', *IEEE Transactions of the Society of Systems Sciences Cybernetics* SSC-4 (1968), 227–41; C. Villegas, 'On Haar Priors', in V. P. Godambe *et al.* (eds.), *Foundations of Statistical Inference* (Toronto, 1971), 409–14; 'Inner Statistical Inference', *Journal of the American Statistical Association*, 72 (1977), 453–8 and *Annals of Statistics*, 9 (1981), 768–76. I want to thank Dr F. G. Perey, of the Oak Ridge National Laboratory, for letting me have a copy of his excellent and insightful presentation of this approach, 'Application of Group Theory to Data Reduction', Report ORNL-5908 (Sept. 1982).
23. The 'nice properties' referred to in the text are topological properties of the group; if it is locally compact and transitive (for any y and z in the set there is a member g of G such that $g(y) = z$) then the left Haar measure is unique up to a multiplicative constant. However, in order for P to be also independent of the choice of reference point x_0, the left and right Haar measure must be the same; this is guaranteed if the group is compact.

Chapter 13

1. 'A Note on Jeffrey Conditionalization', *Philosophy of Science*, 45 (1978), 361–7.
2. See my 'Rational Belief and Probability Kinematics', *Philosophy of Science*, 47 (1980), 165–87.
3. I. Hacking, 'Slightly More Realistic Personal Probability', *Philosophy of Science*, 34 (1967), 311–25.
4. Cf. F. P. Ramsey, 'Truth and Probability', repr. in H. E. Kyburg Jun. and H. E. Smokler (eds.), *Studies in Subjective Probability* (Huntingdon, NY, 1980), 23–52; 3 p. 40.
5. See my 'Rationality does not Require Conditionalization', forthcoming.
6. I think of time here as discrete, but the unit can of course be chosen as small as you like. The proposition $E(t)$ can be identified as follows: it is the logically strongest proposition X in the domain of P such that $P(t)(X) = 1$. Note that we must take into account also the case of someone who gives positive probability to a proposition which he

gave probability *zero* before. That case is left aside here but discussed in 'Rationality does not Require Conditionalization', where it is shown that nothing very advanced is needed to substantiate the assertion that such a person as here described can always be simulated by a perfect Conditionalizer.

7. For this history, I am especially indebted to G. H. Moore, 'The Origins of Zermelo's Axiomatization of Set Theory', *Journal of Philosophical Logic*, 7 (1978), 307–29; and 'Lebesgue's Measure Problem and Zermelo's Axiom of Choice: the Mathematical Effects of a Philosophical Dispute', *Annals of the New York Academy of Sciences* (1983), 129–54. The Measure Problem is stated in H. Lebesgue, 'Intégrale, longeur, aire', *Annali di Mathematica Pura ed Applicata*, 3 (1902), 231–359.
8. We may note in passing that Banach proved in 1923 that the Measure Problem does have solutions for dimensions 1 and 2 provided we settle for finite additivity (measure functions, as I called them). But he also proved a much stronger negative result for measures properly speaking: quite aside from requirements of congruence, there cannot be a measure defined on all subsets of [0, 1] which (like Lebesgue measure) gives zero to each point (i.e. to each unit set $\{x\}$).
9. The crucial results appealed to in the following are Theorems of Kuratowski, Birkhoff, and Horn, Tarski, and Maharam; see G. Birkhoff, *Lattice Theory*, 3rd edn. (Providence, RI, 1967); and D. A. Kappos, *Probability Algebras and Stochastic Spaces* (New York, 1969), chs. 2. 4 and 3. 3. For a more extensive discussion focusing on the relation between probabilities and frequencies, see my 'Foundations of Probability: A Modal Frequency Interpretation', in G. Toraldo di Francia (ed.), *Problems in the Foundations of Physics* (Amsterdam; 1979), esp. 345–65.
10. The following argument was first given in a different setting, see my 'A Demonstration of the Jeffrey Conditionalization Rule', *Erkenntnis*, 24 (1986), 17–24, 'Symmetry Arguments in Probability Kinematics' (with R. I. G. Hughes), in P. Kitcher and P. Asquith (eds.), *PSA 1984*, 851–69 (East Lansing, Mich., 1985), and 'Symmetries in Personal Probability Kinematics', in N. Rescher (ed.), *Scientific Inquiry in Philosophical Perspective* (Lanham, Md., 1987).
11. See further my papers cited earlier in this chapter, and also my 'Discussion: A Problem for Relative Information Minimizers', *British Journal for the Philosophy of Science*, 32 (1981), 375–9, and 'A Problem for Relative Information Minimizers in Probability Kinematics, Continued' (with R. I. G. Hughes and G. Harman), *British Journal for the Philosophy of Science*, 37 (1986), 453–75.
12. At this point the argument follows that of P. Teller and A. Fine, 'A Characterization of Conditional Probability', *Mathematical Magazine*, 48 (1975), 267–70.

13. For the proof of convergence, see John Collins's Appendix to my 'Symmetries in Personal Probability Kinematics'.
14. See van Fraassen 'A Problem for Relative Information Minimizers'; van Fraassen, Hughes, and Harman, 'A Problem for Relative Information Minimizers, Continued'; van Fraassen 'Symmetries in Personal Probability Kinematics'. These articles also contain the calculations omitted below.
15. R. D. Levine, and M. Tribus, *The Maximum Entropy Formalism* (Cambridge, Mass., 1979); J. E. Shore and R. W. Johnson, 'Axiomatic Derivation of the Principle of Maximum Cross-Entropy', *IEEE Transactions Information Theory*, IT-26 (1980), 26–37; J. Skilling, 'The Maximum Entropy Method', *Nature*, 309 (28 June 1984), 748–9; Y. Tikochinsky, N. Z. Tishby, and R. D. Levine, 'Consistent Inference of Probabilities for Reproducible Experiments', *Physical Review Letters*, 52 (1984), 1357–60.
16. P. Diaconis and S. Zabell, 'Updating Subjective Probability', *Journal of the American Statistical Association*, 77 (1982), 822–30.
17. Levine and Tribus, op. cit.
18. For this problem, see A. Hobson, *Concepts in Statistical Mechanics* (New York, 1971), 36, 42, 49.
19. See P. M. Williams, 'Bayesian Conditionalization and the Principle of Minimum Information', *British Journal for Philosophy of Science*, 31 (1980), 131–144.
20. R. W. Johnson. 'Axiomatic Characterization of the Directed Divergences and Their Linear Combinations', *IEEE Transactions Information Theory*, IT-25 (1979), 709–16.
21 K. Friedman and A. Shimony, 'Jaynes' Maximum Entropy Prescription and Probability Theory', *Journal of Statistical Physics*, 3 (1971), 381–4. See also A. Shimony, 'Comment on the Interpretation of Inductive Probabilities', ibid. 9 (1973), 187–91.
22. For the most sensitive treatment so far, see B. Skyrms, 'Maximum Entropy as a Special Case of Conditionalization', *Synthese*, 636 (1985), 55–74, and 'Updating, Supposing and MAXENT', forthcoming.

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