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A MICROSCOPIC VIEW OF NUCLEAR COLLECTIVE PROPERTIES

Michel BARANGER - M.I.T.

Résumé - Après avoir discuté les notions de forme, de déformation et de mollesse nucléaire, les diverses théories microscopiques du mouvement collectif sont passées en revue et le problème de leur interdépendance est soulevé. L'approximation adiabatique du champ Hartree-Fock dépendant du temps est traitée plus en détail.

Abstract - After discussing the concepts of nuclear shape, deformation and softness, the various microscopic theories of collective motion are reviewed and the problem of their relationship is raised. The adiabatic time-dependent Hartree-Fock approximation is treated in more detail.

We talk about collective behavior if many nucleons are doing the same thing. Classically, this means that there exists a single-particle quantity

$$U = \sum_{i=1}^A u_i$$

which has a large average value. Quantally, the same quantity has large matrix elements between stationary states. We can distinguish static collective properties and dynamic collective properties, or collective motion. In the static case U is large and independent of t . The quantal translation of this is that U has a large average value, or a large diagonal matrix element $\langle \psi | U | \psi \rangle$, in a stationary state ψ . For collective motion, on the other hand, $U(t)$ is large and changes with time. The quantal translation is that the diagonal matrix element $\langle \psi(t) | U | \psi(t) \rangle$ for some non-stationary states $\psi(t)$, is large. But we are more used to dealing with stationary states only; then we can use the equivalent definition that the off-diagonal matrix element $\langle \psi_i | U | \psi_j \rangle$ is large between certain pairs of stationary states.

As an example of static collective property, we have the nuclear shape. Classically, you would define the shape of a nucleus at any instant of time by taking a snapshot of it. This would yield a collection of dots such as those of Figure 1, one dot of each nucleon. The shape is then defined in terms

of the distribution of dots in the picture. For instance, we might use the magnitude of the quadrupole moment as a shape parameter. To this end, let us denote by $q_\mu(i)$ the five components of the quadrupole moment for nucleon i , i.e.

$$q_\mu(i) = r_i^2 Y_{2\mu}(\theta_i, \varphi_i)$$

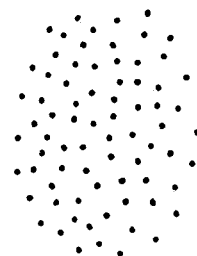


Fig.1

The total quadrupole moment is $Q_\mu = \sum_{i=1}^A q_\mu(i)$,

and its magnitude is $|Q|^2 = \sum_{\mu} |Q_\mu|^2$. In the pic-

ture drawn in Figure 1, this quantity is statistically different from zero, and therefore this particular classical nucleus may be said to possess a certain quadrupole deformation.

The quantal equivalent of this definition of the shape is as follows. Now you have a stationary state wave function $\psi(x_1, x_2, \dots, x_A)$ and you want to know its shape. Once again you take a snapshot, i.e. you make a simultaneous measurement of the coordinates of all the nucleons. This may be very hard experimentally, but it does not violate any law of quantum mechanics. You repeat the experiment many times on identical nuclei in the identical state ψ . The result is a distribution of patterns like that of Figure 1. The probability associated with each pattern is $|\psi(x_1, x_2, \dots, x_A)|^2$. If you have chosen a dysprosium nucleus in its ground state, for instance, there will be many different patterns possible, but every one of them whose probability is not negligible will show a strong deformation similar to Figure 1. For lead-208, on the other hand, the patterns will be much more spherical. In either case, you can calculate $|Q|^2$ for each pattern as in the classical case, and then you have a probability distribution for $|Q|^2$. This will probably look like Figure 2.

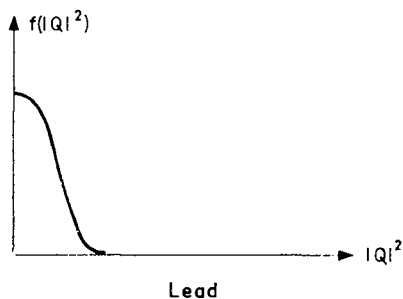
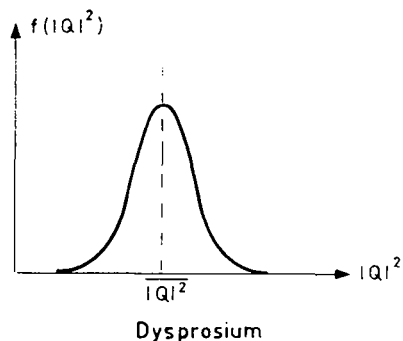


