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2003

“Explanation in Social Sciences. Hempel, Dray, Salmon and van Fraassen revisited”

Un document produit en version numérique par Jean-Marie Tremblay, bénévole,
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Résumé

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Plusieurs explications proposées par les économistes reposent sur le principe de rationalité que les historiens invoquent implicitement, pour leur part, quand ils recourent aux explications que William Dray qualifie de « rationnelles ». Cette circonstance est l'occasion de repenser à nouveaux frais la question de l'application à ces sciences sociales des modèles d'explications mis au point par Carl Hempel. Il est montré que, interprétées à la lumière du principe de rationalité, les explications rationnelles peuvent difficilement être rejetées au nom des critères hempelliens de scientificité. Toutefois, tout ce débat doit être repensé dans le contexte de la remise en question de la théorie hempellienne, en particulier par Wesley Salmon qui y a opposé la primauté de la pertinence, le rôle de la causalité et le refus, discuté ici, d'associer anticipation et explication. En opposition aux thèses de Salmon, les perspectives ouvertes par la théorie pragmatique de Bas van Fraassen sont considérées, discutées et appliquées aux sciences sociales. Enfin, le débat autour du réalisme dans le cadre duquel s'inscrivent les thèses respectives de Salmon et de van Fraassen est reconsidéré de manière à ce que puisse être au moins entrevue la possibilité d'une approche de la question de l'explication scientifique qui tienne compte de ces diverses contributions.

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1) Explaining without the rationality principle

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If the rationality principle is so important for social sciences, and especially for economics, it is largely because it plays a crucial role in most explanations — and, to a lesser degree, in many predictions — provided by these sciences. If these latter were not oriented towards providing explanations, economists and other social scientists could almost dispense with the rationality principle. Let us try to figure out what economics, for example, would look like without the rationality principle. Economists would be able, with a reasonable degree of precision, to describe facts such as variations in prices, demand, interest rates or money exchange rates, etc. without involving themselves in questions concerning why prices, demand or rates increase or decrease in such or such a fashion. Moreover, economists would be able to make predictions, but only those based on observed regularities or, if one prefers, on laws that express observed regularities. These laws could even be used to provide a certain kind of explanation. For example, an economist could explain an increase in the price of good X by invoking the stability of X's supply in addition to an observed increase in demand, together with a law according to which if the demand for a good whose supply is fixed increases, then the price of this good will increase. This law might be based on statistical analysis of data concerning supply, demand and prices. Measuring the stability of supply and variations in prices would be relatively straightforward, whereas measuring demand would be based on more or less sophisticated inquiries or experimental devices. People could be asked how much they would buy of such or such a good were it sold at such or such a price, or alternatively, their reaction to various prices could be tested in various experimental situations. However, if one were to examine why people tend to buy more when prices are low or why sellers tend to raise their prices when demand increases, no possible answers could satisfy these legitimate questions. Buying more of a good when its price increases

and buying more of it when its price decreases would have to be considered as two strictly equivalent possibilities, and the fact that we tend to privilege the latter alternative would have to be imputed only to the unexplained fact that, according to observation, it is the one that prevails. An economist's a priori preference for this alternative would be a totally unjustified and antiscientific subjective preference similar to a Newtonian physicist's aesthetic preference for a law of universal repulsion over a law of universal attraction of bodies. The fact that, in both the economic and the physical examples, it is the second alternative which prevails would have to be registered as a fact of which it would be odd to require an explanation.

[6]

The comparison with Newtonian physics shows that this kind of situation is quite normal in natural sciences, where laws are exclusively based (directly or indirectly) on such regularities. However, this fact does not seem to reduce the scope of the explanations that natural scientists provide. Thanks especially to physics, we understand a number of phenomena, but the laws involved in those explanations are founded on regularities and consistencies and have evidently nothing to do with any kind of rationality attributed to natural entities. Phenomena must happen in some determinate way because they obey such or such laws in full consistency with other laws involved in similar phenomena. Why should we expect something else to happen in such situations ? What happens is precisely what is expected according to well established laws of nature. Thus, what actually happens is explained ; why look for more explanations ? Asking why bodies are attracted to rather than repulsed by other bodies is a silly question unless one envisages a more general system of laws according to which this fundamental fact becomes a particular case deducible in particular circumstances from those more general laws. For example, the question as to why light travel in a straight line was a silly question until Einstein's relativity theory made the character of light trajectory deducible ¹. However, at any moment in time, a few fundamental facts are considered as given and, in the absence of a scientific revolution, questions about them are

¹ For examples of this type, see van Fraassen (1980), p. 111-112.

perceived either as silly or as purely metaphysical, similar to the venerable question "Why is there something rather than nothing?"

One cannot apriori exclude the possibility for social scientists and for economists in particular to proceed as physicists do. Why do consumers tend to buy less of a good when its price increases? What kind of forces keep them from buying as much of this good as before in such circumstances? One would like to answer that consumers tend to keep their money in order to buy more cheaper goods, but this only pushes the problem a step further because the point is precisely to explain why people tend to obtain more of a good when it is cheaper rather than less of it when its price is higher. One cannot invoke, here, the fact that it is clearly in their interest to pay less for a given good and to get, in this way, both the good and part of the money which would otherwise be paid in order to obtain it, because such an answer would introduce the notion of self-interest, which is precisely the kind of notion that must be avoided if our explanation is to [7] exclude the rationality principle and rather be modelled on explanations provided in physics. Post-Aristotelian physicists never invoke anything akin to falling bodies' self-interest in order to explain their attraction to the Earth.

Economists may try instead to explain why consumers tend to buy less of a good when its price increases by invoking general neurological laws from which they would derive typical economic behaviour, but given the complexity of a behaviour which, in fact, does not strictly obey a law and given the difficulty raised by the relation to be made between neurological phenomena and human action, very few economists, if any, have been tempted to explore this avenue. Other kinds of naturalistic explanation inspired by natural selection and game theory are more attractive and draw more and more attention from economists. However, throughout the history of economic thought, attempts at developing an economic theory without drawing on the rationality principle have remained quite marginal, at least until recently. In fact, the question as to whether economics can seriously be developed without the help of this principle was debated as early as the nineteen-fifties when Israel Kirzner objected to Gary Becker's heroic attempt to derive standard results of microeconomics from postulates according to which agents act in a perfectly irrational

and even stupid way ². Becker relied on the limitations of an agent's consuming power (whether irrational or not) in order to derive a declining demand curve and, in a rather summary fashion, on natural selection in order to suggest that maximal results can be reached. Kirzner objected, however, that such fragmentary results could not explain what is essential for an explanatory economic analysis, namely the progressive adjustment to equilibrium, which, according to him, requires that agents be rational enough to experience disappointment when faced with unsatisfactory results and take action to modify the situation. It was certainly not clear that natural selection whose results are typically reached in the long term could really help in explaining those short term adjustments. In any case, it is not the goal of the present paper to evaluate the possibilities of evolutionary economics, which has made considerable progress since Becker's paper. It will be sufficient to observe that the bulk of the explanations provided by economics and other social sciences, including all other Gary Becker's contributions, was not based on such audacious attempts to build a science free of any trace of a principle referring to intentional behaviour like the rationality principle does. Indeed, the typical [8] explanations that economics provides invoke the rationality principle in a fashion which has no equivalent in any natural science. From Adam Smith who, in order to explain the stability of what he called "the natural price", claimed that landowners, workers or capitalists would be rational enough to draw their resources away from this market upon realising that their respective revenues have fallen below their natural rates, to rational choices theorists who claim that almost every human behaviour can be explained through some kind of computation by a rational agent, the fundamental role of the rationality principle has been pervasive in almost all economic explanations ³.

² See Kirzner (1962), Becker (1963) and, for a discussion of this debate, Lagueux (1993).

³ See Lagueux (2004).

II) Hempel's unified theory of explanation

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This situation raises a particularly troublesome epistemological problem. If social sciences can develop a different kind of "scientific explanation" based on principles totally different from those on which the scientific character of explanations provided by natural sciences is grounded, one may question either the scientific character of these alleged scientific explanations or question the very notion of a "scientific" explanation. If there are various ways to explain scientifically, how might one neutralise the pretensions of astrologers and other charlatans who claim that they provide "scientific" explanations as well ? In this context, it is hardly surprising that the question of the nature of a scientific explanation has occupied an important place in the philosophy of science, but since it is full of ambiguities, it has constantly divided philosophers of sciences for more than a century. The most sophisticated debate took place between philosophers of physics, but since a parallel debate was taking place regarding explanation in history, a discipline in which the role of the rationality principle is as important as it is in economics, I will start by considering this latter debate. After discussing the problem in this context and applying the results to economics (where no similar debate took place), I will try to clarify various problems by considering and discussing more recent developments in the debate among philosophers of physics. This transition from explanation in history (or in economics) to explanation in physics (and vice-versa) will be greatly facilitated by the fact that debates in both domains were provoked by papers published by Carl Hempel regarding scientific explanation.

[9]

Hempel's deductive-nomological (DN) model of explanation was even exposed for the first time in a paper addressing the problem of explanation in history (Hempel, 1942) though it was fully explicated only six years later in a paper written with Paul Oppenheim (Hempel and Oppenheim, 1948). Whereas, according to Hempel, a scientific

explanation must be derived from a set of empirical laws joined with other well established facts, a few philosophers under the influence of R.G. Collingwood's ideas regarding history (Collingwood, 1946) — William Dray (1957) being probably the most representative among them — contested Hempel's view and claimed that an explanation can be scientifically valid without the help of any law. According to Dray, historians in particular, invoke typically "rational explanations" based on agents' rationality rather than on any law. Before pushing further, it might be worthwhile to note that a significant part of the considerations raised in this debate were anticipated by German philosophers involved at the end of 19th century and at the beginning of the 20th in the debate concerning the alleged opposition between the natural sciences which allow us to explain (*Erklären*), and the human sciences which allow us to understand (*Verstehen*). In the 19th century, however, human sciences were mostly focussed on the interpretation of biblical and classical texts, whereas, in the mid-20th century, the context was quite different, due to the important development of social sciences, whose pretension was also to explain. Therefore, the debate between Hempel and Dray did not refer to understanding, but concerned only two different views of explanation, one being typical of natural sciences and the other of history. Before trying to see whether these two views can be made compatible or not, let us recall the essentials of each of them.

