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Causality and Medicine

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INTRODUCTION

The philosophers of science who viewed causality as a metaphysical headache were right. Yet when they concluded that it is of no scientific import and of less practical import, they were clearly in error. I say clearly because they thereby recommended that we replace cause by mere empirical correlation, which obviously will not do. Here is an obvious example which proves them in error without even touching upon the question of what science is.

Smoking is correlated to both cancer of the lungs and emphysema. We all agree that the correlation to emphysema is satisfactory because (notice this “because”) we share the idea that emphysema occurs because (is this the same sort of “because” as the previous one?) of scars that happen because (is this the same sort of “because” as the previous one?) of wounds caused (is this the same sort of “because” as the previous one?) by scratches made with foreign bodies. That is to say, we imagine minute particles in smoke inhaled by patients that scratch the outer surface of the lungs the way gravel or sand scratches our backs when we slide hard. We have some ideas about the causes of cancer, but they are neither elaborated in as much detail nor agreed about. And so the evidence for the correlation between smoking and cancer of the lung is merely empirical. And this is deemed unsatisfactory both by those who endorse the evidence and those who reject it.

We should stress that the rejection of the correlation of smoking and

emphysema is *prima facie* a sign of ignorance but not the rejection of the correlation of cancer of the lung and smoking. I say *prima facie*, because one who rejects a correlation which seems obvious may have a new good reason for his rejection, and vice versa; but let us keep for a while to the *prima facie*. The person who denies that smoking causes cancer of the lung need not reject all the evidence marshaled by the U. S. Surgeon General in support of that contention. The denial merely means that if you stop smoking you do not improve your chances of avoiding cancer of the lung. It may be supported by some statistics – I do not know how reliable – that in big cities the disease is prevalent and in small islands rare, yet in each place there is little or no significant difference between smokers and nonsmokers. Of course, the endorser of the correlation will put the blame on smoke in the city atmosphere will conclude that legislation should forbid smoking in public as a public hazard. And, of course, those who reject the correlation will say that both smoke and cancer are correlated to big city life and so, by default, also to each other.

What I wish to stress here are two points: one in criticism and one by way of posing the emerging problem, the problem of the present essay. The criticism is simple. Whether right or erroneous, and regardless of our views of the nature of science, the previous paragraph makes sense which is clear enough to lay and to medical people alike, practicing or in research. Yet it cannot possibly make sense if we banish causality altogether and speak of nothing but constant conjunction (Hume) or functionality (Mach) or statistical correlation (Reichenbach) or some similar pure description with no metaphysics or ontology. That is not to say that to speak of causes is as easy as pie: it is problematic for sure; but whereas without causality the world looks too easy and unproblematic, like the world of mere surface, or mere ghosts and shadows, with

causality we bump into real things and at times recoil with pain. There are many problems regarding causality, and it is the measure of progress of contemporary philosophy that it gropes with them, at times with a modicum of success. In particular, it is possible now, I contend, to use recent progress in order to solve the following problem that may emerge from contrasting our previous example of the causes of lung cancer with some other examples.

As we saw, having no causal explanation of some alleged fact at times justifies our skepticism about the truth of the allegation. Yet, it is a well-known sign of dogmatism to deny any allegation of fact as long as it is unexplained. Many facts are utterly unexplained and even very puzzling and disturbing, and as long as we do not admit them we will not seek their explanation. Indeed, the very postulate of the existence of vitamins was made before these could be isolated as a step toward the explanation of diseases caused by vitamin deficiency. And, I should add, this was at the time of the victory of Pasteur's germ theory of disease, when it was as puzzling to meet with vitamin deficiency diseases as it was natural to seek their causes by attempts to isolate some germs responsible for them.

The question, then, is too obvious: When is it reasonable to doubt unexplained correlations and when is it sheer dogmatism to do so?

For, as we saw, it is sometimes reasonable and it is sometimes unreasonable to doubt unexplained correlations. *When?*

I shall now briefly survey the study of causality in an attempt to present my preliminary answer to this question. I claim that doing so will help us view medicine as a Science proper rather than a mere art, a mere correlation, a mere knack of discerning right and wrong correlations.