Fig 2

It is clear that dysprosium is strongly deformed and that lead is not. In dysprosium, it makes sense to characterize the shape by the mean value of the magnitude of the quadrupole moment

$$\overline{|Q|^2} = \langle \psi | |Q|^2 | \psi \rangle = \langle \psi | \sum_{\mu} Q_{\mu}^{\dagger} Q_{\mu} | \psi \rangle .$$

The second-quantized definition of the single-particle operator Q_{μ} in this formula is

$$Q_{\mu} = \sum_{\alpha\beta} c_{\alpha}^{\dagger} (q_{\mu})_{\alpha\beta} c_{\beta} .$$

Another example of a static collective property is the amount of pairing in a spherical nucleus. This is defined in a way quite analogous to our definition of the quadrupole shape. Instead of Q_{μ} , we introduce the pairing operator

$$P^{\dagger} = \sum_{\alpha\beta} f_{\alpha\beta} c_{\alpha}^{\dagger} c_{\beta}^{\dagger}$$

where $f_{\alpha\beta}$ is some suitable wave function for two nucleons coupled to $J=0$. This is not really a single-particle operator, but it is no more complicated since it also contains two c 's. In analogy with our definition of $|Q|^2$, we can define

$$|P|^2 = \langle \psi | P^{\dagger} P | \psi \rangle .$$

If this is statistically different from zero, the nucleus can be said to possess a static pairing deformation. As before, we could also look at the probability distribution for $|P|^2$.

We said at the beginning that a collective variable could be recognized from its large average value, or its large diagonal matrix elements. Let us ask now : large compared to what ? The answer is that it must be large compared to the value it would have for a completely chastic, uniform, non-collective "substrate". For the collective motion of a classical gas, for instance, the substrate is the equilibrium state of the gas given by statistical mechanics, and the collective phenomenon might be sound of long wavelength superimposed on this. For the two nuclear examples above, the substrate might be independent particles in a spherical well ; one could also add short-range correlations to this picture and make it a spherical drop of nuclear matter. Collective properties grow like the number of

particles, while for random motion the growth is proportional to the square root.

A useful distinction, at this point, is that between hard and soft nuclei. Dysprosium is hard, because its shape is almost rigid, as shown by the probability distribution of Figure 2. There are many soft nuclei, for which $f(|Q|^2)$ looks like Figure 3; they have no definite shape, but it would be wrong to call them spherical. Similarly, some nuclei are "pairing hard" and some are "pairing soft". A hard deformed nucleus can be set in rotation; this is a kind of collective motion which manifests itself by a rotational band. Similarly, a pairing-hard nucleus belongs to a band of ground states of adjacent even-even nuclei. A soft deformed nucleus undergoes a mixture of rotations and shape oscillations. A pairing-soft family of nuclei shows pairing vibrations.

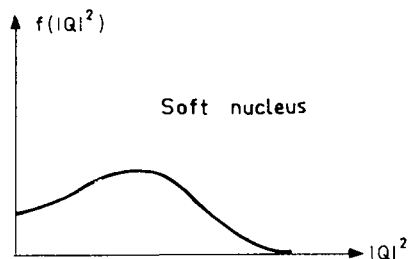


Fig. 3

From now on, I shall talk mostly about collective motion. First, I want to make a survey of microscopic theories of collective motion. Suppose that you know the many-body hamiltonian H ; how do you calculate collective motion? For this, you should first be able to calculate the substrate mentioned earlier. I shall assume that Hartree-Fock theory is adequate for this. This is not really correct; we should really be doing Brueckner-Hartree-Bogoliubov: Brueckner for dealing with short-range correlations, and Bogoliubov for pairing. But these are, after all, minor refinements on Hartree-Fock; if we can do a microscopic theory of collective motion for the Hartree-Fock case, it should

not be hard to add Brueckner and Bogoliubov.

In all microscopic theories of collective motion, the first problem is to choose collective variables; let us call them u_i . This is actually a very big problem and I shall come back to it. Then, the aim of the theory in the classical case is to derive equations of motion for $u_i(t)$. In the quantal case, what you are looking for is a Schroedinger equation

$$\mathcal{H} f(u) = \mathcal{E} f(u),$$

where $f(u)$ is a wave function of the u_i 's and \mathcal{H} a hermitian hamiltonian operator acting on f . In some of the theories, \mathcal{H} comes out as a second-order differential operator

$$\mathcal{H} = -\frac{1}{2} G^{-1}(u) \sum_{ij} \frac{\partial}{\partial u_i} g_{ij}(u) \frac{\partial}{\partial u_j} + V(u)$$

where $g_{ij}(u)$ is a symmetric matrix and $G(u)$ is the weight function to be used in calculating the normalization of f . This is a familiar type of Schroedinger equation. It looks like a quantized version of a classical theory in which there is a potential energy $V(u)$ and a kinetic energy quadratic in the velocities, with inertial coefficients depending on u . One has a lot of intuitive feelings for the solutions of such a Schroedinger equation, and in simple cases one also has a lot of experience in solving it numerically, since it is a differential equation. Some theories of collective motion actually go through a classical stage, then they obtain a hamiltonian of the above form by quantization. Others obtain this form directly. Finally, there are theories which do not give an \mathcal{H} of this form: in principle $\langle u | \mathcal{H} | u' \rangle$ could be the most general hermitian function of two sets of collective variables.