Since Hempel's models are very well known, I will be very brief in describing them, but since economists and historians tend to perceive them as purely formal schemes, I will nonetheless try to illustrate why it is after all quite a sensible theory. According to Hempel's DN model, two elements are required to have a scientific explanation : at least one law covering the phenomenon to be explained (designated as the explanandum), which justifies its being called nomological, and deducibility of this explanandum, which justifies its being called deductive. In such a view, a scientific explanation is nothing but an argument in virtue of which the explanandum can be logically deduced from a set of well-confirmed facts together with some empirically established laws. While apparently purely formal, this theory allows us to put the finger on a central element of a scientific explanation. This fact can be illustrated clearly with the [10] help of one of the famous Hempellian examples borrowed from automobile

mechanics. Suppose that you are totally ignorant of mechanics and that, while taking out your car on a chilly morning, you notice a peculiar liquid flowing underneath it. You conclude that this liquid is coming from your car and that this is bad news. Looking for an explanation, a set of alternatives comes to mind : your car is a poorly maintained "lemon" ; a gang of local vagrants have amused themselves by perforating some part of your car ; it was caused by malignant and powerful extraterrestrial creatures arriving in a UFO ; the perforation might have been the result of an evil curse cast on you by a sorcerer who was insulted by your sceptical attitude regarding voodoo practices during your last visit to Haiti. You consider all of these alternative explanations when a friend well versed in mechanics, passes you on the street and immediately looks below the hood. He observes that the liquid (pure water without antifreeze) is leaking from your antiquated radiator, which clearly has burst the previous night. He reminds you that the temperature dropped below zero last night and concludes that science provides an explanation for this unfortunate event. According to one law of physics, any quantity of water submitted to a temperature below zero Centigrade will change into a larger volume of ice, which will necessarily break any content too small for it. Since all the facts involved are effectively present and the law empirically established, the conclusion is irresistible : the breaking of the radiator should have been predictable to anybody with an awareness of the relevant facts on the previous night, since it is simply a matter of what happens regularly in such circumstances. The point here is that the explanation is clearly a scientific one, which means that you may put aside thoughts about lemons, vagrants, UFOs and sorcerers, since, now, you hold a scientific explanation ⁴.

It is difficult indeed to resist to such an explanation, but what makes it an explanation at all ? Why does this deductive argument satisfy our need for explanation ? It would be a mistake to attribute its virtue to the deductive power as such. As is well known, Hempel himself, realizing that most scientific explanations in modern physics are based on statistical laws, introduced his IS (Inductive-Statistical) model in order to cope with this situation. ⁵ The remarkable fact about

⁴ For a recent analysis and discussion of Hempel's D-N model with applications to economics, see Mongin (2002).

⁵ See in particular Hempel (1965), Part 3.

this move is that the deductive dimension of the DN model was sacrificed in order to preserve its nomological dimension. With the IS model, a strict deduction is no longer possible, though a [11] relatively safe inductive conclusion can be reached, given the high probability implied in the statistical law involved. This law is now a statistical one, but it remains a law that emphasize the regularity of the phenomenon to be explained. Hempel and his successors provide a great number of examples of IS explanations drawn from modern physics, but it is interesting to note that this kind of scientific explanation can also be met, though clearly less frequently, in social sciences, including economics. Suppose that you want to buy a picture by your favourite painter and that, influenced by the dramatic continuous drop in the price of computers, you expect and wait for a similar drop in the price of the painting you have in mind. And suppose that the painter dies and that his death generates more interest in his painting. You go to the gallery and discover with great disappointment that the price of the painting has gone up considerably. You look for an explanation and consider various alternatives. Perhaps the Gallery holder knew of your fondness this painter and increased the price in order either to make an excessive profit or to punish you for something you had done which profoundly hurt him in the past ; or perhaps a kind of fatal doom or genetic misfortune is there to block any favour that life offers you. An economist of your friends rejects all these hypothetical explanations and replaces them with a scientific one. He observes that the greater notoriety of this painter has increased the demand for paintings whose production was interrupted by death and he adds that there is a statistical law of demand according to which if the demand for a good whose supply is fixed increases, then the price of this good will increase. From this, it is easy to induce that the increase in price for these paintings should have been expected since it is simply a matter of what happens regularly in these circumstances. Why, in such a situation, look for far-fetched explanations ? Thus, we can say that explanations conforming to either one of Hempellian models are explanations because they convincingly let us see that the explanandum is nothing more than a case of a standard phenomenon which happens every time (or almost every time) a similar situation prevails. If this is the case, it would be silly to keep looking for an

explanation since what happened is precisely what was expected according to this law.

III) The role of "rational" explanations in social sciences

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It is interesting to consider now the views of William Dray, not so much in order to emphasise his objections to Hempel's thesis, but to illustrate that a quite different type of [12] explanation, exemplified by what Dray calls a "rational explanation", might be scientifically significant, even if it looks very similar to the common-sense kind of explanation associated with the notion of "good reason". Here again, the classical example used by the author will help in understanding in what sense such a claim can be sustained. Dray took as an example the explanation, proposed by the historian G.M. Trevelyan, of the fact that in 1688 Louis XIV, a particularly opportunist and ambitious French King, refrained from invading Holland, a country he coveted, in spite of the fact that King William of Holland was fully occupied by the conquest of England. Trevelyan, who estimated that the king's ambition extended to all of Europe and not only to tiny Holland, offered the following explanation : "(Louis XIV) calculated that, even if William landed in England, there would be civil war and long troubles, as always in that factious island. Meanwhile, he could conquer Europe at leisure." ⁶ For Dray, the important point is that, in such an example, no covering law was available to Trevelyan and none whatsoever was necessary, because what such an explanation "aims to show" is that the thing done by Louis XIV "made perfectly good sense from his own point of view", not that a similar thing would necessarily have been done by other conquering kings (or even by Louis XIV himself) in similar circumstances. Indeed, since it is not based on a law, such an explanation does not imply that it would have been possible to predict that Louis XIV would have done what he did.

⁶ Trevelyan (1938), pp. 105 ; quoted by Dray (1957) p. 122 ; see also Dray (1963), p. 109.

Before discussing the scientific character of such an explanation, let us consider a relatively similar one found in economics. The standard explanation of the phenomenon associated with the so-called Giffen goods illustrates this fairly well. For sure, the problem at hand is more theoretical than Trevelyan's, but this is normal when passing from history to a science based on generalised phenomena such as economics. Whereas the quantity demanded of any good normally decreases when the price of this good goes up, it was noted on rare occasions that the quantity of potatoes bought by poor people paradoxically increased when their price went up. The standard textbook explanation is as follows : potatoes are a widely consumed "inferior good" and the consumption of such "inferior goods" may increase in spite of rising prices if the income effect of such price increases outweighs its substitution effect. One might argue that in this case, in contrast with Louis XIV's case, we have a lawlike statement concerning the relative impact of income and substitution effect, but one must also admit that this statement is far from being an [13] empirical law. The explanation draws its force not from this "law" but from the fact that consumers are supposed to be rational and that if it is rational to consume less potatoes when their price increases, it is also rational, when discovering the negative effect on one's available income of this price increase, to abandon the consumption of more expensive superior goods and replace this consumption with that of still more potatoes, which, in spite of their increased price, remains cheaper than superior goods. This explanation parallels Trevelyan's because, in both cases, there is puzzling behaviour to explain (not invading Holland and buying more potatoes) since, in the described situation, the opposite behaviour (invading and buying less potatoes) seems more appropriate. In both cases, it is the rationality principle which plays the decisive role in the explanation (since the king is rational, he refrains to invade Holland ; since consumers are rational, they abandon superior goods and buy more potatoes) once a flawed assessment of the situation is corrected (the king is in a better position to invade all of Europe once William is out of the picture ; consumers take into account the effect of increased prices on their incomes and not only the increased attractiveness of substituting potatoes with other goods). Moreover, one must note that, in both cases, it is the same rationality principle that is the source of the puzzling situation requiring an explanation (given their alleged

rationality, the king should have invaded Holland and consumers should buy less potatoes, as their respective interests seems to suggest).

On what basis can one claim that the behaviours of Louis XIV and of our consumers have been explained ? Clearly it is because, in both cases, a puzzling situation, one caused by an apparent contradiction between what happened and what was logically expected, turned out to appear — due to the cognitive content and the logical considerations conveyed by the explanation — perfectly normal since what had happened corresponded exactly to what should have been expected in the given circumstances. Incidentally, in some sense, it is for the same reason that DN or IS explanations explain. The leaking car and the increased price of paintings were seen as puzzling events since they contradicted what was expected, but, due to the information and to the logical considerations provided by relevant explanations, these events turned out to be perfectly normal given their exact correspondence to what should have been expected under the given circumstances. In spite of their opposition to such "rational explanations", Hempellians would not have great difficulty agreeing on the functional similarity between the two alleged kinds of explanation since they believe that explanations based on the rationality principle and even on [14] common sense frequently qualify as explanation "sketches" providing what could be characterised as an explanation's substantial components (see Hempel, 1942, section 5.4). Thus, their disagreement does not concern the explanatory but the scientific character of such "rational explanations".

IV) Why rational explanations can qualify as scientific explanations

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What makes Hempellian explanations more scientific than those associated with common sense ? Essentially two things : in a scientific explanation (1) all the facts invoked are, in principle, empirically well-established, and (2) the explanandum is correctly derived with

the help of an empirical law. The first point can make a crucial difference between Hempellian and common sense explanations, but not between the former and the "rational" explanations used by historians and economists. At least in principle, historians establish their facts with the best critical techniques available, while economists, if they were explaining a phenomenon with the help of an argument of the type illustrated in the Giffen goods case, would normally establish their facts with the best econometrical techniques. Given the particular difficulties associated with the assessment of human behaviour, one may argue that, actually, natural scientists establish their facts in a way more compelling than historians and economists, but there is no reason to insist on a difference of principle on this ground. In any case, it is the second point which is most crucial. The function of empirical laws is to reduce explanandum to simple law-covered cases by assuring that at any time the situation is present, the explanandum is to be expected. Without these laws, the reason one expects that the explanandum will happen disappears. This is why Hempel claimed that, in order to qualify as scientific, Trevelyan's explanation should be supported by a general law according to which, in a situation similar to Louis XIV's, any rational agent would invariably (or with high probability) refrain from invading a country like Holland (Hempel, 1963, p. 155). Only the addition of such a premise would have permitted a prediction of Louis XIV's action. This law-based predictibility seems important indeed, because if it is admitted that Louis XIV could have, in the same circumstances, chose to invade Holland, how can we say that the fact that he did not is explained? Such a law could have one of the two following origins. It might be a statistical law based on the observation of leaders' decisions in a situation similar to Louis XIV's, but, if one follows Dray's argumentation, this possibility can be forgotten because it would be impossible to find a significant number of situations, if any, similar on all relevant grounds, [15] and because, even in the most similar cases, the possibility that the opposite decision had been made cannot be excluded. Alternatively, a law could be based on neurophysiological considerations which would allow us to conclude that, in that situation, Louis XIV necessarily chose not to invade, but even if some scientists may hope that, at some stage in the future, it will be possible to explain all human decisions this way, this possibility appears, at this moment in time,

like a Laplacean dream and, in the absence of more knowledge, it is simply a denegation of the (at least apparent) fact that, in many situations, human beings can decide either way. In the case of the Giffen good example, some economists would probably be prepared to contemplate a statistical law according to which consumers, after experiencing a decrease in available income caused by the rise in the price of a widely consumed inferior good, abandon the consumption of superior goods and increase their consumption of this inferior good, but can this lawlike proposition, which appears reasonable but hardly concerns observable situations, be seriously considered an empirical law ?

