Acting against one's best judgement
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"Dispositions are as shameful in many eyes as pregnant spinsters used to be - ideally to be explained away, or entitled by a shotgun wedding to take the name of some decently real categorical property. It is time to remove this lingering Victorian prejudice. Dispositions, like unmarried mothers, can manage on their own. They have been traduced, and my object here is to restore their good name."

Had I been writing well over twenty years ago, and were I an accomplished writer, I might well have begun the present chapter thus. As it is, I borrow someone else’s words, namely Mellor’s preamble to his ‘In Defense of Dispositions’ (Mellor 1974). Like Mellor, I believe that for some decennia dispositions have been unjustly reviled and defamed; like Mellor, I hold this to be short-sighted and damaging. I, too, would have it that dispositions regain their good name, in much the same way as unmarried mothers by now, in 1996, finally have regained theirs.

The barricade on which I fight for dispositions, however, differs from that on which Mellor stands. Mellor’s defense of dispositions springs from his conviction that dispositions are in fact "real properties" (cf. also Mellor 1983, especially 292). As a rule, dispositions are sharply separated from real properties. Whereas a disposition only tells us how something will behave in such-and-such a situation, a real property is said to reveal how the thing is "in itself"; the idea is that properties are somehow more solid or more substantial than dispositions. Mellor, however, deems this demarcation to be incorrect. He thinks that dispositions are real properties which have not yet been identified. If we say that $x$ is fragile and that it, therefore, will behave so-and-so then-and-then, we indicate that $x$ has a physical property - called fragility until further notice - which causes $x$’s behaviour. It is for physicists in particular to identify that property, and in doing so they must at first rely on dispositional predicates (Mellor 1974, 70). Whether or not the latter observation is correct, Mellor’s point is clear enough: if dispositions themselves are real properties, they do not need a shotgun wedding to make
Contrary to Mellor, however, I do not believe that dispositions must be properties in the strict sense of the word. In my view, a disposition may well be a property in Mellor’s sense, but it does not have to be one in order to count. Dispositions have a significance of their own; whether or not they can also be described as real properties is quite another matter. I return to this point in Chapter VIII.

In my attempt to thrash out the *akrasia* problem, I shall reconstruct beliefs and desires as dispositions. This may sound paradoxical, for did Chapters II and III not suggest that the dispositional approach generates *akrasia* as a problem? How, then, could this approach ever tackle it? The answer is simple. The dispositional approach is encountered in several different modes, and in Chapters II and III I examined only one of them, to wit the Hempelian variant. The latter runs into the problem of *akrasia*, but of course it does not follow that all versions do. I think that in particular my version of the dispositional approach, to be explained later, circumvents the problem.67

I will even go one step further. Not only shall I follow Hempel in treating beliefs and desires as dispositions, I will also follow suit by describing those dispositions in terms of reduction sentences à la Carnap. Whereas my first step still can be regarded as a perhaps fairly common move, the second one may sound surprising. Why use reduction sentences in describing dispositions? After all, in reduction sentences conditionals are framed as generalised material implications, which generate notorious paradoxes. Therefore, it is often argued, an adequate formulation of dispositions requires a possible worlds semantics rather than a classical approach. Notably, a subjunctive conditional couched in a modal apparatus has been labelled as the proper representation of dispositions. Wesley Salmon even went so far as to suggest that subjunctive conditionals form the final solution to all problems concerning dispositions:

"It is now generally recognized, I believe, that Carnap’s reduction sentences, ingenious as they are, do not handle the problem of dispositional concepts, for the dispositional predicate can only be meaningfully applied if one or

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67 For attempts to solve the *akrasia* problem by parting with the dispositional approach altogether, see for instance Jeffrey 1974 and Holström-Hintikka 1991.
another of the test conditions - for example, having iron filings in the vicinity or being moved through a conducting coil - is satisfied. The problem of dispositional terms is precisely that of specifying conditions for their applicability when no test conditions happen to obtain. It appears that a subjunctive conditional is what we need for this purpose. Thus, the problem of dispositional concepts falls neatly into place along with the problems of counterfactuals, laws, and modalities." (Salmon 1976, xvii).

However, I am doubtful whether Salmon’s resolute optimism is justified. Quine, to mention one dissident view, has offered a well-known chain of objections to modal reasoning: modal logic is guilty of confusing ‘use’ and ‘mention’, modal notions are either empty or simply disguised extensional concepts, modal operators are referentially opaque, and last but not least, thinking along modal lines brings along a platonistic ontology of possible worlds. Whatever one may think of Quine’s attacks on modalities, it certainly is an overstatement to say that the final word about dispositions and subjunctive conditionals has been spoken: subjunctive conditionals as the proper expressions of dispositions are still a matter of dispute, as are modal logic and the entire possible worlds semantics.

Doubts about subjunctive conditionals (although not about subjunctive statements in general) have been expressed also by John Pollock:

"Remarkably little has been written about dispositions in the last decade. I suspect that this is due largely to philosophers feeling that this problem has been solved, or at least successfully reduced to the problem of analyzing subjunctive conditionals. For example, it seems initially plausible to suppose that

(1) That liquid is flammable

is analyzable as:

(1*) If that liquid were heated, it would burn.

