Remarks on the Establishing the Theory of Nuclear Forces

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It is a great honour to participate at the Jubilee Conference in Kyoto devoted to 50 years of Hideki Yukawa brilliant prediction of mesons, moreover acting as a chairman of a session. First of all I am obliged to convey to Professor Ziro Maki, Chairman of the Organizing Committee of MESON50, the best wishes for the success of the Conference from the Rector of the Moscow University (Professor A. Logunov) and the Dean of its Physics Faculty (Professor V. Fursov) recalling that Professor H. Yukawa was a Doctor honoris causa of our University. It may be permitted to myself to remind our long term colleagial, friendly relations with unforgettable Yukawa, his deep attention to our papers, also his, now rather widely known famous inscription with a chalk on the wall of my working cabinet at Physics Faculty, made after discussions at a workshop: "Nature is simple in its essence", has written Yukawa, formulating so to say his motto, reflecting his deep philosophical conceptions, also in a sense "polemicizing" with motto of P. A. M. Dirac, previously also written with chalk: "Physical law should have mathematical beauty" (1956). (Both precious inscriptions, together with two others, brought later by Niels Bohr and J. A. Wheeler, are preserved under glass.) H. Yukuwa visited Soviet Union in 1959, participating at the 9th Conference on High Energy Physics in Kiev, and afterwards visited Moscow.

Not entering in details I intend to draw attention on some important points of the early years of the whole modern nuclear physics, as well as the theory of elementary particles, reminding first of all some most difficult points of the establishing proton-neutron nuclear model, proposed by me (April 1932) after the discovery of the neutron by Chadwick (February 1932). It is well known that the old proton-electron constitution of nuclei met with many serious difficulties, being not capable to explain spins, statistics, magnetic moments, etc., of nuclei. It was felt at the end of 20's and beginning of 30's, that something radical must be done, e.g., Niels Bohr suggested the construction of a quite new theory of "nuclear electrons" based on his beloved non-conservation of energy. One has spoken about the loosing of spin, etc., by internuclear electrons (cf. Heitler), tried to change the Minkowskian relativistic flat space-time geometry inside nuclei (trying to solve simultaneously the divergence of electromagnetic field energy problem of point electron by introducing a kind of discrete space-time lattice, cf. D. Ivanenko and V. A. Ambarzumian, also W. Heisenberg, 1930). All this was wrong! But our analysis of the behaviour of leptonic type so light electrons in small distances proved to be useful later; the hypothesis of a lattice space-time, independently from nuclear problems was, let us remark by the way in itself interesting and stimulated many investigations, continued in modernized form up to recent days, cf. Darling, Schild, Rompe, Moeglich, Snyder, Finkelstein, Caldirola and other authors; perhaps indeed, not at nuclear distances $\approx 10^{-13}$cm, but at Planckian "smallest" distances $\approx 10^{-33}$cm, some kind of discreteness arises?

Anyhow, we were more and more inclined to the idea that under influence of some protonic ("hadronic") forces the electrons not simply lose their spins etc., but perhaps are entirely lost as individual particles, being in some sense "joined" to protons (that was some kind of new "prediction" of neutron *à la* Rutherford and Harkins). Not entering in details of important discussions after the discovery of the neutron, though I remained so to say not supported by other colleagues, I decided to publish a short paper, proposing the hypothesis that the nuclei are built only by "heavy" protons and neutrons, (baryons) without electrons (this paper appeared in *Nature* 28, May 1932 and was immediately supported by Heisenberg, who mentioned my paper already in the first so important article of his well-known series of 3 articles ; the first one has the date of June 8th, so only ten days after my note ; clearly Heisenberg already worked in analogous directions some time before that date). The deep idea of isospin, introduction of exchange type nuclear forces and other consequences of my proton-neutron model developed by Heisenberg are well known. But by the way one must not forget that p-n model was not immediately accepted, e.g., F. Perrin and P. Auger simply introduced neutrons in nuclei alongside with protons and electrons (only later F. Perrin in a paper in *C. R.*, Paris, has agreed with my p-n model). The chief point of all my reminding here shortly this situation of 1932 is to emphasize that Heisenberg had not accepted p-n model completely, but put previous intra-nuclear electrons somehow inside neutrons! His second article, with expression of gratitude to Niels Bohr, is devoted to treatment of the scattering of γ-rays on nuclei as the scattering on the intra-neutronic electrons ; and this is not simply a passing remark, but these are attempts to explain in this way the experiments of L. Meitner, etc. Apparently Heisenberg, like N. Bohr, did not admit as yet the Pauli hypothesis of neutrino and tried to explain β-decay electrons as the argument for hidden existence of β-electrons somewhere inside neutrons. In my second paper (published later in 1932 in *C. R. Ac. Sci. Paris*) it is clearly declared that β-electrons are born as new particles, like photons.