I shall review briefly some microscopic theories of collective motion that I know about:

1 - The random phase approximation (RPA). This is the theory which has been most used in the practical calculations of the past. It can be derived in many ways: one is through time-dependent Hartree-Fock, which I shall discuss later; other derivations go through quasi-bosons, or Feynman diagrams, or

linearized equations of motion. The RPA is the only theory in which the collective variables do not need to be chosen a priori ; they come out of the formalism. However, the RPA is severely restricted because it approximates \mathcal{H} by a harmonic oscillator hamiltonian ; it is a linearized theory. Therefore it is usually valid only for vibrations of small amplitude in the vicinity of static equilibrium. This corresponds to equally spaced energy levels and harmonic oscillator selection rules. For instance, fission could not possibly be described by the RPA. The RPA can be improved slightly by introducing small anharmonic terms, but even then it is still a very restricted theory. An advantage of the RPA, however, besides the fact that you do not have to guess the collective variables, is that it is valid for arbitrarily large oscillation frequency, or level spacing.

2 - Adiabatic time-dependent Hartree-Fock (ATDHF).

In this approximation, you assume that the wave function is a time-dependent Slater determinant. You pick a family of Slater determinants $\Phi(x_1, x_2, \dots, x_A; u_1, u_2, \dots)$ parametrized by the collective variables u_1, u_2, \dots and you assume that the wave function has to be one of these at all times. From this assumption, you derive classical equations of motion for $u_i(t)$. I shall give more details later. You can also derive an expression for the energy in terms of the collective variables u_i and the associated velocities \dot{u}_i . But you find that this energy is not of the familiar type quadratic in the velocities. In order to force it to be of this type, you make the adiabatic approximation, which is an expansion in powers of the velocities, stopping at second-order. You can then quantize in the usual way, and this gives a hamiltonian which is a second-order differential operator, as written earlier. The ATDHF approximation is valid under conditions opposite to those for the RPA. In the RPA, only small deviations from static equilibrium are allowed ; in ATDHF the deviations can be arbitrarily large. In the RPA, it is the excitation energies which can be arbitrarily large, while in ATDHF they have to be small compared to single-particle excitation energies (this is the adiabatic approximation).

3 - The Born-Oppenheimer-Villars approximation (BOV)

Here, you make a change of coordinates, from the original $x_1 x_2 \dots x_A$ to a new set which includes the collective variables. Call it $x'_1 x'_2 \dots u_1 u_2 \dots$. The way to do this is to introduce some constraints on the particle coordinates, one for each collective variable, the constraint expressing the fact that the collective variable has a certain value. The proper handling of the constraint from then on is of course crucial. After rewriting the many-body hamiltonian in terms of the new variables, you look for solutions of the form

$$\Psi(x'_v, u_i) = \Phi(x'_v, u_i) f(u_i) \quad .$$

Here Φ is like the electronic wave function in the Born-Oppenheimer approximation; it contains x'_v as variables and u_i as parameters. You obtain it by solving the Schroedinger equation with fixed values of u_i . This can be done, for instance, by static Hartree-Fock in the presence of the constraints. Afterwards, you are left with a Schroedinger equation for the collective wave function $f(u_i)$ alone, the analogue of the nuclear wave function for molecules. The eigenvalue of the Φ problem becomes the potential energy for the f problem. The collective hamiltonian in this approximation is also a second order differential operator. The BOV approximation seems to be closely related to ATDHF. The Born-Oppenheimer and the adiabatic approximations are obviously similar. However, in BOV there is no intermediate classical stage, and therefore no need to requantize. This is an advantage, because the quantization procedure is ambiguous.

4 - The generator coordinate method (GC) of Griffin-Hill-Wheeler.

