Retrospective examination of three articles published in the *Revue des Questions Scientifiques* in 2005 and 2006.

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Abstract

Three articles published by the same author in the *Revue des Questions Scientifiques* in 2005 and 2006 are reexamined with special attention paid to a theorem published by Allahverdyan and Nieuwenhuizen in 2002 concerning Thomson's formulation of the second law, as well as to the results of the so-called "before-before" experiment performed on entangled pairs by Stefanov, Zbinden, Gisin and Suarez. As far as thermodynamics are concerned, it is explained here that a macroscopic observer can generate *quasi* "cycles", whose cyclical characteristics are in fact only valid from the macroscopic point of view, that can potentially enable her/him to retrieve work from a thermalizer without causing any *perpetual motion*. Concerning quantum entanglement, it is recognized – in contrast with what the author originally published in 2005 and 2006 - that superluminal transmission of decipherable information cannot be achieved if the Copenhagen interpretation of quantum theory remains universally valid. It is also pointed out that the foundations of general relativity cannot be easily reconciled with the implications of Einstein-Podolsky-Rosen measurements.

Introduction

During summer 2005, the author of the present retrospective published in the *Revue des Questions Scientifiques* an article [1] whose main purpose was to challenge the universality of the second principle of thermodynamics. Later during the same year, the same author also published an article [2] dealing with the completely unrelated subject of teleportation. This latter article resorted to a semi-classical approach to deal with quantum measurements, and its conclusions were totally wrong. At the time of these publications, the author, i.e. myself, had already stopped to work as a professional physicist for more than ten years. After a few months, I realized that my article on teleportation was fundamentally wrong. Unfortunately, instead of immediately accepting my failure, I hastily tried to figure out a way to "save" the possibility that my article on teleportation might still be corrected in some way. As a result, I published in 2006 in the same Journal another article called *Correction and complement to the article "Teleportation and Information Decoding"* [3]. Regrettably, these so-called "correction and complement" only served to add more confusion. I finally resolved myself to write a shorter *erratum* [4], which marked the end of my naive efforts to raise new questions about the nature of quantum measurements.

Eventually, my attempts to propose new ideas in the field of teleportation have never produced any substantial result. On the other hand, as far as the domain of thermodynamics is concerned, it seems that the content of my first 2005 article has never been challenged, and this article may perhaps deserve to receive more attention than has been the case so far. Admittedly, the style of its redaction was rather eccentric. Its main claim dealt with the second principle of thermodynamics, but this fact was not emphasized strongly, whereas digressions concerning the irreversibility of time, Russell's paradox and even ancient Chinese philosophy filled practically the entire second half of the article, even though these considerations did not contain any significant innovation. Bibliographical references were also particularly scarce. No mention was made of the large variety of "challenges" to the second law of thermodynamics compiled by Vladislav Čápek and Daniel Sheehan in a monograph [5] published during the same year 2005.

My aim in the present retrospective article is therefore double:

(a) As far as thermodynamics are concerned, I wish to discuss the content of my 2005 article anew in the light of the work published by other authors, notably Čápek, Sheehan, Allahverdyan and Nieuwenhuizen.

(b) As far as teleportation is concerned, I wish to recognize clearly that teleportation cannot be used *per se* to achieve superluminal communication within a universe described by "standard" (Copenhagen) quantum mechanics. I also intend to point out that new fundamental problems arise when gravitation is added in the picture. The question of how gravity may possibly affect teleportation, or conversely, of how quantum mechanics may affect gravity, appears quite puzzling.

A. Comments on different challenges to the second principle of thermodynamics

The set of contemporary challenges to the second law that has been collected by Čápek and Sheehan in 2005 appears very large indeed [5]. A lot of time and space would be necessary to examine each of these challenges in detail. Here, we shall limit ourselves to criticize two schemes referenced in chapter 3 of Čápek and Sheehan's monograph, as well as another one contained in their last chapter (chap. 10). We shall show that none of these schemes has any chance to be able to break the second law. We shall then turn back our attention towards the simplest physical reasons accounting for the overwhelming success encountered by the second law. These reasons are so widely applicable that it is unreasonable to try to challenge the second law while ignoring them. Lastly, I shall emphasize why my proposal dating from 2005 successfully bypasses all these hurdles.

A.1. The "fish-trap model" of Čápek

Let us first examine the so-called "fish-trap model" of Čápek, which is presented by Čápek and Sheehan as being "the first truly quantum model leading to behavior challenging the second law". The total Hamiltonian of this "fish-trap model" is composed of three terms:

$$H = H_S + H_B + H_{S\text{-}B} \qquad (1)$$

where S indicates the *System* and B a *Bath of phonons*.
H_S is assumed to be the sum of three terms:

$$\begin{aligned} H_S = J(c^\dagger_{-1}c_0 + c^\dagger_0 c_{-1})\otimes|d\rangle\langle d| + I(c^\dagger_1 c_0 + c^\dagger_0 c_1)\otimes|u\rangle\langle u| \\ + {}^{\varepsilon}/_{2}[|u\rangle\langle u| - |d\rangle\langle d|]\otimes(1 - 2c^\dagger_0 c_0) \end{aligned} \qquad (2)$$

For simplicity, we shall suppose that the energy ε is positive. The operators $c^\dagger_{-1}$, $c^\dagger_0$ and $c^\dagger_1$ correspond to the creation of an itinerant particle respectively on the left, the middle and the right of a molecule whose configuration $|u\rangle$ (i.e. "*up*") becomes energetically favorable in comparison with $|d\rangle$ (i.e. "*down*") whenever an itinerant particle occupies the middle site (site 0).

