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Discussions with Jens*

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I first met Jens in 1949 in Birmingham. The winter of 1949-50 was a high point in my career, because it was during that time that I played the one and only game of chess with Jens that I did not lose--it was a draw. Since 1949 I have visited Jens many times, lost many games of chess, and had many discussions on various aspects of physics. I would like to recall some of these discussions in this talk.

I realize that those visits to Aarhus and, in particular, the discussions with Jens, have influenced substantially many aspects of my work, but I never spent the ten consecutive years with Jens, required to produce a joint publication. A short note that came closest to being published was a comment on "The Energy of Zero-Point Vibrations of a Liquid Surface", bearing the date of August 4, 1970 (Fig. 1). It has to do with an interesting consequence of quantizing the vibrations of an idealized incompressible liquid drop with surface tension γ.

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The liquid drop model of nuclei was introduced in the thirties and its quantization was discussed in the forties, in a paper by Fierz. If you take such an idealized drop, solve for the normal modes of the vibrating surface (which are proportional to spherical harmonics $Y_{\ell,m}(\theta,\phi)$), and quantize the small-amplitude vibrations, you find that you are dealing with an <u>infinite</u> number of harmonic oscillators (since the number of normal modes is infinite), just as in quantum field theory. Each normal mode has a finite zero-point energy $\hbar\omega_n/2$, say. If you try to sum these to infinity, you get an infinite zero-point energy, just as in field theory. But if you introduce a cut-off in your sum--say a kind of Debye cut-off at the point where the spacing between the nodes of the normal modes is about equal to the spacing between the particles of which the drop is made--then you get a finite result. The remarkable feature of the result is that the sum over the zero-point energies turns out to be proportional to the area of the drop:

$$E_{ZP} = \Sigma \frac{1}{2} \hbar\omega_n \propto 4\pi R^2 = k(4\pi R^2\gamma) \qquad \text{say,}$$

$$\therefore (E_{ZP} + 4\pi R^2\gamma) = (1+k)(4\pi R^2\gamma) = 4\pi R^2\gamma' \quad ,$$

where the constant of proportionality k turns out to be given by

$$k = \frac{\pi^2}{14}\left(\frac{9\pi}{2}\right)^{1/3}\left(\frac{\hbar^2/mr_o^2}{4\pi r_o^2\gamma}\right)^{1/2} \alpha^{-7/2} \quad ,$$

where $\qquad \alpha = \dfrac{\text{node-spacing for cut-off vibration}}{(\text{volume per particle})^{1/3}}$

Here m = mass of particles constituting the drop,

R = radius of drop $\equiv r_o A^{1/3}$

A = number of particles.

One can do the same calculation for the small quantized vibrations of a plane liquid surface, obtaining the same result and demonstrating directly that the zero-point energies give a contribution that is proportional to the surface area and therefore indistinguishable in its effects from the originally assumed surface energy γ.

The numerical values one gets for k are interesting. For nuclear matter

$$k = \left(\frac{E_{ZP}}{4\pi R^2\gamma}\right) \approx 2 \text{ if } \alpha = 1.$$

This says that the zero-point energy "correction" could be of the same order of magnitude as the original surface energy! It seems that an essential renormalization of the surface-energy coefficient γ is called for. But, since the result depends essentially on the ill-defined cut-off value of α, the situation is not clear cut. The last paragraph in our manuscript says: "It is not the purpose of this note to attempt a reformulation of the problem of the surface energy of nuclear matter, necessitated by the possibly large effects of collective zero-point vibrations. A consistent formulation of the problem appears at first sight to involve subtle questions concerning the relation of single-particle and collective degrees of freedom."

As far as I am aware, that is still where the problem stands today.

There are two footnotes to this story. When I was spending a year in Copenhagen in 1977-78, I learned from Henning Esbensen, Aage Winther, and Ricardo Broglia that, in the theory of nucleus-nucleus collisions that they and their collaborators were working on, the zero-point vibrations of the approaching nuclei might play an important role. This caused a renewed interest in understanding the surface properties of a quantized liquid drop. The second footnote is an exotic suggestion due to Sakharov, concerning the nature of gravity. I came across it in the book on gravity by Wheeler, Misner and Thorne, also during my 1977-78 stay in Copenhagen. Sakharov starts with a calculation of the zero-point energy of a quantized field (representing some fundamental particles) carried out in a curved space-time manifold. The total zero-point energy, again summed to a definite cut-off wavelength, turns out to depend on the space-time curvature (just as the total zero-point energy of a liquid surface would be found to depend on the curvature of the surface). The dependence has apparently precisely the form one finds for the curvature dependence of the Lagrangean in a Lagrangean formulation of General Relativity. Sakharov now makes the astonishing suggestion that the physical reason for the dependence of the energy of space-time on curvature, implied by General Relativity, might, in fact, be due to the response to curvature of the zero-point energies of the various quantized fields of particle physics. (Wheeler calls it the elasticity of space-time.) It looks like a wild suggestion, and I do not believe it has led, so far, to more concrete results, but I was struck by Sakharov's utter originality in attempting to make gravity out of the annoying infinite zero-point energies that, up to then, everyone else had been trying to get rid of as quickly as possible. And the idea that the zero-point energies summed to a fixed cut-off would depend on the curvature of the manifold in question, was quite familiar from our discussions with Jens on the quantized liquid-drop problem.

