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Review paper

How everything started: A retrospective

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A R T I C L E I N F O

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A B S T R A C T

Recalling some of the most important events and persons during his education and career, the author sketches his growth from a young engineer, educated in the sanctuary of solid state reactions, to an involved fully devoted scientific career for the study of fluids in the deep Earth. Most important in this respect was the discovery of CO_2 inclusions in granulites, which triggered years of discussion on fluid-absent or fluid-assisted granulite metamorphism. To some extent, this debate is a continuation of the former granite controversy, but it shows also how the famous battle of "soaks against pontiffs" could have been easily avoided.

1. Introduction

About 10 years after my official retirement in Amsterdam (see for e.g. Andersen et al., 2001), I feel very honored and, to say the truth, extremely happy that younger colleagues, who now have the future of our discipline within their hands, took the initiative to organize another special issue in this rapidly growing journal, *Geoscience Frontiers*. Daniel Harlov asked me to write a general paper on recent developments in the field of metamorphic petrology. I tried, but found rapidly that so much was to be said that any paper of decent size would be grossly biased and incomplete. Discussing with colleagues and former students, a number of them present in this issue, I had the feeling that much is known about my activities in Holland, less about the first part of my career, in Nancy and Paris. Funny enough, these two parts- of approximately equal duration, each slightly more than 20 years- correspond also to some change in my name: Jacques Touret in France (the French use only the first name), J.L.R. Touret in Holland (the Dutch write exactly what stands on your identity card). This has not a major importance, except possibly for bibliographic markers like citation index, luckily unknown at this time. But feeling that only few friends knew about my early days has induced me to put on paper the reasons which led me to devote a great part of my scientific life to the study of minute bubbles in rocks issued from the most extreme depths of our mother Earth.

2. The Ecole de Géologie (ENSG) in Nancy (France)

When I ended my secondary education in 1953, in the Lycée Chanzy of Charleville (Ardennes), I was rather undecided about the follow-up of my studies. My parents were school teachers in a small village, almost on the Belgian border, and I had spent all my youth in a rural environment, also in a region which, since the "Siècle des Lumières", had provided the slates covering a number of European palaces. My first decision was to cope with the rather peculiar Napoleonic system of higher education, namely to enter either a university or a *Grande Ecole*. In France, the *baccalauréat*, final exam at the end of the last year in a "Lycée", opens freely the doors of any university. Napoléon, who owed much to the education that he had received in the Collège de Brienne, wanted above all that the elites of his empire would not be in too close contact with universities, that he considered as dangerous assemblies of "*libre penseurs*". But he wanted also to have the very best in his own service, admitted after a rigorous selection in few high-education institutions. Almost

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unique in any country, these did not depend on the Ministry of Education, but on any ministry according to their specialty. The best coveted *Grande Ecole* is the Ecole Polytechnique, intended in the mind of Napoléon for the formation of army officers. Almost no students from the Ecole Polytechnique enters the army today, but the Ecole is still headed by an army general, and is funded by the Ministry of Defense. The best students in the *Lycée* were allowed to enter a preparatory school (*Classe Préparatoire*), also located in the Lycée and run by its best professors. Napoléon himself was a skilled mathematician, being elected a regular member of the Academy of Science after having solved what is now known as the Napoléon problem, namely how to find the center of a circle with a pair of dividers only. One of his closest collaborators and founder of the Ecole Polytechnique was Gaspard Monge, Comte de Péluse (1746–1818), inventor of descriptive geometry and one of the greatest mathematicians of the time. The programs of the Classes Préparatoires relied then firstly on mathematics, and secondly on other sciences, such as physics and chemistry. In 1953, they had not changed much since the time of Napoléon.

Having obtained the right to apply for a Classe Préparatoire, not in the small lycée of the Ardennes (which did not have any), but in the greater institution of Lille Nord, my first choice was to prepare the entrance examination of the Institut National Agronomique, which is for agriculture what the Ecole Polytechnique is for the army. This preparation lasted for 2–3 years, depending on the success in the exam. Again, the strongest disciplines were mathematics and, to a lesser extent, physics or chemistry, considered less as a necessary background for further studies then as the easiest, most objective way to ensure a drastic selection. As far as the Grandes Ecoles were concerned, the only significant change, which had occurred since the time of Napoléon had been the addition, after World War II, of a number of institutes depending on the Ministry of Education. These cover disciplines formerly considered as relatively minor, not important enough to justify the formation of specialized engineers. This was notably the case for geology, which during the war had demonstrated its importance in finding new mining resources. The Ecole Nationale Supérieure de Géologie (in a typical French fashion, the complete name is quite large, Ecole Nationale Supérieure de Géologie Appliquée et Prospection Minière) was created in Nancy in 1946, under the direction of Marcel Roubault (1905–1974), with his regular co-author René Perrin (1893–1966), one of the leading figures of the transformist school and stubborn tenant of solid state reactions in petrology (Fig. 1).

