Fred Mumpton retires as Editor-in-chief
Towe takes over for one year

Fred Mumpton gives the Editor’s report to the CMS Council meeting in Jackson, MS, October, 1986. Flanked by D. Pevey and N. G. Owen.

Sacramento surplus
Jim Post, chairman of the 1989 Annual Meeting held in Sacramento, is happy to inform the membership that $2462.00, the annual meeting surplus, has been deposited in the Society’s endowment funds, which are used for scholarships, awards, and plenary lectures.

Happy Birthday!

K. M. Towe, Treasurer of the CMS, is a Research Geologist at the Smithsonian. He has five years’ experience editing The Journal of Foraminiferal Research, published by Allen Press.

Manuscripts for Clays and Clay Minerals should be submitted to Dr. Kenneth M. Towe, Department of Paleobiology, Smithsonian Institution, Washington, D. C. 20560. Towe can be reached by telephone at (202) 357-2406.

Inside ...
Interview: S. W. Bailey
Shell Oil Group by Jack Burst
Walter Keller Birthday Celebration
Pakistani Clay Studies
Commentary by Linus Pauling

Society Office moves
The CMS Office has moved from Evergreen to Boulder, Colorado. The new address is P.O. Box 12210, Boulder, CO 80303. The new telephone is (303) 444-6405.

Professor Walter D. Keller, CMS Treasurer
Letters

Clay samples needed

Editor:

I am writing to ask the members of the Society for help. I am in the process of searching for kaolinite samples, either pure or containing contaminants that can be removed physically or chemically, to complete a research project on oxygen isotope fractionation in clays. More specifically, I am looking for relatively well-characterized natural kaolinites of weathering, diagenetic, and hydrothermal origin for which the temperature of formation is known or somewhat constrained.

As of today, my search has not been very successful. Most of the samples I have collected or have had donated contain unremovable impurities and, when not of weathering origin, have poorly constrained temperatures of formation.

I would be very grateful to anyone who could provide me with adequate samples or with any information on how to obtain such samples. I only need 100 to 200 mg of pure kaolinite and would perform the purification treatments if necessary. You can contact me at the Dept. of Geological Sciences, Case Western Reserve University, Cleveland, Ohio 44106.

Tel: (216)368-6523; Fax: (216)368-3016; Binnet GIRARD@CWRU.

Your help is much appreciated.

Jean-Pierre Girard
Cleveland, Ohio

Conference Down Under

Editor:

A recent issue of CMS News included a report on the Ninth International Clay Conference in Strasbourg last year. As a non-participant, I enjoyed this report, but as Technical Program Chairman for the forthcoming Tenth International Conference, I also feel bound to point out an important geographical error in the report regarding the location of this next conference. The Tenth International Conference will be held from July 18-25, 1993, in Australia, but in Adelaide, not Melbourne, as the report in the newsletter stated.

And the 700 km between the two cities make quite a difference. No doubt Melbourne has its big city attractions, but, as a recent newcomer to Adelaide, I would like all members of The Clay Minerals Society to know of the charms of this highly cultured city. Did you know that William and Lawrence Bragg, the X-ray diffraction forebears of clay mineralogy, came from Adelaide? Adelaide is a well-planned, attractive city which allows easy access to all of its parts and which basks in a pleasant, Mediterranean climate.

National (encompassing all Australian states and New Zealand) and also local organizing committees are busy

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Thanks...

To the following people who contributed to this issue:

S. W. Bailey
John F. Burst
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Linus Pauling
J. L. Post
Brij L. Sawhney
Don Scafe
Rodney Tettenhorst
James F. Westcott
Art White
Gene Whitney
M. J. Wilson
Susan Wintsch

Many thanks to our advertisers this month, International Centre for Diffraction Data and Radix Instruments for helping make the newsletter possible.
Pakistan clay studies undeterred by bandits

The Islamic Republic of Pakistan includes some of the most dynamic geologic terrain in the world. The ongoing collision between the Indian Plate and the Asian Plate, combined with harsh climate and sparse vegetation, has produced a veritable museum of geologic processes and materials within an area slightly smaller than the combined areas of California, Nevada and Utah. It is a trip of 1000 miles horizontally, and 28,250 feet vertically, from the Arabian Sea coast in the south to the summit of Godwin Austen (K2) in the Karakoram Range in the far north, and the geologic panorama along the way is breathtaking. Paleozoic salt, Jurassic paleosols, Recent molasse sediments, and ophiolitic ultramafics have been crumpled into a wall of mountains surrounding the flat and fertile Punjab Plain. Over 100,000,000 people work and live in the mountains, on the plains, and along the majestic Indus River.

The geologic variety and opportunity for study are almost endless, but the joy of studying in Pakistan is diminished by the working conditions. Aside from the normal health concerns associated with working in the remote areas of the world, there are special problems in trying to do science there. Imagine trying to maintain an electron microscope where the electricity goes off for several hours each day. Imagine trying to pursue a new avenue of research when few books, maps or journals are available. Imagine doing field work where you may be kidnapped or shot at by bandits.

In order to help the Pakistan government in their geologic studies, the U.S. Geological Survey and the Geological Survey of Pakistan have engaged in a cooperative program to study the geology of Pakistan. This program includes the study of clays, and Gene Whitney of the U.S.G.S. has made two trips to Pakistan to help provide training and to help establish analytical facilities for clay mineral studies there. Working closely with counterparts in the Geological Survey of Pakistan (G.S.P.) and the Hydrocarbon Development Institute of Pakistan (H.D.I.P.), Whitney's clay studies in Pakistan have three primary goals. First, Pakistani geologists are being trained (both in Pakistan and in the U.S.) in the principles and techniques of clay mineral identification and clay petrology. Second, specific clay studies have been started in Pakistan to understand the geology and geochemistry of various types of clay deposits, and to assess the economic value of clay deposits. These studies include bentonites, laterites, underclays, and shales in the Salt Range and in Balochistan. Third, Pakistani laboratory facilities are being upgraded (funding provided by the U.S. Agency for International Development) under the guidance of U.S.G.S. personnel to provide Pakistani scientists with the facilities to pursue sophisticated geologic studies independently. Both G.S.P. and H.D.I.P. have recently purchased new X-ray diffractometers for clay studies. Unfortunately, service and technical support for these instruments are expensive and difficult to obtain in Pakistan. Service personnel sometimes must travel from Singapore, Japan, or Europe to make repairs. A service call may require several weeks and cost several thousand dollars. In addition, regional electrical supply is frequently so unreliable that even the smallest computer requires an uninterruptible power supply. Otherwise, valuable data may be lost and the computer hardware damaged during one of the frequent power outages.