My periodic attempts to understand Special and General Relativity were often stimulated by my contacts with Jens. From the early days of the Institute of Physics in Aarhus, in 1956-7, I remember the great care and originality with which Pablo Kristensen, Olaf Pedersen, and Jens worked out their lectures on quantum mechanics and relativity. As regards special relativity I had felt uncomfortable, since my college days, about bringing in light and light signals into the exposition of relativity. Surely, once allowances for the historical development have been made, relativity is not specifically related to electromagnetic radiation. Purely mechanical experiments with pendulums or colliding balls could, in principle, have led to the discovery of special relativity. But does one need to introduce any mechanical or electromagnetic experiments at all to discover special relativity? What is the most primitive thought experiment that goes to the heart of special relativity without introducing confusing irrelevancies?

During my stay in Aarhus in 1969-70 I came up with the following suggestion. I take the point of view of Minkowski, according to which the essence of special relativity is that we live in a space-time where distances and time intervals are related by

$$dx^2 + dy^2 + dz^2 - k^2dt^2 = 0 \quad ,$$

where k is a constant.

How could one have discovered this Minkowski geometry?

Well, how could one have discovered Euclidean geometry on a flat piece of paper?

I drew two dots on a piece of paper, got myself a rectangular grid of lines (a piece of transparent millimeter paper or a transparent chess board), and measured the distances $(x_1y_1),(x_2y_2)$ from the two points to the edges of the grid (Fig. 2). Then I shifted and rotated my grid and measured again the distances $(x_1'y_1'),(x_2'y_2')$. Here is what I actually found in a set of four runs (the numbers are in millimeters):

Run	Point 1 x_1	Point 1 y_1	Point 2 x_2	Point 2 y_2	$\sqrt{(x_1-x_2)^2+(y_1-y_2)^2}$
1	32.5	40.5	68.5	52.6	38.0
2	27.0	37.2	65.2	34.5	38.3
3	35.2	13.0	39.5	51.4	38.6
4	26.0	56.9	44.7	23.5	38.3

I don't know what inspired me to form the weird combination of numbers shown in the last column, but it must have been Euclid's spirit. Anyway, I did not get beyond the fourth run, because a mathematician friend of mine was watching me and at that point he exclaimed: "By God, your measurements obey the rules of Euclidean geometry! The points on your sheet of paper seem to make up a Euclidean space, to within an accuracy of a percent or so. Here is a bunch of useful results I can derive for you at once. For example, the <u>contraction</u> of a ruler viewed at an angle."

Now what I did to discover special relativity was, first, to make myself a space-time grid--an "event meter". I did this by taking my chess-board grid and gluing a set of flat stop watches on it. Just ordinary stop watches, set to read more or less the same time and so constructed that if I tapped one, it would stop. Thus, reading off the position x_1y_1 of the stopped watch on the chess-board grid and the time t_1 on the stopped watch, I would have the coordinates $(x_1y_1t_1)$ associated with the event consisting of tapping the watch. If I first tapped one point and then another, I would get $(x_1y_1t_1)$ and $(x_2y_2t_2)$ of two events. Now I was ready. I took two of the chess-board "event meters", put them on top of one another (at an arbitrary angle), and set them in relative motion (in an arbitrary direction). Then I tapped one point and then another, getting a set of numbers $(x_1y_1t_1)$, $(x_2y_2t_2)$ for one board and $(x_1'y_1't_1')$, $(x_2'y_2't_2')$ for the other. (The boards are supposed to be thin enough and the tap strong enough so that a tap stops two watches, one on each board.) I could even take several boards, set them in relative motion, and get several event readings for each tap.

Board	Tap 1	Tap 2	$\Delta x^2+\Delta y^2+\Delta t^2$	$\Delta x^2+\Delta y^2-\Delta t^2$	$\Delta x^2+\Delta y^2-k^2\Delta t^2$
1	$x_1y_1t_1$	$x_2y_2t_2$	NOT CONSTANT	NOT CONSTANT	NOT CONSTANT EXCEPT WHEN $k=3\text{x}10^{10}$cm/sec
2	$x_1'y_1't_1'$	$x_2'y_2't_2'$			
3	$x_1''\ y_1''\ t_1''$	$x_2''\ y_2''\ t_2''$			
4	$x_1'''y_1'''t_1'''$	$x_2'''y_2'''t_2'''$			

I started playing around with the numbers formed by taking $\Delta x = x_2-x_1$, $\Delta y = y_2-y_1$, $\Delta t = t_2-t_1$, and I guess it was Minkowski's spirit that inspired me to form the weird combinations shown in the last column in the table, where Δt was multiplied by a number k. Even so, I could not make the resulting sum of squares into a constant, except when I took the special magic conversion factor $k = 3\text{x}10^{10}$cm/sec. Then it worked. My mathematician friend, who was still watching, got really excited and shouted: "By all the Gods, your distance and time measurements form a Minkowski manifold with signature (1,1,-1). I bet that if you repeat your experiment with three space dimensions, you will find a 4-dimensional Minkowski manifold with signature (1,1,1,-1). Here is a bunch of useful results I can derive for you at once--for example, the contraction of a ruler and the slowing of a watch when they are set in motion. Also, you have just discovered a universal characteristic constant of space-time, the magic conversion factor $k=3x10^{10}$cm/sec. Hurry and send in this result to Gerry Brown in Physics Letters!"

Note that light (electromagnetic radiation), or the sending of signals back and forth, was never mentioned. Everything could have been done by blind people, reading watches and distances by touch and unaware of even the existence of light. Later, as a result of quite separate experiments, one would find that photons and neutrinos travel at the speed of the characteristic space-time number k, at least as far as one could tell. But it seems clear to me that, in the exposition of the essence of special relativity, there is no more need to talk about light signals than there is about neutrino signals, sound signals, or peanut-butter sandwiches.

