

## Reflections on *Philosophy for physicians*, a text by Mario Bunge

Reflexiones a raíz de *Filosofía para médicos*,  
un texto de Mario Bunge

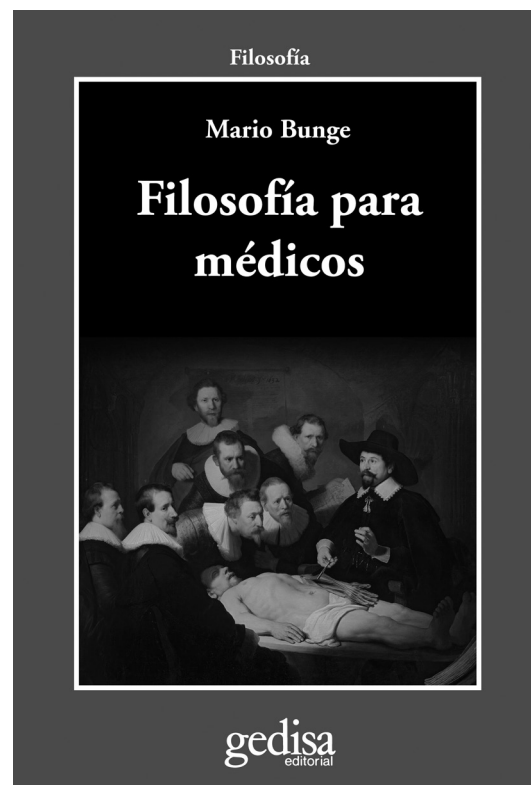
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I write this review to reflect on the contents of the book *Philosophy for physicians* (1) written by the renowned Argentine epistemologist Mario Bunge. The book was recently published in Spain and gave rise to various conferences and interviews in December 2012. This text is not a classic recension, but a set of opinions on the ideas the book covers or suggests. Due to the wide range of topics the book addresses, and the controversial nature of many of them, the material is of great relevance to public health professionals.

From my point of view, the material offers a great deal of insight, but also casts some shadows that should be critically analyzed. Such an analysis corresponds with the work's invitation for discussion: "in the sciences, doubt is better than the firm belief in dogmas" (1 p.91). It is therefore not my aim to give a thorough review of the book. While this text is rather extensive, it is not because there are many points with which I disagree, but rather because the justification of the points with which I dissent calls for an unhurried and responsible reflection, an examination of the academic precedents so as to heed the warning of Bunge himself that certain destructive criticisms can be irresponsible (1 p.169).

*Philosophy for physicians* is a book that sheds light on transcendental topics, debunks myths and reminds us of many truths that all health care professionals, physicians in particular, would



greatly benefit from knowing. Sadly, however, it contains confusing messages, suffers from some misconceptions and contains a number of mistakes. The present text highlights the merits, but does not avoid the examination of its fundamental

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problems. I hope this review can serve as a contribution to a topic of notable importance.

## BUNGE'S STRONG POINTS

Among the most prominent merits of the work, we can find the expository eloquence and the simplicity with which the author communicates relatively complex ideas. Bunge displays an elegant and clear language that makes the reading more pleasant while at the same time conveying his vast knowledge through precise and appropriate examples and through a considerable amount of historical information and invaluable philosophical and methodological details.

The clear, flawless criticism to which he submits the pseudosciences deserves special praise. Although it is something that Mario Bunge has addressed in many of his previous works, his observations about false panaceas and his denouncement of fraudulent therapies such as homeopathy and folk medicine are established as solid and grounded acts of exposing medical practices alien to science. They also reveal the risks to which public health praxis is exposed when those involved ignore the importance of submitting their convictions to the judgment those procedures merit from a philosophical perspective and within an appropriate methodological framework.

Ultimately, Bunge highlights, time and time again, that nothing surpasses verdicts derived from objective experience, and especially from clinical trials in those areas in which they are permitted. A particularly suggestive proposal of his is to consider them not only as "gold standard" tests – something with which few people disagree – but also as a necessary condition to reach a higher stage in terms of resources for the generation of new medical knowledge (1 p.40). I am referring to what he calls the "platinum test," which is given when the arguments, apart from being supported by the gold standard test, are supported by explanations of the mechanisms that allowed the standard to be bettered.

The understanding of the role of the placebo effect, as well as the fundamental importance of the theoretical plausibility of the hypotheses, the fallacies derived from the transgression of logical

thought and the necessity of finding, to the extent possible, the causal mechanisms of the processes associated with health and illness, keeping in mind the systemic nature that defines them, are leitmotifs throughout the work.