To conclude this lengthy introduction, I should explain my own position

vis-à-vis my readers. As I say, I find hostility to metaphysics eminently comprehensible, since metaphysical problems are ever so frustrating. (An example will be offered very soon, from the problem of causality.) Yet, of course, I think the hostility is misplaced because the frustration is real and unavoidable. The result of this is that most people today, most practicing physicians and medical research workers included, are at times anti-metaphysical, at times blatantly metaphysical themselves. Nor is this so very surprising or objectionable: it is only very expensive. I therefore invite my reader to an experiment in holding a somewhat more consistently metaphysical view for a while and trying it on for size. The reader has nothing to lose but some illusions and, I think, a lot to gain.

DOES THE STONE SHATTER THE GLASS WINDOW?

The impatience practical people show with philosophy is, to repeat, abundantly understandable. One of the greatest discoveries of modern philosophy is David Hume's observation that we do not see a stone shatter a glass window. We can of course repeat the experiment as long as we have enough glass at hand: hurl a stone, perceive it approach the glass pane and penetrate it, and perceive the simultaneous disintegration of the glass into splinters. We may take high-speed motion pictures of the event and reproduce it in slow motion, in enlargements, in different lightings, and so on. We may be fascinated by the repeated experience, and be deeply convinced of the causal nexus between the impact of the stone on the glass window and its going to pieces. But, says Hume, in no way can we see, actually observe, perceive, sense, the causal connection. There is no sensation of a causal nexus even if there is a complete picture of all the details pertaining to it down to the smallest fractions.

This story is very disturbing and aggravating. Reader of the present essay who do not find it disturbing may make an experiment and show it to

friends and relations and see them aggravated by it. The more they will wish to deny, refute, resist, combat, dismiss, perhaps even ridicule, David Hume's thesis repeated in the previous paragraph, the more aggravated they will be. Finally they will be exasperated and try to dismiss it as a mere sophism. Questions:

1. Is Hume right?
2. Does it matter?

These are momentous questions. They chime and ring loud and disturb and reverberate throughout the halls of fame of the philosophy of science. The history of the philosophy of science of modern times is the history of attempts to justify the answers to them. In the present essay I shall argue the following.

1. In a sense Hume is right; in a sense not.
2. For some discussions and their histories Hume's argument matters a great deal, but by and large it is now past history. The sooner we all realize this, the better for us.

The sense in which Hume is right is now very easy to articulate: the theory of perception, which he held, left no room for the perception of causes. And this verdict is final. The theory in question is known as sensationalism, and is one which paradoxically stands out in the history of science both as the most popular through the ages up to and including our own age, and as the one most often refuted, especially by the many exciting experiments in perception reported in recent decades. The modern version of sensationalism, which gained popularity in the Age of Reason, is the one formulated by John Locke. It is the theory which says that our perceptions are collections of single separate elements of perception or sensations, which we put together in order to have ideas

about our environment. Of course, those who still believe it to be true deny that it ever was empirically refuted. And though most of them are philosophers and natural scientists, some of them are psychologists and even perception theorists. But this is another matter.

Back to Hume. He showed that according to sensationalism causality cannot be perceived. And he assumed that sensationalism is true. What could he do in that situation? What can anyone do in that situation? Supposing we cannot perceive causality, can we postulate perhaps that it exists? Opinions differ. Some say, with Hume, the very concept of causality is meaningless, and when we say that A causes B at most we can mean to say that whenever A occurs B also occurs. This, of course, is much less than what we want to say, for we do say that night always follows day without wanting to say day causes night, and we do say shattered glass follows hurled stone, wanting also to say stone causes glass to shatter. The question is, can we say more? Some philosophers say no; some say yes. Those who say no say our wish to say more is a mere illusion. The most we wish to say is about something we can never put our finger on for, suppose we do postulate causality; can we ever prove it in at least one instance? No. Can we say, probably in such and such a situation a certain A causes a certain B? Some say yes; some say no. Hume says no all the way and for the same reason.

In the sense in which Hume is right he indeed is right all the way: the force of his discovery is the force of logic itself. Given sensationalism, whether Locke's theory of perception or rather an improved modern variant of it, we can deduce Hume's conclusions with as much validity as any deduction outside the strict field of formal mathematics has ever attained. We can conclude that we cannot see causality, that we cannot assert causality; we likewise conclude that even though in the past stones

hurled at glass windows were followed by shattered windows, this evidence does not tell us that tomorrow's hurled stone will tomorrow be followed by shattered glass. There is not even high probability that past events will repeat themselves: we cannot learn from past experiences alone about future events.

