

MEMORIES FROM 18 YEARS OF COLLABORATION WITH PHILIPPE

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(Contribution to the „Symposium in honor of Philippe Quentin“, held at CEN Bordeaux-Gradignan, April 29, 2013)

When I had finished my PhD in 1972, I was invited by Gerry Brown to spend my first postdoc year at Stony Brook, State University of New York. Although the academic year started in September, he urged me to come already in August, because that was the month when he used to invite many physicists from all over the world for intensive scientific discussions. He told me that he would also invite two French colleagues doing Hartree-Fock calculations for nuclei with Skyrme interactions. Since I had started my scientific research with Strutinsky and done my PhD thesis on his shell-correction method, Gerry thought it might be interesting to discuss and compare the purely microscopic HF approach with Strutinsky's phenomenological method.

So, in August 1972 I met for the first time Philippe Quentin, who had come to Stony Brook together with Dominique Vautherin from the Orsay group. Vautherin brought and explained me his HF code, and with Philippe we soon planned to perform a self-consistent microscopic test of the Strutinsky approach. Somehow, while discussing these plans, we felt that we had several things in common, and a friendship started to grow. At weekends, we all went to South Hampton Beach, and while working we once sent our wives to see New York. So there went three physicists' wives, Mireille, Lis, and the wife of Tuomo Kouki. Here is the proof for two of them (Lis took the picture):



To the right is Mireille, beautiful (as all our wives) and rather frail-looking; later I learned how strong a woman she could be as a mother of six children and a very busy physicist's wife! I remember well how nervous Gerry was during the day they went to New York, because 40 years ago that city was by far not as safe as now. Today you can stroll around Times Square even at night – at that time this might have become suicidal. But our wives returned safely to Stony Brook.

The project with Philippe became my main research program during the first year at Stony Brook, and our results started to show that Strutinsky's "energy theorem" could not only formally, but also numerically be justified from the HF calculations with a good precision. I was shocked when Gerry on a spring day announced to me that Arthur Kerman in Boston had done similar calculations, together

with Larry Willets at Seattle, and claimed to come to different conclusions. As was typical of Gerry, he immediately had me invited to the MIT for a seminar. I became, of course, extremely nervous because this was to be my first external seminar in the USA -- and about a topic that brought me up against Kerman and Willets! But John Negele, whom I had previously met in Copenhagen, cleverly arranged for a private discussion with Kerman an hour before the seminar. Then, Kerman had an occasion to fire all his critical questions at me. I stood the test, and the seminar went smoothly. Gerry was delighted because Kerman was, above all, his "beloved enemy".

By correspondence, Philippe and I agreed that we both should apply for postdoc positions at the Niels Bohr Institute for the next year. So, from the fall of 1973 we both worked in Copenhagen. We further developed and refined our HF-versus-Strutinsky tests. The ultimate paper which we published on this subject came out in 1981 in the Sven Gösta Nilsson memorial issue of Nuclear physics:

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**THE STRUTINSKY METHOD AND ITS FOUNDATION
FROM THE HARTREE-FOCK-BOGOLIUBOV APPROXIMATION
AT FINITE TEMPERATURE**

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M. Brack, P. Quentin / The Strutinsky method

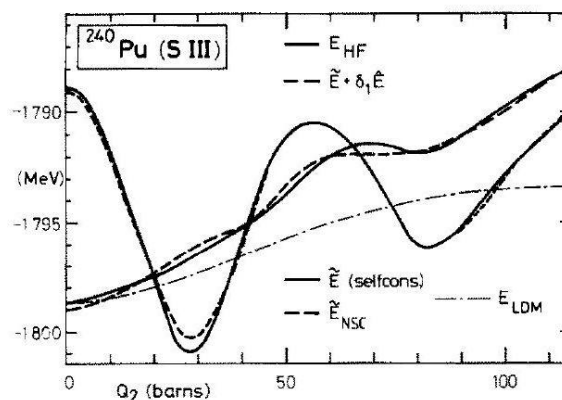


Fig. 6. Fission barrier of ^{240}Pu calculated with the Skyrme SIII effective force. The HF energy E_{HF} is compared with averaged energies \bar{E} (both self-consistent and non-self-consistent) and with its Strutinsky approximation $\bar{E} + \delta_1 \bar{E}$. The liquid drop model ⁸⁶⁾ estimate E_{LDM} of the fission barrier is also given. The total quadrupole moment Q_2 was constrained as in fig. 4.

