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PASCUAL JORDAN

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EARTH EXPANSION
GENERAL RELATIVITY
PROBLEMS OF ABSTRACT ALGEBRA

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Hamburg University
FINAL REPORT 1967

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Contents

§ 1. Preface. This Final Report considers, as in former years of my contract work, three different branches of research:

I. Study of empirical facts giving evidence concerning Dirac's famous gravitational hypothesis.

II. Problems of General Relativity.

III. Mathematical theory of abstract algebras.

This time, from grounds to be explained just in the following lines, by far the greatest part of my Report is devoted to I; the research concerning II, III is treated in the following only to an appreciably less extent.

During the two years of contract work reported here I have been busy in the first line to write my book about EARTH EXPANSION, published 1966 by Vieweg at Brunswick. (An English edition is in preparation). I think this book to be one of the chief results of my scientific endeavour during all my life, and I was glad to be able to write in its preface: "This research was supported by the Aerospace Research Laboratories of the Office of Aerospace Research, USAF." A copy of the book is delivered along with the copies of this Report.

Naturally the content of the book cannot be recapitulated here; the book itself may serve as a chief part of my Report. But the topic of this book remained fascinating for me also after its completion, and the study of this matter has been continued still after its publication, several months ago. In several points new progress has been made in the meantime already; and new results of other authors gave the material for further new progress in this matter - the leading ideas of my theory of Earth expansion seems to me to be confirmed (and refined) also by all new published results touching upon this field of research. These new additions to the picture given in my book are the topic of my Report, part I.

Essential points are especially these ones:

A) A modification of my concept about the transition of core material of the Earth into mantle material leads to a formulation of my expansion theory without application of Ramsey's hypothesis, which probably must be discarded.

B) My result (discussed in my book) that Dirac's hypothesis can be formulated also in the manner that the ephemeridic time T of astronomers is not the same as time t measured by atomic clocks - in reality we have $T = (1 - \varepsilon t)t$ with a very small ε - showed a possibility to a direct experimental test of the hypothesis. This test has been begun (at first without any theoretical hypothesis) by specialists of atomic clocks measuring the relation of T and t. And then other specialists of atomic clocks evaluated the registrations of the first ones, according to my formula connecting T and t. They found that probably indeed $\varepsilon \neq 0$ (perhaps $\varepsilon = -2.10^{-10}$/year), but the registration must be continued in order to reduce the error limit which today is still too great. But clearly here a decisive test of Dirac's hypothesis is going on; and if the definitive result will indeed be positive, then one of the most sensational experimental results of the second half of this century will be attained. -

I had the pleasure, that the physicist, geophysicist and geologist W. Elsasser, the well renowned researcher in the theory of Earth magnetism, and one of the first readers of my mentioned book, wrote about it: "It is a magnificent collation of material which, to my knowledge, cannot be found in the same concentrated and lucid fashion at any other place, and by this very fact it constitutes a major contribution to earth science."

In Chapter II of the present Final Report, I give a characteristic piece of the work of my Hamburg Seminar for General Relativity. Though the chief aims of the work of this Seminar are lying in the study of exact solutions of Einstein's field equations and in the study (connected with these, but also with the general theory of gravitational waves) of "congruences", we have been busied also with the famous problem - which gave work already to many internationally prominent relativists, beginning with Einstein himself - of deriving the equation of motion for a small body ("mass point") from the general field equations. Especially my coworkers Beiglböck and Kundt made very valuable progress in this famous problem, using mathematical methods (topology) not applied in the former treatments, but decisively necessary. This beautiful example of the endeavour of our Seminar, given here as Chapter II, was formerly contained in one of my Administrative Reports.

In Chapter III, the shortest one of all, I give a short indication of the present status of my work in this field. One part of this work - investigation of half groups of idempotents - has been my preferred field of work 3 years ago; but my investigation of these problems has been interrupted and delayed by my endeavour to write my mentioned book. I hope to come back to this topic in the near future. Another part of my research concerning abstract algebra has been devoted to a certain generalisation of what American mathematicians call "Jordan algebras". It is the chief intention of Chapter III to show how my work in pure mathematics is interwoven with general mathematical research work of many mathematicians, partly in Europe and partly in USA.

CHAPTER I.

Introduction.

§ 2. The intention of this paragraph is at first to give a short summarizing account of the basic ideas of the interpretation of Earth development as caused by Dirac's decrease of gravitation. Special weight will be given to the indication that this interpretation is not an hypothetical one which could be correct (or could also be wrong). But this interpretation to a high degree is the unavoidable result of an analysis of empirical facts, quite free of hypothetical elements.

1) The existence of a process of Earth expansion, going on at least during the geological presence, is shown by the great worldwide system of connected a) oceanic rifts, and b) continental "Grabenbrüche". This has been discussed in my book, and is in agreement with the conviction of a series of prominent specialists - recapitulation of details is not needed here.

2) The essential topic is, to explain the differences between continents (including shelves) and oceans, considering the following facts:

A) Two preferred hypsographic levels in the surface of the Earth; separated from each other by a mostly steep continental slope.

B) The sialic material of the continental lumps, chemically different from the SIMA material, und from the upper ~~mant~~ mantle material.

C) The law of isostasy, showing not only the continental lumps situated in a swimming equilibrium in the denser lower material, but proving - together with A) - also the sial layer to have all over the world approximately the same thickness.

Only mountain folding caused some deviations from this con= stancy of thickness, on limited areas which are nearly to be neglected in comparision with the whole of the continental areas.

This point C) is the decisive one of the whole matter - it must be supplemented by the assertion that there is no possibility to invent any process able to create such a constancy of thickness from any former state of affairs lacking such constancy. Especially erosion would not be able to create such constancy independently from climatic differences; or to create sharp boundaries in the form of a steep continental slope.

Acknowledgement of this statement leads to two further consequences:

I) Corresponding to the spatial constancy of the thickness there must be also constancy in the course of time.

II) The total continental area (dry land plus shelves) must have remained constant during geological development.

For in absence of any agency able to create spatial constancy of thickness from deviating conditions - or to restore it after any destruction - also maintenance of this spatial constancy must have been impossible if change of this thickness in the course of time took place. After this, II) is a mere consequence, because the whole amount of sial could not vary if no process of creation of new sialic material took place - such a creation would have violated again I).

Therefore the thickness and the total area of the sial layer must be today the same as caused during the formation of the sial layer in its original form. There can be thought of only one condition which has been able to create a sialic layer showing the same thickness in its whole area - we have to infer that at a certain time the still fluid sial formed a closed layer around the whole globe. At that time the surface of the Earth must have been approximately equal to the present sum of the continental areas; the radius of that original Earth must have been about 65% of its present value.

The empirical facts of geography and paleontology, distribution of plants and animals, geological structure of the boundaries of continents, paleomagnetism and so on tend to show that a reconstruction of the original Earth in agreement with the ideas summarized above is possible. The reconstruction given by Brößke seems to be a good first approximation.

Concerning the famous problem of mountain folding the concept of expansion makes it probable that the chief primary cause ~~lies~~ in the fact that the continental lumps (no mountain folding has been proven in the deep sea) have been forced, lay by expansion, to diminish their curvature. of the folding

But expansion - according to the theory represented in my book - is only a consequence of the concept that, as Dirac assumed, the "constant" of gravitation is decreasing in the course of the development of the universe; and serious further consequences arise from this concept. The luminosities of stars are strongly dependent from the gravitational constant, and they must be therefore strongly decreasing with gravitation.

In this connection a recent theoretical result of R. Dicke and his colleagues may be mentioned; I learned about it when visiting him at Princeton in the fall 1966. The well known theory of star development allows greater precision, than in former treatments has been attained. With this enhanced precision it can be shown that according to the older hypothesis

- real constancy of the gravitational constant - temperatures at the surface of the Earth must have been below the freezing point during the whole times lying back more than ages showing already organic life.

This surely is a totally impossible consequence. As long as temperatures were below the freezing point, no origin of organic life could have been possible. Therefore there is no possibility to maintain the hypothesis of a really constant "constant" of gravitation, other than hoping for any mistake in these theoretical considerations and calculations - and that would be quite a weak hope.

Though in this manner Dicke gave a proof that the old assumption of an exactly constant "constant" of gravitation cannot be correct, he proved only a very slight variation of the gravitational constant G, and he is inclined to take indeed $-\dot{G}/G$ as considerably smaller than I did in my book.