What happens if no empirical law of the proper type is available to historians or even to economists ? Should we conclude that they fail to provide the scientific explanations they pretend to offer ? Or does the Hempellian challenge force us to reassess the very meaning of an explanation ? Could Trevelyan's explanation be characterised as scientific without the help of any law ? What principle, if it is not a law, would allow us to infer that the decision not to invade must now be considered as the one expected of Louis XIV ? Clearly, the "principle of action" ⁷ invoked by Dray implicitly derives from the rationality principle, which implies that rational people makes rational decisions. Such a principle suggests that Louis XIV, after all, is rational and that he will make a rational decision. Hempel's strategy (1963, p. 155) was to tentatively consider that an appropriate application of the rationality principle might play the role of the covering law in Trevelyan's explanation, but is the rationality principle an empirical law ? It seems reasonable to claim that human beings are rational at least in the minimal sense of being not stupid and of being self-interested, as economists readily admit, but should we give scientific credibility to the idea that a particular individual such as Louis XIV is rational to the point of making the decision which seems the most appropriate in a given circumstance ? Surely not, but here we have to take [16] into consideration a remarkable fact, one that does not seem to have been underscored in the discussion about this type of explanation. It is the fact that the

⁷ This is the expression used by Dray (1957), p. 132, who refers to "rational explanations" but not, as far as I am aware, to the rationality principle as such.

rationality principle, on the basis of which the decision not to invade is seen as the decision to be expected, is precisely the same principle on the basis of which the decision to invade was previously seen as the decision to be expected. It is precisely because Louis XIV was considered a rational fellow that his decision not to invade conflicted with what appeared to be the logically expected decision, namely to invade, and it was this conflict that required an explanation. But, if, once our knowledge of the facts is rectified, it is exactly the same view about his rationality that allows us to conclude that what is expected actually corresponds to what he did, there is no point in inquiring about the empirical status of this view. Had we no reason to trust it, it would be less the explanation than the question itself which would loose its very meaning : how would it be possible that someone who is not at all convinced that rational people tend to avoid "silly" decisions be particularly puzzled by the alleged inappropriate decision of Louis XIV ? It is only if we accept, at least hypothetically, that Louis XIV was a rational fellow who tended to avoid silly decisions that a problem is raised by his decision not to invade. Thus, joined with the empirically documented facts revealed by Trevelyan's analysis, a principle, which is necessarily accepted if there is a problem to solve and anything to explain, allows us to conclude that we hold an explanation which removes the problem and the puzzlement. Since, in this explanation, only empirically established facts and a necessarily accepted principle are involved, there is no reason to deny its scientific status.

The situation is quite different regarding the radiator and the price of paintings since there the laws invoked to conclude that the observed event was to be expected were absolutely not involved in what raised the problem : on the contrary, in these cases, it is the ignorance of those laws (the law of dilatation and the statistical law of demand), and not their acceptance as was the acceptance of the rationality principle, which raises a problem requiring an explanation. Concerning the case of Giffen goods, it is similar to that of Louis XIV. There is no such thing as an empirical law according to which people experiencing a decrease in available income due to an increase in the price of a largely consumed good will increase their consumption of this good in order to compensate the forced diminution of their consumption of a superior good. It is probable that

too many people will continue consuming in accordance with their habitual consumption patterns to leave room for even a statistical law of this type. However, if an [17] explanation was required, it is because the observed phenomenon (increased potatoe consumption) was contrary to what is expected on the basis of the principle according to which people are rational enough to reduce their consumption of a good whose price increases significantly. However, once the economist has established that the income effect of this price reduction is important enough to induce a rational consumer to replace superior goods by still cheaper potatoes, it is precisely the same rationality principle that allows us to explain that the increase in potatoe consumption is exactly what should be expected. Were the principle according to which people are rational to be rejected, the increased consumption of more expensive potatoes would not correspond to a puzzling situation demanding explanation since it would be admitted from the start that variations in consumption are more or less randomly determined and have nothing to do with people's rational endeavour to satisfy their self-interest. However, since we have admitted that economist's explanation of a situation involving Giffen goods would be based on facts empirically established together with this necessarily accepted principle, there is no compelling reason to deny its scientific status either.

V) Hempel's theory challenged by Salmon : the role of relevance and of expectation

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Clearly, the attribution of a scientific character to explanations that are not based on any law is not compatible with the Hempellian ideal of a unique type of scientific explanation. However, the authority of Hempellian views have been slowly but considerably eroded by an increasing number of objections which have been addressed against most of its tenets. Thus, before returning to the discussion of the status of "rational explanations", it is appropriate to look at the important developments in the theory of explanation which have taken place in the philosophy of natural sciences, especially during the 1970ies and

1980ies. It would be beyond the scope of the present paper to seriously discuss the variety of arguments which have been proposed in order to point out insufficiencies in the Hempellian "received view" and which have forced philosophers to develop, during this period, new theories of scientific explanation. I will recall only three of the most famous counterexamples which have eloquently brought to light these insufficiencies. The Hempellian thesis according to which explanation and prediction are two faces of the same argument was probably the most frequently contested, using counterexamples such as the fact that, long before Newton, mariners could predict the tides with the help of a law derived from the observation of the moon's positions and phases, while being [18] totally unable to provide an explanation for such a phenomenon (Salmon, 1989, p. 47). Other examples illustrate still more clearly that a perfectly well formed DN argument cannot be an explanation, the most often cited probably being the flagpole, whose height can be correctly deduced, but hardly explained, from the length of its shadow together with elementary laws of geometrical optics. It is clear that many examples of this kind are met in economics as well. Whenever we observe a continuous increase in the price of a fix-supplied good, we can correctly infer from the (correctly inverted) law of demand that there is an increase in demand for it, but it would be absurd to conclude from this that the increase in price explains the increase in demand. Thus, correct inference from a law (together with relevant facts) is far from being a sufficient condition of an explanation. Thus, all correct inferences are not explanations, but should all explanations be inferences ? Clearly not, since in the discussion of IS explanations, it turned out that the inference (deductive or inductive) of the explanandum does not seem to be a necessary condition either. In a paper published in *Science*, Michael Scriven (1959, p. 480) observed that, paresis being a sickness developed by about 25% of those with untreated (with penicillin) syphilis, one can claim that the absence of penicillin treatment of one's syphilis explains one's later paresis, even if the alleged law, according to which untreated syphilitics will develop paresis, is false in 75% of cases. Incidentally, this example illustrates through a different way that explanation and prediction must be dissociated since, in spite of the fact that paresis is correctly explained by untreated syphilis, one would be safer in predicting that an untreated syphilitic will not develop paresis. More generally, one can say that rare events, which

happen only, but far from always, in rare circumstances, can frequently be explained by the presence of these circumstances, even if they cannot be predicted, or inferred from them.

Given these difficulties in Hempel's theory of explanation, the idea that an explanation is either a deductive or inductive argument from which the explanandum can be inferred has been seriously questioned. Wesley Salmon is among those who radically rejected this view (Salmon & alia, 1971, p. 9 ; 1989, pp. 101-107). According to him, the crucial element for an explanation is not the high probability which allows us to infer the explanandum, but the relevance of the factors involved with respect to the event to be explained. To illustrate his point, he used the following example (which was his favourite). It is perfectly true that any man who takes contraceptive pills will never become pregnant, a truth from which the observed fact that contraceptives-taking John [19] Jones never became pregnant can be safely deduced. Nonetheless, this correct application of Hempel's criteria cannot be accepted as an explanation of Jones' non pregnancy ! (Salmon, 1971, p. 34 ; 1989, p. 50). To take an economics related example, let us say that an astrologist claims that consumption will increase each time interest rates drop drastically during a particular conjunction of the stars. It is clear that a correct deduction from this probably unfalsified law cannot be invoked as an explanation of the eagerness to consume following a drastic interest rates reduction announced by the National Bank precisely when such a star conjunction prevails. In both cases, one must disentangle the factors relevant to the explanation (manhood, interest rate reduction) from those which are not explanatory (contraceptive, star conjunction). What seems very easy when these far-fetched examples are considered raises a serious problem when it comes to defining unambiguous criteria in a theory of explanation.

Be that as it may, Salmon was so firmly convinced that high probability has nothing to do with explanation that he repeatedly claimed that (1) a relevant explanatory factor is one which changes the prior probability of an event either by increasing it or even by reducing it and therefore that (2) being explained has nothing to do with being expected. Let us consider these two points in turn.

According to Salmon, if a spurious argument like the one concerning Jones' pregnancy can satisfy Hempel's requirements for a

scientific explanation, it is because Hempel's IS explanation theory puts the emphasis on high probability, whereas what really matters in explanation is not high probability but relevance. Therefore, he devoted a book including papers by Jeffrey and by Greeno (Salmon & alia, 1971) to develop a new theory of explanation, the statistical relevance (S-R) theory, from which irrelevant factors such as Jones' ingestion of contraceptives are eliminated. The key idea is based on a comparison between the prior probability of the event to be explained with its (posterior) probability obtained once relevant factors are taken into account. Even if the probability that John Jones did not become pregnant once he had ingested contraceptive pills is 100%, this undisputable fact does not explain his non-pregnancy, because it changes nothing of the prior probability (which was equally 100%) of a man avoiding pregnancy. However, the notion of relevance is not so easy to define. For example, one could claim that, according to this view, a sudden drop of a barometer reading is a relevant factor in the [20] explanation of a oncoming storm since this barometer drop changes considerably the probability of a storm occurring. To deal with such a case, in which both the storm and the barometer drop are actually explained by atmospheric conditions, Salmon introduced the notion of screening off (1971, p. 55 ; 1989, pp. 65-66). Roughly speaking, it can be said that the drop in the barometer reading is screened off by atmospheric conditions if the probability of the storm given both of these events is equal to the probability of the storm given the atmospheric conditions alone, an equality which is clearly obtained in this case (assuming that the barometer works properly). In relation to Salmon's attempt to solve the problem of irrelevancies in the theory of explanation, it would be interesting to consider the techniques of econometricians, which, by way of sophisticated mathematical tools, has developed means of discriminating between true and false correlations and of excluding irrelevant factors from economic models. However, I will leave this study to someone more familiar with contemporary econometrics, but not without recalling that developing efficient methods of excluding irrelevancies from scientific explanations is not the same as explicating the very structure of a scientific explanation apt to cope with such irrelevancies. That being said, if high probability no longer plays a decisive role in an explanation, the idea that an explanation is an argument concluding in the explanandum could hardly be maintained. What is determinant is

no longer what allows us to conclude that the explanandum will surely happen but rather what allows us to conclude that something is changed in the possibility of its occurrence. For Salmon, this change of perspective was so fundamental that he radically dissociated the fact of being explained and the fact of being expected by means of high probability. Influenced by Richard Jeffrey's thesis according to which when we toss dice, for example, the explanation of an improbable result is of exactly the same type as the explanation of a probable result since in both cases pure chance is invoked (Jeffrey, 1969), Salmon did not hesitate to claim that "showing that the outcome is highly probable, and that it was to be expected, has nothing to do with the explanation" (1989, p. 62). Therefore, Salmon characterised an explanation as an "assemblage of facts statistically relevant to the fact-to-be-explained regardless of the degree of probability that results" (cf. 1989, 67, Salmon's emphasis). However, as is well known, the probability of a single event has to be measured in relation to reference [21] classes resulting from more or less arbitrary partitions. Taking this into account, Salmon defines the notion of statistical relevance in the following way :

“such partitions can be affected by a property C which divides the class A into two subclasses, $A.C$ and $A.\overline{C}$. A property C is said to be statistically relevant to B within A if and only if $P(A.C.B) \neq P(A,B)$ ”. (Salmon, 1971, p. 42).