Gene Whitney in Pakistan with the necessary field gear.

Clearly, maintaining sophisticated analytical instruments in that environment is a challenge. U.S.G.S. advisors are also helping G.S.P. to upgrade their infrastructure (electrical, plumbing, air-conditioning, etc.), and have made the improvement of publication and library facilities a top priority.

During a recent stay in Pakistan, Whitney and his Pakistani counterpart sampled laterites and bentonites from the Kala Chitta Range, west of Rawalpindi, and collected samples from a coal-bearing shale section in Balochistan. The Balochistan work was complicated by bandit activity in the study area. The government would allow foreigners to enter the area only if accompanied by armed guards. Thus, the field party included extra "assistants" carrying machine guns. Some G.S.P. geologists tell of their experiences of being kidnapped or shot at while doing field work, and these exploits clearly add a new dimension to the challenge of scientific work in Pakistan. Nevertheless, many Pakistani geologists are doing excellent work under these less-than-ideal conditions. As the facilities are improved and additional training and resources are provided, the quantity and quality of geologic work done by Pakistani geologists in Pakistan, including clay mineralogy, will improve.
Interviews with the clay scientists

S. W. Bailey

The interview was conducted in March 1990 in Chicago by Stephen Guggenheim, formerly a student of Bailey, now a professor at the University of Illinois-Chicago Circle. Dr. Bailey responded to additional questions by mail.

CMS: Let's start with some early history. You worked with W. H. Taylor as your Ph. D. thesis advisor at the Cavendish lab at the University of Cambridge. I was wondering if you had an opportunity to work directly with any of the Braggs.

BAILEY: Well, yes. As a matter of fact, Sir Lawrence Bragg was my official thesis advisor. This came about because I went over there on a Fulbright Fellowship. That was the first year Fulbrights went to England. Sir Lawrence elected to be my advisor, because I was the first Fulbright scholar to study at the Cavendish. In fact, he was head of the entire Cavendish, so he didn't have much time for personal advising. Although Bragg was my official advisor, W. H. Taylor did the actual thesis advice. Even Taylor didn't give much advice, perhaps once a week to see how I was doing. Most of the instruction in how to do single crystal X-ray crystallography was done on the buddy system with your fellow graduate students; in my case, it was J. V. Smith, who was my office mate. But I did know W. L. Bragg rather well.

CMS: Were you ever invited to his house?

BAILEY: Oh, yes, several times. He was a very kindly, friendly individual, and he would have six or so of the foreign graduate students over to his house for supper. His wife, Lady Bragg, was the mayor of Cambridge at that time, which was largely a ceremonial post. By the fireside he would tell us some of the points of his early history, such as working out the structure of beryl in half an hour; and how he worked out the details of the ionic bond. The chemists of the time begged him to reexamine his data for NaCl to show that, perhaps, one sodium really was closer to one of the chlorines so that there was a conventional chemical bond between them, rather than an ionic bond with six closest neighbors. I recall also that he was one of the few faculty members who owned a car at that time. In rainy weather he would ferry us home after supper—despite the strict rationing of gas (petrol over there).

CMS: I think Bragg was working on, as I recollect from reading old papers, soap bubbles during that period.

BAILEY: Yes. In order to get my degree from Cambridge, I had to learn a lot of physics, because the degree was in physics, and one of the courses I took was in physical optics, given by Bragg. One of the things he liked to demonstrate was to show the soap bubble model to illustrate close packing and deformity or dislocations, and so forth...really a nice demonstration.

CMS: Before you went to the Cavendish, you were an undergraduate at the University of Wisconsin at Madison. That was in the late 1940's?

BAILEY: Oh, thank you! I go back a decade before then.

CMS: Okay! My question actually was planned to lead to another: what instructor did you have at U.W. Madison who may have suggested that perhaps crystallography was the thing to do?

BAILEY: Well, I had as my official advisor, when I was an undergraduate, A. N. Winchell. He gave the lectures for the course in mineralogy, and R. C. (Con) Emmons was the laboratory instructor. That's the course that really got me interested in minerals. Before then, I didn't know anything about minerals or rocks. Winchell and W. Mead had purchased a General Electric X-ray diffraction unit... continued on next page
Bailey, continued

back in 1925. Winchell was in charge of the X-ray laboratory which was operated primarily by graduate students. Because of my interest in minerals, and because he was my advisor, he assigned me, as an undergraduate, to help out in the X-ray lab. I prepared samples, learned how to do powder X-ray diffraction and Laue photographs, how to do identify powder photographs, and so forth. That was as a junior. As a senior, which was in the year 1940-1941, I found myself in charge of the X-ray lab because the graduate student previously in charge had made a hasty exit for personal reasons that I will not go into.

And I knew at that time, because of my work in the X-ray lab and because of the reading I had to do to keep on top of the job when I was in charge of the lab, that I wanted to do X-ray work. But it was to be eight years before I got a chance to do anything along those lines. In the 1941-42 school year (that was my first year of graduate study at Wisconsin), I was planning to do something with X-rays and minerals. Unfortunately, World War II came along, and I spent four years in the U.S. Navy instead. After the war, in the fall of 46, I came back to Madison, hoping to do some X-ray work, but I found that Prof. Winchell was retired, the former X-ray lab was now occupied by the junior's staff, and the X-ray unit had been thrown out as being obsolete. No one was doing any work with X-rays, so I did a Master's thesis on liquid inclusions and started to do a Ph.D. degree in economic geology under Gene Cameron. But I still wanted to do X-ray work; so one day I asked Gene Cameron whether the department had any plans to buy a new X-ray unit, to get a faculty member to have charge of it, and to encourage research in that field. He didn't say much, but one week later I found myself called into the chairman's office. The chairman was Con Emmons. He made me a proposition. It turned out that they had been looking for a faculty member in X-ray crystallography since the end of the war, but hadn't been able to find anybody that they liked or felt was suitable to them. Being reminded of my interest, he said that if you will go over to England and study with W. L. Bragg and W. H. Taylor and get this field, we will guarantee you a faculty position when you come back. So that was how I got into X-ray work.