My book contains also a discussion of the paleoclimatic consequences of the Dirac hypothesis, taken in the stronger form of $-\dot{G}/G = 10^{-9}$ per year. (Dicke prefers to assume only 10^{-10} or even 10^{-11}). According to Teller in this case the solar constant must have been considerably greater in the geological past than now; and according to ter Haar we must infer (if Dirac's hypothesis is true, and if the rate of $-\dot{G}/G = 10^{-9}$ per year is approximately the real one) that the Earth under the influence of a considerably greater solar constant must have shown in paleozoic times a closed layer of clouds around it. That our empirical knowledge seems to be in good agreement with this conclusion, has been shown in my book - paleobotanists give descriptions of climatic conditions in the carboniferous age which are in accord with the concept of a closed layer of clouds. We come back to this topic later.

Paleomagnetic results, interpreted in terms of earth expansion by van Hilten and by Khramov and Komissarova [1]) show that the radius of the Earth has been in the carboniferous age about 80% of the present one. Results of my book make it probable that then G must have been about 20% greater than now. According to Teller (and to the mechanical influence of decreasing G on the orbital movement of the earth) the solar constant then would have been about 5 times that of today.

The acceleration of falling bodies at the earth surface would have been in the carboniferous about 1,6 of that of today. At the other hand also the atmospheric pressure must have been about 1,6 of that of today (if the atmospheric mass remained approximately the same in the meantime).

To the latter numbers the following remark may be allowed: With greatest caution we can perhaps think that the well known phenomenon of extremely great insects in the carboniferous may have a connection with these data. We know that in the carboniferous age there existed (in different forms) dragon flies with wings up to about 30 cm. Surely there existed also such ones with wings of only one to two cm. But one does not know whether there existed also small flying insects, such as drosophila for instance. Perhaps for them the conditions of paleontological conservation may have been too bad. But the possibility seems to be open to further research whether they could not yet exist because the greater values of falling acceleration and of atmospheric pressure were more favourable for greater wings.

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[1]) A.Y. Vlassow, Rock Magnetism and Paleomagnetism. Krasnoyarsk 1963. (I got an English translation by Prof. B.C. Heezen).

§ 3. Comparision with Wegener's ideas. The theory of earth expansion has been put forward in my mentioned book in a very short and sketchy manner - only the great outlines have been discussed, leaving many details out of consideration. The empirical evidence for the basic ideas has been extracted practically only from recent, modern literature - in order to secure the best possible adaptation to recent results, and the modifications of older ideas and results, contained in them.

But now it seems to be useful to undertake a more complete comparision to A. Wegener's old ideas, as laid down in the last (fourth) edition of his pionier work "Die Entstehung der Kontinente und Ozeane" (1929). The new theory, being far from coinciding in all details with the old concepts of Wegener, retains essential similarities with his ideas, and the fascination going out of his own representation of his theory seems to be apt to give also additional interest to the theory of Earth expansion in those details which are similar to such of Wegener's theory. At the other hand it seems to be useful to look if deviations of the new theory from the old one really can help to improve the agreement with empirical facts.

~~At first~~ Surely there are several deep going differences:

1) Wegener believed in movements of the continents so fast that they would be observable in geodetic measurements. For instance he believed in a relative movement of Greenland, compared with Europe, in the order of magnitude of 30m/year. That is about the 10^4 fold of what ~~our~~ the new theory believes to be the case.

This part of his ideas surely must be held to be totally wrong. Certainly he overestimated the accuracy of geodetic measurements of his lifetime. It is probably not interesting to look for the causes of the erroneous geodetic results which at his life time seemed to prove movements which in reality are not existing.

Iceland seems to be the only place where geodetic measurement (finding out the velocity of the separation of the two halves of Iceland) possibly can help to prove processes of the type sought for by Wegener (to be interpreted now as proof of Earth expansion) still going on today - in a rate perhaps 10^{-4} of that assumed by Wegener . The results of the Iceland expedition of Niemczyk, Emschermann, Bernauer are discussed in my book.

2) Wegener's inclination to assume extremely young ages for many of the developments in the relative positions of the continents is probably a consequence of his wrong concepts of the velocities of continental movements. Especially the age of the northern part of the Atlantic is much greater than he thought. The southern Atlantic is, according to modern results, perhaps or probably younger than the northern part. No real proof seems to exist showing that the connection of Africa and South-America lasted still beyond the permian age. Clear evidence about the age of the South Atlantic is given by South African mountain foldings ("Swarte Berge") having an obvious continuation in South America. Folded in paleozoic times these mountains show that then the two continents, separated today, formed still a single continent.

Investigations of Brouwer, cited by Wegener, show strong parallelism between volcanism in Africa and South America still in jurassic ages. Surely this too is proof for old connection between both these continents; but it scarcely allows to infer that still at the time of the eruptive processes (jurassic age) the two present continents must have been connected. Only those deeper layers containing the sources of the eruptive material probably belong to the ages of still existing Gondwana; in later times with already separated continents the similarities of the underground still may have caused parallel development of volcanism. Only mountain folding can give (according to our interpretation of this process) sound proof for connection still at the time of the folding process. Even sedimentational developments can have been performed in close similarity in Africa and South America still after separation - as long, as the primary Grabenbruch was still of limited breadth.

Du Toit gave many details, mentioned by Wegener, about geological evidence concerning the former connection Africa/Sout America. But it seems to me that all really impressive points show connection only in paleozoic times; only weak or diffuse arguments have been given in favour of connection still in mesozoic times.

A modern representation of the matter, given by Woodford, [1]) discussed especially the so-called mesosaurus horizon as a proof for former connection of South America and Africa. This mesosaurus horizon, detected in parts of both these continents, contains not only the remains of mesosaurus, but also of other species of animals which lived in the triassic age; and for these different species clear similarities are to be seen between the African and the American forms. We have to acknowledge that this case as clear evidence for the old Gondwana continent. But only similarity exists, not identity - to such a degree that Woodford speaks of a barrier, separating the African and the American area of this horizon in the time of the existence of those species of animals. No sufficient arguments show that this "barrier" has not been the beginning of the Atlantic, still not very wide in triassic times. The common ancestors (not yet detected) of the residents of the two parts of this horizon may have lived in the paleozoic age.

Concerning the more northern parts of the Atlantic the eels show clearly - the European and African ones having

[1]) A.O. Woodford, Historical Geology. S. Francisco 1965

their fawning place near together with the Northamerican ones, in the Sargasso Sea - that the Atlantic in this part must have been much smaller than today, when the eels originated. But surely no possibility exists to infer from this that the age of the northern Atlantic would be smaller than paleozoic; since silurian times any part of the paleozoic ages may have seen the North Atlantic as a still relatively small but already existing ocean.

(These conclusions are not affected by a new hypothesis according to which the eels from the east, when swimming in the direction to the Sargasso Sea, do not reach it before dying. New eels for the east are then coming from the reserve of the American eels - they are by chance or by ocean currents deviating from the intended way west. I think this to be a quite unprobable hypothesis, neglecting the fact- it seems to be a fact -that the American eels need one year of growing in the ocean, but the eastern ones need 3 to 4 years).

Indeed concerning the western coast of Spain/Portugal Wegener himself inferred from the absence of correspondences with features in America that here the separation has to be a quite old one - older than the carboniferous age. Mountain folding showing old connections, took place 1) in the carboniferous age, 2) from silurian to devonian times, 3) in the precambrian age. Also the devonian "Old Red" sediments show old connections of North America, Greenland, Spitsbergen and Europe, without giving information about the times of separation.

But Wegener mentions only one geological fact seeming to him to show these connections to have been still existing even in diluvial times; this special arguments seems to me to be extremely weak.(Coincidence of the south boundaries of the diluvial ice cap, in America and in Europe, if Wegeners reconstruction is acknowledged). No really strong argument seems to exist which would prove that the old continental connections through the North Atlantic remained in existence still in mesozoic times.

In a similar manner also other examples discussed already by Wegener himself must be rediscussed today. For instance there seems to exist no convincing evidence that the connection of India with Madagascar and Africa held enough in order to give India a position south from the equator still in permian times.

It is highly interesting that Wegener discussed already also the idea that the sial layer may have been spread out over the whole globe originally. One of the most characteristic ideas of the expansion theory therefore was already contained in the old store of pioneer ideas put forward by Wegener. Evidently Wegener felt that all concepts of continental movements tend to make this idea of a sialic layer all over the globe a quite unavoidable consequence, without which this concept would lack the finally convincing round off. Not considering the possibility of Earth expansion, Wegener naturally had to use a further ad hoc hypothesis in order to make the idea of an originally closed sial layer possible. He thought that processes of mountain folding might have diminished the total area of the sialic layer from originally 100% to about 40%. In order to make this idea believable he emphasized that the old precambrian rocks show an especially high degree of folding.