With the help of this definition, it is possible to illustrate more precisely what Salmon means when he says that an explanation has nothing to do with the degree of probability. Let us consider his example (Salmon & alia, 1971, p. 64) of an equal mixture of uranium 238, which has one chance out of ten to produce a click on a Geiger counter in a given period of time, and of Polonium 214, which has 9 chances out of 10 to produce the same result. This mixture is such that its chance of producing a click in a given period is somewhere between the two probabilities mentioned above. Let us say that it is .5. If a click is produced during this period, it might be interesting to know whether it was produced by the disintegration of polonium or uranium atom. The best way to understand what is involved here is to make an appropriate partition of the mixture. Whereas a partition

between the right side and the left side of the mixture would be irrelevant since it would not change the probability of a click, the situation would be quite different with a partition between polonium and uranium because the degree of radioactivity of either of these components is statistically relevant given that their respective probabilities of obtaining a click are considerably changed. Nonetheless, if this probability is increased from .5 to .9 in the case of polonium, it is reduced from .5 to .1 in the case of uranium. For sure, the click might be explained by the disintegration of a polonium atom, but it might also have been produced by the disintegration of an uranium atom. And even if the latter event is much less probable than the former, it must be explained in exactly the same fashion, namely through the probability, however small, that it can be produced by chance. This systematic indifference to the degree of probability was the source of many objections to Salmon's theory. For example, in a paper entitled "Causal Laws and Effective Strategies", Nancy Cartwright observes (1979, p. 425) that "what makes uranium count as a good explanation for the click in the geiger counter" is evidently not the low probability but rather the causal law according to which "uranium causes radioactivity". She illustrates her point by observing that the survival of plants sprayed with a defoliant effective at 90% can not be explained by the fact that they have been sprayed in this [22] way, even if, according to Salmon's rule, this spraying was statistically relevant since it reduces the probability of survival to .1 in the same way as the partition between polonium and uranium in regards to the probability of a click. In any case, Salmon's S-R model faced other difficulties, both in relation to the definition of a proper partitioning of the reference class and to the explanation of theoretical laws, in such a way that, even at the early stages of the development of this model, Salmon became progressively convinced of "the necessity of appealing to causal mechanisms" (1989, 106) in order to develop a new theory of explanation.

Before coming back to Salmon's later views on causal explanations, let us consider more specifically his rejection of the idea that expectation has something to do with explanation. For Hempellians as well as for those who invoke rational explanations, it seems natural to consider that being expected is a good criterion for being explained, respectively because an expected explanandum is

nothing but what, according to a law, happens in given circumstances and because it corresponds to the rational thing to do in those circumstances. However, if it is relevance rather than inference through deduction or induction which is determinant for an explanation, it seems reasonable to conclude that the fact that an event is expected (due to an inference which, for Hempellians, should make it predictable) has nothing to do with it being explained. I think, however, that this conclusion overshoots the mark, since it rests on an unwarranted assimilation of "being expected" into "being predicted". Were someone playing "Russian roulette" (only once), my best prediction would be that the person will not be killed since the chance of survival in this case is five over six. If the person were to survive, I would naturally say that this fact is not puzzling at all given its relatively high probability. However, if the person were actually killed, I would say that such an event could not be predicted, though it would be odd to be puzzled by it and to say that such an event was totally unexpected. Were I invited to engage in this kind of experience myself, I would surely decline the invitation, since I would expect a deadly result to a degree quite sufficient not to try, though I would nonetheless admit that my best prediction concerning the consequences of going through with it is that I would survive. I would say that, in this situation, death is a disjunctively expected result. Disjunctive expectation is not so strange a notion : when a coin is tossed, it seems odd to say that, since no result is highly probable, it is impossible to expect any result at all ; it seems more sensible to say that we expect either heads or tails. Salmon and those who reject any association [23] between expectation and explanation took the verbs to expect and to predict as strictly synonymous, there remains however a semantic difference between them, one illustrated by the fact that one event can be expected to the same degree as others, but cannot be predicted to the same degree as others : predicting either a head or a tail is not being able to predict anything.

One could object that if expectation is dissociated from high probability in such a fashion, I should, for similar reasons, refrain from air travel since I should expect a crash, being aware that such an event happens in, let us say, 1 case in 1,000,000. But this situation is totally different and this difference has nothing to do with the degree of probability. In the Russian roulette example, the possible results are

exactly at par. One is not more mysterious than the other. If death occurs, it would be silly to demand an explanation for the occurrence of such a result, a result that was sufficiently expected, at least in the sense of being absolutely not puzzling. Naturally, the fact that someone agrees to play "Russian roulette" needs to be explained, but this is a quite different matter, having nothing to do with the fact that death rather than survival be the actual result. Playing Russian roulette is statistically relevant in the explanation of death since it increases dramatically the probability of the involved person's death in the next hour (in contrast, for example, to playing chess, which does not significantly modify this probability), but if it is explanatory, it is because it transforms this dreadful potential event, which was, until then, unexpected, into an (already overly) expected (though not predictable) event. The case of a plane crash is totally different. If a crash occurs, it is not at all silly to demand an explanation even when we know that such an event occurs once in a 1,000,000. The event is considered possible, but not expected. It is therefore puzzling and the comity in charge of its explanation will not rest until new information on relevant factors transforms this unexpected event into an event which, given this information, should have been expected. The explanation provided will possibly show that, under the circumstances, the plane had a 9 out of 10 chance of crashing, which will be considered a fair explanation, but this does not mean that high probability as such is an essential element of the explanation ; what is explanatory and what transformed the event into an expectable event is rather the particular conditions under which the plane found itself, the knowledge of which being what allows experts to determine this high probability. If one invents a sophisticated version of Russian roulette (one which, for example, emits at random a lethal radioactive element) in which the probability of death is exactly 1 out of 1,000,000, the very [24] improbable event of death would count among the expected results and its eventual occurrence would be fully explained (it would be unjustified to demand more of an explanation). Similarly, one who buys a lottery ticket can safely enough predict not to win, but nonetheless expect to win, at least in such a way that if it turned out that the ticket were the winning one, no particular explanation would be required for the understanding of this improbable eventuality.

Incidentally, what I call disjunctive expectation to characterize situations in which one expects either something or something else is closely related to Jeffrey's and Salmon's somewhat unintuitive thesis according to which "we understand the low-probability outcomes of any given stochastic process just as well as we understand the high-probability outcomes". (Salmon, 1989, p. 67) Indeed, where it concerns stochastic processes, such as those associated with the tossing of dice or the disintegration of radioactive atoms, Salmon's claim is perfectly true since the occurrence of an improbable event is not, as such, more mysterious or more puzzling than the occurrence of a probable event. This is precisely the reason why I say that, when their occurrence is purely a stochastic matter, improbable events are expected (without supplementary explanation) just as probable events are. But when their occurrence is commanded by other factors (as is the case in the plane crash), improbable events remain puzzling and unexpected until an explanation transforms them into expectable ones. The difference in wording might be purely semantic, since in both formulations, the necessary connection between high probability and explanation is rejected and the idea that probable and improbable stochastic events are explained in the same way is warranted, but the wording I propose seems to me more intuitive and, together with the insistence on the difference to be made between stochastic (where all possible results are expected, once its stochastic character is understood) and non stochastic processes (where those results are not necessarily expected), it allows us to avoid objections such as the one illustrated by Cartwright's defoliant whose lethal effect is not a pure affair of chance. ⁸

⁸ Peter Railton dealt with a Cartwright-inspired example by claiming that the spraying "is part of the explanation of survival", while denying that it is "a probabilistic cause of survival", the latter expression being reserved "for factors that do raise the probability of an event in the circumstances" (Railton, 1981, p. 238). A probabilistic explanation, in contrast, "is a form of explanation in its own right, charged with the distinctive task of dealing with phenomena that came about by chance". (p. 237) No doubt that my example of playing Russian roulette counts among such phenomena, but not a plane crash, which requires a causal explanation. Incidentally, Railton's distinction between "phenomena that came about by chance" and "deterministic" phenomena corresponds exactly to distinction between stochastic and non-stochastic phenomena, which I refer to, in my discussion of expectation.

[25]

VI) The role of causality

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As we have seen, Salmon, as well as Cartwright, who criticized his views on statistical relevance, solved this problem by "appealing to causal mechanisms". There is no doubt that the view according to which explanation is the elicitation of causal mechanisms that bring about the phenomenon to be explained can neutralise most counterexamples plaguing either DN, IS or S-R theories. Since the defoliant clearly does not cause the plants' survival, there can be no question of invoking it in order to explain that survival. Similarly, since mariners did not know the cause of tides, they were unable to explain it. Since the length of a shadow does not cause the height of a flagpole, it cannot explain this height. Since the drop in a barometer reading does not cause a storm, nor does it explain it. Since Jones's contraceptives did not bring about his non-pregnancy, it does not explain it either. In contrast, since, in some sense, the absence of penicillin treatment for syphilis actually causes paresis, the former explains the latter, irrespective of the fact that, in most similar cases, this causal link does not hold since no paresis is observed. Thus, at least at first glance, it seems difficult to dissociate explanation from causality. One can even say, though not without raising some specific problems, that Louis XIV's decision not to invade Holland was explained by Trevelyan through his eliciting of events taking place in Europe that caused the king's rational decision. Similarly, various explanations provided by economists can be presented as an elicitation of features of economic situations causing economic agents' decisions, as was illustrated and discussed in a recent paper by Daniel Hausman (2001, pp. 314-315 and pp. 321- 323).

