CHAPTER I

INTRODUCTION

As Michael Fara puts it:

> The glass vase on my desk is fragile. It should be handled with care because it is likely to shatter or crack if it is knocked, dropped, or otherwise treated roughly. The vase has certain *dispositions*, for example the disposition to shatter when dropped. But what is this disposition? It seems on the one hand to be a perfectly real property, a genuine respect of similarity common to glass vases, china cups, ancient manuscripts, and anything else fragile. Yet on the other hand my vase's disposition seems mysterious, 'ethereal' (…) in a way that, say, its size and shape properties are not. For my vase's disposition, it seems, has to do only with its *possibly* shattering in certain conditions, conditions which I hope will never be realized. In general, it seems that nothing about the *actual* behavior of an object is ever necessary for it to have the dispositions it has. Many objects differ from each other with respect to their dispositions in virtue of their merely possible behavior, and this is a mysterious way for objects to differ. (Fara)

In this context, I will investigate the issue whether dispositions are causally efficacious properties, or are instead epiphenomenal. For this purpose, I will mainly focus on Stephen Mumford's views.

The gist of Mumford's view is that the ascription of any property entails a subjunctive conditional, only ascriptions of dispositions entail conditionals *as a matter of conceptual necessity*. Mumford defends the view that what is special about ascriptions of dispositions, as opposed to ascriptions of categorical properties, is that they are ascriptions of properties that play a certain causal or functional role, a role that is best captured in conditional terms. For example, on Mumford's view it is a conceptual truth that if something is fragile then (roughly) it would shatter or crack if it were dropped or knocked (in ideal conditions). This is a *conceptual* truth according to Mumford. He thinks that dispositions can play the metaphysical role traditionally

ascribed to real ‘powers’: the that-in-virtue-of-which-something-will-*G*, if *F*. For him, this concept’s extension is causally efficacious; and when it comes to causal relations, as I understand, he proposes that all the other things supervene on dispositions. I consider his approach teleological.

Throughout this thesis, I will present and evaluate Mumford’s theses to come up with my conclusion, stating, unlike him, that dispositions are causally impotent.

CHAPTER II

MUMFORD'S POSITION

Stephen Mumford tries to show that the objections, which we will see, that dispositions are a priori causally impotent are not secure. From his point of view, this means that dispositions are not logically precluded from being causes and so an ontology cannot be dismissed simply because it involves a causal role for dispositions. He sees dispositions as causal-role occupying properties, by definition. He claims that the characterization of a property dispositionally is the description of a property according to (at least one of) its functional role(s).

He constructs an argument in favour of the causal efficacy of dispositions, which he names the argument from behavioural difference. But before presenting his argument, let us have a look at his understanding of dispositions and his main intention – in our context.

According to Mumford, dispositions are properties and properties play causal roles in a thing's interactions with the world about it. As he stresses, the issue of the causal role of dispositions is entwined with the question of the use of disposition ascriptions in explanation. For him, disposition ascriptions have an explanatory value which justifies the use of the dispositional idiom. He also claims that their ascription must have an explanatory content for there would be no point in making them otherwise. But what is this content? As he admits, there is little agreement about exactly what explanatory role disposition ascriptions play or even that dispositions have an explanatory role at all.

He tries to justify the claim that disposition terms are a class of explanatory concepts. He says that we have a dispositional vocabulary to use when there is an explanatory gap in our account of how something behaves the way it does. He insists that dispositions fill such gaps, if only temporarily, and in order to fill such gaps for any time at all dispositions cannot be causally impotent nor disposition ascriptions wholly pointless. What he proposes to do is bolster the causal status of dispositions against objections and clarify the explanatory role of disposition concepts. He begins with some – what he calls – prima facie claims about dispositions which he aims eventually to legitimize.

From his point of view, prima facie dispositional explanations are a brand of causal explanation. As he points out, if we consider the questions of why some objects break when dropped and why some substances dissolve when in water, one kind of answer is that such things break and dissolve because they are fragile (in other words, 'having the power' to break) and soluble respectively. He also mentions that these statements may be true to the extent that the disposition terms 'fragile' and 'soluble' are true of the objects or substances involved but these statements are also offered as *explanations* of breaking and dissolving, and this is the point that is controversial. He says that being fragile is a causally relevant property to the object breaking when dropped; being soluble is a causally relevant property to a substance's dissolution upon immersion in water. As he stresses, it needs to be acknowledged straightaway that these explanations are not being offered as complete causal explanations. He emphasizes that such facts about the possession of these properties only explain their manifestations given the right (ideal) background conditions and stimulus events.

He proposes that the justification of the causal potency of dispositional properties can be set out in the following argument. As he considers, not all substances will dissolve upon immersion in a specific volume of water in a certain set of background conditions; some will dissolve and some will remain in their solid state. He emphasizes that we could have two substances in identical test conditions and one will show a certain reaction while the other does not. He argues that such a scenario is best explained by the theory that something about the substance is relevant to the behaviour it exhibits, otherwise, given that all other conditions are the same, everything would exhibit the same behaviour in the same circumstances. Then he adds that it is manifestly false that everything behaves the same for any test and set of background conditions. According to him, it seems that we are obliged to grant some causal role to the particular object or kind involved. He suggests that it is this causally relevant *something* about the object that is the property of the object that can be called, according to an acceptable use of 'cause', the cause of the manifestation. Hence, he stresses that if this property can be described correctly as a dispositional property, then we have a justification for the thesis that dispositions are causes.

The Argument From Behavioural Difference

He refers to this argument as *the argument from behavioural difference*. He presents a more precise statement of the argument as follows:

> 1. For some test F and set of background conditions C_i, there exists at least one x and one y for which relation G is true of x and false of y.
> 2. This difference in behaviour of x and y is best explained by the possession by x (or y) of some causally relevant property or property complex P, not possessed by y (or x).
> 3. There are circumstances in which this property P can be correctly described as a dispositional property. (Mumford, Dispositions, 119-

120)

Therefore, he states that dispositions are causally relevant properties.

As he stresses, premise 1 is an empirical premise which can be taken to be unproblematically true and confirmable with ease. And he adds that premise 2 rests on a theoretical claim about the cause of a thing's behaviour. He says that it relies on the claim that if *x* and *y* differ in behaviour, then there are cases where this difference in behaviour has a cause; that is, there are cases where the behaviour of *x* and *y* is non-indeterministic. If all other factors are the same, then he puts two options in front of us:

a) The best explanation of such difference in behaviour is a difference in properties;

b) The possibility of (a) is not undermined by the possibility of another case. This is where *x* and *y* differ in behaviour but are identical in all their properties. The coherence of this rests on the possibility of *x* and *y* possessing the same probabilistic disposition which just happens to get manifested for *x* but not for *y*.