In this approach, you start by choosing a family of wave functions depending on the collective variables as parameters. I shall call them $\Phi(x_1 x_2 \dots x_A; u_1 u_2 \dots)$. Since I have assumed a Hartree-Fock substrate, we can take these to be Slater determinants. Next you assume that a stationary wave function of the nucleus can be written as the most general linear combination of the Φ 's

$$\Psi(x_1 x_2 \dots x_A) = \int du_1 du_2 \dots f(u_1 u_2 \dots) \Phi(x_1 x_2 \dots x_A; u_1 u_2 \dots)$$

You use Ψ as a trial wave function in the variational

principle and you come out with a Schroedinger-like equation for f , which plays the role of collective wave function. This equation is

$$\int (u|x|u') du' f(u') = E \int (u|\eta|u') du' f(u')$$

$$\text{with } (u|x|u') = \langle \Phi(x,u) | H | \Phi(x,u') \rangle$$

$$(u|\eta|u') = \langle \Phi(x,u) | \Phi(x,u') \rangle .$$

This is obviously a very general method ; if you work with a complete set of Slater determinants you can even get the exact answer. The other approximations are probably all special cases of this, although the only complete proof that I know of is for the RPA. Anyway, it is meaningless for someone to say : I am using the approximation of generator coordinates. It is not an approximation. What is important is to ask : what Slater determinants does he include in his working space, and with what approximations does he solve the Schroedinger equation for f ?

5 - The method of Klein and Collaborators. The three previous approaches, ATDHF, BOV and GC, all attempt to derive a collective Schroedinger equation. Klein, on the other hand, adopts the Heisenberg point of view. Let me recall, for instance, how Heisenberg looked at the one-dimensional harmonic oscillator. He said : the problem is to find two matrices Q and P such that $QP - PQ = i$ and such that the matrix $H = \frac{1}{2} (P^2 + Q^2)$ turns out to be diagonal. This problem has a unique solution, which is of course identical to the Schroedinger one, although for the oscillator it is much easier to get it in the Heisenberg way. In a similar fashion, Klein starts by choosing a set of operators, usually single-particle operators of the type mentioned at the beginning of my talk, which he guesses will be important for a certain kind of collective motion. He expresses the many-body hamiltonian in terms of them, which involves some approximations. He calculates the commutators between the various operators, and again approximates by dropping pieces which cannot be expressed in terms of the operators he has chosen. In this way, he obtains a closed algebra, similar to Heisenberg's oscillator algebra, but usually much more complicated. He then finds matrix solutions by iterative methods. In this fashion, Klein's group has obtained some beautiful and sometimes very accu-

rate solutions for some relatively simple problems. It is my personal feeling, however, that, just as for most problems of ordinary quantum mechanics the Schroedinger method is far more powerful than the Heisenberg one, the same will be true in the microscopic theory of collective motion.

Now I shall give some details about the ATDHF approach. I actually know three different versions of it, and there may be more. It shall present a version which was worked out last year by M. Vénérone, C.W. Wong and myself. The other versions are by M. Saraceno and F. Villars and by A. Kerman.

First of all, a Slater determinant can be specified by giving the single-particle density matrix ρ , defined by

$$\rho_{\gamma\alpha} = \langle \Phi | c_{\alpha}^{\dagger} c_{\gamma} | \Phi \rangle .$$

The condition $\rho^2 = \rho$ is equivalent to the statement that Φ is a Slater determinant, and ρ is much more convenient to use than Φ . Now, take the Heisenberg equation of motion

$$i \frac{d}{dt} \langle \Phi(t) | c_{\alpha}^{\dagger} c_{\gamma} | \Phi(t) \rangle = \langle \Phi(t) | c_{\alpha}^{\dagger} c_{\gamma} H - H c_{\alpha}^{\dagger} c_{\gamma} | \Phi(t) \rangle$$

and assume that $\Phi(t)$ is a Slater determinant at all times. By straightforward application of Wick's theorem, you find the following equation of motion for ρ

$$i \dot{\rho} = W \rho - \rho W ,$$

where W is the usual Hartree-Fock hamiltonian, i.e.

$$W(1) = K(1) + \text{Tr}_2 V(1,2) \rho(2) .$$

I have assumed that the exact hamiltonian H has a one-body part K and a two-body part V . The TDHF equation of motion above is a generalization of the static Hartree-Fock condition, which is

$$0 = W \rho - \rho W .$$

You can easily show that the energy

$$E = \langle \Phi | H | \Phi \rangle = \text{Tr} K \rho + \frac{1}{2} \text{Tr}_1 \text{Tr}_2 \rho(1) V(1,2) \rho(2)$$

is independent of time if ρ is solution of the TDHF equation.

