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Philosophy of Uncertainty and Medical Decisions

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1. Probabilistic Strategy

We are always surrounded by uncertainty. It is true that we sometimes say, 'It is certain that a typhoon is coming', but what is the source of that claim of certainty? Perhaps it is the information we have at the moment. However, most of the information we get in our daily life is only second-hand, by means of those in authority or the mass media, hence in principle we should not give complete credence to it. Strictly speaking, even primary information that we experience directly or that originates with the authorities or eyewitnesses themselves could be regarded as uncertain because of our epistemic limits on such factors as perception, vague words used to describe such information,[1] or memory. As is suggested by the typhoon example, we often use information that we have already had to predict future events or to justify general scientific laws. This type of procedure is called induction. Yet, this whole procedure of induction must be taken to be uncertain from a philosophical point of view, because that information relied on is simply uncertain. Additionally, nobody can know what will happen in the future. Undeniably there is an intrinsic asymmetry between the past and the future. In other words, if inductive procedure aims to justify general scientific laws in a perfectly rational way or to predict the future with absolute certainty, that aim is completely hopeless from the outset. Someone might hesitate to accept such all-out uncertainty about induction, thinking that we could reach certainty with regard not to

1. There has been a massive accumulation of philosophical literature on the problem of vagueness in connection with the sorites paradox. As to logical problems about vagueness, Priest (2001) section 11 is very helpful for quickly understanding its significance.

information itself but to a logical form of inference in dealing with information. However, this is a problem of the validity of inference rather than a problem of certainty. The validity of inference guarantees the truth of a conclusion only if the premises are certainly true. If the premises are uncertain, even a valid inference must involve uncertainty.[2]

Here I will focus my attention on some aspects of the problem of induction mentioned above. Currently, no philosopher seeks to explain how to justify general scientific laws in a perfectly rational way or how to make an absolutely certain prediction about the future when discussing the problem of induction, because he or she fully recognizes that only clairvoyance could accomplish such tasks. So, instead of facing the problem directly, he or she evades it by only elucidating the relation between evidence and hypothesis in terms of introducing the notion of confirmation. In short, if evidence (e) positively supports a hypothesis (h), in other words, e makes h more acceptable, then it can be said that e confirms h. On the contrary, however, if e makes h less acceptable, we say that e disconfirms h. Roughly speaking, two strategies have been dominant in philosophy as theories of confirmation, that is, the *deductive* strategy and the *probabilistic* one. According to the deductive strategy, evidence confirms a hypothesis if the evidence can be deduced from the hypothesis, whereas the probabilistic strategy claims that there is a conditional probability for any hypothesis given any statement of evidence, wherefore confirmation relations are analysed in terms of probability relations.[3] However, it seems that the deductive strategy cannot adequately reflect an actual circumstance of confirmation in scientific activities, where the confirmatory process is dynamically carried out by degrees as new evidence is obtained, even though no certainty is expected. In this sense, I think that the probabilistic strategy deserves the most careful consideration, which also actually fits well the observation about uncertainty that I have made.[4]

In the following, I examine first the most influential method in the probabilistic strategy, namely, *Bayesianism*, considering the crucial objections to it. Then I will propose a different approach to correctly understand the problematic situation that those objections describe, that is to say, an approach to face problems about confirmation by finding confirmation involved in some process of decision-making. I will suggest it by referring to medical cases. Those arguments seem to show that Bayesian ideas are unacceptable, but I finally try to indicate that Bayesian ideas can still be highly significant, although in a restricted and different sense.

2. Problems concerning deductive inference where premises are uncertain, that is, the logic of uncertainty, are now actively discussed in the context of examining a probabilistic interpretation of conditionals since Ramsey and Adams. Edgington (1995) is the basic literature.

3. Glymour (1980), pp. 12–13.

4. The following argument, in particular the first half of it, is partly based upon my previous paper, Ichinose (2004).

2. Bayesian Confirmation Theory

Well, exactly, what are Bayesian ideas about confirmation? Let us take a simple example for the sake of understanding the argument clearly. Suppose that an oculist proposes this hypothesis:

(h_1) consuming blueberries helps our eyes to function well,

and that I have this evidence:

(e_1) I actually suffered from less eye fatigue in front of my computer screen after consuming blueberries than when I didn't have them.

Obviously e_1 gives some positive support to h_1, therefore we can safely say that e_1 confirms h_1. The main idea of the probabilistic strategy is to interpret this confirmation relation as 'e_1 raises $P(h_1)$ (i.e., the probability of the truth of h_1)'. A relation for evidence to raise the probability of a hypothesis is regarded as crucial to confirmation. One of the most influential approaches following this idea is definitely *Bayesianism*, or exactly *Bayesian confirmation theory*, in which changes of probability of a hypothesis are expressed by *Bayesian Conditionalisation*. According to that, we can formulate the principle of how to update the probability of a hypothesis (H) on the receipt of evidence (E) in terms of conditional probability and Bayes's Theorem (that is why it is called Bayesianism) in this way.

$$P_{pos}(H) = P_{pri}(H \mid E)$$
$$\frac{P_{pri}(H\&E)}{P_{pri}(E)} = \frac{P_{pri}(E \mid H)P_{pri}(H)}{P_{pri}(E)} \qquad (P_{pri}(E) \neq 0)$$
$$= \frac{P_{pri}(E \mid H)P_{pri}(H)}{P_{pri}(E \mid H)P_{pri}(H) + P_{pri}(E \mid \sim H)P_{pri}(\sim H)}$$

This is the principle of Bayesian Conditionalisation (BCOND), in which $P_{pri}(X)$ stands for prior probability of X and $P_{pos}(X)$ posterior probability of X. Here I take the bearer of probability (E or H in this case) to be sentences.

Two basic characteristics of Bayesian confirmation theory must be mentioned here. First, subjective probability that has been traditionally defined as degrees of belief since Ramsey may be used, in particular as *prior probability*.[5] This is the most well-known characteristic of Bayesianism, which also generates very broad applicability of the Bayesian approach.[6] Second, Bayesians stipulate that such degrees of belief should satisfy the basic mathematical rules of probability, which is usually made through the so-called Dutch Book Argument (or sure-loss contract). As this second characteristic suggests, Bayesians are supposed to offer their arguments as *normative* ones rather than *descriptive* ones, because we often unknowingly violate the rules of probability as a matter of psychological fact. In any case, apparently Bayesian confirmation theory skillfully evades the traditional insoluble problem of induction, because, as Hacking expresses well, in Bayesian arguments 'the question is not whether these opinions (i.e., hypotheses) are "rational"'. The question is whether we are reasonable in modifying these opinions in the light of new experience, new evidence'.[7] Bayesian confirmation theory is also undoubtedly a lucid and plain methodology for estimating degrees of confirmation. If $P_{pos}(H) > P_{pri}(H)$, then H is confirmed by E, and if $P_{pos}(H) < P_{pri}(H)$, then H is disconfirmed by E. That is simple and seems to be intuitively reasonable enough to explain our common-sense feeling about induction.[8] Actually, Bayesian confirmation theory or Bayesian epistemology acquires strong applicability and practicability in a current context by means of developing a graphical model which is called *Bayesian Networks* (or *Bayesian Nets*). Bayesian Nets

5. I won't put forward here an argument concerning how to interpret the concept of probability, which is a quite perplexing subject. As to interpretations of the probability concept, see Gillies (2000), which examines the propensity theory minutely as well as gives a subjective interpretation, so is highly useful as an overview of the controversies about this problem.