What my pedagogical thought experiment is supposed to stress is that the essence of special relativity is a certain property of space and time, space and time meaning simply a mathematical structure into which it proves useful to embed our measurements obtained with everyday rulers and clocks. One finds by conceptually simple measurements that we appear to be living in a Minkowski manifold where space and time are interrelated in a special way and that there is a conversion factor $k=3x10^{10}$cm/sec characterizing this interrelation. This is just simple geometry--kinematics if you like.

If one <u>now</u> goes on to quite a new level, namely the setting up of the laws of mechanics and field theories in such a way that these laws should not depend on location and orientation in the Minkowski space-time, then one finds a lot of fascinating <u>dynamical</u> consequences. For example, the increase of mass with velocity and the fact that accelerated electric charges would emit radiation that travels at a speed equal to the characteristic space-time number k. But to me, as to Minkowski, the essence of special relativity is a certain simple geometrical property of space-time, which one discovers using rulers and watches, and which antecedes any dynamical considerations.

I don't think Jens or Jørgen Kalckar look at this question in quite the same way, but, since we do not, of course, differ on how one calculates anything in practice, it is mostly a question of emphasis and logical precedence.

From the question of how to look at the nature of flat Minkowski space-time, we went on, more than once, into discussions of how to look at the curvature of space-time. For example, we went over the usual arguments which say that a curved space could be made to look flat, and flat space could be made to look curved, if your measuring rods expanded or contracted in just the right way as you moved around, making your measurements. (The presence of a suitable temperature field that affects the rods is sometimes mentioned in this context.) Even though these are logical possibilities, they strike me as red herrings. After all, what would I do if I were a two-dimensional being and wanted to decide if a surface I was livng in was flat or curved? I could, for example, lay off a unit radius, draw a circle, and see if the circumference was 2π or not. O.K., so here on this blackboard is a unit circle laid off with the span of my hand, and its circumference is about 6, so the blackboard is pretty flat, within some errors. Now here is a globe and a unit circle drawn with the span of my hand (the circle happens to be close to the equator) and the circumference is 4, so the globe is curved.

Now if you are a red herring, you will object that, as I was going around the equator of the globe, my hand expanded because of a temperature difference or some other trick, and that is why I only counted four spans. It is your privilege as a red herring to investigate such hypotheses if you think it is worthwhile (especially if you are a paranoid red herring and believe in conspiracies). But in the case of the globe it is obvious that it is the person who just accepts that the globe is curved rather than the person who believes in conspiracies, who ends up with a more sensible view of the world.

Now, when we are faced with the question whether the four-dimensional space-time manifold that we discovered a little while ago is flat or curved, there is no logical difference, only the curvatures tend to be smallish and harder to get at experimentally. (Actually, they are not at all small in neutron stars or black holes.) So, as experimentalists, we have to be properly careful and make sure we are working well outside experimental errors. But if evidence for curvature well outside errors accumulates in conceptually simple experiments, then trying to revive conspiracy theories rather than accepting at face value what the measurements are trying to tell us may not be a useful activity.

What I am trying to stress once again is that both the Minkowski nature of the space-time manifold and the flatness or curvature of this manifold are primitive questions of space and time not involving mechanics, electrodynamics, or gravitation. In principle, that is conceptually, they can be settled by means of everyday rulers and everyday clocks.

You may begin to guess that, in my discussions with Jens, I have often both admired and fought against his subtlety and inclination to see everything related to everything else. Even though seeing the proper relations between everything around us is also my ideal, I am nevertheless attracted to the opposite extreme of consciously oversimplifying things, in order to see clearly at least the next step ahead. This difference in our methods may well be related to the fact that in chess I can only see one or two moves ahead and Jens can see a dozen.

Some years ago, in trying to neatly order in my mind what it is one does in a lot of everyday physics, I evolved the following little oversimplified catechism. After having familiarized yourself with the problem at hand, go through these steps:

1. Decide what degrees of freedom are likely to be the most relevant ones.
2. Write down a sensible Hamiltonian in terms of these degrees of freedom.
3. Crank the canonical handle. (This means solve the equations of motion--the Schrödinger equation--using whatever techniques seem appropriate, classical or semi-classical methods, tricks of statistical mechanics, etc.).
4. Compare with experiment and recycle, using suitable modifications in steps 1 and 2.

The key quantity in this scheme is the Hamiltonian. I once included this little bit of catechism in a talk and showed a picture of Hamilton (Fig. 3). Jens seemed to like the picture well enough to have it hang in his office for some years, but he never liked my scheme. He would say, in effect, there is no irreversibility in Hamiltonian mechanics, no arrow of time, so there is something essential missing. So we had many discussions on this question. In what sense there is or is not irreversibility, both in classical and quantum mechanics.

As regards classical mechanics, I have convinced myself that I see no problem and that the question "how do you get irreversibility from time-reversible equations of motion" is another red herring.

This problem was brought to a focus during my stay in Copenhagen in 1977, during discussions with Jørgen Kalckar and Jens Bang. We asked ourselves the following question: Suppose you are studying a classical billiard-ball gas in a box. Someone takes two snapshots of the system at times t_A and t_B, but you don't know whether t_A was before or after t_B. What you do know is that in snapshot A all the molecules are in the left half of the box, in snapshot B they are spread more or less evenly. How, using only classical mechanics, would you argue that t_B is most likely later than t_A?

