PHYSICS AND PHILOSOPHY:

ACTION AT A DISTANCE IN 20TH CENTURY PHYSICS*

José M. SANCHEZ-RON

ABSTRACT

In this paper I review the different opinions held by scientists and philosophers as regards the status of the action-at-a-distance concept within relativistic physics. It is shown that in spite of the fact that the prevailing opinion has been that special relativity precludes actions at a distance, some important physicists have continued employing that concept throughout the present century. The key to understand that "anomalous" behaviour lies, in fact, in the relationships existent between quantum and classical physics ("inverse" principle of correspondence).

INTRODUCTION

The publication, in 1687, of Newton's Philosophiae Naturalis Principia Mathematica marked in many senses the opening of a new area in the development of physical sciences. So it happened, for instance, with the problem of how different bodies affect (that is, interact with) each other. Until the publication of the Principia, and with the possible exception of Aristotle's natural motions, which can be assimilated in some sense to actions at a distance, aethereal mediums had no rivals as far as the explanation of the nature of interaction is concerned. The most influential theory, or, better, conceptualization of nature, (not only before but also after 1687) was René Descartes' vortex system put forward in his Principia Philosophia, first published in 1644. Descartes disliked all theories that depended on unspecified mechanisms, or actions at a distance, for the spatial transmission of forces or actions. He considered that actions at a distance would culminate by endowing material particles with knowledge, to the point of making them "*truly di-*

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vine, so that they may know without any intermediary that wich happens in places far away from them and exercise their actions upon them".

As I said before, Newton's Principia meant the real introduction of the action-at-a-distance concept in physics. To appreciate this fact we have to remember that book II of the Principia, where the system of the world is put forward, is based on the inverse square law, a law of standard action-at-a-distance structure, since it depends only on the positions of the interacting bodies (that is, no intervening medium appears). In this sense, it may be thought that the action-at-a-distance component of Newton's system is constrained to his theory of gravitation. As it turns out this is not the case; Newton's assumption that when bodies at a distance (that is, not in direct contact) are moving relative to one another the third law still holds, implies in principle (i.e. if not other specification is made) that the interaction between them takes place instantaneously, for if the transmission takes time, then the action of, let us say, A on B may not be simultaneous with that of B on A and therefore in general not equal to it at all times. But the only instantaneous interaction wich makes sense is instantaneous action-at-a-distance.

However, the introduction of actions at a distance through the Principia was far from being accepted or even really believed. It is well known -remember his 1693 letter to Richard Bentley- that even Newton thought that actions at a distance were "*so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it*". (It is perhaps a mark of Newton's greatness as a physicist that, nevertheless, he did allow such concept in his work). Neither Huygens, Leibniz or some of the Bernoullis could accept a physics in wich the action-at-a-distance concept played some role.

As a matter of fact it was only by the 1750s -that is, more than sixty years after the publication of the Principia- when Newton's mechanics and theory of gravitation were virtually universally accepted in the Continent as accurate. Thus, the development of physics throughout the eighteenth and first half of the nineteenth centuries tended to improve the status of the action-at-a-distance concept with respect to its rivals, the action through a continuous medium and the action by impact concepts. It might suffice, in this sense, to remember the names of Laplace, Coulomb, Ampère or Weber. Nevertheless, it is important to emphasize that "*that strong instinctive feeling which denies action at a distance*"[1] was never completely lost, not even during the years in wich the action-at-a-distance model was hegemonic (so, for instance, Ampère at times speculated, as Newton did, as to the "cause" of action-at-a-distance forces, conjecturing that the forces between currents might be transmitted by an aethereal fluid).

On the light of such feelings it was only too natural that the advent of Maxwell's field electrodynamics, together with Hertz's 1888 experiments, were to mean the death knell of action-at-a-distance physics. An interesting example in this sense is provided by Arthur Schuster who, after defending on epistemological grounds the action-at-a-distance concept, went on to state (in 1911):

> *"If I am anxious that you should realise how weak are the grounds on which we deny on principle action at a distance, it is only to lay the greater stress on the real advance which science has made in consequence of the belief that all action between two bodies is transmitted through a connecting medium... At present, we may take it, that the doctrine of "no action at a distance" has secured its successes, mainly because it opened out a new field, which could be brought to the test of experiments. These successes must be placed to the credit of the school of science which explains the unseen by means of models such as we can construct with material bodies"*[2].