6. However, precisely because of allowing subjective probability, the Bayesian approach is often avoided by some philosophers. Instead, likelihood (that corresponds to $P_{pri}(E \mid H)$ in the formulation on the previous page) is sometimes highlighted, since it can be evaluated in a relatively objective way, and since the likelihood ratio is a very easy and convenient tool for comparing different hypotheses. Such a standpoint is occasionally called likelihoodism. As to some problems concerning Bayesianism and likelihoodism, see Sober (2002), pp. 21–38. Howson also makes an interesting point about likelihoodism, namely that in order to make likelihoodism meaningful, we eventually appeal to Bayesian theory. See Howson (2002), pp. 51–53.

7. Hacking (2001), p. 256.

8. Talbot (2001) compactly explains the main ideas and positive effects of Bayesian epistemology, as well as its potential problems. That is quite helpful. Bovens and Hartmann (2003) is an eminent work about Bayesian epistemology in the present context, which considers problems of coherence or reliability as well as confirmation.

consist of two components: (1) a directed acyclic graph in which evidence and hypotheses are taken to be variables and are connected by arrows, and (2) a probability distribution (or a probability specification).[9] That is a tool by which we can easily recognize dependency and independency between variables.

However, as usual with regard to any philosophical theory, many criticisms have been levelled at those Bayesian ideas, whereby a lively controversy has been aroused until now. In the course of this, Bayesians have actually refined their ideas to answer the criticisms and surely achieved some improvement, but we have to say that fundamental difficulties still remain as far as a simple application of Bayesian ideas or BCOND is concerned. Broadly speaking, it seems to me that there are two directions of crucial difficulties, namely, (1) BCOND might include some cases irrelevant to confirmation as examples of confirmation, and (2) BCOND could not explain some cases relevant to confirmation. To put this in a nutshell, BCOND is sometimes too tolerant, and sometimes too intolerant.

3. Evidential Relevance

The first difficulty is directly concerned with the essential question, that is, why can we connect the issue about degrees of confirmation simply with conditional probability? As Glymour straightforwardly pointed out, 'the evidence confirms or disconfirms the hypothesis with respect to the theory',[10] so 'if a hypothesis is not tested by a piece of evidence with respect to a theory, there is always some *other* theory with respect to which the evidence confirms or disconfirms the hypothesis'.[11] In other words, confirmation is established between evidence and hypothesis BY VIRTUE OF background theory or background knowledge, hence the relevance of evidence to the background theory (and to the hypothesis through the theory) is intrinsically required. This is suggested by the previous blueberry case. We believe that e_1 confirms h_1, which is supported by, at least, our background knowledge about the influence that a computer screen has on our eyes. Were it not for such knowledge, we could not establish that confirmatory relation. Nevertheless, BCOND itself doesn't impose such a

9. See Pearl (1988), pp. 116–31, and Williamson (2005), esp. pp. 14–48. According to Williamson, Bayesian Net does not necessarily imply that we use Bayesian subjective probability, and the reason why this is called 'Bayesian' is only that it appeals to Bayesian Conditionalisation. Williamson puts forward a view of *Objective Bayesianism* which is based upon prior probabilities that are publicly acceptable.

10. Glymour (1980), p. 110.

11. Ibid., p. 120.

requirement for the evidential relevance to such background knowledge or theory, because it simply regards Bayes's theorem, a mathematical equation, as the expression of confirmation. This situation of BCOND might produce some counterintuitive cases that are taken to be confirmation relations despite lacking such evidential relevance. In order to recognize this clearly, let us take two counterexamples to BCOND.

First, I want to give an example about a causal relation which is related to the blueberry example again. Certainly, as I said, we can accept that my reduced eye fatigue after consuming blueberries (e_1) raises the probability of the hypothesis (h_1) that consuming blueberries causes our eyes to function well. But what about the next hypothesis?:

($*h_1$) having our teeth discoloured by blueberry juice causes our eyes to function well.

Perhaps whenever we consume blueberries we automatically discolour our teeth with blueberry juice more or less. Then, according to our experience, it can be said that having our teeth discoloured by blueberry juice has been regularly conjoined with the amelioration of our eye function. Now, let us think of the next evidence:

($*e_1$) I actually suffered from less eye fatigue in front of my computer screen after discolouring my teeth with blueberry juice than when I didn't do that.

As far as conditional probability is concerned, we have to say that $*e_1$ would also raise the probability of $*h_1$, i.e., $*e_1$ confirms $*h_1$. However, this confirmatory relation is utterly unacceptable, because we don't think that discolouring our teeth with blueberry juice causally affects our eye function. In reality, we firmly believe that, even if we discolour our teeth with artificial blueberry juice, the discolouration would have no effect on our eye function, because there is no causal relevance. This is nothing but the problem of spurious cause in Suppes's terminology in the context of probabilistic causality.[12] Of course, such a problem could be coped with by applying the idea of 'screening off'. Suppose that we compare the case (1) of consuming blueberries while carefully trying not to discolour our teeth with blueberry juice with the case (2) of consuming blueberries without so trying, and the probability of the amelioration of our eye function remains the same in either case. Let EA be 'my eye function is ameliorated', CB 'I consume blueberries', and DTB 'I discolour my teeth with blueberry juice', and that the conditions can be expressed like this:

12. Suppes (1970), pp. 21–28.

$$P(EA \mid CB \& DTB) = P(EA \mid CB).$$

That is to say, CB screens off DTB from EA (or we can also state that CB screens off EA from DTB), which means that DTB is just a spurious cause of EA. But this very situation seems to show that BCOND by itself is insufficient to treat that confirmatory process with causal relevance. Thus, we have to be careful of the conceptual difference between conditional probability and causal relation, both of which are intertwined but nonetheless must be distinguished from each other. BCOND cannot correctly consider this delicate difference between probability and causality, as far as it appeals only to conditional probability.

However, the current notion of Bayesian Nets tries to refine Bayesian confirmation theory by simply incorporating an operation of 'screening off' into the Nets. When networks are constructed, causal relations among variables are often used as a foundation of the Nets. Such a process of construction is supported by the next principle called *Causal Marcov Condition:*

Causal Marcov Condition
Each variable is probabilistically independent of its non-effects, conditioned on its direct causes.[13]

This Causal Marcov Condition is linked with '*Principles of the Common Cause*', which 'claims that if two variables are probabilistically dependent then one is the cause of the other or they are effects of common causes and those common causes *screen off* one variable from the other, i.e., render the two variables probabilistically independent'.[14] In this way the operation of 'screening off' is integrated into Bayesian Nets. However, even though such refinement is introduced, the Nets still could not perfectly distinguish a probabilistic relation from causation. For there can be a case of probabilistic dependence that could not be screened off although such dependence is not causal at all. For instance, Williamson mentions the case about 'having flu' and 'infection of Orthomyxoviridae (a general class of virus including flu as one subclassification)'. They have related meaning and are probabilistically dependent, so the one cannot screen off the other even if a common cause is supposed, but nevertheless there is no causal relation between them. Yet, if we simply infer by using Bayesian

13. Williamson (2005), p. 50.
14. Ibid., p. 51.

Nets, we will have to conclude that there is a causal relation between these two variables.[15] This is a serious defect in developing the theory of confirmation, since many hypotheses to be confirmed definitely involve causal relations.[16]

Second, I mention an example created in connection with logical relations. Let us appeal to the blueberry example again. As already recognized, e_1, evidence of my reduced eye fatigue after consuming blueberries, confirms h_1, a hypothesis of a causal relation between consuming blueberries and amelioration of eye function. Then consider the next hypothesis:

(h_2) swallowing mercury causes our brain to work better.