Salmon realised that the explicitly causalist character of its new theory of explanation places it in a quite different class from many of those developed since Hempel's theory, the latter included. He therefore proposed a threefold classification of epistemic, modal and ontic conceptions of scientific explanation (Salmon 1984, pp. 15-20

and ch. 4 ; 1989, pp. 86 and 118-121). The modal conception, according to which an explanation must establish a relation of "nomological necessity" between the event-to-be-explained and its antecedent conditions, is illustrated in D.H. Mellor's theory of explanation (Mellor, 1976), and will not be considered here. According to Salmon, "the epistemic conception is oriented toward the notion of scientific expectability" (1989, p. 120). In other words, it is a conception for which an "explanation could [26] be described as an argument to the effect that the event-to-be-explained was to be expected by virtue of the explanatory facts" (1984, p 16). Salmon admitted, however, that this conception cannot be described univocally. He therefore distinguished three versions of this conceptions : information-theoretic, inferential and erotetic. The information-theoretic version, for which it is the transmission of information involved in an explanation that is the key element, is illustrated in James G. Greeno's theses (Greeno, 1970). Here again, since the present paper is largely a discussion of the two other versions and of the third conception, this first version of the second conception will not be discussed either. Hempel himself, for whom it was so important to present explanations as arguments from which the explanandum can be derived, is the most illustrious representative of the inferential version. The third version, which Salmon calls "erotetic", and according to which an explanation is essentially an answer to a question, usually a "Why ?" question, is illustrated by R.B. Braithwaite, by Sylvain Bromberger and, more systematically, by Bas van Fraassen whose theses will be examined later in this paper. All those associated with the three versions of the epistemic conception are not so eager as Hempel to claim that an explanation is an argument, but they tend to consider that an explanation is primarily related to knowledge and is even, in some respects, knowledge-dependent. In contrast, the third conception, adopted by Salmon, is characterised as "ontic" because it claims that "explanations exist in the world" (Salmon, 1989, p. 86) or, put less radically, that it reports facts existing in the world. In such a conception, an explanation has nothing to do with an argument ; it simply, by any means, exhibits that events-to-be-explained "fit into the causal structure of the world" (Salmon, 1977, quoted in Salmon, 1984, pp. 19 ; see also p. 121) or, put more generally, it brings to light the mechanisms which bring about this event.

Salmon is perfectly aware that the idea of defining explanation by reference to causes is not original, recalling that it goes back at least to Aristotle (Salmon, 1984, p. 135). And he is equally aware that the notion of cause is not particularly easy to manipulate in a philosophical satisfying way. Everyone is familiar with the fact that David Hume found the notion of causality highly questionable. One might also mention Auguste Comte, one of the fathers of the philosophy of science, who considered one of his main contributions to be the freeing of philosophy from the notion of cause which, according to him, should definitively be replaced by the notion of law (Comte, 1934). However, this somewhat cumbersome metaphysical notion cannot so easily be [27] dispensed with. It reappeared with John Stuart (Mill, 1961) and made its way in modern philosophy of science in spite of objections raised by some analytical philosophers. In this context, it is clear that Salmon's contribution does not reside in the idea of introducing the notion of causal explanation as such, but in his sophisticated attempts to reinterpret the notion of cause in a more satisfying way. He proceeds by turning attention away from causal relations between events to causal processes and to causal conjunctions and interactions between phenomena. Concerning causal processes, the problem is to distinguish genuine processes from pseudoprocesses. An example of a pseudo process would be the continuous movement of the shadow of a moving car, whose movement is, in contrast, a genuine causal process (Salmon, 1984, p. 143). Drawing on Einstein's special theory of relativity, Salmon underscores the fact that only genuine processes can transmit signals, whereas pseudo-processes are unable to do so. Being unable to transmit messages, pseudo-processes may move at any velocity, even faster than light, since their movement does not correspond to the movement of some particle whose velocity would be strictly limited by Einstein's law (1984, pp140-143 ; 1989, pp. 107-108). In order to better characterise the notion of transmission of a causal process (Salmon, 1989, pp. 107-109), Salmon invokes what he calls his "at-at theory of mark transmission" (inspired by Bertrand Russell's solution of Zeno's paradoxes) according to which a mark "that is imposed at point A in a process is transmitted, to point B in that same process if, without additional interventions, the mark is present at each intervening stages in the process". (1989, p. 110, Salmon's emphasis). And in order to deal specifically with the problem of common cause,

the theory was completed by a probabilistic discussion (inspired by Reichenbach's work on the topic) concerning forks of conjunctions and interactions between phenomena. Thus, with the help of a conceptual analysis, only sketchily described here, Salmon proposes an "ontic" theory according to which scientific explanation is an elicitation of spatiotemporally continuous genuine causal processes which, through a correctly identified net of forks, produces the phenomenon to be explained.

This theory, however, like its predecessors, has been submitted to various criticisms. For example, Philip Kitcher (1985, p. 638) is bothered by the circular character of Salmon's discussion of marks, which, while being the key element in Salmon's criterion of genuine causal process, can hardly be characterised without invoking a causal interaction. This brings him to conclude that "although Salmon has made progress in meeting the [Humean] challenge, a familiar [28] type of difficulty seems to remain."⁹ More radically, Bas van Fraassen, equally unconvinced of Salmon's analysis of genuine processes and of marks propagation¹⁰, raises questions about the viability of the causal conception when it comes to applying this view to explanations provided by quantum mechanics (van Fraassen, 1985, p. 643-651) — in contrast to relativity theory which, as van Fraassen admits, reinforces Salmon's causal theory (p. 650). In his book, Salmon himself had acknowledged this frustrating difficulty (1984, p. 279 and 1985, p. 653) without considering it fatal, even if it appears to dramatically limit the scope of a causal theory of explanation. van Fraassen also observes that many scientific explanations, especially those based on 'laws of coexistence' such as Boyle's law of gases, do not seem to be causal explanations in Salmon's sense (van Fraassen, 1980, p. 122-123). Salmon readily admits this fact in his 1984 book in which he quotes Boyle's as well as Kepler's laws in this regard, but he consistently, though not very convincingly, claims that these laws "do not, by themselves, do much in the way of explaining other phenomena." (Salmon, 1984, p. 136) In any case, it is not clear that Salmon would reject the idea that some explanations disclose

⁹ In his rejoinder, Salmon acknowledges this hardly avoidable difficulty, and hopes that a satisfactory solution may be found (Salmon, 1985, p. 652).

¹⁰ van Fraassen (1980) pp. 120-121 criticising Salmon (1978) who anticipated on this point Salmon (1984).

underlying non-causal mechanisms, a possibility of which he seems to be aware, at least in his later writings. (see Salmon, 1989, p. 134) At a more general level, van Fraassen raises a problem, one that could be addressed to any ontic theory, concerning the determining of what information is to be included in the explanation. If explanation consists in bringing to light the causal process and its network of forks, how might one limit the amount of information considered relevant to this explanation? For van Fraassen, who defends a pragmatic (erotetic) conception according to which an explanation is nothing but an answer to a why-question, this amount of information depends on the background of the questioner. However, in regards to Salmon's ontic conception, van Fraassen observes that "what the relevant information is depends on the event to be explained, and not on facts about the questioner" (1985, p. 640). And it is difficult to see how the event to be explained can determine by itself which among the multitude of elements related to its occurrence might be included in the explanation. Salmon argues that this problem should be largely resolved with the help of some concepts introduced by Peter Railton (Salmon, 1985, p. 653), a point which will be considered later in this paper. Let us conclude, in any case, that a causal theory of explanation may solve [29] most traditional difficulties associated with the Hempellian theory of explanation, but will not do so without raising serious problems of its own, not to mention particular problems which might appear in attempts to apply it to "rational" explanations provided by social sciences, in which genuine processes propagating causality from reasons to actions are even more difficult to manipulate.

VII) van Fraassen's pragmatic theory of explanation

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Let us see now whether van Fraassen's theory may fare better in this regard. van Fraassen readily admits that his conception of explanation is "much more liberal" than Salmon's "when it comes to the form of the explanation in general" (1985, p. 650, emphasis

added) though, according to him, this fact does not diminish the severity of the standards applied when it concerns the evaluation of the explanation. According to van Fraassen, an explanation is nothing but an answer to a why-question ; such an answer may therefore take extremely various forms, depending on the nature of the question and of the interests and background of the questioner, both of which determine what is the relevant information to be included in the explanation. For example, the fact that the analysis of causal processes was perceived as the canonical form of explanation in classic physics does not preclude that quantum mechanics can provide perfectly satisfying answers to those who have the background required to understand this kind of explanation, in spite of the fact that causal processes cannot be analyzed in quantum mechanics in the same way as they are in classic physics. Similarly, the laws of coexistence, which are not causal, can nonetheless be used to answer certain types of why-questions ; I presume this might be the case in the question of someone puzzled by the increased pressure exerted by a particular gas whose temperature is rising. As for explanations in human and social sciences, they may take any form that the question raised by the questioner requires. van Fraassen is even ready to admit that an explanation may take the form of a story that tells "how things did happen and how the events hang together" (1980, p. 113). Incidentally, this was the thesis of some analytical philosophers of history, such as Arthur Danto (1968, ch. XI), and it is an idea on which the adepts of the rhetorical approach in economics have capitalized. More generally, van Fraassen does not hesitate in claiming that there is no difference between a scientific and a common sense explanation insofar as their form or even "the sort of information adduced" by them is concerned. However, one can identify as "scientific" a certain type of explanation by requiring that such an [30] explanation draw "on science to get this information (at least to some extent) and, more importantly, that the criteria of evaluation of how good an explanation it is, are being applied using a scientific theory" according to rules which van Fraassen carefully exposes (1980, pp. 155-156).