The figure above shows a typical fission barrier of Plutonium 240. The curves with the two deep minima are the self-consistent HF result (given by the solid line) and the Strutinsky result (given by

the dashed line) as a sum of the average deformation energy plus the (first-order) shell correction, both derived from the self-consistently smoothed mean field. The difference between the two curves amounts to less than one MeV; the same was found also in all other cases that we tested.

During that year in Copenhagen, we also did the first (correct) HF calculations for nuclei at finite temperatures, after Ben Mottelson in a seminar had raised the question to what extent the nuclear mean field would be affected by temperature. We could show that up to rather high excitations, the self-consistent mean field (and hence the single-particle levels) remained practically unchanged, so that the excitation energy was mainly coming from the finite-temperature Fermi occupation numbers.

Privately, we made the agreement with Philippe that physics would be discussed in English and all other matters in French. This helped me to acquire a good working knowledge of French. I had had an excellent French teacher at school (actually, I have met him just two weeks ago, after 50 years, at the age of 97 -- still going strong!). He had taught us a solid French grammar, and occasionally I could impress you using a correct subjunctif. My speaking fluency was, however, only developed then and there (and, of course, later when I visited you in Orsay). With Lis, we have very fond memories of that time together with you and Mireille in Denmark. Unfortunately we could not find any photos showing you. But we have one of the Niels Bohr Institute, taken only a couple of years ago:



In 1968 – 1970, I was sitting in the room Kc7 up on the third floor (with the window on the other side towards the park), together with Strutinsky and the other members of the “Funny Hills” team; also in this room were Bill Myers and John Negele during the two successive years. With you (and later Rajat Bhaduri) in 1973 – 1975, we were sitting in a room under the roof more to the right. At that time, there was a little wooden building in front of the institute with rooms for students – the garden definitely looks nicer now without it.

After one year, Philippe returned to Orsay, while I remained in Copenhagen for another year. There I met Rajat Bhaduri who took a sabbatical from McMaster University. He educated me in the extended Thomas-Fermi (ETF) model and became my next physicist-friend. Due to some financial tax intricacies, the NBI could not pay me a full salary that year, but Philippe intervened by inviting me for several months to Orsay, where we could continue our collaboration. I remember that you and Mireille generously allowed me to live in your apartment in Le Guichet while you left for some time. I also vividly remember the night shifts during which we did our HF calculations on the UNIVAC at the Institut de Physique Nucléaire. The computer at that time was so slow that one HF iteration lasted about 15 - 20 minutes. Since crashes could come any time, we had to safeguard the results of each iteration by punching them onto cards, and the stacks of cards had to be used as an input for the following iteration. What a disaster when a stack fell to the floor and the cards came out of order!

From the summer of 1975 I went back to Stony Brook, and in March 1977 I was hired as a senior scientist at the Institut Laue Langevin in Grenoble. Your former teacher, "le Maître" Vénéroni, had recommended me for that position. In September 1978 I joined the physics faculty at the University of Regensburg, where I have stayed for 34 years now, while you took the position at the ILL for some time. Our collaboration continued, partially mediated by our common colleague and friend Jonny Bartel, who became my first PhD student in Regensburg and thereafter your postdoc. In a common paper with the two of you, still in Grenoble, and with Claude Guet, Hans-Bertil Håkansson and myself in Regensburg, the so-called "SkM*" force was developed, which yielded a good description not only of ground-state properties and giant resonances, but also of fission barriers of actinide nuclei.

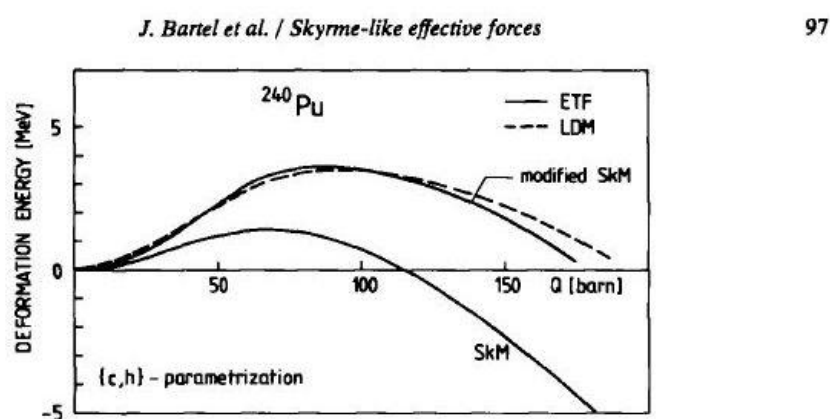


Fig. 10. Self-consistent semiclassical (ETF) ^{240}Pu fission barriers calculated for the SkM and "modified SkM" forces. The liquid drop (LDM) fission barrier is taken from ref. ⁶⁾. The barriers have been obtained using the $[c, h]$ shape parametrisation of ref. ⁶⁾ along the static fission path in the $[c, h]$ plane, and then plotted against the corresponding quadrupole moments.