Then think about this probability of the conjunction of h_1 and h_2 conditional on e_1 (ignoring temporal order, which causes no problem here):

$$P(h_1 \,\&\, h_2 \mid e_1) = \frac{P(e_1 \mid h_1 \,\&\, h_2)P(h_1 \,\&\, h_2)}{P(e_1)}$$

According to the initial supposition, it seems that the likelihood, $P(e_1 \mid h_1)$, is very close to 1, hence, if we follow a logical characteristic of conjunction, the likelihood of $P(e_1 \mid h_1 \,\&\, h_2)$, is also very close to 1. So, as far as the prior probability that $P(e_1)$ is clearly less than 1, it will come to $P(h_1 \,\&\, h_2 \mid e_1) > P(h_1 \,\&\, h_2)$, because $P(h_1 \,\&\, h_2 \mid e_1)$ is mostly equivalent to what results from $P(h_1 \,\&\, h_2)$ divided by a number less than 1. Thus, BCOND must declare that e_1 confirms $h_1 \,\&\, h_2$, but we would find it quite absurd to think that my reduced eye fatigue after consuming blueberries gives evidential support to the claim of blueberries' causal effect on the eyes and mercury's causal effect on the brain. Perhaps Bayesians react to this by asserting that an increase of $P(h_1 \,\&\, h_2)$ given e_1 is less than an increase of $P(h_1)$ given e_1 because the prior probability of $h_1 \,\&\, h_2$ is less than that of h_1. However, such an assertion does not erase at all the fact that e_1 is supposed to confirm $h_1 \,\&\, h_2$ by BCOND. Thus, unless Bayesians introduce some other idea that can consider the relevance of evidence to a background theory like Glymour's bootstrapping strategy, it does not seem that BCOND could avoid this absurdity.[17]

4. The Problem of Old Evidence

Well, let's move to the second difficulty concerning BCOND. That is to say, BCOND cannot describe a confirmatory process in which $P_{pri}(E) = 1$, because in that case, $P_{pri}(H \mid E)$ is equal to $P_{pri}(H)$, so it comes to $P_{pos}(H) = P_{pri}(H)$, which means that evidence has nothing to do with confirmation.[18] Of course, if no confirmatory process occurs when $P_{pri}(E) = 1$, there is no problem. That case is simply irrelevant to confirmation, therefore harmless to BCOND. For example, probabilities of logical truths in the form of a tautology like modus pones or the law of excluded middle must be 1 (in fact this is usually included in the axiom of probability calculus), no matter when we estimate them. However, P_{pri}(tautology) = 1 causes nothing inconvenient to BCOND, since we do not carry out any confirmatory reasoning conditional upon logical truths, although we can make a deductive inference by them. Another harmless case is that we propose a hypothesis in order to explain evidence which we have already had. In this case, although P_{pri}(this evidence) = 1, we have no problem with regard to BCOND, because we do not expect this evidence to confirm the hypothesis. This evidence is rather the basis of the hypothesis, so it is logically entailed by the hypothesis. In other words, this evidence isn't regarded from the start as contributing to confirmation. If we illustrate this in the blueberry

15. Williamson (2005), p. 53.

16. Currently theories of probabilistic causality are developed by being sensitive to the problem of causal relevance. For example, the notion of 'Contextual Unanimity' that Nancy Cartwright introduced borrowing John Dupré's terminology reflects this trend, as this implies that causes are context-independent, i.e., causes are causes no matter what relevance is concerned. See Cartwright (1989), p. 143 et al. This notion itself, however, must be scrutinized further because some counterexamples are imaginable. See Hitchcock (2002), p. 16. Incidentally, Twardy and Korb (2003) are worth noting in connection with this topic, since they propose a new strategy of probabilistic causality by offering the refined notion of 'Objective Homogeneity', at the basis of which contextual unanimity is defined. They also try to combine this notion of objective homogeneity with Bayesian Networks. Their argument might influence my points about Bayesian Networks. But I need time to reflect on that, so I want to make it my next task to consider how to evaluate their arguments.

17. The same absurdity was pointed out by Glymour regarding the hypothetico-deductive account of confirmation (i.e., the deductive strategy). See Glymour (1980), p. 135. As to an application of this absurdity to Bayesianism, see Pennock (2004), section IV.

18. This can be proved easily. According to the definition of conditional probability,

(1) $P_{pri}(H \mid E) = P_{pri}(H\&E) / P_{pri}(E)$.

So, general addition rules of probability tell us that

(2) $P_{pri}(H\&E) = P_{pri}(H) + P_{pri}(E) - P_{pri}(H \vee E)$.

Then, by presupposition,

(3) $P_{pri}(E) = 1$.

So, since E logically entails $H \vee E$, $P_{pri}(H \vee E)$ can't be less than $P_{pri}(E)$. Therefore,

(4) $P_{pri}(H \vee E) = 1$.

Then, by applying (3) and (4) to (2),

(5) $P_{pri}(H\&E) = P_{pri}(H) + 1 - 1$
$= P_{pri}(H)$.

Consequently, by applying (3) and (5) to (1),

(6) $P_{pri}(H \mid E) = P_{pri}(H)$.

example, this evidence corresponds to the past data about the causal effect of blueberries on eye function that probably the oculist has already observed. Those data compose the hypothesis h_1 rather than confirm it.

However, we have to pause here. Are those cases with probability 1 always harmless to BCOND? What should we think of the case in which we subjectively feel that those things with probability 1 play the role of evidence and confirm the hypotheses? In questioning this way, we come across the so-called problem of old evidence. This problem was initially raised by Glymour, who describes it in an unmistakable way like this:

> Newton argued for universal gravitation using Kepler's second and third laws, established before the *Principia* was published. The argument that Einstein gave in 1915 for his gravitational field equations was that they explained the anomalous advance of the perihelion of Mercury, established more than half a century earlier. . . . Old evidence can in fact confirm new theory, but according to Bayesian kinematics it cannot.[19]

The gist of his point is very obvious. Old evidence about the perihelion of Mercury had already had probability 1 at the time of 1915 when Einstein proposed his theory of gravitational field equation, therefore, following the situation in the case of $P_{pri}(E) = 1$ which I mentioned above,

$$\begin{aligned} P_{1915}(GFE \mid PM) &= \frac{P_{1915}(GFE \ \& \ PM)}{P_{1915}(PM)} \\ &= P_{1915}(GFE) \end{aligned}$$

where GFE stands for the gravitational field equation and PM is old evidence about the perihelion of Mercury. That is to say, this old evidence has no confirming power. Nevertheless, as a matter of fact, the old evidence actually confirmed his theory. Thus, BCOND can't deal with this fact appropriately. This seems to be a very serious criticism against BCOND and Bayesian epistemology. Could Bayesian epistemology solve the problem of old evidence? Or should we flatly reject Bayesian ideas because of this problem?