Thus the feeling is that the problem remaining is that of analyzing subjunctive conditionals, and there is really no point in further discussion of dispositions per se. We have presented an analysis of subjunctive conditionals, but
unfortunately we cannot rest content that we have thereby solved the problem of dispositions. The traditional view according to which dispositional statements are analyzable on the model of \((1^*)\) is completely and unalterably wrong." (Pollock 1976, 237).

Pollock believes that by far and away the biggest part of dispositions involves "probabilistic dispositions" and that only very few dispositions may be called "absolute" (Pollock 1976, 238). The difference between probabilistic and absolute dispositions, Pollock argues, is an important one that has not yet been made. That omission he considers to be "a remarkable oversight because, historically, those dispositions which have most interested philosophers have tended to be mental or psychological dispositions, and these are perhaps without exception of the probabilistic variety" (Pollock 1976, 238). Absolute and probabilistic dispositions are also called deterministic and indeterministic dispositions. Deterministic dispositions are, as Pollock says, "capabilities" that tell us "what definitely would happen under some circumstances" (Pollock 1976, 240, 238). Examples of such dispositions are being addictive, absorbant, fragile, soluble or magnetised. On the other hand, probabilistic dispositions are best regarded as "tendencies", which tell us "what would be likely to happen". Examples are being acquisitive, acrimonious, foolhardy, creative, or deceitful (Pollock 1976, 248). Pollock strongly advises against describing those tendencies in subjunctive conditionals; they rather should be expressed in what he calls statements of simple subjunctive indefinite probability (Pollock 1976, 250).68

\[ (s^*_*) \]:

\[ p_{J}(deg_{f}(x) \geq r | J(x)), \]

where \( J(x) \) means \('x is an action of John'\), \('deg_{f}(x)\) symbolises the degree to which \(x\) is foolhardy, and \('p_{J}^\prime\) is a probability function. As a whole, \((s^*\)\) expresses the measure of the extent to which John has the disposition foolhardiness; this measure is "the distribution of the probabilities of an act of John such that \(deg_{f}(x) \geq r\)", where \( r \) is a number not further specified by Pollock (Pollock 1976, 250).

\( (s^*\)\) is called a probability statement because it ascribes to John a probabilistic instead of an absolute disposition. It is called an indefinite probability statement because it does not report the probability of a proposition, but the probability of an event. In this

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68 The idea is the following. Consider the sentence

\( (s) \): 'John is foolhardy'.

Then according to Pollock, \((s)\) should be reconstructed as a simple subjunctive indefinite probability statement, viz.,

\( (s^*): \)

\[ p_{J}(deg_{f}(x) \geq r | J(x)), \]

where \('J(x)\) means \('x is an action of John'\), \('deg_{f}(x)\) symbolises the degree to which \(x\) is foolhardy, and \('p_{J}^\prime\) is a probability function. As a whole, \((s^*\)\) expresses the measure of the extent to which John has the disposition foolhardiness; this measure is "the distribution of the probabilities of an act of John such that \(deg_{f}(x) \geq r\)", where \( r \) is a number not further specified by Pollock (Pollock 1976, 250).

\( (s^*\)\) is called a probability statement because it ascribes to John a probabilistic instead of an absolute disposition. It is called an indefinite probability statement because it does not report the probability of a proposition, but the probability of an event. In this
case the event is an action; Pollock regards John’s foolhardiness as a function of his foolhardy actions, hence \((s^*)\) is about the probability that an (indefinite) action of John is foolhardy. Setting aside the idea that foolhardiness admits degrees, the latter probability might also be given by \(p(F(x) \mid J(x))\), where \(F(x)\) means ‘\(x\) is foolhardy’ (Pollock 1976, 189). Normally, this probability would be interpreted as a relative frequency, but Pollock vehemently opposes this interpretation; according to Pollock, the probability expressed by \(p(F(x) \mid J(x))\) is not equal to the amount of John’s foolhardy actions divided by the totality of his actions (Pollock 1976, 192). Rather, it is a subjunctive probability. It is not just about actual actions of John that proved to be foolhardy (as would be the case if it were a relative frequency), but about any action of John that would be foolhardy (Pollock 1976, 46). As Pollock phrases it with respect to the familiar case of flipping a coin: “For example, if we are flipping a coin, we would like to know not just what proportion of flips already made have resulted in heads, but how likely it is that a new flip would result in heads” (Pollock 1976, 235; my emphasis). Cf.: “[T]hese indefinite probability statements are subjunctive in character. They are not just about actual [actions of John], but also about physically possible [actions of John]” (Pollock 1976, 192). Finally, the subjunctive indefinite probability expressed by \((s^*)\) is called simple (rather than complex) because the subjunctive has the form ‘If it were true that \(J(x)\) for a new and different \(x\), then it would be true that \(F(x)\)’ (Pollock 1976, 233-235; cf. 25, 38, 221).