He thinks that the argument is not affected by this admittedly plausible case for it merely requires the equally credible possibility of the first type of case. In premise 3, he claims that there are circumstances where these properties can correctly be said to be dispositional. As he concedes, this depends on a semantic and ontological theory of dispositions. He maintains the following:

"Already enough has been said to indicate how it is possible to correctly refer to such properties in a dispositional way: if property *P* is characterized functionally, then *P* is being characterized as a disposition. This leads us to the conclusion that in the sense of causal relevance stated in premiss 2, dispositions are causally relevant." (Mumford, Dispositions, 120).

Some Possible Objections

However, Mumford doesn't hesitate to consider some possible objections to his argument. In his words:

> How can the argument from behavioural difference be challenged? It has been suggested, by the opponents of realism about dispositions, that dispositions are valueless as causal explanations, basically because dispositions are not causes of anything. A disposition ascription, on this view, is akin to a *virtus dormitiva*: a trivially true ascription of a causal power that we posit when we are in ignorance of the 'real' causes of changes in things. A *virtus dormitiva*-type explanation is a vacuous explanation in that it offers an explanation for how it is that something has a power to φ by ascribing that very same power to one of its components. Such explanations are thought to be highly undesirable and it is hoped, for instance by (Willard) Quine, that eventually, as science progresses, *virtus dormitivae* will be completely eliminated from explanation and be replaced with 'respectable' categorical causal explanations. (Mumford, Dispositions, 120-121)

Mumford considers whether explanations appeal to dispositions. By contrast to what I have just claimed in a Quinean manner, what he tries to do is to show that causal explanation in terms of categorical properties alone is inadequate. This realization, he hopes, will show the motivation for finding some 'active powers' in the world. He also plans to go some way to illuminating the explanatory role of appeal to dispositions, as connections between an object's states and its behaviour.

As he mentions, the paradigm of scientific explanation is structural explanation that contains no reference to dispositional properties. Quine argues in support of such an interpretation of explanatory practice as dealing only in the categorical. Quine concedes that there remains some reference to dispositions in scientific explanations; he thinks that this merely indicates that physical science is still incomplete. According to him, as it becomes more advanced such references to

dispositions will be eliminated in favour of a completed explanation that contains appeal to categorical properties only. The contention is that we can, in principle if not yet in practice, provide a full causal explanation of all the events that are 'pre-scientifically' understood as disposition manifestations in terms that are entirely 'disposition-free'. We can offer a description of the categorical bases that are relevant to the occurrence of the putative disposition manifestation event and these properties can replace the unscientific appeal to dispositional powers or forces.

Maybe causal, or not; maybe holistic, or not; but it seems as a possible task to complete our science(s) to give explanations free from such metaphysical concepts. I would buy Quine's proposal instead of sticking to such teleological approaches.

Mumford argues that reference to certain categorical properties clearly falls short of an explanation if, following David Hume, we allow no necessary connections between such categorical properties and the events they may precipitate. In other words, according to Mumford, something more is needed if we are to have anything that is a candidate for a complete explanation.

In this context, let us peruse Brian O'Shaughnessy's explanation of 'elevancy', being the disposition of a particular object to rise when in water. In this paradigm example, a scientific understanding of this phenomenon is not in terms of objects having a real disposition or power, but rather in terms of the categorical properties of the 'elevant' particular and the general laws connecting such properties with certain types of behaviour. O'Shaughnessy explains the rising of an object in water in terms of the satisfaction of five conditions:

1) The ratio of the object's mass to its nonpermeable volume is less than one.
2) The object has weight.
3) The object tends to move in the direction of the impressed force.

4) The density of water is 1.

5) Water exerts an upward thrust on objects equal to the weight of the water they displace. (O'Shaughnessy, 2-3)

According to his explanation, what it is about the object that causes it to rise in water is its 'basis', which on this view is non-dispositional:

"*The basis* of a disposition I take to be that categorical state of affairs in the 'disposed' that determines the presence of its disposition; and this is identical with the causal contribution of the 'disposed' to the manifestation." (O'Shaughnessy, 10).

Mumford claims that all O'Shaughnessy needs now argue is that a disposition and its causal base are distinct and the disposition is thereby rendered impotent – powerless, as he puts it – because there is no causal role left for it to play. O'Shaughnessy sees bases as distinct from dispositions because bases explain dispositions, thus "the base is not the disposition it helps to explain, and therefore such dispositions are powerless." (O'Shaughnessy, 5).

It is intended, of course, that this type of disposition-free explanation for elevancy be generalized. We may account for the ability of a billiard ball to roll when struck in terms of it being an equally balanced sphere on a flat surface; we may account for brittleness in terms of weak bondings between the constituent molecules; we may account for the explosiveness of dynamite in terms of its chemical composition. All explanations, it is claimed, will be non-dispositional explanations. Let us put a stop here: We will come to this point later, in bringing the discussion about the causal efficacy of dispositions versus structures (or categorical properties) to an end.

The First Triviality Objection

Now, after providing such preliminary knowledge, let us peruse Mumford's defense by beginning with *the first triviality objection* – to dispositional explanations, with regards to causality –:

> What are we looking for in a causal explanation? David Lewis says that 'to explain an event is to provide some information about its causal history.' This requirement seems easy to satisfy. That a particular had a disposition to ψ prior to it ψ-ing would seem to qualify as part of the causal history of it ψ-ing, for Lewis's statement is neutral as to what sort of entities are parts of a causal history. As he says: 'information about what the causal history includes may range from the very specific to the very abstract.' What is debatable is whether reference to dispositions identifies part of a *causal* history. There are various ways we can causally explain badly: we can give false information, we can give little information, or we can give information that was already possessed. It is the latter charge that the triviality objections identify as the reason that dispositional explanations are no good as causal explanations. (Mumford, Dispositions, 134)

There are two objections that disposition ascriptions are trivial causal explanations and hence that dispositions are not causes of their manifestations. The second is the *virtus dormitiva* objection, which we will consider soon, but this differs from the first type of triviality objection that Mumford describes now.

The first triviality objection to dispositions having a causal role, and hence having any role in causal explanation, goes as follows:

> To be fragile means nothing more than to break if dropped; to be soluble means nothing more than to dissolve if immersed in water. A similar analysis can be given for any disposition: to say that something possesses a disposition is just to say that the appropriate response will follow upon the appropriate stimulus. But if solubility means just 'dissolves in water', then any explanation of why a substance dissolves in water in terms of it being soluble will be nothing more than a trivially analytic explanation, which is no explanation at all. If fragile means nothing more than 'breaks when dropped', then it is no explanation of why something breaks when dropped. (Mumford, Dispositions, 134)

Mumford says that this objection relies on a certain understanding of dispositions.

According to his suggestion, we must construe a disposition ascription as follows:

"[Df$_R$] x is D =$_{df}$ if x is F-ed, then x will G." (Mumford, Dispositions, 135).

He emphasizes that if we accept Df$_R$, then we cannot explain why x G-ed, when F, in terms of x being D because x being D means nothing more than that x will G, if F.