CMS: Times have changed.

BAILEY: Yes, yes... I never had to interview for a job. That was very nice.

CMS: During the years, you've known some of the great clay mineralogists, like Brindley, and others, and I was wondering if you have any recollections about your meeting with Brindley, perhaps for the first time, or in the years since, that you can tell us.

BAILEY: Well, I met George Brindley for the first time at the Cavendish Lab. George was a professor at Leeds University at that time. But Keith Robinson, who had worked with Brindley on structures of kaolinite, halloysite and so forth, had come down to the Cavendish as a graduate student, switching fields to the structures of metal. But George was still Keith's external advisor. So for purposes of consulting, and eventually for defense of thesis,

By the fireside, Bragg would tell us some of the points of his early history, such as working out the structure of beryl in half an hour; and how he worked out the details of the ionic bond.

George would come down to the Cavendish. I met him for the first time then. Later on, when I went back to the U.S. in 1951, M. L. Jackson took me to the second Clay Minerals Conference in Columbia at the University of Missouri. George Brindley was there, so I met up with him again. Over the years we had a rather close working relationship, primarily over the phone and through conversation, because we were quite far apart geographically. But especially in matters of nomenclature, we worked together.

CMS: But I don't think you ever published a paper with him.

BAILEY: Only in nomenclature; but, we did correspond a great deal, and we reviewed each other's papers.

CMS: You interacted with M. L. Jackson almost the first year that you arrived back in Madison.

BAILEY: Even before then. When I was still an undergraduate, Jackson had charge of the X-ray diffraction unit in soils. When I was thinking about whether to go into the field of X-ray work and to go over to England, I went to Soils and talked with M. L. to see whether he would recommend that I do this. He encouraged me, and when I came back, he encouraged me further, and also started to get me interested in clay mineralogy. I had done a thesis on feldspars at Cambridge, and was continuing to do research on feldspars back in Madison. But Stan Tyler, who was chairman of the Geology Department at the time that I got back from Cambridge, got me into a cooperative study with him on clay minerals associated with the Lake

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Bailey, continued

Superior iron-ores. This really got me interested in clay minerals, because I immediately came upon powder patterns of some aluminum-rich serpentines and chlorites that simply were not in the literature. This got me interested in the structures, in the polytypism, in what it was about these structures that could cause different powder patterns, and so forth. M. L. Jackson encouraged me to continue along these lines, pointing out that someone who is an X-ray crystallographer really has a lot to contribute to clay mineralogy, because very few clay mineralogists knew much about structures, single-crystal methods, and the study of phyllosilicates. I think Jackson, Brindley, and Bill Bradley were the three people who probably were most influential in switching me from feldspars to clay minerals. I had met Bill Bradley at Cambridge also. He came over to some meeting and made a tour of the Cavendish. I was assigned to be his guide on the tour. Since I knew that I was coming back to Wisconsin, and he was at the Illinois Survey, we knew that we would be close neighbors. So after I did come back to Madison, we kept in close contact with one another.

CMS: I think one of your greatest contributions was working out the polytypism of the serpentines and chlorites, and straightening up all that nomenclature with regard to how polytypes seem to work. Interestingly enough, however, the mica polytypes, which are the simplest, were worked out by your old office mate about five years after you first became involved in looking at layer silicates. How did that come about? Why was J. V. Smith involved in that?

BAILEY: Well, I think Joe Smith was involved because of his cooperation with Hat Yoder. Hat Yoder had done, with Hans Eugster, synthetic preparation of muscovites and phlogopites and so forth, and had found different polytypes. He called on Joe Smith to do some single-crystall work of natural specimens. In so doing, they had to work out the possible stacking sequences to know what the theoretical polytypes were like, and whether or not these conformed with natural ones.

CMS: How instrumental was that paper in the development of your career?

BAILEY: Oh, very much so, because that paper came out in 1956. Before then, as I mentioned previously, I had found powder patterns of aluminous serpentines and chlorites that did not fit anything in the literature, and I knew that these had to be polytypes. I was not a clay mineralist, of course. I'd never had a course in it, and I learned about it by reading George Brindley's first edition of the X-ray Identification of Clay Minerals, put out by the British Mineralogical Society. But when Smith's publication came out, I immediately started to do the same thing with the serpentines and chlorites. In 1957 I derived all of the theoretical standard poytypes of serpentines, the twelve whose periodicities run from one to six layers, although I didn't publish that until 15 years later, primarily because the computer facilities at Wisconsin in 1957 were not adequate to calculate what the powder pattern should be for a six-layer polytype. I wanted to do it right. Although I taught these things for 15 years, I didn't publish them. After that, in the early sixties, I started on the chlorites which were even more interesting than the serpentines because of their complexity.

CMS: I thought that your best work was the polytype derivations. What do you think is your best work?

BAILEY: I think that's correct. It's certainly the work that gave me the most satisfaction because I enjoy the geometrical aspects of it—stacking these layers on top of one another, seeing if they are different from one another, and calculating their diffraction patterns. The greatest satisfaction, I suppose, comes when you've made a comparison of something that exists in nature with one of your theoretical polytypes, and can show that these things do exist, and that they have some significance. As it turns out, at least in chlorites, there's some sensitivity of the different stacking sequences to temperatures of formation.

CMS: I think the most fundamental part of that work was realizing how hydrogen bonds must link up, because that's what determines the polytype arrangements, at least in the serpentines and the chlorites. That probably had to be the first thing you recognized in making a polytype.