His treatment of the social framework in which public health practice should be placed deserves special mention. Although the comprehension of the deeply social nature of the physician's practice has long since been recognized and has been studied by prestigious figures especially since the 1950s, Bunge highlights it in an opportune manner. With a noticeable capacity for synthesis, the author establishes a difference between legality and morality and highlights the conflict that can arise between the rightful desire for personal well-being in the case of a health care professional, or for revenue in the case of a company that operates in the health field, and the commitment that each must make to truth and to the people they supposedly are to benefit.

## SOME SPECIFIC MISTAKES

Some of the statements made in the book are simply erroneous. They may not have an intrinsic importance, but in light of the biting tone of the statements that make up the book, they are at the very least irritating. Let us see some examples.

The book states that the diagnostic hypothesis implicit in the syllogism "if the patient shows the sign or target indicator S, then it is possible that he/she has the disorder E" is based in biomedical studies, "especially in clinical trials" (1 p.16). This is incorrect, because experiments in humans are not used to identify target indicators typical of an illness, but to evaluate the effects of possible medical and public health interventions, as Bunge himself clarifies: "The central aim of clinical trials is to identify the real effects caused by the treatment being tried" (1 p.131). The elements used for the diagnosis can be the causes of the illness (like the presence of a virus in the development of dengue) or manifestations derived from them (like fever or itching skin from that same ailment). Causal elements are excluded from study for ethical reasons, since it is not acceptable for them to be induced in an experimental group in order to register

whether the occurrence of the illness is greater in one group than in another that was not the object of such intervention. The manifestations following the occurrence of the ailment cannot be evaluated by this method either, because clinical trials are, necessarily, prospective: it is not possible to create groups with and without said manifestations to examine the occurrence of the disorder in them, which occurs before.

In the diagnostic sphere itself, according to the book, if a refractory cough is observed, tuberculosis may be suspected, and x-rays or a blood test will allow the mechanism to be confirmed or denied practically in a conclusive way. And the author adds: "A cause has been found, which is, at the same time, the necessary and sufficient condition for the effect or sign in question to occur" (1 p.88). What mechanism is he referring to? What is the necessary and sufficient condition for refractory cough to occur? The mentioned diagnostic methods could have revealed the presence of Koch's bacillus, for example, but this is not a necessary condition for the appearance of the cough (many other agents can produce it) nor it is a sufficient condition, since not all bacillus carriers suffer from cough.

In other parts of the book, Bunge offers other erroneous examples related to the categories of necessity and sufficiency. He mentions that child malnutrition is a necessary and sufficient condition for delay in their development (1 p.143); but the reality is that such a condition is not necessary (malnutrition can appear as a consequence of insufficient intake in a child whose development has been perfectly normal) nor is it sufficient (children can have delays in psychomotor or even physical development due to neurological problems that have nothing to do with their nutrition). Something similar occurs with dehydration and death. Dehydration is not a necessary and sufficient cause for death, as it is affirmed. It is obvious that one can die without having suffered from dehydration, as is the case with a car accident or an opiate overdose.

Finally, a small detail, perhaps owing to an oversight: the statement that "only two outcomes are possible for any disease: recovery or death," (1 p.163) is considered one of the big discoveries of the Hippocratic physician. Since this assertion is not questioned, Bunge is, in fact, validating it. This is, of course, not true. Many people never recover

from an illness (for example, someone that suffers from retinitis pigmentosa, psoriasis or arthritis), but that disease does not lead to their death, either.

## SUPERFICIAL CRITICISMS

Generally, Bunge is extremely biting, concise and conclusive. He rarely – perhaps never – uses expressions like "in my opinion", "from my point of view", "I think that." It is his style. Sometimes, his works lack references to opinions that diverge from his point of view and that could or should be considered. But, generally, when we examine a text, we should take into account only what it is said in the text, without blaming the author for what he or she omits (a).

Now, within that style, the text presents global criticisms that quickly minimize the work of incredibly important and influential thinkers in the field of philosophy and medical thought. In this sense, the work aligns, at times, with the current tendency of the trivialization culture (2) (in this case, its scientific and philosophical aspects), making simplistic concessions that are surprising in a work with the scope and rigorosity demanded by the topic.

For example, in Bunge's opinion, "physicians philosophize all day" (1 p.13), while neither Friedrich Nietzsche nor Martin Heidegger were philosophers, but mere "paraphilosophical writers" who "by ranting against rationality and morality" made a single contribution: to Nazism (1 p.123). The least we can say is that this categorical sentence is not serious and that it transmits a ridiculous and fragmented image of both intellectuals. While both thinkers awaken reservations in the political field, because the ideas of the former (who died 30 years before the birth of Nazism) were used by the National Socialist regime, and because the latter was evasive in criticizing the regime, which also "used" part of his theories, they are extraordinarily important thinkers whose strictly philosophical work is enormous and, as a result, studied by the most important specialists the world over.