The bath is composed of bosons whose Hamiltonian appears particularly simple:

$$H_B = \Sigma_k\ \hbar\omega_k b^\dagger_k b_k \qquad (3)$$

The coupling between the system and the bath is then written:

$$H_{S\text{-}B} = {}^1/_{\sqrt{N}}\Sigma_k\ \hbar\omega_k(b_k+b^\dagger_{-k})\otimes\{G_k[|u\rangle\langle d|+|d\rangle\langle u|] + g_k c^\dagger_0 c_0\} \qquad (4)$$

The basic idea of Čápek can be summarized as follows: whenever an itinerant particle presents itself on the left side of the molecule (site -1), it can move towards the middle position (site 0) through zero-point energy quantum motion, as implied by equation (2). However, as soon as site 0 is occupied by an itinerant particle, configuration $|d\rangle$ becomes energetically costly. Interaction with the bath through $H_{S\text{-}B}$ can therefore lead to the thermal relaxation of $|d\rangle$ into $|u\rangle$. Delocalization of the itinerant particle towards the right (site 1) through zero-point energy quantum motion becomes thereafter possible. As soon as the itinerant particle reaches the right side of the molecule, molecular configuration $|u\rangle$ becomes energetically costly. Interaction with the bath through $H_{S\text{-}B}$ can therefore induce another thermal relaxation, this time from $|u\rangle$ to $|d\rangle$. As can be expected from equation (2), the motion of itinerant particles is fundamentally asymmetric: whenever the central molecule adopts the $|d\rangle$ configuration, the transfer of an itinerant particle from left to right is rendered possible by zero-point energy fluctuations, which remain efficient even at zero temperature, whereas the reverse motion from right to left can only be initiated through thermal relaxation, which is temperature dependent, and totally inefficient at zero temperature. These considerations have led Čápek and Sheehan to believe that $|d\rangle\otimes|1\rangle$ constitutes the asymptotic final state of the system (hence its name "fish trap", which suggests that itinerant particles can be "trapped" like "fish" at the right side of the molecule). Assuming, as we have done, that an itinerant particle initially located on the left side (while the molecular "central system" is in the "down" state) eventually ends up at the right side, the energy of the itinerant particle remains unchanged insofar as the mean energy of Hamiltonian H_S is zero. However, if a supplementary term in $\varepsilon_1 c^\dagger_1 c_1$, with $\varepsilon_1>0$, is added to H_S, the transport of an itinerant particle towards the right can increase its potential energy. According to Čápek and Sheehan, "the question arises as to the source of the energy increase. The answer is that phonon emission or absorption processes mediate the particle transfers in each elementary act of the combined process" (cf. ref. 5, p. 82).

Unfortunately, this conclusion cannot hold due to a rather obvious reason: according to Čápek's scenario, during each transport of a particle from left to right, the thermal bath twice *receives* an energy quantity approximately equal to ε. If one followed Čápek and Sheehan, one should therefore conclude that both the bath and the rest of the system receive more energy during each cycle. This would constitute a challenge not only for the second principle of thermodynamics, but also for the first one! Since Čápek's proposal resorts entirely on unitary quantum theory and does not suppose that quantum measurements need to play any unconventional role, the energy of the entire system is necessarily conserved.

One of the most obvious omissions of Čápek and Sheehan lies in their lack of consideration for the quantum Zeno effect, which can be expected to play a dominant role if we suppose, as they do, that $H_{S\text{-}B}$ is much stronger than the terms in J and I present in H_S (cf. eq. 2). The Zeno effect renders zero-point energy transfer ineffective, and the reason to believe that zero-point delocalization (from left to right) should be more efficient than thermally authorized transfer (from right to left) disappears.

In fact, Čápek and Sheehan have not entirely ignored this difficulty, since the role of the terms in g_k contained in $H_{S\text{-}B}$ serve to try to avoid it. Čápek and Sheehan argue that dephasing (due to g_k) "implies continuous energy exchange between the system and the bath, thus, final energy conservation is always possible". Unfortunately, they also confess (see for instance ref. 5, p. 80) that nowhere do terms in g_k play any crucial role in their calculations. Exactly the same conclusions can be drawn from their so-called "sewing-machine model", which we examine briefly below. All things being considered, the invocation of dephasing terms in $g_k c^\dagger_0 c_0$ cannot change the fact that the perturbative approximations used by Čápek and Sheehan to calculate transfer rates are fundamentally invalid. This is also clearly indicated by the fact that the results of their calculations lead to a collapse of the *first* law even when all the terms in g_k are strictly equal to zero.

A.2. The "sewing machine model" of Čápek

Let us now show that the so-called "sewing machine model" originally due to Čápek, referenced in part 3.6.4. of Čápek and Sheehan's monograph (cf. also ref. [6] for a more detailed account) constitutes a more complicated version of the "fish-trap model" considered above, which does not offer any new fundamental insights. The single itinerant particle used in the "fish-trap model" is replaced by a *pair* of itinerant particles in the case of the "sewing machine model". As a result, the total Hamiltonian of this latter model appears discouragingly long to write, although the basic underlying idea of the "sewing machine model" is not particularly complicated and can be summarized as follows: pairs of itinerant particles (initially described by a combination of creation operators written by Čápek and Sheehan

$^{1}/_{\surd 2}[c^{\dagger}_{left}g^{\dagger}_{right}+c^{\dagger}_{right}g^{\dagger}_{left}]$) initially disposed symmetrically on both sides of a molecule can delocalize simultaneously towards the centre of the molecule even if the temperature is arbitrarily low, thanks to zero-point quantum fluctuations. When a pair of itinerant particles arrives at the centre, the initial configuration of the central molecule becomes energetically unfavorable. Interaction with the bath can therefore lead to the thermal relaxation of the configuration of the molecule. Once the configuration of the central molecule has been modified, both itinerant particles can move together towards the left, or towards the right, through zero-point (non-thermal) fluctuations, and the central molecule can again relax towards its original configuration by releasing some energy within the bath. This leads Čápek and Sheehan to believe that the asymptotic state of the itinerant particles of this system can be written $^{1}/_{\surd 2}[c^{\dagger}_{left}g^{\dagger}_{left}+c^{\dagger}_{right}g^{\dagger}_{right}]$. If this were true, it should be possible for an external observer to let the system evolve through thermal fluctuations towards an asymptotic state of higher energy than the original state, and to extract some energy from it by transforming mechanically the asymptotic state into the original state as many times as wished.