5. Three Responses to the Problem

Bas van Fraassen neatly presents three possible responses to the problem of old evidence.[20] (1) First, no one is certain about the old evidence, namely,

19. Glymour (1980), p. 86.
20. van Fraassen (1988), p. 154.

P(old evidence) ≠ 1. 'This response would be the most effective possible, it would remove the problem altogether'. (2) Second, when saying that the old evidence confirms the hypothesis, 'we make reference not to our present actual epistemic state but to some alternative(s) thereto'. That is to say, we are considering such a counterfactual conditional that if the old evidence had been established sufficiently later than it actually was, it would have confirmed the hypothesis. (3) Thirdly, 'the phrase "E confirms H" may well be used in the speech of the vulgar, but attention is being drawn to something else that has to do with E, and really does confirm H for us'. To put it another way, the problem should be interpreted as rather concerning our subjective state of belief beyond the relation between evidence and hypothesis *simpliciter*.

The first response claiming P(old evidence) ≠ 1 is quite attractive in order to eliminate the problem of old evidence altogether. For example, Mark Kaplan suggests that probability 1 could be assigned only to logical or mathematical truth.[21] Timothy Williamson also maintains that evidential probability cannot always keep 1 by denying a tacitly presumed claim about evidential probability, that is, MONOTONICITY, which says, 'once a proposition has evidential probability 1, it keeps it thereafter'.[22] Williamson finds BCOND to allow propositions to acquire probability 1, but not to lose it, which leads to *monotonicity*. But he refuses this idea by emphasizing the factor of our forgetting, and claims that evidential probability could go down to less than 1 as time passes, saying, 'Bayesians have forgotten forgetting'.[23] If that is the case, the problem of old evidence will instantly vanish, since this problem stems entirely from the situation of P(old evidence) = 1.[24]

However, this response against P(old evidence) = 1 is not perfectly reasonable, for if we restrict probability 1 to logical or mathematical truth, the concept of evidence seems to lose a foothold. If obvious data of observation we have here and now, which is typically taken to be evidence, have only less than probability 1 in any sense, it is difficult to talk about evidence. Williamson's argument avoids this defect, since he allows the case in which evidence has evidential probability 1 for some time before

21. Kaplan (1996), pp. 50–51.
22. Williamson (2000), p. 218.
23. Ibid., p. 219.
24. I am fully aware that we have to take into account *Jeffrey Conditionalisation* as well in this context of questioning whether evidential probability is always 1 or not, since the main point of Jeffrey Conditionalisation is to seriously consider the possibility that probability of evidence is not 1. But I think that focusing my attention on scrutinizing BCOND is not off the point, because that is definitely still the very core of Bayesian epistemology. See Jeffrey (1983), Chapter 11.

we forget it. Yet, how about the case where we get a particular observational datum, and then happen to stumble upon a particular hypothesis independently of the datum immediately after the datum (therefore keeping evidential probability of the datum 1), and then come to realize that the datum confirms the hypothesis (which is not so rare actually, as I will mention later)? In that case, the problem of old evidence seems to appear again, as far as the term 'old' is used in a loose way, as in our ordinary life. However, in spite of that, I want to think of much of this response from a bit different point of view, which I shall mention later.

Let us glance at the second response, namely the one to avoid the problem by introducing counterfactual conditionals. Howson and Urbach give a typical argument of this reaction. They say that in the formulation of the problem of old evidence against Bayesian theory, 'it is clear that the theory has been incorrectly used. It is equally clear where the mistake lies, namely, relativising all the probabilities to the totality of current knowledge. They should, of course, have been relativised to current knowledge *minus e*'.[25] Then, there are the usual objections against this reaction. How can we go about having a uniquely determined set of background knowledge minus that particular old evidence? How should we treat propositions logically deduced from the sentence of evidence? As far as those counterfactual conditionals involve many fictitious suppositions about the history of science, we should ask how we could verify the truth of those counterfactual conditionals.[26] In short, all of those objections point out that those counterfactual suppositions are too unclear to be enough to clarify and solve the problem of old evidence. I think those are absolutely right. But so what? The fact that counterfactual conditionals are hard to verify cannot be the reason at all to exclude them from our analysis of BCOND, since any theory of confirmation must involve some counterfactual analysis in theoretically analysing the relation between evidence and hypothesis, or in making predictions. When predicting something, we have to make comparisons between several future possibilities by taking into account each probability, which is definitely the process of counterfactual analysis. Actually, such a sort of counterfactual consideration is already embedded in the initial formulation of BCOND, for BCOND offers the device of analyzing a confirmatory process by supposing the situation before we get the evidence even though we have actually had it.

Well, how about the third response? This type of response is represented by Garber's argument, whose substantial idea is succinctly expressed by this:

25. Howson and Urbach (1993), p. 404ff. See also Horwich (1982), p. 52.
26. See van Fraassen (1988), pp. 155–56, and Glymour (1980), pp. 87–91.

> If old evidence can be used to raise the probability of a new hypothesis, then it must be by way of the discovery of previously unknown logical relations. In the cases that give rise to the problem of old evidence, we are thus dealing with circumstance in which hypotheses are confirmed not by the empirical evidence itself, but by the discovery of some logical relation between hypothesis and evidence.[27]

If this is the case, the problem of old evidence would be solved, because the discovery of logical relations between hypothesis and old evidence is made now, before which the old evidence hasn't been regarded as evidence. As it were, that is newly discovered evidence which has old history. As is the same with the two previous responses, some philosophers have already raised an objection against this response, whose kernel seems to be very simple. For example, John Earman says, 'The original question was whether the astronomical data F confirmed GTR [the general theory of relativity] (for Einstein if you like). Garber, Jeffrey, and Niiniluoto replace this question with the question of whether Einstein's learning that $T \vdash E$ raised his confidence in the theory. Not only are the two questions not semantically equivalent; they are not even extensionally equivalent'.[28] In other words, the gist of this objection is that responses like Garber's concern not the issue of confirmation properly, but just the psychological process of belief of a person who studies the case in question.

I think that this objection isn't fair to the Bayesian theory of confirmation. It seems to me that there is a highly doubtful presupposition in the objection, namely that confirmation is a sort of objective relation between data and hypothesis independent of the human process for dealing with beliefs. However, we cannot deny at all that confirmation is primarily accomplished regarding our belief, or to put it another way, regarding our epistemic process, as Bayesian epistemology takes for granted. I completely agree with Bayesians in that they correctly accept the intrinsic connection between confirmation and our epistemic process. This could be made quite explicit if we adopt a deterministic view, since in that case confirmation in the form of raising probability obviously has nothing to do with objective reality.[29]

27. Garber (1983), p. 120.
28. Earman (1992), p.130. See also van Fraassen (1988), p.163.
29. Certainly what is called objective Bayesianism thinks much of objective factors in assessing confirmatory power, but such objective factors are only concerned with determining prior probability in some impersonal way. In particular, a sort of a principle of indifference is often relied on to determine that, but apparently the principle is hopelessly difficult to clearly formulate. See Talbot (2001), section 5.1.

