Group-theoretic Atemporality in Physics
and its Boundaries

Another look at reversibility, irreversibility, and their philosophical implications

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Abstract: French philosopher H. Bergson criticizes general philosophy insofar as it neglects or even ignores the temporality of time. Concerning general philosophy, Bergson's remarks are probably outdated, whereas contemporary philosophy of science *does continue* to encounter analogous problems.

For essentially group-theoretic reasons, physics, despite the presence of a temporal dimension in physical spaces, describes atemporal systems. These group-theoretic reasons being at the origin of physical atemporality also ensure the extraordinary epistemic power of physics based on the possibility of distortion-free partial approaches, symmetry in prediction and retro-diction, experimentation to be repeated under identical conditions, idealization, renormalization, and so on.

But the investigation field of physics allowing such group-theoretically founded approaches represents a highly improbable exception. So any tentative to transpose physics beyond the boundaries of its group-theoretically delimited investigation field unavoidably leads to the problem raised by Bergson: a time reduced to something without temporality. This point undermines certain contemporary speculations advanced in the name of physics, such as "chaosogenesis" and, above all, linkages between multiverse approaches based on eternal inflation and the so-called "weak anthropic principle."

Keywords: Atemporality, Bergson, the epistemological status of physics, evolution,
group theory, group theory in physics, idealization in physics, irreversibility,
laws of nature, mathematical Platonism, multiverse,
renormalization, renormalization group, reversibility, scientific Platonism,
space-time, temporality, time in physics,
weak anthropic principle (WAP).

0.Introduction

French philosopher H. Bergson wrote that philosophy generally does not grasp what is essential for time. According to author of *L'Evolution créatrice*, time is confused either with the *parameter* t, or with time intervals $|t_b - t_a|$. In our days, this criticism may be outdated. Philosophers coming from various horizons had worked on time *per se*. Nevertheless, specifically in philosophy of physics, the problem raised by Bergson is still topical.

Let us try to identify this problem.

None of us would confuse a point P or a line P_aP_b with the space embedding these figures. Analogously, instants t and durations $|t_b - t_a|$ are not to be confused with time as such. And yet, honestly, can we say that the distinction we make between instants t or durations $|t_b - t_a|$ and time *per se* is as spontaneous as the distinction between points or lines and the space embedding them? Maybe it is better not to be too categorical in this regard. *Denoting* respectively instants and durations, the signs "t" and "$|t_b - t_a|$" nevertheless *evoke* "time." A quick glance at physical terminology to the extent that it concerns us here confirms the foregoing. Randomly chosen, the Wikepedia article about the Minkowski space-time mentions "*time*" as an imaginary fourth *coordinate*. It is also significant that other Wikipedia articles dedicated to special relativity evoke *length* contraction and *time* dilation. We also find there expressions like "'time' registered by clocks" and so on. Of course, in absolute terms, the foregoing does not prove much, and this paper does not have the vocation to compile statistics on usage in the physical literature of "time" instead of "duration", knowing that we do not really need such statistics. It is sure that the reduction of time to duration Bergson had criticized regarding general philosophy manifests itself within physics.

Now, an important point to specify is: *If physics currently uses this kind of misnomer being finally rather comfortable, it is because this choice* – in contrast to what may happen in general philosophy – *does not generate trouble within the investigation field of physics*. In this way, we begin to approach the Bergsonian challenge of this paper. For deep reasons we will have to identify, *the investigation field of physics consists of phenomena where the reduction of time to parameters/variables t or durations* $|t_b - t_a|$ *is not a problem, whereas beyond the boundaries of the investigation field of physics, this kind of reduction leads to anti-scientific inconsistency absolutely analogous to what Bergson criticizes in the area of general philosophy.*

But let us not advance too quickly. First, what should we mean by "the boundaries of the investigation field of physics"? This issue *has* a sense we can find through Bergson's statements about what the current contemporary philosophical jargon denotes by "spatialization of time."

In space – here it can be our intuitive Euclidean space – there is no privileged direction. For any given points P_a, P_b, , … we do not see any intrinsic difference between the translation $P_a \rightarrow P_b$ and its inverse translation $P_b \rightarrow P_a$. Concerning time, it *seems* to be different. "Common sense" and "intuition" tells us that "we cannot go back to the past." From this intuitive perspective, we *would* assert – but please, pay attention to the conditional – that time expresses an intrinsic asymmetry opposed to the symmetry $(P_a \rightarrow P_b) \leftrightarrow (P_b \rightarrow P_a)$ characterizing space. Acting *as if* there were also an analogous symmetry $(t_a \rightarrow t_b) \leftrightarrow (t_b \rightarrow t_a)$, we would "spatialize" the time and so amputate its *temporality* (Bergson 1888, pp. 7f; p.54; pp. 71ff. Touchet, non-dated, pp. 9 ff.).

However, common sense and intuition are not systematically good guidelines. In physics, we are not interested in space, time, space-time or even abstract spaces *per se*, but in phenomena *positioned* in space, time, space-time or abstract spaces. So the expression $(t_a \rightarrow t_b) \leftrightarrow (t_b \rightarrow t_a)$, independently of its pertinence or non-pertinence, does not concern us *directly*. Focusing on *phenomena positioned in time* to be noted in some way like $\phi(t)$, the expression $(t_a \rightarrow t_b) \leftrightarrow (t_b \rightarrow t_a)$ is to be replaced by $(\phi(t_a) \rightarrow \phi(t_b)) \leftrightarrow \phi(t_b) \rightarrow \phi(t_a)$, and from this perspective, temporal asymmetries are not so simple, nor so obvious as common sense and intuition seem to suggest it.

Certainly, in the light cone, any ($\phi(t_a)$, $\phi(t_b)$) appearing to a given observer so that $t_b > t_a$, conserves this temporal order for any observer able to observe ϕ. But physical laws are – nevertheless with a notable exception – symmetrical under prediction and retro-diction. There is no intrinsic difference between a prediction $\phi(t_a) \rightarrow \phi(t_b)$, $t_b > t_a$, and the corresponding retro-diction $\phi(t_b) \rightarrow \phi(t_a)$, $t_b > t_a$. The notable exception is of course *irreversibility*, even if this provisional formulation later will be revised. Anyway, for the moment, let us retain that systems being symmetrical under prediction and retro-diction would evolve "in the same way" if "time was inverted." So, reversible systems are *atemporal*, whereas irreversible systems affected by the asymmetry between $\phi(t_a) \rightarrow \phi(t_b)$, $t_b > t_a$, and $\phi(t_b) \rightarrow \phi(t_a)$, $t_b > t_a$ essentially express temporality.
The foregoing simultaneously allows us to characterize – for the moment just characterize – the boundaries of the investigation field of physics and to correctly transcribe Bergson's criticism into the context of at least ideally reversible/atemporal physics encountering temporality through irreversibility *in a wide sens* (see below), that what necessarily leads to epistemological clashes.
Consisting of ideally reversible phenomena, i.e. phenomena with certain underlying symmetries, the investigation field of physics *stricto sensu* regroups phenomena formalizable by group-theoretic structures. The links between symmetry and group theory still need further considerations, whereas the sole group-theoretic formalizability of a given phenomenon is a necessary but not sufficient precondition for the phenomenon in question to belong to the investigation physics. We will return to these points. But the foregoing already should at least characterize the group-theoretically determined boundaries of the investigation field of physics. Inside the investigation field of physics, the possibility of group-theoretically formalizable approaches *does* ensure atemporality. So, outside, phenomena being non-formalizable in terms of group theory are subject to temporality, knowing that the so-called "physical" irreversibility is just a special case of essentially temporal phenomena. Even if "physical" irreversibility interferes with physics, it remains within the latter a kind of foreign body. What is interesting here is the obvious fact that the foundations of physics implying atemporality inevitably come into conflict with the temporal aspects of the world, as it is reflected by pages and pages dedicated by the philosophy of physics to "physical" irreversibility.
Subsequently, the transcription of Bergson's criticism into the context of at least ideally reversible/atemporal physics encountering temporality profiles itself as follows: *The group-theoretic foundations of physics confer to the latter an extraordinary epistemic power we do not find anywhere else. So, physics can succumb to the temptation to transpose its specific foundations beyond its boundaries. But these essentially reducing approaches not only lead to fragile speculation, but also to characterized epistemological nonsense*.

In this way, the present paper begins through its sections 1.nm to 3.nm by a gradual elucidation of the group-theoretic roots of physical atemporality.
Section 4.nm undertakes a generalization of the so-called "physical" irreversibility comprising any phenomena subject to temporality because of their non-formalizability in group-theoretic terms, before analyzing the exact nature of the unavoidable temporality-atemporability clash.
On this base, section 5.nm first reformulates according to our context Bergson's criticism concerning the spatialization of time, and then investigates two contemporary speculations advanced in the name of physics and being *très à la mode* these days: (i) "chaosogenesis" and (ii) the WAP-fine tuned universe "justified" by the inflation-multiverse hypothesis, knowing that points (i) and (ii) have a common denominator. Whereas the consistency of their respective inferences presupposes strictly group-theoretic frameworks, both approaches deliberately (?) neglect the non-group-theoretic transitions going from non-order to order and so reduce the temporality of time to a misplaced atemporality.

Note that more technical developments of this paper appearing in small characters possibly can be neglected and even ignored.

0.1 Some indispensable terminology clarifications

In our introduction we we used the term "duration" as a synonym for "time interval" $| t_b - t_a |$. The following developments will conserve this current sense of "duration". However, we have to specify that Bergson means by "duration" – *la durée* – something *essentially different*. Moreover, Bergson denotes by "duration" that which, according to him, is the "real time" – *le temps réel* – while relegating parameters/variables t_a, t_b or time intervals $| t_b - t_a |$ to "the time as seen by physicists."(Bergson, 1907, pp.196 ff; Bergson, 1969, pp. 7 ff.) Since "duration" belongs to the main keywords of Bergson's work, it seems legitimate to ask whether the choice to use in this paper "duration" in a sense opposite to that Bergson gives it is really a good idea.
Well, in this context, there is no globally satisfying solution. Bergson's view of time is not an aim *per se* of the present paper. It is rather more a *background motivation* for the elucidation of an issue concerning specifically the investigation field of physics embedded in an universe where the reducibility of any phenomena to physical laws is more (!!) than disputable. So, it is perhaps better for us to use the term "duration" in its current sense shared by physics. On the other hand, Bergson was an authentic pioneer in the area which interests us here. As we mentioned it at the beginning of the introduction, several authors from various horizons meanwhile have addressed the specificity of time; their conceptualization work allows us in our days to denote by "temporality" what Bergson meant a century ago with "duration."
A time interval $| t_b - t_a |$ can be directly measured by clocks. Provided we bear in mind that the possibility of such a measurement does not specify time with respect to space, nothing prevents us from using the term "*duration*" as synonymous with "*time interval*." What actually *expresses* the specificity time with respect to space consists, according to Bergson, of interactions between multiple factors which, measurable or not, are necessarily embedded within and related to a non-specified and non-specifiable *whole*, knowing that the latter point is supposed to generate the irreversibility essentially characterizing temporal phenomena (Bergson, 1907, pp. 18 ff.). On this subject, Bergson evokes "duration", but (i) since we prefer to use "duration" in its current sense and (ii) since "temporality" as a term being firmly rooted in the contemporary philosophical vocabulary denotes what Bergson calls "duration", we attribute in this paper the quality of *temporality* to any phenomenon expressing the specificity of time. It is perhaps a way to favor clarity by the recourse to contemporary terminology without betraying Bergson's thoughts.
That being said, and at the risk of repeating ourselves, the present paper is not about Bergson. Here, we tackle in the context of physics a problem *raised by* Bergson in an essentially different context which does not concern us directly. The present paper analyzes the clash between (i) the characteristic atemporality of the investigation field of physics and (ii) other areas where the illegitimate exportation – or importation – of this atemporality leads to epistemological non-sense.
So, let us begin elucidating the roots of physical atemporality.