Though this additional hypothesis of reduction of the sialic layer from 100% to 40% seems at first sight to be probably a little exaggerated, we prefer not to deny its possibility without precise argumentation. Our argument is that this idea makes it again necessary to understand the spatial constancy of the thickness of the sial layer as a fact resulting from causes acting throughout the geologic development - and it has been the aim of the basic discussion in § 2 to show that it is urgent to construct a theory, liberating us from the necessity to make such assumptions, ~~and~~ but explaining this constant thickness as a remained effect of conditions during the origin of the Earth.

Therefore Wegener's remarks about the possibility of an originally closed sialic layer around the Earth seem to me to show additionally that our picture of Earth development cannot become really satisfying without taking account of the expansion of the earth; and at the other hand that the basic ideas about this development necessarily must be just those assumed in my book. Only the Ramsey hypothesis, enclosed into the set of basic ideas of my theory, obviously must be abandoned. This will be discussed later.

Naturally all these mentioned differences between Wegener's and our ideas do not diminish the basical merit of Wegener about the understanding of earth structure. His clear statement that all provable "land bridges" which existed in former geological times must be regarded as proof of former connection of today separated continents, surely was a pioneer act of doing in the history of geology. That all ideas of transmutation between regions of the deep ocean bottom and continental regions are to be discarded has been shown by Wegener in wonderful clarity by the remark that all marine sediments on the continents have been deposited in shallow seas, never in the deep sea.

§ 4. Paleoclimatology in Wegener's and in the new theory. A further point of difference between Wegener and the modern concept of Earth expansion is that he believed in polar migration. In this point still many authors of today hesitate to discard the old idea - though a) the catastrophic failure of the concept of polar migration to explain the facts o paleomagnetism, as emphasized by Heezen, and b) the mechanical impossibility of polar migration, shows us the necessity to discard it entirely. This topic has been discussed in detail in my book. Concerning b), it is to be emphasized that also the hypothesis of slow convection currents in the mantle of the Earth can scarcely do anything to overcome the argument that relatively instanteneous adaptation of the oblateness of the Earth to a directional change of the axis of the Earth would be necessary to make polar migration mechanically possible. For also by acknowledging the slow convection currents, we could justify only an extremely slow adaptation of the oblateness to such directional changes. Polar migration therefore cannot remain a part of any modern theory of Earth development.

Many interesting points are contained in Wegener's ideas about the paleoclimatology especially of the paleozoic ages. The modern problem of distinguishing between glacial and pseudo=glacial structures - the actual situation of today has been discussed in my book - has been of great importance already during Wegener's life time: Many facts giving perhaps evidence of former glaciation are perhaps only similar to such evidence, being in unable to prove really glaciation. That much controverse and many discrepancies exist in modern thought about this topic, has been emphasized especially by Schindewolf, as mentioned in my book. Indeed one can perhaps say that in a great percentage of all single cases it remains left to a very personal decision of the researcher to say whether the investigated case represents an example of glaciation or of pseudoglaciation. Therefore it may be useful to emphasize once more (as done already in my book) the <u>statistical</u> point of view: Any obvious example of <u>heaping</u> of glacial <u>or</u> pseudoglacial structures makes it probable that at least some parts of them are really glacially caused.

Some modern authors expressed the judgement that <u>tillites</u>, where existing, are convincing proof of <u>real</u> glaciation. Therefore it seems to me to be highly interesting that <u>Wegener</u> quotes with consent <u>Van Waterschoot van der Gracht</u> who very decidedly opposes to the meaning that tillites are in all cases real proof of glaciation.

This point is of special interest in connection with the problem of permocarboniferous glaciation in Antarctis. Different investigators emphasized their result that glaciation <u>there</u> has been absent in permocarboniferous times. But later tillites have been found there, and some authors believe that now a sure proof of glaciation is given. But perhaps this conclusion remains uncertain?

No less interesting is the following point. In my book I said that it would be methodically dangerous to try to make decisions about the glacial or pseudoglacial character (for instance of conglomerates) at the basis of a theoretical (or better: hypothetical) picture of the questionable paleoclimatic conditions.

If we allow us to be influenced concerning our interpretation of single cases, as glacial or pseudoglacial, by any theoretical picture of the whole matter, we cannot use our results as contributions to the <u>proof</u> that this picture is the correct one. We better try to clear the single special cases <u>independently</u> from theoretical assumptions - only in this manner we are able to gather material which can give really proof or negation of theoretical ideas. To show that the distinction between glacial and pseudoglacial structures can be made in such a manner as to correspond to a chosen interpretation of the whole of facts, is a statement the value of which remains open to doubt. And all details of our decisions must be rediscussed if a basically new, other picture of the whole matter comes to be discussed.

In my book I mentioned a case where an author was inclined to think a certain case to be one of pseudoglaciation because in the same region - soon afterwards in the geological development - a more warm climate has been recognised from other sources of information. The author was convinced that such rapid change of climatic conditions would be unlikely. But the new picture of permocarboniferous glaciation, as given in my book, gives also quite new viewpoints about what might be probable or not. The chief points of this picture are the following ones:

That old glaciation (or any series of varying glaciations) in contrast to the dilucial ones was not circumpolar, but covered huge areas of the whole surface of the earth, preferring a certain chiefly equatorial belt. It developed under a closed layer of clouds, and it showed comparatively much change in its areas and its strength. Its features probably remained - in changing strengths and varying areas - beginning probably already in devonian time, and extending in some regions perhaps to triassic ages, only the maximal glaciations being confined to the carboniferous and permian ages. These points are secondary consequences of the fact that ~~towards~~ about the end of paleozoic times there must have been a closed layer of clouds around the whole earth - if Dirac's idea is correct. (Details are discussed in my book). till

From this general picture we must infer that nearly every conclusion drawn from the old conviction of close similarities between the permobarbonian and the diluvial glaciations hits nearly exactly the contrary to what must concluded from the new picture.

For instance the existence of considerable permocarboniferous glaciations in central Africa was to be thought a quite unprobable feature at the basis of all older (and especially also Wegener's) ideas about paleoclimatology. For us - taking the new, changed theoretical picture - it would be a very natural fact (if indeed it is a fact). It would be wrong surely to infer that now we are obliged to acknowledge without further examination the results of Sluys about glaciation in the Congo basin. But surely this case now has to be rediscussed, in such a manner that we avoid to be influenced by any theory, looking only for local evidence for or against real glaciation. Perhaps in this case the statistical point of view, mentioned above, may be applicable, but naturally not without greatest care. Wegener was inclined to judge that in the Congo basin there might have been only pseudoglacial features - because it seems impossible to invent hypotheses of polar migration able to make the Congo situated in a polar region. But he put forward also another argument: In South Africa the glaciation seems to show a certain northern limit; this according to Wegener makes it improbable that another region of glaciation would lie beyond that limit. But also this argument is taken from the perhaps totally wrong axiom that the permocarboniferous glaciation must have been similar to the diluvium. The new picture resulting from Dirac's hypothesis does not contain the thesis that there existed only two (polar) closed regions of glaciation - the vast regions in which there existed favourable conditions for glaciation were probably only partially (in sometimes comparably fast change) glaciated.

Wegener mentioned also that the authors Hobson and Tschernischew believed to see traces of ice in the Ruhr basin, respectively the Ural. He felt himself so sure that that must be false, that he did not even give the literature concerning this point. Surely it becomes now a very actual and urgent task to rediscuss all those older reports which have been discarded, only because they were not in harmony with dogmatical hypotheses. Prejudices inferred from theories now discarded must be abandoned.