No doubt the most striking part in Pollock’s analysis is the rejection of relative frequency in favour of subjunctive probability. This rejection is based on the well-known argument that relative frequency does not make sense when the reference class is an infinite set (Pollock 1976, 189-192). If there are infinitely many \(x\) that have the property \(J\), we have to restrict ourselves to the examination of a finite sample and determine how many \(x\) in this sample also have \(F\). On the basis of the discovered ratio, \(F(x) / J(x)\), we can then construct a limit for relative frequency. However, the problem is that we might as well have chosen another finite sample which would have yielded quite another limit. Although the above argument against relative frequency is valid as it stands, it seems to lose its weight in Pollock’s hands. For in the case about which Pollock is speaking the reference class is not infinite: John is supposed to be a normal mortal human being, hence the series of his actions will come to an end. However, one might reply that Pollock’s point actually is another one. Pollock invokes his argument against relative frequency to emphasise that we are not interested in earlier actions of John that were foolhardy, but that we wish to know the probability of his next action to be foolhardy. In order to know the latter probability, we should consider the person’s actual situation as a whole. As Pollock says: “In judging whether a person is foolhardy, what we want to know is the probability of a new action of his taken without sufficient attention to his personal safety. In computing this probability, we should take into account everything that is true about the person’s actual situation” (Pollock, 1976, 250). But this confronts us with the akrasia problem in all its glory. If we take into account everything that is true of the person’s situation, we take into account what he wants most
Apart from Quinean and Pollockian objections to the subjunctive conditional, however, there are other reasons for treasuring the material implication. What is the best way to formalise a conditional? The answer to this question depends on one’s purpose. For instance, one might wish to express that a conditional statement in conjunction with its antecedent always entails the consequent; according to some scholars, exactly that was Philo of Megara’s motive for claiming that a sound conditional never begins with a truth and ends with a falsehood (Kneale and Kneale 1962, 130). Given such a purpose, the material implication is the best, or at least the simplest, formalisation that one can produce. On the other hand, if one’s purpose is the avoidance of the truth of conditionals whose antecedents are false, then the material implication is indeed a bad formalisation. In that case, counterfactuals and subjunctive conditionals seem to have much better credentials. However, my primary purpose is not to circumvent the notorious paradoxes of material implication: I am not particularly troubled by the fact that a material implication is true if its antecedent is false or its consequent is true. My purpose is to avoid the problem of *akrasia*. This problem typically occurs when the antecedent is true, so I do not need a counterfactual or a subjunctive conditional to avoid unwished-for situations springing from the mere falsity of the antecedent. In other words, my problem is not that the conditional is true if the antecedent is false, my problem is that the conditional is true if the antecedent is true. In order to solve that problem, it might be enough to modify the reduction sentences; there is as yet no need to discard them.

My modification of Hempel’s theory consists in a ‘gradualisation’: I ‘gradualise’ Hempel’s theory by rendering it open to degrees or grades. This gradualisation is explained briefly in Section 3, and more in detail in Chapter IX. But first, in the following two sections, I point out a common misunderstanding of Hempel’s position.

and judges best in that situation. Given those wants and judgements, the probability that the agent will act accordingly equals 1 - and as I have been arguing in the preceding chapters, this exactly is the *akrasia* problem. As will become clear in the text, I also have an objection to relative frequency. My objection, however, is a different one: it is directed against relative frequency as the basis for defining the meaning of a disposition. Cf. Chapter VIII, Section 2.1 and Chapter IX, Section 3.3.5.

69 I apologise for the fact that the coinage ‘gradualise’ or ‘gradualisation’ is artificial and somewhat ugly. However, it yields the most appropriate connotation.
I. Positions in the debate on action explanation

After having examined its origins (Part One), I sketched how the problem of \textit{akrasia} reappeared in contemporary philosophy (Part Two). The problem had followed an unexpected route: it re-entered the philosophical arena as an entirely unforeseen by-product of the debate on action explanation among philosophers of science. In this debate, I distinguished three major positions: the position of Hempel (Chapter II), that of the LCA adherents (Chapter III), and that of Davidson (Chapter IV). The core of Hempel’s position, I argued in Chapter II, consists in the claim that reason explanations have the same structure as causal explanations, although reasons are not causes. The two remaining positions I have depicted as two different criticisms of Hempel’s view. The supporters of the LCA criticise Hempel in holding that, since reasons are not causes, reason explanations cannot possibly have the same structure as causal explanations. Davidson, on the other hand, castigates Hempel’s view in maintaining that reason explanations differ essentially from causal explanations, although reasons \textit{are} causes. (I am here referring to the most recent Davidson, the one that I have called ‘the Davidson of 1978’ in the preceding chapter.)

In the chapter on Davidson’s argument, Chapter IV, we encountered yet another view of action explanation. This position, the fourth one in the debate, is taken by the classical causalists. Their view I have not listed as a criticism of Hempel’s opinion. For like Hempel but unlike Davidson and the LCA champions, the classical causalists argue that reason explanations and causal explanations indeed are structurally similar. They do, however, also hold that reasons are causes; in that respect they agree with Davidson and disagree with Hempel and the LCA champions. Putting all four positions together, we obtain the following table:
VII: Dispositions and Reduction Sentences

Positions in the debate on action explanation

<table>
<thead>
<tr>
<th></th>
<th>(i) reason is cause</th>
<th>(ii) r-expl. is c-explan.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hempel</td>
<td>no</td>
<td>yes</td>
</tr>
<tr>
<td>LCA</td>
<td>no</td>
<td>no</td>
</tr>
<tr>
<td>Davidson</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td>causalists</td>
<td>yes</td>
<td>yes</td>
</tr>
</tbody>
</table>

Each of the four views affirms or denies each of two statements: ‘reasons are causes’ (i), and ‘reason explanations have the same structure as causal explanations’ (ii). Since my view has much in common with Hempel’s, I wish to elaborate upon his position before I start explaining my own. In particular, I would like to point out a common misinterpretation of Hempel’s text.

