Hospitals without Doctors

The Science of Medicine vs. The Art of Medicine

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'76.4% of all statistics are meaningless.'
(Author Unknown)

The scope of this essay (DATED 03/JAN/2007) is to consider the possibility of writing a book of instructions constructed in such a detailed way that, by following these instructions only, non-doctors act better than doctors. This essay also contains a theoretical exemplification of a simplified version of my confirmation theory applied to the problems of health care.

Introduction

Not rarely do two senior doctors offer a different answer to the same problem.
In such a case, *at least* one of them is wrong.
Is there a *method* to determine who is wrong, or, at least, who operates better than the other?
This is the starting question of this work and it is a major concern for all of us, especially if we, or a dear person to us, are not healthy. In fact, for example, if we suffer from a cancer and we are "treated" by an ineffective homeopathic drug then we die. If this happens it is mainly because of the lack of method to prove the soundness of a therapy objectively. Thus methodology, and therewith philosophy of science, are of *vital* interest indeed.
This paper will try to show the usefulness of owning an *ideally-perfect methodology*, namely an ideally-perfect confirmation theory and an ideally-perfect decision theory, for contributing to solve the problems of medicine.
As an extreme consequence of this hypothetical situation, even the role of the human creativity and judgment would seem to be useless and erroneous respectively with regard to the following of a pre-made medical algorithm mechanically.
Implementation of this algorithm by machines will be considered too.
If such a radical prospective were ever possible then the presence of doctors by patients would be *at least* superfluous and so the 'disappearance of the sick man from medical cosmology' from the 1770-1870 medicine (Jewson, 1976) would convert to the 'disappearance of the doctor from medical cosmology' from future medicine.

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Ambitions of present medicine: Evidence-Based Medicine

> 'My students are dismayed when I say to them ''Half of what you are taught as medical students will in 10 years have been shown to be wrong. And the trouble is, none of your teachers knows which half''.'
> (Dr.Sydney Burdell, Dean of Harvard Medical School)

The English epidemiologist and statistician Austin Bradford Hill was the first to use a randomised trial in medical evaluations, even though the foundation of such technique was laid in the 1920s by the great English statistician Ronald Aylmer Fisher and applied to agriculture first. By using Fisher's ideas Bradford Hill was also the first to demonstrate a clear connection between smoking and lung cancer[2].

Archie Cochrane, a Scottish medical researcher, had Bradford Hill as a teacher and was deeply influenced by his lectures (Hill, 2000 and Armitage, 2003). Cochrane is probably most widely remembered for his contribution to the development of epidemiology as a science by calling for the establishment of a central international register of clinical trials and for his advocacy of randomized controlled trials (RCTs) at the beginning of the 1970s.

A RCT is a quantitative, comparative, controlled study in which people are randomly assigned between receiving two (or more) clinical interventions: one (or more) is the experimental and one is the control. The control intervention, which may be a standard practice, a placebo, or no intervention at all, that serves to compare of the *effectiveness* of the experimental intervention[3].

RCT is one of the simplest and most powerful tools in clinical research. For this reason, they're called the "gold standard".

Randomized controlled trials are carefully planned projects that study the effect of a therapy on real patients. They include methodologies that reduce the potential for bias (randomization and blinding) and that allow for comparison between experimental groups (which are subject to the experimental intervention) and control groups (which are subject to the experimental intervention).

RCTs played a fundamental role in what has become evidence-based medicine (EBM) nowadays, but the name and the full identity of EBM were laid around 1992.

Though Cochrane haven't lived long enough to see his eponym, the Cochrane Library, an international super accurate register, is approaching reality. This database is built by the Cochrane Collaboration, an international network of medically qualified volunteers

[2]Even though, in a strict technical sense, that study was not a RCT study, but a cohort study. Anyway they both share a similar statistical foundation.

[3]The outcome of comparison between the experimental intervention and the control intervention determines the *efficacy* of the experimental intervention.

who classify each article on clinical studies according to *strict methodological criteria* and prepare structured abstracts of them.

David Sacket, one of the main pioneers of this new movement in medicine defined EBM as 'the *integration* of *best research evidence* with *clinical expertise* and *patient values*'[4], but, however, as professor Brian Hurwitz told me directly[5], the way in which this integration should be carried out precisely is not established yet and is currently under investigation. I would define Evidence Based Medicine as the explicit ambition of applying the scientific method to medicine in order to standardize it. This standardization is thought to be carried out by constructing a huge database of well-validated systematic studies to apply to as many individual medical cases as possible.
As a practical social consequence, EBM requests the production of an expansive whole new system of medical research, that will increase the costs of health care in name of the hope of a near greater benefit.

Most of textbooks say that EBM has arisen from the need for valid information (about diagnosis, therapy, and prevention) in place of the inadequate traditional information (e.g., see Straus, Scott Richardson, Glasziou and Haynes, 2005). However, medicine has always wanted valid information and always thought to work by means of valid information. My view is that medicine has started wanting valid information explicitely since it has seen the availability of new better-valid information in place of the traditional one. The main reason of this is due to the use of a new methodology, especially RCTs, which could provide both new techniques and new strategies.
Nevertheless, EBM cannot be considered just an "old hat" of medicine with a new label (Sacket et al, 1996) because the medicine's new focus on EBM is a radical attempt at formalizing that "old hat" and at basing it on the "strongest" empirical evidence possible. "War on quackery" (Morrice, 2006) has always been present in medicine, but the actual war fights in name of the strongest appeal to evidence possible. The consequences of this choice are quite heavy, as it's been said in the American talk-show 'Health Outrage of the Week': '..... this would put 80 to 90 per cent of accepted medical procedures in this country under the heading of quackery!'[6] This is a substantial methodological difference.