That the new theory is independent from polar migration hypotheses seems to me to be one of its best properties. For the old glaciations gave - in the former paleoclimatological discussions - the chief motivation for assuming polar migration: The authors (including Wegener) were inclined to believe that glaciation (where and when it must be acknowledged) proves that its region must have been not far from one of the poles at the time of its glaciation. This concept made it necessary to believe in polar migration. Paleomagnetism has been regarded by many authors as a confirmation of polar migration, because from paleomagnetic data one could calculate the path of the magnetic north pole during geological ages. Astonishingly many authors were inclined to think that also the following fact, emphasized by Heezen, gave no necessity to discard the idea of polar migration: Any continental region (as for instance Australia, or India), analysed sufficiently about its paleomagnetic properties, can be used to calculate the path of the pole; but any pair of different continental regions, used in this manner, gives results which in most cases are in full contradiction. Interpretation of this fact - without discarding the idea of polar migration as the leading idea of interpretation - has been tried by assuming a rotational degree of freedom for each greater continental region; and naturally by this additional hypothesis all paleomagnetic data can be made interpretable as result of polar migration - not alone the real data can be interpreted thus, but also any other set of data (invented arbitrarily) could be interpreted in such a manner: This possibility does not show a property of the real dates, but is a mathematical triviality, independent of all properties of the empirical facts. And this interpretation can be performed with use of any arbitrarily chosen path of the pole - after this path has been chosen arbitrarily, the empirical results from a region C can be used to determine the rotational movements of C in the course of time. No information at all is gained by such a method, about the question what might have been the real path of the pole. Also the assumption that the pole would have made no considerable movement at all remains in agreement with this manner of interpretation. Therefore the facts emphasized by Heezen contain the clear proof that paleomagnetism gives us reason to discard the concept of polar migration totally, because it is to be seen as totally useless, if applied so as not to be in obvious contradiction to the facts. And surely nobody would be inclined to maintain still today the idea of polar migration, if there were not the old glaciations, far from polar regions of today. The concept of glaciations below a closed layer of clouds - so that the polar regions must not be centres of glaciation - therefore gives us the freedom to interprete old glaciations without using any hypothesis about polar migration.

In my book I came to the result - from the study of diluvial glaciations with use of Dirac's hypothesis - that the former ideas about the duration of the diluvium have to be corrected. The old meaning was that the diluvium filled a time interval of about 0,6 million years; and I came to the result, that in reality this assumed duration must be corrected by a factor 2 or 3 . I found, before concluding my manuscript, that facts of African prehistory have already given empirical arguments in the same direction.

But only afterwards I learned also that purely geological and oceanographic evidence caused a modification of dating, giving 1,5 million years[1]) instead of 0,6 . This seems to me to be a beautiful confirmation of my considerations.

Addition to § 4. The following point may be mentioned here, which is to be added to our information about the rotation of the Earth - it is only loosely connected with paleoclimatology. From the study of fossil corals Wells found that in Devonian times the year had 400 days (and about 380 in the Carboniferous). This has been mentioned in my book. But when writing it I did not yet know that Scrutton found - apart from daily and yearly growth rings - also monthly bands; they probably are caused by the fact that from month to month the tidal variation of the water in the course of the day shows periodicity. From these monthly bands one infers that in the Devonian the month had 28.5 days.

From the dates mentioned already in my book, and discussed there in connection with other facts, I came to the conclusion that probably in the present time there does not really exist enough tidal friction of any kind in order to give an observable effect in the motion of the Moon: Spencer Jones' revised evaluation of the empirical facts seems to show that the existence of the empirical fact which Jeffreys tried to explain by his famous theory of tidal friction cannot be proven really. But in the meantime (as

[1]) Ericson, D.B., and G. Wollin: The Deep and the Past. New York 1964.

discussed in my book), <u>Munk</u> and <u>MacDonald</u> found that tidal friction in the Bering Sea in reality is to small in order to justify <u>Jeffrey's</u> theory.

Now the monthly bands in Devonian corals show undoubtedly that in former geological ages tidal friction must have been in action. I should like to emphasize that this is exactly what must be expected from the theory of earth expansion. For from this theory there is to be inferred a consequence - and <u>Egyed</u> has shown this consequence to be empirically confirmed, so that we have here a <u>fact</u>, independent from any theory - namely, that in the past there were considerably greater shelf areas than today. (Today there is <u>only</u> the Bering sea as a <u>big</u> shelf area). Therefore there must have been in action an oceanic tidal friction much greater in Devonian and Carboniferous ages than today.

<u>§ 5. Ramsey's hypothesis about the interior of the Earth.</u> The chief progress made in my research concerning the Earth and Dirac's <u>hypothesis</u>, since the publication of my book, is the result that and how the <u>Ramsey</u> hypothesis, concerning the core of the Earth, can be avoided as a necessary element of the theory of Earth expansion. This <u>Ramsey</u> hypothesis has been assumed to be correct in my book, and the picture of Earth development, as presented in my book, made essential use of this hypothesis. It seemed to me, when writing the book, that this hypothesis would be necessary in order to give to the theory of Earth expansion a really satisfactory form - so that I was inclined to conclude that the success of the expansion theory would give also a strong argument in favour of <u>Ramsey's</u> hypothesis - though already some years ago <u>Edward Teller</u>, when I visited him, warned me against this hypothesis.

I had to change this conviction after a long discussion with W. Elsasser whom I could visit at Princeton in the fall 1966. (The visit has been made possible by Aerospace Research). By this discussion I learned that from quantum theoretical calculations it is to be seen that Ramsey's hypothesis is really impossible.

Therefore I had to think about the question how to make the theory of Earth expansion independent of Ramsey's hypothesis - this problem gave to me my chief business from the date of our discussion at Princeton September 1966 to the end of the year. I believe to have shown that indeed a new formulation of my theory is possible, avoiding the Ramsey hypothesis. The new concept has been developed in close contact with M. Elsasser, with whom I had a lively correspondence since my visit to Princeton. I am deeply indebted to him for his valuable advices and informations. What I shall write down here belongs to the material of an article prepared now.

The content of Ramsey's hypothesis was the following one: The (outer) core of the Earth is not chemically different from the mantle, but represents another phase of the (probably olivinie) material of the mantle - we have there at the sphere about 3000 km below the surface, only a phase limit, not a chemical limit. Why this idea seemed to me to be well suited to agree with the concepts of the expansion theory, will be seen in the following.

This Ramsey hypothesis has been taken seriously also by Bullen in his famous book about Seismology.[1]) This book

[1]) K.E. Bullen, An introduction to the theory of seismology. Cambridge. 3.ed. 1963

summarizes, as well known, chiefly those methods of research, concerning the interior of the Earth, which may be called methods of classical physics.- The 3. edition brought also an essential addition treating Bullen's latest results defining a new parameter "η" which - definable from seismologically measurable quantities - can give information about chemical homogeneity or inhomogeneity. ~~In the deduction of the Debye temperature of the material comes into the play.~~

But there is ~~still another~~ also a possibility to gain information about the interior of the Earth by using quantum mechanics; and from here comes the decision that Ramsey's hypothesis cannot be maintained.

Modern development of high pressure physics - especially with application of shock waves - gave many instructive results beyond the scope of the older famous research of Bridgeman. But for still greater pressures there is a gap in the empirically founded informations - and this gap could be filled out by theoretical work: Elsasser 1951 reported shortly the chief results of Feynman, Metropolis, Teller, Slater and Krutter, Jensen in this field. [1]) Applying the method of Thomas-Fermi these authors came to very clear results concerning the physical properties of different materials under pressures as they are given in deeper zones of the Earth interior. A very special consequence of these computations is, that really any material of the chemical composition of Olivine cannot possess (in any of its phases) the density of the outer Earth core under those pressures which exist there.

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[1]) W. Elsasser, Science 113, 105 (1951).

Surely further quantum mechanical calculations would be possible and useful. The results attainable by the Thomas-Fermi method are already known to a high extent. But the applieation of the Hartree-Fock method, to ion-lattices, will get us very valuable still more precise and more instructive results - research in this direction, possible though necessitating much work of calculation, has only scarcely begun. We shall see, that the definitive decision about Dirac's hypothesis probably would be possible already now, if the results of these theoretical calculations would already be available. For what we sketch in the following, will give a very narrow path to a quantitative theory of Earth expansion - and the only possible one. Only in this manner - sketched below - can it become possible to maintain Dirac's hypothesis as a key to the history of the Earth. And the possibility to come to a really satisfying theory (in a quantitative manner) could be tested in all details, if the needed theoretical calculations would already be performed.

That I have been inclined to believe in Ramsey's hypothesis and to see in it an essential point in connexion with the theory of Earth expansion, came from the following circumstances. In § 2 the basic concepts of the expansion theory have been recapitulated. It may be mentioned additionally that the interpretation of processes of mountain folding in terms of diminishing curvature of continental lumps has been studied in the last months by my coworker ~~[illegible]~~ Glashoff in an article which seems to me in a similar manner fundamental for the understanding of orogenesis, as the ideas of my late coworker Binge were fundamental for a real understanding of volcanism.

(Details about this in my book). The article of Glashoff is to be published soon.

We saw in § 2 , that at the beginning of Earth history the radius of the Earth must have been about 65% of its present value.

But in Carboniferous time the radius of the earth had still only about 80% of its present value. This number of about 80% comes from paleomagnetic measurements, interpreted not at the basis of polar migration, but at the basis of Earth expansion; van Hilten and at the other hand Khramov and Komissarova [1]) came to this conclusion, which is in very good agreement with what we concluded above (§3) concerning the beginning for instance of the formation of the Atlantic.