The error of Heisenberg in this important point and my insistence on the complete non-existence of electrons inside nuclei were repeatedly analyzed just at recent conferences and articles devoted to 50 year Jubilee of the Neutron discovery and in such way the beginning of the whole modern nuclear physics with its immense scientific and technical applications and its deepest influence on the whole modern civilization, with its social relations. I may draw attention, e.g., to the very interesting discussions at the conference devoted to the early post-neutron years (held in U. S. A., 1977, Minneapolis, Minnesota ; proceedings edited by Professor R. Stuewer), there one finds the deep thoughts of Peierls, Wheeler Wigner and Bethe on the proton-neutron model and clear indication on Heisenberg error in the above mentioned point, also with mentioning of my papers. It is especially interesting to follow the polemicizing between Bethe (first considering the error of Heisenberg as not significant but later after strong arguments of Wigner, admitting that this point is quite important).

I may support here, at Kyoto MESON50 Conference the opinion of E. Wigner, that the history of science must be "honest" and, so may I continue, we shall put the question how such great theorist as W. Heisenberg made such an error and recognized the whole p-n model only later (cf. his important report at 7th Solvay Nuclear Congress in Bruxelles, at the end of 1933, a month after the lst Soviet Nuclear Conference in Leningrad, end of September 1933, where my report was devoted to arguments for p-n model with special indication on the famous paper of Blackett on cosmic ray cascade showers discovering the

real possibility of the birth and annihilation of electrons; in his paper Blackett mentions my work on β-decay and on the other side Dirac prediction of the possibility of the birth of electrons (and positrons) as theoretical predictions of his discoveries. By the way, it must be emphasized that the Leningrad Nuclear Conference (at the Phys. -Techn. Institute led by A. Ioffe, then the chief centre of Soviet physics and the cradle of Soviet nuclear physics) was essentially the first modern type nuclear international conference, taking account of neutrons and positrons and, alongside with Solvay Bruxelles Congress, definitely accepting p-n model; also giving — in Bruxelles — strong arguments for Pauli's neutrino (and demonstrating there the first hesitation of N. Bohr on his energy non-conservation ideas). With Soviet nuclear physicists in Leningrad arrived Dirac, Joliot, F. Perrin, Rasetti, also Weisskopf, G. Beck and some others; Dirac, Joliot and Perrin in Bruxelles, so to say, repeated their Leningrad reports. Proceedings of the lst Soviet nuclear conference with reports and discussions, invaluable for historians of physics, were published in 1934. Before the war we have organized 5 Nuclear Conferences.

My answer to the question (put by "honest" history of science), how great Heisenberg committed such non-accidental error (putting electrons inside neutrons) is: because my proton-neutron model considers necessarily neutron as an elementary particle like proton (not consisting of neutron plus positron as proposed by F. Perrin senior) and the beta-decay must be treated as the birth of electrons like radiation of photons is essentially a relativistic process; for Heisenberg, one of the chief great founders of the nonrelativistic quantum theory, the mother language, so to say, was non-relativistic, though later he has contributed so much to the relativistic quantum field theory both for electromagnetic case and later (end of 50's and in 60's) in trying to develop with H. P. Duerr and other collaborators the unified theory of matter on the basis of quantized non-linear spinor equation (non-linear form of the Dirac equation proposed by myself already in 1938 and called by Heisenberg as a predecessor of his version, which, by the way, was further developed in our works and in modernized form is revived in recent years; to this version of unified theory is paid attention e.g. in important papers of H. Terazawa in Japan).*),**)