BAILEY: Oh yes, but that wasn't original with me. That was known beforehand because of the work of people like Hendricks, Brindley, and McMurchy, who was one of John Gruner's students.

CMS: So you had that down as an idea that you had to follow. continued on next page
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BAILEY: Yes. If one did not have that as a limiting criterion, then the number of polytypes would be infinite.

CMS: Well let’s move along into more recent times. In particular, you’ve just retired, and Wisconsin has hired a faculty replacement for you. Your replacement is not an X-ray crystallographer, and I was wondering if you see that X-ray crystallography is perhaps perceived as in decline. How do you feel about the future of crystallography and perhaps clay mineralogy as well?

BAILEY: First, let me say that my replacement (Jill Banfield) is primarily interested in high resolution TEM research, which is a nice tool for studying clays as well as other materials. She has already studied the weathering of granite to clays with Tony Eggleton at Adelaide. She is a member of The Clay Minerals Society, and she is interested also in learning X-ray crystallography. So you may see us involved in some collaborative work in the future. Getting back to the point of your question, I do think X-ray crystallography is in a decline, and I think the reason for this is that it has become so easy. Since the probability methods of determining crystal structures were worked out, we now have software that allows essentially a novice to come in, collect data and determine the structure, even for an unknown structure, rather readily. You no longer need the skilled personnel that you had to have before that. There is still a great deal of crystal structure work going on in chemistry, especially with organics, but not so much in mineralogy. I suppose that would extend to phyllosilicates as well, simply because the key structures have already been done and refined. There aren’t that many new ones that offer challenges.

CMS: Does that speak against the need for a crystallographer in a geology department? Or does this imply that if it is so easy, and people don’t have the background, then they may fall into traps unsuspectedly? Perhaps you’re implying that crystallography shouldn’t be in the decline?

BAILEY: Well, I think there is still need, if not for someone who is trained as a crystallographer, then at least for someone who has a knowledge of how to work with single crystals using X-ray techniques. They may apply it to petrology instead of to structural refinement; but I think there is still a need. You have to have an X-ray background, a structural background, to deal with crystal chemistry, which is still a very important part of mineralogy and of clay mineralogy.

CMS: Where does clay mineralogy fit into all this? What kind of background do you think you need to understand the crystal structure of clay minerals?

BAILEY: Well, I should emphasize that I don’t consider myself a true clay mineralogist. I’m an X-ray crystallographer by training. My interest in clay minerals, of course, is in the structures and in the crystal chemistry. It helps a great deal for the crystal chemistry of clay minerals to have a knowledge of X-ray crystallography. Then it’s easy to understand clay structures.

CMS: Why is it that clay mineralogy hasn’t made inroads into those universities, such as the Ivy League universities, that people seem to think are the better universities? Why is it that Harvard or some of the other major schools don’t have a clay mineralogist?

BAILEY: Well, Harvard has a clay mineralogist in the same sense that Wisconsin had me as a clay mineralogist. They have Charlie Burnham, who is an X-ray crystallographer but is interested in structures of micas. He doesn’t go over to the other clay minerals as I have done, but he does have that interest. Why don’t we have more of both X-ray crystallographers and clay mineralogists in some of the big universities? If I can backtrack a bit, you asked why my replacement is not an X-ray crystallographer or a clay mineralogist. We did interview X-ray crystallographers. In fact we made an offer to one, but he turned us down. We couldn’t find an X-ray crystallographer who was doing research on minerals that were of interest to the others on the faculty. The available people were more often doing material science, and so forth, which didn’t interest our staff members. In terms of clay mineralogy, I think clay mineralogy is still a very vital field. You only have to open the covers of *Clays and Clay Minerals* to see that there are articles there on all the different aspects of clays, not only soils and mineralogy, but chemistry, material science, physics, ceramics and so forth. The interest in mineralogy or geology perhaps has declined somewhat, but I think some of that may be cyclical. A lot of the em-

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Bailey, continued

Employment has to do with the petroleum industry making use of the conversion of smectite to illite. Because oil is so cheap and the oil companies aren’t making as much money, they aren’t hiring the clay mineralogists. I think this is a cyclical pattern, and I think it’s starting back up already. Studies in other areas, such as interstratification and intercalation of different things between the layers, is very strong. Pillared clays, for example, have great applications. It is up to the clay mineralogists to make some of the initial research into clay minerals and to show industry and various employers what the applications can be.

There are a number of areas within clay mineralogy where there is the potential for future development, and one of these areas is chlorite which potentially, I believe, has a lot to offer. The chlorites are structurally and chemically very complicated, but that in turn means that there can be a lot of information stored within chlorites that may be trying to tell us something about the conditions of crystallization and so forth. People have not tried very hard to relate chlorites to the environment. The only person that comes to mind is Cathelineau of France, who has tried to make a geothermometer out of chlorite. Potentially there is a lot of information there.

CMS: So for someone going into crystallography today: you are suggesting that they have to start applying crystallography, rather than just taking the theoretical approaches that you’ve done in the past.

BAILEY: Yes, I think that’s true. My interest was in some of the geometrical aspects of polytypism, in the

It is up to the clay mineralogists to make some of the initial research into clay minerals and to show industry and various employers what the applications can be.

careful refinement of key structures for which one could get single crystals, in looking at things such as order and disorder of cations (this, of course, would be long-range order), and in trying to relate some of the structural variations to see what their importance is in terms of the crystal chemistry. I think today many of the plums have already been picked in that area. If I were to start today, I think I would go into something like electrostatic energy calculations. There are a lot of structural problems that are not amenable to solutions by conventional crystallographic techniques because you can’t get good crystals. So there are lots of things for which we don’t know the answers. Some things that come to mind are why are dickite and nacrite high-temperature forms? As you may recall, nobody has been able to make these in the laboratory on a systematic basis. If they get dickite, it’s by accident, and they can’t repeat the work. What are the relative energies and stabilities of all of these different polytypes and those of serpentines and chlorites? Again, the latter can’t be made reproducibly in the laboratory. Therefore we don’t have experimental evidence on stabilities and energies. But I think the calculations of the sort that have been done by Dave Bish, Jeff Post, Rick Abbot, and Charlie Burnham may have a lot to offer this area. Also, I think more and more we’re learning the importance of short-range order, as opposed to long-range order. NMR work is telling us that we do have these submicroscopic domains, that may be different from one another, in which we have short-range order. I think we’re really waiting for some additional tool here, something that will enable us to look at short-range order in an easier fashion. Right now, with NMR, there is a lot of mathematical calculations you have to go through; but I suspect the way the science is developing that we will get some tool pretty soon that will enable us to do that more easily.