Feeling somehow that the answer to this question had more to do with common sense than Hamiltonian mechanics, I tried to answer it in a note entitled "What would a Sensible Person conclude from playing around with reversible classical solutions of the problem of 100 molecules in a box?" The last paragraph in this note makes the following claim:

"It seems to me from all this that the Sensible Person, using reversible classical mechanics, arrives at sensible conclusions regarding so-called irreversible behaviour of large systems observed in the real world. As far as I have understood what 'arrow of time' is supposed to mean, the Sensible Person has been able to give a sensible account of that aspect of our experience."

I sent this note to Jens and, in reply, he wrote a letter disagreeing with my contention that there is no problem, within the framework of the reversible equations of mechanics, of reconciling reversibility with irreversibility. He then explains some of his own views and ends with a paragraph that is, perhaps, typical of his style:

"What I have tried to illustrate here, is that the reversible equations are, by themselves, singularities, just like black holes are singularities and electromagnetic self-energy of an electron is a singularity: They contain an arbitrariness of choice, and the arbitrariness can become arbitrarily arbitrary."

I was at that time, in 1977, particularly interested in the question of irreversibility because I was working on a macroscopic dynamics of nuclear shape evolutions, where a new type of viscosity was supposed to operate. Because of this viscosity the resulting equation of motion for the shape evolution was irreversible, even though the underlying model was that of a gas of individual nucleons bouncing about reversibly in a deforming nuclear potential well. One had to be prepared for the question: "How do you get irreversibility from reversible equations of motion?"

Now the situation here is no different, in principle, from the case of the ordinary viscosity of a gas, whose value can be derived from the kinetic theory of gases, based on reversible equations of motion. [The expression for the viscosity coefficient η of a gas of density ρ, consisting of molecules with mean free paths λ and mean speeds $\overline{v}$ is $\eta = \frac{1}{3}\rho\overline{v}\lambda$.] This is such a down to earth, practical calculation, tested experimentally, that surely there is no mystery here and it is just a question of common sense to clear up this point.

Well, what happens, of course, is that at some stage in the derivation of the formula for η, one injects a "Hypothesis of Randomization", and that is where the irreversibility comes in. But then the critic asks, what do you mean by randomization in the context of reversible classical equations of motion?

The way to answer this question is to look very closely at the critic who asked it. If you stare at him steadily for a while, you begin to suspect that there is something fishy about him; in fact you suddenly realize that, once again, he is a red herring. This at least is my contention: that, in the context of classical mechanics, the question of irreversibility and randomization is a red herring, an uninteresting pursuit, a little like the conspiracy theory of the curvature of space-time. In fact, that it is not a question belonging to mechanics at all.

To illustrate what I mean, take two new packs of cards and shuffle them. Why did the cards get randomized? You are welcome to ponder this profound question, but I hope you will agree that it is not a question within the realm of Hamiltonian mechanics: you do not try to delve into the equations of motion of the hands doing the shuffling and then make coarse-grained averges in phase space. Cards get shuffled because one card gets in between others and the order gets destroyed. The same thing happens when streaming molecular layers in a gas get tangled up to produce viscosity. The destruction of order has nothing to do with Hamiltonian mechanics.

As regards the question of irreversibility in the real--that means quantal--world, I am much less dogmatic. Without having clarified the situation even to my own satisfaction, I tend to feel that there may, indeed, be a profound relation between irreversibility and quantum mechanics, as stressed on many occasions by Niels Bohr. It seems that an essential part of the consistency of the quantal description is related to the truth that you cannot have your cake and eat it, too. You are supposed to hand in to God a complete specification of the experiment you intend to perform, and God tells Schrödinger to tell you what to expect. After having finished the experiment--that is eaten your cake--you are not supposed to say, "Please, God, just pretend I hadn't eaten that cake--can I have a different one? In other words, just pretend I hadn't done the experiment I did, but a different one, what would I have found?"

If the general scheme of things is indeed the sensible one--that you cannot have your cake and eat it--then you need something like irreversibility to play the role of the eating of the cake, which is (usually) a one-way process.

When you stop to think of it, some scheme like that, where you are only allowed to ask well-defined questions, one at a time, and cannot backtrack and start over, seems quite sensible and somehow in harmony with our primitive experience of a unidirectional flow of time. One could even argue that the opposite, the classical fiction that we have the infinite luxury of running the movie representing the evolution of nature back and forth as we please, is an aberration, caused by being overly impressed, since Newton's days, by the precise regularities of planetary motions and other simple and idealized systems. But why this classical aberration should have met its doom in the precise way associated with the presence of Planck's constant h, whose dimensions are those of action (i.e. angular momentum) I do not really understand. And even less how to fit the whole scheme in the Minkowski space-time manifold. It is one of my ambitions to understand this a little better by the time of Jens' 120th anniversary.