2. A common misinterpretation

As is shown by the table in the preceding section, Hempel differs from the classical causalists in denying that reasons are causes. Yet the two positions are often put on a par, due to the common fallacy of thinking that Hempel does regard reasons as causes. Simon Evnine is among the philosophers who plummets into this pitfall. In an otherwise fine monograph Evnine writes:

"Another positivist thesis (particularly supported by Hempel) was that the reasons for which we perform actions are the causes of those actions." (Evnine 1991, 5).

But Hempel never maintained that reasons are causes. On the contrary, he maintained that certainly they are not. This immediately follows from three claims that Hempel makes. Here is the first claim: in a genuine neopositivistic manner Hempel stresses that causes are events:

"Causal explanation is a special type of deductive
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nomological explanation; for a certain event or set of events can be said to have caused a specific 'effect' only if there are general laws connecting the former with the latter ..." (Hempel 1965, 300-301).

"Consider first the explanatory use of what may be called general statements of causal connection: these are to the effect that an event of some kind A (e.g., motion of a magnet through a closed wire loop) causes an event of a certain other kind, B (e.g., flow of an electric current in the wire)." (Hempel 1965, 348; my emphasis).

Hempel’s second claim is that reasons should be regarded as dispositions. In matters of dispositions Hempel adheres to the view of the behaviourist Gilbert Ryle, who characterised dispositions as "abilities, tendencies, or pronenesses to do ... things of lots of different kinds" (Ryle 1949, 114). As abilities or tendencies (and this is Hempel’s third claim) dispositions differ from events, happenings, incidents or states of affairs:

"Dispositional statements are neither reports of observed or observable states of affairs, nor yet reports of unobserved or unobservable states of affairs. They narrate no incidents." (Ryle 1949, 120).

"The expansion of a motive-expression is ... not a report of an event." (Ryle 1949, 109).

"inclinations and moods, including agitations, are not occurrences and do not therefore take place either publicly or privately. They are propensities, not acts or states." (Ryle 1949, 81).70

70 Cf. Van Heerden and Smolenaars: "Dispositions are usually not events that actually occur but, rather, a name for the occurrence of events that are linked together in a specific way. Your politeness is not an event, but a characterization of your demeanour at the dinner table as you react promptly to the request to pass the salt." (Van Heerden and Smolenaars 1989, 298).
The three claims above reveal the erroneous character of Evnine’s statement: if causes are events, if reasons are dispositions and if dispositions differ from events, then neither reasons nor dispositions are causes. Thus Hempel is completely consistent when he states that "... possession of the dispositional property M would not ordinarily count as a cause" (Hempel 1965, 487), and when he quotes with approval the following passage from *The Concept of Mind*: "to explain an action as done from a specified motive or inclination is not to describe the action as the effect of a specified cause. Motives are not happenings and are not therefore of the right type to be causes." (Ryle 1949, 113; quoted in Hempel 1965, 487, footnote 38).

Following David Lewis, and possibly also influenced by the Davidsonian approach, John Searle has scolded Hempel and other behaviourists for ignoring “the” causal relations between reasons and actions (Searle 1992, Lewis 1966). After having accused Hempel and his fellow behaviourists first of vagueness and then of circularity, Searle comes up with a third charge:

"A third ... objection to behaviorism was that it left out the causal relations between mental states and behavior ... [I]f we try to *analyze* beliefs and desires in terms of behavior, we are no longer able to say that beliefs and desires *cause* behavior.” (Searle 1992, 35; emphasis by Searle).

Searle’s phrasing of this "objection" might easily mislead us. The point is not that Hempel is "no longer able" to say that beliefs and desires cause behaviour, the point is that he does not *wish* to say it, since he does not *believe* it to be true. In Hempel’s view, there are no such things as causal relations (let alone "the" causal relations) between reasons and actions, since in their rôles as dispositions the former can never cause the latter. A similar misunderstanding may well have deluded Jaegwon Kim.71

71 During the conference ‘Human Action and Causality’, April 1996 in Utrecht, Kim delivered an interesting paper. It was entitled ‘Reasons and the First Person’, and contained a skillful explanation of Hempel’s nomological covering law model. Kim argued that this model is based on a third person view. He then asked himself whether the (first person) understanding of one’s own action is compatible with an explanation of that action along Hempelian lines. According to Kim, the question bifurcates into the following two subquestions:
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On the other hand, we might well understand that Searle, or Evnine and Kim for that matter, have difficulties in grasping Hempel’s ideas. For it cannot be denied that Hempel’s position appears somewhat peculiar. If reasons do not cause actions, how then can reason explanations resemble causal explanations? In what does the resemblance exactly lie? In other words and applied to our table: how is it possible for Hempel to deny claim (i) if at the same time he affirms claim (ii)? This question will be dealt with in 2.1.