To finish with the p-n model problems (what is necessary for treatment of nuclear forces and understanding the Yukawa meson theory), I permit me to point that the priority of Soviet science in establishing proton-neutron nuclear model was recognized not only by Heisenberg (who mentioned my papers in his first nuclear article and in his Solvay report), but by physicists and historians of physics; it is sufficient to mention e.g. Bethe-Bacher so well-known review, Feather's one the first modern books on nuclei, and among the quite recent very authoritative works the brilliant review of E. Amaldi published recently in "Physics Reports" September 4 issues, vol. 111 (1984). I may draw attention to the fundamental works on the History of Physics, of all times, by M. V. Laue (writing rightly about p-n model of Heisenberg, Ivanenko and supplementing this with mention of Ig. Tamm, due to later work on forces); M. Gliozzi, Italian historian of science, explains in his valuable book my p-n model, writing then, that it was developed

*) Various non-linearities now play in many branches important role, even biological objects seem to possess essential non-linearity, which, e.g., A. S. Davydov recently investigated as solitons transferring energy in bio-molecules (Kiev, USSR).

**) It is amusing that not standard spinorial description of fermions but use of antisymmetric tensors was early proposed (D. Ivanenko, L. Landau, Z. Phys. 48, 340) already in 1928, then rediscovered by E. Kaehler in 1960 (using Cartan's exterior forms) and now discussed by many authors (cf. D. Ivanenko, Yu. Obukhov, Ann. Physik, Leibzig (1985); vol. 42, p. 59; and ICTP, Trieste, No. 2, Preprint 1985).

in important papers of Heisenberg ; among Soviet historians of science most serious and detailed analysis of the *p-n* model was given by D. S. Kudriavtsev (3 volume work on the Physics History and a more recent single volume), by A. N. Vialtsev (in volume *"Neutron"*, 1975) ; Publ. House "Nauka", Moscow ; and his 2nd volume of the series devoted to discoveries of particles (Nauka, Moscow, 1984). By the way, the simplified description of the *p-n* model is entered in school text books, also in many general Encyclopedia, e. g., in biographical notes devoted to 'Heisenberg', also to 'Ivanenko', e. g., in *great* Soviet Encyclopedia (2nd and 3rd, exists also complete English translation) in recent editions of *Brockhaus* and *Meyers Encycl.* (West Germany), *Meyers* (East Germany), also other U. S. A., Poland, etc., analogous publications, not to remind the fundamental *Poggendorff* well-known many volumes, many editions work of biographies of scientists, and Chicago University single volume of biographies of phys. -math. scientists, etc. I am obliged to recall this because unfortunately some few German even such respected scientists like A. Sommerfeld (friend of A. Einstein as well recognized) (known for his chauvinistic approach to the history of science, e. g., neglecting publication of the famous Poincaré's work, where special relativity was definitely established parallel to Einstein) ; this great theorist, Sommerfeld, permitted himself in the well-known volume devoted to quantum mechanics, paying some attention also to nuclei, to mention, even in rather pathetic form, Heisenberg "decisive" solution of the old proton-electron model difficulties, neglecting completely even a slight mention of my papers. I regret that Professor Brown, in his so contentful report at our Kyoto MESON50 Conference, has spoken about "Heisenberg's proton-neutron model" also not paying attention to Soviet work on this subject.

Passing now to nuclear forces one must recall that *p-n* model immediately led to proposing many phenomenological types of forces without any deeper theoretical argumentation : exponent form, square well, etc. ; anyhow one was obliged to proceed without delay to calculate nuclear effects taking account short range type, also exchange type (proposed by Heisenberg, Majorana, Bartlett, etc.) ; discussing with V. A. Ambarzumian these problems we considered also Seeliger-Neumann version (end of 19th century) of changing Newton's gravity law by means of supplementing it with exponent : $e^{-\alpha r}/r$, to solve the well-known difficulties of Newtonian cosmology — but we disliked this Yukawa type (!) version for nuclei finding it without any reasonable dynamical quantum field basis, and having of course no idea about the value of "α", though had a talk about all this at a seminar. So when Fermi's beta-decay theory appeared (using, as E. Amaldi emphasized in his 1984 review in Phys. Rep., our basic idea of the birth of electrons like radiation of photons) we understood, with Ig. Tamm, that electron-neutino pair can and must realize some force between proton and neutron, and in such way the first not phenomenological, not ad hoc arbitrary theory of nuclear forces was elaborated. The most difficult point, so far I remember, was to admit that the field of force can be realized by particles possessing rest mass (i.e., electrons) ; (so trivial now in the days of W and Z mesons it sounds) previously one had only long range electromagnetic and gravitation forces realized by massless objects (photons and gravitons) ; later I published (after our common short papers in Nature 1934, with Ig. Tamm) more detailed calculations with a pupil of mine, A. Sokolov ; these calculations were anew performed later with all modern refinements — dispersion relations, etc., used — by Feenberg and Sucher, confirming Tamm-Ivanenko r^{-5} law, for the fermionic pair force (neglecting $\exp(-mr)$ term on short distances, with m the mass of electron here). Of course such force exists, but does not