van Fraassen's conception is thus admittedly epistemic and context-dependant. For example, in the event of a plane crash, the explanation that should be offered to an uneducated member of a

victim's family cannot be the same as the explanation required by the engineers who designed the plane, even if in both cases genuinely satisfying explanations should be provided. This contextdependence might be seen as negative for a philosophical theory, but in a theory of explanation, there is a *prima facie* argument in favour of an epistemic approach by the very fact that explanation essentially concerns knowledge: causality is, by definition, an ontic reality, but explanation designates a process through which information is conveyed from one person to another. The question at stake here is whether the selection of appropriate information should be determined by facts of the external world or, as van Fraassen claims, by the interest and background of the questioner. For sure, the theory of explanation must not be a psychological one, which means that it should not consist in a description of what is going on in the mind of the questioner once the explanation is provided. However, one must not confuse the content of the theory and the characterisation of the problem that it solves. The problem raised by the very idea of an explanation can hardly be defined otherwise than by reference to the interests and background knowledge of the questioner, which does not mean that explicating what is an explanation must be a matter of describing a psychological process. Put in a very general way, I would say that this problem is to determine what type of informational and logical considerations are required to allow one who finds a question puzzling (given one's interests and background knowledge) to realize that such puzzlement no longer has any (logically admissible) reason to persist. However, once the problem is defined in this way or otherwise, a psychologist will analyze, after taking into account all of the emotions involved, what should be going on in the mind or in the brain of the questioner when a state of puzzlement is transformed into a state of intellectual satisfaction. In contrast, the philosopher of science will analyze, after taking into account the context of the question, what kind of considerations are logically required — either in the form of an argument, of an assembly of facts, of an elicitation of causes or in any other form [31] — to allow a hypothetically rational questioner to realize that the allegedly puzzling situation has no longer any acceptable foundation. Economists are familiar with distinctions of this type between psychological and non-psychological approaches: the theory of decision, for example, deals with situations which cannot be characterized otherwise than by reference to the

interests and background knowledge of deciders. But once the problem is posed in such a way, both, psychologists and economists find themselves in constant disagreement since psychologists are eager to describe with the greatest possible precision what takes place in the minds of moderately rational individuals, whereas economists are only interested in the logical implications of a rational decision ¹¹. Thus van Fraassen's theory of explanation is admittedly epistemic and pragmatic, but it cannot be charged of psychologism.

Let us look more closely at van Fraassen's theory of explanation consisting in the logical analysis of what constitutes an appropriate answer to a why-question, an analysis which necessarily implies a discussion of the characteristics of the why-question itself. In order to illustrate this, let us consider the following why-question which might be addressed to an economist : "Why did the price of fuel increase in Canada last June ?" Since, as van Fraassen observes, not all whyquestions can be answered, it is important to examine the presuppositions of the question. A presupposition of a question is defined by van Fraassen as "any proposition which is implied by all direct answers" to that question, (1980, p. 140) a direct answer being an answer which "gives enough information to answer the question completely, but no more". (p. 138) A first presupposition of a why-question capable of being answered is simply the truth of what van Fraassen calls the topic of the question, which is, in our example, the proposition saying that "the price of fuel increased in Canada last June". A second presupposition concerns what van Fraassen calls a contrast class. For example, the propositions "the price of fuel decreased in Canada last June" and "the price of fuel remained stable in Canada last June" along with the topic ("the price of fuel increased in Canada last June") are members of a contrast class pertaining to that question. The second presupposition of an acceptable why-question is that, with the exception of the topic, all other members of the contrast class are false. However, an interesting feature of a why-question, which, according to van Fraassen, was first underscored by Bengt Hanson (van [32] Fraassen, 1980, p. 127), is that such questions may have more than one contrast class, each of which requires quite a

¹¹ The debate between the psychologist Daniel Kahneman and the economist Charles Plott (Kahneman 1996, Plott, 1996) provides a remarkable illustration of such an opposition.

different explanation. For example, the propositions "the price of milk increased in Canada last June", "the price of fuel increased in Canada last May", "the price of fuel increased in France last June" are members of three different sets of contrast classes, each of which includes the topic proposition itself and, eventually, some other propositions contrasted to other members of its class through the same part of the proposition. I think that a supplementary specification is required if we want to claim that all of the members of a contrast class except the topic are false. Naturally, "the price of fuel increased in Canada last June" and "the price of fuel increased in Canada last May" could (unfortunately) both be true, but were this the case, one should conclude that both could not be members of the same contrast class. This restriction makes sense since the puzzlement associated with the fact that the price of fuel increased last June in Canada cannot have been generated by the fact that the price of fuel increased last May as well. The contrast class is the class of those propositions which, through the kind of contrast they entertain with the topic, determine the very meaning of the question. The question and the explanation clearly differ depending on whether the questioner is puzzled since conditions allowed one to expect a decrease in the price of fuel, which nonetheless increases, or because it is precisely last June that an expected increase occurs whereas it did not occur during the previous months, or because the increase occurs in Canada whereas other countries were not affected. It seems to me that this notion of contrast class is particularly clarifying when it comes to analyzing the various explanations typically provided by social sciences. Suppose that the topic of a question is "there was famine in Chad in 1986" (date and country chosen arbitrarily for the sake of the example). It is easy to identify three different contrast classes concerning respectively the famine, Chad and 1986. Let us consider the last two of them. The first includes "there was famine in Mali in 1986", "there was famine in Burkina Faso in 1986", etc. ; the second includes "there was famine in Chad in 1985", "there was famine in Chad in 1987", etc. All of these four last sentences are supposed to be false. If the question emphasizes the contrast class including the propositions concerning Mali and Burkina Faso, the explanation might refer to something like the carelessness of the Chad Government, but if it is the contrast class including propositions concerning 1985 and 1987 that is emphasized,

it is rather explanations such as exceptional drought in the Sahara that should be considered ¹².

[33]

According to van Fraassen, the last fundamental presupposition of an acceptable why-question concerns explanatory relevance. A why-question presupposes that, in a given context, at least one proposition (the core of an eventual answer) is relevant to the topic with respect to the contrast class involved. Even when a single contrast class is considered and when, consequently, the question bears on the same element of the topic, quite different considerations, depending on the interests and the background knowledge of the questioner, are likely to make an answer relevant, and this can totally change the meaning of the question and the content of the requested explanation. This fact is clearly illustrated by the example of the engineer and the members of victims' family who required different explanations of a plane crash, the former in order to improve the design of planes, the latter in order to find peace of mind and to identify who was responsible for the accident. For his part, van Fraassen refers to the case of a mechanical versus a functional explanation given in answer to the question "Why does the blood circulate through the body?" (1980, p. 142) He also illustrates his point with the example of the various kind of factors which, depending on the interest of the questioner, can explain the warping of a conductor in a power station : human error, closure of a switch, moisture condensing on the switches, or standing conditions such as the presence of a sufficiently strong magnetic field which might have made the event possible, or even the function fulfilled by this conductor in the station. As clearly seen through this last example, the determination of what is relevant in this sense implies a choice of the relevant factors among those which play a role in a usually very complex causal network. The magnetic field, the closure of the switch and human error are some factors which might constitute the necessary conditions of the event, but an explanation must invoke only those among which, in van Fraassen's words, make the answer "relevant (in this context) to the topic with respect to [the involved] contrast class" (1980, p. 142).

¹² For a brief discussion of application of van Fraassen's thesis to economics, see Hausman, 2001, p. 320-321.

The idea of invoking the particular interests of the person requesting the explanation as the key element in the solution to the problem of choosing relevant (or salient) factors in an explanation is not new. van Fraassen mentions N.R. Hansen as invoking such interests in a similar context in a book published in 1958 in which he takes the example of an automobile accident in order to distinguish the causes set out respectively by a physician (multiple haemorrhage), by a barrister [34] (driver negligence), by a carriage-builder (brake system defect) or by a civic planner (presence of tall shrubbery at the turning of the road). It is interesting to note that, at the same period, philosophers of history, in their discussion of causality, treated this problem in roughly the same fashion. Edward Hallett Carr, for example, considers the same kind of automobile accident and discusses it in very similar terms. (Carr, 1961, pp. 137-140). In a more systematic study devoted to causation in the Law and in history, H.L.A. Hart and Tony Honoré propose, in order to illustrate the same point, the frequently evoked example of the man who, after eating parsnips, suffered indigestion explained by his wife as the result of eating a vegetable she described as indigestible and by his physician as the result of the ulcerated condition of his stomach.¹³ In a book of methodology written before he died during the second World War, the historian Marc Bloch, one of the founders of the *Annales* school of history, considers the case of a man who falls into a precipice when walking on an Alpine path and discusses the various elements necessary to explain this fall. Though he did not explicitly associate any one of the excluded elements with specific interests, he observes that, for the historian of the event, the unfortunate step was the appropriate cause to be invoked, even though it is not more necessary than other antecedent conditions such as the existence of gravity and the seasonal migration of sheep flocks, which was responsible for the laying out of the path. (Bloch, 1954, p. 190-191). For all these authors, the most important aspect of the question is to determine the criterion that allows one to select the factor that qualifies as 'the cause'

¹³ Hart & Honoré (1985, 1959), pp. 35-37 ; in the same discussion, they present (p. 35) a case quite similar to the example of famine in Chad that I have used to illustrate the question of contrast classes, but since they did not distinguish the question concerning contrast classes from that concerning relevance, their examples are lumped together as cases of context-dependency.

of an event, and noteworthy is the variety of criteria invoked by them. van Fraassen, for his part, illustrates this variety of criteria by citing those invoked in writings published between 1953 and 1957 by L.W. Beck, Ernest Nagel, R.B. Braithwaite and David Bohm.¹⁴ Though van Fraassen's analysis of explanatory relevance — whose only main lines are exposed here — does not pretend to determine what is the most appropriate criterion for choosing such "a cause", this analysis integrates into a sophisticated pragmatic theory of explanation the kind of context-dependency that is responsible for this variety of criteria.

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VIII) Solutions and objections

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van Fraassen is particularly confident that his theory of explanation has solved two persistent problems, namely the problem of asymmetry and the problem of rejection of demands for explanation, the former being solved by him in a quite original relativistic fashion and the latter being rarely considered by those who adopt less pragmatic approaches¹⁵. Let us first consider this last problem. A demand for explanation can be rightfully rejected, but this same demand can become legitimate at some other period. As an example of a question whose rejection was well-grounded, van Fraassen gives that of the question posed by the Aristotelians to the Galileans regarding the reason "why does a body free of impressed forces retain its velocity". (van Fraassen, 1980, p. 111) He also mentions the question as to why there is such a thing as a gravitational force, a question which was considered legitimate even in Newton's time but which, at a later stage, was classed as "intrinsically illegitimate", just like the question about the retention of velocity. (p. 112) It is difficult for DN or IS Hempel's theories or for Salmon's causalist theories to take into account such a phenomenon since these theories, be they epistemic or

¹⁴ Quoted, with appropriate references, by van Fraassen (1980), p 125

¹⁵ Peter Railton is among the few theoreticians of explanation who have explicitly addressed this problem (Railton, 1981), p. 248.

ontic, tend to be context-independent. But for van Fraassen's theory, which is overtly context-dependent, the rejection can be explained by the absence of at least one of the required presuppositions of the why-question. Let us recall that, according to these presuppositions, the background knowledge accepted in a given context must imply both the truth of the topic and the falsity of the other members of the contrast class, and must not imply the denial of the relevance to the topic (with respect to the contrast class involved) of any proposition (likely to become the core of an eventual answer). (cf. 1980, pp. 145-146) It is clearly because the denial of the relevance of any proposition concerning the appropriateness of a gravitational force was not yet implied by the background knowledge accepted in Newton's time, but was implied by the background knowledge accepted at a later stage that the demand for an explanation regarding this question was accepted in Newton's time and rejected later. This way of speaking may sound a bit tautological, but it allows us to integrate rejections of demands for explanation into a theory of explanation and to formulate in the very language of this theory the criteria justifying such rejections.