The prestige obtained by Maxwell's electrodynamics was such that finally a new world-view arose: The Electromagnetic View of Nature. Let us remember that, in some of its presentations, that view claimed that all laws of nature -hence Newton's laws also- are reducible to properties of the ether-field. That is, one of the implicit aims of this world-view was to replace all actions at a distance for fields.

The soon-to-follow "special relativity revolution" did not change, as seen by most contemporaries, the situation.

For my purposes here a most important fact is that ever since special relativity was formulated it has aroused the (still!) extended belief that only field theories can cope with the modifications brought by Einstein's theory. A few examples in this sense are, no doubt, convenient.

In 1974, W. Berkson[3] wrote:

> "*(In his paper "On the Electrodynamics of Moving Bodies") Einstein began research which eventually finished off both Newtonian and action-at-a-distance*";

on a similar vein Infeld and Plebanski[4] stated:

> "*According to Relativity Theory, no action can be propagated with a velocity greater than c -the velocity of light. Thus the field propagating from one point to another, largely because of Relativity Theory, becomes a physical reality, as real and as material as corpuscular matter*".

Even more categoric was recently Garret Birkhoff[5]:

> "*Since (special) relativity denies the possibility of 'action at a distance', Newton's Third Law... can only be interpreted in terms of a 'field' theory* ".

Of course, , in the precedent quotations there are different questions involved, but not all of them will be considered here (actually, most of such questions have been repeatedly discussed over a number of years by Peter Havas). There is, however, one argument which has been, in one way or another, specially important in order to support the belief that special "relativity denies the possibility of action-at-a-distance". It is based on the fact that Maxwell's field electrodynamics did not need any modification in order to be incorporated into the framework of special relativity. John Synge[6] expressed it concisely in the following words:

> "*As far as electromagnetism was concerned, the controversy of 'action-at-a-distance versus action-trough-a-medium' was settled in an extraordinary and unexpected way. Einstein's relativity might be said to favour action-through-a-medium, since it accepted Maxwell's partial differential equations*".

Just another expression of this kind of argumentation is to emphasize that the second postulate in the standard (but by no means unique; remember von Ignatowsky) presentation of special relativity is closely attached to Maxwells' field theory. Max Abraham[7] stated it clearly when, in 1914, wrote:

> *"Many supporters of the Theory of Relativity derive from the first postulate the unnecessity of the conceptio of a space-filling medium, an "ether". To be sure this postulate indeed makes the ether undetectable with reference to uniform, straight line motion... The second postulate, that of the constancy of the velocity of light, cannot be understood properly without reference to the wave theory... The radical relativists, the enemies of the ether, would like to disavow that origin".*

(The only thing to add to this illuminating passage is that Abraham's fears seem a little out of place if one considers how strong has been the commitment to the field concept by most physicists after 1905).

As a matter of fact, these scientists rightly pointed out the outstanding role played by Maxwell's ether-field electrodynamics in the initial development of special relativity, but at the same time they denied to the latter theory the possibility of transcending the scientific atmosphere in wich it was born (context of discovery), not seeing (in the context of justification) the fact that Einstein's theory is not a dynamical theory but a kinematical formulation, and that consequently it said nothing either about the structure of matter or about the existence of a medium for the propagation of interactions.

Of course, not all the community of physicists viewed special relativity as a kind of subdiscipline of electrodynamics. Einstein and Planck, for instance, were fairly clear on its status (indeed, the fact that his relativistic kinematics did not depend on the knowledge of Maxwell's equations was specially important for Einstein, as he himself acknowledged later in his life, because his studies on the black-body radiation problem had convinced him that Maxwell's theory was only approximately true). Others, like Sommerfeld, thought that the Lorentz-Einstein viewpoint fell within the limits of the mechanicistic tradition.

It is, therefore, true that there was in fact a wide variety of reactions towards special relativity. On the whole, however, and considering not only the inmediate reaction but a more extended one, I think that the arguments against the action-at-a-distance concept did prevail. (Of course, one should never forget, among other thongs, that the force of such arguments was enhanced and even mixed up with the successes and heuristic appeal of general relativity, a field theory). We already saw the opinions which some historians of science (Berkson), physicists (Infeld and Plebanski) and mathematicians (Birkhoff) have put forward, but there is still another, and most important, evidence: Albert Einstein himself. Everybody knows that, specially after 1909, Einstein turned with a growing enthusiasm towards the "field approach". So, in 1949 he wrote[8]:

> "*Field theory does exist as a program: 'Continuous functions in the four-dimensional (continuum) as basic concepts of the theory'. Rigid adherence to this program can rightfully be asserted to me*".