I will not go on with details that are somewhat beside the point, but it is enough to say that some of Nietzsche's ideas (as well as his poetry and philological work) are among the most notable and

suggestive of the 19<sup>th</sup> century, while the work of Heidegger is simply monumental, and left its mark in the later philosophical undertakings not only of German and Anglo-Saxon intellectuals, but also of fundamental philosophers of Latin Europe, like Jean-Paul Sartre, Miguel de Unamuno and José Ortega y Gasset.

I am not an expert in psychology and much less in psychoanalysis. However, I strongly suspect that Bunge is right when he points out some apparently ridiculous aspects of the psychoanalytical theory originally described by Sigmund Freud and later developed – and utilized – by his followers. Indeed, Karl Popper observed that Freud's theories were not falsifiable, which allowed the data to be reinterpreted to uphold the hypotheses even if they did not have empirical support.

However, there is a big difference between making this observation and belittling Freud's entire work. Bunge may be sure that this researcher and thinker was "the worst psychologist of the century" (1 p.91), but this type of statement gives the impression that he is more given to emotion than to a paused examination of history and reality. In Bunge's opinion, those who were taken in by Freud were either ignorant or looking to make a profit. I have the impression that things are not that simple. For example, I do not think that Clark University of Worcester, Massachusetts awarded Freud Doctor Honoris Causa (1930) for those reasons. Of course, all of this is a matter of opinion, and Bunge's opinion is very respectable. However, to affirm that the Freudian *cobwebs* were swept away by a "storm of psychotropic drugs, which replaced the psychoanalytic myths" contains a fallacy.

Although it is to a large extent correct that the Freudian myths have been *swept away* – despite the fact that his thought is still studied in many universities and schools of philosophy, even if only to criticize it, and that his ideas survive to some extent at least in the "Lacanian" circles – the problem lies in attributing that merit to drugs without a systematic reflection from a scientific perspective, considering most especially the contributions of brilliant intellectuals and thinkers over the last century and even recently, as is the case of Michel Onfray (3).

No storm of psychotropic drugs has swept away, for example, the cognitive-behavioral

orientation of psychotherapy, dominant today. We are facing a manifestation of the confusion regarding psychotropic drugs, some expressions of which have been harshly questioned at the same time that millions of people have been tricked by the sophistry deriving from the avarice of their promoters (4). This uncritical fascination increments when its victims are irresponsibly misinformed by physicians (5), or when the prescribers themselves are manipulated to the point that they start to believe that drugs are much more effective than they really are, giving way to a massive over-treatment with antidepressant and antipsychotic drugs, as Peter Gøtzsche recently documented in an article appearing in the prestigious journal *Trials* (6).

We owe a lot to psychotropic drugs, but I am afraid that the "storm" of psychotropic drugs, particularly of the most famous drugs – selective inhibitors of serotonin recapture (such as fluoxetine and paroxetine) (7) – has been more harmful than Freud. As has been thoroughly documented (8), not only is there no corroboration whatsoever that low levels of serotonin produce mental disorders, but there is in fact significant evidence against this hypothesis, in addition to the other serious collateral damage produced by these drugs (9).

Bunge is more reticent to criticize the Austro-British philosopher Karl Raimund Popper (1902-1994), who became a fundamental thinker after being accepted as part of the philosophical elite of his time (the so-called Frankfurt School) and later (in the 1950s and the 1960s) as part of the famous seminars of the London School of Economics. Popper's central idea can be summarized in that science evolves through refutations and not through affirmations, and his argument is that a scientific idea can never be deemed true, because despite the many observations that ratify it, an idea can always be incorrect, so long as a single experiment or observation that contradicts that idea is enough to demonstrate its falseness (10). To that, he added that theories can always be criticized rationally and objectively, searching for and removing errors in such a way as to make it easier to distinguish between better theories and worse ones. Bunge uses a strange simile connected to vegetables to refute these notions. He writes:

Contrary to what Karl Popper taught, knowledge does not evolve by refuting conjectures, but rather by finding truths, that is to say, confirming conjectures, especially hypotheses about action mechanisms. Similarly, the cultivation of vegetables involves weeding, but we eat vegetables, not weeds. (1 p.90)

I confess that I could not understand the analogy (what is analogous to what?) and that I cannot find any sign in it that could bring down the highly renowned Popperian point of view.