2.1 Events as causes

Compared to the LCA and the classical causalistic view, Hempel’s position is an intricate one. Whereas both (i) and (ii) are false according to the LCA and true according to classical causalism, Hempel takes the counterintuitive position that (i) is false while (ii) is true. As far as this particular intricacy is concerned, Hempel has found his match in Davidson. Davidson too takes a compound view in maintaining precisely the opposite of what Hempel claims, i.e. in affirming (i) while denying (ii).

However, as we have seen, Hempel does not consider the agent’s reasons to be causes. The predicament that Kim discusses accordingly vanishes.

72 Oddly enough, Davidson has assured me in a private communication that nothing in Hempel’s theory of action explanation is incompatible with his own. On then being asked if every detail in Hempel’s theory may be applied to his (Davidson’s) view, Davidson rejoined: “Yes”.

"(a) If an agent understands his own action in terms of his reasons for choosing to do the action, can he also understand or explain his action in terms of his reasons taken as causes (that is, nomological, necessitating causes)?, and (b) in the same situation, can there be a third-person causal explanation of the action which takes the agent’s reasons as causes?" (my emphasis).
the assertion of (i). These distinctions, however, are *Fremdkörper* in Hempel’s work. Nowhere does Hempel talk about mental versus physical events, and nowhere does he refer to singular causal statements as something different from general causal explanations.\(^{73}\) Hence Hempel stands in need of other means to establish his complicated position. Which means? Perhaps the best way to answer this question is to point out again Hempel’s Rylean leanings.

What Hempel especially appreciates about Ryle’s approach is what made *The Concept of Mind* famous, viz. its exorcistic tendency. Hempel greatly approves of Ryle’s efforts to drive out spirits and other demons, and he praises Ryle for his inveighing against "the conception of motives as ghostly causes of overt behavior, and against the notion that ‘in history we have to do with a world of mental agencies, mysteriously lying behind the world of physical bodies and actions, separate from it and yet controlling it’ ..." (Hempel 1965, 487).

The quotation inside this quotation is taken from Gardiner 1952. Five years ahead of William Dray, Gardiner argued for the claim that reason explanations are not causal explanations. Hempel endorses Gardiner’s claim on condition that it boil down to a denial of (i); only if Gardiner’s statement means that reasons cannot cause actions (since they are dispositions), is Hempel prepared to lend his support: "... when Gardiner remarks that an explanation of the form ‘x did y because he wanted z’ does not refer to a causal relation between events, he is right in the sense that the statement ‘x

\(^{73}\) Of course Hempel does distinguish between descriptions and things described; this distinction (called DISTINCTION 1 in Chapter IV, Section 3) is however not very important in Hempel’s philosophy. The insignificance is clear from Hempel’s lack of interest in the difference between explanandum-phenomena and explanandum-sentences. After having introduced the argument to which all explanations are geared, the Deductive-Nomological argument, Hempel writes:

"The conclusion \(E\) of the [Deductive-Nomological] argument is a sentence describing the explanandum-phenomenon; I will call \(E\) the explanandum-sentence or explanandum-statement; the word ‘explanandum’ alone will be used to refer either to the explanandum-phenomenon or to the explanandum-sentence: the context will show which is meant." (Hempel 1965, 336).

Thus while Hempel explicitly acknowledges DISTINCTION 1 in his theory of scientific explanation, it does not play a crucial rôle.
A common misinterpretation

wanted \( z' \) does not describe an event, but ascribes to \( x \) a broadly dispositional property.\" (Hempel 1965, 487). However, if Gardiner meant his claim as a denial of (ii), then Hempel disagrees. For like all other dispositional explanations, reason explanations are in fact causal explanations - not because reasons are causes (as would be maintained by classical causalists), but because the conditions under which the reasons or the dispositions manifest themselves can be said to have caused the actions. Hence Hempel can write:

"... a dispositional explanation invokes, in addition to the appropriate dispositional property \( M \), also the presence of the circumstances, say \( S \), in which the property \( M \) will manifest itself by ... behavior of the kind \( R \) ... whose occurrence is to be explained. For example, the attribution of venality to an agent will explain his having committed treason only in conjunction with further suitable assumptions, such as that he was offered a large bribe, which in virtue of his venal propensity led to the act in question. Here the offer of the bribe ... may be said ... to have caused the explanandum event.\" (Hempel 1965, 486 - my emphasis).

In other words: the treasonous action is caused by an event (the offer of a bribe) and not by a reason (the desire for affluence in conjunction with the belief that accepting graft will satisfy this desire).

By considering events rather than reasons as the causes of actions, Hempel again sides with Ryle. Ryle's standard example of a dispositional explanation is that of a window pane breaking after it has been struck by a stone. The question of why the glass broke, Ryle argues, has two answers. The first is the dispositional answer. It ascribes to the glass a disposition ("being brittle") which explains why the glass fragmented when struck. The second answer is the causal one. It involves the report of an event ("a stone hit the glass"), which stands to the fracture of the glass as cause to effect (Ryle 1949, 88). Ryle suggests that either of the two explanations is sufficient to account for the given event; but Hempel noted that neither of them will do (Hempel 1965, 458; the same point was made by Jerry Fodor in Fodor 1975). The dispositional statement that the glass was brittle, Hempel argues, can explain the breaking of the glass only \textit{in conjunction}
VII: Dispositions and Reduction Sentences

with the report that a stone hit the glass. Similarly, the latter event is only a cause on account of the dispositional statement: "... it is in virtue of ... the dispositional attribution that being hit by the stone becomes a cause rather than an accidental statement" (Hempel 1965, 458 - my emphasis). Thus Hempel affirms claim (ii) and denies claim (i). He considers reason explanations as causal explanations, not because reasons are causes, but because certain events become causes when they are described as antecedent conditions in dispositional statements. This is made more precise in 2.2.