There are many examples of instance in which, over a long period of time, Einstein attacked actions at a distance. Let me present just one[9]:

> "*I admit, however, as perfectly possible that perhaps physics cannot be based upon the field notion, that is, upon continuous elements. But then nothing would be left of all my constructions -taking also into account the theory of gravitation- as well as of present physics, almost nothing*".

Did Einstein mean here that special relativity -one of his constructions- is only compatible with the field notion? Although, apparently he did, I do not think (because of other evidences) that he really thought so. But the point is that through his numerous writings he did not help by any means the belief that actions at a distance might still have, from a logical point of view, a place in relativistic physics.

ACTIONS AT A DISTANCE IN ELECTRODYNAMICS: SCHWARZSCHILD, TETRODE AND THE "QUANTUM CONNECTION"

In spite of what have been said before concerning the viewpoints held by most scientists, it turns out that fields are not singled out as the only possibility for describing (relativistic) electromagnetic phenomena. Physics research in our century has taught us that, in some respects, that the nature of physical interactions could differ from our anthropomorphic expectations. We shall see that what I call the "inverse principle of correspondence" (meaning that problems within quantum mechanism suggest developments in classical theories) was behind the fact that in some sense the action-at-a-distance concept "is useful after all".

A first step in the direction of proving the usefulness of the action-at-a-distance concept in electrodynamics appears in the works of Karl Schwarzschild in 1903 when he was at Göttingen, the Dutchman Hugo M. Tetrode in 1922, and that of his fellow countryman Adriaan D. Fokker in 1929, where they proved that Maxwell's equations (and I am saying equations, not theory) could also be derived from an action-at-a-distance standpoint (they used a variational principle where only particles appear). But let us have a closer look at these developments.

Schwarzschild's work of 1903[10] was carried out as part of his research on the variational formulation of the theory of electrons. He found a variational principle where no fields appear -due, essentially, to the reduction of partial differential equations to integral equations-, but he did not paid special attention to such fact. That is, the action-at-a-distance concept came to electrodynamics through the back door, to say so. However it probably could hardly have been otherwise in the Goettingen of Minkowski, Abraham and Wiechert where the Electromagnetic View of Nature has solids roots. Consequently, the relevance that Schwarzschild's work has had in the action-at-a-distance -field controversy has made itself specially evident many years after 1903, with the benefit of hindsight.

It was in the twenties when some physicists began to worry about the field concept. Their motivation laid essentially in the quantum domain, and this is a fact that must be stressed as one of the lessons which will turn out from the present considerations. Maxwell's theory became, in the hands of H.A. Lorentz, the electron theory and this together with the work of spectroscopists, atom designers, and other pre-quantum mechanics physicists, paved the way to the search for a modified or new electrodynamics, which eventually came to be established under the name of "quantum mechanics". But when physicists set to the task of developing a quantum theory, whether in its preliminary (pre-1925) or in its final (post-1925) form, they encountered difficulties. This led to some of them to think that maybe after all the difficulties laid in a resort of the classical domain, the field concept.

This was the case, for instance, with Walter Schottky in 1921. In his paper "The Problem of Causality in the Quantum Theory as a Basic Question for Modern Natural Science as a Whole"[11], that has been commented by Paul Forman in his now classic "Weimar Culture, Causality and Quantum Theory, 1918-1927"[12], Schottky put forward the idea that there might be a direct connection between the emitting and the absorbing atom by retarded action-at-a-distance, so that at the moment when a quantum is emitted it is already predetermined where, when and by which atom it will be absorbed. But Schottky did not develop his idea, nor did he find any kind of mathematical support for it.

One year later, in 1922, a physicist from Amsterdam, Hugo Martin Tetrode, wrote a paper entitled "On the Action Function of the World. An Extension of Classical Dynamics"[13], where the field concept was also criticized. But before turning to this paper I cannot avoid the temptation of saying a few words about the life of this extraordinary physicists, in as much as he is very little known and does not appear in any dictionary of scientific biographies. As you will see this fact is quite justified.

Hugo Martin Tetrode was born on March 7, 1895, and died, prematurely, on January 18, 1931, of tuberculosis[14]. His father, Pieter Johan Conrad Tetrode (1863-1955), Dr. of law, was wellknown in the world of finance. He was managing director of the "Nederlandsche Bank" (the official bank of the Netherlands) from 1919 to 1934.