### FASCINATION WITH DRUGS

In reality, Bunge's uncritical seduction with drugs reaches beyond his opinion about psychotropic drugs. "There are two pharmacopoeias: the effective, for rich people, and the illusory, for poor people," Bunge states at one point (1 p.124). Part of the idea that the author seeks to transmit is correct, because it points to the well-established truth that pharmacological research centers on drugs that have a powerful market and disregards the typical illnesses of underdeveloped countries whose inhabitants cannot buy expensive drugs. But it also affirms a widely held but erroneous conviction that the drugs bought by "rich people" are effective. It is not difficult to refute this fallacy; one has only to delve into the vast amount of information on the subject.

During an international seminar carried out under the suggestive title of "The Hidden Incentive of Medicine" [*El incentivo oculto en la medicina*] in Mendoza, Argentina, in 2002, the president of the Federal Medical Association [*Asociación Médica Federal*], Miguel Matta, said ironically: "...in Argentina, for example, twelve thousand pharmacological formulations can be found, 12 times more than in Switzerland. The Swiss must be very sick, because they do not have our wide range of therapeutic options" (11). The total number of pharmacological specialties and formulations registered in Spain is 50% bigger than in Argentina, amounting to 18,250 in 2004 (12); these drugs produce, annually, around 19 million adverse effects and cause the death of 6500 patients (13,14).

Sadly, physicians take an active part in the creation of such tragic statistics (15); they also contribute to the loss of many millions of dollars in our national economies each year, as noted by Gøtzsche in his article in the journal *Trials* (6).

### THE LEGACY OF IVAN ILLICH

Today, the reality described in the preceding section is well known. Four decades earlier it was only vaguely discerned, until the Austrian Ivan Illich burst onto the scene. Bunge's criticism of Illich's work is astonishing: "His writings are bad for individual and public health" (1 p.167).

I consider the book *Medical Nemesis* (16) to be a true classic that played a key role in the historical moment it was produced. It shook the foundations of the hegemony and the (very often irresponsible) arrogance of the medical class, and did so through a brilliant exhibition of ideas that nobody had dared to develop with such clarity and ease. It is not acceptable to eliminate in a few lines one of the most original thinkers to analyze the problems associated with medical practice during the second half of the last century. According to Bunge, Illich's book illustrates the predominance of an ideology that generates "suspicion of science and of reason in general" (1 p.195). However, such a vision ignores that it was not science and much less reason that were on trial; on the contrary, through an impeccable rationality and a powerful scientific weaponry, deviations that affected the medical practice of that time – because of their irrationality and because they turned their back to the data provided by science – were under examination.

Bunge reacts angrily against Illich for "accusing medicine of 'promoting illness'" (1 p.167). Accusing *medicine* as such in that manner would be a stupidity, in which Illich, of course, does not take part. That would be like accusing the iron and steel industry for the deaths caused by weapons. His accusation was against the way medicine was institutionalized, the medical *establishment*. Condemning the abominations affecting medicine cannot be confused with speaking "against medicine," as Bunge tendentiously states (1 p.166), especially when Illich repeatedly admits many of its victories.



*Medical Nemesis* begins by affirming that “institutionalized medicine has become a serious threat to health” (16 p.9). In this area, Illich was, above all, a visionary. He was ahead of his time when he defined and outlined a phenomenon that has not ceased to grow alarmingly: the invention of illnesses (17) and the medicalization of society (18). Bunge’s own definition of this latter concept is insufficient; he describes it as “the attempt to disguise social problems as medical problems” (1 p.167). In some way that statement is true but, as Teresa Forcades (19) brilliantly explains and illustrates, what is sought through medicalization is to face social problems through medical interventions done on the individual that suffers from them. Additionally, the medicalization of society includes an important problem that his definition does not cover: the transformation of natural processes or features into illnesses (baldness, old age, erectile dysfunction at an advanced age, shyness, menopause, etc.) (20-25) and the management of vital events as if they were ailments (birth, unhappiness or grief due to the death of a loved one) (26-27), with the resulting intervention – unnecessary and very often iatrogenic – through therapeutic actions.

To illustrate the rigorousness found in the whole of Illich’s book, I take the liberty of quoting only one paragraph (the eight references that appear in this paragraph correspond to the respective studies that Illich cites and comments at the bottom of the page):

In the United States, the volume of the drug business has grown by a factor of 100 during the current century.<sup>105</sup> 20,000 tons of aspirin are consumed per year, almost 225 tablets per person.<sup>106</sup> In England, every tenth night of sleep is induced by a hypnotic drug and 19 percent of women and 9 percent of men take a prescribed tranquilizer during any one year.<sup>107</sup> In the United States, central-nervous-system agents are the fastest-growing sector of the pharmaceutical market, now making up 31 percent of total sales.<sup>108</sup> Dependence on prescribed tranquilizers has risen by 290 percent since 1962, a period during which the per capita consumption of liquor rose by only 23 percent and the estimated consumption of illegal opiates by about 50

percent.<sup>109</sup> A significant quantity of “uppers” and “downers” is obtained in all countries by circumventing the doctor.<sup>110</sup> Medicalized addiction<sup>111</sup> in 1975 has outgrown all self-chosen or more festive forms of creating well-being.<sup>112</sup> (16 p.95).

