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Speculative Remarks about Conservation and Unification *)

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The text below has been reduced to a strict minimum, so that its best *Summary* is contained in the Title of this paper.

Up to the present time, a 'principle of conservation', applicable to energy, has had a great influence on the development of physics. Historically, this principle has its roots in the antique idea of conservation of substance; the notion of energy became a substitute for that of substance after its conservation (for most cases) had been proved within the context of Newtonian mechanics and then, in the early 20th Century, matter had been defined by energy. So, energy and energy conservation came to play leading roles, while it escaped the notice of scholars that this might include more anthropomorphism than is useful for a sound development of Science.

The indefinite meaning of potential energy and the non-existence of a total energy in non-conservative cases should have drawn the attention of old authors to difficulties to come. Nevertheless, following the controversy between NEWTON and LEIBNIZ about *vis viva* and *momentum*, two kinds of conservation came into consideration, one being a mechanical principle (conservation of momentum), the other being a mechanical theorem needing generalization in order to become a physical principle. Physicists were then subjugated by the success of the introduction of new forms of energy (heat, electromagnetic energy,... mass), which were all meant to save the conservation principle from breaking down; and the importance of each new quantitative notion of a form of energy grew in practice.

So far, except for mass, Galilean relativity has been assumed. Already for this relativity, the question arises: what is the correct mathematical expression to describe conservation? If it reads

$$\frac{d}{dt} \int dC = 0 \,,$$

*) Dedicated to Professor E. C. G. STUECKELBERG-VON BREIDENBACH on the occasion of his 60th Birthday by one of those who had opportunity to learn already in the thirties to appreciate him as a teacher and scholar of high rank.

where t denotes time and C the conserved quantity, the $\int$ is to be conceived of as a Stieltjes' integral, it need not be a Riemannian one. Only in the latter case can the expression be transformed into an equation of continuity,

$$\frac{\partial \gamma}{\partial t} + \varDelta\,(\gamma\,\boldsymbol{v}) = 0\ ,$$

γ being the density of C and $\boldsymbol{v}$, its current-velocity. It presupposes an empty space susceptible of being filled by a continuum, something like a 'field of C'. Of course, γ might be assumed to take the form of a sum of delta-functions to represent 'particles', but this involves a very special notion of particles with all its danger of divergences.

EINSTEIN's special theory of relativity confirmed, what electromagnetic theory had already suggested, that a conservation principle, as applied to energy, is only the time-part of the more elaborate expression for the vanishing of the big Divergence of a tensor density. But a correct conservation equation – at least for special relativity – should concern only the vanishing of the little divergence of a vector-density. Only in this case can there be a scalar quantity correctly conserved.

In General relativity, even the big Divergence relation becomes doubtful. A pseudo-tensor-density and other similar artifacts have been invented in order to 'save' this kind of conservation (still called, by many authors, conservation of *energy*). Two opinions have arisen. The first is that energy-conservation is unimportant in General relativity; whereas according to the second opinion, the primal condition of a well made theory of general relativity should be that it assumes (or proves) an absolutely covariant energy conservation equation. The former position appears to be of a pragmatic and the second of a metaphysical nature.

Misled by a premature announcement of an apparent experimental break-down of energy conservation in the Compton effect, theoreticians were induced in the thirties to consider assertions of non-conservation. This of course ended in a natural withdrawal. But together with the above position, called pragmatic, it shows that very serious minded authors are nowadays ready to drop altogether an idea which tradition had made more or less taboo in physics. Of course, two quasi-necessities remain: to find the correct modification of what has been a most successful notion of energy conservation and to clarify the question as to whether there is a fundamental conservation of 'substance'.

To that last question, any student of electrodynamics might give the easy and seamingly evident answer: charge is conserved. Indeed, electric charge is Lorentz-invariant, a necessary condition for a quantity to be 'truly' conserved. However Lorentz-invariance does not by itself imply conservation. For a conservation equation to apply, we need a special kind of physical concept, the definition of which may be looked for along either of two lines. Along the global line: an additive quantity; along the space-time description line: a current-density field.

If classically, i.e. in non-relativistic pre-quantum theory, mass (of density μ), charge (of density ϱ) and energy (of density ε) should all fulfill a conservation relation

$$\frac{d}{dt}\int \begin{pmatrix}\mu\\ \varrho\\ \varepsilon\end{pmatrix} d\tau = 0\ ,$$

then why should there be a difference between the three quantities? Surely they should be reduceable to one another, for any experiment expressly prepared to verify nothing but conservation itself would not differentiate between kinds of substance. When a non-relativistic quantum-theoretical conservation equation of the form

$$\frac{d}{dt} \int | \psi |^2 \, d\tau = 0$$

was assumed, $| \psi |^2$ was soon interpreted as a measure of ϱ and eventually of μ.

From the point of view of quantum field theory where (free) fields and particles are 'the same', no doubt $| \psi |^2$, even relativistically, is to be considered as the density of the substance conserved, since all (common) Lagrangian densities are basically of that form. This saves the antique idea. But at the same time it is not the energy which is thereby concerned.