2.2. Application to reduction sentences

Hempel’s idea that actions are caused by events rather than reasons can be made more precise on the basis of reduction sentences. In Chapter II and III we saw that Hempel defines disposition terms, and thus reason terms too, in bilateral reduction sentences. If \( R \) is a term referring to a reason, then \( R \) is defined by the bilateral reduction sentence (VII.1):

\[
(VII.1): (x) \{S(x) \rightarrow (R(x) \leftrightarrow A(x))\},
\]

where \( x \) ranges over persons, \( S \) denotes a situation in which the person happens to be, and \( A \) denotes an action. (VII.1) is, as we saw, a conditional definition: it determines the meaning of \( R \) only for objects which have the property \( S \). Furthermore, (VII.1) is equivalent to the conjunction of (VII.2) and (VII.3):

\[
(VII.2): (x) \{R(x) \rightarrow (S(x) \rightarrow A(x))\}
(VII.3): (x) \{S(x) \rightarrow (A(x) \rightarrow R(x))\},
\]

where (VII.2) and (VII.3) are sentences expressing symptoms of \( R \)'s presence. (VII.2) expresses a necessary condition for application of \( R \): it entails that \( S(x) \rightarrow A(x) \) and hence \( \neg(S(x) \land \neg A(x)) \) is necessary for \( R(x) \). Sentence (VII.3) expresses a sufficient condition for application of \( R \): it says that \( S(x) \land A(x) \) is sufficient for \( R(x) \).

When Hempel maintains that actions are caused by events, he has in mind events such as those described by \( S(x) \), events that Dretske called "triggering causes" (Dretske 1988, 42-43). These events, and not such inclinations as described by \( R(x) \), count as the causes for \( A(x) \). Granted, \( R(x) \)
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is indispensable for the description of $S(x)$ as a cause of $A(x)$, for it is only by virtue of $R(x)$ that $S(x)$ becomes a cause. But this need for $R(x)$ does not render reason explanations different from ordinary causal explanations, since in causal explanations too it is only by virtue of a causal law that a particular event comes to be the cause of another.

Considering $S(x)$ as the cause of $A(x)$ is unrelated to the question of whether $R$ is a simple or a broad (multiple) reason. If $R$ is a simple reason, then $R$’s definition consists in exactly one bilateral reduction sentence. If that sentence is (VII.1) it describes the only way in which $R$ manifests itself. Under the assumption that (VII.1) is a conditional definition, the necessary condition, described in (VII.2), coincides with the sufficient condition, described in (VII.3); for given that $S(x)$ is true, $\neg(S(x) \land \neg A(x))$ and $S(x) \land A(x)$ have the same extension: both are true if $A(x)$ is true and both are false if $A(x)$ is false. Hence, in this simple case, $S(x)$ is the cause of $A(x)$. On the other hand, if $R$ is a broad reason, then $R$’s definition consists of a set, $U$, of $n$ different bilateral reduction sentences $u_1$, ..., $u_n$, each of which describes another way in which $R$ manifests itself. Let $S_1$, ..., $S_n$ describe $n$ different situations and let $A_1$, ..., $A_n$ describe $n$ different actions corresponding to these situations. Then the partial definition of $R$ is the set $U$ (cf. Chapter III, 3.3):

\[
\begin{align*}
(u-1): \quad & (x) \left[ S_1(x) \rightarrow (A_1(x) \leftrightarrow R(x)) \right] \\
(u-2): \quad & (x) \left[ S_2(x) \rightarrow (A_2(x) \leftrightarrow R(x)) \right] \\
& \quad \vdots \\
(u-n): \quad & (x) \left[ S_n(x) \rightarrow (A_n(x) \leftrightarrow R(x)) \right].
\end{align*}
\]

Every $(u-i)$ (1\leq i \leq n) is equivalent to the conjunction of a sentence $(u_i)$ and a sentence $(u'_i)$:

\[
\begin{align*}
(u_i): \quad & (x) \left[ R(x) \rightarrow (S_i(x) \rightarrow A_i(x)) \right] \\
(u'_i): \quad & (x) \left[ S_i(x) \rightarrow (A_i(x) \rightarrow R(x)) \right],
\end{align*}
\]

where $(u_i)$ expresses a necessary condition $C_i$ and $(u'_i)$ expresses a sufficient condition $C'$ for application of $R$, $C_i$ being described by $S_i(x) \rightarrow A_i(x)$ and $C'$ by $S_i(x) \land A_i(x)$. Contrary to the simple case, there are more than one necessary conditions and more than one sufficient conditions: for instance
C_i, C_j, C_i, C_j (1 ≤ i, j ≤ n, i ≠ j). Consequently, necessary and sufficient conditions need not coincide in the broad case. Hence in this case, each A_i(x) (1 ≤ i ≤ n) has a unique cause, S_i(x) (1 ≤ i ≤ n), and each A_j(x) has a unique cause, S_j(x). Typically, A_i and A_j are not the same, and neither are S_i and S_j. But the crucial point is of course that R(x) has no causal power here, any more than it had in the simple case. It is the particular S_i or S_j that constitutes the cause.