Oh, I admit, many philosophers of good standing and high repute disagree with my modern verdict of Hume's correctness. Indeed, deeper than the schism between scientific philosophy and irrationalism of all sorts is the schism within the camp of scientific philosophers, between those who follow Russell and Wittgenstein and Popper in endorsing Hume's conclusions and those who follow Husserl and Wittgenstein and Carnap in repudiating Hume's conclusions. I cannot help it and must now leave it at that.

What are we to do about Hume if we agree with him? We may feel disgusted with the whole affair and go play backgammon. This was the reaction Hume himself confessed to feeling. We may feel exasperated with philosophy and turn our attention to science: This was reasonable too, but, to repeat, it may be more profitable to try to do better. There are two other ways still open to us. The first is to deny the truth of the sensationalist theory of perception, which, I say, has been amply refuted anyway. The other is to deny the truth of the sensationalist theory of meaning or the theory of the language of perception as it is sometimes called. Indeed, once sensationalism is rejected, it becomes obviously pointless, if not downright impossible, to stick to the sensationalist theory of meaning. In any case, most modern linguists and psycholinguists say it too has been amply refuted by empirical means. All this may sound quite discouraging, but it really is quite inviting and hopeful. The road open to us once we remove both of these obstacles is quite exciting.

DO WE SEE THE STONE SHATTER THE GLASS WINDOW?

There is one important fact that those modern thinkers who reject sensationalism (as a theory of perception and/ or meaning) take as their point of departure. It is a fact which is surprisingly very easy to ascertain. It has been ascertained by many empirical researchers, whether perception theorists, child psychologists, criminologists, or others. Irrationalists have repeatedly presented it as the reason to eschew rational philosophy altogether. The fact is this.

We do not know what we perceive, we see meaningful facts but not the items that make us see them.

To take a simple analogy, consider a novel you read, or a political speech you heard, or a movie you saw. Try to report a detail from it from memory. It does not matter what kind of detail, or what size. Try to note how you know the detail which you have just reported. At times the answer is simple: you remember a sentence from the novel word for word, or you can hear in your mind's ear a resounding expression from the speech, or you saw the scene in the movie and the images can be flashed again in your mind's eye at will. More often than not you do not remember the detail you have just reported as described in the novel, speech, or movie. The way the novelist made you imagine things, the way the politician made you surmise the facts you have surmised, the way the movie maker made you see what you remember having seen, are parts of their art. Not only do we all report having read, heard, or seen in a movie scenes that were only hinted at, we at times report a scene that was described, narrated, or shown, yet cannot remember what it was. One hero makes some threatening gesture to the other, and we remember there was a threatening gesture but not what it was; we remember that one hero said something to anger the other, yet we do not know what was allegedly

said. At times the novelist tells us that the annoying expression was made, and at times while repeating it in all its local forgettable colors. At times the movie is so made that nothing clear can be heard off the sound track, yet the meaning of the insult is conveyed in a crystal-clear manner that makes us gasp. Indeed, it is the well-made forceful movie that only suggests the crucial remarks so that each member of the audience feeds into the movie whatever takes his fancy. In the most detailed novel of a nineteenth-century (pseudo-)naturalist writer the crucial sentence is cleverly omitted only to be filled by the reader's active imagination.

Lest this sound remote from science and technology, let me say that these facts were always crucial to both modern science and its official philosophy. For, both Sir Francis Bacon and Rene Descartes noticed that because we feed our ideas into our observations our studies are at best worthless unless they are very, very careful. It is the very careful repetition and analysis of our perceptions that has made Locke, the disciple of Bacon, invent his classic variant of sensationalism, and it was Locke's philosophy that Hume said he was presenting in a better, more streamlined, and consistent variant. And on this, to repeat, Hume was both right and so disturbing.

The modern refutation of sensationalism, then, is not the mere fact that we read meaning into perception but the further allegation that *we cannot avoid doing so*. We constantly read meanings into perceptions, we constantly see meaningful facts and *we do not know what portion of our observation is perception and what portion of it is theorizing*. (This thesis was empirically demonstrated already by William Whewell and by Hermann von Helmholtz – both, quite significantly, scientists and empiricist philosophers who followed the anti-empiricist philosophy of Immanuel Kant and tried to reconcile it with empiricism!)