As was already the case in the "fish-trap" model, a detailed examination of this proposal shows that the thermal bath twice receives more energy during each cycle, without ever having the need to release any. During each cycle, a pair of itinerant particles is also supposed to move towards an asymptotic state of higher energy. As for the "fish-trap" model, each cycle would not only break the second principle of thermodynamics, but also the first one!

A.3 Thermosynthetic life

At the end of their monograph, Čápek and Sheehan discuss yet another scheme, which relies on biological microorganisms to try to break the second law. From the point of view of physics, the biological nature of these organisms does not play any fundamental role in this scheme. Its only notable peculiarity is that is resorts to a pyramidal disposition of biological cells for enabling charged molecules to climb step by step, through thermal fluctuations, from the base of a pyramid to its top, where the electric potential is at its highest. Another quantum transformation, energetically rewarding for the observer, is then supposed to enable these charged molecules to return to the level of the base of the pyramid in just one step. Čápek and Sheehan calculate that the ratio of charge occupation probabilities p_{rel} between the base and the vertex of the pyramid should be of the order of:

$$p_{rel} = {}^{g(base)}/_{g(vertex)} \exp({}^{-\Delta E}/_{kT}) \qquad (5)$$

Here, g(*base*) and g(*vertex*) correspond respectively to the number of available sites for charged molecules at the base and the apex of the pyramid. If the base of the pyramid is large, this ratio can be huge (as large as 10^6 in the case considered by Čápek and Sheehan). According to eq. (5), the probability that a charged molecule may reach the top of the pyramid can therefore become significant even if ΔE is markedly larger than k_BT. Once a molecule has reached the apex of the pyramid, its return towards the bottom of the pyramid is supposed to be carried out mechanically by the observer along a trajectory whose only two points of contact with the cells of the pyramid are located at its apex and its base, so that the inverse factor $^{g(\text{vertex})}/_{g(\text{base})}$ plays allegedly no role for this come back.

The intuitive idea behind equation (5) is that a larger pyramidal base will multiply the chances of charged molecules to hop towards the top. However, in fact, equation (5) is totally invalid, and quite the reverse is true: the larger the number of available states at the base of pyramid, the smaller the probability for a charged molecule to be found elsewhere than at the base of a pyramid.

The discussion by Čápek and Sheehan has the merit to illustrate that, even if the second law had some loopholes, one should not be very surprised by the fact that the biological realm does not seem to have taken advantage of them: Čápek and Sheehan show that the relevant orders of energy magnitudes needed by biological cells would render their task quite difficult. However, from a fundamental point of view, the idea proposed by Čápek and Sheehan for a biological violation of the second law is simply incorrect.

A.4. Fundamental reasons for the failure of the models examined above

So far, we have only reviewed a very limited number of models to conclude that, in those cases, the second law is not violated. A few other proposals listed by Čápek and Sheehan have also already been refuted by other authors, notably by Wheeler in the case of a gravitational challenge [cf. ref. 5, p. 197], and by Cruden in the case of plasma "paradoxes" [cf. ref. 5, p. 262]. Čápek and Sheehan argue that these objections are not fully satisfactory. However, the task of providing a fully convincing argument in favor or against the second law should arguably fall primarily on those who wish to violate it, since it has never been experimentally broken so far.

The list of challenges similar to those enumerated by Čápek and Sheehan is potentially infinite, and trying to review all of them would be a fastidious and endless task. It is therefore methodologically more fruitful and instructive to start by examining the most essential reasons that contribute to the success of the second law. Such preliminary effort may enable us to eliminate without too much effort at least some of the challenges listed by Čápek and Sheehan. In a way, this is precisely the methodology that Čápek and Sheehan themselves have already tried to follow at the beginning of their monograph [cf. ref. 5, chap. 2]. This has led them to conclude, in particular, that "going beyond weak coupling appears (*a priori*) advantageous for second law challenges" [cf. ref. 5, p. 67], which is another way of saying that weak coupling has little chances to overthrow the second law. However, the distinction between *weak* and *strong* coupling is not necessarily fundamental, since it depends strongly on the base chosen to describe quantum states. All closed systems possess bases of state-vectors for which weak-coupling approximations can apply. If the occupations of *all* the state-vectors of such bases converge irreversibly towards the Maxwell distribution, it is not easy to see how any phenomenon escaping this trend could be observed along another base.