This result means that we had two different parts of the process of Earth expansion - which has been very slow in older times, ending about 270 million years ago; and was more rapid (to one full order of magnitude) since about the middle of the Carboniferous.

It must be emphasized that this result, seeming at first a little paradoxical, is not a peculiarity of the expansion theory. We have to face analoguous paradoxies also in the frame of Wegener's older theory: According to Wegener the greater part of the history of Earth saw only one huge continent and one huge ocean; but beginning at a time only 270 years ago (or, according to Wegener's own estimates, still later) the separation of this one primordial continent

[1]) Zitat ~~wie~~ S.11. ~~[illegible]~~

into smaller fragments has been performed. The paradox is the same in both theories, <u>without</u> and <u>with</u> expansion. Only the formulation (or description) of the paradox changes, if we go from <u>Wegener</u>'s theory to the new one.

It can be easily understood that this situation led me to hope that Ramsey's hypothesis might be correct. For at the basis of this hypothesis the following interpretation of the two different parts of the history of Earth expansion would seem probable: In the first (mostly precambrian) part the process of expansion of the Earth has been an elastic deformation - owing to the decrease of the gravitational constant G the Earth performed a very slow elastic expansion. Coworkers of Dicke calculated, that in this case - according to Bullen's model of the interior of the Earth - the following relation between expansion and gravitational decrease must have been fulfilled:

$$\dot{R}/R = -0{,}1 \cdot \dot{G}/G \quad .$$

This surely gives only a very slow expansion, unsuited for what we inferred from the facts about times after the carboniferous age. But now let us look at the role of mantle and core in the process of expansion.

The boundary between the mantle and the core, if a phase boundary, would decrease in the course of time, and in very old ages it must have lain practically directly under the sial layer. But as soon as a considerable part of the mantle phase had come to existence, the chief part of further expansion must have been performed by phase transformation, producing more mantle material from former core material. It is evident - at least in a qualitative manner - that in this second part of the expansion process the rate of expansion would have to be considerably greater. Therefore a tentative theoretical interpretation of the seemingly paradox facts would have been possible, by the assumption that the formation of the mantle, by phase transformation of core material, would have begun in the course of the Carboniferous.

But consideration of the results of Thomas-Fermi calculation necessitates us to discard the Ramsey hypothesis, and we have to look for possibilities to maintain the basic principle of our explanation now without this hypothesis. It seems to me unavoidable to maintain the idea that during the second part of the expansion process matter from the core has been added to the mantle. But now we can think about such a possibility only in such a manner that the Fe/Ni of the mantle underwent a process of chemical separation - giving away other elements which before this separation were solved in the core.

Now in older literature one mostly thought that the mantle of the earth would be nearly chemically homogeneous; and then there would seem to be no possibility that in former time considerable parts of its material could have been solved in the core. Olivin contains a great amount of oxygen; and one knows that fluid Fe/Ni can solve only small amounts of oxygen. [1]) Therefore new difficulties seem to arise; but they are removed - or reduced to questions which can be tested only by quantitative theoretical results not yet available - in consequence of very interesting

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[1]) B.J. Alder, J. Geoph. Res. 71, 4973(1966).

results of Anderson [1]). Already Bullen discussed in the third edition of his famous book the problem of chemical homogeneity or inhomogeneity in the mantle of the earth, and he came to results unfavourable for the old hypothesis of homogeneity. He defined a parameter η which must be equal to 1 in the case of homogeneity, but > 1 in inhomogeneous matter. Anderson in a very ingenious manner made use of laws of the physics of solid matter, coming from the theory of crystal lattices - though valid only in a rough approximation these laws can give very valuable additional information about the interior of the earth, if applied in the manner of Anderson. In this manner he won from known empirical data a more clear information about the value of η in different layers of the mantle. His results are summarized in the following table, giving the thicknesses of different layers, beginning below the crust, and going down to the inner boundary of the mantle. These layers are separated by well known spherical boundaries, as the Byerly level (400 km deep), and the Repetti level, 1000 km deep.

The results are:

layer	η
33 - 400 km	1,0
400 - 1000 km	1,8
1000 - 2500 km	1,4
2500 - 2900 km	3,0

[1]) O.L. Anderson, Transact. New York Acad. Sci. II, 27, 298 (1965)

As one sees, only above the Byerly level homogeneity seems to be quite certain; the deeper layers, forming the greater part of the mantle, probably are not homogeneous.

From this statement the following modification of my interpretation of the transition of core material to mantle material seems to be suited for further discussion: We must distinguish between that part of the mantle lying above the Byerly level, from the other one beneath it. The layer above this level must be interpreted as the original or old part of the mantle, formed already in the primary time of earth development. But the deeper, inhomogeneous part may be assumed to be the result of separation of formerly solved matter out of the fluid Fe/Ni.

This idea surely must be tested still in the future by quantitative theoretical research concerning the physics of the possibly involved elements at high pressures. But in contrast to an interpretation making use of Ramsey's hypothesis this modified concept does not contradict any known physical fact or law.

This modified interpretation is also in best accord with some further considerations about the origin of the system earth and moon. As one knows, the smaller density of the material of the moon, as compared with the average density of the earth, caused many discussions. The idea of a division of a primary whole planet into the present earth and the moon is since long years dismissed from further discussion, though for some time it has been thought to give the convincing answer. But many authors believed in the hypothesis of a capture of the moon - which might have originated elsewhere (perhaps even outside of our planetary system, as some authors are inclined to believe) - by the earth. Severe arguments contradict such an idea of "capture". According to the laws of Newton's mechanics, such a capture would be possible only as the result of an encounter of (at least) three bodies - otherwise the capture would have been mechanically impossible. But assuming any third body which would have made possible the capture by a near encounter with earth and moon (extremely improbable in itself), we must make any suitable hypothesis about the third celestial body, and its fate before and after the encounter. One of the three bodies must have come indeed from the outer interstellar space - and must have left the planetary system afterwards again. For there does not exist in the planetary system any body which could be identified with the third partner of the hypothetical encounter; and then again it seems to be ununderstandable that the moon came by such an encounter into its present orbit around the earth.

Now Bullen again found a help to understand these paradox facts in an unifying manner from the Ramsey hypothesis: Taking this hypothesis as correct, one would be able to assume that the earth and the moon in spite of their different densities would have the same chemical composition; and that seemed to be again a strong argument in favour of Ramsey's idea. But now we know from atomic theory that a different explanation must be found.

In order to avoid any capture hypothesis one has to assume that already in the process of formation of earth and moon a chemical separation must have taken place,- the <u>greater</u> part of the primordial matter (or the "proto planet") assembling itself around the iron part of this matter; and some residual matter - unable to join the chief part, in consequence of too great rotational impulse - being left for the moon. This idea seems to be very natural. Thinking about the protoplanet as an assemblage of particles from gas molecules and dust to small and great meteoritic bodies we have to infer that movement induced by gravitational attraction must have been slowed down by considerable friction. Therefore the iron meteorites must have fallen into the developing central mass with greater velocities than the stony meteorites. In the centre a core developed consisting chiefly of iron, but with considerable contributions also of other elements, there being no cause to prevent that also some amount of stony meteorites fell into the mass centre. As soon as the building process of this central mass caused heating and melting, a chemical separation began - in consequence of the low solubility of oxygen in fluid iron, a mantle of oxydes formed itself around the fluid Fe/Ni core; this core still containing great amounts of other elements, but without more than perhaps 10% of oxygen.

It seems to me to be probable that this mantle (and its crust), the formation of which belonged to the oldest part of the development of the earth, is identical with that part of the present mantle which is outside of the Byerly level; and that the other, greater part of the mantle, below this level, came to existence by a process of separation, of the major part of those elements from the core which in precambrian times were contained in solution in the Fe/Ni.

If this interpretation is correct, the separation began not at once after the formation of the primordial earth, but only several billions of years later, in paleozoic times. This consequence surely is not unnatural: After the formation of the core and the original mantle there was at first an equilibrium of long duration. But in the course of expansion, conditions of pressure and temperature in the earth interior changed - very slowly, but with great effect in the course of time. Only several billion years later an effectual separation of solved matter from the fluid core began.

In this manner, it seems to be possible to give a quite satisfying sketch of earth development, including earth expansion, without making use of Ramsey's hypothesis which surely cannot be maintained.