At the time when, despite Jens' disapproval, I was trying to arrange my everyday calculations in neat little Hamiltonian schemes, I was wondering about the following historical lesson. In nuclear physics there are several examples where people would be doing calculations that, taken at face value, really implied a definite Hamiltonian, i.e., a definite model, but because the model appeared overly idealized or happened to be in disrepute, they would not explore the model thoroughly and so would miss some striking discovery that was staring them in the face. A good example is the model for describing nuclear binding energies by representing the nucleus as a uniformly charged liquid drop with surface tension. I imagine that it must have been partly because, in 1935, people did not take the model at its face value, but considered it perhaps as some lucky parametrization, that they did not take the obvious step of checking up on the stability of a charged drop against fission. The criterion for the stability of a <u>conducting</u> drop against fission had been known since Lord Rayleigh's day, namely that the electrostatic energy should be less than twice the surface energy. As it turns out, the criterion for the stability of a <u>uniformly charged</u> (nuclear) drop is exactly the same. Niels Bohr must, of course, have been thoroughly familiar with Lord Rayleigh's papers on liquid jets and liquid drops, and one can understand his feelings in

1939 when it was Frisch who had to point out to him the probable connection between the loss of stability of a sufficiently charged drop and the newly identified phenomenon of nuclear fission. If only someone in 1935 had taken the liquid drop model of nuclear binding energies seriously and posed the question: what are the possible equilibrium configurations, defined by

$$\delta(\text{Binding Energy}) = 0 \quad , \tag{1}$$

nuclear fission would have been predicted theoretically several years earlier.

But even now, several decades later, is the story really over? Do we understand all the aspects concerning the equilibrium and stability of a charged nuclear drop that follow from solving eq. (1)?

Stimulated by such thoughts I spent some time when visiting Aarhus in 1970-71 trying to answer that question.

The simplest liquid-drop binding energy formula for a nucleus with A nucleons (Z proton and N neutrons) looks like this:

$$BE = -a_1 A + a_4 A I^2 + a_2 A^{2/3} f(\text{shape}) + c \frac{Z^2}{A^{1/3}} g(\text{shape}). \tag{2}$$

It consists of a volume term (modified by a term quadratic in the relative neutron excess, I, defined as (N-Z)/A), a surface energy and an electrostatic energy. The function f(shape) gives the dependence of the surface energy on shape (it is the surface area of the drop in units of the area of a sphere), and g(shape) gives the dependence on shape of the electrostatic energy. The coefficients a_1, a_4, a_2, c are constants known empirically from fits to nuclear masses. What happens when we take an idealized nucleus, represented by eq. (2), and increase the atomic number Z, while adjusting A to stay on the valley of stability, given by

$$\left.\frac{\partial(BE)}{\partial I}\right|_A = 0 \quad ? \tag{3}$$

As I mentioned before, the nucleus loses stability against fission when the Rayleigh-Bohr-Wheeler fissility parameter x, defined by

$$x = \frac{\text{Electrostatic Energy of Sphere}}{2(\text{Surface Energy of Sphere})} \tag{4}$$

exceeds 1. This corresponds to a superheavy nucleus for which one finds Z = 141, A = 395 when using some rather out of date values for the parameters a_4, a_2, c). Most people lose interest at this stage, but if you persist in stuffing more protons on the nucleus, you are rewarded with the discovery that at x = 2.016 (Z = 426, A = 1794) two brand new solutions of eq. (1) come into existence: they are both in the shape of thick-walled bubbles, one unstable, the other stable against radial expansions. What has happened is that the electric force can now cause the drop to cavitate. You pull the nucleus radially over a barrier and then it settles down to an equilibrium hollow shell, with a ratio of inner to outer radius of about 1:2. As you now keep increasing Z the bubble gets blown up like a balloon, the relative wall thickness decreasing. Then, somewhat unexpectedly, the trend gets reversed and the relative wall thickness starts increasing with increasing Z. At another critical value (Z = 2199, A = 47867), again characterized by a fissility parameter x = 2.016, the hollow-shell solutions of eq. (1) disappear and a solid nucleus is the only (radially symmetric) solution. If you keep going up in Z you now find that at Z = 5608, A = 627587 the fissility x becomes 1 and beyond that the nucleus would regain stability against fission--assuming, that is, that eq. (2) was valid. However, you note that the beta-stable nucleus is now almost pure neutron matter (that's why it is stable against fission) and neutron matter is believed not to be stable against the emission of neutrons (neutron drip). So if one wanted a really stable system, one would have to increase Z still further, until gravity stabilized the system as a neutron star.

However, another interesting thing happens much earlier, when Z is of the order of

$$Z \sim \frac{5}{4}\sqrt{\frac{5\pi}{6}}\left(\frac{\hbar c}{e^2}\right)^{3/2} \approx 3200 \quad .$$

Around this value of Z, the bulk of the atomic electrons surrounding the nucleus become relativistic and, as a result, collapse onto the nucleus. The electrons now play an essential part in the binding-energy and stability considerations. Again one may try to follow solid or hollow configurations. One particularly fascinating possibility that I looked at with Jens' help was a large, thin-walled double bubble: a bubble of nuclear matter, whose positive electric charge is neutralized by a somewhat thicker bubble of relativistic electrons.

As I recall, we never finished the discussion of the stability of such systems and I believe that the general question of surveying the various stable or unstable configurations of Z protons and N neutrons (and Z electrons) is still very much open. This is becoming somewhat relevant today since, apart from neutron stars, the theories of the collapse of supernovae have recently renewed interest in huge, neutron-rich nuclei and in unusual topologies such as bubbles or foam-like arrangements of nuclear matter.