Within the frame of statistical quantum dynamics, the most essential reason for the validity of the second law appears actually quite simple: when two particles $|1\rangle$ and $|2\rangle$, of energies E_1 and E_2, enjoy the possibility to collide randomly and to transform themselves through collisions into a single particle $|3\rangle$ of energy E_3, and when no other transformations are allowed, the respective occupation probabilities $f(E_1)$, $f(E_2)$ and $f(E_3)$ of states $|1\rangle$, $|2\rangle$ and $|3\rangle$ necessarily verify, under equilibrium:

$$f(E_3) = f(E_1)*f(E_2) \qquad [6]$$

whereas, in the case of usual collisions:

$$E_3 = E_1+E_2 \qquad [7]$$

Equations [6] and [7] suffice to ensure that, statistically (after thermalization), the distribution of energy occupations of particles such as photons, phonons, etc., can be expected to converge asymptotically towards a Maxwell distribution of the type:

$$f(E) \sim \exp[^{-E}/_{kT}] \qquad [8]$$

If we admit that such convergence applies for single particle states such as those describing photons and phonons, and if baths of photons or phonons can serve as thermalizers for other systems, then similar Maxwell statistics necessarily remain valid to describe the equilibrium states of basically all kinds of closed systems. This reasoning suffices to provide us with the most elementary and most universal features of the irreversible trend towards thermal equilibrium.

More sophisticated mathematical descriptions of irreversibility have also been presented, which all confirm the validity of the second law. A most cogent one has been published by Allahverdyan and Nieuwenhuizen in 2002 [7]. The gist of their article would deserve to be reproduced in textbooks due to its fundamental relevance and to its remarkable simplicity. Allahverdyan and Nieuwenhuizen demonstrate that *no work can be extracted from a closed equilibrium system during the cyclic variation of its Hamiltonian, when this Hamiltonian is supposed to be controlled by an external operator.* This allows them to refute convincingly two commonly held views about the second law: the first, held notably by Landau and Lifshitz, considers that "the second law is incompatible with the microscopically reversible quantum dynamics, and the second law can somehow be connected with the quantum measurement process" (cf. ref. 7 p. 550). The second view, even more widespread in the litterature (and which, in spite of the reservations formulated by Allahverdyan and Nieuwenhuizen, remains arguably quite relevant), affirms that "the second law does arise as a consequence of the interaction between a quantum system and its thermal environment." (cf. ref. 7 p. 551).

At first sight, the demonstration provided by Allahverdyan and Nieuwenhuizen seems to rule out for ever the possibility of breaking the so-called "Thomson's formulation" of the second law. According to Thomson's formulation, it is impossible to transform *heat* into *work* by submitting a closed system to cyclical transformations. This version of the second law is characterized by a highly universal formulation, since the concepts of *entropy* and *temperature*, which are essentially macroscopic, are absent from it. Thomson's formulation also corresponds to the one which one would be most interested to break in view of practical applications.

In order to perform their demonstration, Allahverdyan and Nieuwenhuizen only need to suppose that the initial density operator of their system commutes with the initial Hamiltonian, and that initial state populations are regularly ordered as a function of energy, so that for any pair of states $|i>$ and $|j>$, $|i>$ is initially more occupied than $|j>$ whenever its energy is lower than that of $|j>$. Most readers of Allahverdyan and Nieuwenhuizen have probably concluded that Thomson's formulation of the second law is definitely unbreakable, as Allahverdyan and Nieuwenhuizen seem to have believed themselves.

However, in spite of the exact and general character of Allahverdyan and Nieuwenhuizen's demonstration, one can still notice that the two conditions required for its valitidy are not fully universal. First of all, the condition that initial *eigenstate populations should decrease as a function of energy eigenvalues* is never exactly fulfilled at the macroscopic level, even when thermal equilibrium is attained! This is due to the fact that whenever the spatial length required to distinguish between two different *eigenstates* exceeds the thermal coherence length of the system, thermalization of such different eigenstates cannot impose their respective occupations to be energetically ordered. The second prerequisite needed for Allahverdyan and Nieuwenhuizen's demonstration is that the Hamiltonian of the system should be exactly identical at the beginning and the end of each cycle. Although quite reasonable in appearance, this condition rules out the possibility of taking full advantage of the macroscopic size of a thermalizing bath: in the case of an infinite bath (like, perhaps, the cosmic microwave background), one could imagine that such a bath could be decomposed into an infinite number of finite baths, so that only one thermal "cycle" would need to be performed with each particular bath. This loophole leaves open the possibility for finding a way to extract some measurable quantity of energy from each bath only once, without having to worry about the impossibility of fabricating a *perpetuum mobile*. At first sight, the distinction between nearly identical *quasi* "cycles" and perfectly identical cycles may appear far fetched. However, this slight difference is a crucial ingredient of the scheme presented in the *Revue des Questions Scientifiques* in 2005. In this scheme (cf. Fig. 1), the repetitive adjustment of the moment of inertia of a compass produces random (non perfectly cyclic) effects on surrounding thermalizing fluids. The adjustment of the moment of inertia does not act on the eigenstates of the *entire* system in a cyclic way. Instead, the cyclical features of the transformations apply only to *that* part of these eigenstates describing the compass.

A.5 Energy conversion of relaxation times: success of the scheme presented in the *Revue des Questions Scientifiques* in 2005

If we consider the irreversible trend towards thermalization from a global point of view, it seems that performing perfectly controlled (adiabatic) manipulations can only serve, at best, to slow the trend towards irreversibility. The only way that remains potentially open for an observer to master partially the statistical consequences of thermalization lies in playing with the rates of thermal relaxation itself, without trying to control its randomness. This is precisely the direction that has been followed in the 2005 article entitled *Perte d'information et irréversibilité en thermodynamique*, which is reexamined here.

In 2005, I proposed to examine a cycle (in fact, a *quasi* cycle, whose cyclical properties are only macroscopically valid) during which the rate of rotational thermal fluctuations of a compass could be adjusted mechanically. This adjustment affects the interaction of the compass with the fluid above which it is floating, and with a second thermal bath composed of a gas of magnetic particles contained in a box (cf. Fig.1). The relaxation time of the magnetic fluctuations of the gas is written T_1. During each thermodynamic cycle, the only two adjustable parameters correspond respectively to the distance between the box and the compass, and to the moment of inertia of the compass. Although the mechanism conceived to adjust this moment of inertia (and hence the time scale of the rotations of the compass) appears admittedly very cumbersome, the overall scheme is remarkably simple.