The practice of EBM advocates that clinicians search the published literature to find answers to their clinical questions. There is a huge number of published reports, journal

[4] D. Sackett et al. *Evidence-based medicine: how to practice and teach EBM.* Churchill Livingstone, Edinburgh, 2000.

[5] *Ipse dixit*! He himself brought major contributions to the EBM.

[6] Source: from 1996 slides of CEBM. Beside it, also authoritative sources like the 1995 BMJ Editorial stated that 'only about 15% of medical interventions are supported by solid scientific evidence'.

articles, and studies available to clinicians. Systematic Reviews usually focus on a clinical topic and answer a specific question. Anyway, also choosing the best resource to search is an important decision to care for. The well-established large database MEDLINE is still the best starting point for EBM queries in general. For therapy questions, however, the Cochrane Library provides access to systematic reviews which help summarize and rank the results from a number of studies. The (UK-founded) Cochrane Library has edged ahead as it now contains more controlled trials than the (USA-founded) MEDLINE.

In 1976, Gene Glass, an American social scientist coined the term 'meta-analysis' to refer to an analysis that combines the results of several studies that address a set of related research hypotheses sing accepted statistical methodology as if they were from one large study; meta-analysis is 'the analysis of analyses' (Zhao, 1991). Clearly for what said before, current meta-analysis includes a critical appraisal of RCTs for analysis.

Probably the main guideline of EBM is that clinicians should consider meta-analysis at the top of the so-called "pyramid of evidence". The pyramid[7] serves as a further guideline to the hierarchy of evidence available.

In general, these guidelines of EBM are made by a group of experts and they consist in recommendations of actions with regard to diagnosis and therapy and suggestions on how a given disease should be cared for.

Unfortunately, often there is no meta-analysis or randomized controlled trial or "best evidence" in the literature to address clinical judgment. In that case clinicians should consider again the pyramid of evidence and look for the next best level of evidence made of other types of studies.

So, EBM is not restricted to randomised trials and meta-analyses, also because some questions about therapy cannot wait for the trials to be conducted. And if no RCT has been carried out for our patient's predicament, doctors must follow the trail to the next best evidence and work from there. Examples of lower-level evidence are the collections of reports on the treatment of individual patients. Because they are reports of cases and use no control groups with which to compare, they have no statistical validity.

As I have expressed before, I think EBM aims to make medicine more scientific, to put it "on firmer scientific grounds" and to apply scientific method to fundamental parts of medicine such as diagnosis, therapy, prognosis, etiology, prevention, clinical exam, and economic analysis. By scientific medicine I mainly mean a medicine endowed with empirically adequate rational theories in order to make good predictions. This is in contrast with artistic medicine which too can make predictions, but without rational theories.

[7] Which has the apparent problem of discontinuity in its gaps: see (Strauss, 2000).

David Sackett stated that EBM is not "cookbook" medicine (Sackett, 1996) because it is the expertise that decides whether the evidence of literature applies to the individual patient at all and, if so, how it should be integrated into a clinical decision, after having discussed results and opinions with the patient.

Nonetheless, my view is that EBM does aim to become "cookbook" medicine: as a matter of fact, it suggests that, in a scientific fashion, also clinical expertise must be based solely on the evidence, both the evidence from the particular patient and the evidence from the literature made out of random studies on population. Indeed the EBM mission is to provide all health professionals 'an accessible mechanism for obtaining objective, detailed information on clinical practice guidelines and to further their dissemination, implementation and use.'[8]

Therefore, if clinical decisions must be blended with individual clinical expertise, patient preferences and with the best evidence possible, then clinical expertise is not needed anymore for clinical decisions.

Then EBM would aim to become a mindless application of population studies to the treatment of the individual. Is this new line of medicine good? This is the investigation of this essay.

Moreover, doctors make mistakes frequently[9]. Then, is there a way to remove doctors from the health stage?

Can we make a write a book of so accurate instructions[10] such that even non-doctors cannot fail by following it literally?

The quest for the ultimate book of EBM

Hereafter I stay in the current context of the standard conception of modern state[11].

Every state is the institution which vests the authority to hold political power over the people of its territory.

[8]Source: National Guideline Clearinghouse: a public resource for evidence-based clinical practice guidelines built on the initiative of the U.S. Department of Health and Human Services.

[9]Just a few examples. 1)There is often great difficulty for doctors to get conduct effective searches to identify the best evidence and 'All too often clinicians are unaware of the available evidence or fail to apply it' (Guyatt, 2004). 2)I imagine that clinicians are reluctant to change their practice. If patients with the same disease provide alternative descriptions then doctors tend to discount unspecified possibilities. (Redelmeier, 1995). 3)Nowadays every member of the staff of hospitals doesn't receive an economic incentive to come up with the correct diagnosis (Leonhardt, 2006). 4)Clinicians bias their actions because of their personal lore. 5)Doctors often lack of confidence. ...