“What was radical in 1974 is in some sense mainstream,” said the famous editor of the *British Medical Journal*, Richard Smith, in an editorial written 30 years after the publication of *Medical Nemesis* (28). Smith praises the power of the book as “undiminished” and highlights Illich’s notable prescience; he concludes by recommending the study of the book to every physician and student of medicine. In effect, Illich was ahead of his time in denouncing so-called “disease mongering,” as it is known today.

This phenomenon has been widely discussed in recent years. Bunge makes a lukewarm but correct allusion to invented illnesses by making reference to “the hormonal deficiencies and mental illnesses fabricated by certain pharmaceutical laboratories” (1 p.66). He also mentions homosexuality, which “despite being no more than a deviation of the statistical norm, was included until 1974 in the list of mental disorders of the American Psychiatric Association.” Personally, I do not believe that this sexual inclination is a deviation, whether statistical or of any other type (b), like being exceptionally gifted, left-handed or redheaded. I do not know what a “statistical norm” [*norma estadística*] is, but in Spanish, a “norm” [*norma*] is a rule one must follow or a rule to which conducts must adjusted. If it was once considered a psychiatric disorder, or if in some areas it is still conceptualized as a deviation, it is not because of a statistical feature, but because of scientific nonsense or because reprehensible cultural prejudice prevails.

Now, the problem of disease-mongering is not circumscribed only to a small number of imaginary illnesses, but it is something deeply rooted; one has only to see the vast literature that addresses the topic (29-32) and denounces the overwhelming amount. In any case, the solid and consistent contemporary dissection of the phenomenon is direct heir to the seminal work of Ivan Illich.

Finally, Bunge makes the unbelievable statement that Illich’s work maintains that “medicine

causes more illnesses than those it cures,” and he loftily adds that Illich did so with no objective data to support his statement. He does not say that the work is insufficiently supported by research, which would be equally surprising, but could at least be accepted given that insufficiency is relative and subjective. But no, Bunge affirms that Illich’s work has not “the least empirical backing” (1 p.195). One asks oneself if Bunge really read Illich’s work or if he does not remember it well, since one of its most striking features is the overwhelming amount of data Ivan Illich provides to support his statements (c), as we saw in the paragraph quoted above. I have the impression that precisely because Illich knew that his book would arouse the furious reaction of the medical stratum, he put special energy into supporting every assertion with precise and convincing data.

### **Pharmaceutical sales representatives, manipulation and physician-centrism**

Some of the book’s surprising perspectives can only be explained by a considerable lack of information. The most disconcerting example is Bunge’s consideration that, to be up-to-date on the advances in medicine, physicians should educate themselves through “pharmaceutical sales representatives” (1 p.184), because in his opinion they constitute one of the three sources of current information (the other sources are reading medical literature and attending seminars and conferences). Although in the Preface he suggests that physicians must be vigilant and filter the information pharmaceutical representatives provide, this warning vanishes in the rest of the book. It is no secret that the only task assigned to these workers – whose salaries are generally paid by international drug companies – is to guarantee that the drugs produced by their employers are prescribed, whether or not rigorous studies that put them in question exist, and very frequently through the use of direct or cloaked forms of bribery.

It is rather obvious that we should not let foxes guard the henhouse, even if we are watchful of their conduct. Personally, I find Bunge’s suggestion an awful way of keeping atop scientific progress; the evaluation of such progress demands, above all, an atmosphere

of transparency, honesty and debate. Although the transcendent French philosopher Michel Foucault (d) may have made some mistakes, his reflection that truth in the collective imaginary is that which is established by power is a dramatically pertinent and current observation. The asphyxiating dominance of media power, and of the companies to which the media’s power is subjected, is what ends up imposing “the truth.” Health professionals – in particular, those who prescribe drugs and therefore hold enormous potential to generate profits for the laboratories – are far from immune to such manipulation.

Bunge’s reflections are tinged by a physician-centrism that is present directly or indirectly throughout the text. It is true that it is a book “for physicians,” as the title of the work suggests. However, to accurately examine the tasks and ethical and philosophical problems associated with physicians, the large group of professionals that interact with them must also be taken into account. For diagnosis or for therapeutic, preventive or rehabilitative actions, current medical sciences demand interdisciplinary interactions including many other specialists: cybernetics experts, physicists, biochemists, nurses, biotechnologists, odontologists, statisticians, psychiatrists, psychologists, pharmacists, engineers, and even lawyers, journalists, social workers, documentary makers and economists, just to mention some of the disciplines.