Only three characteristics of my model have perhaps not been sufficiently commented upon in my 2005 article. The first consists in the fact that, after the completion of one "cycle", the respective motions of the compass and of the particles in the box can be considered as independent, since the box can be placed as far from the compass as we wish. This allows us to affirm that another macroscopic "cycle" can start, whose efficiency is expected to be comparable to that of the preceding cycle, at least as long as the particles in the box are sufficiently numerous to "forget" the microscopic details of their previous interaction with the compass within a time of order T_1 (which means that the three dimensions of the box need to be significantly larger than the thermal de Broglie wavelength of the gas). However, from the microscopic point of view, the initial situations of successive *quasi* cycles are all different from each other, so that *no perpetual motion is generated*. To be more precise, each time the moment of inertia of the compass is adjusted, the details of its interaction with neighboring fluids are different, so that our *quasi* "cycles" do not enter in the category considered by Allahverdyan and Nieuwenhuizen, who impose that the Hamiltonian of their microcanonical system should be exactly identical at the beginning and the end of each cycle.

From a more global point of view, Allahverdyan and Nieuwenhuizen's demand that the Hamiltonian of the system should be perfectly cyclical may be considered to be always verified, since it is always possible to describe an all-encompassing system with a Hamiltonian that never changes! However, in case one would wish to use such a global Hamiltonian to describe the controlled adjustments of our thermal machine, Allahverdyan and Nieuwenhuizen's second condition that *initial eigenstate occupations should decrease in function of energy* would be very far from verified, so that their theorem would become clearly inapplicable. If one wants to focus one's attention on a smaller Hamiltonian describing only our magnetic compass and its neighboring fluid, then Allahverdyan and Nieuwenhuizen's first condition, which demands that the Hamiltonian at the beginning and at the end of each cycle should be perfectly identical, cannot be verified, precisely because the quantity of work accumulated by the observer prevents this to happen. Quantum entanglement between the box of magnetic particles, the compass, its neighboring fluid and the observer increases with each *quasi* cycle, so that, ultimately, it may be presumed that the thermodynamic efficiency of the cycles our scheme may diminish after a certain number of cycles. However, even from the purely theoretical point of view, worrying about the accumulation of quantum correlations when more *quasi* cycles are performed does not really make sense, since quantum decoherence effects within thermal baths are expected to destroy quantum correlations within delays that can be assumed to be much shorter than the time intervals between different “cycles”.

Another characteristic of my 2005 proposal lies in the fact that the adjustment of the moment of inertia of the compass was controlled by a very awkward *macroscopic* mechanic process. I have made no efforts to refine my model on this point, since I have merely intended to discuss a *gedanken* experiment without examining the difficulties of its practical implementation, and since, from the purely theoretical point of view, there is no fundamental difficulty in assuming that an adiabatic transformation, whether microscopic or macroscopic, can be perfectly reversible. It is also worth pointing out that, although the mechanism considered in 2005 for the adjustment of the moment of inertia of the compass was built inside the compass itself - a very cumbersome supposition indeed -, this awkward feature is not necessary. In theory, one may be able to find a way to adjust the moment of inertia of the compass *externally* just as well. Once the fundamental validity of my 2005 *gedanken* experiment is recognized, a totally different kind of effort may be needed to observe its experimental relevance. At that time, it may become absolutely necessary to find a way to miniaturize the compass considered in Fig.1, perhaps by replacing it by a large number of independent molecules possessing two different stable spatial configurations.

A third characteristic of my 2005 proposal is that it has been shown to be valid within the frame of *classical* mechanics only. However, it does not seem that any assumption made in my 2005 article would be invalid in a purely quantum regime, since the classical frame used in 2005 actually *belongs* to the vaster domain of quantum mechanics.

B. How can the equivalence principle of general relativity influence the measurement of entangled quantum states?

One of the most striking properties of the measurement of entangled particles has been hinted at by Einstein, Podolsky and Rosen [8] and later clarified by Bohm and Bell, among others. It can be observed when two observers conventionally named Alice (A) and Bob (B) share a pair of entangled particles, for instance two spin $^1/_2$ particles in the singlet state $^1/_{\sqrt{2}}(\uparrow_A\downarrow_B-\downarrow_A\uparrow_B)$. In case A and B have agreed beforehand to measure the spins of their particles along the same direction, the results of their measurements will be opposite to each other, *i.e.* both are certain that the result of their partner's measurement will be opposite to their own. Although neither of them seems to be able to influence the result of the measurement of their partner, both of them enjoy the possibility to play a kind of quantum "lottery" game and to share a common knowledge of its results without having to synchronize their measurements in any way, which classical physics would never allow *per se*. Even before a strong experimental confirmation of the so-called "EPR paradox" was obtained in 1982 [9], Eberhard had shown theoretically that superluminal communication could not be achieved by using EPR measurements [10,11]. In spite of Eberhard's demonstration, Mittelstaedt estimated in 1999 that the arguments put forward to show that non-local effects cannot be used for superluminal communication are neither "really stringent" nor "convincing" [12]. As a matter of fact, to prove that a phenomenon is forbidden is an epistemologically highly challenging task. The propensity of the present author to doubt that the domain of validity of "impossibility" statements such as Eberhard's can really encompass reality in its entirety has unfortunately led him to propose, in 2005 and 2006, some ideas challenging Eberhard's conclusions. Now, I wish to admit that I am fully convinced that Eberhard was right, and that "standard" quantum mechanics – in other words, quantum mechanics as they are known today – do not allow EPR measurements to be used as a tool for superluminal communication. This theoretical conclusion is also corroborated by the highly interesting experiment performed by Stefanov, Zbinden, Gisin and Suarez in 2002 [13], which has shown that the predictions of "standard" quantum mechanics remain valid even when A and B perform their measurements from within two different moving frames wherein the chronologies of their measurements are mutually inversed, i.e. when both A and B estimate, from their own singular point of view, that their own measurement has been performed before the measurement of their partner. The logic behind the results of this so-called "before-before" experiment renders the idea that superluminal communication may be achieved through EPR measurements particularly improbable. Of course, this does not mean that standard quantum mechanics should necessarily forbid superluminal communication altogether. Other phenomena than EPR have been alleged to render such communication possible, such as the Casimir-like effect studied by Barton and Scharnhorst [14], whose discussion lies outside the scope of the present article.