1. Irreversibility is not the negation of reversibility

At first glance, the title of the present section seems eminently farfetched. Common sense, practical logic, grammar, orthography and whatever you want tell us that irreversibility is "obviously" the negation, or, if you prefer, the opposite of reversibility.
In fact, it is not so simple, and what we have to approach here will give us the key for our further investigations concerning the temporality/atemporality clash.
First, note the following detail: For any factor A to be the direct negation of the factor B, both

factors A and B must refer to the same type of system. Certainly, common sense still s*eems* to indicate that irreversibility satisfies this condition: A gas constituting an isolated system Σ characterized by its entropy variation $\Delta S \geq 0$ *seems* to remain the "same" system Σ between its initial state Σ_I corresponding to a low entropy S and its final state Σ_F with maximum entropy. And *if* the entropy of the system Σ returned later to its initial minimum value – which is in fact infinitely improbable – should we not still mention the "same" system just evolving differently?
However, contrary to the false evidence of "common sense", the initial state Σ_I and the final state Σ_F of a *same* irreversible *process* does *not* engage a system remaining the "*same*" throughout the transition.
Consider an ideal watch without internal frictions etc. whose needles turn by their own inertia at a constant speed. This system, as long as nothing disturbs it, is reversible with regard to the spatial configuration of its needles; it will return to any configuration it occupies at a given moment.
Under these conditions, the system (i) is characterized by an entropy variation $\Delta S = 0$ and (ii) "remains the same" because it conserves its *functioning mode*. Now let us create an irreversible situation by projecting the system violently to the ground. This time the entropy variation gives $\Delta S > 0$, while the system – reduced to fragments – *does not* conserve its *functioning mode*. Nobody would seriously say that the fragments scattered on the ground are the "same" system as the ideal watch in operating condition.
In a more general way, the ideal reversibility of a physical system $\Sigma \equiv P_i(x_i) = \varphi_i(..., x_j, ...)$, i,j = 1, ... n, *presupposes* the conservation of the functioning mode $fm(\Sigma)$ while its real irreversibility *is* simply the non-conservation of $fm(\Sigma)$. Or, in other words, the conservation of the functioning mode $fm(\Sigma)$ *underlies* the ideal reversibility of a physical system Σ, whereas irreversibility manifests itself *on the level* of $fm(\Sigma)$.
"*Underlying*" v/s "*on the level* of" : this is at least a first indicator denoting that irreversibility is not the direct negation of reversibility. But, obviously, this point must be deepened.
For the moment, let us shorthand "reversibility" by "$fm(\Sigma) \Rightarrow ct$" where and "irreversibility" by "$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$"; where "$\Rightarrow ct$" is just a shorthand sign for "remains unchanged." Nevertheless, the second expression "$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$" is in fact more than simple shorthand. This point will be specified later.

Intuitively, let us say that reversibility and that reversibility and irreversibility are two essentially different aspects of the universe. Because of their essential differences, irreversibility can not be the negation of reversibility and vice versa. Precisely this paper tries to show that ideal reversibility is a perhaps insufficient but absolutely necessary precondition for physics, and that subsequently the passage from ideally reversible phenomena to intrinsically irreversible ones is equivalent to the passage from physics to non-physics, with the well-known adverse consequences concerning the Second Law of Thermodynamics, which as a kind of foreign body within physics is the source of endless discussions.

2. The precondition for reversibility; deepening the assertion "Irreversibility is not the negation of reversibility."

Before turning to the highly special epistemological status of physics within human knowledge, we have to elucidate the formal conditions for a given system to conserve its functioning mode through change. Admit in a general way that a system consists of a certain number of transformations. In our context, the precise "nature" of these transformations can be neglected. By contrast, we must focus on the possibility of seeing a given transformation transformed into another one. Now, under which conditions does a system conserve its identity through all its transformations which are being

transformed in other transformations?
There is a concise answer: The system in question must be ultimately formalizable in terms of the Klein **V**-group. Indeed, the **V**-group consisting of four transformations e, f, g, I, where any combination of transformations belonging to **V** leads to a transformation belonging to **V**, knowing that I is the *identity transformation* which conserves identically any transformation combined with it. The Klein **V**-group has a remarkable property: the combination of all transformations belonging to **V** is equal to I: $I \perp e \perp g \perp f = I$

A bit more technically, note the Klein four-group $\mathbf{V} = (\{I, e, f, g\}, \perp)$. The four elements I, e, f, g are transformations. $\perp$ symbolizes the transformation of any $a \in \{I, e, f, g\}$ into a transformation $b \in \{I, e, f, g\}$. "I" designates the identity transformation such as $\forall a \in \{I, e, f, g\}$, $I \perp a = a \perp I = a$. For e, f, g, we set $\forall a \in \{I, e, f, g\}$, $a \perp a = I$ and $e \perp f = f \perp e = g$, $f \perp g = g \perp f = e$, $g \perp e = e \perp g = f$. It is easy to show that $I \perp e \perp g \perp f = I$. In fact, $I \perp e = e$, so $(I \perp e) \perp f = e \perp f = g$. Since $I \perp e \perp f = g$, $(I \perp e \perp f) \perp g = g \perp g = I$.

The sentence $I \perp e \perp g \perp f = I$ *formalizes any system remaining* identical *through all its transformations*.
So, concerning reversibility, we obtain $(fm(\Sigma) \Rightarrow ct) \Leftrightarrow (fm(\Sigma) \Rightarrow \mathbf{V})$. Any form of irreversibility in contrast expresses the transition $\mathbf{V} \rightarrow$ non-**V**. The functioning mode $fm(\Sigma)$ of the system Σ is obviously an *intrinsic* property of Σ, whereas the transition $\mathbf{V} \rightarrow$ non-**V** occurs *independently of the intrinsic* properties of the concerned system Σ. Consider several systems with very different properties: a watch, a car engine, our Earth with its moon revolving around it etc. Let us smash this watch against a wall, forget to put oil in the car engine, imagine that a mega-meteorite pulverizes our Earth. The result implies in all cases the *same* transition $\mathbf{V} \rightarrow$ non-**V**.
So we have a compact formal framework determining whether a given system is reversible or irreversible, whereas it seems proved that reversibility and irreversibility, epistemologically speaking, represent two different things.

3. The particular epistemological status of physics and its group-theoretic roots

The laws of physics – with the notable exception of the Second Law of Thermodynamics, but we will come back to the thorny status of irreversibility *within* physics – are *ideally* reversible. So any physical system, always with a significant exception, *ideally* specifies in one or another way the Klein **V**-group. Since the latter, as its name indicates, is a group, *ideally* reversible physical systems necessarily express group-theoretic structures. As such, the determining importance of group theory – we find presentations of its extent in (Tegmark, 2007, pp. 5 ff.) or (Yanofsky and Selcer, 2013, pp.3 ff.) – is not astonishing; we will see it through the following developments. What is astonishing – "infinitely" astonishing – is the fact that *there are* phenomena consistent with a group-theoretic approach.

3.1 "Repeating an experiment under identical conditions"

Every high school student knows – or at least should know – that the scientific validation of an experiment presupposes among others the possibility of repeating this experiment as often as we want under identical conditions.
At first glance, this point seems trivial. And yet, there is a fundamental problem making the only possibility of experimenting "under identical conditions" highly exceptional. On the one hand, the context of a given phenomenon is constantly evolving. It is useless to repeat once again the (too) famous Heraclitean quotation. So, if we want to experiment "under identical conditions", we have to isolate the phenomenon to be investigated from its constantly evolving context. On the other

hand, we do not need to be great Hegelians to realize that the knowledge of a phenomenon taken out of its context is *intrinsically distorted* knowledge. Subsequently, in principle, *intrinsic distortion* should be the price to pay for "experimenting under identical conditions."

Now, on the one hand, physics certainly encounters superficial distortion due to experimental errors, disturbing factors and so on, but *no intrinsic distortion* at the level of its laws which are discovered despite any contingency hampering physical research. On the other hand, there is one and only one possibility allowing the partial *and* distortion-free approach of a whole consisting on related factors: Such a distortion-free approach is possible if and only if (i) the global system Σ is formalized by a group G, *and* (ii) the partial systems Σ_i, Σ_j , … of Σ are formalized by groups G_i, G_j , … being subgroups of G, since any subgroup G_i, G_j , … of G conserves identically over its domain the properties it shares with G.

This first – and elementary – point is not the only one which denotes the essential role group theory plays in physics. There are others.

3.2 Idealization

Of course, physical laws are idealization. It would be highly improbable that we find in the physical reality some phenomenon expressing the corresponding physical law in its purest state. According to certain voices, physics is "just idealization", or "only idealization."

In fact, the very *possibility* of idealization in physics represents, epistemologically speaking, a unique situation and explains in turn a good part of the epistemic power distinguishing physics from any other science.

Concerning physics, idealization consists at least in a first approximation of the *suppression* of physical factors which – being contingent or not – dismiss the actual "behavior" of a given phenomenon from the law describing it "theoretically." Here below, you find a little more technical and so a little more exact approach of idealization. If you prefer skip this passage, please retain in the preceding sentence the noun "suppression." We will have to come back to the notion of suppression after the following technical note.