represent the chief proton-neutron force, as was immediately remarked by Yukawa, who has in a really genial way developed our ideas, proposing his meson hypothesis.

Not entering in further details I shall remind that my work with Ig. Tamm not only played so to say a role of a necessary step to Yukawa great epoch making meson theory, but being supported by Heisenberg, Weizsaecker, Kemmer, etc., immediately contributed to the whole theory of nuclei. Indeed on the basis of T-I theory the non-central part of nuclear force, so important, was predicted; on its basis N. Kemmer elaborated the symmetric theory of nuclear forces (later repeating his fundamental results on the basis of Yukawa meson, etc.). In this connection I permit me to remind that Niels Bohr has not accepted Yukawa's bold hypothesis, and during his visit to Japan (when Yukawa's pions were not yet discovered) remarked: for what purpose do you need such new particle? (qualitative quotation of his words, following very interesting recollections of M. Taketani). And I may ask anew how such great leading theorist as Niels Bohr had not understood deep right ideas in the whole new approach to nuclear forces (of Tamm-Ivanenko, who used neutrinos, against what N. Bohr protested) and urgent necessity of introducing new type of U-field, i.e., Yukawa mesons. And we others, remembering that the history of physics must be "honest" (following E. Wigner thesis, 1977), cannot forget that the great founder of the first quantum version of atomic electron's theory in Rutherfords planetary atom model, one of greatest leaders of the 20th century physics, Niels Bohr not only was against mesons, he also not admitted neutrinos, was in doubt on the Dirac's vacuum theory and its prediction of the birth and annihilation of electrons and positrons). My — preliminary — answer is: all this series of wrong Niels Bohr ideas, concerning most fundamental directions of the modern physics, is due to his not sufficiently relativistic approach; quite like above mentioned Heisenberg error with beta-decay electrons (I am not entering here on the N. Bohr protests counter shell model of nucleons, discussed e.g. by E. Wigner at 1977 Minneapolis Conference, this has nothing to do with relativity, though being an important point clarified by Goeppert-Mayer, and partly connected with our work with E. Gapon (1932)). Indeed, the most famous 1913 N. Bohr model, the compound nuclei approximation, the so successful theory of fission (established by N. Bohr with J. A. Wheeler in 1939, parallel to the analogous work of Ja. I. Frenkel in Leningrad, whose results Bohr and Wheeler already mention) all these fundamental results were not relativistic; I shall not enter here in the discussion of N. Bohr's complementarity and his discussion with Einstein, which later in my opinion, as well remarked by some other authors, cannot be considered as definitely finished, though immensely interesting and stimulating for subsequent investigations.

I hope that respected colleagues reading this short remarks shall understand well that to draw attention to some not sufficiently underlined difficulties of establishing Yukawa meson theory as essentially relativistic modern type field theory, I was obliged to recall some points of the establishing of the proton-neutron model and illustrate the difficulties on a most convincing manner by pointing on the errors and hesitations of W. Heisenberg and Niels Bohr.