Some people take this significant fact to be license, and in the following way. Classical philosophy of science employed sensationalism in order to relieve us of our subjective injection of private meanings into publicly attested objective facts. But this venture has failed. Hence, these modern irrationalists conclude, we may now feel free to inject subjective meanings, and each to their own. It is now, they say, more or less free for all. The question of how one decides which meaning to read into facts was answered by Maurice Merleau-Ponty in his world-famous mind-blowing *The Primacy of Perception* by saying one makes an irrational decision and commits oneself to a dogma, and that is that. More recently Paul Feyerabend went even further and declared, “anything goes!”

I think that this is a bit of an exaggeration, that there are better and wiser ways to go about than mere arbitrariness – whether to make one arbitrary decision or a series of them. In particular, we can try and see why it is that, though we all project meanings into what we see, some of us see better than others, and some of us see interesting things while others see nothing to report. Of course, here also we read meanings into facts, and we do not quite know the facts. When we say that Roentgen discovered X-rays we read meanings into the facts, and we do not know the facts. Most of us do not know the facts. Most of us did not even read Roentgen’s original report, but even those who did cannot remember quite what he said, and what he said is not quite what he saw. Similarly with, say, Fleming’s discovery of penicillin, which certainly contained no “observation of the compound by that name, which contained an observation of a failed culture, an observation – alas, all too common before “and after Fleming. Indeed, many people who lived at the time of the discovery can easily say why Chain’s contribution – the isolation of the antibiotic and its successful application – was of import but are

uncomfortably too vague about Fleming!

Let me stress this fact and conclude this discussion by explaining why the response to Hume's study by disregard to the whole matter, though reasonable, is not the best known to us. For, we all do inject meanings into perception, and we may well wonder how we do it, how those of us known as better or keener observers do it, and how the rest of us do it, and perhaps also by what means can we learn how best to do it. Most of those who give up Hume's problem remain sensationalists. They are convinced that they do perceive correctly and in some causal mode or another; Hume's criticism of their conviction notwithstanding, they are convinced that perception causes them to see causation. Thus, giving up the whole study makes them unable to benefit from Hume's criticism. But benefit they can. This is the road open once the obstacles of sensationalism are removed: we can observe ourselves observing, develop some theories of observation, and hopefully use these theories to improve our powers of observation. All this has been developed in recent years, and it much deserves a synoptic look.

DO WE KNOW HOW THE STONE SHATTERS THE GLASS WINDOW?

Let me make some empirical observations. At times we observe – I use the word “observe” to mean perceive and at the same time read meaning in what we perceive – a causal nexus with no explanation, with no idea of a causal mechanism; in such cases we may say that we make a purely empirical observation. Examples abound, especially in medicine; so much so that many say, while reporting such observations, medicine is largely purely empirical, or medicine is an art, or we are still groping, or some other remark to that effect. That is to say, such remarks come to justify the fact that we accept an observation of a causal nexus despite the fact that it comes with no causal mechanism to explain it or to nail it down.

Why this justification? Justification indicates possible - but not actual - culpability, a culpability it purports to dispel. What is this rejected culpability? Still better, is there prior to the justification any cause for suspicion of any culpability? If yes, then of what kind? If not, why justify?

I honestly do not know. I wish, however, first to observe that the suspicion does exist, and second to attempt to explain it. I do observe that many research workers, particularly those who are reputed for their scientific caution, at times refuse to endorse an observation of a causal nexus on the very ground that the observer has not offered any explanation of the causal nexus, any postulation of a causal mechanism.

I have offered examples in my introduction, and I see no need to weary the readers with more examples, because I trust them to be able to furnish all by themselves quite a number of additional examples from their own experiences, some that go hither and some that go thither, convincingly or unconvincingly as the diverse cases may be. Nevertheless I wish to report an additional example, not because it is needed but because it is one that has impressed me personally very strongly. The famous physiologist W. B. Cannon, the father of the modern theory of homeostasis (*The Wisdom of the Body*) has observed a case of “voodoo death”, that is, murder by magic. His observation was utterly ignored until a biochemical mechanism was offered to explain it. It was explained, incidentally, in a manner very similar to Cannon’s own way of theorizing. It was seen as a death caused by a kind of shock created by the secretion of two different chemicals, one related to fear, one to despair, each alone seemingly beneficial, but a large dose of both allegedly lethal. I do not know if the theory about the mechanism is true, yet it also explains another repeatedly reported alleged fact, namely, that primitive people can effectively will

themselves to die. As a result this report is nowadays accepted too.