[36]

It is a quite different type of solution that van Fraassen proposes for the problem of asymmetry. This problem, illustrated by the flagpole example evoked above, is satisfactorily solved by a causalist theory of explanation such as Salmon's theory, insofar as we forget problems associated with the notion of causality as such. Given his warnings against an ontic theory, one presenting explanation as describing causal processes going on in the real world, van Fraassen could not invoke the direction of such causal processes to solve the problem the way Salmon did. Consistent with his context-dependence pragmatist approach, he instead refuses to flatly reject the idea that the length of the shadow could explain the height of the flagpole. To illustrate this bold position, he tells a charming story involving a tower designed by a gentleman in such a way that its height projected a shadow covering the spot where he had ignominiously killed one of his servants a long time ago (1980, pp. 132-134). Though this story is not particularly typical of stories concerning the relation between a tower and its shadow, it illustrates fairly well the fact that, even when the direction of the causal link seems to be self-evident, an

explanation has not necessarily to follow the same direction to be an appropriate answer to a why-question. Naturally, that does not mean that the height of the tower was caused by a posterior event, namely the projection of a shadow, whose length involved in the argument was necessarily obtained only once the tower had attained its full height. If something 'caused' its height, it is clearly the designer's previous desire to bring about his secrete goal by means of a shadow' length. However, according to van Fraassen's theory, what is at stake does not concern the direction of the causal process, which, whatever its exact nature, goes necessarily from the tower to the shadow rather than in the reverse direction. It concerns the explanation, which is not to be inseparably linked to causality, as is the case in a causalist (and ontic) theory. Thus, the height of the tower causes the length of the shadow, but, in this fictional example, the length of the shadow nonetheless explains the height of the tower, since it is the key element of a correct answer to a legitimate why-question about the height of the tower. Less far-fetched illustrations of such a situation are frequently found in economics. For example, when it comes to analyzing a Government's tax cut, it would seem normal to claim that such a tax cut caused the increased economic activity which was observed during the following months, but it would be odd to say that this stimulated economic activity is the cause of the tax cut, which preceded it. Nonetheless, if the Minister of [37] Finance is asked by a Member of Parliament why he decided to incautiously reduce the State's resources, the minister might answer that it is the desired increase in the economic activity (presently observed) which explains the implementation of the tax cut. Incidentally, such an answer will look particularly appropriate to anyone convinced that revitalized economic activity generates still more taxes for the State and that this fact has guided the Minister of Finance. In any case, the point is that if the tax cut causes the bullish mood of the economy, it is the latter which is nonetheless cited in order to explain the tax cut. Finally, if Salmon's theory can make room for Dray's rational kind of explanation, insofar as one is prepared to include in a genuine causal process the impact of Louis XIV's state of mind on his decision, with van Fraassen's pragmatic approach, this kind of explanation is unconditionally legitimized. Whether it is appropriate or not to say that Louis XIV's decision caused (through its impact on William' activities) a state of things favourable to the conquest of Europe, it

would be odd to say that this posterior state of things caused his decision. However, whether subsequently followed by an actual conquest or not, this posterior state of things was nonetheless the key element in Trevelyan's explanation of Louis XIV's decision.

In fact, in each of these three examples, a state of things or an event (the tower height, the tax cut and the king's decision) is explained by the posterior realization of another state of things (the spot-hiding length of the shadow, the increased economic activity and the favourable military situation). It seems clear that, in each case, the cause of what is explained has to be looked for in the state of mind of the main actor (desire to hide the site of a crime, desire to revitalize the economy and desire to exclude a dangerous enemy from countries to be subdued). This view corresponds exactly to what Salmon — following Larry Wright on this point — says about teleological and functional explanations (Salmon, 1989, pp 111-114) in order to show that causes are never posterior to their effects and that an explanation can be described as an elicitation of a cause even when explanations of this kind are involved. van Fraassen implicitly acknowledges the same thing in a statement — to be interpreted in an empirical rather than ontological way — saying that what his curious tower and shadow story illustrates is that "the relevance changes from one sort of efficient cause to another, the second being a person's desires". (1980, p. 132) van Fraassen, however, would add that, being answers to why-questions, explanations may refer to any "sort of efficient cause", regardless of the kind of mechanism involved, insofar as a cause [38] of this sort is relevant with respect to the correctly interpreted question to be answered. More precisely, van Fraassen, for whom "Aristotle's fourfold typology of causes" ¹⁶ is a source of inspiration, would say that, in the above examples, it is the "functional role" (the

¹⁶ van Fraassen (1980), p. 131, see also Kitcher and Salmon (1987), p. 326, where allusion is made to a personal communication of van Fraassen on this point, and van Fraassen (1989), p. 186, for an implicit reference to Aristotle's four causes theory ("preceding events, standing conditions, material composition, or functional role"). Van Fraassen's reference to Aristotle's four types of causes might introduce some confusion since what modern philosophers usually call a "cause" corresponds uniquely to the Aristotle's "efficient cause". It seems clear, however, that van Fraassen has found in this theory a tentative typology of relevance relations rather than the basis of a redefinition of the notion of cause.

Aristotelian final cause) which corresponds to the relation of relevance implied by such why-questions. Relevant answers to such questions do indeed bear on the goal, or the reasons, that motivated someone to undertake a given action (the construction of a tower at an excessive height, the implementation of a tax cut, the unexpected refusal to invade a country). If such a goal is usually revealed or induced through various clues, it is immediately characterized and illustrated by the state of things that normally results from the action. Therefore, this state of things, presented as the content of an intention, can be retained as the core of an appropriate answer to the question.

van Fraassen's radically epistemic theory was subjected to various objections. I will consider only those two on which Salmon places particular emphasis. They concern respectively the role of why-question and the criteria for scientific explanation. Various authors, long before the publication of van Fraassen's thesis, had suggested that explanations might be answers not strictly to why-questions but to some other types of questions as well. William Dray devoted the last chapter of his 1957 book to showing that, though he assumed throughout most of this work that an explanation is an answer to a why-question, other kinds of questions, among which how-possibly questions, are also requests for explanation. (Dray, 1957, ch. VI) In this connection, Salmon mentions other authors and other types of questions. (Salmon, 1989, pp. 137-138) One could attempt to neutralize this objection by reformulating questions of other types in such a way that they are transformed into why-questions¹⁷, but this might be difficult especially when one considers that, as illustrated by Salmon (1989, p. 137), a how-possibly question may be quite [39] satisfactorily answered by alternative potential explanations. Such potential explanations might show how an event that is puzzling because it was reputed impossible can look perfectly possible once the questioner is brought to envisage a sequence of possible events or alternatively other sequences of equally possible events. With such questions, the event to be explained can even be purely hypothetical :

¹⁷ Even if Hempel showed that how-possibly questions can be reinterpreted in order to be dealt with by covering laws explanations (Hempel, 1965), pp. 428-430, it might be more difficult to transform them into why-questions in such a way that they could, in the spirit of van Fraassen's thesis, require an answer which would straightforwardly illustrate what by definition is an explanation.

one may explain to me how an event which never happened and that I consider impossible is perfectly possible in principle. This kind of explanation is frequent in a theoretically oriented science like economics. The upward sloping demand curve associated with Giffen goods is such an example, but one can also mention the debate about reswitching, which concerns the sheer possibility of this hypothetical phenomenon. It would be difficult to transform into why-questions the questions raised in this debate without forcing the meaning of the word as would be the case if one were to ask "why is reswitching possible?" instead of "how is reswitching possible?" (or how could reswitching be possible?) when there is no observed case of reswitching to be explained. I think a better way of defending van Fraassen's theory of explanation would be to generalize it by claiming that an explanation is an answer to a puzzlement-induced question. How-possibly questions as well as why-questions, and as well as a few other types of questions, are puzzlement-induced, as suggested by Dray who, on occasions, uses the term "puzzlement" in connection with such questions.¹⁸ This is evidently not true of all questions. For example, the question "What is your name?" is not puzzlement-induced and consequently it is not a request for an explanation. It is true that such generalizing van Fraassen's theory would require adapting notions of contrast class and of relevance, but this adaptation, though out of the scope of the present paper, would not be so difficult to bring about.

The second objection to van Fraassen's theory concerns the criteria which allows one to characterize an explanation as a scientific one. This problem is particularly acute since this theory is applicable to common sense as well as to scientific explanation. The fact that an answer to a why-question can take so many forms renders the need for a characterization of a scientific explanation particularly pressing. As we have seen, for van Fraassen, an explanation can be said [40] "scientific" if it "draws on science" to get the information on which it is based, and if it is evaluated as an explanation with criteria "applied

¹⁸ Dray (1957), p. 165 ; see also (1963), p. 108. It would be worthwhile to relate the possible generalization of van Fraassen's theory proposed here to Dray's most general views on explanation (in contrast with his views on "rational" explanation) and also to Bromberger's analysis of answers to various kinds of questions (Bromberger, 1966).

using a scientific theory" (1980, pp. 155-156). van Fraassen therefore completes his theory with a discussion of the criteria to be applied in order to evaluate answers by proposing three ways of evaluating an answer, ways which concerns, respectively, its truth as such, the degree to which it favours the topic of the question by contrast with other members of the contrast class and the degree to which it should be preferred over other possible answers in regards to its probability, its relation to the topic and its relative relevance (1980, 146-151). I will not discuss as such these criteria, criteria with which van Fraassen declared himself not fully happy (1980, 146 ; 1989, p. 186). Kitcher and Salmon have challenged their trustworthiness by objecting that a carefully devised astrological explanation of the fact that John F. Kennedy died exactly on November 22, 1963 could fulfil all of van Fraassen's criteria for a "scientific" explanation (Kitcher & Salmon, 1987, pp. 322 ff and Salmon 1989, pp. 141 ff). Since any answer to this objection will bear on the question as to whether the astrological "explanation" really draws on science, there is little doubt that the difficulty of characterizing a scientific explanation is closely related to the well-known difficulties of solving the longstanding problem of demarcation.¹⁹ Whether one aims at characterizing a scientific explanation or science in general, it seems that one must renounce perfectly defined criteria and be contented with the fact that at least a pragmatic demarcation between science and pseudo science — and between scientific and pseudo-scientific explanation — functions well enough to allow challengers of such demarcations to be, paradoxically, so firmly convinced that astrological explanations are indisputably pseudo-scientific (and easily demarcated from scientific explanations) that the fact that it apparently fulfils van Fraassen's criteria constitutes in itself a proof that the criteria do not work.