Apparently Hugo Martin, a very clever young of poor health and of a retiring disposition, never held any academic position, nor even got a degree. We know that he was registered at the University of Leipzig during 1910-11. At Leipzig he must have written his first paper dealing with the chemical constant and the quantum[15]. He was then

16 years old. There is no evidence he continued his studies at some other German university.

Apparently, Tetrode did not enjoy at all direct communications. Ehrenfest invited him several times to visit him at Leiden (remember he was living in nearby Amsterdam), but he always refused. The story that Einstein, who as we shall see had a very good opinion of Tetrode's habilities as a physicist, wanted to pay him a visit and was sent back at the front door was told to Prof. Casimir by several people. Around 1930 Pauli complained that physicists in Holland knew Tetrode existed but that nobody seemed to know him. No doubt, the fact that Tetrode was financially independent may have enhanced a tendency towards seclusion. On the other hand it enabled him to do outstanding work, notwithstanding his poor health.

But forgive me for having used this opportunity to pay a small hommage to this Dutch physicist, to whom, I have to confess, I do admire, and let us proceed with our story, and in particular with his paper "On the Action Function of the World. An Extension of Classical Dynamics".

Tetrode thought that theories based on the field concept, like Mie's, cannot explain the quantum events. It seems that these events *are not connected with the field model*". For this reason he chose:

> "*a different standpoint... considering as primary elements those whose existence is given to us directly and experimentally -the negative and positive- electrons which can even be counted and whose individual motions can be traced after*".

He also

> "*assume(s) that each change of the motion of an electron depends on other electrons and ... treat(s) the field as a mathematical construction that in the limit case in which the quantum nature of the events can be disregarded is suitable to represent the interaction between the electrons, but leave(s) us with misleading impressions*".

Tetrode also arrived to the variational principle already discovered by Schwarzschild, but he did not spend much time with it, because as I have said his problem was the quantum. So, he asserted that

> *"we want to generalize this expression which has been derived from the classical equations of the electromagnetic field, limiting the number of mathematically possible solutions in such a way that are permitted to expect* that the generalization *will cover the quantum phenomena as well".*

Taking into account these equations and what we know of Albert Einstein's scientific struggles, it is hardly surprising to find that late in August 1922, the creator of the relativity theories would write to his friend Paul Ehrenfest

> *"a very inspired work by Tetrode concerning the quantum problem. Perhaps he is right; in any case this work indicates a mind of the first rank. For a long time I have not seen anything so simply presented".*

A measure of Einstein's real interest on Tetrode's ideas comes from the fact that he, who never was good in remembering other's works, did refer again to Tetrode's paper during the ceremony which took place when he was named, on March 4, 1923, corresponding member of the Royal Academy of Sciences in Madrid. On that occasion Einstein said[16]

> *"You* (that is, Blas Cabrera) *have also taken into consideration the weak point of the theory of light quanta, difficult problem of our physicists' generation. I believe that these difficulties could only be overcomed through a theory which will modify not only the energy principle, but which will perhaps extend the causality principle. Recently, Tetrode has pointed out precisely such possibilities".*

Even in the forties Einstein would remember, as we shall see, Tetrode's ideas.

Finally, and as this is not the occasion to discuss in detail Tetrode's electrodynamically-inspired ideas on the quantum, let me point out that he also had a clear understanding of the counter-intuitive consequences entailed by the introduction of non-instantaneous Lorentz-invariant actions at a distance, and that nevertheless, he was willing to accept them. Thus, he wrote in a now famous quotation:

> *"The sun would not radiate if it were alone in space and no other bodies could absorb its radiation ... If for example I observed through my telescope yesterday evening that star which, let us say, is 100 light years away, then not only did I know that the light which it allowed to reach my eyes was emitted 100 years ago, but also that the star or individual atoms of it knew that I, who then did not even exist, would view it yesterday evening at such and such a time ..."*

In this sense Tetrode's ideas of the physical meaning of direct interactions were very much like the ones which a few years later would be vigorously defended by the American chemist-physicist Gilbert Newton Lewis, ideas that have been studied in detail by Roger H. Stuewer[17] to whose paper I remit here.