Here you see the smooth part of the Plutonium-240 fission barrier. The dashed line is the empirical liquid drop model (LSM) barrier. The lower solid curve is the average (ETF) barrier obtained with the Skyrme force SkM that had been fitted by Bogias *et al.* including giant resonances, and the upper solid curve that obtained with our new "modified SkM" force, henceforth called SkM*. In spite of hundreds of other Skyrme force parametrisations that have been developed all over the world since 40 years, SkM* has survived to our day and is still being used by many colleagues. According to "Google Scholar", that paper has been cited 740 times by now. It ranks amongst the four most quoted ones of my publication list.

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**TOWARDS A BETTER PARAMETRISATION
 OF SKYRME-LIKE EFFECTIVE FORCES:
 A CRITICAL STUDY OF THE SkM FORCE†**

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Altogether we have had active collaborations over nearly 18 years. I have learned enormously from you, Philippe, over that time -- physics- and otherwise. From you I learned the rigorous mathematical approach to microscopic physics, including HFB theory, and the scaling-type approach to collective excitations. Our last common paper -- the 13th if I have counted correctly -- was published in 1990 in *Annals of Physics*; it was about nuclear giant resonances and involved Jacques Meyer and another of my Regensburg students, Peter Gleissl:

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**A Density Variational Approach
 to Nuclear Giant Resonances
 at Zero and Finite Temperature***

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Below is a figure showing how the SkM^* force performs for the nuclear isoscalar giant monopole resonances (GMR). Here we employed two versions of the scaling approach. First we used one single scaling variable, leading essentially to the same results (shown by crosses) as the phenomenological LDM fit (solid line). Then we used a second scaling variable, having both the bulk density and the surface thickness oscillating independently (a technique which I had previously studied in a collaboration with Wilhelm Stocker in Munich). This led to the boxes (connected by the dashed line) that by and large follow the experimental data (vertical dashes). The improvement reached for medium and light nuclei by the two-mode approach is evident.

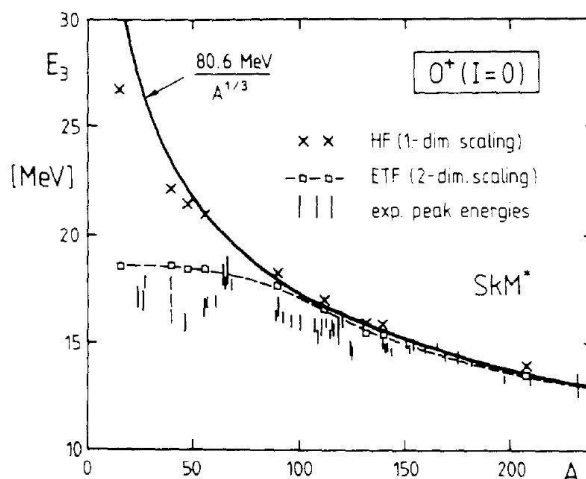


FIG. 3. Breathing mode (GMR) energies obtained with the SkM* force. Crosses: HF energies E_3 with operator (2.21), as in Fig. 2. Squares: lowest of two coupled modes from 2-dimensional scaling of ETF densities. Error bars: experimental values as in Fig. 2. Solid line: LDM fit proportional to $A^{-1/3}$.

(Later in the 1990s together with Paul-Gerhard Reinhard from Erlangen and my student Stephan Kümmel, we extended this technique to what we finally termed the “local current approximation”, LCA, and applied it to collective excitations in metal clusters.)