For my part, I want to assert that all of the three responses to solve the problem of old evidence are somehow plausible, or that none of them are mistaken. However, I don't think that they are perfectly satisfactory, since they are just responses to the problem, so they don't seem to reach the systematic analysis of how a viewpoint supporting each response could contribute to the theory of confirmation. In addition, if those responses aim to defend Bayesianism, they are insufficient in that respect as well, because it isn't clear how they could cope with the difficulties of evidential relevance that I discussed before mentioning the problem of old evidence. Therefore, I will propose an idea to break the deadlock. I will try to do it by taking up the issue of medical cases. Actually, medicine is, I think, the most appropriate subject for verifying some philosophical theories about empirical knowledge obtained through induction or confirmation, since medicine is definitely a genuine empirical science. In my opinion, when we discuss the philosophy of science, it is an unrealistic one-sided view to focus on physics or evolutionary theory and ignore other areas, particularly relating to our practice of such as dietetics, exercise physiology, or ethology, as well as medicine, for actually science comprises immense fields of research. Furthermore, medicine is especially suitable to examine Bayesian theory which appeals to degrees of belief and subjective probability, because in medicine subjective belief essentially matters. For instance, subjective impressions that a physician receives from or gives to patients at the clinical scene is often crucial for diagnosis or treatment, or subjective belief can be an object of medicine in the case of psychiatry.

Now, let us consider the next situation as a typical example.

> A patient comes to the emergency room of a community hospital with signs and symptoms that the chief resident calls equivocal for appendicitis. He decides to consult with the chief of surgery, who agrees that the symptoms and signs are equivocal. She knows that patients with such symptoms and signs often have NSAP (i.e., nonspecific abdominal pain) and, if an operation is performed, will have had unnecessary surgery. Some such patients, however, have an inflamed appendix which may perforate by the time of surgery. She wonders if it might be beneficial to hold this patient for six hours in the emergency room to see whether the symptoms improve or worsen (or remain the same) before deciding whether to operate.[30]

30. Weinstein and Fineberg (1980), p. 13.

This is quoted from a famous textbook about clinical decision, so someone may doubt how relevant this decision problem of whether to perform an operation is to our context of discussing problems of confirmation. Of course, for the time being, decision problems about medical treatment should be outside my argument. However, what I focus my attention on is the uncertainty of medical diagnosis, as in the example the chief resident describes as 'equivocal'. Apparently diagnosis and treatment are two major tasks in medicine, which are supposed to be distinguished but actually are interconnected in a complicated way. Then, nobody denies that diagnosis is nothing but a theoretical task of confirming a hypothesis (e.g., this patient suffers from appendicitis), while treatment is corresponding to a practical application of the theory. In this sense, I can connect medical examples with problems of confirmation by specifically putting the case of medical diagnosis into question.

Well, how do physicians or clinicians confirm their diagnoses? I am asking this question from a descriptive point of view, although I know that Bayesian theory of confirmation is proposed in principle as a normative theory, as I said earlier. The reason why I intentionally do so is that any normative theories must be based on understanding actual matters of fact which empirical descriptions provide, otherwise they would not be persuasive. In this sense as well, it is highly beneficial to scrutinize medical diagnosis in order to solve problems of confirmation, since there has been plenty of empirical data already accumulated in the course of the study of evidence-based medicine (EBM). Anyway, we have to check how physicians or clinicians make their diagnoses. First of all, it should be noted that at the clinical scene evidence or data to confirm a hypothesis or diagnosis is not simply given but should be collected on purpose. Collecting these is usually done with consideration of what sort of diagnostic tests should be carried out and what kinds of risk is taken to conduct such tests. Undoubtedly, this is nothing but the process of decision-making. As a matter of fact, confirming diagnosis is basically regarded as one of the processes of clinical decision. According to Weinstein and Fineberg, '. . . most clinical decisions fall into one of two categories, namely, (1) decisions regarding whether to seek additional information (and, if so, how), and (2) decisions concerning which treatments, if any, are to be employed'.[31] Or, Chapman and Elstein say, '. . . the patient and physician must decide whether the benefits of a treatment outweigh the side effects or whether the risks of a diagnostic test are worth taking, given the information it will provide'.[32]

31. Weinstein and Fineberg (1980), p. 23.
32. Chapman and Elstein (2000), p. 189.

In fact, even computer-based clinical diagnostic decision support systems have already been developed.[33] In connection with the appendicitis example above, to hold this patient for six hours in the emergency room is a sort of diagnostic test to collect additional clinical data, which is actually, as the example clearly shows, involved in their decision-making. If so, we can say that confirmation of diagnostic hypothesis intrinsically includes a decision-laden aspect.

This decision-laden aspect could be recognized from a different angle. That is concerned with how to determine prior probabilities of a particular disease or a particular symptom, and how to determine the likelihood of a particular disease given a particular symptom.[34] How do physicians determine those? Usually they initially try to find relevant primary studies in the medical literature. However, this task itself involves probabilistic situations, because, for example, there might be publication bias (i.e., positive studies are more likely to be published than negative studies),[35] or because data published in those studies are not necessarily applicable to the patient at hand.[36] In other words, if they stick to such objective hard data, they have to choose or make a decision among them. This is also the appearance of a decision-laden aspect. But actually, physicians often face a clinical case in which it is not possible at all to get objective hard data. Nevertheless, 'decisions must be made and are implicitly based on judgment about probabilities of uncertain events'.[37] That is, physicians often rely on subjective probabilities, which make clinical cases fit Bayesian analysis. Yet, unfortunately, 'Information obtained by physical examination or a diagnostic procedure may be intrinsically ambiguous and may thus be interpreted differently by different observers. . . . Observers may differ in their ability to detect these signs and in their propensity to record them'.[38] Some psychological factors to produce cognitive bias when physicians determine basic probabilities have been pointed out; for example, representativeness, availability, or anchoring,[39] which are generally called 'confirmation bias'.[40] Of course, according to the Bayesian idea, such bias could be revised into an approximate agreement as evidence is

33. See Miller and Geissbuhler (1999), pp. 3–34.

34. Even generally speaking, many philosophers regard the problem of how to determine prior probability as a fundamental difficulty about Bayesianism. See Hesse (1975) and Swinburne (2002).

35. Hunink and Glasziou (2001), p. 225.

36. Ibid., p. 232.

37. Weinstein and Fineberg (1980), p. 172.

38. Ibid., p. 2.

39. Tversky and Kahneman (1974), pp. 1124–31.

40. Chapman and Elstein (2000), p. 187–88.

accumulated. But this is not the case with medicine, since clinical diagnosis and treatments are more or less urgent, with no time for collecting plenty of sufficient evidence. Thus, in order to reduce such bias properly and to be as precise about probability assessment as possible, even a method of decision by groups of experts, which is called the Delphi method, has been proposed.[41] In any case, these circumstances strongly suggest that there is a decision-laden aspect lurking in the clinical confirmation process.