CMS: You recently wrote a chapter in a book about clay minerals and the origin of life. It seems to me that electrostatic calculations and similar concepts could go hand in hand with that. Do you feel that the origin of life is connected to clay minerals in an obscure sort of way?

BAILEY: I have no idea. I contributed that chapter simply because I was asked to by Graham Cairns-Smith, who is one of the editors of that book, just to give the readers a background on the structure of clay minerals. I really have no idea whether or not it’s related to the origin of life, but it is an interesting pursuit.

CMS: You were talking about the future development of clay mineralogy. You recently went to China to discuss at various Chinese institutions how they should set up their programs. Could you give us some information about what they wanted to know about setting up programs, and some of the suggestions you made to them.

BAILEY: Well, I’ve been to China twice, first to Nanjing University about four years ago. Then in October of 1989, I went for a month to the China University of Geosciences which is at Wuhan. This is a very large geological institute, in that it is entirely devoted to the geosciences. They have about 500 students, both undergraduate and graduate, and I was invited to make a critical inspection of what they call the Test Center there, where they have concentrated all of the high-technology expe-
Bailey, continued

sive instruments that are used for various sorts of testing of minerals. This includes X-ray crystallography and clay mineralogy. So I stayed there for a month and gave a series of lectures. I was sent by the National Academy of Sciences, and one of my duties was to give advice on curriculum, programs of research, and so forth. I found at this particular institute that they had a very good program of research in clay mineralogy already established. They had a professor in charge who was very competent. The only problems are that he does not have all the facilities he needs, and that he has been cut off from the rest of the world for many years. Communication with the outside

If I were to start today, I think I would go into something like electrostatic energy calculations.

world and with modern techniques is not as good as it might be, but I think they are doing some good science there. They have been provided with modern equipment by the World Bank, but they have problems in making the best use of the equipment and in service and repairs.

CMS: How about clay science and layer silicate mineralogy in other parts of the world? Do you find that the Europeans working in this area are following parallel approaches to those in the U.S., or are they going their own way and developing uniquely?

BAILEY: Well, in general I think that the developed countries are doing much of the same work that we are doing in the U.S. The less developed countries are doing more application. They're interested in those particular projects that have economic advantage to their country. In terms of the science itself, I think it is very uneven. In many countries I've found clay scientists who are really top-notch. There also are others, especially those that are interested just in application, where the science is not so great. There are a lot of these in lesser developed countries. It certainly used to be the case in Russia, although Russia also has top-notch clay scientists, people like Drits, Zvyagin, and Bookin.

CMS: Should the U.S. try to encourage applications rather than just basic science, as they seem to be encouraging now at the NSF? Do you think that the Federal government should get involved in directing, at least in a general way, research in the sciences?

BAILEY: Personally, I don’t think that they should. I would like to see the federal government more involved with basic research, rather than with applied research. I would like to see the applied end done by private companies. But, of course, application is a tremendous driving force. If companies can see something in clay mineralogy that will give them an economic benefit, then they’re going to go after it. I think clay mineralogists who are doing basic research are the ones who can point out what areas will have economic significance in terms of application.

CMS: It’s my experience that private companies tend not to stress even applied research, unless it’s done in-house. Are you suggesting perhaps that private companies should expand beyond that and develop greater interaction with universities?

BAILEY: I would like to see more grants by private companies to scientists at universities. I think this would be very helpful on both ends. At private companies they would get a better appreciation of basic research and what this has to offer, and also the scientists at universities could perhaps appreciate more what the companies are interested in from the application point of view.

CMS: Sounds great, but why isn’t this happening now?

BAILEY: Well, it is to a certain extent. But I think you and I, who are in the geological end, are biased because we’re on one side of the spectrum. We’re affected by the application to petroleum, things of that sort, which are down right now. People who are doing clay mineralogy that involves application in chemistry, material science, ceramics, and so forth, are doing just fine. I don’t think they are in a downturn.

CMS: It’s interesting that you bring that up, because, for example, DuPont has a policy of not giving grants to geology departments, although they commonly give grants to chemistry departments. Why do you suppose a company of that stature and size would neglect geology departments? Is there something inherent in the system, or is it just DuPont’s problem?

BAILEY: I have no idea, but I think my impression of DuPont is that it is primarily a company devoted to chemistry and chemical engineering. Therefore, the directors of research don’t tend to think of mineralogy and geology. I think that’s true of a lot of the big companies. Even, unfortunately, of the petroleum companies.

CMS: So there is going to have to be a change in the continued on next page
Bailey, continued

thinking of the big companies before they're really going
to come around and get involved in university research, at
least in geology.

BAILEY: At least in geology, yes. But I think it is up to
us to show that clay mineralogy can be useful to them, can
have an economic advantage to them.

CMS: In recent issues of Time and Newsweek, it has
been stated that environmental problems will become the

Clay mineralogists can have a big impact
here because clay minerals have a great
deal to do with certain aspects of environ-
mental work.

issue of the 1990's, especially with the winding down of
the Cold War and with armaments not being most impor-
tant. Where does this bring clay mineralogists?

BAILEY: Well, clay mineralogists can have a big impact
here, because clay minerals have a great deal to do with
certain aspects of environmental work. As you know, I
did not give a course in clay mineralogy for many years.
The reason for that, of course, was that when I came to
Wisconsin, M. L. Jackson was there in soils, and he gave
the clay mineralogy course. I restricted my offerings to
the structures of the phyllosilicates and to crystal chemis-
try. After Jackson retired, which was just a few years ago,
I did start to give a course in clay mineralogy. We had
people from chemistry, geology, and soils, of course, and
engineering. But in environmental science there was a
very, very great interest from the point of view of the con-
tainment of various sorts of pollution, radioactive waste,
and so forth. I think clay mineralogy will very definitely
be involved with this.