FOKKER AND THE "INVERSE" PRINCIPLE OF CORRESPONDENCE

I shall now discuss the work of Fokker, Lorentz's former Ph. D. student, who in 1929 published a paper entitled "An Invariant Variational Principle for the Motion of Several Charged Mass Particles"[18], dealing with the same kind of problems treated by Tetrode. Fokker's main result was what Schwarzchild and Tetrode had already proved, that is, that Maxwell's equation can be derived from an action-at-a-distance standpoint (there are, however, no references to these papers in Fokker's work; the same happens in Tetrode's with respect to Schwarzschild). Fokker's motivation was essentially the same as Tetrode's, however, his paper was less imaginative; but this was not a weakness, on the contrary, it was the core of its strenth, because being less dispersive, its message could be less easily forgotten (so it happens that in the present literature the variational principle discovered previously by Schwarzschild and Tetrode is usually called Fokker's principle). The task faced by Fokker was clear from the very first line of his paper. He wrote:

> *"Quantum mechanics has taken its starting point by a correspondence to the classical theory for the dynamics of a single particle, and has formulated its laws and methods for the one-body problem by relating them to Hamilton's canonical equations. But when it took into consideration the interaction between*

> *several particles it still has not found any form which can satisfy the invariance requirements with respect to Lorentz transformations. Here there is missing even the preliminary work from the foundations of the classical theory*"[19]

In this quotation we see that Fokker had clear and precise understanding of what I have termed the "inverse principle of correspondence". Tetrode was well within the "quantum connection", and in this sense he had much in common with Fokker, but the way in which he saw the relationship between quantum and classical physics was much more rudimentary than Fokker's. This was due to several reasons: In the first place, no quantum *mechanics* existed in 1922, when Tetrode did his work. The second reason is closely related to the previous one: once Heisenberg-Schrödinger's *non-relativistic* quantum mechanics was established, it became obvious that an inmediate problem to solve was to find its relativistic "generalization"; at this stage Fokker realized that, as far as the *relativistic* interaction between *several* particles was concerned, the problem was *really* serious because it was not even know how to set the *classical* Hamiltonian formalism. Naturally, and this is my third reason, which merges itself with the second one, to appreciate the meaning of "*really* serious" one has to take into account that Fokker was at the time under the influence of Niels Bohr "philosophy", one of whose tenets was the *principle of correspondence*. In order to use Bohr's principle it was compulsory to know the corresponding classical theory. Fokker realized that on this occasion there was none. So, he tried to solve the quantum problem turning towards the resolution of the corresponding classical one. Twenty years later P.A.M. Dirac[20], a great supporter of the inverse principle of correspondence, would follow the same idea that Fokker, to open what is now a flourishing field of research in classical relativistic dynamics: The "relativistic dynamics of interacting (*any* interaction in principle) particles". He wrote then

> "*The existing theories of the interaction of elementary particles and fields are all unsatisfactory in one way or another. The imperfections may well arise from the use of wrong dynamical systems to represent atomic phenomena, i.e., wrong Hamiltonians and wrong interactions energies. It thus becomes a matter of*

great importance to set up new dynamical systems and see if they will better describe the atomic world[21]"

Naturally, what he intended to do was to find the algebra of classical Poisson brackets, which would lead him immediately to the algebra of quantum commutators.

ELECTROMAGNETIC RADIATION, QUANTUM THEORY AND ACCTIONS AT A DISTANCE: WHEELER AND FEYNMAN

One of the features -probably the most important and intriguing- of the Schwarzschild-Tetrode-Fokker's formulation of electrodynamics is that it introduces advanced as well as retarded interactions between the charged particles. Actually, this is a necessity if one wants to write up an action-at-a-distance variational principle leading to Maxwell's equations, but at the same time it means that electromagnetic radiation does not appear in the theory. Let us leave aside for a moment this important problem, and point out that there were other physicists who did not use fields in their works on electrodynamics, but that however remained attached to what we might call the "retarded tradition". One of the principal exponents of this line of thought was Walther Ritz who chose to face the electrodynamical puzzles by developing[22] an emission theory that is also an action-at-a-distance one. Among the points on which Ritz insisted was the denial of advanced interactions. This was also one of the issues of debate in the discussion he held with his friend Einstein, a discussion which was summarized in a short joint paper[23] published in 1909.