Let me stress this. The claim made on the strength of observation, namely, that death by mere psychological means is quite possible, this claim was not accepted until some experiments showed that fear secretes one hormone, that despair secretes another, and that together these can kill a mouse. The facts of this endocrinological claim are not well-attested, and even if they were one could doubt their relevance to the facts they allegedly help explain. Yet in historical fact it was the doubtful explanation that legitimized the less doubtful factual reports.

Does this not permit us to adopt the unfair and biased policy of discrimination? Is it not possible to arbitrarily use two different standards? Suppose we would allow ourselves at times to offer some observation of empirical facts of causal nexus even without explanations by causal mechanisms; that is to say, if and when we like the observations in question. And suppose we would reject other observations of such empirical facts on the ground that we have no knowledge of the mechanisms which allegedly bring them about; that is to say if and when we dislike or suspect the observation in question. Will this be irrational? Will this be permissible?

But there we go again. It is perhaps possible to behave as unfairly as that, but most students of science, we can assume are usually not that unfair. Granted. But how do we know that? We can answer by saying that we feel convinced when scientists explain, why they accept one observation of a causal nexus without an explanation of it by a causal mechanism, and we feel convinced when the same scientists explain why they reject another because they want first to know the causal mechanism. Yet this answer will not do. We feel convinced, no doubt; but the feeling may be caused by nothing more than self-deception; it may mean no more than

that we are accomplices in the scientists' unfair conduct.

No; I do not mean to hint that every scientist is biased who at times endorses an empirical observation of a causal nexus on purely empirical grounds and at times rejects an empirical observation on account of the absence of any theory about the causal mechanism allegedly behind it. Indeed, I think all scientists are prone to such discriminations, and rather than condemn the conduct they all share as reasonable and arbitrary it may be much more interesting and useful to explain it. We may try and see when it makes sense to demand a causal explanation, and what function the explanation performs once it is offered; if we have such a theory then we may check it against historical facts.

Here, I must say, I deviate from current philosophical practices in a manner that may indeed call for a remark, at least in order to warn my readers against a rather likely mix-up. For, even though there is practically no publication specifically dedicated to this topic, there is a kind of consensus from which I now deviate: the facts that are clear-cut enough, I think many philosophers will say, must be accepted on their own merit, but those lacking in credibility have to be rechecked. Alternatively, a philosopher might concede that a causal mechanism may help - but only help to test the factual claim; it has to be accepted, they will insist, only after it was attested. Only under duress will any philosopher admit that a factual observation report may gain credibility from an adequate explanation of the causal mechanism that brings it about, and then only if it cannot gain it from passing the test of repeated observation.

I reject all this. Admittedly many people think scientific caution is a merit and credibility a must; and of these most will admit a clear-cut repeatable fact as credible; likewise they will accept a mechanism, when plausible,

as lending support and credibility to the facts it helps explain. For my part, I think scientific caution is a mark of conservatism and so justified only where conservatism is justified - on which more later - and the demand for a mechanism is at best a conservative delay tactic. That is to say, at times the demand is simply not met, thus bringing about a stalemate; at times it is met and the result is a theory of a causal mechanism that may be questioned, thus bringing in schools of red herrings. Only at times, I should observe in fairness, the mechanism opens to a conservative a way to reconcile the innovation with his older prejudices; which pleases him no end and justifies his practice in his eyes and thus reinforces his tendency to demand an explanation of an alleged causal nexus by a theory about a causal mechanism every time the allegation in question arouses his suspicion.

For my part, I think we will do better if we try to know what a good theory of a causal mechanism does when it does some good. We will then, I think, be in a better position to judge when the demand for one is reasonable and when it is an unreasonable delay tactic and nothing more.

DO WE OBSERVE HOW THE STONE SHATTERS THE GLASS WINDOW?

I do not know why we all observe that the stone that shatters the glass window does that. It is a fact that we do. Ask any child, any psychologist, any police officer. But let us suppose we are in doubt; that is to say, let us leave it to the readers to consider a case which they consider doubtful and let them construct the analogy along the following lines. I shall offer two examples myself soon enough.