Let us admit that a physical phenomenon **e** is expressed – in a pertinent way *or not* (see below) – by a mathematical entity $P(\mathbf{e})$ taking the form $P_i(x_i) = \varphi_i(..., x_j , ...)$, i,j = 1, ... n,m. Measuring empirically the phenomenon **e**, we will find in most cases for each e_i, e_j a higher or lower deviation Δx_i, Δx_j, … from the theoretical values x_i, x_j … Now, *idealization* consists among other of the *suppression* of factors supposed to play an effective role concerning the gap Δx_i, Δx_j, … between the theoretical x_i, x_j … and their empirical measures. These factors can be frictions, diverse kinds of energy loss and so on. Note that idealization also can comprise the passage of "very large" values to infinity and analogous choices, but a more detailed approach of this point would lead us too far. Anyway, the suppression of friction and so on eventually completed by other choices implies *ipso facto* corrections δx_i, δx_j, … rectifying *as well as possible* the $(x_i + \Delta x_i)$, $(x_j + \Delta x_j)$, … in form of $(x_i + \Delta x_i +\delta x_i)$, $(x_j + \Delta x_j +\delta x_j)$, … Of course, despite these corrections δx_i, δx_j, … , we generally do *not* obtain $(x_i + \Delta x_i +\delta x_i) = x_i$, $(x_j + \Delta x_j +\delta x_j) = x_j$, … as the corresponding law $P(\mathbf{e})$ predict it. Indeed, the gaps Δx_i, Δx_j, … result not only from in principle corrigible parasite factors such as friction and so on, but also from experimental errors and/or measurement problems. But *if* the sufficiently often repeated experiment makes tend the $(x_i + \Delta x_i +\delta x_i)$, $(x_j + \Delta x_j +\delta x_j)$, … to the *limits* x_i, x_j, … predicted by the law $P(\mathbf{e})$, the idealization in question is *operating* and so validates at least in a given context the law $P(\mathbf{e})$ we initially qualified as not necessarily pertinent.

In fact, it is more complicated. Norton advances an exceptionally exhaustive analysis of idealization in physics, distinguishing above all "idealization" and "approximation" (Norton, 2011,pp.3 ff.; pp.6 ff.). Nevertheless, the foregoing gives us the the essential minimum we need in this little paper.

Now, whether we skipped the (a bit more) technical note or not, we have to retain the following point. Consisting – among others – of the suppression of certain undesirable factors, idealization is a manner of putting the corresponding phenomenon out of its global context. In subsection 3.1 we had specified that this way of proceeding generally led to *intrinsic distortion*. However, within the investigation field of physics, idealization, far from leading to distortion, *extracts* physical laws

from their disturbing context by setting *invariant limits* to experimental fluctuations.
Since only mathematical groups admit a distortion-free partial investigation of related factors, the fundamental role of physical idealization is in turn an expression of the group-theoretic foundations of physics.
Note that a more rigorous group-theoretic approach of idealization obviously would require the recourse to renormalization groups. We will effectively evoke the notion of renormalization group in subsection 3.33, but deepening the links between renormalization and physical idealization would overload this paper.

3.3 Touching on other group-theoretic roots of physics

3.31 Paradigm shifts and unity of physics: Approaching the issue of paradigm shifts, we can literally retake the group-theoretic considerations we advanced in subsection 3.1 about distortion-free partial approaches. Nevertheless, we have to distinguish the "classical" and the "non-classical" conception paradigm shifts. According to the "classical" conception, the occurrence of a new paradigm implies the transition from the ancient theory T_i to a larger theory T_{i+1} so that $T_i \subset T_{i+1}$. The foregoing seems trivial, but it is not so. Within the expression "$T_i \subset T_{i+1}$", T_i appears as a partial consideration of T_{i+1}. Instead of being a particular case of T_{i+1}, T_i in principle should be affected by distortion, constituting a foreign body within T_{i+1}. Since in physics – at least in the part of physics where the "classical" view of paradigm shifts is pertinent (see below) – the transition from T_i to T_{i+1} conserves T_i in a distortion-free way, the concerned domain necessarily is formalizable in terms of appropriated mathematical groups. More generally, for a *series* of "classical" paradigm shifts implying the corresponding theory transitions $T_i \rightarrow T_j$ so that $j > i \Rightarrow T_i \subset T_j$, there must be for any i, j, … a group G_j formalizing T_j so that G_i as a subgroup of G_j formalizes T_i. The standard example of "classical" paradigm shifts consists of the transition from Newtonian mechanics to SR, and from SR to RG, the Galileo group being a subgroup of the Lorentz-Poincaré group, whereas the latter is a local case of RG-manifolds. But now we have to recall that the "classical" paradigm shift schema *does not* cover the general case of paradigm shifts. With regard to quantum physics, reducing the Planck constant h to zero certainly allows the derivation of the current macro-physical laws, but this is *not* sufficient to say that macro-physics is just a particular case of quantum physics according to the"classical" schema $T_i \subset T_j$. (D'Ariano, 2015, pp.1f.) All these endless more or less speculative controversies revolving around wave function collapse, decoherence, Everett parallel universes generation and so on are the expression of an emergence problem between the quantum and macro scales (see Landsman, 2005, *passim*). If in this case we want to conserve the schema $T_i \subset T_j$, its "classical" interpretation in terms of "one" theory included in "one" other broader theory is too reducing. In $T_i \subset T_j$, at least T_j, must be reinterpreted as an "inter-theoretic construction" consisting on several theories … T_{jr}, T_{js}, … being related. Note that for reasons of generality, it is better to consider T_i in turn as an "inter-theoretic construction" on the basis of related theories … T_{iu}, T_{iv}, … , without excluding the particular case of a homogenous T_i. But what could we understand by inter-theoretic constructions consisting of "related theories"? There is doubtlessly an infinity of possibilities to "relate" theories being perhaps very different *per se*. However, we certainly cannot admit *just any* manner to relate theories in order to obtain a whole supposed to belong to physics. A relatively intuitive way to delimit inter-theoretic constructions consistent with physics is – at least indirectly – given by Butterfield's Nagel-inspired approach of the emergence issue. (Butterfield, 2013, pp.6 ff.; Butterfield and Bouatta, 2013, pp. 37 ff.) Recall that according to "reductionism", complex phenomena can be "reduced" to more fundamental ones. So, for "physical reductionism", or "physicalism", any phenomenon is ultimately reducible to physical phenomena. Now, other voices assert that there are phenomena which are not reducible to physics. From this *standpoint*, and *referring to physics*, these phenomena are "emergent", i.e. they exist but we cannot not really explain by the sole laws of physics where they come from. Emergence is the subject of endless debates. The reference paper of Paul Humphreys (Humphreys, 1997), the response to Humphreys by Frederick Kronz and Justin Tiehen

(Kronz and Tiehen, 2002), or, in another register, the critical overview of Peter Corning (Corning, 2002) reveal the "infinite" complexity of this issue. On the one hand, emergence, since it carries a substantial dose of epistemological fragility, does not seem to be very popular among physicists. On the other hand, it would it be rather dogmatic to retain physical reductionism unreservedly. Butterfield, while recognizing that physical reductionism in its "classical" form obviously raises serious problems, advances following Nagel the possibility to redefine physical reductionism in an inter-theoretic way. Complex phenomena effectively are irreducible to physical theories as they have traditionally been developed through last centuries, could be reduced to inter-theoretic constructions elaborated on the base of physical theories. In fact, the formulation "on the basis of physical theories" is not very pertinent, and in a following more technical note we will precise why. But for the moment, let us admit this formulation, considering such an inter-theoretic construction T_j we suppose obtained "on the basis" of theories $\dots\ T_{jr}, T_{js}, \dots, T_{ju}, T_{jv}, \dots$ To escape the problem of a too extensive conception of inter-theoretic constructions, Butterfield, following Nagel, impose the constraint that the theories $\dots\ T_{jr}, T_{js}, \dots, T_{ju}, T_{jv}, \dots$ must be related by "bridge laws", "bridge principles" or "correspondence rules" relevant to these theories, without necessarily belonging to them. (For more details, see Butterfield and Bouatta, 2014, pp. 37 ff.) Nevertheless, in order to remain consistent with ourselves, we have to require that within the inter-theoretic construction T_j, any theory among the $\dots\ T_{jr}, T_{js}, \dots, T_{ju}, T_{jv}, \dots$ can be *individually* identified. If this constraint is not satisfied, we fall back into emergence physicists refuses as mysticism. Indeed, if we conceive T_j as an inter-theoretic construction obtained "on the basis" of theories $\dots\ T_{jr}, T_{js}, \dots, T_{ju}, T_{jv}, \dots$ without being able to identify each T_{jg} individually, i.e. separately from each T_{jh}, g ≠ h, our conception resembles something like "The whole is greater than the sum of its parts", where we recognize the Aristotle-inspired "fundamental postulate" of "general systems theory." Now, this postulate – with some reservations concerning the term "sum" – is not meaningless, quite the contrary. Yes, but physics *as such* does not know what to do with the fundamental postulate of "general systems theory", and this exactly for the same reasons which *make emergence suspect to physics*. On the other hand, the individual identification of any T_{jg}, T_{jh} within T_j is necessarily a partial consideration of T_j, and so we encounter once again the distortion problem intrinsically related to partial considerations. Subsequently, a distortion-free individual identification of any T_{jg}, T_{jh} within T_j presupposes the existence of a group G_j formalizing T_j so that each T_{jg}, T_{jh} is formalized by a group G_{jg}, G_{jh} being a subgroup of G_j. Even if we reinterpret the "classical" paradigm shift schema $T_i \subset T_{i+1}$ by a "non-classical" paradigm shift schema where at least the theory T_{i+1} is in fact an inter-theoretic construction, the group-theoretic constraints we met above manifestly are conserved.