Standard quantum mechanics appear to the modern physicist as a wonderfully ubiquitous theory. At the fundamental level, however, this theory has never been able to provide a clear, consistent view of what quantum measurements consist of. If one accepts the opinion according to which standard quantum mechanics provide us with a satisfying picture of reality, and according to which all the branches of a quantum state always continue to "exist" even when they become unobservable due to the effects of quantum decoherence, one cannot but conclude that quantum eigenstates of the entire universe have no way to interfere with other similar eigenstates, since no mechanism exists that would allow this to happen. Under such conditions, it is hard to see how any sub-part of a macroscopic eigenstate of the universe, for instance a biological sub-part, could ever make the experience of being distinct from the entire eigenstate to which it belongs. It would even be impossible for any sub-part of the universe to make the experience of evolving in time, since the ontology of perfect eigenstates of a quantum space is intrinsically timeless.

The linearity and unitarity of quantum mechanics have always been experimentally confirmed. Both of these ingredients are also crucial for the outcome of EPR measurements. But it remains too soon to claim that the domain of validity of this linearity and unitarity is truly limitless, particularly since one - and admittedly only one - phenomenon continues to pose severe problems of compatibility with quantum mechanics, namely *gravity*. The perception that gravitation and quantum mechanics are difficult to unify is not new. A majority of contemporary physicists seems more willing to modify general relativity than quantum mechanics in their quest for a more unified theory, although a smaller number of physicists, which includes notably Roger Penrose, has argued for the other way round. In his large opus entitled *The Road to Reality* [15], Penrose speculates [cf. chap. 30, p. 847] that if $|\chi\rangle$ and $|\varphi\rangle$ represent two possible quantum states of a particle, both identical except for the fact that they are centered around two different spatial positions, any combination $\alpha|\chi\rangle+\beta|\varphi\rangle$ is gravitationally unstable as soon as $\alpha\beta\neq0$. Penrose points out to the fact that the state of the universe just after the big-bang has been characterized by a tremendously low level of gravitational entropy, which he would like to explain by a consistent physical law. According to Penrose's hypothesis, the road towards such hypothetical law could lead to the destruction of the perfect linearity of quantum mechanics by gravitation! However, Penrose's ideas concerning the non-linearity of quantum mechanics and the constraints imposed on the original state of the universe are only speculative, so that his idea that a link may exist between both appears even more speculative! It may also be stressed that Penrose's reasoning raises unprecedented epistemological difficulties. *Physical laws* are usually supposed to apply indiscriminately to any set of *contingent* initial conditions. The so-called "universal" physical laws that have made the success of physics so far cannot constrain their domain of applicability to contain only one possible universe, otherwise there would be no reason to call them universal. Naturally, nothing proves *a priori* that physical laws will always deserve to be considered universal. On the other hand, since Penrose's speculations have not yet been tested experimentally, it may seem at least premature to push them too far.

Let us just note briefly one potential consequence of Penrose's ideas on EPR measurements. If, as indicated above, it is true that any combination $\alpha|\chi>+\beta|\varphi>$ is gravitationally unstable when $|\chi>$ and $|\varphi>$ represent two spatially distinct but otherwise comparable quantum states of a particle and when $\alpha\beta\neq0$, an EPR measurement producing the random result that we note {either $|\chi>$ or $|\varphi>$, with a 50% probability for each possibility} might prove energetically more favorable than the measurement producing the random result {either $^1/_{\surd 2}(|\chi>+|\varphi>)$ or $^1/_{\surd 2}(|\chi>-|\varphi>)$, with a 50% probability for each possibility}. Some physicists will probably conclude that this renders Penrose speculations even more difficult to believe.

In the rest of this article, I wish to show that, quite independently of Penrose's speculations, the question of the alliance between gravitation and quantum mechanics *necessarily* raises the problem of the physics involved in EPR measurements when gravity plays an important non-linear role, and that the standard EPR picture originally apprehended by Podolsky, Einstein and Rosen *needs to be complemented in some way*, whatever it is. Let us first remember that one of the simplest points of departure of EPR experiments consists in disposing of a wave function $^1/_{\surd 2}(\uparrow_A\downarrow_B-\downarrow_A\uparrow_B)$ that describes two spin $^1/_2$ particles possessed by two observers named Alice (A) and Bob (B). If A could find a way to transform $\uparrow_A$ into $\downarrow_A$, while keeping $\downarrow_A$ unchanged, the total wave function would become $^1/_{\surd 2}\downarrow_A(\downarrow_B-\uparrow_B)$, so that B would necessarily find out that the spin of his particle is oriented in the "down" direction when performing his measurement along *x'x*. A and B would therefore be able to communicate. The unitarity of quantum mechanics forbids this to happen, but it is not clear why general relativity should necessarily be constrained to obey to the same rules.