[10]It would correspond then to a huge unique algorithm with a lot of "if... then...".

[11]Following Max Weber's influential definition: a state has 'monopoly on the *legitimate* use of physical force within a given territory' (Stanford Encyclopedia of Philosophy). This context can be generalized, for instance also to tribes or to family matters, by passing from the concept of political power to the psychological concept of *social influence*.

So the person's state holds the political power over the person's health.

Then, how might a state write a "cookbook" for medicine that even layman could heal better than doctors just following passively its commands? Now on, for the sake of brevity, I will refer to such an "ultimate book of EBM" as "the book".

Let's then try to imagine that a future "scientific" state appoints an hypothetical office of national medicine ONM, a committee formed of the best physicians, in order to write the protocols which would be considered as "best" by its own state. A protocol is a strict step-by-step process by which a disease will be cared for (or 'papers that tell you what to do' (Greenhalg, 2001)).

Health professionals and not professionals should base their health decisions just following protocols. This however would not imply removing freedom from the patients: for example, a state could decide that the last person in charge to decide for the patient's health is the patient.

With regard to this issue, in fact, a European survey showed that 91% of respondents from Switzerland felt the patient should have a role in treatment decisions, while from Spain it was only 44% (Coulter and Ellins, 2006). This single instance is sufficient to justify the reasonableness of a national basis for the book, instead of an international one[12].

How would people get the protocols decided by the ONM?

By a PDA[13] permanently connected to internet wireless.

A lot of interactive medical PDAs are already available "on the market". Even more numerous are the web based systems for diagnosis such as Isabel or DiagnosisPro. For example, the latter instantly generates a hierarchical list of diagnoses from its database of over 11,000 diseases, 30,000 findings, and 300,000 relationships.

These computer systems are tools for decision support[14] and they that can incorporate reminders, directives, and all incentives, as well as audits, feedbacks, online surveys, communications to the ONM and so on and so forth.

Trough the internet connection, these PDAs represent also a way to speed up the research made by the ONM: in fact, by means of feedbacks (e.g. automatic full transmission of all medical cases analysed), the ONM can test if its "recipes" work and how much they work or what can be done better. Moreover the internet connection guarantees an opportunity to keep the PDAs updated by the new findings or corrections from the ONM.

A significant result strictly related to the problems of this essay is that the use of

[12] However, it would be useful that each ONM exploits the research from other states.

[13] A Personal Digital Assistant (an electronic handheld information device).

[14] Computer-aided detection and diagnosis are already an indispensable for some branches of medicine. For example, modern imaging techniques enable physicians to make evident information which is inherent in the image data but not evident to the observer.

computer assisting diagnoses has led to improvements (e.g. Johnston, 1994) and it justifies the involvement of artificial intelligence in medicine and the usefulness of evidence-based medicine. These devices improve patient safety and quality of care by augmenting providers' knowledge and cognitive skills in hospital and family practice.

Let's imagine now that this futuristic scenario will occur, then we can also imagine that, at a further future time, the ONM will not need to update the PDAs anymore because their results were well-satisfactory already.

In this context then, it possible to write on paper the algorithm used by the PDA and the possibility of the ultimate book of EBM, of *the* book, holds. Obviously, we cannot reject the possibility of a new unknown medical phenomenology, but in this case, the last page of the book would be about how to contact the ONM for "legislating" new *evidence-based* protocols.

But then, how to write the book?

For example, what should a disease be according to *every* ONM?

I'd define disease as an aspect of some (constructed) functions of the body or of the mind that is regarded to lower the expected utility[15] from the subjective or from the social point of view, an aspect whose cause is not directly manifest to our senses. A few examples:

A deterioration of our body from a meteorite (that hits us) is not regarded as a disease.

A deterioration of our body from polluted air is regarded as a disease.

A deterioration of our body from tear-gas is not regarded as a disease.

A deterioration of our body from a horse's kick is not regarded as a disease.

A deterioration of our body from invisible bacteria is regarded as a disease.

A deterioration of our body from yeast bacteria is not regarded as a disease.

Polluted air and invisible bacteria can become evident to our mind, but not to our senses. Having a temperature is a disease, because the subject sweats, feels hot and so on. That is regarded as a worse thing than the normality[16] and the cause of this is not manifest to our senses. Maybe the temperature arouse because the subject went to ski without any jacket? Well, so did her friend, but then, the necessary and sufficient cause of the temperature is not manifest: feeling *cold* doesn't cause a temperature necessarily. Its cause then is not manifest[17].

Then, in this definition I've distinguished between the subjective and the social point of

[15] Or 'pleasure' as I've called it in my own 'solipsistic' version of decision theory.

[16] That is, a decrease in expected utility.