§ 6. Quantitative values about Dirac's hypothesis.
As shown in my book, Dirac's hypothesis is highly meaningful also with respect to the theory of the planetary system. Without going into details here, I summarize the result of this part of my investigations in the following manner:

I. The concept of ephemeridic time remains definable and meaningful also in the case that Dirac's hypothesis is correct.

As well known, ephemeridic time has been introduced into finer discussions of astronomical measurement, in order to become independent of changes in the rotation of the earth. This ephemeridic time is defined in such a manner that the year and also the frequencies of the orbital motions of the other planets remain constant - and all finer details of these motions, calculable by Newtonian mechanics, as arising from the mutual disturbances between the planets, are eactly in agreement with this theoretical analyse. If in spite of Dirac's ~~theory~~ hypothesis G would be exactly constant, then this ephemeridic time would be identical with the time coordinate of an exact system of inertia, measurable with atomic clocks. It is by no means trivial that this definition of ephemeridic time remains applicable also in the case that Dirac's hypothesis is right.

II. But if this hypothesis is correct, then the ephemeridic time scale ceases to be identical with the time given by atomic clocks.

We can now consider this as a new equivalent formulation of Dirac's hypothesis, that ephemeridic time and atomic time are different from each other, as precisized in the following:

III. The ephemeridic time T, expressed as a function of atomic time t, takes the form

$$T = (1 - \varepsilon t)t \; ;$$

$$-\varepsilon = \frac{\dot{G}}{G} .$$

Now there is a possibility of a direct empirical approach to this new formulation of Dirac's hypothesis; and I learned shortly ago that results in this direction are already available. My information about this matter is today only a preliminary one, and I have to omit here details of the topic. But this seems to become such a revolutionary progress that I feel that I must not omit it from a short consideration.

The decisive fact is that precision measurements about ephemeridic time, by atomic clocks, are already going on since about 10 years, in current registration. The two authors performing these measurements, obviously did not know about Dirac's hypothesis and the connection of this hypothesis with their measurement. But two other authors recognised this connection, and evaluated the results according to my formula $T = (1 - \varepsilon t)t$. Today the registration (about 10 years) is not yet long enough in order to allow a definitive decision - the limits of probable error are still a little too great. But already now it has become probable, that there exists at least a positive effect, to the amount of

$$- \varepsilon = 2.10^{10}/\text{year}.$$

It is to expected that further registration may lead quite fast to a reduction of the error limit. If this indeed gives a definitively positive result, Dirac's hypothesis will be proven - that would be certainly one of the most sensational experimental results of the second half of this century.

At the other hand the value of $\dot{R}/R$ (with R = radius of the earth) according to paleomagnetic measurements, can now be given with greater precision and certainty than before: The result, discussed in my book, of van Hilten has been confirmed by Russian authors, as mentioned above. Therefore the value of $\dot{R}/R = 10^{-9}$, as estimated in my book, can now be replaced by the better value

$$\dot{R}/R = 6.10^{-10} \text{ /year .}$$

At the other hand, from the increase of the length of the day, measured astronomically and with the help of modern clocks, in the ephemeridic time scale, we have in absence of tidal friction

$$\dot{R}/R + \dot{G}/G = 2.10^{-10} \text{ /year;}$$

this is the greater one of two possible values, the smaller one being one half of it, as discussed in my book.

Comparing these two values with the empirical result above, about ε, we can infer two consequences:

1) Probably the real value of ε must be twice as great as the preliminary value given above.

2) The absence of tidal friction - in the present age - is necessary, if our interpretations are correct. For otherwise this tidal friction would necessitate a still greater decrease of the rotational frequency of the earth.

According to Ehlers and Schücking the devonic coral dates show that at that time there was

$$\dot{R}/R + \dot{G}/G = 0{,}3 . 10^{-10}/\text{year};$$

that would mean that $\dot{R}/R$ in the Devon amounted to only about $4 . 10^{-10}$ /year, about 65% of today. That seems to be in satisfying agreement with our ideas about the transition of former slow expansion to the later more rapid expansion, in the course of the paleozoicum.

CHAPTER II

§7. Field equations and equations of motion

In Newtonian theory the motion of an extended body, or system of extended bodies is referred to its center of mass, or center of gravity, or center of motion. The union of these centers in 4-dimensional spacetime (for varying time) is called its center line. For a long time, physicists did not know if a unique relativistic generalization of this definition existed, and if it was useful. It will be shown in the following that indeed such generalizations exist, and are at the basis of the problem of motion.

Consider a finite matter distribution described by its symmetric 4-momentum density $T^{ab} = T^{(ab)}$. Its time-time-component

(1) $$\mu = \mu(u) \quad := T^{ab} u_a u_b$$

(for arbitrary timelike direction u_a, $0 \leq a \leq 3$, $u^a u_a = -1$, velocity of light = unity), is the rest energy density measured by the observer corresponding to u_a, and will be assumed non-negative. Conservation of 4-momentum is expressed by the divergence law

(2) $$T^{ab}{}_{;b} = 0 \ .$$

Suppose spacetime to be flat. The center of mass X^a of the distribution T^{ab} at time x^0 is defined, with respect to Minkowskian coordinates, as

(3) $$\boxed{X^a \ := \int_u x^a \mu \, d^3x \Big/ \int_u \mu \, d^3x} \ .$$

Here the integrals are understood to be taken over the hyperplane x^0 = const, or, in physical terms, over the rest space of an observer u_a (whose tangents satisfy $u_a dx^a = 0$). The gravity center X^a is thus observer-dependent: $X^a = X^a(u)$. Insertion of (1) yields

(4) $$X^a = u_b \int_{\mathcal{U}} x^a T^{bc} d\overset{*}{x}_c \Big/ u_b \int_{\mathcal{U}} T^{bc} d\overset{*}{x}_c$$

in which $d\overset{*}{x}_a := \frac{1}{3!}\eta_{abcd}\, dx^{bcd}$ is the 3-space vector volume element: $d\overset{*}{x}_a \sim u_a$, and u_a is assumed constant. Multiplying equation (4) by its right hand side denominator, one arrives at the equivalent formula

(5) $$0 = u_b \int_{\mathcal{U}} (x^a - X^a) T^{bc} d\overset{*}{x}_c \quad .$$

~~.....~~ Or, by adding zero

(6) $$\boxed{0 = M^{ab}(X(u))u_b}$$

where

(7) $$\boxed{M^{ab}(x) := 2\int_{\mathcal{U}} (y^{[a} - x^{[a})\; T^{b]c} d\overset{*}{y}_c}$$

is the 4-angular-momentum-tensor of the matter distribution with respect to a world point $x \leftrightarrow x^a$. Equation (6) says that the angular momentum is purely spatial, if referred to the gravity center X (for any observer u_a). This condition has been shown to be a restatement of definition (3).

Let T^{ab} be given, and a constant observer field u_a specified. What is the equation of motion of the matter? We write

(8) $$M \;:= \int_{u} \mu \, d^3x \;=\; - u_a \int_{u} T^{ab} \overset{*}{dx}_b$$

for the <u>mass</u> contained in the rest space of u_a . M is positive by assumption, and conserved in time:

(9) $$\frac{d}{dx^c} M = - u_a \int_{u} u^c \partial_c T^{ab} \overset{*}{dx}_b = (\delta^c_a - h^c_a) \int_{u} T^{ab}{}_{,c} \overset{*}{dx}_b = 0$$

Here $h^c_a := \delta^c_a + u_a u^c$ is the projection tensor onto the observer's rest space; for $u^a \overset{*}{=} \delta^a_o$ its non-spatial components vanish so that the integrand turns into a space divergence, whence does not contribute to the integral. The other terms involving Kronecker's tensor vanishes on account of (2). Now we differentiate (4) , and insert (8), (9) :

$$\frac{d}{dx^c} X^a = - M^{-1} \int_{u} \underbrace{u^d \partial_d (x^a \; u_b \; T^{bc}) \overset{*}{dx}_c}_{u^a u_b T^{bc} + x^a (h^d_b - \cancel{\delta^d_b}) \partial_d T^{bc}}$$

Partial integration yields, for $u^a \overset{*}{=} \delta^a_c$

(10) $$\frac{d}{dx^c} X^a = \delta^a_o + M^{-1} \delta^{\alpha}_{\beta} \int_{u} T^{\beta c} \overset{*}{dx}_c = M^{-1} \; P^a$$

where by definition

(11) $$\boxed{P^a \;:= \int_{u} T^{ab} \overset{*}{dx}_b} \quad , \quad (\Rightarrow P^o = M),$$

is the 4-<u>momentum</u> of the matter distribution (as observed by u_a). Equation (10) is the expected <u>equation</u> of <u>motion</u> The 4-velocity of the center of mass is given by the 4-momentum divided by the mass. Note that P^a is conserved in time, which follows as in (9). The center of mass therefore "moves" along a timelike geodesic.