Let us suppose, for instance (cf. Fig. 2), that the components $\uparrow_A$ and $\downarrow_A$ of the wave function $^1/_{\surd 2}(\uparrow_A\downarrow_B-\downarrow_A\uparrow_B)$ are located at an identical altitude *d* of a few kilometers above the earth's surface, within the plane of the equator, along two slightly different meridians, so that the initial distance between $\uparrow_A$ and $\downarrow_A$ reaches, let us say, a few kilometers. We may note the different initial geographical locations of $\uparrow_A$ and $\downarrow_A$ by the respective indices $_{(1)}$ and $_{(2)}$. Components $\downarrow_B$ and $\uparrow_B$ are supposed to be located on the moon or on another planet, where

Bob lives. Let us now suppose that A transforms $\uparrow_{A(1)}$ into $\downarrow_{A(1)}$, while leaving $\downarrow_{A(2)}$ intact. In a second step, she sends both $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ along two gravitational orbits towards the north pole, at the same speed, so that both trajectories meet at the same altitude above the north pole at the same time. According to the equivalence principle of general relativity, both $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ should transform into practically the same wave function $\downarrow_{A(\text{north pole})}$ when they meet above the north pole. Incidentally, this conclusion illustrates how much the principles lying behind the equivalence principle and the unitarity of quantum mechanics are different! One could try to object that when Alice manipulates $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$, her own interaction with $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ may kill the coherence existing between $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$. However, this objection is not compelling, exactly for the same reason as macroscopic mirrors do not kill the coherence existing between different optical sub-beams in optical experiments. If we now suppose that A keeps $\downarrow_{A(\text{north pole})}$ located above the north pole after $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ have merged at time t_m, B should be able to observe that the spin of his particle is oriented towards the "down" direction along $x'x$, starting from the "same time" t_m, although the question of what chronology should be adopted to describe the order of A and B operations is relativistically highly problematic.

Since our example suggests that A and B may be able to communicate with each other at superluminal speeds if the equivalence principle happens to be really universal, most physicists will probably conclude that this example simply demonstrates the limits of the equivalence principle, which needs to be modified by quantum physics in a way that remains to be discovered. This may constitute, indeed, the least expensive way for nature to resolve our problem. Another opposite option consists in trying to maintain the universal validity of the equivalence principle, while modifying the linearity of standard quantum mechanics. This is presumably the option that Penrose would like to follow. A third option may consist in supposing that neither the equivalence principle nor the unitarity of quantum mechanics break down, in which case A and B may presumably be able to communicate at superluminal speeds, which is unfortunately not easy to reconcile with the relativistic principles that one wished to save in the first place. If one insists that the possibility for superluminal communications should remain open at this stage, one has also to face the difficulties raised by strange causality paradoxes (such as: Alice receiving the answer to a question she has not yet raised) which have been discussed in an entertaining way by Davies [16]. From the philosophical point of view, some proponents of absolute determinism may defend the view that causality paradoxes are acceptable, since our impression that the future is determined by the past is itself subjective and no less paradoxical. However, it is unlikely that physicists like Einstein, among other tenants of a certain form of determinism, would have been willing to defend this view. In case superluminal communication is indeed possible, *only one* consistent scenario seems capable to solve the difficulty raised by causality paradoxes without much difficulty: according to this scenario, *one* singular reference frame would impose its *absolute chronology* on *all* events of the universe, and the highest speeds of communications among all different kinds of moving frames would be adjusted accordingly. Concretely speaking, the only reasonable candidate for such a singular reference frame seems to correspond to the frame within which the dipolar anisotropy of the cosmic microwave background radiation is zero. According to the present state of our knowledge, such a scenario may not be completely ruled out, but the new constraint that it would add in our description of the universe appears quite uncongenial to the notion of relativity itself, which does not plead in its favor.

From the experimental point of view, trying to transform $^1/_{\sqrt{2}}(\uparrow_A\downarrow_B-\downarrow_A\uparrow_B)$ into $^1/_{\sqrt{2}}\downarrow_A(\downarrow_B-\uparrow_B)$ through the equivalence principle (cf. Fig.2) would be so challenging that nobody will probably try to perform this test just for the sake of confirming the impossibility of superluminal communications. However, even if it were true that superluminal communications are ruled out by our universe, the question of *why* no one could transform $^1/_{\sqrt{2}}(\uparrow_A\downarrow_B-\downarrow_A\uparrow_B)$ into $^1/_{\sqrt{2}}\downarrow_A(\downarrow_B-\uparrow_B)$ through gravitation would still be worth pondering. Let me stress here that, all things being considered, the *gedanken* experiment described in Fig.2 appears somewhat less inaccessible than *gedanken* experiments concerning black-holes or the big-bang, to which the physics community often limits itself when discussing the relationship between gravity and quantum mechanics. It is also worth stressing that, in contrast with the so-called *Felix* experiment proposed by Penrose (cf. ref. [15], chap. 30.13), the *gedanken* experiment proposed in Fig.2 leads one to challenge the principles of standard quantum mechanics *according to the principles of general relativity itself,* without resorting to any untested assumption. This is the main reason that justifies the need to understand how nature would react to the *gedanken* experiment of Fig.2, independently of any considerations concerning quantum communication.