So, we have an equation of motion which describes the trajectory of the point ρ in the space of all Slater determinants. This equation, however,

is not in the usual form of a second-order differential equation, linear in the second derivative. Equivalently the energy is not in the usual form of a potential energy plus kinetic energy, the latter quadratic in the velocities. The reason for this is that all usual coordinates are even under time-reversal, and all usual velocities are odd, while here we are using a variable ρ which is neither one nor the other. Our ρ plays the role of both coordinate and velocity, and this is why the equation of motion is first-order only. So, we extract from ρ a time-even coordinate. This is done by writing

$$\rho = e^{i\chi} \rho_0 e^{-i\chi},$$

where both ρ_0 and χ are hermitian and time-even, and ρ_0 is a Slater determinant, i.e. $\rho_0^2 = \rho_0$. If we demand further

$$\rho_0 \chi \rho_0 = (1 - \rho_0) \chi (1 - \rho_0) = 0$$

it can be shown that ρ determines ρ_0 and χ uniquely. Now ρ_0 , which is a time-even Slater determinant, is our coordinate. If you ask any one to provide you with a family of Slater determinants to be used in some problem of nuclear collective motion, he will almost invariably give you nothing but time-even determinants, because he will take a family of time-even potentials and fill them to some closed shell. Hence, ρ_0 is actually the kind of Slater determinant that one always thinks of, and it is completely appropriate as a coordinate. Now, you can rewrite the equation of motion as two coupled equations for ρ_0 and χ , then you can eliminate χ and obtain a second-order differential equation for ρ_0 alone.

This equation, however, is still not of the familiar form, i.e. linear in $\ddot{\rho}_0$. This is because there is no reason to expect a kinetic energy quadratic in the velocities. If you want it quadratic, you have to make it so by expanding in powers of the velocities, and this is the adiabatic approximation. Now, if the velocities are small, it means that at all instants of time ρ is almost in static equilibrium, and therefore its time-reversed

$$\rho_T = e^{-i\chi} \rho_0 e^{i\chi}$$

must be almost equal to ρ . This means that χ is small, and an expansion in powers of the velocities

is also an expansion in powers of χ . Hence we proceed to expand either the equations of motion or the energy in powers of χ and stop at second-order. We write the expansions of ρ and W as follows

$$\rho = \rho_0 + \rho_1 + \rho_2 + \dots$$

$$W = W_0 + W_1 + W_2 + \dots$$

with

$$W_0(1) = K(1) + \text{Tr}_2 V(1,2) \rho_0(2)$$

$$W_1(1) = \text{Tr}_2 V(1,2) \rho_1(2)$$

etc....

I shall simply write the collective energy, since it is what we need later in order to quantize. One finds

$$\mathcal{E} = \mathcal{K} + U$$

The kinetic energy \mathcal{K} is the part of \mathcal{E} that is second-order in χ , while the potential energy U is zero-order and given by

$$U = \text{Tr} K \rho_0 + \frac{1}{2} \text{Tr}_1 \text{Tr}_2 \rho_0(1) V(1,2) \rho_0(2) \equiv \langle \Phi_0 | H | \Phi_0 \rangle.$$

This is simply the expectation of the many-body hamiltonian for the Slater determinant ρ_0 . It is what everybody has been calling potential energy, potential energy surface, or deformation energy surface. The real virtue of ATDHF, then, is that it provides one with a kinetic energy to be used jointly with the usual potential energy. This is

$$\mathcal{K} = \text{Tr} W_0 \left[(1 - \rho_0) \rho_1^2 (1 - \rho_0) - \rho_0 \rho_1^2 \rho_0 \right] + \frac{1}{2} \text{Tr}_1 \text{Tr}_2 \rho_1(1) V(1,2) \rho_1(2) \dots$$

It is quadratic in ρ_1 , and ρ_1 itself can be expressed linearly in terms of $\dot{\rho}_0$ through the equation

$$i \dot{\rho}_0 = W_0 \rho_1 - \rho_1 W_0 + W_1 \rho_0 - \rho_0 W_1$$

which is just the first-order part of the original TDHF equation. \mathcal{K} is therefore quadratic in the velocity $\dot{\rho}_0$.

These expressions are very general, and of course one should not think of solving them numerically with unrestricted ρ_0 . The thing to do, instead, is to pick a family of ρ_0 's depending on a few collective parameters, $\rho_0(u_1, u_2, \dots)$. Then the potential energy becomes a function $U(u_1, u_2, \dots)$. We can write $\dot{\rho}_0$ as

$$\dot{\rho}_0 = \frac{\partial \rho_0}{\partial u_1} \dot{u}_1 + \frac{\partial \rho_0}{\partial u_2} \dot{u}_2 + \dots,$$

so that the kinetic energy takes the form

$$\mathcal{K} = \frac{1}{2} \sum_{i,j} m_{ij}(u) \dot{u}_i \dot{u}_j.$$

This restricted problem can then be quantized, and the Schroedinger equation solved numerically.