In that paper Einstein put forward the idea that "irreversibility" -very much attached as we know to electromagnetic retarded interactions- "rests exclusively upon grounds of probability", and that to him there was nothing wrong with the fact that the elementary interactions were time-symmetric. As it turns out this was one of the clues which in the early forties would permit the Princeton physicists J.A. Wheeler and his then Ph.D. student Richard P. Feynman, to solve the problem of how to introduce the electromagnetic radiation into the Schwarzschild-Tetrode-Fokker's formulation. Wheeler and Feynman's papers[24] of 1945 and 1949 gave to the action-at-a-distance concept a status of respectability that has remained since them. In the U.S., for instance, a large number of physicists read their papers when they appeared. Alfred Landé for example, wrote in 1950[25]: "*Their theory has provoked much* unpublished *favorable as well as adverse comments*". Probably because of the personality of the authors, the attractive way in which the theory was presented, or because it appeared in the issues of the prestigious Reviews of Modern Physics dedicated to Bohr (1945) and Einstein (1949), on the occasion of their sixtieth and seventieth birthdays, respectively, the fact is that Wheeler and Feynman's electrodynamics has had, and still has, a great influence.

Similarly to the cases already discussed, Wheeler and Feynman's motivation was not to enlarge or continue Tetrode's or Fokker's contributions to classical electrodynamics. (As a matter of fact they did not know of the existence of these papers until they were hard at work; Einstein informed Wheeler about Tetrode's ideas). What they wanted was -once more- to face some problems in the physics of the microcosmos. Wheeler, as he tells us[26], was struggling with the idea of whether the electron would not be after all the basic particle whose theoretical description would enable him to set up a satisfactory nuclear theory. He found support for his idea in Dirac's successful relativistic theory of the electron and he was not deterred by the discovery of the neutron (1932), nor by Fermi's theory of the β-decay (1933). Thus, if the electron were really the basic particle, and if he were to question what he called the Stron-Force Credo,

> "*how then could one escape a careful look at the interaction between highly accelerated relativistic electrons*". *This was the reason for taking a fresh look at the theory of electromagnetic action-at-a-distance of Schwarzschild, Tetrode, Frenkel and Fokker*"[27]

As for Feynman, one reads in his Nobel lecture that his reason for focusing precisely on the action-at-a-distance concept was due to the idea -common to numerous physicists throughout our century- that if many of the problems which appear in quantum theory, and specially in relativistic quantum theory, are recognized as due to the infinite degrees of freedom introduced by fields, why not dispense with them in the most straightforward manner?[28]

After his work with Feynman, Wheeler kept interested for a while in action-at-a-distance theories. Thus, his student Gilbert N. Plass submitted in 1946 a Thesis entitled "Black Body Radiation in the Theory of Action at a Distance", the results of which should have been included in the finally never published sequel to the 1945-49 Wheeler and Feynman's papers. Also, Wheeler tried to develop an extension of his and Feynman's theory which would cover gravitation too. There was, in this sense, a rather interesting interchange of letters between Einstein and Wheeler latter in 1943. Let me just quote a few paragraphs of the three interchanged letters[29].

On November 3, 1943 Wheeler wrote to Einstein:

> "*Since the time when I got leave of absence from Princeton University to help with the war project which you initiated, I have had little opportunity to return to Princeton, and even less to work upon the theory of action at a distance. However, since the time when Mr. Feynman and I discussed with you the interpretation of the force of radiative reaction in terms of advanced and retarded action at a distance, I have had some further success with the extension of this theory, which may be called the 'theory of world lines'.*
>
> *I should like very much to visit you and see if you cannot help me better to see where the force of gravitation fits into the point of view of the theory I have mentioned*".

Three days later Einstein answered Wheeler. He was very glad if Wheeler could visit him, but

> "*I am afraid that I cannot help you very much in your endeavour. What I approve wholeheartedly in your attempt is the principle that nothing should be introduced in the fundament which is not symmetric concerning the time direction. On the other hand it seems to me that a concept like a finite Minkowskian distance can never be reconciled with the principle of general relativity*".

After the visit, which took place the 14th of November, Wheeler wrote, on December 2, a very long letter to Einstein. Let me just quote a few sentences:

> "*I left you* (that is, Einstein) *with a better idea how to develop a "principle of correspondence" between the general theory of relativity and the general theory of action at a distance. In the meantime it has proved possible to clear up an apparent discrepancy between the two points of view, a discrepancy in the idealized case of a universe containing two masses. The nature of the problem becomes apparent when the theory of action at a distance is briefly recapitulated*".