As an additional warning that we should not too readily assume that there are no interesting solutions of the equilibrium problem posed by eq. (1), we have direct empirical information on the existence of some exotic configurations of metastable equilibrium. For example, if you take a beta-stable system with $Z \approx 1.7\mathrm{x}10^{51}$ then we know for a fact that one solution of $\delta(BE) = 0$ will be in the form of a very asymmetric configuration where one piece is spherical, with a diameter of about $12.8\mathrm{x}10^{8}$ cm, and the smaller one, tangent to the first, has a ratio of axes of about 6:1 and a major axis about 1.8x10 cm. (See

Fig. 4). There may be more than one small piece tangent to the sphere, but the one I have in mind is really amazing and no one would have guessed it as a possible solution of the equation $\delta(BE) = 0$. Even though not absolutely stable, it has an empirically determined lifetime greater than 1.89×10^9 sec, and we are here to celebrate that happy event. Figure 5 shows a detailed view. Happy birthday, Jens!

Figure Captions

Fig. 1. Title page of a vintage 1970 manuscript, maturing undisturbed in the darkness of an oak desk drawer.

Fig. 2. The first step in discovering Euclidean geometry on a flat sheet of paper is the construction of a separation meter, a rectangular grid of lines, to measure the locations x_1y_1, x_2y_2 of two points.

Fig. 3. A picture of Hamilton.

Fig. 4. A beta-stable system with a sufficiently large number of protons and electrons can assume unexpected configurations of metastable equilibrium.

Fig. 5. A detailed view of the system in Fig. 4.

Fig. 6. A memento of our discussions. The chessboards with stop watches are the event meters used to discover Special Relativity without bringing in light or light signals. (If, when teaching relativity, the urge to send signals becomes nevertheless overwhelming, a brass automobile horn is provided at the bottom, so that the signals will at least be sound and not light signals.) The globe (showing Denmark and some neighbouring countries) is for practicing one's curvature-measuring skills. The playing cards are for putting in randomization by hand.

Figure 1

THE ENERGY OF ZERO-POINT VIBRATIONS OF A LIQUID SURFACE*

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ABSTRACT

The object of this note is to point out that the zero-point vibrations of the surface of a liquid may contribute a total zero-point energy which, like the surface energy, is proportional to the surface area. In the case of a liquid with the density and binding properties of water the total zero-point energy of the surface vibrations, (summed up to frequencies at which the wavelength is comparable with the spacing of the water molecules) is of the order of magntidue of a fraction of a percent to a few percent of the observed surface energy. In the case of a liquid with the density and binding properties of nuclear matter the effect is about 70 times larger, which means that the zero-point energy of the surface vibrations may be of the order of magnitude of the observed surface energy itself.

A quantitative evaluation is not attempted, but the order of magnitude of such a quantum-mechanical effect, without analogy in the