A little more technical approach of inter-theoretic constructions confirms the foregoing.
The formulation "inter-theoretic construction T_j obtained '*on the base*' of theories $\dots\ T_{jr}, T_{js}, \dots, T_{ju}, T_{jv}, \dots$" we used above for evident reasons of simplicity finally is not very significant. To obtain an appropriated inter-theoretic construction, we have to work with intersections $T_{jr} \cap T_{js}$, unions $T_{ju} \cup T_{jv}$, intersections of unions $(T_{jr} \cup T_{js}) \cap (T_{ju} \cup T_{jv})$, unions of intersections $(T_{jr} \cap T_{js}) \cup (T_{ju} \cap T_{jv})$ and so on, where all theses items are linked by *operations* and/or *transformations*. Now, the idea of intersections, or unions and so on of *theories has all chances* to be problematic. Intersections or unions and so on initially concern *sets*, whereas the extension of these notions to theories – and other forms of organized complexity – generally risks resembling the non-void "intersection" of a bird and a fish consisting of the molecules effectively shared by both. Manifestly, the use of set-theoretic operations seems – at least in the general case – not very appropriate to theories. And yet, *there are* solutions, however affected by highly restrictive constraints. Let us admit that the notion of mathematical structure represents the minimal form of *organized* sets. Since a structure $(\mathbf{S}, \perp)$ is a *set* $\mathbf{S}$ fitted by any mean $\perp$ allowing to connect the elements of $\mathbf{S}$ – the symbol $\perp$ can be for an operation, a transformation, an order and so on – set-theoretic notations such as $(\mathbf{S}_{jr}, \perp_{jr}) \cup (\mathbf{S}_{js}, \perp_{js})$ or $((\mathbf{S}_{jr}, \perp_{jr}) \cup (\mathbf{S}_{js}, \perp_{js})) \cap ((\mathbf{S}_{ju}, \perp_{ju}) \cup (\mathbf{S}_{jv}, \perp_{jv})), \dots \dots$ are legitimate, provided the connectors $\perp_{gh}$ go together without any inconsistency. Excepted simple cases like "$(\mathbf{S}_{jr}, \perp) \subset (\mathbf{S}_{js}, \perp)$" where the *same* connector $\perp$ is adequately defined over $\mathbf{S}_{jr}$ and $\mathbf{S}_{js}$, knowing that $\mathbf{S}_{jr}$ as such is effectively included in $\mathbf{S}_{js}$, it is sometime hard, but not *a priori* impossible to satisfy the latter constraint for the general case comprising sets $\mathbf{S}_{jr}$, $\mathbf{S}_{js}$ fitted with *different* connectors $\perp_{jr}, \perp_{js}$. Setting for example $\perp_{j1} \stackrel{\text{def}}{=} (a+b)(1+c)$, $c \in \{0, -1\}$ and $\perp_{j2} \stackrel{\text{def}}{=} +$, while presupposing that $\perp_{j1}$ and $\perp_{j2}$ are defined

respectively over $\mathbb{R}$ and $\mathbb{N}$, there is nothing preventing us from writing correctly $(\mathbb{N}, \perp_{j2}) \subset (\mathbb{R}, \perp_{j1})$ or $(\mathbb{N}, \perp_{j2}) \cap (\mathbb{R}, \perp_{j1}) = (\mathbb{N}, \perp_{j2})$.
Now, in physics we need absolutely distortion-free and subsequently reversible structures, knowing that reversible structures are groups. On the other hand, as everybody knows, groups are particular cases of mathematical structures. All we said about the general case $(\mathbf{S}, \perp)$ with regard to the legitimate extension of set-theoretic operations required for inter-theoretic constructions, can be repeated for groups, however with a *very restrictive supplementary constraint*: For expressions like $(\mathbf{S}_{jr}, \perp_{jr}) \cup (\mathbf{S}_{js}, \perp_{js})$ or $(\mathbf{S}_{jr}, \perp_{jr}) \subset (\mathbf{S}_{js}, \perp_{js})$ and so on, where $\mathbf{S}_{jr}$, $\mathbf{S}_{js}$ are groups, to be consistent, the connectors $\perp_{jr}, \perp_{js}, \ldots$ must in turn belong to corresponding transformation groups $\boldsymbol{G}_j : \perp_{jr} \rightarrow \perp_{js}$.
So, provided the foregoing condition is satisfied, the group-theoretic foundations of physics represent the common response to the double constraint being related (i) to the necessity of distortion-free partial considerations and (ii) to the in turn necessary extension of set-theoretic operations to inter-theoretic constructions.

3.32 Group theory and symmetry breaking: Touching upon other aspects of group-theoretic foundations of physics, we also have to consider a case which apparently contradicts the foregoing, or at least could potentially generate serious misunderstandings.
The language of physics, in first approximation, denotes by "symmetry" all characteristics of physical systems which are formalizable by mathematical groups. Now there are numerous physical systems – from macro- to subatomic physics – which are the support of "symmetry breaking." This latter term is not without ambiguity (Castellani, 2002, p.1). "Symmetry breaking" could – but please pay attention to the conditional – suggest the idea of a passage from symmetry to asymmetry, i.e. a transition $\mathbf{V} \rightarrow$ non-$\mathbf{V}$. Now we have defined *irreversibility* by the same expression $\mathbf{V} \rightarrow$ non-$\mathbf{V}$. (See section 2) At first glance, this definition – but once again, we use the conditional – could imply an insalubrious consequence: "Irreversibility" and "symmetry breaking" are synonyms. Trying to show that physics is essentially based on group-theoretic foundations, we apparently encounter substantial troubles, since symmetry breaking phenomena seem to undermine physics by "non-group theory."
In fact, symmetry break is *not* the passage from symmetry to asymmetry. Moreover, symmetry break theory *does* belong to advanced group theory. Such as it has been formulated in a very adequate way by Elena Castellani, "(...) the situation where this symmetry is broken is characterized by a lower symmetry than the situation where this symmetry is not broken. In *group-theoretic* terms, this means that the *initial symmetry group is broken into one of its subgroups*. It is therefore possible to describe symmetry breaking in terms of relations between transformation groups, in particular between a *group* (the unbroken symmetry group) and its *subgroup(s)*." (Castellani, 2002, p.2; my own emphasis; comp. Yanofsky and Selcer, 2013, p. 6). Here we recognize spontaneously a possible interpretation – not the unique one, but a possible interpretation among others – of the principal constraint striking inter-theoretic constructions T_j which, to belong to physics, must be formalizable in terms of a group G_j specifying the Klein $\mathbf{V}$-group, so that each inter-theoretic component T_{jk} of T_j is formalizable by a sub-group G_{jk}, $G_{jk} \subset G_i$.

3.33 Renormalization and renormalization groups: Contemporary physics would not be imaginable without renormalization. Initially conceived as an purely heuristic tool (Butterfield, 2013, pp. 10 ff.) supposed to master "as well as possible" some inextricable problems emanating for example from too-high energies inaccessible to experimental means, renormalization is nowadays an intrinsic part of the conceptual edifice of advanced physics, modifying – among other things – what epistemology traditionally meant by objectivity and subjectivity (comp. Lesne,1999, p. 4; comp. Butterfield, 2013, pp. 10 ff.), playing doubtlessly a significant role concerning the epistemic shift which seems to be coming on the horizon. In our context, the technical nature of renormalization has a deep sense. In a first approximation, renormalization consists of the "amputation" within a model supposed to represent a physical phenomenon, of certain factors leading to undesirable situations like "infinite quantities of energy" or senseless expressions such as ∞/∞ and so on.
Now – our discourse becomes routine – an amputated model of a given phenomenon in the general case would be *distorted*. Such distorted models would have condemned renormalization to

remain a purely heuristic approach. However, this is not the case. In its own way, renormalization leads to a new kind of distortion-free exactitude. Below, a little note furnishes you some details. But anyway, the distortion-free aspect of renormalization advocates in turn group-theoretic foundations of physics.

Following A. Lesne (Lesne, 1999), let us try to identify the epistemological interest of renormalization through a current example on quantum field theory encountering the very high energy problem. The first step in any QFT approach is necessarily the adoption of a model $\mathcal{M}$ of the physical system to be investigated. But immediately a first difficulty arises. On the one hand, absolutely speaking, there is no objective factor justifying any upper limit of the model $\mathcal{M}$ with regard to the energies/frequencies engaged within the system to be modeled; so we adopt an initial model without an upper limit we note $\mathcal{M}_\infty$. On the other hand, the model $\mathcal{M}_\infty$ is necessarily distorted. Our experimental knowledge about the very high energies/frequencies is absolutely insufficient, and even non-existent. The model $\mathcal{M}_\infty$ consists merely of an extrapolation of phenomena known up to arbitrarily large frequencies and wave vectors. Note $\mathcal{M}_\Omega$ any model with an upper limit Ω of frequencies and characterized by parameters $\theta^R(\Omega)$ we qualify as "parameters fixed according to Ω." Beyond $\mathcal{M}_\Omega$, $\mathcal{M}_\infty$ expresses so-called "ultra-violet divergences" designating not a serious disturbance of the system to describe, but merely the dubious adequacy *as such* of $\mathcal{M}_\infty$ regarding the description of the system in question when the limitation Ω of energies/frequencies is violated. (comp. Lesne, 1999, p. 4). For its part, $\mathcal{M}_\Omega$ escapes the "ultraviolet divergences", since *by construction*, $\mathcal{M}_\Omega$ takes into account exclusively frequency phenomena ω such as $|\omega| < \Omega$, *whereas* for $|\omega| > \Omega$, all phenomena ω related to the "parameters fixed according to Ω" we noted here above "$\theta^R(\Omega)$" become the object of *conjectures*. If we now consider a series of upper limits $\ldots, \Omega_i, \ldots \Omega_j, \ldots, j > i$, then there is no objective reason to privilege any framework $\theta^R(\Omega_i)$ over any other $\theta^R(\Omega_j)$. From a "*classical*" viewpoint, *here*, seen from a "'pre-renormalization' viewpoint", *any* choice Ω_i represents an *approximation* to Ω_j, $j > i$, while *all* choices $\ldots, \Omega_i, \ldots \Omega_j, \ldots$ fall globally under *subjectivity.* (comp. Lesne, 1999, p. 4) m
It is however precisely at this level that renormalization expresses a deep perspective shift. Indeed, some relations between the choices Ω_i, Ω_j, can lead to intrinsic properties of the system. This becomes effectively possible, if there is a *transformation* $\mathcal{R}_{\Omega i,\Omega j}$ determining the modeling change $\mathcal{M}_{\Omega i} \rightarrow \mathcal{M}_{\Omega j}$ when we pass from Ω_i to Ω_j, $i \neq j$, with the two additional conditions (i) that $\mathcal{R}_{\Omega i,\Omega j}$ constitutes necessarily a distortion-free one-to-one relation between the (Ω_i, Ω_j) and the $(\mathcal{M}_{\Omega i}, \mathcal{M}_{\Omega j})$, and (ii) that for any i,j, the properties of $\mathcal{R}$ are conserved for $(\mathcal{M}_{\Omega i}, \mathcal{M}^*)$, where $\mathcal{M}^*$ is the model corresponding to a given "critical point" not to be exceeded. *If* (!) such a relation $\mathcal{R}_{\Omega i,\Omega j} : \mathcal{M}_{\Omega i} \rightarrow \mathcal{M}_{\Omega j}$ exists, let us call it the renormalization of $\mathcal{M}_{\Omega j}$ by $\mathcal{M}_{\Omega i}$. Now, it is clear that to be able to link in a distortion free way *any* $\mathcal{M}_{\Omega i}$, $\mathcal{M}_{\Omega j}$ in both senses $\mathcal{R}_{\Omega i,\Omega j} : \mathcal{M}_{\Omega i} \rightarrow \mathcal{M}_{\Omega j}$ and $\mathcal{R}_{\Omega j,\Omega i} : \mathcal{M}_{\Omega j} \rightarrow \mathcal{M}_{\Omega i}$, the set of all models $\ldots \mathcal{M}_{\Omega i}, \ldots \mathcal{M}_{\Omega j} \ldots$ defined through their respective upper limits $\ldots, \Omega_i, \ldots \Omega_j, \ldots$ have to be the support of a transformation group $(\boldsymbol{\Omega}, \boldsymbol{\mathcal{R}})$ we call "*renormalization group*." (Also comp. Reutlinger, 2014, pp.10 ff.)
Since experience denotes that *there are* effectively renormalization groups being suitable for a vast range of *essentially different* physical systems going from QED, QFT or QCD to absolutely tangible fields like polymerization (Lesne, 1999, p. 5), renormalization contributes a major part to the emergence in physics of a new kind of immaterial second order objectivity (comp. Butterfield, 2013, pp. 10 ff.) expressing perhaps some aspects of an epistemic shift seeming to be in the air. But there are also good chances to see renormalization groups ensure the unity of physics beyond the unprecedented upheavals physics probably will have encountered in the very near future.