At the end, however, Wheeler abandoned his approach. Some time ago (29 June 1978) he wrote to me:

> "*Action-at-a-distance is a wonderful way to describe interaction; but it presupposes, Feynman and I now agree, what we now know is quite a wrong idea of a particle. That is why I gave up my 1949-50 work on action-at-a-distance in gravitation ...*"

Nevertheless, once a theory or world-view becomes influential it does not take long before other scientists set out to apply it to different topics. It would seem now natural to speak of the many contributions of Havas, Hogarth, Bondi, Hoyle or Narlikar, and also, no doubt of the so-called predictive relativistic mechanics approach, but that would take me too far. Moreover, it is never a good thing to get too close to our days.

THE PHILOSOPHER'S PERSPECTIVE

On return, on this last part of my lecture, I shall deal with a few philosophical topics which show the interest of paying attention to the action-at-a-distance concept in XXth century physics.

A fundamental assumption for many empirically-minded philosophers of the first decades of our century was the existence of a distinction between theoretical and nontheoretical terms, distinction which was considered crucial for a proper understanding of the concepts and methods of science. Precisely this fact is one of the reasons that makes the action-at-a-distance and fields concepts philosophically important to me: their characteristics with respect to the distinction "theoretical-non-theoretical" are totally different. So if we take a sufficiently weak -and remember that I am taking a historian's point of view- definition of nontheoretical term, allowing microscopic physical objects such as electrons and other elementary particles to be considered nontheoretical terms, then it is clear that the action-at-a-distance concept belongs to this last class of terms. On the contrary the field concept -only indirectly measurable by means of test particles- is a theoretical term.

The importance of the previous considerations becomes evident when one remembers that two of the schools which dominated philosophy of science during part of the present century, namely, operationalism and logical positivism, suspected of unobservables. One could think therefore that it would have been quite natural for the followers of these doctrines to prefer the action-at-a-distance concept instead of the field one. However, nothing of the sort happened because both doctrines are different. Bridgman (i.e. operationalism) accepted action-at-a-distance while those logical positivists that made clear their positions (at least to the best of my knowledge) favoured fields. To understand this behaviour it will suffice to review briefly the essential ideas of both doctrines.

To Bridgman a scientific concept had to be linked to measuring procedures. He observed that[30]:

> "*if by convention we agree to use only those concepts in describing physical situations to wich we can give a meaning in terms of physical operations, then we are sure that we shall not have to retract*".

Although responding to the same basic idea, i.e. physical concepts beyond experimental control should be regarded as metaphysical, logical positivism had more flexible requirements: theoretical concepts are allowed, something which does not happen in Bridgman's system.

Taking into account that the field concept is one of those theoretical terms, the study of Bridgman's ideas in connection with the action-at-a-distance and fields concepts takes on a special significance. As it turns out Bridgman was somewhat in favour of the convenience of using actions at a distance, pitilessly criticising at the same time the notion of fields. Let me just present two pieces of evidence in this sense. In his contribution to the Schilpp's volume, Einstein: Philosopher-Scientist, Bridgman[31] wrote:

> *"The advantage and necessity of the field point of view is usually considered to be that it avoids the difficulties of the old action-at-a-distance point of view... I believe, however, that an analysis of the operations that are used in specifying what the field is, will show that the conceptual dilemma by no means has been successfully met... One has in no way exorcised the mistery of the successive appearance of a force at successive test-bodies by the invention of the field".*

And his classic The Logic of Modern Physics, he stated[32]:

> *"The situation with respect to action at a distance is typical of the general situation. I believe the essence of the explanatory process is such that we must be prepared to accept as an ultimate for our explanations the mere statement of a correlation between phenomena or situations with which we are sufficiently familiar"*

Although he was completely right in pointing out the fallaciousness of some objections people used against actions at a distance, Bridgman's methodological opposition to fields shows in an extremely clear manner the limitations of his philosophy.

As I have already mentioned, the formal possibilities offered by logical positivism differed in at least one important aspect from those provided by operationalism: theoretical terms have a place into logical positivism, and fields are thus allowed. However, perhaps someone would have expected of logical positivists at least a sympathy for a "more empirical" concept (namely, the action-at-a-distance one), arguing that they were supposed to suspect of unobservables. Nevertheless, the little evidence that I have found suggests that what happened was that many logical positivists considered the action-at-a-distance as refuted by the physics of their time (by relativity in particular). (Let me say in this connection that although it is not proved here, I believe that in general the field concept exerced a strong attraction on logical positivists; one of the reasons for this could have been precisely that this concept provides a very good example of the utility of introducing "theoretical terms" as basic constituents of scientific theories.)