With only about 25 students each year, the Ecole de Géologie was too small to have a separate Classe Préparatoire. The preparation was the same as for the Institut Agronomique, with a separate entrance examination. It happens that when I was in the second year of the *Classe Préparatoire* in Lille, 4 of my fellow students wanted to apply for the exam for the Ecole de Géologie. But the local center required at least 5 applications. I was relatively young and thought that I would not have any chance of success anyway, either to the *Agro*, or to the Ecole de Géologie. The average duration of the preparation in the whole country was close to 3 years, and only one student in Lille had succeeded to enter one of the two Ecoles since the creation of the Classe Préparatoire, some 10 years ago. So I decided to join, first of all to spare my colleagues a strenuous trip (almost one day by train) and difficult stay in Paris. We were apparently a rather strong group. To the surprise of our professors, 3 of the 5 were accepted in both Ecoles. Marcel Roubault, who had a strong sense of communication, came in person to the oral examination. He described the adventurous life of an exploration geologist, at a time when France was desperately searching for the riches of its threatened colonial empire. He was so convincing that I forgot about agronomy and chose geology instead. It is fair to say that I had been influenced by a number of persons, who, during my youth, had introduced me to the wonders of Nature. The first was a priest in Esperaza, southern France where, during the war, my parents had fled to escape the battles raging in northern France. He had shown me human and other fossils. I was only about 6 years old then, but remember perfectly the broken jaw and rounded balls, which I realized many years later were dinosaur eggs. This region is now known to host Mesozoic fossil deposits of worldwide importance. Another person, who greatly influenced me, was my natural history teacher in the Ardennes. He was in charge of a local *Société d'Histoire Naturelle* (natural history society) and led excursions during the weekends to the classical outcrops of the Meuse valley.

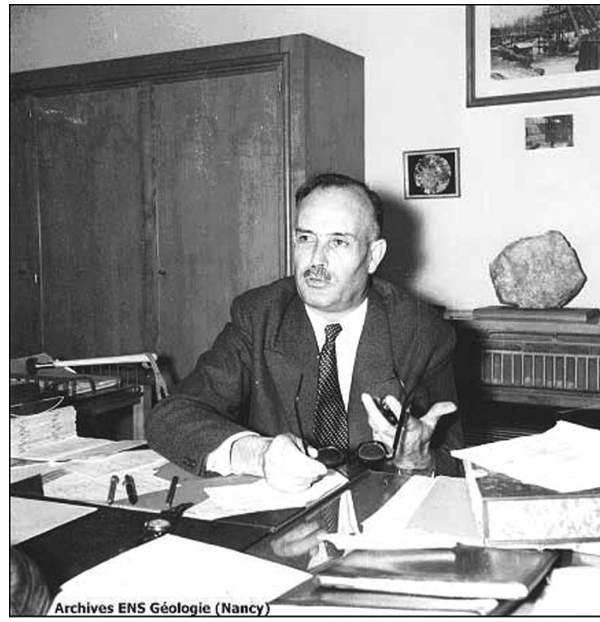

Figure 1. Marcel Roubault at his desk in the Ecole de Géologie, Nancy, c.a. 1960. (Archives Ecole de Géologie)

3. Ingénieur Géologue and Licencié es Sciences

I entered the Ecole de Géologie in the fall of 1955, with the equivalent of a bachelor in Sciences, without having had a single lesson in geology. The Ecole at this time was a part of the University of Nancy. Basic courses in mineralogy, petrology, stratigraphy or paleontology were offered. Applied geology and mining, from ore geology to mining and civil engineering, were exclusive to the Ecole. Marcel Roubault, who had made long expeditions in the deserts of Algeria during his early career, had even included courses in survival techniques and automobile mechanics. After three years, those who had passed the many exams successfully received two degrees, viz. *Ingénieur Géologue* (geological engineer) and *Licencié des Sciences*, roughly equivalent to a masters degree in the Anglo-American world. I got both degrees in June 1958 and, alone among all my classmates, decided to continue my career in the university. This means in fact that I gave up all advantages linked to my title of engineer. It was then a time of economical boom, the very beginning of the *glorious sixties*. The first traces of oil had just been discovered in the Sahara, and trained geologists were in great demand. I remember that, during my last year of study, I was proposed a *presalary* to work for an oil company in Gabon, exceeding by a factor of 3 what I would receive one year later as university assistant. But I wanted above all to do research in petrology. I developed my interest in this discipline not so much during the scarce courses given by