[17] So unwanted pregnancies are not a disease, but they were in a past where no causal relation was understood to hold between the pregnancy and the (manifest) seminal liquid.

view. In fact, being anti-objectivist, I disagree with both Boorse's appeal to *dis*function and Cooper's condition of being a bad thing objectively: let's suppose we have a time machine and we bring a philosopher with pederastic interests from the Ancient Greece to Bristol. We'd mainly see him suffering from a disease, but he and his previous society would mainly disagree. Conversely, if we took a pederast who spent five years in jail and we sent him to Ancient Greece, he'd feel diseased, but not his new society. Other sexual practices are a typical example about how a same phenomenology varies its social judgment during the course of history and across the surface of the Earth. My definition does not demand potential medical treatability or misfortune (unlike in Cooper, 2000) either. For example, in chapter 9:8 - 9:12 of Exodus we find that God inflicted boils as 'sixth plague of Egypt'; no one could be healed, Moses knew that boils was not "potentially medically treatable" but he still reasonably considered the plague of boils as a disease, an *untreatable* deserved disease.

As another interesting instance, I offer the case of my sister. My sister felt a severe stomach ache since she was young. She tried several doctors, but each of them, after a long number of medical tests, stated that she didn't suffer from any "real" disease but it was just a (self-generated) psychosomatic effect. After approximately 20 years, development in medicine could find a rare virus that was living in her stomach and could finally kill it. Eventually the disease was accepted also by her society as a "real disease". According to my (constructivist) account, disease is a constructed entity that arises from a social request (see Moynihan, 2005) and not from a biological one: biology itself is a social activity that comes from a social interaction. Besides, mental diseases are "easier" to construct due to the trivial fact of literally not being capable to see what's (manifest) in people's head.

Finally, as for the technicality of the expected utility I go with the example of ageing: it is not considered a disease because the cost from a different option (struggling for arresting ageing) is higher yet than its probable benefit (useless effort). Nonetheless, in a wished future we will hopefully be able to stop the cell degeneracy currently-intrinsic in the cell duplication process and therefore the phenomenon of aging; we'll look at it as something to heal (on the same way it happened with tooth decay). Just then the cost from a different option (struggling for arresting ageing) will be lower than its probable benefit (eternal youth).

Now that the ONM can recognise a disease (a social one), it must look for its (non-manifest-to-senses) causes and for the best strategy to adopt in order to heal it. The ONM would need to estimate how much a theory of a disease works and how much a health theory of a therapy works and so on.

The ONM would need an algorithm to evaluate how much a theory is confirmed by evidence.

Ideally the ONM would need an universal confirmation machine for anything.

What meta-analysis should the ONM adopt?

A proposal of foundation of meta-analysis

Standard decision theories analyses decision-making using the concepts of utility and probability.

Utility is the balance of costs and benefits. The costs and benefits considered are of different type: in this context, mainly the costs will be in money and the benefits in public health. It is therefore pre-supposed a possible comparison on the same scale between different types of costs and benefits[18].

Then such theories proceed by formalizing the expected value rule and the utility maximization principle: carry out the action z so as to maximize the expected utility $EU(z)$ (Hacking, 2001 and Weirich, 2006). The expected utility is the sum of all the possible weighted according to their probability.

But so, how do we assess the value of probability correctly?

Despite the fact there are now some authoritative schools of thought on how to determine the probability of a theory by means of the available evidence[19], here I propose an simplified formulation of a confirmation theory I've personally developed and whose simplicity and intuitiveness can hardly be denied. I do so also for the purpose of rendering more intuitive my next arguments.

I will show its simple application[20] to an easy example. Remarkably, the derivation of confirmation of my approach is indeed based solely on the evidence (apart from the human creative construction of new theories to test and in spite of the lack of any prior probabilities of any hypothesis[21]). After having devised possible theories we let the evidence alone judge how good they are.

[18] In this respect I regard the general formulation of my own decision theory, outlined in *Pleasure Theory*, to have the quality also to clarify on what scale the concept of utility is intrinsically reduceable. I claim (but it's not difficult to verify) that standard decision theories are a simplification of Pleasure Theory in the approximation that a group of individuals (as the ONM) can be seen as a single individual; however I will list my criticisms and weak points to theories of decisions on a suitable paper.

[19] Bayesianism is the most popular among philosophers, but for some criticism to it see my *Sixteen Arguments against Bayesianism*.

[20] But with no loss of soundness for example at hand. For the general formulation see *Explaining a Simple Physical World*.

[21] In spite of Bayesianism, which intrinsically needs prior probabilities, my confirmation theory works also in the absence of any prior probabilities of any hypothesis (for the relevance of this to medicine see for example Frankel, 2003).

As an evidence we take the (discrete) two-data sequence $\mathcal{E}$[22]

$\mathcal{E} = \square, \blacksquare, \square, \blacksquare$

For this example let's also suppose we have constructed four candidate theories to explain this evidence: a theory of intermittence $\mathcal{T}_I$ (according to which there is always white and black alternately), a theory of coin $\mathcal{T}_C$ (according to which the colors are given by the tossing of a(n unbiased) coin), a theory of black world $\mathcal{T}_B$ (according to which all the colors are black only) and a theory of white world $\mathcal{T}_W$ (according to which all the colors are white only).