Those considerations thus far help us to find that there is the same decision-laden aspect at a deeper level. That is to say, we have to make a decision about which background theory must be relied on to give a satisfactory diagnosis. As to the appendicitis example again, presumably clinicians of Chinese herbal medicine will have a completely different way of thinking to arrive at a diagnosis; for example, they may judge the cause as a strained abdominal muscle. If so, a physician who has been educated in both Western medicine and Oriental medicine (such physicians are currently not so rare) ought to make a decision about which system it would be more appropriate to apply. Mercury, which I took as an example (h_2) earlier, is well known to have been thought to be actually good for the health in early modern times although no one adopts such a system nowadays. Here we find a case in which a choice of theories must be made. Similar points can be raised at a less radical stage. Let's go back to the eye fatigue example. Symptoms classified as eye fatigue can equally appear in the case of a brain tumor or psychosomatic illness. Therefore, a physician seeing a patient with this symptom, exactly speaking, must make a decision about which department of medicine should treat her: ophthalmology, brain-surgery, or psychosomatic medicine. This could be regarded as a sort of choice of theories. If we probe deeply into this idea, it seems that decisions about how to collect diagnostic data or how to assess probability as I have discussed could also be taken to be a sort of decision about theories.

As a matter of fact, the same thing could be corroborated by the case of physicists as well as of physicians. Different reactions experienced by Lorentz and Poincaré to the Michaelson-Morley experiment could be one of the typical examples in which differences (i.e., different decisions) about background theory deeply influence the confirmation relation. Lorentz regarded the result (i.e., the null result) of the experiment as evidence of the contraction hypothesis by sticking to the theory of aether, while Poincaré took it to be evidence of the principle of relativity by questioning the theory of aether.[42] To put these points another way, evidence or data to support a diagnosis is inseparable from a background theory, which precisely

41. Hunink and Glasziou (2001), p. 237–38.

42. Cushing (1998), pp. 202–4.

corresponds to what I noted earlier by mentioning Glymour's basic idea and his bootstrapping strategy.[43] In fact, the same circumstance lurks in the idea of Bayesian Nets as well. When we construct networks, we have to 'choose' a variable as a root[44] and 'determine' the prior probability of the variables.[45] Obviously a process of rational decision-making lies, although unconsciously, at the bottom of those situations. Additionally, 'prior knowledge of causal relationships' is needed in order to construct Bayesian Nets,[46] which suggests that we have to make some decisions about causation in advance, particularly when we apply networks to the case in which there is an etiological controversy, such as a case about improving our physical condition. Therefore, we must say again, evidence or data to support a hypothesis or diagnosis is inseparable from background knowledge, i.e., from selecting particular knowledge rather than others.

7. Decision-Oriented Aspects of Confirmation

Thus, we have to consider a decision process for confirming a hypothesis in order to understand the problems of confirmation, at least as far as medical cases are concerned. This suggests that we must take into account a reciprocal relevance between a philosophical decision theory and a clinical decision analysis for the sake of establishing an adequate theory of confirmation. Actually, Chapman and Sonnenberg give the following significant suggestion: '. . . the study of medical decision making should be a two-way street: decision theory should benefit medical practice, and medicine should advance the study of decision making. . . . Medicine can contribute to decision theory because medical decisions sometimes pose unique challenges that spur advance in both decision analyses and descriptive decision theories'.[47] My argument so far happens to have the same way of paying attention to medical decision analysis from a descriptive point of view to examine philosophical problems, but in my case

43. My argument here might correspond to Christopher Hitchcock's argument about causation, as well as to Glymour's. Hitchcock stresses that causation should be studied not as a binary relation between cause and effect, but as a ternary one of cause, effect, and an alternative cause inferred from the context or background information. However, I am more interested in how we choose the context. That question is not seriously posed in Hitchcock's argument. See Hitchcock (1996), pp. 267–82.

44. Pearl (1988), p. 122.

45. Williamson (2005), p. 128.

46. Ibid., p. 49.

47. Chapman and Sonnenberg (2000), p. 7.

medical decision analysis is supposed to be linked with confirmation relation by noting the decision-laden aspect in the process of confirming a diagnosis.

In any case, next we ought to question a normative form of the decision-laden aspect of confirmation beyond the descriptive examination that I have been engaging in, insofar as my argument is related to decision theory.[48] What is absolutely necessary first to address such a question adequately is to clarify what sort of utility or value should be considered here. But, for the time being, the answer to this question is very simple in an official sense. That is to say, whether in diagnosis or treatment, a good quality of life of the patient at hand is the very utility to which a medical decision is directed. That can be just a life in contrast to death, longer years of life, or longer quality-adjusted life years (QALYs),[49] depending on the situation. However, I want to point out some additional factors that are rarely mentioned in this context. I raise three additional factors or values to be considered, which could be deduced from the quite pragmatic standpoint of regarding confirmation of diagnosis as a physician's practical action to make clinical records or to inform a patient of the diagnosis. First, as a matter of fact, physicians have to take into account the persuasiveness (perhaps gained through clarity and intelligibility) of their diagnoses, because currently physicians ought to go through the process of informed consent at the clinical scene. Second, a physician should somehow consider how significant giving each diagnosis is to the physician herself or the clinical institution she belongs to as regarding her or its reputation. This will be particularly essential when a patient has a very special social status or is suspected of having a very rare infectious disease. Third, a sort of psychological effect that patients receive from being informed of a diagnosis (I am not sure whether to call it the placebo effect or not) must be counted when a diagnosis is recorded or given. In reality many patients would be relieved at being given a particular name for their disease in contrast to the situation in which they are anxious about what disease they have. Or they might be healed by the fact that a professional physician explains a diagnosis based on detailed tests, perhaps in the form of a considerate chat. In these respects, giving a diagnosis to a patient has a certain clinical value by itself. If those factors or values,

48. As to the distinction between 'descriptive' and 'normative' in the context of decision theory, I learned a lot from Mellor (2003), which stresses the important role of descriptive and objective aspects in decision theory in contrast to the standard trend to think much of normative and subjective aspects.

49. Crisp (2004) directed my attention to the theory of QALYs, which is currently becoming important in the context of medical ethics.

which must involve careful decision-making, are clinically relevant to diagnoses, we have to say that confirmation of a diagnosis leads to decisions about such values. In other words, confirming a diagnosis is a decision-laden process as I discussed (where the value to be considered is simply the patient's good quality of life), but ultimately it is also oriented to decisions about those additional values or utilities.

Well, then, how can we think about the difficulties of confirmation I examined earlier by exposing the decision-theoretic (i.e., decision-laden and decision-oriented) aspects of confirmation? I think that those difficulties will be simply solved, or at least we will have a deeper understanding of the difficulties. As to evidential relevance, it comes to that such relevance must be simultaneously determined as decisions are made about what sort of diagnostic tests should be conducted. In a certain sense, decisions about diagnostic tests are nothing but decisions about the relevance of evidence to background theories, although such decisions could be challenged and revised later by new data. Then, how about the problem of old evidence? In reality, confirmation through old evidence is not rare at the clinical scene. Remember the appendicitis example. In the example two hypotheses corresponding to the particular symptoms are considered, namely, it is appendicitis or NSAP. But, according to Weinstein and Fineberg, '. . . the patient with acute abdominal pain may be diabetic'.[50] If so, and if some clinical test of the blood, for example, suggests quite strongly the hypothesis that the patient suffers from diabetes after the physician has been wondering whether the patient has appendicitis or NSAP, then evidence of acute abdominal pain is nothing but old evidence which confirms the new hypothesis that the patient suffers from diabetes.[51] Nevertheless, no theoretical problem occurs here, although the physician may be forced to change her strategy.