P^a is not only conserved in time but even independent of u_a : the proof follows from (2) by application of Gauß's theorem

$$(12) \qquad \int_{\text{boundary}} T^{ab} \overset{*}{dx}_b = \int_{\text{interior}} T^{ab}{}_{,b} \overset{*}{dx} = 0 ,$$

so that the integral is equal for all pairs of integration hypersurfaces which can be bent (at infinity) into one closed hypersurface.

For constant u_a, the gravity centers defined by (6) form a geodesic when x^0 is varied; they form the center line belonging to u_a. In general, however, different observers will obtain different center lines ~~xxxxxxxx~~ (in flat spacetime). As we have seen, each such line must be a straight line in direction of (the constant vector) P^a. The union of all these parallel straight lines forms a solid 4-cylinder of maximal radius

$$(13) \qquad r = \left| M_{ab}(X(P)) \quad M^{ab}(X(P)) \right| / \left| P^c P_c \right|^{1/2} .$$

In words: r is given by the magnitude of the eigen angular momentum divided by the rest mass. For a proof, note first that there exists one preferred center line X(P): the line belonging to $u_a \sim P_a$. This is the line found by an observer who is at rest relative to the center of the distribution. Now consider all the hyperplanes through one of its points, and the gravity centers X(u) calculated for them. From (4), or (6) we have (compare (8), (7))

$$(14) \qquad M\,X^a(u) = -\,u_b\,M^{ab}\,(X(P))$$

so that

$$(15) \qquad r^2 := X^a X_a = M^{-2}\,(M^{ab}u_b\,M_a{}^c u_c) \;.$$

Decompose u_a orthogonally with respect to P_a : $u_a = {}_{\shortparallel}u_a + {}_{\perp}u$

Because of (6) we have $M^{ab}P_b = 0$, and (8), (11) imply

$$(16) \qquad r^2 = M^{ab}\,{}_{\perp}u_b M_a{}^c\,{}_{\perp}u_c \;/\; P^d\,{}_{\shortparallel}u_d\,P^e\,{}_{\shortparallel}u_e$$

$$= M^{ab}\,\tilde{u}_b\,M_a{}^c\,\tilde{u}_c \;/\, |P^d P_d|$$

with

$$\tilde{u}_a := \frac{{}_{\perp}u_a}{{}_{\shortparallel}u_b} \quad , \text{ whence } \; 0 \leq \tilde{u}_a\,\tilde{u}^a \leq 1 \;.$$

But the value of the non-negative quadratic form $M^{ab}M_a{}^c\,\tilde{u}_b\tilde{u}_c$ on the unit sphere must be smaller than, or equal to its trace

$$(17) \qquad M^{ab}\,M_a{}^c\,\tilde{u}_b\,\tilde{u}_c \leq M^{ab}M_{ab}$$

which completes the proof of (13).

From now on we will restrict considerations to preferred center lines only, and drop the attribute "preferred". Center lines (in flat spacetime) are therefore <u>unique.</u> They are characterized by (compare (6)):

$$(18) \qquad \boxed{0 = M^{ab}(X(P))\,P_b \;.}$$

In flat spacetime, the direction of P_a agrees with the velocity of an observer m_a who measures <u>minimum</u> <u>energy</u> (mass):

(19) $M(m) \overset{!}{=} \min. \Rightarrow 0 = P^b \delta u_b \quad$ for all $\delta u_b \perp m_b \Leftrightarrow m_b \sim P_b$,

Observers with different velocities measure a bigger mass because of their relative motion. A center line can therefore likewise be characterized as a solution of (6) for the minimum energy observer m_a .

What happens when we drop the flatness assumption ? In order to arrive at covariant statements we first of all have to give reasonable definitions for integrals over tensor fields such as (7), (11). How does one generalize "hyperplanes", and "constant vector fields" to curved space-time? We shall see that one has different choices which are physically on equal footing.

For any world point x, and timelike direction u_a in x we define the (locally) geodesic hypersurface $\sum(x,u)$ as the union of all (spacelike) geodesics through x whose tangents are normal to u_a (in x). $\sum$ generalizes the term "hyperplane". For metrics of differentiability class C_∞ , $\sum$ will be locally C_∞ , and globally $C_{0(\infty)}$.

Integration of tensors along $\sum$ can be covariantly defined in either of the following two ways: 1) Take the tensor components with respect to a tetrad parallelly transported from x along the generating (radial) geodesics. 2) Take the tensor components with respect to Riemannian normal coordinates. (This latter possibility corresponds to Euclidean parallel transport of a tetrad in the tangent space at x followed by the exponential map). For both prescriptions one may have to assume that radial geodesics do not intersect, or at least add further instructions when they do. These prescriptions lead to tensors in x (for fixed u_a) for they are unique up to linear transformations of the tangent space in x under which the proper transformation law is evident.

We are thus in the position to define a covariant 4-momentum $P^a(x,u)$, and a 4-angular-momentum $M^{ab}(x,u)$ for each couple (x,u) of a world point x, and a timelike direction u_a in x, by using the definitions (11) and (7); (the coordinates y^a occurring in (7) are of course normal coordinates with origin x^a). And (18) suggests the following definition for a center line in curved spacetime

(20) $$\boxed{0 = M^{ab}(X,U) \quad U_b}$$

in which U_a stands for either (the direction of) $P_a(X,P)$, or the tangent direction t_a to the center line $X(x^o)$, or the minimum energy direction $m_a(X)$. These alternative suggestions have been made by DIXON, MADORE, and BEIGLBÖCK respectively, and lead in general to pairwise different center lines. We should mention that the possible deviations are negligible as long as the Schwarzschild radius of the matter distribution is small in comparison to its maximal radius, and that all three definitions coincide in flat spacetime, or if the system of bodies has three symmetry planes. The "best" choice among them will be that choice for which the equations of Motion (referred to the center line!) assume their simplest form. We shall restrict further discussions to DIXON's choice:

(21) $$\boxed{0 = M^{ab}(X,P) \quad P_b(X,P)} \; .$$

This equation needs some explanation: the center line $X(x^o)$ implicitly defined through (21) is the union of world points X such that for a suitable direction $\wedge$ P_a in X, the integral P_a defined in (11) is parallel to this direction, and is an eigenvector of M^{ab} calculated likewise for this direetion. There naturally arises the question of existence and uniqueness of such a line.

Let us first observe that $P^a(x,u)$ defined in (11) is no longer a constant vector: equation (12) involves ordinary differentiation whereas the conservation law (2) involves the covariant divergence. P^a thus slightly varies with x and u_a. This makes equation (21) a complicated one (to be solved by iteration). And a glance at equation (19) teaches that in general we have $P_a \neq m_a$.

For not too dense (finite) matter distributions, the center lines C defined by (21) have the following properties: 1) C's exist, 2) The C's are contained in the (suitably defined) convex hull of the world tube of the matter. 3) There exists at most a countable number of C's. 4) C's are timelike. 5) C is unique. These properties are listed in increasing order of strength of the assumptions (on the matter distribution) needed. Proofs were given by BEIGLBÖCK in his thesis; compare also his contribution to the International Conference on Relativistic Theories of Gravitation, Vol.II, London July 1965. Instead of giving here an independent presentation, we shall only sketch the essential facts needed in the proofs:

1), 2). BRAUER's fixedpoint theorem says that a continuous map of a Euclidean n-ball into itself has at least one fixedpoint. This theorem implies that a continuous vector field on an n-ball pointing inwards on its boundary must vanish in at least one inner point. As the vector field we choose

$$(22) \qquad V^a(x) \quad := \quad M^{ab}(x,P) \quad P_b(x,P) \qquad ;$$

(the existence, and uniqueness of P_b will be treated under 5)). $V^a(x)$ is continuous on every space section $\sum$, and points inwards on the boundary of every convex domain which contains the matter; whereby "convex" means "convex with respect to normal coordinate systems". MADORE has pointed out that it suffices to assume geodetic convexity: with any two matter world points a geodetically convex domain contains their joining geodetic arc. In this case one can likewise conclude that $V^a(x)$ points inwards on the boundary, because a geodesic through the origin of a normal system is a straight line. The space projection of $V^a(x)$ thus satisfies the assumptions listed above; it therefore vanishes in some inner point. But we have $V^a P_a = 0$ which implies that V^a is spacelike; so that V^a itself vanishes in that point yielding a solution of (21). The existence of a C is hereby established.