3.4 A first recapitulation

Physics is possible because *there is* an investigation field consisting on phenomena being formalizable in the way we describe here above. The existence of such phenomena is not self-evident. On the contrary, the very fact that *there are* phenomena formalizable in terms of G and constituted by sub-phenomena formalized by G_i, G_j, … so that $G_i \subset G$, $G_j \subset G$, … represents the essential part of the "infinite" improbability separating the investigation field of physics from what does not belong to it.
Since the investigation field of physics implies phenomena which are formalizable in terms of group-theory – with the notable and necessarily problematic exception of the so-called "physical"

irreversibility (see below) – the non-formalizability of a given phenomenon in terms of group theory is – but once again we have to insist on the presence of an undesirable exception – incompatible with the belonging of the phenomenon in question to the investigation field of physics. Subsequent to the "infinite" improbability separating the investigation field of physics from what does not belong to it, the separation between physics and non-physics is very sharp. Certainly, we can find outside of physics phenomena being group-theoretically formalizable, however, in this case group-theory is not necessarily among the foundations of the corresponding scientific edifice.
Time is an essential dimension within physics. But since all physical dimensions, *comprising time*, are covered by group-theoretic structures, physics – apart from the above mentioned exception – is intrinsically atemporal.

4. Physical atemporality v/s Bergsonian temporality

Following the foregoing, the group-theoretically formalizable and subsequently atemporal investigation field of physics is embedded in a world submitted to temporality. On the other hand, physics, for identical group-theoretic reasons, is simultaneously characterized by its "infinitely" high improbability and its more than astonishing epistemic power, notably with regard to prediction, and more precisely to symmetry between prediction and retro-diction. For the same reasons, it is "infinitely" improbable that other sciences with their respective investigation fields could reach an epistemic power comparable to that of physics. Now – once again, please pay attention to the conditional – *if* such a choice were possible, it would be pleasant for scientific activities outside of physics to "import" from the latter its functioning mode. But this is not the case. Any tentative to transpose physics to non-physics unavoidably leads to a clash between group-theoretic atemporality and the temporality characterizing all that which does not belong to the investigation field of physics.
The foregoing represents a major problem for contemporary philosophy beginning with physics itself when it has to *integrate* irreversibility, and continuing in all fields where something equivalent to the epistemic power of physics is felt to be missing.

4.1 Irreversibility and its generalization

In section 1, we had identified reversibility as the conservation by a given system of its functioning mode through change, shorthanding "functioning mode of a system Σ" by "$fm(\Sigma)$", "the conservation of $fm(\Sigma)$ by an ideally reversible system Σ" by "$(fm(\Sigma) \Rightarrow ct)$", and "irreversibility" by "$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$." Within the latter expression, the term "$fm(\Sigma) \Rightarrow ct$" at the left of the transition arrow is absolutely necessary to denote an essential problem concerning irreversibility: handbook chapters supposed to characterize irreversibility as a phenomenon *de facto* use formulations resembling the following: "The motion of all molecules constituting an initially ordered gas contained within an enclosure is 'in principle' governed by *reversible* Newtonian mechanics. So, the gas in question evolving with time theoretically should return its initial ordered state, whereas 'in fact' we always observe a transition from order to disorder." So, irreversible transitions necessarily contain at least implicitly the term "$fm(\Sigma) \Rightarrow ct$".
In section 1. we saw that irreversibility, far from being the direct negation of reversibility, is *undermining* ideally reversible systems, or superposed to them, if you prefer. Indeed, the *current* interpretation of "$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$" in terms of a transition from order to disorder *as it occurs in physics* is just a particular possibility among an infinity of essentially different potential interpretations. First, recall that in the physical translation of

"$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$", i.e. the Clausius formula $\Delta S \geq 0$ – or rather the Clausius *formulation*? – the entropy S has the dimension of an energy. Now let us shuffle a card pack being initially ordered in some way. Here again, the variation of the *internal* state of the card pack consists of an irreversible transition from order to disorder, however without implying any energy dimension, since this process can be described *for example* in terms of the physically dimensionless Shannon entropy. Manifestly, irreversibility does not exclusively concern physics, unless we want to qualify as "physics" everything and its opposite.

Now, there is a more important point to consider. Formally speaking, the non-conservation of $(fm(\Sigma) \Rightarrow ct)$, beyond the usual interpretation of "$(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$" as the transition from order to disorder, can also take the form of a transition, for the system Σ, from a given functioning mode fm to another one which we will note for the moment fm'. Provided both fm and fm' are constant during certain time intervals, *this kind of* non-conservation of $fm(\Sigma)$ can be written "$(fm(\Sigma) \Rightarrow ct) \rightarrow (fm'(\Sigma) \Rightarrow ct)$", knowing that the latter expression, by its nature, is to be generalized to "…, $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$, …, $(fm_j(\Sigma) \Rightarrow ct) \rightarrow$ $\rightarrow (fm_{j+1}(\Sigma) \Rightarrow ct)$, …" That being said, there is, *at a purely formal level*, no intrinsic reason to exclude *a priori* the possibility that some transitions $(fm_u(\Sigma) \Rightarrow ct) \rightarrow (fm_{u+1}(\Sigma) \Rightarrow ct)$, transpose the system from a *lower* organization state $fm_u(\Sigma)$ to a *higher* one $fm_{u+1}(\Sigma)$.

The foregoing obviously leads to a well known problem. On the one hand, "current" irreversibility expressed by $(fm(\Sigma) \Rightarrow ct) \rightarrow$ non-$(fm(\Sigma) \Rightarrow ct)$ is just a special case of the more general formulation …, $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$, …, $(fm_j(\Sigma) \Rightarrow ct) \rightarrow (fm_{j+1}(\Sigma) \Rightarrow ct)$, … On the other hand, the general formulation comprises transitions $(fm_u(\Sigma) \Rightarrow ct) \rightarrow (fm_{u+1}(\Sigma) \Rightarrow ct)$ *increasing* the organization of the system Σ, as it is *currently* observed in biology, while "*current*" irreversibility attributes *exactly to this kind* of transitions $(fm_u(\Sigma) \Rightarrow ct) \rightarrow (fm_{u+1}(\Sigma) \Rightarrow ct)$ "infinitely" high improbabilities, so that these transitions – *de facto* – never occur.

How can it be possible that something current *in biology is* currently *to be excluded in physics*? Unless we blindly admit in the field of biological evolution the occurrence of quasi infinite improbabilities we never would envisage in physics – no *physical* theory holding only in the case that "very improbable but not impossible" things like water freezing on on a heat source *actually* occur, would be taken seriously by *physicists*, while in the area of biological evolution, certain theories seem to consider the occurrence of equivalent improbabilities as something absolutely normal – we have to recognize that the general case of transitions going from lower to higher order as well from higher to lower carry a great epistemological malaise.

In fact, there *is* a big problem hampering knowledge acquisition in many various fields. But there is no intrinsic inconsistency, as we will see here below.

4.2 Non-group-theoretic formalizability beyond group-theoretic physics

4.21 Formal considerations

The general formulation …, $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$, …, as we saw it above, admits *both* particular cases consisting of the transition within Σ (i) from higher to lower and (ii) from lower to higher organization. But, contrarily to appearances, the problem *does not* concern the opposition between that what we call currently "physical" irreversibility and what we observe in some fields like biology. The real problem lies on the side of the insurmountable abyss separating what is formalizable in terms of group theory and what is not. The expression $fm_i(\Sigma) \Rightarrow ct$ is a non-sufficient but absolutely necessarily precondition for the belonging of Σ to the investigation field of physics. So, supposing that Σ effectively belongs to the investigation field of physics, any transition of the kind $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$ belongs in turn to the the investigation field of physics *if and only if* it is intrinsically formalizable in terms of a mathematical group. In other words, the transition in question must emanate from a transformation group $\boldsymbol{G}_\Sigma$ transforming $fm(\Sigma) \equiv fm_i(\Sigma)$ into $fm_{i+1}(\Sigma)$ formalized respectively by the groups G_i and G_{i+1}, so that $G_i \subset \boldsymbol{G}_\Sigma$ and $G_{i+1} \subset \boldsymbol{G}_\Sigma$. This is *not* the case for irreversibility, *nor* for systems whose organization state increases at least during a certain time interval, as it is observed in biology. With regard to irreversibility, the foregoing proposals are easy to prove: the transition from a given initial state of Σ characterized by an ideally constant $fm_i(\Sigma)$ to its simple *non-existence* merely dispenses us from asking the question whether the final state of Σ is formalizable in terms of a group G_{i+1}, so that $G_i \subset \boldsymbol{G}_\Sigma$. Concerning the kind of biological systems interesting us here, we obtain an analogous result, operating by *reductio ad absurdum*. Imagine such a system by a series of transitions …, $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$, … … $(fm_j(\Sigma) \Rightarrow ct) \rightarrow (fm_{j+1}(\Sigma) \Rightarrow ct)$,", where each …, $fm_i(\Sigma)$, $fm_{i+1}(\Sigma)$, $(fm_j(\Sigma)$, $(fm_{j+1}(\Sigma)$, … supposed constant over a given time interval denotes a step in the *development* or *evolution* of the system in question. Now let us admit (i) that there *are* for *any* transition $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$ two groups G_i, G_{i+1}, formalizing respectively $(fm_i(\Sigma)$, $(fm_{i+1}(\Sigma)$, and (ii) that there is a group $\boldsymbol{G}_\Sigma$, so that $G_i \subset \boldsymbol{G}_\Sigma$ and $G_{i+1} \subset \boldsymbol{G}_\Sigma$. If this is the case, then all the … G_i, G_{i+1}, ,G_j, G_{j+1}, … are included in $\boldsymbol{G}_\Sigma$. In other words, there is a global group $\boldsymbol{G}_\Sigma$ covering all the *development* or *evolution* of the system Σ which is subsequently governed by a global $fm(\Sigma) \Rightarrow ct$, whereas not only the diverse … $fm_i(\Sigma)$, $fm_{i+1}(\Sigma)$, … $(fm_j(\Sigma)$, $(fm_{j+1}(\Sigma)$ but also their corresponding transitions $fm_i(\Sigma) \rightarrow fm_{i+1}(\Sigma)$, … $(fm_j(\Sigma) \rightarrow (fm_{j+1}(\Sigma)$ are *distortion-free partial considerations* of the system Σ. Obviously, the *possibility* of such *distortion-free partial considerations* of the system Σ is based on the *atemporality* of Σ expressed by $fm(\Sigma) \Rightarrow ct$, but the atemporality of Σ contradicts by definition its *development* or *evolution*.