At the same time, he stresses that it can be acknowledged that there is some initial credibility to Df$_R$; for it is, indeed, in virtue of something inducing sleep when ingested that we call it dormitive, it is in virtue of something breaking when dropped that we call it fragile, and in virtue of something dissolving when in water that we call it soluble. He points out that whatever arrangements of structures or processes are at work in such manifestations are irrelevant to the meaning or truth-conditions of these ascriptions; for dispositions in general, it is purely in virtue of the satisfaction of such functional criteria that they are the dispositions they are. He also says that this explains the analytic connection between a disposition and its manifestation: 'solubility' has a conceptual connection to 'dissolves in water in ideal conditions', so if in water, soluble and in ideal conditions, dissolving is entailed analytically.

However, according to Mumford, dispositions are not really subjects of quite so much triviality and hence can be causally efficacious, for he finds such an analysis of disposition ascriptions incomplete. He argues for functionalism, rather than such a kind of behaviourism, about dispositions. In his words:

> As I showed with the argument from behavioural difference, there must be some reason why not everything breaks when dropped from a moderate height. There must be something about the subject of a disposition ascription which makes the ascription true – something which plays a causal role in dissolution in water and breakage when dropped. This, and only this, must be added to Df$_R$: [Df$_M$] x is D =$_{df}$ x has some property P (and P is a cause of x G-ing if x is F-ed in conditions C_i). (Mumford, Dispositions, 135)

He claims that this puts a stop to immediate triviality, for it is no longer trivial to explain why a substance dissolves in water by saying that one of its properties is responsible.

After all the above, I would like to evaluate his ideas as follows:

First of all, why employ such an extra suspicious burden which is put here as the 'functional' (or teleological) property *P*, just to escape from the triviality objection against dispositions? Who decides that functions are not trivial; who ascribes functions or functional properties to entities; who has a right to do that, for what reason? Mumford's proposal creates another problem, in addition to the first one, with regards to causation. For Df_R, Mumford asks the "why?" question once – and employs a property to give an answer. When we recall an electron's noncausal fall (or the decay case below), then even such an attempt would be futile. There might be *no cause or reason* behind a thing's breaking when dropped from a moderate height. Whereas he boldly claims that "*there must be some reason* (my emphasis) why not everything breaks when dropped from a moderate height." (Mumford, Dispositions, 135).

Moreover, regarding Mumford's own version of the formula, in addition to the first "why?" question, we also need to ask another "why?" question (diagnosing another problem) with respect to the property which *x* has, and so on; and infinite regress...

Scientific cases can enhance our vision and understanding of the causal aspects of the issue. For instance, Albert Einstein realizes that an atom's decay, where it transits from a high (stimulated) energy level to a lower one with lower energy, is very like an atom's radioactive decay. During an atom's decay, light

radiates. Niels Bohr develops his own atom model by using Einstein's statistical ideas. Bohr emphasizes that in this atomic spectrum some lines are thicker or darker than the others, because some transitions between energy levels are more probable. He cannot explain why this must be so. During those days, like other scientists who inquire about radioactivity, Einstein thinks that the statistical calculation table is a temporary method – not a solid and stable account for reality – and he hopes that following experiments will reveal *why* a transition happens at a time *t* rather than some other time. But, right at this point, quantum theory gets away from classical principles, and there can be found *no cause* behind radioactive decay and transition of energy at atomic level. In other words, these changes are statistical changes which happen merely by chance, and this scientific fact brings along some fundamental philosophical problems as well. In the classical world, every effect has a cause (as Mumford claims). We can trace back to every effect's cause, the cause of that cause, the cause of that as well, and so on... Either to the Big Bang, or to an omnipotent creator, or even to some other thing as the first cause... But in the quantum world, when we investigate the phenomenon of radioactive decay or transitions at the subatomic level, *such kind of a direct causation ceases to be*. An electron doesn't transit (or shift) from an energy level to another at a certain time with a definite cause. And there is no way to predict the time of this possibly upcoming transition (or shift). It *just happens* at that time, rather than some other.

The Second Triviality Objection

Now, let us examine the second triviality objection to disposition ascriptions, namely

the *virtus dormitiva* objection. Mumford states that this objection is one that differs from the plain triviality objection though the two are easy to confuse.

The *virtus dormitiva* label comes from Molière's joke at the expense of philosophers. Reportedly, when asked why opium puts one to sleep when ingested, the candidate philosopher answers that it does so because it has a *virtus dormitiva* – a dormitive virtue:

> *Bachelierus*: I am asked by the learned doctor for the cause and reason that opium makes one sleep.
> To this I reply that there is in it a dormitive virtue,
> whose nature is to make the senses drowsy.
> *Chorus*: Very, very, very, well answered. The worthy candidate deserves to join our learned body. (Molière, 328)

The general lesson to be drawn from Molière's quip might be that the possession of a disposition can play no causal or explanatory role with respect to the occurrence of that disposition's manifestation, since the possession of a disposition conceptually necessitates the occurrence of the manifestation (in the right circumstances), and conceptual necessitation is not a kind of causal or explanatory connection.

Mumford states that the charge of triviality here differs significantly from the first case in that it is not a charge the force of which rests upon assuming Df_R. His point in stating this is that it cannot, therefore, be dismissed on those grounds. As he stresses, some causal power in the opium seems to be acknowledged by Bachelierus. For Mumford, since Bachelierus is asked what in the opium causes sleep when ingested, this question and his answer, in acknowledging a cause of sleep in the opium, are acknowledging something that Df_R does not. He proposes that the triviality of the exchange can be reduced to the following paraphrase:

"Q: what property in the opium causes it to induce sleep upon ingestion?

A: a dormitive virtue; that is, something that causes sleep upon ingestion."

(Mumford, Dispositions, 137).

According to him, this clearly does not rely on Df_R because the dormitive virtue in question is consistent with a functionalist understanding of dispositions such as:

"[Df_M] x is $D =_{df}$ x has some property P (and P is a cause of x G-ing if x is F-ed in conditions C_i)." (Mumford, Dispositions, 137).

Hence, he says that this brings into focus the question of whether disposition ascriptions are akin to *virtutes dormitivae* ascriptions. His answer is that they are, but at the same time he also says that in what follows he justifies their use despite the usual objections to *virtutes dormitivae*. And he asks:

"What objection to dispositions as causes can be built out of this second charge of triviality?" (Mumford, Dispositions, 137).

He makes the following remarks:

> The suggestion seems to be that dispositional properties cannot be properties at all for they are just inventions of the sophist who is ignorant of the true causes of events. Given any event, and the assumption that every event has a cause, then some power can always be invented as the cause of that event. But if that power ascription just means a cause of such an event then the power ascription will be trivially true and uninformative. (Mumford, Dispositions, 137)

He says that the questions to be faced now are whether to concede the triviality charge for dispositions and, if the charge is conceded, whether the triviality is one fatal to the causal status of dispositional properties.

He draws a distinction and suggests that there are two kinds of question that can be asked about the cause of a disposition manifestation and that whether a power ascription is trivial is different in each case. He maintains that the first kind of question is exemplified in:

"Q1. Why, whenever opium is taken, does sleep follow?" (Mumford, Dispositions, 138).