How can we choose suitable families of ρ_0 's? This choice is crucial, of course, for the quality of the approximation. The same problem arises in the GC method and is even more serious there, since in ATDHF we are only looking for time-even determinants, while in GC it might well be necessary to give the determinants a time-odd part, which makes them unfamiliar. The simplest thing to do, and that is what is most often done, is just to guess a family of single-particle potentials $R(u_1, u_2, \dots)$ which look reasonable and fill them with nucleons. This amounts to telling the nucleus: here is a family of shapes that you are allowed to have; choose between them, but every other shape is forbidden. A less autocratic procedure, often advocated by A. Kerman, is to subject the nucleus to a variety of external potentials and let it find its own shape. This is certainly a lot more work for the theorist, but perhaps it is worthwhile. Let the external potentials be S_1, S_2, \dots and give them variable strengths u_1, u_2, \dots . Then the hamiltonian of the constrained nucleus is

$$H_u = H - u_1 S_1 - u_2 S_2 - \dots$$

Letting the nucleus find its own shape means looking for the static Hartree-Fock solution of H_u . This yields a density-matrix $\rho_0(u_1, u_2, \dots)$. The potential strengths u_1, u_2, \dots can then become collective coordinates and the collective energy can be calculated as outlined earlier, with H as hamiltonian, not H_u .

It is interesting to think about the relationship between ATDHF, BOV and GC. The first two are certainly very close, but I do not know what the exact relationship is. BOV does not need to be quantized and therefore, if it should turn out to be no more complicated than ATDHF, it should be preferred. For the same reason, and also because GC is

very general, one would like to know how to derive ATDHF from GC. I do not think any one has done this yet. It seems to me that it will not work as long as people choose, as they almost always do, a GC family of determinants which is purely time-even. The evidence for this is well-known and was brought out by Peierls and Thouless long ago. It consists in looking at the simplest kind of collective motion, translational motion, and using the various theories to calculate the total mass of the nucleus. ATDHF gives the right answer, using as ρ_0 -family the various static Hartree-Fock solutions centered on all points in space. GC with the same ρ_0 -family gives a wrong answer. Peierls and Thouless showed that, to get the right answer, you need a two-parameter family whose determinants are not all time-even. A similar discrepancy between ATDHF and GC also exists for the next simplest kind of collective motion, rotations, even though in that case the exact answer for the moment of inertia is not known a priori. It seems to me that, even if the ATDHF approach should some day be found too crude for accurate work, it will always be useful for the purpose of manufacturing non-time-even Slater determinants to be used in the GC method. In principle this would work as follows. To every set u of time-even generator coordinates you associate a set of velocities \dot{u} , thus doubling the number of generator coordinates. Given the time-even $\rho_0(u)$ and given \dot{u} , you calculate ρ_1 by solving the linear equation written earlier ($i\dot{\rho}_0 = \dots$). Since ρ_1 is essentially the same as χ , you are now able to use $\rho(u, \dot{u}) = e^{i\chi} \rho_0 e^{-i\chi}$ as your generator determinant. One would expect such a theory to be at least as good as ATDHF in all cases.

The derivation of ATDHF from GC is only one particular way of getting a Schroedinger equation of the familiar type, i.e. a second-order differential equation, starting with the GC approach. There exist other ways of approximating the GC expressions to achieve the same result. Some nice work along these lines has been done by B. Banerjee and D. Brink recently. But I have discussed pure theory long enough, and it is time to ask, perhaps, whether any of this can be useful in present day nuclear physics.

One striking feature of present day nuclear theory, in my opinion, is that it is becoming more and more unified. In the old days, we used to have a collection of separate models, each describing part of the phenomena. We had structure theorists and reaction theorists, and they never talked to each other. We had a collective or hydrodynamical model for many properties of heavy nuclei, which was unrelated to the shell model. We were content with a purely statistical description of neutron resonances. Nowadays, on the other hand, people talk the same language more and more. Structure and reaction theorists cannot live without each other. Many of the old pieces are now fitting together into a gigantic whole. Not only is it allowed, but it is fashionable to calculate collective properties from the shell model. And it is not unthinkable to try to calculate the density of neutron resonances, averaged over a suitable energy interval, from more fundamental theory.

Every theorist has the microscopic point of view at least at the back of his mind. Moreover, everybody agrees what the microscopic theory should be. It goes through the shell model, and then the shell model has to be justified from the nucleon-nucleon interaction. Only the optimists, however, are hoping for a complete explanation of all nuclear phenomena *ab initio*. For most of us the field is too vast, there are still too many missing connections, and we still very much need the old models (collective, statistical,...) as crutches to help our explorations. The difference with the old days, however, is that we are learning to connect the models with each other, we believe they are connected, and we are not afraid of mixing models when discussing a single phenomenon.