Marcel Roubault, who always found good reasons to cancel his lectures, but in the excellent courses offered in crystallography, mineralogy and ore geology, given notably by Joseph Bolfa (mineralogy), Raymond Kern (crystallography and crystal growth), and André Bernard (ore geology). An important aspect in the education of engineers was the long traineeships (up to about two months) in mines during the summer. I had a strong desire to discover the world outside my home country and managed to accomplish all these traineeships abroad. My first traineeship was in Germany in a coal mine near Saarbrucken in the vicinity of the famous Siegerland iron mines. Continuously operating since the Middle Age, these mines were approaching of their working life. However, they had preserved all the skill and expertise developed by generations of miners since Agricola. They closed one year later after my visit. The siderite ore, with a significant manganese content, occurs in almost vertical dykes, extending on 100-m length. The iron is said to have been used for Charlemagne sword. Miners drilled in the ceiling, above their heads, while filling progressively the empty space by rough, barren waste. Ore falling on the waste was extracted through a central chimney parallel to the direction of the dyke, with a diameter progressively increasing with depth. Miners were able to built those chimneys with a piece of rope and loose blocks of basalt under the sole light of their head lamps while standing in hazardous balance on the steep slope of the waste. The transport of waste and ore to the shaft or galleries over distances of several kilometer's was still partly done by horses. These, particularly fond of the tobacco given to them by the miners, were so much accustomed to the mines that they could find their way without any hesitation in total darkness.

The most important traineeship, essential for obtaining the title of engineer, occurred in the summer between the second and third year, for a period of more than 2 months between July and September, 1957. I had the good fortune to qualify for an international exchange program, which allowed me to work in the dressing plant of the Sydvaranger iron mines, Kirkenes, northern Norway. The work, which consisted mainly in the control and management of magnetic separators, was not too complicated. The ambiance, however, was unique. Destroyed during the war, the dressing plan had re-opened only 4 years before. It was still under construction by Norwegian workers coming from everywhere in the country, eager to enjoy long summer days after the long, dark Arctic winter. During my stay at the dressing plant, only one Norwegian engineer was present. All other people (about 10 in total) were students from all over the world. Later, I met the geologist of the mine, who told me that, if he had known that a geologist had been in the group, he would have been most delighted to have taken him as a field assistant. I enjoyed the work in the plant and the cheerful student atmosphere, but I had also understood that this was not something that I would like to do for the rest of my life. Traveling all along the Norwegian coast on the coastal steamer, I discovered the beauty of Norwegian landscapes and the unique freshness of outcrops, compared to the weathered Variscan rocks that I knew from France. I knew that if I wanted to do research in petrology, it would in this country.

4. Entering the university

Having completed my studies, it did not take much time to find a proper position. It was a period in which anyone with university degree could immediately find a job, either in university or in industry. Because of extreme differences in salary, geological engineers who decided to apply for university positions were extremely rare and, by consequence, received with open arms. It was not an organized plan, certainly not a request of the professors, accustomed to see engineers heading en masse into industry, but a simple coincidence, that 3 of the best students of the Ecole de Géologie took the same decision for 3 successive years: Bernard Poty in 1957, myself in 1958, and Alain Weisbrod in 1959. We all became assistants at the Ecole de Géologie or, for Bernard Poty, *stagiaire de recherches* with the Centre National de la Recherche Scientifique (CNRS), the difference being mainly a lesser amount of teaching duties. Most important was then to choose a PhD supervisor and a subject for a thesis, which at this time was the huge *Thèse d'Etat*, for many a work that occupied them during their entire life. This thesis was intended to prepare for a professorship, a position for which one was considered after at least 10 years of practice. It was common to work on his thesis for years, secured by a tenured position as university assistant.