According to him, here, a *virtus dormitiva* explanation can be true non-trivially, as in the case of 'uncombable hair syndrome'. However, he declares that there is a second type of question exemplified in:

"Q2. Why does opium make one sleep?" (Mumford, Dispositions, 138).

For him, here a *virtus dormitiva* explanation is true trivially. Nevertheless, he argues that, in spite of this, dispositions can still be causes of their manifestations. He deals with each type of questions in depth, he says.

On Keith Hutchison's thought experiment about uncombable hair syndrome, Mumford argues that understanding a disposition ascription as implicitly carrying a claim of a causally relevant property is all that is required to defeat the objection: if we construe dispositions as functional properties, then it tells us that the cause of sleeping was in the opium and this is to rule out various other possibilities. For instance:

"It rules out opium merchants administering a genuine soporific each time they see someone take opium, it rules out sleep being a constant but accidental conjunct with the ingestion of opium, it rules out divine intervention." (Mumford, Dispositions, 138).

But it still doesn't inform us about the exact cause, I think. Hutchison considers the apparent *virtus dormitiva* in the statement:

"Researchers in the United States have confirmed what some parents have long suspected – that you cannot do a thing with some children's hair. And they believe unruly hair is caused by a little-known condition called uncombable hair

syndrome." (Hutchison, 246-247).

Hutchison takes uncombable hair syndrome to be akin to a *virtus dormitiva* but thinks it not useless as an explanation of unruly hair, for it assigns a responsibility for the unruly hair to the hair itself. In Mumford's words:

"This rules out parental self-delusion about their child's hair, atmospheric disturbance moving the hair, other children having ruffled it, a psychosomatic cause, the laziness of the child when it comes to combing, and witchcraft." (Mumford, Dispositions, 138-139).

Hutchison argues that *virtutes dormitivae* explanations may be false, hence they are not trivial. I can understand why Hutchison holds such a claim. However, our problem is to get access to such a falsely considered or unknown fact's true ontological background, not to reduce the putative explanations wrongly or rightly (by chance) into one – and put the burden on a suspicious functional property for no scientific reason at all. The sophist is right in his worries: We'd rather seek for satisfactory answers for the second type of questions above if we want to arrive somewhere and acquire brand new knowledge about something's real cause: structural knowledge! This is what I understand about a scientific and philosophical attitude.

Nevertheless, for Mumford, Hutchison's strategy is not satisfactory. Mumford similarly states that Hutchison is answering questions of type 1, above, instead of questions of type 2. According to Mumford, the first type of answer would have sufficed only if Bachelierus had been asked why, whenever opium was taken, sleep followed. If this had been the question, Mumford claims, the response that sleep was caused by a property of the opium would have been informative. Instead,

Bachelierus was asked why opium *makes* one sleep, which already acknowledges the causal responsibility of the opium; "thus witchcraft and atmospheric disturbance are already ruled out in the terms of the question." (Mumford, Dispositions, 139).

For Mumford:

"In answer to the question of why something causes a certain effect, the charge of triviality must be allowed to stand." (Mumford, Dispositions, 139).

Well, I don't think so. For him, a better response is to concede the triviality of *virtutes dormitivae* and dispositional explanations whenever offered as an answer to type-2 questions. After all, on the view he thinks the most convincing, dispositions are causal-role occupying properties by definition. He claims that they are functionally defined properties and a functionally defined property is bound to be trivially connected to its functional, in this case causal, role. He suggests that this point can be conceded but the triviality of a disposition ascription's explanatory content is not lethal to the causal efficacy of the disposition ascribed. He compares Df_M, above, with δ:

"[δ]: The cause of *G* caused *G*." (Mumford, Dispositions, 139).

As he emphasizes, δ would be an inadequate causal explanation of *G* but we do not want to say that, because of this, the cause of *G* was not the cause of *G*, for that would be a contradiction. And he argues similarly that if by a disposition term *D*, we mean the cause of *G*-ing upon being *F*-ed, as he claims we do mean by a disposition term, then it would be nonsense to claim that the cause of *G*-ing upon being *F*-ed was not the cause of *G*-ing upon being *F*-ed because it is logically connected to *G*-ing upon being *F*-ed.

He proposes that this is little more than an endorsement of the division

between the extensionality of the causal relation and the intensionality of the relation of causal explanation. He argues for his proposal as follows:

> If A causes B, it does so no matter how A is described (and no matter how B is described); but for description α of A to be part of an informative causal explanation of B, under description β, the intensions or meanings of α and β are relevant in that they fix their referents by picking them out in a certain way, by a certain mode of presentation. Whether these modes of presentation can be related in an informative way depends on what they are. To dismiss dispositions from causal efficacy on the ground that they provide bad causal explanations is thus to put the cart before the horse. Weakness of a causal explanation does not preclude causation between events though without causation, there is no true causal explanation holding between those events. Nothing about ontology is at stake in questions of explanation for explanatory success is contingent upon the modes of presentation of *explanans* and *explananda* and relative states of knowledge and ignorance. (Mumford, Dispositions, 140)

Causes Par Excellence

Mumford has a proposal, called causes *par excellence*, as follows:

> The functionalist theory treats concrete, non-abstract, dispositions as causes *par excellence*. The example of 'the cause of *G*' in δ was more than just an example of an uninformative attempt at causal explanation. The characterization of a property dispositionally is the description of a property according to (at least one of) its functional role(s); hence it is the reference to a property in a manner like 'the cause of *G*' rather than in a manner like 'molecular composition H_2SO_4'. Dispositions can thus regain the metaphysical role traditionally ascribed to real powers: the that-in-virtue-of-which-something-will-*G*, if *F*. (Mumford, Dispositions, 141)

He sees dispositions as causal-role occupying properties, by definition. According to him, because dispositions are causes *par excellence*, they can, in certain contexts, have an informative and explanatory role. He suggests that their ascriptions can be informative when they indicate what potencies lie within the objects of ascription. For him, they indicate what events will be causes in what conditions within an assumed context. He claims that their ascriptions can be explanatory. In his words:

> A certain occurrence can be attributed to a property of an object which

> causes the occurrence, rather than to some external condition such as atmospheric disturbance. That two objects differ in their behaviour can be explained in terms of their having different properties rather than there being other conditions active upon them. Hence, while disposition ascriptions can have a trivial, uninformative role in certain contexts, they can have non-trivial and informative roles in others. (Mumford, Dispositions, 142)

And he adds the following:

> [D]ispositions are causes though they can be poor causal explanations of their manifestations. The poverty of such explanations resides in the fact that they provide no detail of the mechanisms involved in a disposition manifestation, they tell us only that a property is possessed which plays such-and-such a causal role, thus the property is characterized only functionally, not 'structurally'. This necessary causal-role occupancy is thus the source of the *virtus dormitiva* objection, but it is an objection that is avoided if we deem it as having application only to causal explanation, rather than causation itself. (Mumford, Dispositions, 142-143)