To finish my modest contribution to MESON50 I cannot miss such an extraordinary occasion and shall permit me just few remarks drawing attention to two series of our papers, first one concerning the building of a so fashionable now anew unified theory of matter and secondly concerning to include gravitation in such a grand unified picture of the universe at the end of 20th century. Firstly, I believe that the nonlinear generalization

of Dirac's spinor equation introduced by me in 1938 (in symbolic form ; $D\psi+\lambda(\psi^3)=0$) and proposed as a base for describing pre-matter, supported and quantized by Heisenberg (with Duerr, Yamazaki et al.) (end of 50's and in 60's) not only is interesting due to its solitonic solutions, specific symmetry properties, geometric meaning which can be given to non-linear-pseudovectorial type — supplement as due to treating Dirac equation in the space endowed with torsion — the very fine result of my pupil Professor V. Rodichev, and its other properties, not only for all this, but some kind of non-linear equation is reasonable to use for describing the pre-mordial, sub-quarkian preonic, say, state of matter ; i. e. remembering some preliminary (you call it "qualitative") success of old unifying attempt of Heisenberg's group in Münich and our Moscow group, anyhow yielding some shadow picture of the spectrum of hadrons up to Ω particle and even the value of the Sommerfeld fine structure constant not too far from empirical value ($1/115 \sim 1/120$, i. e. near to $1/137$) ; one shall try with caution to revive some modernized use of our spinorial non-linearity (alongside with approaches of Terazawa, supersymmetrical versions, Kaluza-Klein type theories, etc.). Indeed, with all miraculous successes of Glashow-Salam-Weinberg (electro-weak, better to be called electro-asthenonic) unification (about which we heard so contentful remarks of Nishijima, Yang, Maki, Ting and others at MESON50) and taking account quantum chromodynamics one sees too many arbitrairiness in Higgses, and ad hoc description of vaccuum ; so among other colleagues I am voting for going deeper to the sub-quarkian, preonic level (together with Salam-Pati, Terazawa et al.), [cf. D. Ivanenko, the article : "Perennial modernity of Einstein theory" in the Einstein Centenary Volume of the National Italian Academy of Sciences, published in Italian (Firenze, 1979) and secondary in English by Johnson Reprint Corporation, N.-Y. 1979, under the title ; "*Relativity, Quanta, Cosmology*", where many references are given].

The second and last point concerns our treatment of gravity applying some refined version of gauging gravitational field, which in a necessary way introduces torsion alongside with Riemann-Einsteinian curvature (i. e. the development in various versions of the well-known series of papers of Utiyama, Ivanenko-Brodsky-Sokolik, Hehl's group, Kibble, Trautman's group, C. N. Yang, Cho, etc. beginning from 1956 ~ 1961 up to recent days). Anyhow with all tremendous successes of Einstein-Hilbert general relativity, there are some unsolved problems even in the classical non-quantized GR, e.g. no unanimous agreement about treating singularities, or define the energy of gravitational field, etc. Clearly we are already in post-Einsteinian period, with a host of extensions of GR (torsion, Kaluza-Klein multidimensionality, versions of supergravity of above mentioned authors, also versions of Treder, Salam-Sezgin, Logunov group in Moscow, strings, etc.).

Together with G. Sardanashvili we tried to clarify more the gauging of Poincaré's group — i. e., Lorentz rotations with translations — also the sense of equivalence principle, etc., applying fiber bundle formalism and emphasizing the properties of gravitation as a kind of Goldstone type field ; (in other versions remarked also by Trautman, Ne'eman ; in preliminary form pointed earlier by Heisenberg, Ivanenko, and supported by Treder) cf. : D. Ivanenko and G. Sardanashvili, Phys. Rep. **C94** (1983), 1; also the recent book : The gauge theory of gravitation (in Russian, Publ. House, Moscow University, 1985, written by D. Ivanenko, G. Sardanashvili, P. Pronin). Now we try to treat in the same way not only gravity, but also supergravity (submitted to publication in *Prog. Theor. Phys.*, Kyoto and in *Trans. Superior Schools,* Physics in Russ. Tomsk). Containing we may hope at least a "grain of truth" ("atom of truth"), such Higgs-Goldstonic treatment

of vacuum, gravity and supergravity would constitute our humble contribution to elaborating the most unified theory of physics reality, developing in this manner the Yukawa-Sakata unified theories attempts.*)

*) For Goldstonic gauge treatment of gravity, cf. D. Ivanenko and G. Sardanashvili, Phys. Rep. C **94** (1983), 1.