Instead of undertaking the enormous task of signaling the many points in the text where omissions could be important, it is more practical for me to concentrate on an area in which Bunge’s anachronism manifests itself most evidently: his conceptions regarding Nursing.

The patriarchal language he uses – always making reference to the “*médico*” [male physician] and “*enfermera*” [female nurse] – is surprising, because, although the masculine desinence is all-inclusive in Spanish, which is something that could justify the lack of the noun “*médica*” [female physician], that condition is also present in the word “*enfermero*” [male nurse], expression which he nonetheless never uses. Additionally, all of his considerations correspond to the traditional model that regards the nurse as a mere auxiliary to the medical consultation and promotes an elitist endogamy within the medical body. It is worthwhile to quote him *in extenso*:

In recent years, many Faculties of Nursing have been established in which graduate degrees are offered and professors can receive research subsidies [...]. Without a doubt, it was time to recognize the importance of Nursing. But, is this craft benefited by separating it from medicine and making it pass as a science? And are patients benefited if we replace their physician with a nurse or if a hierarchy of care which is based in a hierarchy of knowledge is subverted? In other fields, craftspeople are professionally recognized without academic pomp. For example, we all respect good parents, constructors, electricians, X-ray technicians, pilots, laboratory experts and filmmakers, but nobody suggests giving them advanced academic degrees. Master's Degree in fatherhood? Charlie Chaplin, PhD? Craftspeople do not learn through books, but through work, and they are useful when they work under the supervision of experts with wide and deep knowledge who are up to date with specialized literature. Strictly speaking, medical technology [...] is something different: it requires not only ingenuity and manual ability, but also a great deal of scientific knowledge. (1 p.189) [Own translation]

In an unfortunate homage to the medical class, Bunge cautions against breaking the health care hierarchy (that is to say, for the physician to no longer be conceptualized as more important than the nurse), since it is based in a knowledge hierarchy (in other words, that the knowledge of physicians is more important than the knowledge of nurses). It is not difficult to understand that it is not a matter of who knows more when the knowledge is regarding different though complementary fields (34). In addition, I do not think that by giving academic status to Nursing anybody is proposing that physicians should disappear, and that the care of the patients should depend only on specialists of the Nursing discipline. However, Bunge seems to be taking for granted that something like that could happen, and he deems it necessary to call attention to such a dangerous change of course for patients.

His words can be summarized in this way: we should respect nurses but put them in their place; we should not forget they are mere crafts-

people (e) who have nothing to research and who should work under the supervision of those that truly possess wide and deep knowledge. They should be obedient and dedicated to their subordinated task.

If it were up to him, the many scientific journals on Nursing registered in *Scopus* and *Medline* might perhaps be eliminated, as well as the thousands of books on this discipline being produced at present. Scientific literature is unnecessary for those who only need manual ability and ingenuity, like construction workers.

He ignores that for many years Nursing has had its own functions, including health promotion, maintenance and recovery, which are crucial not only for the treatment of ailments and disabilities, but also for their prevention (35). Clinging to a conception long since overcome, Bunge asserts that they do not have their own system of knowledge, and he defends, on that basis, an operative and intellectual subordination that would carry health care back to the models that were dominant in both theory and practice at the beginning of the 20th century. In effect, in 1902, Joseph McGregor Robertson, with a medical degree from the University of Glasgow confirmed: "A nurse must begin her work with the idea firmly implanted in her mind that she is only the instrument by whom the doctor gets his instructions carried out; she occupies no independent position in the treatment of the sick person" (36). And, in 1917, Sarah Dock, a registered nurse from Kentucky, wrote:

No matter how gifted she may be she will never become a reliable nurse until she can obey without question. The first and most helpful criticism I ever received from a doctor was when he told me that I was supposed to be simply an intelligent machine for the purpose of carrying out his orders. (37)

But some of Bunge's ideas take actually us a century and a half back. Let us see Florence Nightingale's ironic view of this subject in 1860:

No man, not even a doctor, ever gives any other definition of what a nurse should be than this — "devoted and obedient." This definition would do just as well for a porter. It might even do for a horse. (38)



Skills and ingenuity are, of course, necessary, like in any other profession, medicine included. But it would be impossible to list all the nursing tasks that call for independent decision-making when dealing with patients as well as highly specialized and subspecialized knowledge – tasks therefore unable to be carried out efficiently by anybody not well-versed and scientifically up to date in those domains.