CMS: When you offered a course that has such a diver-
sity of students, what topics did you teach? What was the
general outline of your clay mineralogy course?

BAILEY: Well, my clay mineralogy course was quite
different from the course that had been offered previously
by M. L. Jackson. Professor Jackson’s training had been
chemistry, and he emphasized the chemical nature of clay
mineralogy. My interest, of course, is in structure, so I
emphasized the structural aspects, and I told them ahead
of time that I would do this. But I talked about the basic
outlines of the structures of clay minerals. I talked about
polytypes and how one identifies polytypes by either pow-
der methods or single crystal methods. I had Carl Bowser,
who is our low-temperature geochemist, available to ad-
vise and to give us information on some geochemical and
chemical aspects of clay minerals. Carl also was inter-
ested in Bob Reynolds’ program for calculating mixed-
layer structures. He demonstrated this for the class on
the computer. And then I went into selected applications
of clay minerals, for example, pillared clays, and the smectite
to illite conversion that is of such interest to the petroleum
industry. And we went into fundamental particle theory,
of course, and interstratification of all sorts, regular and
random. I tried to give a fairly balanced view of applica-
tions that were of interest to geologists and soils people,
chemists and environmentalists.

CMS: So you covered a lot of material on surface interac-
tions also?

BAILEY: One of the things we did in the course was to
have students write term papers on things that interested
them. Since we had some chemists in the class, one of
whom in particular was interested in the surface applica-
tions, I had him take over that part of the course.

CMS: What do you see your role as a teacher when it
comes to a clay mineralogy course? Is it to impart basic
information? Is it to encourage students to get involved in
research of their own? Or is it to show the diversity of the
subject?

BAILEY: All of those.

CMS: How did you get the nickname of Bull?

BAILEY: That is certainly the question that I am asked
most frequently. It takes so long to explain that I usually
pass it off by saying the name fits my personality. I say
that deep down inside I’m really a red hot papa—you
know, the macho type that women go ga-ga over. But
when my wife hears that, she titters and gives me away. I
also have given out various other stories because I enjoy
keeping people guessing. Some of my colleagues go so
far as to insinuate the name is short for B. S., but I assure
you that is a dastardly lie! But now that I am retired, this
is an appropriate time to let it all hang out. So here is the
straight poop.

I got the nickname when I was about 18 in my home
town of Waupaca, Wisconsin. I had a good friend named
Gordon Doefler, but everyone called him Ding Dong
Doefler. Later he set up a boat livery business on the
Waupaca Chain O’ Lakes under the name Ding’s Docks.
Ding Dong was bound and determined that I should be
saddled with a nickname, too, and of course it had to be an
continued on next page
Bailey, continued

alliterative name starting with “B.”

Waupaca was on the edge of the former lumbering district of Wisconsin, and Ding Dong settled on a variation of a name that is very well known in the lumber camps—namely Bull of the Woods. If you aren’t familiar with the name, you can find it in various references. It is a complimentary, respected name. But in my case it was to be Bull of the Weeds, and it was not intended to be complimentary.

It seems there were two young ladies in town named Weed, who served as cocktail waitresses on Saturday evenings. They claimed to be afraid of walking home alone at night because they had to go through a long and unlighted region. My friends volunteered me as their escort home. In fact, I probably needed protection more than the Weed girls did, because they were older and considerably larger than me. At any rate, Ding Dong saw his chance and pounced on the association. He proclaimed to one and all that I was “rolling in the weeds with the Weeds” and should henceforth be known as “Bull of the Weeds.” Disgusting, isn’t it? But the name caught on, partly I suppose because I objected so vehemently to the name and everyone knew the allegations were false. The name follows me everywhere in abbreviated form because (a) it is incongruous for my physique and (b) no one can remember or spell correctly my real first name. The name did not accompany me to Cambridge, or at least they refused to use it there. They used my middle name instead, and I am known in Cambridge as Bill Bailey.

CMS: What do you plan to do in your retirement?

BAILEY: I have been retired for a year now, and I can’t see a great deal of difference. I don’t teach or supervise students now, and I don’t get paid. Oh, yes, a pleasant point is that I don’t have to sit on many committees now—just a few carry-overs. My main activity is research.

I also have agreed to spend two-week stints abroad, lecturing at universities and institutes. Because of the nature of my research, I am pretty well restricted to advanced-level audiences. Because of my allergy problems, I also am restricted to warm weather intervals. So far I have been to China, and I hope to go to Yugoslavia next year. My wife, of course, likes to go along to shop and spend my pension money. I plan to continue to do these sorts of things as long as my health holds out.

CMS: How do you feel about the two awards you will receive in October?

BAILEY: I feel a mixture of euphoria and humility. I can’t believe I deserve all of this attention. But it’s really great to receive such recognition from your peers. And I am especially pleased to be recognized for teaching as well as research—both of which are important to me and which I try to integrate as much as possible.

CMS: Are you the first to receive both awards?

BAILEY: Yes, so I’m told. The Roebling Medal (for research) has been awarded since 1937 by the Mineralogical Society of America. The Neil Miner Award (for teaching) has been given since 1953 by the National Association of Geology Teachers.

But I want to point out that this is not a one-way street. Any good researcher knows that he is no better than the students who work with him. I have been fortunate over the years to have had a succession of talented and dedicated graduate students. Much of my research has been published jointly with them, and they have provided both research results and positive feedback for my teaching. I really should cut up the awards and distribute the pieces to all of my former students.

Bailey’s Students

Society Archives

Pecten Texanus* and the Clay Mineral Paradigm

By Jack Burst

An interesting word appeared in the December ’89 issue of Clays and Clay Minerals: paradigm. It has long been one of my favorites. A paradigm is an example of a pattern. Simple enough. So simple that use of the word in its normal context does not convey the dynamism of its intent or interpretation.