The idea now is to consider as a verification[23] of theory by evidence its binomial probability U, i.e. the probability that the hypothetical theory creates an outcome with the same frequencies of the evidence[24], multiplied by the total number of verifiable cases of the theory by evidence T, that weighs the significancy of the previous quantity U[25].

$$V = U \cdot T \tag{1}$$

It's worth to observe that $V \neq T$! Reasonably, if we consider our four theories, they are verified in a different way even if their number of verifiable cases is the same.
In fact, in this example we get $V_{\mathcal{T}_I} = 4$, $V_{\mathcal{T}_C} = 1.5$, $V_{\mathcal{T}_B} = 0$ and $V_{\mathcal{T}_W} = 0$.

Then, I find quite natural to define as $\Pi_{\mathcal{T}_i}$, the probability of the theory i, its verifications divided by (or, relatively to) the total of verifications of all theories[26].

$$\Pi_{\mathcal{T}_i} = \frac{V_{\mathcal{T}_i}}{\sum_j V_{\mathcal{T}_j}} \tag{2}$$

In our case

$$\Pi_{\mathcal{T}_I} = \frac{V_{\mathcal{T}_I}}{V_{\mathcal{T}_I} + V_{\mathcal{T}_C} + V_{\mathcal{T}_B} + V_{\mathcal{T}_W}}$$

thus

$$\Pi_{\mathcal{T}_I} = \frac{4}{4 + 1.5 + 0 + 0} = 0.\overline{72}$$

If within the concept of theory we comprise also the cause that creates the phenomenology, then we obtain that when a theory is confirmed then also its cause is confirmed[27].

[22] 'Evidence' derives from Latin 'evidens', which means 'visible, evident', as it is for this example with colors.

[23] Verification and confirmation can sometimes be meant as synonyms. I prefer verification for I want to determine how many verifications a theory has had, how many times the theory has been verified by the evidence.

[24] If we consider more than 2 colors, instead of the *bi*nomial probability, there will be the *multi*nomial probability.

[25] An equivalent reading is to consider a theory to be verified V times by the evidence whereas V is the number of T cases of evidence multiplied by the likelihood U of the evidence conditionally to that theory, that weighs the significancy of the previous quantity T.

[26] This confirmation theory works without any difference for theories of "strictly-probabilistic" nature. A philosophical-mathematical property will be that the theories, that have a theoretical frequency on the same evidence equal to the theoretical one, will be the most verified ones.

[27] In respect to medicine, this can be useful also in the important problem of etiology.

Since we may see the law of intermittence as the cause of the theory, then we get that the probability of that cause is $0.\overline{72}$!

Often we start seeing a cause just as an accidental relation and not as the real cause. The line of thought about causality implied by this confirmation theory entails that the reason of that is just due to other models (with other causes) that have become more general, *more verified* (and so, other causes have become more real).

If now we indicate as $\Pi_{\square\mathcal{T}_i} = 1$ the probability of the next future color to be white according to the theory $\mathcal{T}_i$, then we have $\Pi_{\square\mathcal{T}_I} = 1$, $\Pi_{\square\mathcal{T}_C} = \frac{1}{2}$, $\Pi_{\square\mathcal{T}_B} = 0$ and $\Pi_{\square\mathcal{T}_W} = 1$. So, here, the natural idea is to consider all theoretical probabilities and to *weigh them according to how much their corresponding theories are verified*:

$$\Pi_{\square} = \frac{\sum_i \Pi_{\square\mathcal{T}_i} \cdot V_{\mathcal{T}_i}}{\sum_j V_{\mathcal{T}_j}} \tag{3}$$

In[28] our case

$$\Pi_{\square} = \frac{\Pi_{\square\mathcal{T}_I} \cdot V_{\mathcal{T}_I} + \Pi_{\square\mathcal{T}_C} \cdot V_{\mathcal{T}_C} + \Pi_{\square\mathcal{T}_B} \cdot V_{\mathcal{T}_B} + \Pi_{\square\mathcal{T}_W} \cdot V_{\mathcal{T}_W}}{V_{\mathcal{T}_I} + V_{\mathcal{T}_C} + V_{\mathcal{T}_B} + V_{\mathcal{T}_W}}$$

thus

$$\Pi_{\square} = \frac{1 \cdot 4 + \frac{1}{2} \cdot 1.5 + 0 \cdot 0 + 1 \cdot 0}{4 + 1.5 + 0 + 0} = 0.7\overline{72}$$

These results of this example have been found with theories whose single "color distribution" elementarily applies to *all* cases determining the evidence. Nevertheless the general formulation encompasses the use of theories made of several "elementary sub-theories" each of them is made of a single "color distribution" that applies to some cases determined "by an elementary way"[29]. In the general case, the theory will be verified (more than 0) whenever all its elementary sub-theories will be verified (more than 0); if this happens, the verifications of the theory will be equal to the sum of the verifications of the elementary sub-theories.

From this, formalizing how to determine the confirmation of any theory by any evidence is quite straightforward.

Integration of evidence with expertise and ethics

[28] If we introduce formula (2) in the last one we get the other important equivalent formulation

$$\Pi_{\square} = \sum_i \Pi_{\square\mathcal{T}_i} \cdot \Pi_{\mathcal{T}_i} \tag{4}$$

[29] An example of non-elementary way is: black in the even cases whenever the sequence is shorter than ten cases, black in the odd cases, whenever the sequence is longer than ten cases. This non-elementary way of assigning black is formed by two elementary ways. Verification of only one of the two is not verification for the two altogether. The concept of elementary, though, is a pragmatic issue ultimately.