## PROBABILITY THEORY AND SUBJECTIVITY

Apart from some specific errors and statements that, in my opinion, arise from a process of thought that is rigid or stuck in conservative visions, most of the critical observations made thus far point to Bunge's exaggerations, simplifications or trivializations. But that is not the case when the author delves into the world of probabilities and Bayesian thought, where he shows himself to be especially erroneous. The detailed examination of Bunge's mistakes in this matter demands technical considerations that go beyond this reflection, and can be consulted elsewhere (39), but I will allow myself to outline the essence of such errors.

His fundamental errors – and the foundation of his entire incursion in the topic – reside, firstly, in failing to understand that *arbitrariness* and *subjectivity* are two completely different concepts; and, secondly, in considering that probability is a notion exclusively applicable to intrinsically random processes (in the sense they are not governed by a casual mechanism). Let us review briefly both topics.

Bunge affirms that “because they are subjective, Bayesian probabilities are arbitrary” (1 p.99). Arbitrariness and whim compromise any scientific discourse, but it is well-known that subjectivity in science is inevitable, as it is almost universally admitted (40,41). This reality includes the field of probability. Based on that misunderstanding, Bunge attributes a capricious conduct to Bayesian statistics. “The Bayesian assigns whatever probabilities he feels like” (1 p.99) Bunge affirms, and some pages later he goes beyond that to emphatically claim “we cannot speak of probabilities in medicine” (1 p.101).

The idea present in the subjective interpretation of probability is that, given a random phenomenon or a phenomenon conceived as such to solve a certain problem (in the sense that the phenomenon can be verified and that it is impossible to know beforehand what outcomes will occur), a probability representing the degree of confidence or belief that one has in the occurrence is implicitly or explicitly assigned. Under this approach, the probability is not determined beforehand, but it is established according to the available information – quantitative or not – about the situation, as well as the way in which said information is valued.

The essential difference with *frequentist* interpretation lies in that the assignment of values, although conditioned by the available information, is an attribute of each particular observer, without a need for the opinions of different analysts to coincide exactly. Under the frequentist approach, the probability of a given event is a single and ideal number (specifically, the limit of the ratio between the number of times said occurrence takes place and the number of times in which the process that could produce it is carried out, when this last number tends to be infinite), and what can vary are the estimates we make of this number.

When someone says that the probability of Brazil beating North Korea in a World Cup match is equal to 0.95, the probability of a draw is 0.04 and the probability of Brazil losing the match is 0.01, these numbers were not chosen from a table of random numbers nor according to aesthetic preferences. They were set on a subjective basis – the performance of the forwards in recent matches, the number of players who already have a yellow card, the market value of the goalkeepers, the results obtained in recent matches and the quality of the adversaries in those matches, etc. – but they are not arbitrary (f). If betting houses fixed the probabilities on which wins are based simply to suit their mood or aesthetic preferences, rather than making big profits they would go bankrupt.

Obviously, for this interpretation to be successfully applied in an operative framework, it is necessary for those that utilize it to maintain a high degree of rationality in the assignation of probabilities. If we want to make valid inferences, the values determined cannot be the result of the whims or feelings of the person setting them. Once the degrees of confidence about the occurrence of the

events have been assigned through that method, if the relevant values satisfy the Kolmogorov axioms (42), we can work with these probabilities just as we do with probabilities defined in any other way that also fulfill those axioms.

“Only random and randomly chosen events have probabilities” (1 p.100) Bunge maintains. It is clearly difficult or impossible to interpret the concept of an event having a probability. Events do not *have* probabilities; they have probabilities of occurrence assigned to them by one method or another if this is considered to be fruitful. This is not a trivial detail, but rather is fundamental: while the first points to a feature that would supposedly be intrinsic to certain “events,” the second is a convention that generally is adopted for operative purposes.

Bunge communicates to us that Bayesian interpretation is not appropriate for the health sciences. As surprising as it may be, he bases this conviction on the idea that in health sciences there is not a wide diversity of opinions. According to Bunge, patients and physicians fortunately know that “if there are different opinions regarding a treatment or a diagnosis, the opinion of a third party or a group of experts is generally asked for; and these people are expected to give not only their opinion, but also arguments based in the biomedical sciences” (1 p.101). What Bunge does not see is that, in any case, we are talking about an *opinion* that, as such, will inexorably involve some degree of subjectivity to a greater or lesser extent. It could be no other way: biomedical sciences are not only full of uncertainty, provisional truths, controversies and doubts, but they also assume that certain things to be true that are in fact not true (43). And finally, grounded arguments are not exclusive to biomedical sciences; they are also present in the assignation of probabilities, and such assignation can similarly be adopted after consulting with another specialist or appealing to a panel of experts.

Now, the essence of this debate resides in the following question: On what basis can a given methodological approach be accepted or rejected in the context of the solution of a problem? In the framework we are discussing, this can be translated into the following question: Who could be the arbitrator that concedes or denies the validity of assigning probabilities to events that are not intrinsically random?