3. Hempel’s approach gradualised

The preceding sections showed that Hempel’s position is unique, complex, and consistent. Unique, since it cannot be reduced to any of the other three positions in our table. Complex, in that it combines the denial of (i) with the assertion of (ii). Consistent, since it sticks to its premise that events (rather than properties or objects) are the causes of actions, the events being the antecedent conditions in Carnapian reduction sentences. Hempel thus draws a parallel between causal explanations and action explanations without in any sense assuming that reasons are causes.

However, we have seen in Chapters II and III that Hempel’s approach, consistent though it may be, engenders akrasia as a problem. In the sequel I shall try to polish this blemish from Hempel’s blazon. My strategy is that of instilling the concept of grade or degree into Hempel’s theory. In terms of Chapter V, one might also say that I attempt to incorporate into Hempel’s theory the idea of a divided mind. Phrased in less metaphorical terms, this means that I shall insert grades into dispositions (notably into beliefs and desires), and hence into the descriptions of dispositions, viz. the reduction sentences. By thus ‘gradualising’ dispositions as well as reduction sentences, I am in a sense giving a new interpretation to the concept of a divided mind. On the whole, my venture is guided by the ambition to resolve the akrasia problem.

There are different ways in which one can gradualise Hempel’s theory. In order to explain mine, I shall first examine two other ways. The first gradualisation (if it indeed deserves that name) was undertaken by Hempel himself; I deal with it in Section 4. The second gradualisation was inspired by Hans Reichenbach’s work; I discuss that in Chapter VIII. Buttressed by a study of Hempel’s and Reichenbach’s approaches, I shall then, in Chapter IX, explain more accurately my own view of gradualisation.
as a strategy to avoid the *akrasia* problem.

4. Making the reduction sentences probable

In a sense, Hempel did already gradualise the reduction sentences himself. While discussing their rôles as definitions of dispositions, Hempel remarked that often reduction sentences are not strict, but probabilistic in character. Only very rarely does the definition of a reason term $R$ take the shape of a bilateral reduction sentence like (VII.1) in Section 2.2; mostly, it is framed as a probabilistic counterpart of (VII.1). Similarly, the symptom sentences (VII.2) and (VII.3) usually occur in probabilistic forms:

"The construal of symptom sentences ... is an oversimplification in many cases. ... the connection between [$R$] and its symptomatic manifestations will often have to be conceived as probabilistic in character. In this case, the symptom sentences ... take ... statistical forms ..." (Hempel 1965, 461).

What exactly do the sentences (VII.1), (VII.2) and (VII.3) look like when they are wearing their probabilistic colours? As for (VII.2) and (VII.3), one cannot simply place the standard probability operator $p$ ahead of them, thereby construing them bluntly as:

(VII.4): $p(R(x) \rightarrow (S(x) \rightarrow A(x))) = r_i$

(VII.5): $p(S(x) \rightarrow (A(x) \rightarrow R(x))) = r'_i$

where $r_i, r'_i$ are numbers between 1 and 0. For (VII.4) and (VII.5) are not generally accepted as respectable probability statements. Moreover, even if they were, then they would seem to express *a priori* or absolute probabilities: after all (VII.4) and (VII.5) look like $p(X)=r$, where $X$ is a statement. Hempel, on the other hand, regards the probabilistic counterparts of (VII.2) and (VII.3) as expressing conditional probabilities; instead of the form $p(X)=r$, they both display the form $p(X|Y)=r$. This is clear from the fact that Hempel phrases the probabilistic analogues of (VII.2) and (VII.3) as:
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(VII.6): For objects or individuals that have the property $R$ and are under test conditions of kind $S$, the statistical probability of responding in manner $A$ is $r_i$;

(VII.7): For objects or individuals that are under test conditions of kind $S$ and respond in manner $A$, the statistical probability of possessing the property $R$ is $r_j$.

(Hempel 1965, 461). (VII.6) and (VII.7) express conditional, not absolute probabilities. They can be formalised as (VII.8) and (VII.9):

(VII.8): $p(A(x) \mid S(x) \land R(x)) = r_i$

(VII.9): $p(R(x) \mid S(x) \land A(x)) = r_j$.

For Hempel, (VII.8) and (VII.9) are the probabilistic counterparts of (VII.2) and (VII.3). As might be expected, the probabilistic form of (VII.1) draws

74 One might object that there is no genuine difference between (VII.8) and (VII.9) on the one hand and (VII.4) and (VII.5) on the other. Since (VII.4) and (VII.5) are about the probability of conditionals, their form is not given by $p(X)=r$, but by $p(Y \rightarrow X)=r$. Whereas $p(X)=r$ indeed is an absolute or a priori probability statement, $p(Y \rightarrow X)=r$ is not. The latter statement is about the probability that $X$, given $Y$. It is thus equivalent to $p(X\mid Y)=r$, which is a conditional or a posteriori probability statement. Equally to (VII.8) and (VII.9), (VII.4) and (VII.5) would express conditional probabilities.