My desire to work in Norway was facilitated by the fact that in 1955, Marcel Roubault had organized an international meeting in Nancy, intended to mark the triumph of his transformist ideas (Collectif, 1955). Great names had been invited from all over the world, first of all Tom F.W. Barth (Oslo) and C.E. Wegmann (Neuchâtel). Georges Millot, a close collaborator of Marcel Roubault, who had just left Nancy for Strasbourg, made the necessary contacts with Norway. He arranged that I could prepare a thesis there under the joint supervision of T.F.W. Barth and M. Roubault. I went for the first time from France to the Geologisk-Mineralogisk Museum during the early summer of 1958 on a Vespa. I discovered a place where the souvenirs of people like W.C. Brögger, P. Eskola, and V.M. Goldschmidt were to be found behind every door. The three months that I spent in Oslo had a decisive influence on my future career. Professor Barth rapidly saw that, whilst I had a solid background in physics, chemistry and mineralogy, my knowledge of petrology was very meager indeed. Marcel Roubault, first of all busy to run through ministries to get subsidies, had always good excuses to cancel his courses, which most of my fellow students found dull anyway and, frankly speaking, were quite uninteresting indeed. But I had good practice of the polarizing microscope, including the universal (Fedorov) stage which, at this time, was considered the ultimate in instrumentation. Professor Barth offered me a copy of his *Theoretical Petrology* (Barth, 1952), which became my bible and introduced me to the beauty of phase diagrams and magmatic differentiation. I also discovered the *three men book* (Barth et al., 1939) and had the privilege to share a room with K.S. Heier, who was working at this time on the chemical differences between amphibolite and granulite facies rocks. Shortly afterward, he was to become director of the Geological Survey of Norway. By the end of the summer, I could reasonably pretend to do research work at the international level. For my thesis, Professor Barth, who was particularly fond of southern Norway (he had a large house on the island of Flosta near Tvedestrand, Fig. 2), gave me the choice between two subjects: one more mineralogical, on Ødegårdens Verk apatites and scapolites (a mine formerly own by a French company and already studied by Alfred Lacroix), the other more petrological, on spectacular augen gneisses with feldspar phenocrysts up to 10 cm in size occurring in the region of Vegårshei. I chose the latter.

5. Discovering the Bamble province

My first contact with the field was during the summer of 1959. Topographical maps were rather poor, based mostly on German maps made before the war to prepare for the invasion of Norway, but I had war time, American aerial photographs, supplied by the Geological Survey of Norway. Geological maps were practically nonexistent. The only extant maps consisted of a few sketches drawn in the 30's by the only geologist to have been in the region, Arne Bugge. These were published in 1943 by his nephew, Jens A.W. Bugge. Interestingly, after the war Jens Bugge had published a paper on solid-state reactions (Bugge, 1945). Therefore, many people in Norway thought that I had been sent there to compare between Bugge's

Figure 2. Professor Barth on a granulite outcrop in Flosta near Tvedestrand (c.a. 1967). To his right, Ian Starmer (University College London). (Photo: J. Touret)

and Roubault's views. Needless to say, the latter had never even heard the name of Jens Bugge. Jens Bugge, however, was, a very agreeable and competent man, then professor in Trondheim, who showed me a number of key exposures along the Skagerrak coast.

The great achievement of Arne Bugge had been the discovery of a major shear zone, that he called *the Great Breccia* (now usually referred to as the Porsgrunn-Kristiansand shear zone), delimitating the granite-rich Telemark province, to the North, and the Bamble province, along the coast to the south (Fig. 3). He was a strong believer in Wegener's continental drift theories and imagined that the Great Breccia was caused by the collision between two independent continental blocs. A consequence would be that rocks occurring on both sides of the breccia should be completely different. Indeed, in Jens Bugge's sketches of Vegarshei, or other granitoids centered on the breccia, they are shown as half circles, limited to the North by the supposedly homogeneous Telemark granites. These granitoids had been interpreted by Tom Barth as metasomatically feldspathized (*petroblastesis*) products of the Great Breccia. I was supposed to substantiate this hypothesis in my thesis.