It is not difficult to understand what is going on here if we take into account decision-theoretic aspects included in confirmation of diagnosis. Confirmation of the new hypothesis by old evidence is based on decision making that is performed by considering both (1) a newly proposed relevance between old evidence and the new hypothesis through background theory and assessment of probabilities and likelihood, and (2) utility that

50. Weinstein and Fineberg (1980), p. 26.

51. In the process of confirming a new diagnosis of diabetes like this, the next case does not seem to be so rare: a physician recognizes the patient's abdominal pain again, and then learns the result of a blood test from which she reaches the diagnosis of diabetes independently of abdominal pain, and a few seconds later she finds the abdominal pain to be further evidence to confirm the diagnosis. In this case, even if we accept Williamson's denial of *monotonicity*, the problem of old evidence appears.

is newly questioned by the newly proposed relevance. In other words, so-called old evidence appears not as genuinely old evidence but as present evidence as it were in connection with newly proposed relevance and newly questioned utility in the course of decisions made now. This solution I offer seems compatible with three classical responses to this problem. My solution could be consistent with an idea that the probability of old evidence can be less than 1, because probability of evidence is supposed to be assessed in relation to the particular hypothesis through a particular background theory, so under a different theory, probability 1 of the evidence could not work as such probability, or simply may not matter. Next, as far as confirmation is based on a sort of decision making, counterfactual analysis is indispensable, since a decision is usually made considering several possible (counterfactual) results. Finally, Garber's appeal to the discovery of logical relations is very similar to my solution as regards having such an idea that we enter a new stage when we confirm a new hypothesis by old evidence.

However, two crucial questions might be raised. First, my argument is only concerned with the medical situation of confirming a diagnosis, so it is doubtful whether my proposal can be applied to any confirmation relation in general. I admit that this is still an open question, although I tend to think that my proposal is ultimately applicable to any confirmation relation, since, as a matter of fact, any confirmation should be made in the form of a sort of speech act directed to other people. Therefore, decision-theoretic aspects must be embedded there.[52] Second, someone may say that it is not clear whether my proposal of a decision-theoretic approach to problems of confirmation results in reinforcing Bayesian theory or undermining it. My argument, on the one hand, tries solving difficulties Bayesian theory faces, which seems to defend Bayesianism, but on the other hand, my proposal seems to virtually destroy Bayesianism in the end by fundamentally changing the structure of Bayesian theory. Presumably we have to take Bayesian decision theory into account rather than Bayesian confirmation theory, for my proposal is decision-theoretic. Actually, as far as my argument is decision-theoretic, I have to face the problem of what sort of decision theory I should adopt. Is it possible to adopt Bayesian decision theory? If it is possible, my argument would eventually come around to

52. According to Howie (2002), subjective interpretation of probability has always been dominant in the history of natural science from the eighteenth century up to now as a matter of fact, although officially frequency of interpretation of probability has been appealed to. If my proposal, like Bayesian theory, is based even partially on the empirical fact that we use subjective probability in confirming a hypothesis, Howie's argument might suggest that my proposal could apply to other fields of natural science than medicine.

the defense of Bayesianism. But what is Bayesian decision theory? What should be noticed about this at least is that Bayesian decision theory as explanatory is a theory which explains action in terms of degrees of belief and desirabilities, as Eells stipulates.[53] For if so, it seems to me that Bayesian decision theory takes over all the difficulties of Bayesian confirmation theory in that it accepts subjective probability, which leads to BCOND.[54] Furthermore, it is well known that Bayesian decision theory is puzzled by some paradoxes like prisoners' dilemma or Newcomb's problem, in which 'the probability matrix can be expected to change in one way or another as and when one act or another is chosen'.[55] Giving a diagnosis seems to have the same structure as those paradoxical cases according to my proposal, because it sometimes has a sort of placebo-like effect. Of course, Bayesians tried to cope with those paradoxes. For example, they invented a skillful strategy called ratifiability. Ratifiability requires us to make a decision with considering the probability matrix the agent would have if she finally decided to perform that act, on the supposition that it is possible for her not to perform the act she finally decides to perform.[56] Perhaps introducing ratifiability might make it possible to treat those paradoxes appropriately in the Bayesian framework to a certain extent. However, even this strategy doesn't seem to work well on a medical decision about diagnosis. If a physician does not perform the act of informing her patient of her diagnosis at the end of the day despite having finally decided to do so, a whole circumstance of diagnosis and its probability matrix would be changed as regards its placebo-like effects. At the clinical scene everything must be practical, so the supposition of ratifiability is not realistic. Thus, it seems that my proposal is undermining Bayesian confirmation theory or Bayesian epistemology rather than reinforcing it, at least as far as a simple application of BCOND is concerned.

8. Logic of Responsibility

In any case, the next task I should tackle must be to investigate what sort of non-Bayesian decision theory could clarify those decision-theoretic aspects of confirmation. However, that is so huge a task that I could not and should not do it here. Instead of that I will briefly discuss how possible it is to put Bayesian epistemology to some practical use regardless of those difficulties that I have mentioned thus far, because I don't think that Bayesian theory should be abandoned altogether. Undoubtedly Bayesian theory is an excellent and simple formulation of our reasoning, if decision making has been already performed concerning evidential relevance and utility. Therefore, if we apply Bayesian ideas to such cases in which such a decision has already been made, Bayesian theory is expected to show its own ability. What is such a case? What about the case in which we take the issue of past events?[57] It seems that when we retrospectively question past events like people's previous behaviour, relevance and utility have been established precisely by questioning them. Rather, we could say that questioning them arose from recognizing their relevance and utility. In this sense, responsibility is a promising subject to which Bayesian theory might appropriately apply. I conclude my argument by briefly sketching my (although more or less provisional) idea about this, which hopefully would lead to a logic of responsibility.

First, I would like to offer a formulation of a function of *expected harm* (EH) rather than expected utility. That can be given in a parallel way to a standard formulation of expected utility. I illustrate it by the appendicitis example. Suppose that, when I decided to *hold* (i.e., hold the patient with some abdominal pain for six hours in the emergency room), there were three possible results, i.e., improvement (*im*), worsening (*ws*), or sudden death (*sd*), whose probabilities were assessed at that time at 0.5, 0.4 and 0.1 respectively, and whose amount of harm (AH) was, for example, 0, 10 and 90 respectively. AH can be assigned arbitrarily, but in principle AH in the case of benefit or improvement, namely positive utility, should equal zero altogether, no matter how much positive utility is gained, since no harm comes. However, AH in such cases might be negative rather than zero if the result is so valuable that it is really worth risk or possible harm to try it. This corresponds to the case in which a physician dared to perform a hard emergent operation with very small probability of success (and with high probability of generating death) when the condition of the patient was very dangerous and would lead to early death without treatment. AH of the operation could be assessed negatively if successful in that case, and if unsuccessful, AH could be reduced compared with AH of that *simpliciter*. Anyway, EH at time t in the past of holding the patient for six hours in the appendicitis example can be expressed in this way:

53. Eells (1982), p. 41.

54. Actually Bayesian decision theory is often criticized for not taking into account the problem of causal relevance (i.e., one of the problems of evidential relevance) adequately, from which so-called causal decision theory arises. See Resnik (1987), pp. 112–15.

