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Explanation and determination

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Chapter 10

Conclusions

In this chapter, I will summarise the conclusions drawn over the course of this dissertation.

In chapter 2, we first considered the definitions of unification given by the unificationist theories of explanation. Kitcher's proposed definition faces serious difficulties, since it cannot solve the problem of spurious unification; the theory of Schurz and Lambert is more successful in this respect. As unificationist theories of explanation, however, both Kitcher's and Schurz and Lambert's theory are unsatisfactory. They do not appear to give sufficient conditions for explanation, as they cannot generate the notions of causality and lawhood which many believe to be important to characterise explanatory power. Moreover, it was seen that unification is not necessary for explanation, since explanations can have a disunifying instead of a unifying effect. I proposed that the only reason unifying explanations are deemed preferable is that we are often more justified in believing their premises.

We then considered the topic of Inference to the Best Explanation in chapter 3. My general argument was that explanatory loveliness (greater explanatory power *if* true) is not an indicator of truth; that there is not, in other words, a valid inference from high explanatory power to high probability. In the course of the discussion we saw three arguments for IBE: the first claims that there are explanatory virtues which are also epistemic virtues, thus linking loveliness to likeliness; the second claims that we can simply see IBE at work in specific instances of scientific research; and the third claims that IBE has a role to play within the framework of Bayesianism. We then saw that all these three arguments fail – or at least, that they have not yet been adequately defended. What is more, the doubts raised in the chapter lend credence to the idea that a successful defence of IBE cannot be found.

However, it also became clear that explanatory considerations play an essential role in the methodological decisions of scientists. No theory of

scientific method which does not take account of both the importance of explanation and the structure of explanations will be able to make sense of what scientists actually do.

With unificationism out of the way, full room was given in chapter 4 to the causal theory of explanation – more specifically, to James Woodward’s version. I discussed his theory and defended it from the charge of circularity brought against it by De Regt and Glymour. We then discussed the topic of mathematical explanation, including Steiner’s theory. We saw that Steiner’s theory has several weaknesses, and that a more satisfactory theory of mathematical explanation can be constructed by taking the formal structure of Woodward’s theory, replacing the notion of causation by that of ground, and indicating how the notion of ground is to be understood within mathematics. We then repeated this procedure, though at less length, for explanations of laws of nature by other laws of nature: here we saw again that Woodward’s formalism could be adapted to encompass non-causal explanations, this time by drawing on our intuitive judgments about counterfactual relations between laws. At the end of the chapter, a general interventionist theory of explanation was given. In the context of the thesis, this theory functions as something of a preliminary version of the determination theory.

In chapter 5, we saw that the phrase “rather than” can be used in two different ways within explanatory requests: it can be used either to specify a contrast class, or to set up a contrast of parallels. Making a sharp distinction between contrast classes and contrasts of parallels allowed us to show that the apparent counterexamples to the theory that all explanations are contrastive – in the sense that they all involve a contrast class in the explanandum – were in fact no counterexamples at all. I argued that with these counterexamples removed, the contrastive theory must be preferred to the conjunctive theory.

In the second half of the chapter, we saw that not only the explanandum, but also the explanans must be understood as containing a contrast class. I argued that the resulting double-contrast theory should be acceptable to theorists favouring a variety of approaches to explanation; they can all agree that an explanation consists of a contrastive explanans and a contrastive explanandum, while disagreeing about the nature of the relation that holds between them. I also showed that at least one puzzle of explanation – why irrelevant information sometimes (and only sometimes) invalidates explanations – can be solved by the double-contrast theory.

Chapter 6 presented the determination theory. I first argued for the centrality of the notion of determination to our ideas of explanation: wherever we look, we find that A explains B only if A determines B to be the case. Combining this insight with the previously defended claims about contrasts and intervention led to the claim that an explanation is a true deductive

argument where both the conclusion and at least one of the (non-redundant) premises have the form of a fact embedded in a contrast class of alternatives, and where intervening on those facts in the premises changes the fact in the conclusion. This informal claim was formalised in section 6.3. Several elements of the determination theory were then defended, but not the important claim that all explanation is a matter of determination and can be put into the form of a deductive argument.

Defending this claim was the aim of chapters 7 and 8. In chapter 7, I showed that a large class of seemingly non-deductive explanations can be reconstructed as instantiations of the determination theory, specifically, of a model of the determination theory that I called the non-specific deductive or ND model. We used this model in our discussion of two debates about probability and explanation: the size debate and the change debate. The determination theory and the ND model automatically save the intuitions of both sides in the size debate. We can also make excellent sense of the conflicting intuitions in the change debate if we assume that (a) even abbreviated explanations must mention one of the contrastive premises, and (b) making the non-specific premise in an explanation more specific increases understanding. Both these assumptions are plausible. I concluded that the determination theory can successfully account for the role of probability in explanations of deterministically produced events.

The rest of the chapter argued that the determination theory, which obviously cannot handle indeterministic explanation, doesn't have to: in cases of pure indeterminism, we do not have explanations. There is no phenomenon here for the determination theory to save.

Chapter 8 went over some of the same ground, this time discussing a set of arguments recently brought forward by Michael Strevens. I showed that statistical deterministic explanation does not exist. This defeated the argument that the low-level explanation is a bad explanation because it contains many irrelevancies. I then argued that the low-level deterministic explanation is superior to the indeterministic one by showing, first, that critical events are always explanatory, and, second, that Strevens's account violates the principle of deductive closure. This means that we still have good reasons to believe that the deterministic explanation, which mentions more difference makers than the statistical alternative, is the most explanatory explanation – exactly as the determination theory would have it.

The final chapter, 9, saw us discuss a variety of topics. We concluded that all explanations are arguments, but that this does not have any of the bad philosophical consequences it was supposed to have; that explanations need not involve laws, and can be redescriptions; that as long as there is no referential ambiguity, explanations of the same phenomenon can be com-

bined into larger explanations; that the distinction between explanation and understanding does not invalidate my method; that the determination theory suggests a way to see the link between *Erklären* and *Verstehen*; that an explanation has non-zero explanatory power if it implies counterfactuals (of the right kind) that were not implied by the explanatory request; and that this final idea points to notions of intervention that might, in the final analysis, not be purely objective.

Let me end by reiterating the hope expressed in the introduction, that this thesis has contributed to the philosophy of explanation, first, by presenting a theory of explanation that is better than the ones we already have; and second, by adding new and interesting arguments to the discussions on unification, IBE, mathematical explanation, contrastive explanation, deterministic versus non-deterministic explanation, and several other topics. Much work remains to be done, but some progress has, I hope, been made.