Of course, the latter point is subject to diverse speculations. We will come back to this point. For the moment we have to conclude that the common denominator of irreversibility and its *extensions* comprising the possibility of increasing order and even increasing organization consists of their non-formalizability in terms of mathematical groups, where the transition from order to disorder is just a special case.

4.22 "Physical" irreversibility belonging to the periphery of physics

Now, there seems to be a potential objection: *If*, to belong to the investigation field of physics, a given phenomenon has to satisfy the non-sufficient but absolutely necessary precondition of formalizability in terms of group theory, *then* "physical irreversibility" cannot be "physical." While the idea that biology is not "just a part of physics" can at least be defended against ultra-reductionists thinking the opposite, denying the physical status of irreversibility would be a manner of catapulting out of physics the second law of thermodynamics. Well, this issue is thorny. We cannot simply brush it aside. But we also have to recall that irreversibility – sometime formalizable in terms of the physically dimensionless Shannon entropy or something analogous – does not necessarily manifest itself within physics. So, the correct formulation of the *issue* we are tackling here is the following: is irreversibility an essentially physical phenomenon having its consequences or applicability outside physics, just like for example pressure or low current electricity which also intervene in biology? Or is irreversibility an *a priori* non-physical phenomenon *invading* physics where it is playing the disturbing role of a foreign body? The second option seems to be the more adequate. Irreversibility is undermining ideally reversible physical laws, whereas nobody would have the idea to say that ideally reversible physical laws are undermining irreversibility. Thanks to the highly exceptional group-theoretic foundations of its investigation field, physics is able to idealize experimental data by purifying them in a distortion-free way of irreversibility-generating factors. It is literally a miracle that distortion-free physics is possible *despite irreversibility.*

Certainly, irreversible phenomena occur everywhere in physics, concerning directly macro-physics and intervening in subatomic physics *via* the essentially macroscopic experimental device. So it would not be reasonable to exclude from physics approaches like thermodynamics and/or statistical

mechanics *insofar as* they investigate irreversibility touching directly physical phenomena *stricto sensu*. In other words, there are *aspects of irreversibility* we can position in the *periphery* of the investigation field of physics. However, *irreversibility as such* does not satisfy the essentially group-theoretic precondition for being a physical phenomenon *stricto sensu* as we defined it at the beginning of section 3.
The latter proposals may cause vehement protests. But think of the discussion on the *de facto* status of irreversibility which – implying the great and greatest names of physics and/or philosophy like Boltzmann himself, Maxwell, Loschmidt, Zermelo, Carnap, Grünbaum, Popper, Watanabe, Costa de Beauregard and others – had caused a lot of ink to flow. If the *de facto* or "fact-like" status of irreversibility must be opposed to the *de iure* or "law-like" status of physical reversibility, it is because a group-theoretically founded law-like physics does not have the tools to treat non-group-theoretic irreversibility, whereas among the other sciences working precisely in fields not formalizable by group theory, no one until further notice reached something equivalent to the epistemic power emanating from the group-theoretic foundations of physics.
So, there is not a great difference between irreversibility *stricto sensu* and systems tending to higher organization as they are investigated for example by biology. Of course, irreversibility is "familiar" to us, but "familiarity" is not an explanation, and any tentative to explain irreversibility encounters the same difficulties as explanations of systems tending to a higher degree of order: group-theoretically founded physics by definition cannot master transitions which are not transformation groups, whereas outside physics – at the risk of repeating ourselves – there is nothing equivalent with regard to the epistemic power physics manifests in *its* area.
Or, perhaps there *is* a nuance between current "physical" irreversibility and biological systems evolving from lower to higher order. Not being able to explain "physical" irreversibility by its own means, i.e. by its *specific* laws, physics at least tries to elucidate irreversibility by *imported* probabilist approaches *without any physical specificity*. Indeed, unlike quantum probabilities determined by the *reversible* Schrödinger equation, the probabilist approaches supposed to elucidate irreversibility have nothing specific compared to those we use for non-physical processes like the drawing of white or black balls from an urn, or roulette and so on. By contrast, trying to resolve issues concerning evolving biological systems by specifically physical means, all we could expect from current probabilist approaches would be a finding of hopelessness.
Anyway, the problem we are facing does not consist *specifically* of the opposition between reversibility and irreversibility. The real cleavage splitting the acquisition of knowledge in two irreconcilable parts consists of the insurmountable gap separating atemporal investigation fields being essentially formalizable in terms of group theory and the rest of the universe necessarily marked by temporality.

5. Spatialization of time revisited

5.1 A contemporary version of reductive "time spatialization"

As indicated shortly in the introduction of this paper, "spatializing time" according to Bergson means – in our contemporaneous terminology (see subsection 0.1) – "confusing *time* and *duration*", knowing that duration – always in our contemporaneous terminology – is the length of an open, semi-open or closed interval determined by two time *measures* t_a and t_b. Obviously, time and duration are not the same thing. And yet, confusing time and duration is current. How to explain this confusion?
In fact, there is *not systematically* confusion, but *the passage from* physics *with its highly special and "infinitely" improbable investigation field to* non-physics *can* generate *the kind of confusion Bergson rightly criticizes.* Let us try to elucidate this point.

If a given t_a, t_b, or whatever is chosen to be the temporal origin reference t_0 of an appropriated coordinates system, then time intervals delimited by t_0 and any t can be expressed by the sole t which becomes a *parameter* or a *variable* among n – 1 other parameters or variables of a n-dimensional metrical *space*. Now, whereas from a purely mathematical standpoint, there is, absolutely speaking, no reason to privilege within a metric space one given dimension among others, things are *slightly* different from a *physical* standpoint, but nothing more. In the SR Minkowski space-time, the irreducibility of the time dimension to the spatial dimensions and *vice versa* is expressed by the signature of the metric taking *indifferently* one of *both* forms (+ – – –) or (– – – +) conserving each one the invariance of spatial-temporal distances under the Lorentz-Poincaré *group* which, just as any other physically significant group is *ipso facto* symmetrical with regard to prediction and retro-diction. Concerning RG-manifolds where the Minkowski metric has only an infinitesimal validity, or extensions of the Minkowski metric to more abstract spaces with in some cases a higher number of dimensions, there is always the same principle: (i) time, in physical edifices, *does* belong to mathematical *spaces*; featuring a particular status, the time dimension has an impact on the non-temporal dimensions and *vice versa*. (ii) *All* the mathematical spaces in question being *group structures* specifying ultimately the Klein **V**-group formalizing in turn the conservation of *fm*(Σ), they describe despite their irreducible time dimension essentially atemporel phenomena. (iii) Point (ii) is not paradoxical because of the effective existence of an investigation field consisting of phenomena being formalizable in terms of group theory.
But recall that consequently to the "infinite" improbability characterizing the investigation field of physics, its effective existence is highly exceptional. Transgressing the well-determined – group-theoretically-determined – boundaries delimiting the investigation field of physics, *any spatialization of time falls back into paradox and even absurdity* tout court *where no logic can be found*.
Bergson accuses the general philosophy of his days and of his past to "spatialize" time, i.e. to ignore the specificity of time expressed by the temporality of the world. In our context, we have above all to underline that there are two complementary mistakes to avoid: (i) exporting – or importing, if you prefer – the group-theoretic foundations of physics beyond the boundaries of the investigation field of physics exactly delimited by these foundations, and (ii) forgetting, within physics, these group-theoretic foundations and especially the *constraints related to them*.
The non-respect of this principle leads to speculations immediately shattered by the clash between the essentially atemporal foundations of physics and non-physical temporality.

5.2 The circularity of "physical" chaosogenesis theories

Here is a first example illustrating the foregoing points. Certainly, at first glance, we could believe that it is rather a counter-example. But appearances are deceiving.
Of course, on the one hand, nobody would have the idea to exclude astrophysics from physics. On the other hand, astrophysics, among other tasks, describes the evolution of our universe going from less to more organized states, and so from disorder to order. "Common sense", "intuition" and so on could give the impression that this branch of physics tackles well and truly essentially temporal phenomena, confronting this paper with a jolly good dilemma: either we finally *have* to decree that astrophysics does not belong to physics, or all we said about the group-theoretic and subsequently atemporal foundations of physics is nonsense. In fact, things are not so simple.
The following example concerning the birth of a star illustrates – always against "common sense", "intuition" and so on – the necessity of *previously* given order for all pseudo-violation of irreversibility. The contemporary current model concerning the birth of a star advances, roughly speaking, the following scenario: within a cosmic cloud of gas located in a past we often can directly observe, there is initially a permanent struggle between gravity and pressure. Gravity, while compressing the gas, also increases its pressure and fosters "despite itself" the dispersion of the cloud. Common sense often misleading could now suggest a prolonged draw. But a computer simulation (Tegmark, 2014 pp. 38 ff.) taking into account the described situation *in the light of*

gravitation laws and physics of gas indicates a different scenario: since a good part of heat accompanying the pressure ends up being dissipated into space *via* radiation, gravity sooner or later wins, finally generating an overcompression and so a heat overproduction causing the conversion of hydrogen into helium. On the other hand, increased gravity definitively prevents the system from disintegrating, and a star is about to be born. Obviously the transformation of an informal cosmic cloud of gas into a shining star reflects a transition from disorder to more order. *But* (!!) for the Tegmark computer simulation to correspond to reality, the *gravitation laws as well as the laws governing physics of gas* taken into account by the Tegmark computer simulation had to be there *before* the processes leading to the birth of a new star. Now, physical laws being there *before* given concrete phenomena realize them *must be atemporal*, and the possibility of seeing the phenomena in question inscribed in a temporal process – as it is obviously the case for the "birth of a star" – does not change anything on this point. From this perspective, temporal phenomena belonging to physics *stricto sensu* and not to its periphery (see subsection 4.22) express in fact some deeper atemporality.