## MODEL LEGITIMACY

Let us examine more closely the statement that “it is not appropriate to assign a probability to any kind of event. Only random and randomly chosen events have probabilities” (1 p.100).

A model is understood as an idealization or representation of reality which tries to simplify reality so as to examine it better and then use the model’s derivations towards the solution of a problem in the context of complexity that inspired the model. As such, a model only admits a pragmatic evaluation. In other words: since the construction of model responds to an attempt to solve a problem by observing its behavior, the model can only be discredited if it does not contribute to solving the problem that led to its conception.

This is the same logic that can be applied when assessing the legitimacy of a way of behaving in order to resolve any practical problem. Leaving aside the ethical dimension, which is another topic, the legitimacy of that behavior depends on the degree to which it helps solve the problem for which it was employed. It cannot be “illegitimate” if it is useful, nor “legitimate” if it is useless.

It is obvious that if Brazil wins, there is a specific reason (during the match Brazil scored more goals than North Korea); and if Obama wins the elections, it is for reasons that made it possible for him to obtain more votes than his adversaries. The outcome is the result of those causes, whatever they might be; but the outcome can be managed like a random event due to the fact that there is no way whatsoever to identify the outcome with certainty beforehand, even if we know the value of some of the variables that could influence it.

If betting houses were subject to Bunge’s dogma, they would not make the million-dollar profits they make (44); and if Nate Silver would have obeyed Bunge’s methodological mandate, he would not have had the incredible success he had with his predictions about the US elections (45).

Bunge repudiates the use of the probability theory even in situations where by definition subjectivity does not intervene. He goes so far as to say that calling the relative frequencies used by epidemiologists “probabilities” is “doubly erroneous: because frequencies are collective properties and

because the use of probabilities is only justified in reference to random processes, given that underlying processes have causal roots" (1 p.102). And then he returns to the topic stating that:

It is often said that there is a "probability" that a specific treatment can cure a specific illness, but this use of the concept of probability is incorrect, because the concept in question is theoretical, not empirical. The so-called "probabilities" spoken of in medicine and epidemiology are in fact relative frequencies, and these are not necessarily (logically) related to randomness. (1 p.142) [Our own translation]

He then concludes that "physicians [...] will do well if they limit themselves to statistical frequencies" (1 p.142), without considering them to be probabilities. If relative frequencies cannot be considered to be probabilities – a *dictum* invented by Bunge (g) – one could not, just to give an example, calculate life expectancy, a major parameter of contemporary public health based on probability theory.

In conclusion, Bunge's decisive negation of the use of probability theory in the health field

remits us to the controversy that took place in the mid-9th century, when the urologist-surgeon Jean Civiale (1792-1867) attempted to use statistics to decide between two treatments for operating on bladder stones. The debate, with the participation of renowned figures such as Siméon Poisson, was brought to the Paris Academy of Sciences. One of the commissioners, the Spaniard Risueño de Amador (1802-1849) made a pronouncement that could have come from Bunge himself: "The calculation of probabilities appears to be very imperfect, even mathematically speaking, because mathematicians cannot come to an agreement about important aspects of the theory. Its application in medicine, in therapeutic medicine above all, is anti-scientific" (46). Finally, however, the Academy ruled in favor of Civiale's methodology of analysis: "Medicine is a science of observation, exactly like the other sciences; statistics and the calculation of probability have something to tell us regarding the conclusions we should reach, and with what degree of confidence we must reach them" (47).

## ENDNOTES

a. I have to admit, however, that one is tempted to do so. For example, it is disconcerting that in the book there is no mention whatsoever of the internet and the new information and communication technologies, as if they did not have enormous and growing importance in the practical, conceptual, axiological, technological and, consequently, philosophical fields of health praxis.

b. Bunge also addresses this idea earlier in the text. There, he does not talk about homosexuality as a feature, but rather homosexuals themselves, explaining that while in previous years homosexuals were condemned, "today they are considered mere statistical deviations" (1 p.50).

c. In fact, the notes containing these references make up approximately half of the whole volume.

d. The importance of the author of *The Order of Things* is beyond all reasonable doubt, although his work is in large part trivialized by Bunge, who in addition gratuitously and rather maliciously writes that if Foucault "would have understood that 'promiscuity is bad for health', he would not have died of AIDS" (1 p.65).

e. That is, "a person that exercises a merely mechanic skill or a trade," according to the meaning that Royal Spanish Academy gives for the term "craftsperson" [*artesano*].

f. These two teams have never before played against one another, therefore it would be impossible to make a frequentist estimation.

g. I do not know if any other philosopher, epistemologist or mathematician shares this view; I personally do not know anyone who shares it.

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