What is truly important is not paradigm, but changes in paradigm. These are moments of significant scientific discovery or changes in scientific thought patterns popularly characterized as watershed changes or sea changes or, even more recently, as leading edge developments. Do we remember Kepler, Galileo, Newton, Einstein? Each in his time changed the thought patterns of science.

A certain clay mineral paradigm changed in the period 1950-1970, based upon work of iconiclastic clay mineralogists, among which were a dedicated group at the Shell Development Laboratory in Houston. This is a short and no doubt inadequate history of this group which lost every battle but eventually won the war.

It all began when Shell located a laboratory in Houston, Texas, dedicated to fundamental research in petroleum exploration and production, and appointed Dr. Harold Gershinowitz, a student of Kistiakowski, as its leader. Among Gershinowitz’s early appointments, which included some of the outstanding earth scientists of that time, was one clay mineralogist, Richards A. Rowland.

Rowland, originally a structural geologist, came under the influence of Grim and Bradley at the Illinois Geologic Survey, and was converted to clay mineralogy largely through his interest in a new analytical tool known as the differential thermal analyzer, which was based on earlier studies by LeChatelier. Rowland later carried on development of this instrument in cooperation with Bob Stone and Joe Weiss from the University of Texas at Austin. Stone eventually manufactured the first commercially successful DTA units, and thus the work of this group was largely responsible for converting DTA from a laboratory curiosity to a standard analytical tool.

In short order, Rowland, Weiss, and Don Lewis, a co-worker at Shell, collaborated in the development of the oscillating-heating method of x-ray powder diffraction, and built an apparatus which made possible for the first time the detailed observations of the structural changes which take place in minerals during heating. Hugo Steinfink and George Brunton, also co-workers at the lab, contributed to this endeavor as well as to many of the significant developments of the clay mineral group. Along the way, this team introduced the practice of changing the ambient atmosphere in both the DTA and the powder diffraction camera so that mineral changes could be resolved in terms more closely akin to their natural diagenetic habitat.

Burst and Braugel entered the instrumentation building activity by developing and patenting a rebasing circuit for the Norelco diffraction unit which permitted the direct and instantaneous electronic recording of x-ray patterns on a CRT. With this unit, powder diffraction patterns which sometimes took thirty minutes or

* A Cretaceous pelecypod allegedly the model for Shell’s famous logo
Shell group, continued

longer to record by the pen and ink were available in seconds. Chuck Weaver also gained recognition as an instrumentalist by patenting a process of spectral gamma-ray logging which measured K, U, and Th separately. Outgrowths of this work are still being used in petroleum exploration.

But we are getting ahead of the story. Rowland’s greater contribution was in nucleating the first commercially sponsored clay mineral investigative team. This was an important event in altering the clay mineral paradigm, inasmuch as clay mineralogy, which was absent in most institutional research environments and hopelessly underfunded in others, suddenly had the financing to acquire the relatively expensive instrumentation with which to see the unseeable.

At about the same time that Rowland came to Shell, Jack Burst, a student of Keller at Missouri, was operating a crude DTA apparatus consisting of a light bulb, a galvanometer, and a continuously rolling strip of photographic paper. Through their mutual interest in equipment, Rowland and Burst became acquainted, and soon Burst was recruited into the Shell organization. Thus, the early teachings of the two midwest clay mineral learning centers, Illinois and Missouri, became joined.

In due course, Shell added the Penn State influence in the person of Chuck Weaver and subsequently, the east coast thinking through the recruiting of Maurice Powers (by Weaver) and George Griffin (by Burst) from North Carolina. The team tried several times to add a representative from the Berkeley group, but were unable to pry anyone loose from the California lifestyle.

The stage was now (1950’s) set for the significant quantum jump in clay mineral understanding which was to follow.

Shell’s principal interest was, of course, finding the subsurface location of commercial accumulations of petroleum hydrocarbons. The clay mineralologists responded by focusing their attention on clastic sedimentation basins (particularly the Mississippi embayment area of the U.S.) which had already proven to be a prolific oil province.

Through the insights of Chuck Weaver, who headed a group which then included Maurice Powers, Ralph Stevenson, and Dan Shaw, emphasis was directed initially at providing additional information on the interpretation of stratigraphy through the formerly ignored clay mineral fraction. Rationalizing that the fine particle sediment would reflect provenance of far greater areal extent than the more well-understood sand fraction, and therefore offer new insights on the complex reworking of sediments, the clay mineralologists hoped to trace clear pathways from original source to present location.

Weaver’s work demonstrated the detrital nature of most clays in shales and illustrated the variety and potential importance of mixed-layer clays. His classic Shell paper on the distribution and identification of mixed-layer clays in sedimentary rocks, coupled with his previous work at Penn State, earned for him the Mineralogical Society of America award in 1958 as the outstanding contributor under the age of 35. Later, he was also honored with Distinguished Membership in The Clay Minerals Society.

Although this avenue of research produced interesting and often useful information, the realization that clay was reacting diagenetically to its surrounding chemical and physical environment proved much more instructive. Clay, which heretofore had been thought of as an inert filler in the clastic sequence, was rapidly becoming viewed as a relatively sensitive indicator of environmental change. Maurice Powers showed that the chemical nature of clay minerals changed with depth and established the “equivalence level” as a measurable subsurface marker in argillaceous sequences.

Burst’s work took a slightly different turn. In trying to fit Weaver’s stratigraphic concepts and Powers’ equivalence levels to actual field situations, he noticed an heretofore undiscovered phenomenon. In spite of varying provenance, reworking, winnowing, and chemical alteration, the three layer clays acted unidirectional with respect to the conversion of swelling to non-swelling lattices with depth. None ever reversed form. Thus was born a new paradigm, the now-famous smectite/lillite (S/I) relationship for measuring clay mineral diagenesis. Unfortunately, current literature labels the transition as illite/smectite (I/S), an obviously impossible transition in the context in which the term is normally used.