The above-sketched idea of decision theory stands in the context of a consequentialist thesis. Protocols of the book are written according that thesis. Therefore, the validity of the book *cannot* be disproved by showing of a wrongly-predicted consequence: in that case a modification of the book is necessary according to a natural process of knowledge revision. The existence of a counter-example, then, is not contrary to the book, but just virtuous: it is a source of improvement for the book.

For instance, if there is a mortal disease that affected just a couple of persons of the state, whose disease is unlikely to spread in the population, and there isn't too much money for the public health, then no research will be carried out for that rare case. *But* if, however, the state *starts* having a special sensibility for such minority, or the respective research shows to be very cheap (and so on) *then* the book will have to care for that rare disease.

I put forth two definitions:

Something is valid when is highly probable. So, according to the previous confirmation theory, it depends on how much it is verified.

Something is important when is associated to high utility. So, what is important depends also on how much it costs and benefits, according to the available resources, to the value system at hand and to how better the people feel in general.

Meta-analysis deals with how to do the best integration among evidence, expertise, ethics and other factors.

Being best, for an integration, is matter of being a "mixture" of valid and important.

My proprosal of meta-analysis would simply consist in the *maximization of the expected utility from the ONM* with the probabilities assessed according to the confirmation theory above.

Instead, according to the current state-of-the-art of meta-analysis in medicine, one of the most accepted criteria for assigning "grade" of evidence is the "grade approach" (Atkins et al, 2004), which, in comparison to my proposal, appears to be approximative, obscure, conventional, i.e. without a clear foundation.

Let's make an example about diagnosis: my formulas (2) and (3) represent respectively the probability of a certain theory (or for a certain phenomenology) that is derived by the process of weighing the probability of one disease versus that of other diseases to account for a patient's condition. That is exactly the definition of the very fashionable *differential diagnosis*[30] which, however, doesn't have any clear foundation either[31]. The differential

[30]Source: Medical Dictionary.

[31]On the other hand I must acknowledge, at least, the account of Bayesian networks. Even if I have a lot of specific points against that account, even the most stubborn Bayesian couldn't easily deny that my account is less clear than his. 'Nevertheless, it seems that quantitative [Bayesian] reasoning is neither intuitive nor well understood.' (Bianchi and Alexander, 2006) Besides, I cite just one paradoxical point:

consists of the hypothesis found that have the highest probability, however, in my account we should not discard all other hypotheses until we don't have sufficient data to "prove it automatically" (the probability of a certain hypothesis approaches zero).
Nevertheless, differential diagnosis is not enough for clinical decision making: some therapy, for instant, may be too expansive. Then we must recover our decision theory in order to have the help of meta-analysis.

Let's suppose that we don't have economical or ethical problems and that we want just to use the techniques in the proportion of how valid they are. That means considering only the confirmation theory and not the decision theory.
Then, according to the current state-of-the-art of statistical medicine, we should use the techniques of the RCTs because it works more, it's verified more than other techniques. I suspect this last sentence has sounded so natural that it hasn't stand out: the revolutionary breakthrough now is that there is a coherent, intuitive and formal theory to clearly structurize the concept of degree of confirmation and to precisely quantify the overall weight that a theory has. The validity of a theory is *a posteriori*, but it's provided by an *a priori* theory of confirmation.

Now I would like to do a brief parenthesis over RCTs.
RCTs are nowadays considered as the only untouchable gold standard. Are they really so?
Beside several authoritative criticism to this point (e.g. see Worrall, 2002 and Lilford, 2003) I want to cast my doubts that have arisen from a personal experience I had in Bristol! As it is understandable to be, my interest for being part of a RCT study was also for the money they'd have paid to me. The study was on the effect of a new drug on anxiety induced by CO_2. After spending not little time for preparing the papers I needed to enter in the study, I was tested with the CO_2. I've answered in a completely sincere way to their questions, and finally they didn't take me because I didn't become anxious enough. Now, I think that other people "smarter" than me over-reacted[32] to the CO_2 in order to do the test and get the money. This is clearly against a genuine determination of the efficacy of the study, but no tool could prevent it. Therefore I largely admit the possibility of a perfectioning of the techniques of statistical medicine. When this will happen, then, new techniques will be more perfect just because they will be verified more. Consequently, (if we had an ideally-perfect methodology) we would know precisely also how much we should

in Bayesianism, a prior probability that was informally inferred by 2 cases, formally counts as much as a prior probability inferred from 2 millions of cases.

[32]There is some analogy with the case of those students who confirmed some mice were more intelligent than those they were taught to be more stupid.

use the new means in spite of the old ones. Another doubt is the fact that the conditions of a RCT does not reflect the conditions of the people who do use of a treatment: the group of a RCT (test one plus control one) knows that they have a 50% probability of being given an ineffective treatment. I think that also this factor can slightly alter the outcomes with respect to a "neutral" experiment, but maybe just rendering the efficacy of the experimental intervention to hold *a fortiori* (see homeopathy...)