55. Jeffrey (1983), p. 16.

56. Ibid., pp. 15–20. See also Skyrms (1990), pp. 44–56.

57. It is not strange to apply probabilistic strategy to past events. Actually, for example, McCullagh discusses the issue of how to apply Bayes's theorem to statistical inference in history. See McCullagh (1984), pp. 57–64.

$$\begin{aligned} EH_t(hold) &= AH_t\,(im)P_t\,(im) + AH_t\,(ws)P_t\,(ws) + AH_t\,(sd)\,P_t\,(sd) \\ &= 0 \times 0.5 + 10 \times 0.4 + 90 \times 0.1 \\ &= 13 \end{aligned}$$

Let act_i denote a particular action based on a decision (whether confirmation or physical action), r its possible result, then my idea could be generalized in this way under the condition that there are only n possible results from r_1 to r_n:

$$EH_t\,(act_i) = AH_t\,(r_1)P_t\,(r_1) + AH_t\,(r_2)P_t\,(r_2) + \cdots\cdots + AH_t\,(r_n)P_t\,(r_n)$$

$$(P_t\,(r_1) + P_t\,(r_2) + \cdots\cdots + P_t\,(r_n) = 1)$$

Each probability could be calculated perhaps by applying BCOND normatively and retrospectively.[58] That is to say, for instance, this physician ought to have had or updated such subjective probability about such and such results given such and such data. In this respect, my idea seems to fit Bayesian theory very well.[59]

Finally, I propose a formulation of degrees of responsibility (DR) in terms of EH. Let (mah) denote maximum amount of harm in an absolute sense (like, perhaps, harm caused by a brutal murder), suppose that r_m, one of all rs, has actually happened in the end, then DR when an agent has performed act_i at time t in the past and r_m has actually happened can be formulated in this way:

$$DR(act_i \,\&\, r_m) = \frac{\{EH_t(act_i)AH(r_m)\}}{(mah)^2} \times \omega$$

ω is an *intention-weight* whose value can move between 0 and 1. $\omega = 0$ means that the agent is thought to have performed act_i mechanically without any choice, and $\omega = 1$ means that the agent is thought to have performed act_i fully intentionally (which could indicate in the case of DR that it was performed from a selfish motive or *mens rea*). This formulation is reached by thinking analogously to a logical conjunction. It is obvious that DR moves between 0 and 1, which enables this system to work steadily, irrespective of different values that are arbitrarily assigned to AHs. If an agent fully intentionally committed a brutal murder from a selfish motive with a completely certain method like decapitation, then EH_t (*this murder*) = (*mah*), AH(*this death*) = (*mah*), and $\omega = 1$, therefore DR (*this murder & this death*) = 1, which is the maximum. On the contrary, if an agent killed another person through a process that the agent could not physically avoid at all, then, no matter what value EH and AH have, DR (*this killing & this death*) = 0, which is the minimum, because $\omega = 0$. Perhaps in order to make this formulation more persuasive, we should consider how to deal with the case of an attempted action that wasn't accomplished, since the case seems to suggest that AH (*the attempt*) = 0, which would automatically lead to no responsibility. That is counterintuitive. My opinion is if we take into account harm (for example, fear or anxiety) caused by only attempted action when counting EH, then it would come to AH (*the attempt*) > 0.

Perhaps DR is neither equivalent to degrees of guilt nor straightforwardly contributive to the assessment of culpability. Questions of law including medical malpractice suits are judged by examining various other factors, such as an agent's ability to be responsible,[60] social or family circumstances that cause the agent to act in such and such a way, and so on. All that I hope is that my proposal about DR could be at least one device to sort out issues of responsibility at the starting point, probably

58. Hugh Mellor kindly read an early draft of this article and asked me a very insightful question: is expected harm no more than the inverse of expected utility (like, for example, loss of function)? That is to say, he wonders whether an idea of expected harm is conceptually different from that of expected utility. This is a very natural question. But as my argument might show, at least two differences between them can be noted. First, expected harm should be counted only in a *retrospective* way, while expected utility (or average loss corresponding to it) is usually assessed in a prospective way, as it is used for decision making about the future action. Second, utility is *not inversely proportional* to harm. As I said, positive utility is not equivalent to negative harm, because expected harm about positive utility should be zero altogether. However, this is true of loss function, for no matter how positively the products that reach the desired quality are evaluated, their loss is zero. But perhaps negative utility or loss might not always be corresponding to positive harm if an initial condition as a standard to assess utility is extremely fortunate. For example, if a very rich person whose income is one million pounds a month happens to reduce his income to three-quarter million pounds a month, we should say that no harm comes, although utility is assessed as negative, i.e., a loss, from his initial point of view. Thus, the concept of harm depends on such complex factors as norm or context in comparison with concepts of utility or loss which are relatively simple and objective to express numerically, so it seems that expected harm should be formulated independently of expected utility and loss function. I wish some suggestion would be given from experts as to how to refine this idea as a statistical formulation.

59. In fact, Bayesian theory has been already used in the context of jurisprudence, in which responsibility matters of course, in order to understand the relation between hypotheses about crime and DNA evidence. For example, see Dawid (2002).

60. In particular I think that the problem of criminal behaviour performed by mentally disabled people must be carefully scrutinized, since this problem is extremely difficult to place in the traditional philosophical context of debating freedom and responsibility. As to this perplexing problem, see, for example, Schopp (1991).

following Bayesian ideas. In summary, in any case, I have argued that philosophical problems of confirmation raised in the context of probabilistic strategies must be investigated through considering the decision-theoretic aspects, which suggested a peculiar way that could preserve Bayesian theory in a certain sense, that is to say, a decision-theoretic formulation of degrees of responsibility from a retrospective viewpoint, or a logic of responsibility. If that is the case, Bayesian theory would still remain powerful.

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Is a Decision-Theorist a Friend or Foe of a Bayesian-Theorist? Comments on Professor Ichinose's Paper

Daisuke Kachi

In his paper Professor Ichinose effectively described the ways of medical diagnosis and its confirmation, in which the processes of confirmation immediately affect the life of patients. Since doctors are responsible for the results, they have to make confirmations with maximum intensity and seriousness. Through his analysis Professor Ichinose has made it clear that we should take hypothesis-confirmation as a type of action that includes various decision-theoretic aspects in each step of confirmation. He insists that those aspects are not only seen in medical examples but also seen in other sciences, since every confirmation is a kind of speech act directed to other people. I totally agree with him in that respect. Moreover, he has reminded me that confirmation is a kind of incessant and critical action for surviving in a severe environment, which sometimes behaves unpredictably. He has succeeded in evoking the most fundamental features of confirmation that are easily forgotten in theoretical analysis.

Compared with his persuasiveness and clarity as to the importance of decision-theoretic aspects in confirmation, a little difficult to understand is how such aspects are related to Bayesian confirmation theory, especially to the problem of old evidence. He says that the problem of old evidence will be simply solved by adopting a decision-theoretic view of confirmation, because 'so-called old evidence appears not as genuinely old evidence but as, as it were, present evidence in connection with newly proposed relevance and newly questioned utility in the course of a decision made now'. Moreover, he insists that his way of solution is consistent with all three classical responses to this problem.

But each response more or less includes criticism of the other positions. For example, Williamson, who proposed a solution by noticing the non-monotonicity of evidence, showed that we would have to admit a