Conclusion

The present article reiterates the claim, already published in 2005 by the present author, that the constraints of Thomson's version of the second law of thermodynamics can be sidestepped with the help of a scheme that does not try to compete with the randomness of thermal fluctuations, but that simply adjusts their magnitude in a *quasi* cyclical way. The successive thermal *quasi* cycles generated by such a scheme are all macroscopically equivalent from the point of view of the observer, although they all differ mathematically from each other from the microscopic point of view, so that *no perpetual motion* is induced. The validity of this idea has only been discussed from a purely theoretical point of view. In order to test it experimentally, one would need to find a system wherein the magnitude of thermal fluctuations applying at the microscopic level can be conveniently controlled by the observer, which remains a challenging task.

The second problem addressed in the present article, quite unrelated with the first, concerns the coordinated measurement of entangled particles. The author acknowledges plainly that the ideas that he has published on this subject in 2005 and 2006 in the *Revue des Questions Scientifiques* were all wrong. State of the art quantum mechanics offer no way to use entangled EPR pairs to achieve superluminal communication. However, this clear conclusion vanishes as soon as some non-linear gravitational effects are considered. The probability that gravitation could lead to the observation of superluminal effects associated with EPR measurements certainly does not seem very high - although this idea cannot be fully discarded yet. In any case, it can be hoped that the question of *why* and *how* superluminal effects are constrained by both quantum mechanics and gravitation will lead to further progresses in the understanding of what kind of geometry could lead to a better unification of present physical theories.

Figures

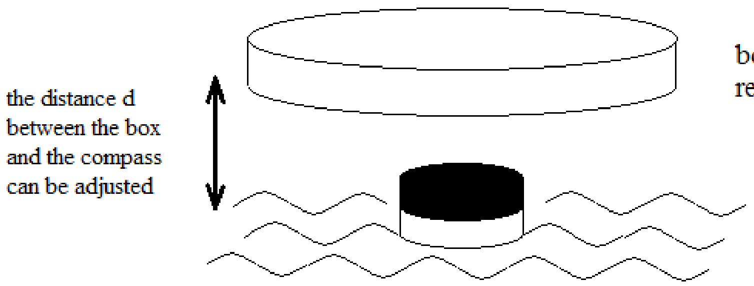

Figure 1: The experimental set-up corresponding to the *gedanken* experiment already proposed in 2005, whose purpose is to sidestep Thomson's formulation of the second law without generating any *perpetual motion*. Each thermodynamic *quasi* cycle" (the word "cycle" being only appropriate from the macroscopic point of view of the observer, since the starting-points of successive cycles are all microscopically different from each other) can be described by the following steps:
[from time t_0 to t_1]: increase of the moment of inertia of the compass;
[from t_1 to t_2]: no external operation;
[from t_2 to t_3]: decrease of the distance *d*;
[from t_3 to t_4]: decrease of the moment of inertia of the compass;
[from t_4 to t_5]: no external operation;
[from t_5 to t_6]: increase of the distance *d*.

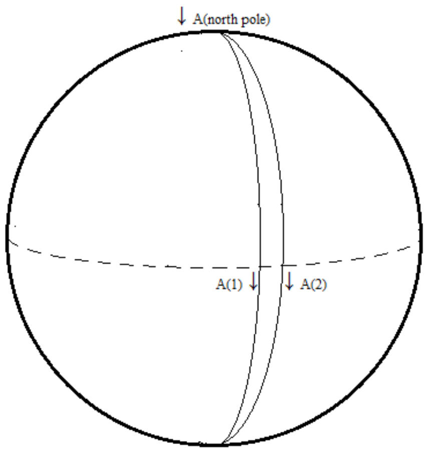

Figure 2: The earth is represented by the sphere on which Alice is living, whereas Bob is supposed to be located far away on another planet. A and B share an entangled pair whose initial wave-function is written $\Psi={}^1/_{\sqrt{2}}(\uparrow_{A(1)}\downarrow_B-\downarrow_{A(2)}\uparrow_B)$, wherein (1) and (2) correspond to two positions separated from a few kilometers above the earth's equator. Alice proceeds to transform Ψ along two successive steps. She first flips the spin of Ψ's component located on site (1), so that Ψ becomes $\Psi={}^1/_{\sqrt{2}}(\downarrow_{A(1)}\downarrow_B-\downarrow_{A(2)}\uparrow_B)$. She then sends both $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ in gravitational orbit towards the north pole, so that when $\downarrow_{A(1)}$ and $\downarrow_{A(2)}$ meet above the north pole, according to the equivalence principle, Ψ should become $\Psi=\downarrow_{A(\text{north pole})}{}^1/_{\sqrt{2}}(\downarrow_B-\uparrow_B)$.

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[4] cf. http://hal.archives-ouvertes.fr/docs/00/09/12/60/PDF/Z-ter.pdf. In fact, this *Erratum* still tried to save the possibility that "immediate" quantum communication may be possible by considering the case of a hydrogen nucleus surrounded by a single electron of wave function: $^{1}/_{\sqrt{2}}(|1s\rangle + e^{i\theta}|2p\rangle)$. The *Erratum* argued that the spatial position of this nucleus is dynamically instable if the energies of $|1s\rangle$ and $|2p\rangle$ are identical and if $e^{i\theta}$ is different from i and –i. If the situation described by $\{^{1}/_{\sqrt{2}}(|1s\rangle + |2p\rangle)$ *or* $^{1}/_{\sqrt{2}}(|1s\rangle - |2p\rangle)$, with a 50% probability for each possibility$\}$ were distinguishable from $\{^{1}/_{\sqrt{2}}(|1s\rangle + i|2p\rangle)$ *or* $^{1}/_{\sqrt{2}}(|1s\rangle - i|2p\rangle)$, with a 50% probability for each possibility$\}$, undelayed quantum communication would be possible. However, this can only happen if the superposition principle and the unitarity of quantum mechanics are not respected. So even this *Erratum* contains errors!

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