CHAPTER III

ALTERNATIVE EXPLANATIONS

I am not satisfied by Mumford's answer. However, I am not too hopeless on the direction of our scientific pursuits to disclose structural mechanisms in nature. In our context, we can borrow Steven Weinberg's words on the issue of final laws of physics, to apply to undiscovered structural mechanisms behind dispositions:

> I don't know even if we will ever get there; in fact I am not even sure that there is such a thing as a set of simple, final, underlying laws of physics. Nonetheless I am quite sure that it is good for us to search for them, in the same way that Spanish explorers, when they first pushed northward from the central parts of Mexico, were searching for the seven golden cities of Cibola. They didn't find them, but they found other useful things, like Texas. (Feynman and Weinberg, 64)

Unlike Mumford's thinking in terms of powers, *scientifically speaking*, the word 'power' (or 'disposition') is meaningless to me – as a causal term offered in such contexts. With an example from Feynman, let me explain this view of mine better:

"[W]hat makes the planets go around the sun? At the time of (Johannes) Kepler some people answered this problem by saying that there were angels behind them beating their wings and pushing the planets around an orbit." (Feynman and Weinberg, 18).

In fact, those *angels* were gravity, of which Kepler was ignorant. And Mumford's *powers* can be good candidates for being today's *angels*. What if we replace the word 'power' (or 'disposition') with the word 'angel' each time Mumford uses it? Does it make any difference in the meanings – if they have any – of such propositions including such terms? What about the referents of such terms – as

causally relevant and efficacious properties – appearing in such propositions? Are there any? I don't think so. Where are they? They may only subsist, but not exist: capacities, powers, golden mountain, angels, round squares, so on and so forth… Briefly, such substitutions don't make any difference in reality, I think. As Feynman maintains, in physics we have to have an understanding of the connection of words with the real world we are going to do experiments with. Only in that way can we find out whether the consequences are true, he urges. And I totally agree!

As Feynman points out, when a law is right it can be used to find another one. When it comes to dispositions as causal actors (or factors), are they promising in this sense? What can we infer from them which we don't already know? Do they permit such an option? Mumford should consider such questions in the context of his account, I think.

Another thing which he should consider can be detected in the following words by Feynman:

"More recently, in the beginning of the twentieth century, it became apparent that the motion of the planet Mercury was not exactly right. This caused a lot of trouble and was not explained until it was shown by Einstein that Newton's Laws were slightly off and they had to be modified." (Feynman and Weinberg, 24).

The thing he should consider is that like the scientific laws above, if causal laws – since they count as scientific laws in explanation – need to be modified, then where should this fact carry us with respect to dispositions – as causal-role occupying properties *by definition*? Do we have to modify dispositions as well in such (or because of such) cases? Then, can Mumford talk about causal-role occupying dispositions *by definition* when they are subject to modification?

When it comes to scientific laws, and when there is no experimental way to distinguish between them – if all the consequences are the same –, psychologically they are very different in two ways, Feynman proposes. From his point of view, first, philosophically we like them or do not like them; and training is the only way to beat the disease. Second, he claims, psychologically they are different because they are completely unequivalent when we are trying to guess new laws. In his words:

> As long as physics is incomplete, and we are trying to understand the other laws, then the different possible formulations may give clues about what might happen in other circumstances. In that case they are no longer equivalent, psychologically, in suggesting to us guesses about what the laws may look like in a wider situation. (Feynman and Weinberg, 53)

Quine would agree on the incompleteness of contemporary science. But since this is the case, how can Mumford insert 'dispositions' as *causal-role occupying properties by definition* into the scientific discourse? Even if there were such entities, wouldn't they be the entities of our incomplete science(s)? What would they look like in a wider situation? Are they *promising* in this respect? They would be the limited elements of a limited science (if that were science) and its ontology and ideology… Because of such reasons, since Mumford hasn't seen the big picture yet (like a jigsaw puzzle), I don't think that it's a plausible attitude to insist on such *magical powers* and their pre-defined functions and causal efficacy.

And how can his theory account for Galileo's discovery, which Feynman reports, that the laws of physics do not remain unchanged under the change of the scale?

When we are back to Feynman, according to him, mathematics is a deep way of expressing nature, and any attempt to express nature in philosophical principles, or in seat-of-the-pants mechanical feelings, is not an efficient way. This view of his can

be used against the seat-of-the-pants thinkers who think in terms of dispositions (instead of numbers and structures), I think.

Feynman is hopeful in believing that in the end the machinery will be revealed, and the laws will turn out to be simple, like the chequer board with all its apparent complexities. The value of scientific pursuits lies within this belief, I think. In a parallel fashion, let us also recall Weinberg's words above. Quine wouldn't be opposed to Feynman's view, whereas Mumford's position is contrary to it.

Can dispositions (or powers) be used to guess new laws to complete the *chequer board*? It seems that Mumford's proposal falls short of satisfying this criterion in our incomplete science(s). Then how can he help us in our pursuit of completing science(s), since (or if) they cannot be used to guess new laws, etc.? Does his theory allow us to make room for new conjectures – other than the knowledge of dispositions we already have?

The Subatomic Structure

As Feynman states:

"The atoms all seem to be made from the same general constitution. They have a nucleus, and around the nucleus there are electrons. We can make a list of the parts of the world that we think we know about." (Feynman and Weinberg, 150).

This list includes electrons, photons, gravitons, neutrinos, mu mesons (muons), mu neutrinos, neutrons, protons, plus over four dozen more… However, we have to double the number of particles, since there are corresponding antiparticles. Why add some unnecessary suspicious elements to this list, such as 'powers', etc.?

This list includes neither capacities nor dispositions. Feynman's list is already sufficient to explain matter and interactions, i.e., all ordinary phenomena can be explained by the actions and the motions of particles.

More importantly, on the subatomic structure, Feynman adds:

"All kinds of details of the way atoms behave are accurately described with this kind of model, at least as far as we know at present. In fact, I can say that in the range of phenomena today, so far as I know there are no phenomena that we are sure cannot be explained this way, or even that there is deep mystery about." (Feynman and Weinberg, 151).

Then, I'm asking: Why dispositions or powers to fill the gaps in causal explanations? Or do we need them in science as casual-role occupying properties *par excellence*? I see nothing wrong with Feynman's structural *scientific* approach.

Feynman gives an example as follows:

> [I]n the case of hypnotism, at first it looked as though that also would be impossible, when it was described incompletely. Now that it is known better it is realized that it is not absolutely impossible that hypnosis could occur through normal physiological, though as yet unknown, processes; it does not obviously require some special new kind of force. (Feynman and Weinberg, 152)

Like powers, for instance… I think that we could paste the previous sentence of mine to the end of Feynman's last words… Feynman continues:

> Probably the most powerful single assumption that contributes most to the progress of biology is the assumption that everything animals do the atoms can do, that the things that are seen in the biological world are the results of the behaviour of physical and chemical phenomena, with no 'extra something'. You could always say, 'When you come to living things, anything can happen'. If you accept that you will never understand living things. It is very hard to believe that the wiggling of the tentacle of the octopus is nothing but some fooling around of atoms according to the known physical laws. But when it is investigated with this hypothesis one is able to make guesses quite accurately about how it works. In this way one makes great progress in understanding. So far the tentacle has not been cut off – it has not been found that this idea is wrong. (Feynman and Weinberg, 165)

However, Mumford seems to *accept* that he will never understand structures.