I want to show another example of the importance of owing an our "universal confirmation machine" (applied to medicine). Continuously, medicine must answer questions like:
'Did the study patients fully represent those who present with this clinical problem?'
'Is the diagnostic test available, affordable, accurate, and precise in the clinical setting?'
'Are the study patients similar to the patient at hand?'
'Have the disease probabilities changed since the evidence was gathered?'
'Would the patient be a willing partner in carrying it out?'
'Would the consequences of the test help the patient in his goal?'
'Is the evidence valid or important?'
'Were all important therapeutic alternatives and outcomes included?'
'Are the probabilities and the utilities of the outcomes valid and credible?'
'Can the patient state his utility in a stable realistic form?'
'Do the probabilities apply to the patient?'
'Is this evidence from an economic analysis valid?'
'Can we apply the valid important results of the economic analysis to the patient?'
'Are the recommendations in the guideline valid (e.g. well-tagged)?'
'Was the selection of the partecipants explicit and appropriate?'

Single answer: put all the evidence you have in the "universal confirmation machine" and let it to grind the response. If the evidence you have is not enough then look for other evidence (if it is "allowed by the decision theory").

Such an ideally-perfect account would clearly rest on a *sola experientia* attitude[33], or better considering the present case, it would be a *sola scriptura* attitude for the users of the book ruled by the *sola experientia* attitude of the ONM[34].
Nonetheless, it will *never* be possible to arrive to such an ideally-perfect account: we should have considered all possibilities (which clearly is not possible). But, the "introduc-

[33] B. van Fraassen. *The Empirical Stance*. Yale University Press, New Haven, 2002.
[34] But also the *sola scriptura* attitude of the users of the book could be thought as a *sola experientia* attitude for it intentionally relies on the latter.

tion" of new unthought-of theories in the "universal confirmation machine" will perfect the adequacy with respect the phenomenology more and more.

Two examples for medicine. Firstly, research has shown that individuals from the same state, but with different genetics respond differently to active principles (e.g., alcohol: Luczak et al, 2002 and 2004).

Secondly, if we have a person that suffers from a disease, and eating pork is validly efficacious with respect to that disease, still we are (hopefully) likely to think it's important to not force that treatment if that person is Muslim. And also suggesting to drink some wine (with alcohol), in order to prevent diseases, is not suggested in this case.

So, "good" ONMs are likely to differentiate their treatments also according to the *ethnicity* and *culture* beside age, sex, diet and so on. But not all differences count with the same weight.

There is a lot of other ethical issues that are more controversial. For example, in (Reili, 2002), an old woman feels poisoned by the heavy drugs suggested by EBM guidelines. She wants to live well for the few last years of her life rather than living long but feeling poisoned all the time. What should the doctor do? The ONM should decide for such cases, as well as for euthanasia, abortion, etc. These are also legal issues (rightly, I think), people die or suffer, the state has responsability to judge according to its own values. Doctors should be removed from the responsability of death or sufference if they act in line to what their state decided it's good to do.

Homeopathy,and complementary medicines in general, too is another "hot" controversial topic. If people feel better with placebo[35], shouldn't be right to let the people feel better? Does it really matter if homeopathic pills don't work for animals? (We are interested in improving health of humans, not of animals!) Or, maybe, ONM thinks it's better to discourage people from the spread of non-scientifically-founded beliefs? Repeating myself, I think each ONM of every state, with its own values should choose the decision most suitable to it.

Values are not the only thing to consider however: 'There is a risk 1/200,000 of a certain type of cancer. Do we prescribe the test of *prevention* as compulsory for everyone?' This is matter also of national economic budget and acceptance from the people of the state and so on. In general, it's just matter of expected utility. However, informing the relatives and friends of patients who suffer from cancer correctly, in order to know how to behave for containing the cancer-related fatigue, is often not carried out properly. Little justification could be found for this.

[35]RCTs have indirectly but extensively shown that placebo has a large effect on the health of people.

Therefore, protocols could work also as a tool to fight malpractice.

But in general, how to separate malpractice from clinical expertise[36]?

In a realistic situation, in fact, the "book with the right answer to every medical question" is not completed with absolute certainty, so, how to integrate the probabilities (or distributions of probabilities) told by a medical expert?

Answer: in exactly the same way as a probability told by a theory. The medical expert at hand, in fact, could always make "oracular" true predictions[37]; but when can we believe that the so-called oracular expert is oracular indeed? Again, only after verifications. Thus, each registered doctor will be automatically given a record of his/her probability to act successfully as their actions are automatically transmitted to the ONM by PDA.

Moreover, not all protocols work in the same way: some can have a very high probability (relatively-many verifications) to succeed while others can have a very low probability (relatively few verifications).

Therefore, for every medical case, there will be a meta-analysis, an integration between evidence and expertise. As an expected result, in cases where there is little evidence from literature, then the expert will have more freedom to act according to his/her *know-how* (if he/she is a good doctor: relatively-many verifications for that medical case).

Henceforth malpractice will be when a doctor does not follow a protocol that has a very high probability to work (well according to the ONM, including ONM's value system).

Let's go now back to the main investigation of this essay, toward a conclusion.

Conclusion

As just said in the last paragraph, if it's possible that there are experts that work better than the book? Why not taking those experts to codify the book?