The collective model consists in picking collective variables and writing a collective Schrodinger equation in a purely phenomenological way, fitting the parameters to the data. Taken alone, the collective model can explain anything, because you are free to introduce so many parameters that you can fix anything : the model has no predictive value. This is why people have been so interested in deriving the collective model from microscopic theory. The present state of the static microscopic theory (the theory for the substrate) is not entirely

satisfactory, but there has been, nevertheless, some very big successes, in particular with the calculations of nuclear matter and of closed-shell nuclei.

If we want to be practical about applying the same microscopic theory to collective motion, we must take into consideration the fact that the best Brueckner-Hartree-Fock calculations of ^{208}Pb , for instance, take several hours of computing time. Collective motion of a heavy nucleus is much more complicated than ^{208}Pb . First the shapes are complicated ; second you need a different BHF calculation for each possible shape. This is completely out of the question with present computers. If we want to do a collective calculation, we must simplify the microscopic theory greatly, and of course we must do so without losing its essential features. And so, it is fortunate that in recent months a simplified version of BHF has been emerging which shortens the computing time by a factor of several hundreds. I am referring to the calculations with forces of the Skyrme type which were done by Brink and Vautherin and other people at Orsay, and also by Moszkowski, and whose justification on the basis of orthodox BHF theory has been given by Negele and Vautherin. Thanks to this approach, a true microscopic calculation of collective motion may see the light in a few years.

If it does, it will be again a very long calculation, which won't be repeated very often. This is not enough ! We want to be able to play around with these things ; we need an even simpler method. Well, there is one ; it has been around for a few years and is very much in use today ; it is called the Strutinsky method, the principle of which has been explained to us several times this week already. Unfortunately, this is one place where the unity of nuclear theory, which I extolled earlier, is absent. The Strutinsky people and the Hartree-Fock people speak different languages and they are not terribly amicable toward each other. In view of the importance of the Strutinsky method in simplifying collective calculations, it is terribly urgent that the connection between Strutinsky and ATDHF be studied. Such study has not gone very far yet. For the collective potential energy, the Hartree-Fock people seem to

agree that Strutinsky is right at the static equilibrium points, but not elsewhere. For the kinetic energy, the Strutinsky people use the cranking formula, which differs from the ATDHF kinetic energy which I gave earlier in the following way. First, the single-particle hamiltonian which you use to build the Slater determinant ρ_0 , which I have called K+R earlier, is assumed to be the same as W_0 . This is not true usually; it holds only for the static HF solutions. Second, the equation connecting ρ_1 and $\dot{\rho}_0$ is truncated and replaced by $i\dot{\rho}_0 = W_0\rho_1 - \rho_1 W_0$. Third, the last term in the kinetic energy, $1/2 \text{Tr} \rho_1 V \rho_1$, is left out. Perhaps some of these approximations compensate each other, but I do not know of any numerical study of these things, and a serious look at them is badly needed.

Among the various microscopic theories which I discussed earlier, I think that ATDHF is particularly suited to the present, rather primitive, state of theory and experiment. This is true of Strutinsky theory too if someone succeeds in connecting the two. First of all, ATDHF goes through a classical stage which appeals to our intuition and makes it the embodiment of the collective model. This classical aspect means that it is restricted to heavy nuclei. No calculations in the sd shell, please! Second, the theory is adiabatic, which means that collective motion must be slow compared to single-particle motion, but the shapes can vary all over the place. This is exactly what we need for the applications that we have in mind, namely quadrupole oscillations of soft nuclei, fission, and collisions between two heavy nuclei. In the even soft nuclei, the collective excitation energies are appreciably smaller than the pairing gap, so that there is no question that the adiabatic approximation is at least roughly valid. In the other two phenomena, for present-day experiments, the velocity of the collective motion is also always small compared to single-particle velocities. For instance, consider the experiments of Flerov and collaborators who bombard ^{238}U with ^{136}Xe . The center of mass energy is 900 MeV,