CHAPTER IV

DISPOSITIONS AND THE PROBLEM OF INDUCTION

Another important issue is that of course there can be spurious or random regularities, as well as the lawlike ones. We can even relate this issue with Nelson Goodman's grue paradox, which is conceived to attract our attention to the distinction between good and bad inductions. There can be accidental generalizations, as well as the nomological ones! And the regularities may change in the long run. How can we say that they won't – for sure?! In the context of our survey, my point is: Since no regularities of any sort can guarantee a causal claim, then what is the best tool to infer some – especially existential – knowledge about dispositions (or powers) and their causal efficacy? How can we *scientifically* measure or observe *real* dispositions? Empirically speaking, is there a way to get access to the knowledge of them – without presupposing their existence first? Eventually, are they epistemically inaccessible to us, like Immanuel Kant's 'noumena'? Then, wouldn't such a *Ding an sich* ("thing in itself") kind of proposal conflict with generally accepted scientific and empiricist attitudes? And how can we infer such a power from a given set of data for no regularity of any sort can guarantee a causal claim – and a singular case might be accidental, etc.?

Spurious vs. Lawlike Regularities

Suppose that everyone here is seated. Then, trivially, that everyone here is seated is

true. Though true, this generalization does not seem to be a law. It is just too accidental. Einstein's principle that no signals travel faster than light is also a true generalization but, in contrast, it is thought to be a law; it is not nearly so accidental. What makes the difference?

This may not seem like much of a puzzle. That everyone here is seated is spatially restricted in that it is about a specific place; the principle of relativity is not similarly restricted. So, it is easy to think that, unlike laws, accidentally true generalizations are about specific places. But that's not what makes the difference. There are true nonlaws that are not spatially restricted. Consider the unrestricted generalization that all gold spheres are less than one mile in diameter. There are no gold spheres that size and in all likelihood there never will be, but this is still not a law. There also appear to be generalizations that could express laws that are restricted. Galileo's law of free fall is the generalization that, *on Earth*, free-falling bodies accelerate at a rate of 9.8 meters per second squared. The perplexing nature of the puzzle is clearly revealed when the gold-sphere generalization is paired with a remarkably similar generalization about uranium spheres:

All gold spheres are less than a mile in diameter.

All uranium spheres are less than a mile in diameter.

Though the former is not a law, the latter arguably is. The latter is not nearly so accidental as the first, since uranium's critical mass is such as to guarantee that such a large sphere will never exist. What makes the difference? What makes the former an accidental generalization and the latter a law?

Some laws are vacuously true: Newton's first law of motion – that all inertial bodies have no acceleration – is a law, even though there are no inertial bodies. But

there are also lots of vacuously true nonlaws: “all plaid pandas weigh 5 lbs., all unicorns are unmarried, etc..” (Carroll).

The majority of contemporary philosophers are *realists* about laws; they believe that some reports of what are laws succeed in describing reality. There are, however, some *antirealists* who disagree.

Not being a coincidence is not all there is to being a law. For example, it might be true that there are no gold spheres greater than *1000 miles* in diameter because there is so little gold in the universe. In that case, strictly speaking, that generalization would be true, suitably general, and not a coincidence. Nevertheless, that would not be a law. Arguably, what blocks this generalization from being a law is that something in nature – really, an initial condition of the universe, the limited amount of gold – accounts for the generalization. Contrast this with the law that inertial bodies have no acceleration. With this generalization, it seems that it is true *because of nature itself*. It seems that for a generalization to be a law, nature itself must be what makes the generalization true.

Goodman thinks that the difference between laws of nature and accidental truths is linked inextricably with the problem of induction. In his *The New Riddle of Induction* (1983), Goodman states that only a statement that is *lawlike* – regardless of its truth or falsity or its scientific importance – is capable of receiving confirmation from an instance of it; accidental statements are not. He claims that, if a generalization is accidental (and so not lawlike), then it is not capable of receiving confirmation from one of its instances.

As Carroll rightly emphasizes, the confirmation of a hypothesis or its unexamined instances will always be sensitive to what background beliefs are in

place. So much so, that with background beliefs of the right sort, just about anything can be confirmed irrespective of its status as a law or whether it is lawlike. Thus, stating a plausible principle describing the connection between laws and the problem of induction will be difficult. In order to uncover a nomological constraint on induction, something needs to be said about the role of background beliefs.

The Historical Roots of the Problem of Induction and Causality

When we come to the historical roots of the problem of induction, as John Vickers reports, the term 'induction' does not appear in David Hume's account. Hume's concern is with causality and, in particular, with the nature of causal inference. His account of causal inference can be simply described, says Vickers:

"It amounts to embedding the singular form of enumerative induction in the nature of human, and at least some bestial, thought." (Vickers).

According to Hume:

"[W]e may define a cause to be *an object, followed by another, and where all objects similar to the first are followed by objects similar to the second*. Or, in other words, *where, if the first object had not been, the second never had existed*." (Hume, Enquiries, 60).

Hume considers a cause to be an object followed by another, and whose appearance always conveys the thought to that other.

If we have observed many *F*s to be *G*s, and no contrary instances, then observing a new *F* will lead us to anticipate that it will also be a *G*. That is causal inference.

As Hume puts, it is clear that we do make inductive, or, in his terms, causal, inferences; that having observed many *F*s to be *G*s, observation of a new instance of an *F* leads us to believe that the newly observed *F* is also a *G*. It is equally clear that the epistemic force of this inference does not reside in the premises alone:

All observed *F*s have also been *G*s,

and

a is an *F*,

do not imply

a is a *G*.

It is false that "instances of which we have had no experience must resemble those of which we have had experience." (Hume, Enquiries, 89).

Hume's view is that the experience of constant conjunction fosters a 'habit of the mind' that leads us to anticipate the conclusion on the occasion of a new instance of the second premise. The force of induction, the force that drives the inference, is thus not an objective feature of the world, but a subjective power, the mind's capacity to form inductive habits. The objectivity of causality, the objective support of inductive inference, is thus an illusion, an instance of what Hume calls the mind's "great propensity to spread itself on external objects." (Hume, Enquiries, 167).

Good vs. Bad Induction

As Vickers point out, Hume's account raises the problem of induction in an acute form: One would like to say that good and reliable inductions are those that follow the lines of causal necessity; that when

All observed *F*s have also been *G*s,

is the manifestation in experience of a causal connection between *F* and *G*, then the inference

All observed *F*s have also been *G*s,

a is an *F*,

Therefore, *a*, not yet observed, is also a *G*,

is a good induction. But if causality is not an objective feature of the world this is not an option. The Humean problem of induction is then the problem of distinguishing good from bad inductive habits in the absence of any corresponding objective distinction.