Evidently, this objection does not distinguish between the probability of a conditional, in casu the material implication $p(Y \rightarrow X)=r$, and a conditional probability, in casu $p(X\mid Y)=r$. Whereas the latter formula indeed expresses a conditional probability, the former still signifies an absolute probability (of a conditional).

It has indeed been argued that there are cases in which probabilities of conditionals do not differ from conditional probabilities. Those are the cases in which the conditional is the unregimented ordinary language conditional ‘If $Y$ then $X$’. Thus in a very influential article Robert Stalnaker defined ‘If $Y$ then $X$’ as a proposition of which the probability is measured by the conditional probability statement $p(X\mid Y)$. Hence, according to Stalnaker, $p(\text{if } Y \text{ then } X)$ is identical to $p(X\mid Y)$ (Stalnaker 1970). Several years after Stalnaker’s article, David Lewis disputed this identity. According to Lewis, the identity is untenable since “there is no way to interpret a conditional connective so that, with sufficient generality, the probabilities of conditionals will equal the appropriate conditional probabilities” (Lewis 1976, 77-78). Lewis argument is that if there were such a way, then probabilities of conditionals "could serve as links to establish relationships between probabilities of non-conditionals" (Lewis 1976, 78). However, such relationships

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upon the probabilistic counterparts of (VII.2) and (VII.3). It is therefore not represented by:

\[(VII.10): p(S(x) \rightarrow (R(x) \leftrightarrow A(x))) = r_k,\]

but by (VII.11):

\[
\begin{align*}
  p(A(x) | S(x) \wedge R(x)) &= r_i \\
  p(R(x) | S(x) \wedge A(x)) &= r_j
\end{align*}
\]

which is the conjunction of (VII.8) and (VII.9). In words (VII.11) says: ‘The probability \(r_i\) that persons perform action \(A\), given that they are in situation \(S\) and have reason \(R\), together with the probability \(r_j\) that persons have \(R\), given that they are in \(S\) and perform \(A\). As distinct from (VII.8) or (VII.9), however, (VII.11) does not express a single numerical probability value. Although the values \(r_i\) and \(r_j\) expressed by the former two statements can of course arithmetically be added (after all they are numbers), the resulting sum does not generally signify a probability value.\(^{75}\)

The fact that reduction sentences - whether bilateral or unilateral - may be probabilistic or non-probabilistic is independent of the question whether \(R\) is a simple or a broad reason. If \(R\) is a simple reason, then the probabilistic reduction sentences are of course (VII.11), (VII.8) and (VII.9). If \(R\) is broad, then (VII.8) and (VII.9) should be construed as (VII.12) and (VII.13):

\[
\begin{align*}
  \text{(VII.12): } & p(A_1(x) | S_1(x) \wedge R(x)) = r_1, p(A_2(x) | S_2(x) \wedge R(x)) = r_2, \ldots \\
  \text{(VII.13): } & p(R(x) | S'_1(x) \wedge A'_1(x)) = r'_1, p(R(x) | S'_2(x) \wedge A'_1(x)) = r'_2, \ldots
\end{align*}
\]

turn out to be incorrect. Convinced by Lewis’ argument, Stalnaker rejected the identity of conditional probabilities with probabilities of a conditional. However, some philosophers still maintain that the early Stalnaker was right.

\(^{75}\) This is due to the fact that the sum of conditional probability statements with different reference classes does not itself express a probability value. For example, the conditional probability statements \(p(H|V)\) and \(p(V|H)\) each expresses a number; they thus can be added arithmetically: \(p(H|V)+p(V|H)\). But the sum - which might well exceed 1 - is not a probability value. Hence \(p(H|V)+p(V|H)\) is not a probability, and neither is \(p(A(x) | S(x) \wedge R(x)) + p(R(x) | S(x) \wedge A(x))\).
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to be read as, respectively,

For objects or individuals that have the property $R$ and are under test conditions of kind $S_1$ ($S_2$, ...), the statistical probability of responding in manner $A_1$ ($A_2$, ...) is $r_1$ ($r_2$, ...);

For objects or individuals that are under test conditions of kind $S'_1$ ($S'_2$, ...) and respond in manner $A'_1$ ($A'_2$, ...), the statistical probability of possessing the property $R$ is $r'_1$ ($r'_2$, ...)

(here, as elsewhere, I have used subscripts to indicate necessary, and superscripts to indicate sufficient conditions; cf. Hempel 1965, 461). The sentences (VII.12) and (VII.13) claim of each single symptom sentence that it is probabilistic in character. By thus rendering the reduction sentences open to degrees of probability, Hempel may be said to have ‘gradualised’ his own theory.

Although it is only natural to assume that the reduction sentences are probabilistic in character, Hempel’s method appears to have serious shortcomings. This will be shown in the next chapter, where I discuss another gradualisation of Hempel’s theory, viz. a gradualisation in the style of Hans Reichenbach. I argue that both gradualisations suffer from the same defect, since they gradualise the disposition’s meaning, not the disposition itself. They thus both centre upon the notion ‘degrees of a disposition’s meaning’, that I will show to be a contradictio in terminis. I shall replace that inconsistent concept by ‘degrees of a disposition itself’, which is not merely consistent, but functions in fact as the key to unlock the problem of akrasia.