but the Coulomb barrier is 600 MeV, so that by the time the nuclei touch they have only 300 MeV, which is less than 1 MeV per nucleon. Since the average kinetic energy of a nucleon in a nucleus is of the order of 20 MeV, this is very slow motion indeed. Any experiment in which the collective energy per nucleon, after going over the Coulomb barrier, is no more than a few MeV, is a candidate for the adiabatic treatment. This does not exclude the creation of a few single particle excitations in addition to the overall adiabatic motion. The bulk of the two nuclei stays in its instantaneous ground state, as the adiabatic approximation would have it, but there is certainly plenty of energy to allow a few nucleons to do funny things. Such a situation would have to be described by a coupled channel calculation, each channel corresponding to a different state of the excited nucleons, with the bulk moving according to ATDHF in every case. We are far from knowing how to do this very well in practice. Such a coupling situation will occur especially easily if the colliding nuclei carry nucleons in open shells; it is essential for the description of transfer reactions; and of course we need it also to calculate the properties of the odd soft nuclei. It is nothing but a restatement of the Bohr-Mottelson idea about coupling single-particle degrees of freedom with collective degrees of freedom. The same single-particle excitations, suitably averaged, may also give rise to the viscosity mentioned by Swiatecki in his talk; it should be possible, somehow, to include it in the collective Schroedinger equation.

I have no doubt about it. Quantal collective motion is nowhere as varied and as rich as in nuclei. The experiments of the next few years will reveal it in its full glory, and I hope that the theorists can keep up.

DISCUSSION

W.J. SWIATECKI (Berkeley)

Your talk dealt mostly with deriving the kinetic energy. What about damping terms? We are worried that they might be overwhelming compared to the kinetic energy. Can you give any first order estimate if the relative importance of damping, say at 30-40 MeV excitation, such as might be of importance in heavy-ion reactions?

M. BARANGER (M.I.T.)

I know at least one case where damping is not important, even negligible. This is the case of soft nuclei, on which I have worked myself. Concerning fission and collisions between heavies, I was convinced by the discussion at the end of your talk the viscosity there would be rather different from the usual viscosity of hydrodynamics. I have never tried to calculate it. I only have a vague feeling that, if there are many kinds of single-particle excitations superimposed on the collective motion, it should be possible to average them and the result would be some sort of viscosity. Your point that the damping might well be much more important than the kinetic energy is well taken. I agree with you that we should try to find out.

G. RIPKA (Saclay)

I want to suggest that α -nuclei (^{12}C , ^{16}O , ^{20}Ne , ^{24}Mg) should be considered as soft nuclei and used to test the models you describe. There are many reasons for this:

- 1 - The collective variables are well defined: they are the distances between the α -clusters.
- 2 - They are soft because the calculations show that they are easy to bend or distort and without appreciably losing energy.
- 3 - They have various equilibrium shapes with weak but observable transitions.

M. BARANGER (M.I.T.)

All right, but I don't think the classical approximation would be valid any more. You could use the method of Villars, perhaps, or a version of generator coordinates equivalent to ATDHF, as long as you bypass the classical step.

A.P. ZUKER (Saclay)

"Not only is it allowed but also fashionable to calculate collective motion with the shell model" I am quoting the speaker. Now I quote Bruce French: "As things stand now most theories are models of the shell model". I would like to protest against the omission of the shell model from the long list of collective theories available presented during this talk. On its own right and with its own methods the S.M. has produced many explanations of nuclear behaviour (collective and otherwise). It is not a theory of single particle motion or a convenient framework for deriving other theories only.

It is certainly true that you are interested specially in "collective" properties, but then let me point out that we can use your definition of collectivity given at the beginning of the talk, with which I very much agree: a dynamic or static behaviour that enhances a given operator. Instead of the quadrupole or pairing operator take the 1 particle operator. You obtain then a most striking example of collectivity: the single particle states in ^{17}O , ^{41}Ca , ...

M. BARANGER (M.I.T.)

The reason why I did not include the shell model in my list of incomplete models is that I believe that the shell model is the microscopic theory. Therefore I agree completely with you. I thought that one of the points of my talk was to say just this. I am sorry if it did not come across.

C.M. NEWSTEAD (Karlsruhe)

You mentioned that it might be possible to calculate neutron level densities on a more fundamental basis. Could you please tell us what you have in mind?

M. BARANGER (M.I.T.)

I have in mind something very vague. I have never done it. I just know that when you start averaging things over energy, the expansions of perturbation theory become more convergent. It

is not unthinkable, therefore, that one might calculate an averaged level density by one of the perturbation methods of statistical mechanics. It is unthinkable indeed to try to calculate the exact levels.

K. BLEULER (Bonn)

The effect of viscosity in nuclear structures should come out naturally through the introduction of collective variables by means of a transformation of variables (Oppenheimer and

Villars). I have the impression that the effect might be larger in heavier nuclei where we have relatively dense levels corresponding to intrinsic excitation (enhancing the change-over from collective excitations). There is, however, a question as to the choice of the collective variables : I would like to suggest the use of parameters which describe the admixing between the different Hartree-Fock solutions (in addition to the variables describing the degeneracy of a single, deformed Hartree-Fock state).