Of course, the idea of physical laws which exist before being realized by actual phenomena – the correct philosophical formulation would be "physical laws preceding ontologically the actual phenomena realizing them" (comp. Mlika, 2007, p. 39) – joined to the other idea of "deeper atemporality", all this seems very perilously close to metaphysics, or more precisely to Platonism, and Platonism is far from pleasing everyone. But we quickly note that the alternative is even worse.

Provided we are not definitively disgusted by group theory, the foregoing can be specified. Let us represent the consecutive process leading to the birth of a star by $(fm_i(\Sigma) = ct) \rightarrow (fm_{i+1}(\Sigma) = ct) \rightarrow (fm_{i+2}(\Sigma) = ct) \rightarrow \ldots$ As we saw above, any transition $(fm_{i+n}(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+n+1}(\Sigma) \Rightarrow ct)$ is governed by physical laws known at the moment when Tegmark's computer simulation was effectuated. Now, the latter is supposed – at least supposed – to reproduce all the successive star generating processes as they produced themselves, and not as things *could have occurred* from our standpoint. For this, it must be possible to consider each $(fm_{i+n}(\Sigma) \Rightarrow ct)$ and each $(fm_{i+n}(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+n+1}(\Sigma) \Rightarrow ct)$ transition in a distortion-free way. In other words, there are two group-theoretic preconditions to satisfy: (i) Any $fm_{i+n}(\Sigma)$ must be formalizable by a group G_{i+n}. (ii) For any $fm_{i+n}(\Sigma)$, $fm_{i+n+1}(\Sigma)$, we must have $fm_{i+n}(\Sigma) \subset fm_{i+n+1}(\Sigma)$. If and only if the preconditions (i) and (ii) are satisfied, the transition from any $fm_{i+n}(\Sigma)$ to $fm_{i+n+1}(\Sigma)$ is in turn formalizable by a transformation group $\boldsymbol{G}$: $G_{i+n} \rightarrow G_{i+n+1}$. Even if the temporal dimension intrinsically belongs to the groups in question, the latter express atemporality.

Now there seems to be a serious objection: Concerning the "birth of a star" described by the our general schema $\ldots \rightarrow (fm_{i+n}(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+n+1}(\Sigma) \Rightarrow ct) \rightarrow \ldots$, for any transition $(fm_{i+n}(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+n+1}(\Sigma) \Rightarrow ct)$ effectively observed, we never observe the inverse transition $(fm_{i+n+1}(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+n}(\Sigma) \Rightarrow ct)$. Although this is not a case of irreversibility – recall that irreversibility ultimately means **V** → non-**V** – we could suspect that the transformations $\boldsymbol{G}$: $G_{i+n} \rightarrow G_{i+n+1}$ are semi-groups and not full groups, knowing that the latter satisfy associativity completed by the existence of a neutral *and* an inverse element or transformation, whereas for semi-groups, only associativity is required. Concerning $\boldsymbol{G}$: $G_{i+n} \rightarrow G_{i+n+1}$, there are *apparently* just two *unsuitable* solutions: either $\boldsymbol{G}$: $G_{i+n} \rightarrow G_{i+n+1}$ intrinsically has no inverse transformation $\boldsymbol{G}$: $G_{i+n+1} \rightarrow G_{i+n}$, or the latter does exist on a purely mathematical level, without any *physical significance*. Do both cases not express a kind of temporality?

In fact, it is not so easy. Consider the free fall of a mobile in the gravitation field as it manifests itself in the immediate neighborhood of our earth. The movement of the mobile is described by *reversible* Newtonian mechanics. Nevertheless, without any exterior intervention, the system earth-mobile never returns to its foregoing states. And yet, there is no contradiction. The actual "behavior" of the system results from a superposition of ideal reversible physical laws and irreversibility-generating factors we can correct in a distortion-free way by idealization as we approached it in subsection 3.2. The components of the system earth-mobile are neither infinitely elastic, nor infinitely rigid and so on, whereas ideally speaking, the phenomenon in question is *symmetrical under prediction and retro-diction* by virtue of physical laws which do not change over time. Analogously, the birth of a star represents in turn superposition of ideally reversible and so atemporal physical laws and irreversibility-generating factors such as heat dissipation in the form of radiation.

Anyway, we have to retain that through the history of the universe, all processes making this history are governed by physical laws which must already be there before these processes appear. Certainly, as mentioned above, such words have metaphysical resonances, but now is the moment to show that the alternative does not sound better in the ears of anti-metaphysician physicists.

Let us try (i) to imagine absolute disorder, or chaos, if you prefer, and (ii) admit the impossibility of immaterial physical laws preceding ontologically the phenomena realizing them. Well, since manifestly our physical universe is not absolute chaos, there must be a first transition from chaos to something which already resembles order. Now we are certainly free to conceive "anyhow" this first transition from disorder to order. But if we are interested in the physical universe as such, we take *ipso facto* into account at least the *possibility* that the disorder → order transition in question is governed by physical laws. However, serious difficulties are rising. Since we have taken the decision to play the role of visceral anti-metaphysicians refusing the possibility of immaterial physical laws preceding ontologically the phenomena realizing them, we can choose between only two manners to continue. Either we decree that our first disorder → order transition is not governed by physical laws, knowing nevertheless that such a choice belongs to the metaphysics we refuse in the name of our anti-metaphysical convictions. Or, we claim that some kind of "chaosogenesis" *simultaneously* generates the disorder → order transition interesting us *and* the physical law(s) governing this transition. Alas, our second potential option not only is not less metaphysical than the other one, but still encounters a genuine *circularity* (comp. Burov and Burov, 2015, pp. 5; 10). Indeed, "chaosogenesis" generating *simultaneously* disorder → order transitions *and* the physical law(s) governing them closely resembles a judge creating him/herself the legislation framing his/her verdicts.

Generally, physicists hate circularity as well as metaphysics. But anyway, we have to choose between the following two options. *Either* we simply accept that all the processes intervening in the history of the *physical* universe express physical laws which must be there before the processes in questions are realized. Perhaps, the atemporality of physical laws belongs to metaphysics. *Or*, we try to replace this metaphysics by a genuine metaphysical circularity conceived in order to avoid metaphysics. Absolutely speaking, each one of us has to make their personal choice.

Now let us return to Bergson. *If* we merely admit that the birth of a star and any other phenomenon belonging to the history of the universe follows physical laws already there, instead of creating *ex nihilo* the laws they follow – it seems difficult to conceive physics where physical phenomena, instead of following physical laws, create them – *then* we have to admit the existence of an atemporal framework physics needs for describing this history in terms of atemporality undermined by contingency. Probably Bergson never denied the epistemic efficiency of physical atemporality in all fields where it is appropriate.

By contrast, *if* we opt for "chaosogenesis" while pretending to remain in the field of physics, we do commit – rather uselessly – *the* mistake which according to Bergson is not to be committed: applying the group-theoretic and consequently atemporal foundations of physics to essentially temporal phenomena, we "spatialize time" amputating its temporality.

Recall that the distortion-free description of a transition $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$ presupposes the existence of two groups G_i, G_{i+1} formalizing respectively $fm_i(\Sigma)$ and $(fm_{i+1}(\Sigma)$, so that $G_i \subset G_{i+1}$, knowing that the possibility of such a distortion-free description is *the* particularity of physics *stricto sensu* and its group-theoretic foundations. Since the satisfaction of the foregoing precondition implies *ipso facto* the existence of a transformation group $\boldsymbol{G}$: $G_i \rightarrow G_{i+n}$, the corresponding transition, beyond its temporal or "historical" aspects, is atemporal.

Now interpret $fm_i(\Sigma)$ by "chaos" and $(fm_{i+1}(\Sigma)$ by any phenomenon with a higher degree of order we *suppose* "chaosogenetical." Perhaps there is a group G_{i+n}, formalizing $(fm_{i+1}(\Sigma)$. But *by definition*, $fm_i(\Sigma)$ as "chaos" cannot be formalized by a group G_i. So, there is no transformation group $\boldsymbol{G}$: $G_i \rightarrow G_{i+n}$ where G_i would formalize $(fm_{i+1}(\Sigma)$ *within* a transition $(fm_i(\Sigma) \Rightarrow ct) \rightarrow (fm_{i+1}(\Sigma) \Rightarrow ct)$ which subsequently does not belongs to physics. Pretending the contrary would mean to decree the existence of the non-existing G_i and $\boldsymbol{G}$: $G_i \rightarrow G_{i+n}$. Or, in another formulation, specifically we should confuse a situation of irreversibility in a large sense with a symmetry breaking $G_i \rightarrow G_{i+n}$ in $\boldsymbol{G}$, as we defined it following Elena Castellani in subsection 3.32, knowing that the confusion between symmetry-breaking and irreversibility – generalized or not – represents absolute nonsense.

From a Bergsonian perspective, "chaosogenetical" theories, *via* purely *fictitious* and even *phantasmagorical* transformation groups ***$\boldsymbol{G}$: $G_i \rightarrow G_{i+n}$, reduce the essentially temporal *idea* of a disorder → order transition to absurd atemporality.

There is perhaps a latent temptation to transpose at all costs atemporal approaches proving an extraordinary epistemic power within the very improbable and so very restrictive investigation field of physics to other fields where they are not at the right place. But it would be hard to conceive such temptations satisfying the minimal demand of rigor concerning scientific work.

5.3 "Weak anthropic principle" speculations in the name of physics

The so-called "weak anthropic principle", its epistemic (?) and or epistemological (?) motivations and the deductions WAP is sometime *reputed* to found, globally represent another case reducing temporality to atemporality as an extension of that what Bergson calls "spatialization of time."

As such, WAP is merely trivial. Since the *homo sapiens sapiens* is fitted with a brain able to think of her/his place in the physical universe, the latter must have characteristics allowing at least locally the progressive emergence of the *homo sapiens sapiens* and his/her brain. This point cannot be denied. If the physical universe were incompatible with our apparition within it, we would not be here discussing. If despite all, WAP continues to be discussed, it is for contextual reasons. WAP is supposed to reconcile the actual apparition of the *sapiens* with the "infinite" improbability of her/his apparition.