On the other hand, the quantity conserved takes on the nature of a quantum number. So the antique idea of indestructibility of matter should not be expressed with the help of energy, even if mass is considered as a form of energy. (Unfortunately many textbooks and teachers still express it so.) The only magnitudes suited to enter in conservation principles are those susceptible of a quantization adapted to the notion of particles, i.e. reduceable to the *true* quanta or least quantities of 'matter' as represented by these magnitudes. Moreover, such a magnitude should be a 'true' invariant, where the word true is insofar unclear, as we must require the magnitude to be invariant with respect to general transformations which we may not know exactly: Galilean, then Lorentzian, Riemannian, Finslerian etc. The 'etc.' leaves open the generality of such transformations since it is always possible that a magnitude believed to be an invariant is no longer such with regard to transformations still to be discovered and/or to be applied in physics. Of course the problem can be reversed, a proposal can be made not to accept transformations for which a supposedly conserved quantity is not conserved. This may restrict the choice of something like a metric. However, one should realize herewith that the choice of something like a metric determines the kind of interpretation of experiments made to verify the conservation with the help of relevant coordinates. Therefore it may be that the same state of affairs will appear as a true conservation in the interpretation in such and such frame, and as something other than a true conservation in another frame. So the pragmatic point of view (with emphasis on '... no importance *in General relativity* ...') may well be the only satisfactory one.

Another remark should be made. Strictly speaking, conservation can reasonably be proved or postulated only with regard to an isolated system, for if an environment in interaction with the system is at hand, only a precise and exhaustive description or knowledge of the exchange processes between the system and its environment will prevent one from allowing for new 'forms' of the exchanged quantity; but this is equivalent to including the environment into the system which so becomes isolated. Following this idea, we realise that the only existing isolated system is the whole universe, i.e. a system which may have elements still undiscovered. Therefore finding the true expression of any conservation principe becomes an unsolvable problem. The system must be replaced by one which is practically (instead of strictly) isolated. In old days, a wall was imagined. But there are no walls, and vacuum (surrounding bodies) is sure to be that 'medium' opposing least resistance to exchanges (though we know of an old treatise which said of vacuum that it is electrically the best isolator). As wall, we know only of a very strong field making a barrier. However, the stronger the fields, the greater the probability of some catastrophic collapse. For a field is a substitute for an assembly of particles, and its strength will be high when these particles are close together, and so unable to avoid collisions, desintegrations and the like.

A further remark arises when we analyze the kind of interpretation given to a class of physical experiments. We think of the observation of all transmutations: it is said that 'particles' collide and/or transmute into other particles. However, the particles are never seen as such, i.e. isolated by themselves. A spur in the emulsion or in a chamber is not the trajectory of a particle, it is a macroscopic effect interpreted with the help of a corpuscular representation. Particles are spread ψ-like in configuration-space. That is all we can say for sure. In a sense, we should drop the idea of particles altogether. Physics around such experiments has evolved into the consideration of a number of symmetries which is the contemporary way of looking at conservations. Certain things appear conserved, others not. Parity was found not to be conserved. That is to say: in the view of an accepted meaning of parity conservation, the principle of its conservation broke down in the light of experimental evidence. But very soon a way was looked for and proposed to save parity conservation: if we extend conservation of parity to the more complete system consisting of 'matter ***and*** antimatter', it seems to be generally conserved. This extension shows that 'matter' may be defined by its parity, there being two kinds of such matter, viz. matter and antimatter; similarly, there are two kinds of another appearance of matter known long ago, viz. positive and negative electric charge. It is convenient to say that there are particles bearing such properties as electric charge, parity and other 'physical numbers'. However, remembering the absence of reasons to distinguish between (classically) conserved magnitudes, one sees no reason either why there should be such distinguishable physical numbers as attributes of matter. We might put it thus: Why should nature have produced matter with a (finite?) number of such attributes, and their conservation becomes extremely, if not infinitely, intricate?

The ψ-like spreading of the 'state of a particle' is not the only reason to be careful about its space-time description. The fact that most 'particles' explode as soon as an attempt is made to localize them (or even explode without any excitation and/or as soon as they find partners for recoil and conservation of certain physical magnitudes involved) shows that autonomous local attribution in space is more or less meaningless. To avoid this difficulty, nonlocal description may be useful. Certain properties of particles are not to be ordinary functions but functionals of (or in) space-time, in order that the whole of space-time be implicated in the definition of the particle. A particle is then no local thing. This representation is alien to classical thought; however it is a better than a classical representation as long as the particle is thought of as tied to, or existing in, space-time.

So the attempt to localize experimentally quantities in arbitrarily small environments is bound to fail, even if a field theory – quantized or unquantized – might allow to localize them theoretically. (Much of what is posited and developed in theory has no counterpart in experiment.) Consequently, if such a quantity is classically conserved, it is no wonder that it need not exist in a more elaborate theory since its localizability may prevent this on the grounds that space-time looses its real meaning.