3), 4). Neighbouring solutions of equation (21) have timelike separation. This can be seen by varying equation (21): $M^{ab}(x,u)$ depends linearly on x^a whereas $P^a(x,u)$ is almost constant.

The uniqueness assertion 5) needs more careful estimates. Existence and uniqueness both follow from the fixedpoint theorem on contractive maps (so that 1) will be proven a second time): Given a compact Hausdorff space $S = \{x\}$, and a continuous map $(x,y) \rightarrow \rho(x,y) \geqslant 0$ of $S \times S$ into the nonnegative numbers, with

$\rho(x,y) = 0 \Rightarrow x = y$; let $x \to f(x)$, be a continuous contractive map: $\rho(f(x),f(y)) < \rho(x,y)$; then $f(x)$ has a unique fixedpoint. This theorem will be applied twice:

a) The future timelike, and null directions form a compact Hausdorff space S when angular distance is used as the metric ρ. With respect to this metric, the application $f: u_a \to P_a |P_b P^b|^{-1/2}$ given by (11) can be shown to be contractive for not too strong curvature. That P_a is always timelike can be guaranteed by assuming $T^{ab} u_b$ timelike for timelike u_a; which means that the matter has nonvanishing rest mass. In that case, therefore, the fixed point theorem establishes the existence of a unique solution

$$P_a = P_a(x,P) \tag{23}$$

for every worldpoint x.

b) We now apply the fixedpoint theorem to the space S of all timelike, or null world lines within the convex hull of the matter tube. A metric ρ on S can be defined as the maximal spacelike geodetic distance. A continuous map f is defined as follows: for given worldline C, construct the geodesic hypersurfaces $\Sigma(x,P)$ in all of its points with P_a taken from (23). For each Σ, solve the equation

$$0 = M^{ab}(x;P)\, P_b(x,P) \tag{24}$$

and obtain a new line $x' = x'(x)$. It remains to be shown that the map $f: \{x\} \to \{x'\}$ of C onto C' is contractive. This step is hard as one must compare integrals over different space sections. One has to use the curved space generalization of equation (12):

$$(25)\quad \int\limits_{\text{boundary}} T^{ab}\, \overset{*}{dx}_b = \int\limits_{\text{interior}} T^{ab}{}_{,b}\, \overset{*}{dx} = -2\int \Gamma^{(a}_{bc}\, T^{c)b}\, \overset{*}{dx}$$ [redacted]

$= small,$

and a formula relating the comparents of T^{ab} in normal systems with different origins: parallel transport along closed loops can be expressed as a 2-surface integral involving the curvature tensor. If the Riemann tensor components are everywhere small - as they really are for reasonable matter distributions in normal coordinates - then the contractive property can be verified, and the existence and uniqueness proof is finished.

We may <u>resume</u> the situation as follows: in curved spacetime, several covariant generalizations of the unique special relativistic center line are possible corresponding to different defining equations, and integration processes. For reasonably small spacetime curvature they all define unique worldlines which are almost identical. They give rise to different equations of motion. A preferred choice is perhaps given by DIXON's equation (21), and the simplicity of MADORE's (yet unpublished) equation of motion (for quadrupole particles) speaks in favour of normal coordinate integration.

CHAPTER III

§ 8. Problems of abstract algebra. The author's mathematical research in abstract algebra concerned chiefly three topics:

A) Formally complex algebras. This topic has been s died quite exhaustively in former years, included in my former contract work. The aim of this study was to give simplified proofs for those results of the theory of group representations which are mostly used in quantum mechanics and so on. This part of my study has been settled since a few years, and not considered in the two years of my last contract work.

B) Certain problems studied in the course of these two years were connected with mathematical investigations performed by this author, partly in common work with the famous late mathematician J. von Neumann and the Nobel prize winner E. Wigner, more than 30 years ago, concerning a new class of abstract algebras, which was later called by American mathematicians "Jordan algebras".

C) At the other hand I continued during these two years to study the mathematical theory of "noncommutative lattices", founded independently by myself and by the Japanese mathematician Matsushita; he was here at Hamburg for one year and we worked together about the topics B) and C).

My old work, carried through partially together with

J.v. Neumann and E. Wigner, caused many other mathematicians, especially in USA, to do further work about the socalled Jordan algebras. Numerous articles and several books treated questions about this field of research. The book of H. Braun and M. Koecher about "Jordan-Algebren", published 1966 (Springer-Verlag) gave a summary of the whole theory, and new valuable results (including contributions of the late famous mathematician E. Artin, who in his last years, living again here at Hamburg, worked chiefly about this topic). This book mentions more than 300 articles of many different mathematicians, treating Jordan algebras, or other mathematical problems related to them.

The defining relations of this class of algebras are

$$(1) \qquad a(ba^2) = (ab)a^2; \quad a(ba) = (ab)a .$$

These obviously are very special cases of the associative law which does not hold generally. Associative algebras therefore are special cases of Jordan algebras; and interesting ourselves now only for the commutative case (in which $a(ba) = (ab)a$ is trivially fulfilled) we can easily construct algebras fulfilling (1) from associative algebras. But our investigations 30 years ago showed also that there exists another possibility to construct irreducible Jordan algebras:

These have only the degree 3, and their construction is based upon a highly interesting special (nonassociative) algebra detected already by Cayley. Simplifying the matter a little, one can say, that nearly all examples of Jordan algebras are derivable from A) associative algebras, or B) the Cayley algebra.

The new interest given to this field by Artin's ideas (especially about "mutations" in Jordan algebras) and by the publication of the book of Braun and Koecher caused also new interest in the following direction: Are there mathematical possibilities to go to further still treatable and interesting generalisations?

It has been shown partly already by my old investigations that the "power-associative law"

$$a^n a^m = a^{n+m} \tag{2}$$

(with the definition $a^{n+1} = aa^n$), which is a consequence of (1), does not give an essentially more general type of algebras than (1) itself. Only a few very special cases fulfil (2) without fulfilling also (1).

But one can show (this was an old result of mine) that every Jordan algebra fulfils also

$$(ab^2)c - a(b^2c) = b\,\{(ab)c - a(bc)\} + \{(ab)c - a(bc)\}\,b \,. \tag{3}$$

The mathematician Petersson in his dissertation independently found this result (3) and saw, that (3) is not equivalent to (1), but weaker than (1): One can indeed construct examples of algebras fulfilling (3) without fulfilling (1). Professor Matsushita and I myself studied some questions belonging to this topic. But the results of Petersson show also that the classes of algebras with (3), and without (1), are very narrowly confined.

It remains an unsolved problem whether any interesting generalisation of the Jordan algebras - giving rise to a new great class of mathematically meaningful algebras exists. Long years ago I was quite convinced that such a generalisation must to be found and would be fundamentally meaningful also for physics. Today I am not sure what to believe about this problem. -

Concerning the mathematical theory of noncommutative lattices ("skew lattices") I performed investigations published in a series of articles; and I had now during Professor Matsushita's one-year-stay here at Hamburg opportunities to talk with him about several still open questions of this theory. The theory of half groups of idempotents is a part of this theory, and my latest endeavour about this field of research has been concerned with this theory of half groups of idempotents, about which I had also interesting discussions with mathematicians in USA. But since 1 to 2 years I postponed this endeavour because I became too absorbed by my work about Dirac's hypothesis and the theory of Earth expansion, discussed in Chapter I of this Final Report.

But I am glad to see that also this part of my pure=

ly mathematical activity lead to close connections with other current work about mathematical problems.[1]) As an example I mention a paper of Torn Saito [2]) who gained (from other considerations) examples of half groups of idempotents. For instance:

	s	e	t	v	f	u
s	s	s	s	s	s	s
e	s	e	t	t	t	t
t	t	t	t	t	t	t
v	v	v	v	v	v	v
f	v	v	v	v	f	u
u	u	u	u	u	u	u

and:

	e_2	e_1	t	v	f_1	f_2
e_2	e_2	e_2	e_2	e_2	e_2	e_2
e_1	e_2	e_1	t	t	t	t
t	t	t	t	t	t	t
v	v	v	v	v	v	v
f_1	v	v	v	v	f_1	f_2
f_2	f_2	f_2	f_2	f_2	f_2	f_2

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[1]) As I learned from Mrs. Professor Braun, the Jordan-algebras developed in the meantime to an useful tool of mathematical research in quite other directions, as especially concerning the automorphic functions of several variables.

[2]) Pacific J. of Mathematics 13, 263 (1963)

Many articles of USA mathematicians, published in the last years volumes of mathematical journals, deal with topies related to my mathematical study. But I cannot try to mention them here.