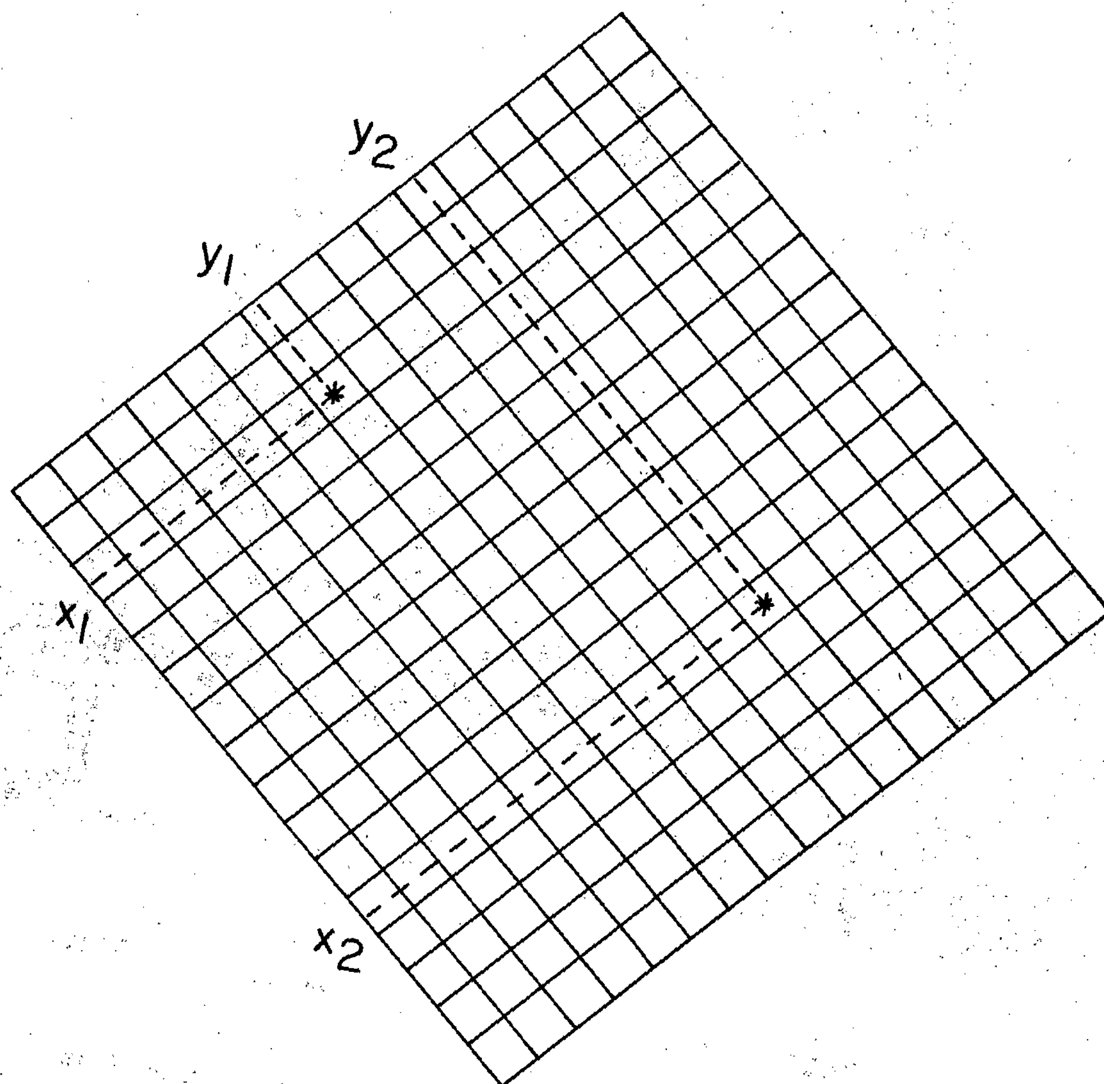
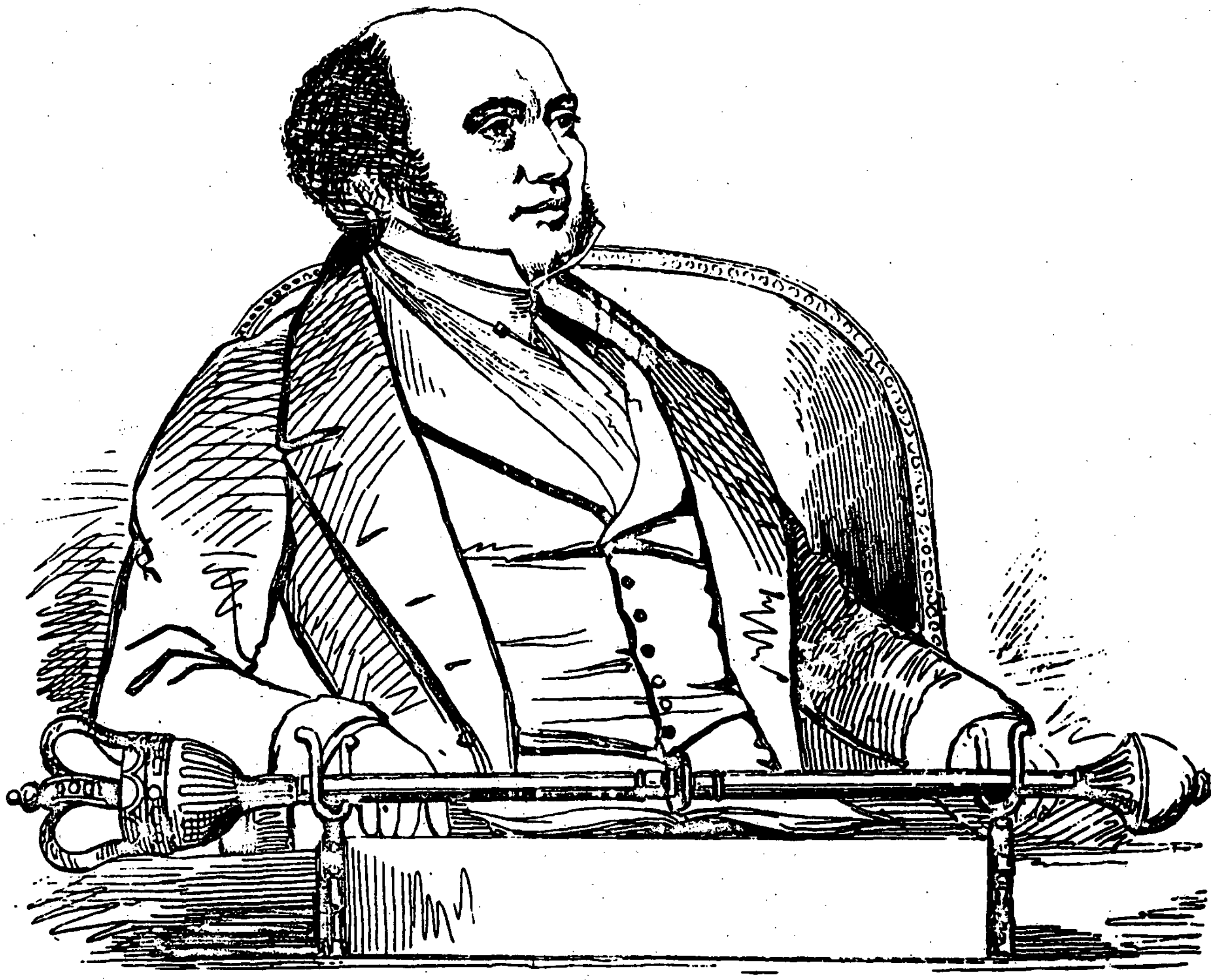


Figure 2

XBL 824-413



Portrait of Sir William Rowan Hamilton.

Note the mace in front, intended for the chastisment of scientists who do not formulate their models in terms of a Hamiltonian function.