As Vickers rightly stresses:

> Two sides or facets of the problem of induction should be distinguished: The *epistemological* problem is to find a method for distinguishing good or reliable inductive habits from bad or unreliable habits. The second and deeper problem is *metaphysical*. This is the problem of saying what the difference is between reliable and unreliable inductions. (Vickers)

And he continues:

> Now as concerns inductive inference, it is hardly surprising to be told that the epistemological problem is insoluble; that there can be no formula or recipe, however complex, for ruling out unreliable inductions. But Hume's arguments, if they are correct, have apparently a much more radical consequence than this: They seem to show that the metaphysical problem for induction is insoluble; that there is no objective difference between reliable and unreliable inductions. (Vickers)

As a short digression, when we come to our main issue, dispositions, in parallel with Hume's *habit*, Fara says:

"Habituals can be true 'by accident' in a way that disposition ascriptions cannot. It might be true, just as a matter of bad luck, that it rains when I leave the house. But the truth of this habitual need have nothing to do with any dispositions,

either of me or of the weather." (Fara).

After all the above, it seems the discussions raise doubts about whether things objectively have dispositions and about whether causal efficacy is objectively in things or is just an illusion of the mind.

CHAPTER V

DISPOSITIONS RECONSIDERED

According to Mumford, although in general dispositions are not identical with categorical properties, each *instance* of a disposition is identical with an *instance* of a categorical property, namely its causal basis. For example, on Mumford's view brittleness is not identical with microstructural property so-and-so; but my glass vase's particular brittleness *is* identical with its particular microstructural property so-and-so. Thus Mumford is "relying upon a notion of property instances or states such that instances of particular dispositions and instances of categorical properties can be identified in what amounts to a token-token identity theory." (Mumford, Dispositions, 159).

Anticipating such a view, Elizabeth Prior complains that it is not at all clear what can be meant by 'property instance' if, as Mumford maintains, an object's instance of property *A* can be the very same thing as its instance of property *B*, even though property *A* is not the same as property *B*.

Generally, we regard dispositions as being causally responsible for their manifestations. We say that the glass breaks because it is fragile, that the rubber band stretches because it is elastic, and that the arsenic kills him because it is lethal. Some philosophers deny this. According to them, dispositions are causally irrelevant to the effects in terms of which they are defined.

This view is defended by Prior *et al.*. According to them, fragility is the second-order property of having some or other first-order property (e.g., a given

molecular structure) that tends to cause breaking under certain circumstances. But then, they infer, it is this first-order feature (the 'causal basis' of the glass' fragility), and not fragility itself, that is responsible for causing the breaking. Fragility is thus conceptually after the fact as concerns the causation of breaking: the glass counts as being fragile only in consequence of its having some other, first-order property that is causally responsible for its breaking when struck. In their words:

> By the Causal Thesis, any disposition (and thus fragility) must have a causal basis. This causal basis is a sufficient causal explanation of the breaking *as far as the properties of the object are concerned*. But then there is nothing left for any other properties of the object to do. By the Distinctness Thesis the disposition is one of these *other* properties, ergo the disposition does nothing. (Prior, Pargetter and Jackson, 255)

As Paul Raymont reports:

> (David) Lewis has always seemed uneasy with this view. He has called it a 'disagreeable oddity' that must be dispatched if the identification of dispositions with second-order properties is to win our unequivocal support. In a recent paper, he takes himself to have done just that. He begins by saying that, 'Sometimes, an event (...) is a having of a certain property by a certain thing'; and sometimes, he continues, 'Two different properties are had in the same single event.' Consider, for instance, the event that consists in the 'having of the [first-order] causal basis' of the glass's disposition to break when struck. According to Lewis, 'This same event is a having of the second-order property,' (viz., fragility). That is, the glass's possession of the molecular structure which serves as the causal basis of its fragility is all there is to its being fragile. The 'havings' of these two properties are one and the same entity. Thus, since the glass's possession of that molecular structure is a cause of its breaking, so too is its fragility. (Raymont)

This attempt to restore causal potency to dispositions does not succeed. With Raymont, let us suppose that some object (c) is F, and that its being F causes e to be G. It does not follow that if c's-being-F is the same thing as c's-being-H (if, in Lewis's terms, these two havings are the same event), then c's-being-H will be causally responsible for e's-being-G. In Raymont's words:

> To see why, suppose that I, weighing 165 pounds, tip the scales, and that anyone weighing more than 120 pounds would also tip them. Clearly, my having the property of weighing more than 120 pounds is efficacious with

> respect to tipping the scales. But I do not tip the scales merely by weighing more than 50 pounds – if I had weighed only 55 pounds, the scales would not have tipped. So my having the property of weighing more than 50 pounds is not causally responsible for tipping the scales. And yet my weighing more than 120 pounds and my weighing more than 50 pounds are one and the same event, for my instantiation of the former property (by weighing 165 pounds) is all there is to my instantiation of the latter one. (Raymont)

As Raymont rightly argues, although his weighing more than 120 pounds is efficacious with respect to tipping the scales, his weighing less than 180 pounds is surely not causally responsible for that effect. This is so in spite of the fact that in my case, these two properties are had in the same single property instance. The same sort of objection can be applied to Lewis's account, substituting 'event' for 'property instance', says Raymont.

In his words:

> To clarify, the criticism is not directed at the claim that the event (or property instance) of my weighing less than 180 pounds is a cause of my tipping the scales. It would be wrong to deny this claim, since its truth follows from the identity of the aforesaid event with the event of my weighing more than 120 pounds (which really did cause the scales to tip), together with the extensionality of the causal relation. Instead, the criticism is intended to show that it is not in virtue of its being an instantiation of the property of weighing less than 180 pounds that the event causes the scales to tip; and that its being a 'having' of this property is therefore causally irrelevant to that effect. In short, Lewis's willingness to countenance events that incorporate more than one property leaves him open to the objection that not all of those properties need be relevant to the events' effects. (Raymont)

The focus thus far has been on a negative argument, to the effect that Lewis has not vindicated the causal relevance of dispositions to their own manifestations. This is just what one would expect, since, as will now be argued, dispositions really are causally impotent.