An interesting example is provided by Moritz Schlick. In 1925 he wrote[33]:

> *"Our interpretation of the concept of force was made on the basis of the empirical fact that "forces which act at a distance" do not exist, and that existence can only be ascribed to actions which are propagated continuously from point to point".*

The many shortcomings that plagued operationalism and logical positivism were to become notorious after a long period in which not very many other voices were heard. Among those who have been trying to revise the basic tenets of these two doctrines is Mario Bunge, whose opinions as regards actions at a distance and fields are interesting, although they do not agree with mine. I will not discuss, however, Bunge's ideas in as much as I have done it already[34]. Let me only say, nevertheless, that although probably the majority of present philosophers think, like Bunge, that only fields ought to be admitted in modern, relativistic, physics, there are some who cannot accept such "rule". One of these cases is Hilary Putnam who in a paper ("In Defence of Internal Realism") shortly to be published in Teorema states that as far as metaphysics is concerned there are no reasons why we should prefer fields to actions at a distance.

All these cases remember us the necessity of a careful analysis of the meaning of this counterintuitive concept, called action-at-a-distance. Modern philosophy pretends, at least in some of its branches, to have adopted high standards of rigor in logical analysis. However, not always it has been able to free itself from old prejudices or from uncritically accepting the points of view of the majority of today-physicists. It is one of my contentions that the case of the action-at-a-distance concept shows rather clearly this point, independently from the fact that actual physics happens to be -and there is no room for any regrets- to a large extent "field-physics".

NOTAS

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[3] *Forces and Fields*, p. 291 (Routledge & Kegan Paul, London 1974).

[4] *Motion and Relativity*, pp. 15-16 (Warsaw 1960).

[5] "George D. Birkhoff's Theory of Gravitation", p. 61 in *Selected Studies*, Th.M. Rassias and G.M. Rassias, eds., pp. 57-77 (North-Holland, Amsterdam 1982).

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[8] A. Einstein, "Reply to my Critics" in *Albert Einstein Philosopher-Scientist*, P.A. Schilpp, ed., p. 675 (Open Court, La Salle, III, 1949).

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[13] "Über den Wirkungszusammenhang der Welt. Eine Erweiterung der Klassischen Dynamik", *Zeits. f. Phys.* *10*, 317 (1922).

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[15] "Die chemische Konstante und das elementare Wirkungsquantum" *Ann. der Phys.* *38*, 434-442 (1912).

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[18] "Ein invarianter Variationssatz für die Bewegung mehrerer elektrischer Massenteilchen", *Zeits. f. Phys.* *58*, 386 (1926).

[19] Emphasis added.

[20] "Forms of Relativistic Dynamics", *Rev. Mod. Phys.* *21*, 329 (1949).

[21] Emphasis added.

[22] W. Ritz, "Recherches critiques sur l'Electrodynamique générales", *Ann. Chim. Phys.* *13*, 145 (1908). Reprinted in *Oeuvres*, p. 317 (Gauthier-Villars, Paris 1911).

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[25] "On Advanced and Retarded Potentials", *Phys. Rev.* *80*, 283 (1950).

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[27] *Ibid*

[28] See in this connections Feynman's "The Development of the Space-Time View of Quantum Electrodynamics", *Phys. Today*, August, p. 31 (1966).

[29] The letters are deposited in the Einstein archive.

[30] P.W. Bridgman, *The Nature of Physical Theory*, p. 10 (Princeton University Press, Princeton 1936).

[31] "Einstein's Theories and the Operational Point of View" in *Albert Einstein: Philosopher-Scientifst*, P.A. Schilpp. ed., pp. 351-352 (Open Court, La Salle III, 1949).

[32] *The Logic of Modern Physics*, pp. 47-48 (Mcmillan Co., New York 1928).

[33] M. Schlick, "Naturphilosophie" in *Lehrbuch der Philosophie*, Dessoir ed., vol II (Berlin 1925). English translation in *Philosophy of Nature* (Philosophical Library, New York 1949).

[34] José M. Sánchez Ron, "Bunge: Cajas negras y translúcidas y acción a distancia" *Teorema* *XII*, 195-213 (1982). See also Bunge's reply, "Cajas negras y translúcidas y acción a distancia: Sánchez Ron" *Teorema* *XV*, 271-274 (1985) which I will answer in a future issue of *Teorema*.

Dpto. de Física Teórica
Universidad Autónoma de Madrid