Salmon, for his part, avoids the problem of demarcation by defining explanation by an elicitation of true or genuine causal processes. An explanation meeting this requirement is necessarily scientific since, in the realist approach implicitly adopted in this context, science is associated with the elicitation of true causes. For Salmon, who defines himself as a realist, causal processes exist in the world in which they can be discovered, at least in principle, and

¹⁹ On the ambiguities of the demarcation problem, see Lagueux, 1998b.

identified in order to provide, by the very fact of their elicitation, objective and scientific explanations of phenomena. [41] As for van Fraassen, he has no choice but to reject such a realist theory of explanation since he is both unconvinced by the way in which Salmon distinguishes genuine causal processes from pseudo-processes and agnostic about the ontological status of unobservable entities, whose real existence is implied by the elicitation of true causal processes (or of most of them). Naturally, this does not mean that, according to van Fraassen, causal processes can be freely invented by the explainer, nor that physicists cannot explain by invoking unobservable entities. It simply means that one must refrain from taking a stand on the real existence of causal processes and of unobservable entities and that one must be satisfied with invoking such processes or entities insofar as they can be empirically inferred in a way that simply "saves the phenomena". The discussion regarding the ontological dimensions of an anti-realist thesis about the existence of unobservable entities is beyond the scope of this paper, but I will claim in conclusion that considerations inspired by a more moderate "anti-realism" speak in favour of van Fraassen's theory of explanation without diminishing the importance of Salmon's emphasis on the fundamental role of causal processes. After all, as we have seen above, for van Fraassen — who incidentally opens his review of Salmon's book on causal explanation by claiming that his "agreement with Salmon is much more extensive than [their] disagreement" (1985, p. 639) —, the determination of the "efficient cause" of the phenomenon to be explained cannot be dispensed with, no more in his theory than in Salmon's.

IX) The prospect of a partial conciliation

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If the determination of an efficient cause plays such a role even in his context-dependent theory of explanation, it is, according to van Fraassen, because "the correct answer [to a why-question] consists in the exhibition of a single factor in the causal net, which is made salient in that context [...]" (1980, p. 132). In some sense, this is what explanatory relevance is all about : the causal net which brings about

any event is so complex that one has to pick out from it the relevant factor in accordance with what is required by a why-question understood with the help of the appropriate contrast class and with due consideration of the interests and the background knowledge of the questioner. I am not absolutely sure that this is perfectly in accordance either with van Fraassen's or with Salmon's view, but it seems to me that what should be called the relevant causal process bringing out the event to be explained is a segment of such a causal net, whose identity (as a [42] particular segment) can be revealed and identified only by being declared relevant to the answer of a why-question or, if one prefers, by being declared explanatory with respect to this why-question. Otherwise, on what basis would this necessary segmentation of a causal net made up of non-denumerable interconnected causal links and of non-denumerable stages in the Salmonian at-at positions of a genuine causal process be effectuated? The famous fable used to vulgarize the chaos theory, according to which the flapping of a butterfly's wings can cause a storm in the antipodes is possibly a fanciful exaggeration, but it nonetheless illustrates the intricacies of causal connections which link together all of the events happening in the world. In order to illustrate the implications of this point, it might be useful to raise again the over-discussed metaphysical question concerning the traits of a world free of conscience. In such a world, all events would continue to be linked (through chains of contiguities) to a non-denumerable set of others in a network of multifarious connections. For sure, these connections, which, in our world, are observed or inferred empirically by scientists, are not freely invented by them; consequently, they would still be working in a world without conscience. The moon would continue to produce tides in exactly the same way as it used to do for conscious beings. In this sense, the entities and events that we used to consider as the cause of the tides would still be there. In the wording of Salmon's at-at theory, at each intervening stage in the causal process that produces tides, the characters of the marks involved would have no reason to be changed by the absence of any conscience. However, this does not mean that the causal processes would be there, waiting for a conscious mind to pick them out and, by this way, be transformed into explanations. Indeed, the required structure of those infinite amounts of contiguous marks (whatever their exact nature may be) which goes from the moon (artificially taken in isolation

here) to the oceans and the tides would no longer be there in the absence of a conscious mind. The physical impact of the sequence of events in the causal net would not be affected, but it is only by reference to a particular question, and depending on the interest and background knowledge of a questioner, that particular segments of this causal net can be chosen and structured by the person providing an answer in such a way that they can become the key elements referred to by propositions constituting a relevant answer to the question, and therefore the required explanation. Thus, if one can say, in a somewhat metonymical fashion, that the attraction of the moon explains the tides, one cannot say [43] that the explanation of the tides must be discovered in a world in which the explanation would already be present in the form of a set of causes. ²⁰

I do not try in conclusion to conciliate Salmon and van Fraassen respective theses. Salmon's realism and van Fraassen's anti-realism — an anti-realism which is, in some sense, an agnosticism in ontological matters — bear on metaphysical questions, and I think that options concerning these questions are too fundamental to be conciliated by the emphasis put on the fact that, from a particular point of view, a certain convergence is manifest. However, as suggested above, it is not necessary to be openly realist to acknowledge the essential role of causal processes and nor is it necessary to be openly anti-realist to admit that an explanation is dependent on an epistemic context. Even if we were ready to characterize them as made up of elements and stages whose very existence is empirically ascertainable, the genuine causal processes bringing about the event to be explained would constitute a causal net so complex and so diffused that, in spite of its potential explanatory content, it would be impossible to translate it as

²⁰ Opposite positions in this debate are not unrelated to those adopted in philosophy of history in the context of the debate on narrativism. Narrations (regardless of whether they are explanatory or not) are frequently used by historians. But are they pre-existing in a narratively structured real world (as claimed by David Carr) or are they freely constructed by historians (as suggested by Louis Mink and Hayden White)? It seems reasonable to insist with Carr on the fact that historical narrations are based on causal links which cannot be freely constructed, while observing, in accordance with Mink and White, that the real world is so complex that historians must themselves structure narrations which cannot be discovered as already existing in the real world. (On this, see Lagueux, 1998a, pp. 71-86 and 2001, pp. 187-202)

a whole into a set of propositions likely, thanks to this content, to constitute an explanation. Among the factors pertaining to that causal net — or related to the explanandum in any fashion, given that not all explanations are causal — only those selected with due regard to the question requiring the explanation should be referred to, after being structured in a relevant manner, by the answer required as explanation.

These considerations should be related to Peter Railton's notion of an "ideal explanatory text", which Salmon presents as helpful for "reconciling the views of the pragmatists and the realists" (Salmon, 1989, p. 161). This notion, which refers to an ideal explanation about which "a whole range of less-than-ideal proffered explanations" aim to "convey information" (Railton, 1981, p. 247) is not defined, however, in a perfectly clear fashion. Railton illustrates it, at the outset (p. 240), by the completely formulated version of a D-N-P model of probabilistic explanation [44] including a deductive phase, an inclusive model previously exposed by him (Railton, 1978), but his notion of an "ideal explanatory text" is apparently still more inclusive. Indeed, a few pages further, he describes the "ideal text for the outcome of a causal process" as "an inter-connected series of law-based accounts of all the nodes and links in the causal network culminating in the explanandum", (1981, p. 247) adding that, given its virtual infiniteness, "there is no question of ever setting such an ideal text down on paper (p. 247). In any case, this apparently impossible task might even be undesirable ; : "[...] we would not always find it appropriate to provide even a moderate portion of the relevant ideal texts in response to particular why-questions. On the contrary, we would tailor the explanatory information provided in a given context to the needs of that context". (1981, p. 244) I am not sure that my own attempt to make room for both causal processes and context-dependence in the theory of explanation fits exactly with Railton's analysis of an ideal explanatory text, nor that I see the conciliatory virtues of this notion in exactly the same way as Salmon, but it does seem that, if one forgets the ontological debate about realism, the prospect of a theory of explanation drawing on the most valuable aspects of Salmon's and van Fraassen's respective views is not out of reach. After all, Salmon seems relatively happy with Railton's approach (Salmon, 1989, especially pp. 159, 163), which, in his view,

is an ontic conception like his own (1989, p. 121), but, if my interpretation is correct, such an "ontic" conception is also congenial to most of van Fraassen's theses on explanation. The theory of explanation whose philosophical interest I have tried to underscore in this paper derives largely from debates holding between philosophers of sciences such as Salmon, van Fraassen and Railton whose primary interest is explanation in physics and who almost never attempted to apply their views to social sciences. Nonetheless, being less restrictive than Hempel's theory — which, ironically, was so frequently tentatively applied by its author to history and other social sciences — the theory of explanation resulting from these debates would seem to better accommodate the kind of explanation prevailing in social sciences such as history and economics. "Rational explanations" are answers to questions of a particular kind familiar both to historians and economists. Given the teleologically oriented context of these questions, they require answers that may differ substantially from answers required by questions, typically [45] met in physics, oriented towards strictly causal processes or towards conditions of coexistence. However, the scientific character of these different kinds of answers is not to be associated with one of these orientations but is to be evaluated in an independent process subject to all the difficulties associated with the demarcation problem. While the elicitation of a genuine causal process may be an essential part of what empowers rational explanations to transform events perceived as puzzling into (disjunctively) expected events, causal processes elicited in such a way may constitute only a small fragment of the relevant ideal explanatory text — a fragment which does not need to include laws as such. Incidentally, Dray was not so far from the idea that any satisfactory explanation is a fragment of a never fully formulated ideal text when, in his somewhat heroic attempt to challenge Hempel's view according to which a correct covering law argument might be a sufficient condition of explanation, he invoked the need to elicit "continuous series of events" instead of a too partially informative deduction, and illustrated his point with the Hempellian example of a motor seizure (Dray, 1957, pp. 66 ff.). In this connection, one might also reconsider the explanatory role of narratives — which play a more and more acknowledged role in social sciences ²¹ and whose

²¹ van Fraassen illustrates this point through an incidental example drawn from

explanatory character has been underlined by a number of social science philosophers — as segments of an ideal text, which are usually free from any law and roughly evocative of some causal processes. There is little doubt that the application to social sciences of the ideas developed in a relatively recent philosophy of natural sciences would need much more elaborate discussions and analyses, but this would be out of place in the present paper, whose goal has more modestly been to characterize and discuss these ideas as such and to emphasize and summarily illustrate the interest and the potential fruitfulness of such an application.

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