Figure 3. In a rowing boat in the middle of Vegar lake, summer of 1959. To the right, Olav Lintveit (Vegarshei), who had been Arne Bugge field assistant. "*From this place, told he, Arne Bugge had seen the collision between Bamble and Telemark*". (Photo: J. Touret)

It took me a few days in this, for me, totally unknown area, to realize that this hypothesis was untenable. The augen gneiss is in contact with the breccia in only one outcrop, only a few 10's meters in size, along the only major road existing in the region. The remainder of the augen gneiss departs significantly from the trace of the breccia, which itself is marked by a very long and deep lake. The augen gneiss ends in the form of a steep, massive circular mountain, Hovdefjell (= High Mountain), which was at this time extremely difficult to access. When, after many efforts, I finally reached the summit, I discovered a strong change in the color of the augen gneiss, namely the greenish to yellowish shade typical for granulite-facies rocks, well known along the coast in the vicinity of the towns of Tvedestrand and Arendal. Moreover, other augen gneiss occurrences, transitional between amphibolite and granulite facies, occurred within a few kilometers distance of Vegårshei, either to the south (Ubergsmoen) or, more surprisingly (at least in the then admitted views on the relations between Bamble and Telemark), to the north of the breccia (Gjerstad).

These discoveries drastically changed the scope of my research. It is fair to say that, by the late 50's, metasomatic granitisation theories had rapidly faded away. One participant of the 1955 Nancy meeting was O.F. Tuttle, who presented for the first time his (and N.L. Bowen's) experiments on the granite system. Their memoir, published two years later, definitively established the magmatic origin of granite, marking the end of the *soaks against pontiffs* battle (Young, 2003). The discovery of rapakiwi textures in the augen gneisses showed them to be deformed granites (orthogneiss), which then had a major importance in understanding the relation between Bamble and Telemarek provinces, as well as the amphibolite to granulite facies transition.

To solve these problems, I would have to map a huge area, from the coast to well within the very wide Telemark province. Surprisingly, this had never been done before. Norway is a vast country. Its southern Precambrian had become spontaneously divided between three different areas, independently studied under the leadership of three great personalities: Bamble (T.F.W. Barth, Oslo), Telemark (J.A. Dons, Oslo), and Rogaland (Paul Michot, Liège, Belgium). Major research interest had started near the center of each of these areas, (Arendal for Bamble; Egersund for Rogaland; Lifjell supracrustals for Telemark), to stop at the shear zones delineating each of these *provinces*. My proposal to link Bamble and Telemark was then well received, but when I proposed it in Nancy, I received a rather strange recommendation from my mentor, in theory still M. Roubault, namely that I should *ask the permission* of C.E. Wegmann, one of the main inspirers of the transformist school. This requires some words of explanation. Having brought to Scandinavia and Greenland the structural methods developed by Alpine geologists, Wegmann had been the strongest opponent of the magmatists during the so-called *granite controversy*. As such, he was considered as a kind of *guru* in Nancy. He was by then at the end of his career and married to a Danish wife. He had thought to make some work in Bamble as his last achievement (Fig. 4). He sent his assistant, Jean-Paul Schaer to Bamble, who did some beautiful structural work on the Bamble quartzites (Nidelva) that unfortunately remained largely unpublished because of his departure to Morocco. Jean-Paul Schaer introduced me to the regional geology but stated, on a personal order of M. Roubault: I will not give you the permission to continue your thesis work in this area if Professor Wegmann feels that it is not appropriate. I found out that I had to solve a diplomatic problem. I wrote a polite letter to Professor

Figure 4. C.E. Wegmann in the field near Risør, southern Norway, ca. 1969. (Photo: J.P. Schaer)

Figure 5. Bernard Poty (left) and Eric Fournier, son of the great guide Roger Fournier (who died in the mountains in 1976) who had guided Bernard to the most inaccessible clefts in the rock. Photo taken in 2009, in front of one of the best crystal cavities discovered by Eric Fournier in the Aiguille Verte. (Photo: B. Poty)

Wegmann and, few weeks later, received a very long answer in 15 hand-written pages, most of it a draft of a paper published later the same year under the title Das Erbe Werner's und Hutton's (Wegmann, 1958). The conclusion was clear. The region of Vegårshei was much too complicated. I would not understand anything. He (C.E. Wegmann) would strongly advise me not to work in this region. Instead, he would advise me to work in the Driva region in the Caledonides, where he did some time later organize a field school that attracted a number of French structural geologists. But I had already made my choice for Bamble and replied that, while thanking him warmly for his advice, I would not change my project. Surprisingly, the recommendation that C.E. Wegmann subsequently sent to M. Roubault was that he liked someone with firm ideas, and he gave me his *bona fide*. I maintained the best relations with him from that point on, such that I received several marks of interest and words of congratulations when I started to publish on the region.