Concerning our private lives, Philippe and I always have had lots of common interests: music, nature, religion, literature. We also liked to go to the movies together -- mainly during conferences and physics schools, when we were free of family duties. I remember that we in Paris saw the nice Danish movie "Le festin de Babette", and in Trieste we watched a rather weird movie that made fun of the clerics; it must have been by Buñuel or a similar producer. Also at Trieste, we once went to the opera house and saw "Falstaff" by Verdi. It was surely not a glamorous, but a good average Italian production. (As an aside: later I was to see Falstaff again at the Met in New York. There, my wife and I shared two subscription tickets together with Dan Strottman and his wife. We had to agree who was going to see which opera, and you had left the Falstaff to us -- do you remember that, Dan?) What I remember most of the performance in Trieste is two things. First, we had bought our tickets at the last minute and thus only obtained seats on the uppermost gallery, all upfront so that we actually could look from behind into the box of the souffleur. He was an elderly Italian with a classical Roman profile and big hands with long fingers. He was actually much busier than the conductor of the orchestra; it was mainly he who conducted the singers (evidently also singing himself -- although we could not hear that). The second thing I remember well is one musical motive of Verdi's music. Falstaff had made a secret appointment with one of the Merry Wives of Windsor, to be realized between 2 and 3 o'clock -- "dalle due alle tre" in Italian. The music for "dalle due alle tre" became a kind of recognition sign between the two of us. Everywhere in the world where we met again, we would greet each other with "dalle due alle tre" -- sometimes even in the middle of a meeting or a discussion -- and have a good laugh.

We also both liked hiking in the mountains -- both in the summer and, on skis, in the winter. In Grenoble I came to know the family of Mireille's brother, Alain Bertrand. With them I had wonderful mountain hikes and, in particular with Alain, some memorable ski tours. Later, when you also worked at the ILL, we went hiking all together.

One important thing that we shared and still share is our religious belief. We are convinced that, however far we may come in using mathematics and physics to describe the material aspects of our world, this will never explain to us what our true origin and purpose is. We share our belief and faith in God as our Creator and our Salvator. Although we came from different churches -- you a devout catholic and I a convinced protestant -- this was no obstacle between the two of us. On the contrary, it led to lots of intensive and deep-going discussions. Something that surprised me initially was that you combined your belief with a socialist political conviction. I remember that in Copenhagen, when the presidential elections in France were coming up, I asked Mireille which candidate the two of you would support. She answered me without hesitation and very convinced: "Mitterrand, of course!" Clearly, Christianity and socialism have very much in common -- at least ideally. Jesus Christ, when he lived as a human on earth, behaved like a socialist in many ways. But in Switzerland and Germany, those political parties which have a "C" (for "Christian") in their names are conservative, while socialists or social democrats often are proud to call themselves atheists. In France, however, you have a different culture, particularly amongst physicists. I think this goes back to persons like Marie Curie. (By the way: Einstein, too, who believed at least in a Creator God, was a convinced socialist. But he warned against Soviet-type aberrations in his remarkable article "Why Socialism?".) I think that these convictions of yours were the reasons for your success as a director of the CSNSM at Orsay, where you not only did research but also took care of many problems of the personnel. And these convictions are surely also the driving force behind your marvelous work in Vietnam and Cambodia.

I want to thank you, Philippe, for all that you have given me during a long time of collaboration and friendship. I also thank Mireille for her warm participation in our doings and for her everlasting hospitality -- be it in Denmark, Grenoble, Orsay, or Bordeaux. I remember that once I was visiting Grenoble while you lived there, we went on a common ski tour. We came into a snow storm -- or at least a strong cold wind with snowfall -- and I had problems with the bindings of my skis. Eventually my forces faded and we had to brake off the tour. Completely exhausted, I was taken back to your home, put in a bed and given hot tea to recover. As far as I remember, that was the last serious ski tour that I endeavored -- in Bavaria we only went for soft downhill skiing with our children, and once when we had a lot of snow, I went on skis from our home all the way to the university and back.

Our scientific paths diverged when in 1988 I started working on metal clusters and later, together with Bhaduri and Murthy, discovered the semiclassical Gutzwiller theory, then used it with many of my students and other collaborators -- and finally remained trapped by classical orbits for the rest of my career. It is very unfortunate -- and I feel still ashamed for it -- that you never were invited to give a colloquium in Regensburg. The reason was that the colleagues in power, misguided by the research politics in Germany of that time, claimed that nuclear structure physics was old-fashioned and without further interest -- the future was meant to lie in medium-energy physics, pions, etc.

But in spite of all this, you generously kept inviting me to Paris and Bordeaux, and our friendship has never faded, even after longer periods of separation. Whenever I had a chance of meeting you, Philippe, it has been a great pleasure -- and so it is here and now to share these memories with you.

I wish you the very best -- most of all our Lord's blessings -- for the future of your work and for the life with your family!