To allow at least locally the emergence of life, humans and above all human brains, the physical universe must be "fine-tuned." If the values of the universal constants were slightly different from what they are, we would not be here. How to explain that the universal constants are exactly what they have to be? In fact, the problem is still more thorny. To finally realize the conditions required for the apparition of humans and human brains, the physical universe must *converge* through all its history to these conditions, and since the history of the universe operates according to physical laws and universal constants, it seems at first glance that there is no other solution than to admit that the convergence in question is *previously programmed*, and this from the beginning of the universe and even before (comp. Burov and Burov 2015, p.10).

The foregoing obviously suggests or at least risks to suggest Design. Hence great efforts have been implemented in order to find philosophically correct alternatives. Since on the one hand, Design belongs to metaphysics, and on the other hand *by definition* any negation of metaphysical propositions represents – even if visceral anti-metaphysicians often forget it – metaphysical propositions, it is foreseeable that alternatives to Design are not less metaphysical than Design. In the context of this paper, we have to add that these mixtures of metaphysics and speculations having nothing in common with physics as it is distinguished from non-physics, exactly *fall into the category of reasoning rightly criticized by Bergson*.

Well, in our days the *classical* conception of an almighty chance randomly underlying *all* the evolution leading to the apparition of the *sapiens-sapiens* and her/his brain seems to be abandoned. Postulating *such* an improbable configuration of improbabilities exceeds even what is allowed in our time where all sorts of speculation are flourishing. So, the *multiverse hypothesis* could not have come up at a better time. Indeed, *if* beside *our universe*, there is an "infinity" – or at least an unimaginably great number – of "parallel universes", we can *envisage* that one among these parallel universes, by chance, satisfies in a sufficiently fine tuned way the preconditions for the apparition of the *sapiens*.

Here, we do not discuss the pertinence of the multiverse hypothesis as such. More especially, *it would not be useful* to repeat for the umpteenth time that "parallel universes cannot be observed", that "the hypothesis of their existence is not 'falsifiable' in the sense of Popper" and so on.

Linked among other to string-theoretic landscape being at the present days the object of epistemologically innovating interpretations, the multiverse hypothesis is philosophically interesting. Perhaps, and even probably, physics is at the dawn of a unseen epistemic shift where only its group-theoretic foundations will ensure continuity. So, people interested in advanced group theory find around the multiverse hypothesis a fascinating investigation field.

But here, we do not have the place to discuss these points. Let us simply accept the multiverse hypothesis as an hypothesis, in order to ask whether the multiverse hypothesis – in case it would be

true – does resolve the difficulties concerning the fine tuning preconditions for the satisfaction of WAP.
Just as it cannot be proved that a formal system with an "infinity of axioms" would escape the Gödel disaster, so also it is perhaps not so sure that an "infinity" of universes comprises necessarily a-fine tuned one consistent with WAP. And there are other difficulties. In a now classical text, A. Guth, one of the founding fathers of the multiverse approach based on eternal inflation, says that – with the notable exception of fundamental conservation laws – "(...) the apparent laws of physics at low energies could *differ dramatically* from one [string theoretic landscape] vacuum to another." (Guth, 2007, p. 10; my own emphasis). Now, on the one hand, any anthropic approach is essentially concerned by low energy physics, and on the other hand, that which is true for STL vacua can be retaken for parallel universes. Knowing that any probabilist consideration presupposes the comparison of comparable elements, it is *at least disputable* whether a multiverse where the apparent laws of physics at low energies could *differ dramatically* from one parallel universe to another, furnishes the appropriate support for the probabilist considerations interesting us. Guth himself evokes another problem. Since inflation generates an "infinity" of parallel universes U_i having their corresponding characteristics $p(U_i)$, the occurrence of each $p(U_i)$ "will happen an infinite number of times." (Guth, 2008, p. 11) In other words, the probability to find a parallel universe U_i with the characteristics $p(U_i)$ is equal to ∞/∞, which is senseless. Well, in fact, the multiverse does not comprise an *actual* infinity of U_i. At any moment – and let beside the very thorny issue concerning the kind time we should have to evoke: cosmic time (?), meta-cosmic time(?), multiversal time (?) or any other ? (comp. Rugh and Zinkernagel, 2012, pp. 4 ff.) – the multiverse comprises an enumerable finite set of U_i increasing *potentially* to infinity. However, Guth himself, in his fundamental paper, mentions further complications which even from this at least theoretically finite perspective cannot simply be swept away (Guth, 2008, p. 11), and each of us is free to subscribe or not to the solutions Guth proposes. But we have to add that, whatever could be our choice, Kolmogorov would not be happy at all about *any* probabilities defined over the multiverse.

Kolmogorov, after having admitted "for long time" that (i) "the frequency approach based on the idea of a *limiting frequency* as the number of trials tends to infinity does not give a foundation for the applicability of the results of probability theory to practical questions, where we deal with a *finite* number of trials; [and that] (ii) the frequency approach in the case of a large but finite number of trials cannot be developed in a rigorous purely mathematical way", adopted later a more nuanced view of point (ii), while maintaining his position on point (i). (Kolmogoroff, 1963, pp.176 f.) To take account of the fact that we always deal with a *finite* number of trials, we have to formulate the law of large numbers in terms of meta-probabilities. Indeed, for a finite number N of trials, the difference between the relative frequency $n(e_i)/N$ of an event e_i and its probability $p(e_i)$ cannot be predicted with absolute certainty. All we can do is to determine the meta-probability $\mathbf{p}$ that this difference, after N trials, is less than or equal to ε, where ε is an arbitrarily small real number. *If and only if* there is a factual probability law $\mathbf{p}(\mathbf{e})$ over the set $\mathbf{e}$ of events e_i which allows us to know *effectively* $p(e_i)$, we can express this meta-probability by $\mathbf{p}[\,|n(e_i)/N - p(e_i)| \leq \varepsilon\,]$. On this basis and introducing a second arbitrarily small real number δ, the law of large numbers takes the form $\forall i, \forall(\varepsilon, \delta), \exists N_0, \forall N \geq N_0,\ \mathbf{p}[\,|n(e_i)/N - p(e_i)| \leq \varepsilon\,] \geq (1 - \delta)$. (Mugur, 2006, pp. 224 f.).The simple reading of the law of large numbers denotes that the latter, instead of "founding" the factual probabilities $p(e_i)$, *presupposes* their *given* existence in order to acquire significance.
Now, do you see any way to *obtain* a factual probability law over a multiverse, and, on top of that, over a multiverse with a cardinal which increases according to a more than uncertain "time" referential?

All the foregoing doubtlessly is to be discussed; nevertheless, this potential discussion as such should inspire us some suspicions concerning the possibility of founding the *de facto* satisfaction of the weak anthropic principle on the basis of the sole multiverse hypothesis.
But anyway, despite all these serious difficulties, let us do *as if* the anthropic multiverse vision was necessarily true. In other words, let us do *as if* the assertion "within an infinity of different parallel universes, there is 'necessarily' one which is fine-tuned according to the WAP preconditions, and this particular parallel universe, 'by chance' is 'ours'" definitely held.
Well, but we must not forget that an universe being fine-tuned according to WAP is a *necessary but*

insufficient precondition for the emergence of the *sapiens* and even of primitive life. An assertion like "the physical characteristics of the exo-planet XYZ are identical to those of our earth, so the exo-planet XYZ is inhabited by living beings absolutely identical to the *sapiens*" probably would not be unanimous.

To "explain" by the multiverse hypothesis the "infinitely" improbable but nevertheless actual occurrence of "our" universe being fine-tuned according to the WAP preconditions is one thing. To "explain" the evolution of living matter starting from inert matter and leading to the emergence of the *sapiens-sapiens* is another. Let aside the fact that "(…) the estimated anthropic limitations on fine-tuning aren't anywhere fine enough to explain the experimental confirmations of the extreme precision of the elegant forms as fundamental laws" (Burov and Burov, 2015, p.8) and that subsequently any tentative to derive *directly* from these fundamental laws the transitions … … $fm_i(\Sigma) \rightarrow fm_{i+1}(\Sigma)$, … …. leading to the emergence of the *sapiens* would be a manner of building on sand. The principal problem, once again, resides in the passage from the investigation field of physics which can legitimately be approached by group-theoretic means, to other fields where group theory becomes senseless, and here we recognize another aspect of what Bergson qualifies as "spatialization of time."

The string theoretic landscape conception has essentially *group-theoretic foundations*. So, the extension of STL-vacua to parallel universes forming a multiverse supposed to "explain" the occurrence of "our" tailor made fine-tuned universe being exactly as it must be perhaps belongs to speculation, but it is at least *physical* speculation. By contrast, and contrary to the history of our (?) inert universe, biological evolution is an evolution *stricto sensu*. Let us represent the biological evolution by our transition schema … … $fm_i(\Sigma) \rightarrow fm_{i+1}(\Sigma)$, … …. If there were for any i going from 0 to an undetermined value a group G_{i+1} formalizing $fm_{i+1}(\Sigma)$, and another G_i formalizing $fm_i(\Sigma)$, so that $G_i \subset G_{i+1}$, the transitions … … $fm_i(\Sigma) \rightarrow fm_{i+1}(\Sigma)$, … …., for any i would be globally governed by a transformation group $\boldsymbol{G} : G_i \rightarrow G_{i+1}$ specifying ultimately the Klein **V**-group. In other words, the transitions … … $fm_i(\Sigma) \rightarrow fm_{i+1}(\Sigma)$, … …. globally *would conserve the same functioning mode*, and this would *by definition* be incompatible with evolution *stricto sensu*. Since on the other hand, the absolute absence of predictivity and retro-dictivity in biological evolution renders useless any consideration of symmetry or asymmetry in prediction and retro-diction, the transformation group $\boldsymbol{G} : G_i \rightarrow G_{i+1}$ does not exist in this field. So, asserting any continuity between the fine- tuned universe and biological evolution starting from inert matter and leading to the emergence of the *sapiens* means to do *as if* the non-existing $\boldsymbol{G} : G_i \rightarrow G_{i+1}$ existed. In this case, it is not *physical* speculation, but speculation *tout court*.

It is astonishing that physics being rightly aware of the improbability characterizing the *physical* – i.e. fine-tuned – preconditions for the emergence of the *sapiens-sapiens* and his/her brain seems (i) to neglect the fact that these fine-tuned preconditions are *necessary but insufficient* ones, and (ii) to consider as negligible the problem of the tremendous improbabilities coming up *beyond* the fine-tuning issue.

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Acknowledgments: I wish to thank Lev Burov, Fermi Society of Philosophy, for having followed step by step, with an extraordinary personal implication, the preparation of this article. My thanks go also to Al Stewart, Pôle Universitaire Léonard de Vinci, Paris, who have contributed his time and patience to revise the English expression of the text.

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