Let us continue with Raymont:

> This view of dispositions is modeled on Jaegwon Kim's erstwhile view that each mental property is reducible to the disjunction of its empirically (or 'physically') possible physical realizers. Unfortunately, once this parallel is noticed, it becomes evident that even though the strategy of

> extensional definition effects a tighter connection between dispositions and their causally relevant first-order bases, dispositions thus conceived do not inherit the causal powers of their first-order realizers. This is because such 'wildly disjunctive' properties as the ones to which Kim would reduce mental features (and to which we have considered reducing dispositions) are not fit to appear in genuine causal laws. In saying this, one need not dispute the claim that disjunctions of properties, even infinitely long disjunctions, are themselves properties. The point is rather that even if they are properties, the possession of them in no way augments the causal powers of their bearers. For something can have the disjunctive property, F1 or F2 or . . . Fn (to which fragility has supposedly been reduced), only by having one of the properties that is cited in its disjuncts, say, the molecular structure F1. Any such thing will act as it does by virtue of having that property. The fact that F1 is part of some other, disjunctive property adds nothing to the thing's causal powers. (...) It may seem odd to deny causal relevance to a disjunctive property in spite of its appearance in generalizations that support counterfactuals and predictions. However, none of these claims in which it appears asserts a law-like, causal connection between the disjunctive property and anything else, for none of these claims is confirmable by its instances. The importance of confirmability as a test of lawhood has been emphasised by David Owens. He notes that while genuine laws are susceptible of confirmation by their instances, generalizations that invoke wildly disjunctive properties in their antecedents and non-disjunctive features in their consequents are not. For example, consider the generalization,
>
> (P) For all x, if x has F1 or F2 or . . . Fn, then x breaks when struck. (Raymont)

Suppose we observe something that has F1 and that breaks when struck, and that thus provides an instance of (P). This does not count as evidence in favour of (P), since it equally supports the following 'rival' of (P) (i.e., a claim that yields predictions that are contrary to those yielded by [P]): (Q) For all x, if x has F1 then it breaks when struck, and if x has F2 then it does not break when struck. In short, while the fact that something which is F1 is observed to break when struck certainly lends credence to the general claim that whatever is F1 breaks when struck, it gives no indication as to the behaviour of things that are F2, and thus equally supports any generalization that conjoins the claim that F1-things break when struck with any claim whatever concerning the behaviour of F2-things. To give equal support to all such generalizations is to confirm none of them.

Note that since any given instance of (P) will count as such an instance only by virtue of instantiating one of the disjuncts in (P)'s antecedent (as well as the property described in (P)'s consequent), we can always in like manner construct a rival of (P) that is equally supported by that instance, thereby showing that the instance at hand does not confirm (P). Since (P) is not confirmed by any of its instances, it is not a causal law.

Hence, let us give our final decision on the issue with Raymont:

> It must be concluded that dispositions are not causally relevant to their manifestations. Still, we must acknowledge that they are not entirely devoid of explanatory significance. We do in fact say that the glass broke because it was fragile, and that the rubber band stretched because it was elastic. But this explanatory significance is of a relatively low grade. It does not bring with it causal relevance. One may ask how a feature can be explanatorily relevant, and even causal-explanatorily relevant (i.e., relevant to a causal explanation), without being causally relevant simpliciter. Ned Block has shown how, taking dormitivity as his example of a disposition. According to Block, the claim that I fell asleep because I took a dormitive pill is causal-explanatory, 'Because it rules out alternative causal explanations of my falling asleep.' For instance, it rules out saying that I fell asleep because I had had no sleep the night before. The appeal to dormitivity rules out alternative explanations (and is thus causal-explanatorily relevant) because it 'brings in' or 'involves an appeal to' the first-order chemical property of the sleeping pill that is genuinely relevant to causing sleep. It is perhaps more accurate to say that our appeal to the pill's dormitivity merely locates the first-order causally relevant property (in the chemical composition of the pill) without specifying it, thereby 'flagging' or outlining the causal path that culminated in my falling asleep. Switching metaphors, it helps to trace the contours of this causal chain without mentioning any of the causally relevant features from which it is forged. I dub such accounts 'flagging' explanations. (Raymont)

We can thus appeal to dispositions in explanation of their manifestation without being committed to their causal relevance. For our appeals to dispositions occur within intensional, explanatory contexts in which the causally relevant factors are merely located without being specified.

CHAPTER VI

CONCLUSION

It is better to finish with Feynman, who counts the three pillars of scientific progress as follows:

> If science is to progress, what we need is the ability to experiment, honesty in reporting results – the results must be reported without somebody saying what they would like the results to have been – and finally – an important thing – the intelligence to interpret the results. An important point about this intelligence is that it should not be sure ahead of time what must be. It can be prejudiced and say 'That is very unlikely; I don't like that'. Prejudice is different from absolute certainty. I do not mean absolute prejudice – just bias. As long as you are only biased it does not make any difference, because if your bias is wrong a perpetual accumulation of experiments will perpetually annoy you until they cannot be disregarded any longer. They can only be disregarded if you are absolutely sure ahead of time of some precondition that science has to have. In fact it is necessary for the very existence of science that minds exist which do not allow that nature must satisfy some preconceived conditions, like those of our philosopher. (Feynman, Law, 148)

For the sake of science(s), I am not taking Mumford's teleological thesis – with such an unnecessarily great amount of metaphysics – for granted! When it comes to teleology, for instance, what is the *telos* of the appendix (as an internal body part), or men's nipples? There is none. The teleological approach is insufficient to account for matter and natural relations among existents.

Dispositions are causally impotent. Structures are sufficient in giving scientific (causal or non-causal) explanations. Dispositions are not causally relevant to their manifestations. Still, we must acknowledge that they are not entirely devoid of explanatory significance. But this explanatory significance is of a relatively low grade. It does not bring with it causal relevance.

For the various reasons above, in concluding my thesis, I consider dispositions as causally impotent. At least in theory, it is possible to discover the structures behind the dispositional properties. Categorical properties constitute the essence of our accumulative sciences. The last words about the categoricals haven't been said yet, for our sciences are incomplete. *Powers* are the inventions of the sophist, who is ignorant of the mechanisms behind natural interactions.

What about the fundamental distinction between structural and dispositional properties? What is a disposition? Dispositions are typically contrasted with 'categorical' properties. But we might equally well ask: What is a categorical property? There are some clear cases: fragility, solubility, irascibility are dispositions, while massiveness and triangularity seem to be non-dispositional, or categorical. But why? What is it about the one group of properties, the dispositions, that sets them apart from the others? The traditional answer to this question is that ascriptions of dispositions do, while ascriptions of categorical properties do not, entail certain subjunctive conditionals. The fragility of my glass vase, on this view, requires that the vase would shatter or crack if it were dropped or knocked. On the other hand, Mumford's view about the distinction between dispositional and categorical properties can be separated into two components. First is the point that ascriptions of dispositions entail corresponding conditionals *as a matter of conceptual necessity*. This part of Mumford's view is just a special case of the unsurprising but important fact that, in general, an analysis of a certain class of statements purports to display not just a *modal* relation between analysandum and analysans, but a *conceptual* relation between them. Whatever the right analysis of disposition ascriptions turns out to be, ascriptions of dispositions will be unique not

in *entailing* their analysanses but in entailing them as a matter of conceptual necessity. The second component of Mumford's view is the thesis that ascriptions of dispositions entail *conditionals* as opposed to some other kind of statement. In responding to this schema, I have pointed out the importance of structural and categorical bases in dealing with sciences and giving *true* explanations of the *real causes* behind the dispositional ascriptions.

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