Suppose we do not know whether the stone shatters the glass window. We ask, then, from those who do, to furnish us with a causal explanation, What makes the stone shatter the glass window? There is a classic answer to the question: the stone transmits impetus to the glass window. If I am

right in my program, then I should be able to show how this causal mechanism is supposed to improve our observation. Does it? The philosopher who wants to improve credibility may here measure the amount of impetus required to shatter the glass window and the amount of impetus which a stone has. If it has then the required impetus, then it should shatter the window. If it has more, then, again, it should only shatter the window if it imparts to it more impetus than it needs in order to shatter, and this means that the impact should be fairly abrupt and rigid so as to impart the necessary impetus. Now, for my part I do not know about credibility, but I do agree that these experiments do make one observe the transmission of impetus as the cause of the shatter.

This is important. The statement reporting of a stone that it shatters a glass window may be true, yet it is so vague that reporting it in its generality raises a problem about its repeatability. Once we speak of impetus rather than about a stone, we exclude slow stones and rapid peas and include many other bodies other than stones, indeed even the accumulated absorption of impetus by the sympathetic vibrations of the glass window from the mere vibration of the medium!

We can go further. Why does impetus break a glass window? The answer will have to be couched in terms of molecular forces and the structure of glass. We will have to specify the level of pressure and the area of penetration. We will exclude very high-impetus small missiles (like bullets that may but need not shatter the window), and we will exclude enormous amounts of impetus spread over a large area, such as hitting the glass window with a flat large board. We will include, on the other hand, even smaller degrees of impetus well aimed to destroy the structure of glass, depending on whether the glass in question is crystalline or super-cooled fluid, on what is its exact degree of brittleness, etc.

Again, what I wish to stress is that the deeper causal explanation of a deeper mechanism may surface with instances peculiar to it, and the more familiar with these instances we are the more we tend to see facts in terms of the hidden mechanisms – even the plainest of facts. Expert students of the behavior of glass will hardly avoid seeing the deep mechanisms when seeing an ordinary event; paradoxically, like our reader of a novel or viewer of a movie, they may not see the ordinary event that they perceives, or they may see it and forget it, but they will see and remember the fact in the light of the deep causal mechanism if they have adopted it and experience as fairly usual the diverse instances of it that most of us consider rather unusual!

My answer, then, to the question, do we observe how the stone shatters the glass window is yes. Does this refute Hume? No. Because the way we see the cause is by seeing better, namely, by improving our way of observing. That is to say, our observations are forever conjectural, and our causal mechanisms are likewise conjectural, yet we expect the better causal mechanism to improve our way of observing, to explain the facts better than its predecessor though not as well as its successor in the sense that the facts it explains are more numerous, in the sense that facts which refute an older theory are explicable by it. This is, in essence, Popper's theory of *science as series of conjectures and their subsequent refutations*, where the role of a conjecture is to explain and to unify.

Let us turn once more to penicillin. What Fleming first observed was that mold destroyed a culture of bacteria. That was all. In a subsequent paper on bactericides he failed to mention that fact. And for a good reason: what kills bacteria inside the host and outside the host are two very different things, and evidence – quite clear-cut evidence – showed that in the host the effect is not as impressive as in vitro. Later, molds and

bacteria were better differentiated, antibiotic chemicals better isolated. So finally we learned that penicillin cures pneumonia by killing bugs. We then discovered viral pneumonia, and we discovered allergies to antibiotics and other complications; we also discovered bacterial immunization to antibiotics. Of course, later on a causal theory of antibiotic interferences with normal biochemical cell activities was discovered so that both the side effects and the immunizations related to antibiotics can now be better handled. There is no formal difference that I can see between this example and the one about shattered glass.

This raises again the question about the evidence correlating cancer of the lungs with smoking. Who is right? Those who endorse it, including the U. S. Surgeon General, or those who doubt the statistical correlation between smoking and lung cancer? And, we should remember, many of these people have scientific and/or medical training. I do not think I can judge this matter. Some of those who doubt the evidence and seek mechanisms may, indeed, have a similar though less definite idea in mind; the vagueness of the data and of their ideas leaves it open whether the requirement for a mechanism stems from curiosity or from dogmatism.

HOW DO WE TREAT AN EXCEPTION?

The picture I have drawn thus far seems simple enough and offers an intuitive grasp of the questions, when do we accept a causal nexus on the mere empirical ground that it has been reported and when do we want a mechanism to explain it before we admit it? For, in my opinion, no factual allegation is quite neat, and when it is too messy we may say so, or we may imply so by the request for an explanation by reference to a causal mechanism that may help clear the mess. For, often enough our manners urge us (erroneously, in my opinion, for what it is worth) that rather than tell people that they are in a mess it is nicer to express the

hope (or even the demand!) that they get out of the mess.