Figure 3

XBL 824-9235

A BETA-STABLE SOLUTION OF $\delta(BE)=0$ FOR $Z \approx 2\times 10^{51}$

LIFETIME $> 1.89\times 10^{9}$ sec

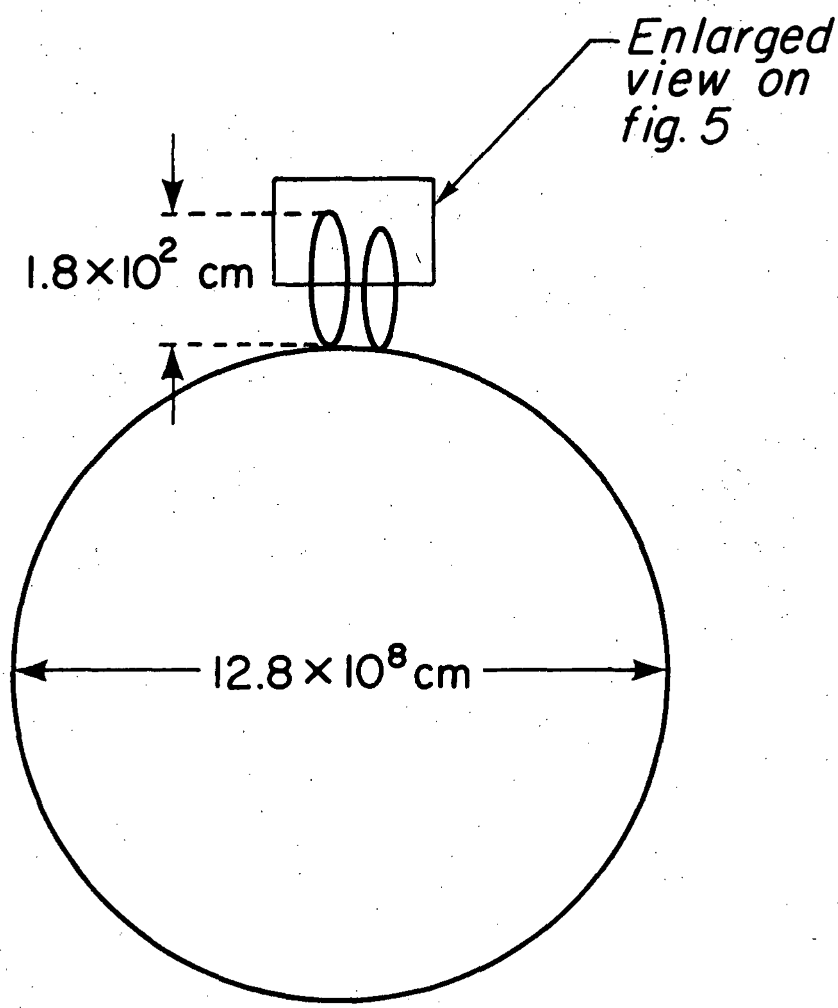


Figure 4

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Figure 5

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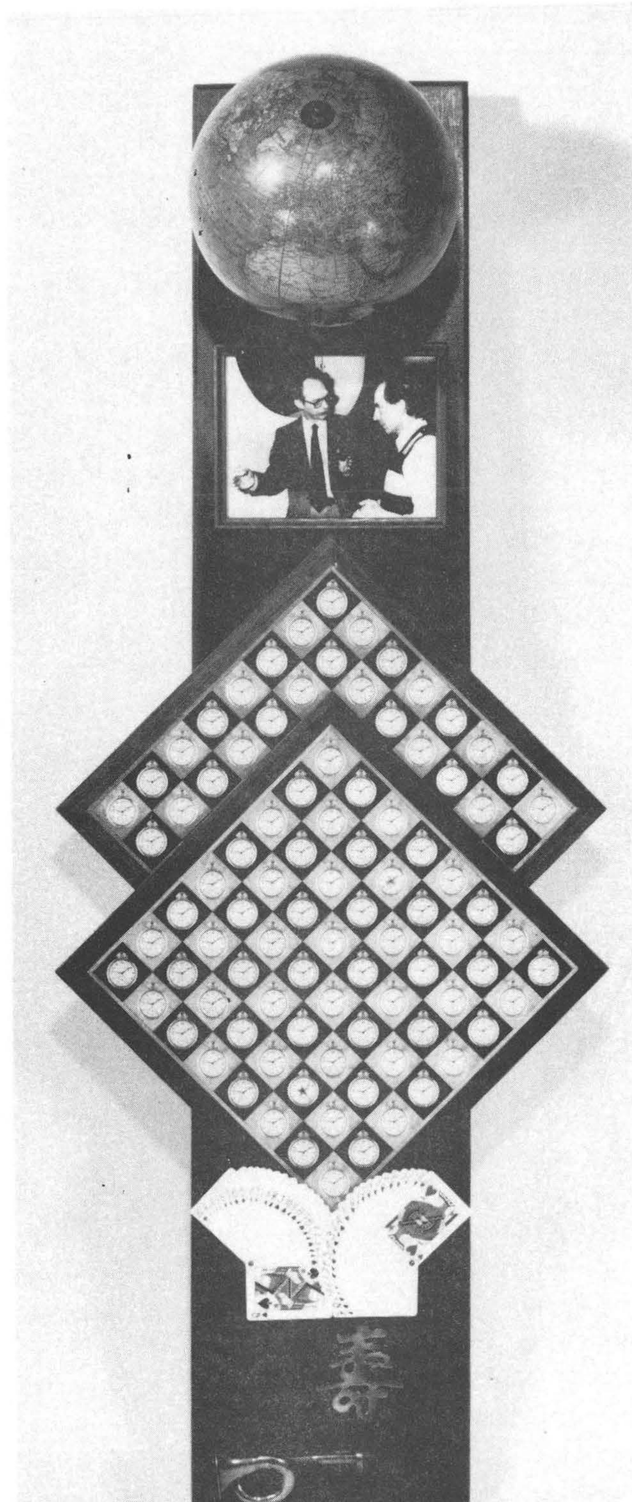

Figure 6 CBB 822-1578

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