This suggests that there is no physical space without a theory of matter in bulk, i.e. without physical statistics and therefore without thermodynamics. Thus the possibility of a thermodynamics is perhaps the physical condition necessary not only for the use of space-time coordinates but also for the existence of canonically conjugate variables, and therefore for the whole dynamical apparatus tied to the introduction of something like a Lagrangian and a variation principle.

If this is true, thermodynamics is the condition of a dynamics. To our knowledge, STUECKELBERG is the only author who, possibly for reasons similar to those above, has attempted to deduce Mechanics (in a covariant form) from a suitably written thermodynamics. One should not wonder why this deduction goes from thermodynamics to mechanics and not the opposite, for reversibility, proper to all dynamics, must be thought of as an extreme case of irreversibility.

If this again is true, an analogy may be sought. The step from reversible physics to irreversible thermodynamical description requires a considerable jump, viz. the passage from an extreme case where dynamical principles find their natural expression in differential equations and the like, to a situation where the case is not an extreme one. (Our Century has revealed two formally similar passages: from the classical case as an extreme where $c \to \infty$ to the relativistic case where c is finite, and from the classical case as an extreme where $h \to 0$ to quantum theory where h is positive. However, in both situations, c and h were found to be finite. Thermodynamics does not make use of a similar constant that would go either to zero or to infinity corresponding to the degenerate case of the dynamics of reversible processes; k does not play such a role. Rather the passage from reversibility to irreversibility goes from an order of natural phenomena to another order, but not from an order of magnitude to another.) The analogy is to be found, then, in the passage from physics (including thermodynamics) to biology. We may try to classify orders of natural phenomena with reference to categories of temporal evolution: reversibility as a first category, irreversibility as a second one, biological finality as a third one and so on. Thus living beings are considered to be governed by 'laws' at all three levels. (These 'laws' are not contradictory, contrarily to what some doctrinary scientists think, but superposable.) In that analogy thermodynamics, if once regularly elaborated, might appear to be conditioned by a 'bio-dynamics', by which no mechanistic doctrine is meant. What we mean is the following: if on the one hand one should not question the fact that living substance is at the lowest level subject to the laws of the various dynamics of reversible processes, at the second level to the laws of thermodynamics (irreversibility ... death), and at the biological level to laws of that third level, it may well be, on the other hand, that the elaboration of a reference frame for a comprehensive thermodynamics should follow from specialization of a 'bio-dynamical' description, just as the elaboration of the frame called physical space may be conditioned by the knowledge of the workings of thermodynamics. (Such a comprehensive thermodynamics we do not have yet since textbook thermodynamics is artificially restricted to reversible processes supposedly implying heat exchange which do not take place in nature.)

These are higher speculations. We ought to conclude with remarks closer to physical topics. If we think of dynamics of reversible processes, we may ask: is there *a* Dynamics which includes all such dynamics. It would be the unified theory. The progress, which, so far, has led from rational mechanics through that of deformable bodies, electrodynamics, relativity, quantum theory to the latest attempts to describe the material processes at very high energies and at the level of elementary particles, seems to be taking one of two directions: either there is a series of more or less related dynamics which generalize and complete one another without there being a capital Dynamics towards which it converges, or there is a growing tendancy toward a unification which will be the unique Dynamics.

If the former is the case, it would seem that each interaction that is not reduceable to other interactions needs a dynamics of its own to be suitably described.

In the latter case, which reflects EINSTEIN's life-dream, a final Dynamics would at the same time be a final stop. It would swallow all irreducibilities in the former sense into a comprehensive description. But then such a final Dynamics should not rest upon some principle similar to a variation principle. For if it did, the principle would require that something like a Lagrangian be given; the Lagrangian would be chosen of this or that form, and accordingly there would be as many interactions as choosable Lagrangians, which is precisely what a regular unified theory should not allow. Indeed, if a philosopher should say that the world of our physical observation was created according to one singular super-Lagrangian, I would ask why this singular one rather than any other one was chosen by the Creator. This is the application of the epistemological principle of sufficient reason. It has never been refuted. So according to this reasoning, a correct unified theory should not be based on the model of physical theories developed so far, for they practically all use a dynamical principle.(In a sense, axiomatic quantum field theory does not, but the difficulty arises of getting something else than free fields.) The correct unified theory – which on purpose is not called here unified field theory – should be *no* dynamics. If you will, paraphrasing Wheeler, it should be a 'dynamics without dynamics'.

Conservation theorems in dynamics have been obtained from the invariance of typically dynamical expressions with respect to coordinate transformations. So may be there will be no conservation theorems in a final theory. However, such theorems are independant of specific laws of interaction – they are identities. Among identities of a theory, some may still be interpretable as conservations.

The contemporary physics of so-called elementary particles suggests that such identities will refer not to the space-time of our sensible intuition but to abstract spaces. If this is the case, the problem of the passage from thermodynamics to dynamics of reversible phenomena becomes confused, for how could the abstract spaces be won from it?

These are all tentative remarks which we leave to the judgement of the reader.

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