I suppose many philosophers will reject my picture because they do not like to admit that science is in a mess. I cannot help it. Rather than argue with them, I would demand a mechanism: I would ask that they tell me by what mechanism science manages so splendidly to stay out of the mess that everything else human is doomed to stay in.

In the meantime I should just briefly indicate a couple of problems that the mess raises, first concerning messy facts, and second concerning hopefully satisfactory theories of causal mechanisms. First then: since facts are messy we may always meet an exception. What should we do about it?

I hope the reader remembers that modern science and its philosophy began with censuring people who take too much liberty with facts. Tradition dies hard. It is easy to explain an exception away *ad hoc*, scientists and philosophy often ruefully admit; but we should not do so, they say; at least not too often, they often piously submit. This is just that: piety.

Suppose you are a medical researcher working on a fairly messy area – on the frontier; suppose it is the field of psychiatry. A patient is brought in, seemingly catatonic. You have to make a quick decision. A catatonic patient may die soon of dehydration unless you administer, say, an electric shock; but you are reluctant to administer an electric shock unless it is essential – either because you are in principle against it or because you know dangerously little about the patient and cannot find things out in a sufficiently short time. You quickly check for signs of catatonia. They do not all check. Will you condemn the list of signs as simply careless, as too stringent, as inaccurate, or as faulty in some other way? (Say, we do not quite know how spastic the catatonic's response should

be.) Will you submit to the list and say, perhaps, here is a case of hysterical imitation of catatonia? Or will you administer shock treatment anyway?

I say those who know the answer to this question are in an excellent position to conjure a dozen more for which they do not. I say medical practice meets problems of this kind fairly regularly and makes allowances for reasonable errors – at times these errors cost patients their lives, yet without thereby throwing adverse light on those who make them. When is an erring practitioner culpable? This question must be clearly decidable. In democracies the rule is simple: we may declare erring practitioners culpable only when there is no reasonable doubt.

I say the theoretical background to this fact is frightfully simple: when a generalization meets an exception we may have one or more ad hoc theories to explain it away, and the ad hoc theory may happen to be true. In my *Science in Flux* I have offered a long list of such cases from the history of the physical sciences, some of which are intriguing, such as the case of the neutrino that was introduced ad hoc but was discovered decades later and caused quite a stir.

It is all well and good when we deal with physical objects, as we do in the physical sciences; but what do medical practitioners do, what are they supposed to do, when they handle urgent matters of life and death? Let me change the wording of the question, since physical objects may also be matters of life and death. When Enrico Fermi built a uranium pile in the heart of Chicago, for example, he was taking a frightful risk – in order to avert the risk of letting the enemy develop a nuclear bomb first, of course; but he could not assess the odds in either case.

The truth of the matter seems to me to be this. There are standards of medical practice. These standards are far from perfect, but in a

democratic society you follow the standards even while you fight them. There are even standards about exceptions, such as, if time permits consult a colleague, preferably reputed experts (you may not think much of their reputation but, as long as it lasts you are still advised to consult them). I do not know all the standards concerning exceptions and would love to have practitioners state them, debate them, try to improve them.

How do we improve standards? We may improve our theory, test it, and show it to be more satisfactory, thus affording a better view of the facts. I have discussed this part of scientific and technological progress at a sufficient length in the volume I have mentioned above. But, as I say all along in this essay, at times we can observe a causal nexus without a theory explaining it by a mechanism. We then simply match facts against facts, factual allegation against factual allegation, and then do our best. At times we do a very poor job for want of anything better, for example, cut out a cancerous tissue as best we can and, after Roentgen and Curie, bombard a cancer as best as we can; and for want of anything better, of course, since from the time cancer was discovered this was considered an unsatisfactory mode of treatment.

But, of course, we also test any new cure that we introduce, particularly if we introduce it on purely empirical grounds, for as many side effects as we can. Still, the less we know of the mechanism, the less we know where to look for a side effect even if it hits us in the eye, as the bums caused by radiation in the early century, or as Thalidomide babies did until someone hit upon the idea that it was Thalidomide that de-formed them. And still, the absence of an explanatory causal model is a serious drawback as, for example, in matters of cancer: only recently we have learned to minimize the use of X-rays!