Working on my thesis took about 10 years. The standard schedule was one to two months field work in southern Norway, a couple of weeks at the Geologisk Museum to discuss with Professor Barth and do some of his favorite analyses (feldspar thermometry). The rest of the time was spent in Nancy, teaching petrology and structural geology at the Ecole de Géologie. The research projects of my two colleagues were not too different from mine and we worked in close collaboration: Bernard Poty developed the techniques of fluid inclusion studies, notably through close contacts with the Russian School (firstly Georg Laemmlein, then Nikolay Ermakov in Moscow). With the help of the best mountain guides, he could collect idiomorphic crystals in the most inaccessible localities of the Mont-Blanc Massif (Fig. 5). Recently available on the Internet, his thesis (Poty, 1967) remains a basic reference for the study of Alpine quartz. Alain Weisbrod's thesis was on the metamorphic and magmatic rocks of the Southern Massif Central. We shared the same room at the ENSG and, thanks notably to Raymond Kern, discovered together the beauties of thermodynamic analysis applied to high-grade mineral assemblages. Alain switched later to high P and T experimentation, but was also an outstanding field geologist, with whom I learned much during the preparation of excursions or the many field courses that we organized for the students of the Ecole de Géologie (Fig. 6).

My early work in southern Norway was marked by an important event, which had a strong influence on my further work. I attended the Norden International Geological Congress in 1960, my first contact with international science. Professor Barth led a well-attended excursion in southern Norway, with a stop at Gjerstad in my field area. I could present my work before great authorities like K. Mehnert and H.G.F. Winkler. The contact with H.G.F. Winkler, notably, was extremely positive. I visited Göttingen several times during subsequent years, where I was exposed to all of the experimental work then being done by a generation of future leaders in the German geology, all of whom became close friends (e.g. W. Schreyer, E. Althaus, W. Johannes, B. Storre).

Except for two years (1963, 1964) that, like all young Frenchman from my generation, I had to spend in the army, these years were quite exciting. As first president of the IUGS (International Union of Geological Sciences), created during the 1960 International Geological Congress, Professor Barth initiated a vast Fullbright exchange program for young scientists, attracting to Norway a number of scientists who subsequently made bright careers in their respective countries (R. Morton, K. O'Nions, A. Sylvester, W. Elders, M.L. Crawford, to cite but a few). Furthermore, southern Norway proved to be a training field of exceptional interest. A number of European universities came every year to do some mapping, under the umbrella of the Norwegian Geological Survey. These included London and Nottingham along the coast (D. Field, P.C. Smalley, I. Starmer; Århus (Denmark) around Kristiansand (T. Falkum); Liège (J. Michot, D. Demaiffe) and Utrecht (A.C. Tobi, C. Maijer) in Rogaland. Norwegian geologists, such as W. Viik, T. Andersen, P. Hagelia, and O.A. Christophersen should also not be forgotten. However these geologists were usually more likely to be sent to the northern Norway than to its southern tip. Further north, the first conservator

Figure 6. Alain Weisbrod in the field in the Tessiner Alps, during the preparation of the 1974 NATO meeting. (Photo: B. Poty)

of the Geological Museum and T.F.W. Barth's close co-worker, Johannes A. Dons (Fig. 7), managed the vast Telemark project, in which geologists from all over the world were involved.

The experimental work done in Göttingen was crucial to understand the rocks I was investigating. It was demonstrated that migmatites are caused by partial melting, instead of solid-state reactions, under conditions strongly dependent on the H_2O partial pressure. This was also the case for the metamorphic isograds, which had been discovered in my working area, notably those marking the boundary of granulite facies. I have described in detail in my thesis the reasons, which led me to think that the transition from amphibolite to granulite facies was less caused by a temperature increase than by a lowering of the water partial pressure, possibly by the occurrence of another fluid, not recorded in the mineral assemblage. Some time before, B. Poty had shown me spectacular CO_2 inclusions in Itrongay (Madagascar) gem orthoclase, a region famous for its granulites (incidentally, quite similar to those in Bamble). I was also aware of the discovery by Ed Roedder (1965) of CO_2 inclusions in mantle xenoliths. Together with theoretical analysis of mineral equilibria phase diagrams, these were the links, which led me to speculate that CO_2 fluids, permeating the lower crust, could be responsible for granulite metamorphism. This is the model that I proposed in my thesis, defended in January 1969 (Touret, 1969). It does not contain a word on CO_2 inclusions in granulites, but indicates clearly the necessity to search for them. Bernard Poty told me that, during the thesis defense, M. Roubault found that I had spoken for too long (as usual) and asked me to stop abruptly, upon which Alain Weisbrod remarked You see Bernard, Jacques is just going to express its most important discovery and Roubault does not even see it.