Yes, it would be a good move to do indeed, but that doesn't assure the same success of the expert. Why?

For what it could suggested by the distinctions between art and science, between 'what is known and what is done'[38], between the Russellian knowledge by acquaintance and knowledge by description, between *know-how* and *know-that*.

As earlier I defined the distinction between scientific and artististic in being (or not) endowed by a rational theory, then it follows that what is artistic can be hardly codified.

[36] By clinical expertise I mean the proficiency and judgment that individual clinicians *acquire through* clinical experience and clinical practice.

[37] Besides, the knowledge of how predictions ought to be made when evidential data are available to rational elaboration, then, can ideally suggest what counts when we make introspections for producing good predictions when evidential data are not available to rational elaboration.

[38] Source: from 2001 slides of CEBM.

For example, every state should appoint an excellent surgeon as a member of the ONM, but that doesn't assure that his know-how can be perfectly codified in know-that. Furthermore, it's desiderable that specific psychologists be appointed to codify the artistic medical experts' know-how in know-that.

This, yet, seems to be a massive problem: staying on the example of surgery, we'd have that the excellent surgeon's procedural knowledge is hardly convertible in propositional knowledge. But finally, will that scientific procedure–laid down by the algorithm and used by a layman–beat the artistic procedure of a surgeon?

I think that now we have to take into account a fundamental factor: the *time*[39]. The completion of such a conversion from the surgeon's actions to their writing in the book would require a lot of time and a lot of pages of the book at least.

So, any user of the book would take a time that is going to be a too long *time* for the desired purposed: e.g. if we have a patient under anaesthesia with organs out of his/her body waiting to be replaced, then the patient will surely die if the layman is not fast enough.

Another aspect may come from the integration with the patient values: they are always contextually depending on the particular psychological state of the patient and on the clinical setting. If we wish to codify all possible psychological states and clinical settings, then it's reasonable to take a huge number of pages (whether it be possible ever). Again, the layman would take a too long *time* to turn over the pages of the book in order to act correctly.

However we could avoid the problem of infinite time: we could be sufficiently satisfacted already within a certain finite precision.

Nonetheless, still, we can imagine that a layman would take longer time than a surgeon to act in a sufficiently satisfactory way.

On the other hand, surgery is a particularly difficult branch of medicine to codify and we could think, however, that in some specific cases a layman might beat[40] a doctor by means of the book. Things work even much better if instead of the book we give the layman a PDA.

In fact, it's worth to mention that 'to keep up to date in Internal Medicine, doctors need to read 17 articles a day, 365 days a year'[41].

So a layman assisted by a PDA is likely to proceed with a much better support of evidence

[39] Hippocrates, the father of medicine, said 'Life is short, science is long; opportunity is elusive, experiment is dangerous, judgment is difficult.'

[40] See the analogous present case in chessing.

[41] Source: from 2001 slides of CEBM.

about "the right thing to do".

In conclusion, I think that the best system between doctors and non-doctors with a PDA is the "hybrid system" of doctors with a PDA. On average, it's more likely to think that a doctor with a PDA would act better than a layman with a PDA.

As it's already been established, in this way EBM is not to 'be conducted only from "ivory towers and armchairs"' (Sackett, 1996): there are a lot of studies that have finally showed that EBM can be normally used by "busy clinicians" with a great help from it.

Nonetheless, my thesis is that EBM is impossible to practise by doctors *perfectly* (in which case we could remove them from the health stage).

And, again, who will the ultimate judge for the match between the science of medicine vs. the art of medicine be finally? The ideally-perfect methodology. Just that will allow to quantify exactly what works better.

Doctors make mistakes, but laymen make mistakes too and doctors have greater probability to mistake less.

The science of medicine can just help the art of medicine, but can't beat it.

Futuristic opposite conclusion

This match, science of medicine versus art of medicine hasn't sided with the former due to limitations of human mind, namely its times.
However, instead of thinking about a "computer-assisted layman" it's possible to think about a "computer-assisted machine", namely a sort of hypothetical *robotic doctor* managed by a quantum computer that would not face such intrinsic slowness and fallibility of the human mind.

Such a robotic doctor could even act as a surgeon or as a psychologist[42].
I imagine that our step in trusting a robotic doctor like a human doctor[43] will be something similar to the past step in trusting the travel by air like the travel by land.

This "science-fiction solution" to EBM, or to medicine *simpliciter* is not more futuristic than an hypothetical end for the accumulation of medical scientific knowledge (the ultimate book of EBM) and it's actually expectable to happen within a few centuries: at that time, it's not so insane to think that machines of the potentialities of the robotic doctor will be available.

In this prospect, *yes*, the science of medicine will win over the art of medicine.

[42] Although being a psychologist without consciousness!

[43] Trust in a human doctor, and the doctor-patient relationship, are a fundamental part of the healing process, specially with regard to psychological medicines.

Now the Jewson's 'disappearance of the sick man from medical cosmology' would finally convert to the 'disappearance of the human doctor from medical cosmology and its substituition by the robotic doctor'[44].

Human doctors make mistakes, but we can program a robotic doctor to mistake less.

The science of medicine can beat the art of medicine.

[44]Therefore we should also convert the title of this paper to 'Hospitals without Human Doctors'.

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