Even though tests for side effects are thus not very enlightened, there are

standards for performing them. The standards are not perfect either, but they are reformable too. They are the business of bureaus for food and drug control, for safety control, of better business bureaus and bureaus of standards and the like. I have discussed them in my *Science in Flux*.

In the same book, I also discussed the theory of standards for plausible theories of causal mechanism. The word mechanism is misleading here, since mechanism is the name for one – classical – type of preferred explanation, which is preferred no longer. There is little doubt that we all do have types of preferred explanation. A bad example is magical explanation-by contagion, or by sympathy – which is still popular on most of this earth but rejected by practically all people with western medical training, at least overtly. An example of a disputed area should be psychiatry, where the medical model competes with the psychological model. The word model only means here that we have a preferred type of explanation, be it this or that. Nor is disagreement about what is the preferred type confined to psychiatry alone, as many medical research workers know.

I cannot discuss all this here except to say that models may be useful in proposing kinds of explanation which may or may not work; and that they may be mere blinkers, dogmas that are used as excuses, as means to overlook anything new. In the field of thinking and observing, no conservatism seems to me justifiable. In the field of action, at times we need a conservative approach so as not to experiment with patients, at times we need a bold approach to bring hope where we think there is none. Which is which? We do not know. Any theory that would tell us will be a general theory of causality or of causal mechanism and thus one contender among many. But though we do not know, to repeat, we act in accord with standards which, themselves, are open to question and may

be questioned and modified.

In conclusion let me observe that the very question I have offered here, namely, how to treat an exception, indicates that no human affairs can be faultless and that the exception is the sign of a fault. Indeed: but for the faulty theory the exception would not even be an exception. Hence, better a faulty theory than no theory at all – but only on the condition that we do not forget that the theory is faulty, on the condition that we do not feel obliged to defend faulty theories with too many excuses! And, indeed, oft times a new theory comes about in the wake of the exception, in the recognition that the exception proves not the rule but the invalidity of the rule. So much so that at times the new theory simply turns the erstwhile exception into the new rule! In psychopathology it was Freud who made us all sick. But we have examples from biology, too, such as the whole field of immunology illustrates, or even the physical sciences, where the exception, quantized light, was declared by Einstein the rule in 1905, and he was viewed as extravagant by even the father of quantum theory, Max Planck, in 1908 and later; indeed, until Bohr showed him right after 1913. After a theory that served us, was limited by an exception, and rudely overruled when the exception became a part of a new rule or even the new rule itself, there are many who are willing to pooh-pooh it as infantile disease of science. But, to repeat the answer of the celebrated father of the electromagnetic field equations, James Clark Maxwell, science is always at its infancy.

CONCLUSION

In my Introduction I observed that people with scientific education in general, and of medical education in particular, exhibit diverse attitudes toward philosophy, from admiration to hostility. I have sought to explain this fact by a very simple mechanism. The business of science is to

observe and explain, to offer better observations and better explanations. Most scientists approach philosophy from the utilitarian aspect of this simple task. If they find philosophy helpful in doing their job, then they like it; if they fear it detracts them from the best path, then they hate it. But some of the admiration and some of the hostility, I think I have shown, are misdirected. The task of the scientist is the subject matter of concerned philosophers, and if they see it clearer they may perhaps help a bit the scientists who are overburdened already. What I personally hope philosophy may do and what I personally see as my task is to contribute to it, is to help science help us free ourselves a bit from our predicament by broadening our horizons somewhat. Once we realize that we are all groping in relative darkness, we may show more gratitude to our teachers and more toleration to our colleagues and students. In the present case I hope I have argued a bit against the tyranny of the alleged purely empirical facts as well as against the tyranny of the demand for causal mechanisms to explain facts. I think these two tyrants may be made to check each other to the benefit of us all. And, I think, better explanations enable us to see the facts clearer, just as much as better facts may better arouse our imagination in the search for more ingenious explanations.

What all this hinges on is very simple. Whenever discussing a general matter, such as causality, we may choose to illustrate it with unproblematic examples or with problematic ones. It sounds very reasonable to take examples that are as unproblematic as we can find. But when the matter is problematic, perhaps problematic examples are better. With unproblematic cases practitioners, be they physicians or engineers, are not concerned with causality, nor need they be. It is when dealing with problematic cases, particularly on the frontier of science, that the desire for a causal explanation is keenly felt. Later on, when the case has

become manageable, we may ignore the study of causation of that case: the frontier has simply moved on.

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