During the following months, I hastily searched for a direct proof of this supposed granulite facies CO_2 fluid. Bernard Poty introduced me to microthermometry, notably on a stage that he had developed during his thesis work, shortening the operation time by several orders of magnitude. It took me several weeks to discover the first high density CO_2 inclusion in a granulite. After this I could rapidly see that they were present everywhere in the Bamble granulites. Thanks to B. Poty's stage, I could make hundreds of measurements during the summer of 1969, many more than Bernard could have done during the entire preparation of his thesis. Commercialized by a maker of instruments for breweries, the Chaixmeca stage was the first of a series (USGS, Linkam), which made fluid inclusion research possible in petrology.

Figure 7. The first conservator of the Oslo Geological Museum, Johannes A. Dons in Telemark, c.a. 1975. (Photo: N. Santarelli)

These first data were presented at a meeting on fluid inclusions organized in Basel by A. Stalder on Sept. 13–15, 1969, the first of what proved to be a long standing series (ECROFI, the name was coined somewhat later). The paper came out two years later (1971) in Lithos, the new journal that Professor Barth had initiated, replacing certain former local journals in the Scandinavian countries (Touret, 1971a,b). I had first written the manuscript in English, my communication language with Professor Barth, but he requested to have it in French. He thought that Lithos could attract a fair number of readers in French-speaking countries. But he also thought that my paper should reach a wide audience. After many discussions, we came on the idea to split the paper in two parts (mineralogical associations, fluid inclusions), giving all this information in the figures with extended captions in both French and English. I am not sure if my paper had any influence on the French-speaking audience, but the fact that it has been written in French has by no means hampered its diffusion. It has gotten over 250 citations till now, by far the most widely cited paper of all my publications.

6. Being a professor

Being a professor was then the norm after a successfull *Thèse d'Etat*. In Nancy, which was progressively becoming one of the most important centers of earth scientific research in France, the three of us could obtain such a position with different organizations: Bernard Poty at the CNRS, Alain Weisbrod in the Ecole de Géologie, and myself in the new Université de Nancy, just separated from the ENSG. Bernard went to Princeton for a couple of years with Dick Holland and he invited us to visit him during the summer of 1970. Our *Grand Tour* led us to visit a number of famous places and meet with great people, first of all the *pope* of fluid inclusions, Edwin Roedder. It resulted some years later in a longer stay in the USA for each of us: Alain at the Geophysical Laboratory of the Carnegie Institution (Washington DC) and myself at Yale with Phil Orville. Contacts established during these occasions lasted for years, notably through research conferences (first of all the invaluable Gordon conferences) or friendly relations with some great names of modern Earth Sciences (Volkmar Trommsdorff, Hel Helgeson, Jim Thompson Jr., Alan Thompson, Doug Rumble, Greg Anderson, and many others). They were continued through specialized meetings combined with field excursions, which had a great success in the 1970's and 1980's. The first of these was the 1974 *Volatiles in Metamorphism*, beginning in Nancy and ending in Zürich, with a number of working sessions and field excursions along a complete traverse across the Western Alps (Fig. 6). Years after, this meeting is remembered with nostalgia by the shrinking group of former participants. It became a model for a number of meetings, which occurred repeatedly during the following 15 years, sponsored by an organization (NATO) which had understood that science was a better way to ensure progress than military threats. One of the last meetings along this format was organized in Norway in 1984 (Fig. 8) (Tobi and Touret, 1985), starting a series of successive meetings in the following years which, all together, give the best overview of the granulite problem. Unfortunately, this promising line of research stopped abruptly when NATO decided to have other priorities.

As a professor, one is free to define his own line of research. These diverged within our *trio*, even if we maintained close contacts for years and were dubbed as the *three musketeers* by some of our friends. Bernard Poty went into applied (ore) geology, founding with great success the CREGU (Research center for the geology of uranium, now Georesources). Alain Weisbrod specialized in experimentation and thermodynamics, while I continued to work on fluids in granulites. Analytical possibilities were drastically

Figure 8. In the forest of Akademgorodok near Novosibirsk, summer of 1977. From left to right: Jacques Touret, Yuri A. Dolgov, N. Shugurova, Roberto Clochiatti (Orsay, France), Anatoly Tomilenko. (Photo: J. Touret)

improved through the explosion of microanalytical techniques, notably micro Raman spectroscopy. Like Bernard, I also made a pilgrimage to Russia (then the Soviet Union), as the guest of Professor Yuri Alexandrovitch Dolgov in Novosibirsk (Fig. 9). It was in this place that fluid inclusions had been analyzed for the first time in high-grade metamorphic rocks (Dolgov et al., 1967), using sophisticated microanalytical procedures, which somewhat unfortunately (all techniques have their plus and minus) were abandoned when micro-Raman spectroscopy was developed. After few years in Paris, my final academic position in Amsterdam gave me the possibility to build up a laboratory entirely devoted to the study of fluid inclusions in rocks. As I have repeated in virtually all my publications, inclusion data can only be interpreted in the light of mineral P-T estimates, not only in the same rocks, but in the same crystals, on the scale of a few micrometers. An apparently trivial, but in fact major problem is to be able to identify the same fluid inclusions utilizing a succession of different instruments. This is not easy if these instruments are in different locations (or, as commonly in France, in different universities). In Amsterdam, the polarizing light microscope, the electron microprobe, and the micro Raman were located in adjacent rooms, which was an ideal situation for studying these very small objects. It is there that, together with a limited, but enthusiastic group of co-workers, consisting either of students or visitors, coming from all over the world, that we did most of our work on granulite fluids. The fact that the granulites did contain special types of inclusions, both CO_2 as well as highly saline aqueous solutions (brines), was never contradicted, but their significance highly debated. The fluid-assisted model of granulite dehydration was endorsed with enthusiasm by some researchers (first of all R.C. Newton at Chicago, now UCLA), but not by a number of experimentalists or isotope geochemists, who preferred to follow W.S. Fyfe or A.J. Thompson and their concept of fluid-absent processes. The granulite controversy was born, recalling by some aspects the former granite controversy between magmatists and transformists in the 1950's. I have discussed elsewhere some aspects of this controversy, which is not entirely over (Touret, 2009). Sufficient here will be to say that if discussions were sometimes quite animated, they had never the aggressiveness or personal character which had been so obvious in the *soaks against pontiffs* debate. This was possibly a question of generation or, more probably, the personal relations that were developed during field meetings like the first one in 1974, continued by the successive NATO-sponsored meetings which occurred 10 years later. I left Nancy in 1972, to spend few years in Paris and finally found my definitive place in Amsterdam in 1980, until my official retirement about 10 years ago. I never ceased -and still continue- to work on fluids in granulites. Progresses realized during these last 30 years are impressive, not only on the characterization of fluids in inclusions and understanding of fluid systems at high P and T, but also on many other domains, essential for the interpretation of fluid inclusion data, e.g. experimentation (mineral solubilities, R.C. Newton and C. Manning at UCLA), mechanisms of mineral growth in metamorphic rocks (A. Putnis and his group in Münster), massive use of thermodynamics to estimate P, T, fluid fugacities of mineral equilibration, isotope geochemistry to trace the fluid origin, etc. As far as granulites are concerned, I can say that the controversy has been very positive, forcing each camp to refine his arguments and develop its reasoning. A spectacular result is the return of

Figure 9. Preparing the NATO-sponsored meeting in southern Norway (Tobi and Touret, 1985). From left to right: H. Zeck (Copenhagen), J.B.H. Jansen and C. Maijer (Utrecht), J.L.R. Touret (Amsterdam), A.C. Tobi (Utrecht). (Photo: J. Touret)

metasomatism, virtually absent in vapor-absent models, now recognized to be of major importance in many fields of geology (Harlov and Austrheim, 2013). This metasomatism, however, is a fluid-assisted process (percolation metasomatism), not the solid state diffusion previously invoked by the transformists. It is somewhat ironic to think that much theoretical background was already present at the peak of the granite controversy. The theory behind metasomatism had been established by D. Korjinski in the early 1950's. Many unnecessary discussions and quarrels could have been avoided, if rocks had been seen as they are not in the light of dogmatic preconceived ideas.

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