Initially the change was ascribed to increasing overburden pressure,
Shell group, continued

which at that time was the normally accepted vehicle for explaining subsurface changes. However, when Steinfink’s calculations suggested that the pressures required were far greater than were obtainable in the sedimentary column, attention turned to temperature. Somebody remembered chemistry 101, and a paradigmatic change took place in the field of sedimentary diagenesis. Diagenetic changes, clays and others, are largely a function of temperature, not overburden pressure.

A few weeks later, an even more important paradigm was proposed. As Burst was plotting the subsurface clay alteration contours, George Griffin noticed that they closely followed the oil production levels in a field he was studying. Eventually it was concluded that a special relationship did exist between the two sets of data, possibly through the medium of displaced interlayer water which could act as a transport medium between oil source rock and reservoir, but almost certainly through the common diagenetic denominator of temperature.

In quick order, many subsurface changes (oil maturation, pollen carbonization, geopressing, etc.) were keyed to the same rather narrow temperature range, and the “Diagenetic Isopleth” concept was formed as a temperature dependent, energy-related level in the subsurface at which the physical relationships normal to the earth’s surface were no longer valid. Below this level, new physicochemical control elements must be considered.

Proposed as a paradox, this concept is currently being championed as new thinking by Gulf Coast stratigraphers who are learning that the isopleth can be seen manifesting itself as a generally horizontal pillow of physical and chemical change weaving its way through the sedimentary column of the Mississippi embayment area.

George Griffin was perhaps the unsung hero of the Shell clay mineral group. Fresh from his work on the Neuse River estuary with Roy Ingram, he quickly established himself at Shell with his studies on the Recent and Post-Miocene clays of the Gulf Coast and his work relating such diverse items as electric log resistivity, pollen carbonization, and shale fissility to stages in the diagenetic smectite/illite transition. Because much of his work was so directly related to Shell’s proprietary interests, it was not eligible for general publication. The impact of his thinking was always present and appreciated by the staff; thus his work, like Steinfink’s, was largely reflected in the writings of others.

As no man is an island, no technical group prospers without the stimulus of peer interaction. The Shell Lab provided this essential element of progress in the form of a constant stream of outstanding scientific minds who contributed to the general milieu of discovery by their advice, their criticism, and simply by their presence.

Bill Bradley, chief clay mineral consultant, was a powerful influence. Consultants Carl Beck and Hendryk Stenzel, who oversaw Burst’s glauconite collecting in the Wilcox, were significant contributors. In-house thought stimulators included the Gulf Coast Miocene Group of Rufus LeBlanc (AAPG Powers Medalist), Bob Ginsburg’s carbonate group, Handin’s rock mechanics team, which included, among others, Mel Friedman and Dave Stearns. The irresistible Ken Hsu, Hugh Bernard, James Lee Wilson, Hal Helgeson, M. King Hubbert, Hank van Olphen, Ted Philipp, Monroe Waxman, Gus Archie, the father of petrophysics, and the list goes on and on as a roll call of dedicated scientists who rewrote the textbooks in their respective disciplines.

Certain careers were launched at the lab through Shell’s summer intern program. Among these best and brightest were F. Michael Wahl, now Executive Director of the Geological Society of America, Herman Ponder, eventually president of the Colorado School of Mines Research Institute, and Herman Roberson, clay mineralogist and professor at SUNY-Binghamton.

The years have not dimmed the contributions of the Shell group, and it is often asked, “How did they do it?” “How did they accomplish so much in so short a time?” “What was the motivation that made this group so outstanding?” Even the people who participated couldn’t define the success base. Eventually, the questions were answered by the man who started it all, Harold Gershinowitz. When he left the laboratory to accept greater responsibilities in the Shell organization, Dr. “G,” as he was affectionately called, left the following note on the desk of his successor, Noyes D. Smith, which explained it all: “The principal responsibility of the director of research is to keep those who would direct research away from those who must do the research.”

José Concha
How cold is low-grade metamorphism?

A conference entitled, “Phyllosilicates as Indicators of Very Low Grade Metamorphism and Diagenesis” was held at the University of Manchester, 4-6 July, 1990. It was convened jointly by the International Geological Correlation Program (IGCP Project 294: Very Low Grade Metamorphism), the Clay Minerals Group, and the Metamorphic Studies Group. A workshop on illite crystallinity techniques preceded two days of talks and a four-day field trip to view low grade rocks in the Lake District and in southwestern Scotland. This international conference was attended by about 100 scientists.

The conference proved to be an excellent opportunity for clay mineralogists and metamorphic petrologists to share points of view. The illite ‘crystallinity’ (Kubler) index is used extensively by the latter group to study low-grade rocks. The index is very convenient, because a single number (the width at half-height of the 001 illite reflection) can be assigned to a rock to indicate its metamorphic grade. Therefore, metamorphic events can be mapped in rocks such as graywackes and shales that otherwise would be difficult to study. However, several papers suggested that particle size and surface area might be more fundamental and illuminating measures of metamorphic reaction.

The field trip passed through beautiful country; the rocks were interesting (isoclinal, soft-sediment folds; slate quarries; graywackes; metabentonites), and the company was excellent. However, the weather was another matter. At the final stop, Professor Martin Frey stood on top of a rain-swept outcrop, knees knocking together, and shouting above strong gusts from the Irish Sea, said, “Now I truly know how cold low-temperature metamorphism can be!”

After the field trip, I visited the Geology Department at the University of Manchester. This department, under the leadership of Professor Charles Curtis, is one of the centers of excellence for the earth sciences that is being created in Great Britain, unfortunately at the expense of other earth science departments throughout the country. The department is very well equipped to study reactions in sedimentary basins (Dixon bombs; flow-through bombs; mass spectrometers; a laser ion probe; electron microscopes; etc.), and has bright and energetic students and faculty. Recalling the contributions of past “centers of excellence,” such as the Cavendish Lab and Los Alamos, might such a center devoted to the study of diagenesis make scientific breakthroughs of similar magnitude?


D. D. Eberl
Boulder, Colorado

Ralph Grim in the 1930’s, not the 1950’s

According to Art White, “The picture of Dr. Grim at his desk [mislabeled in the June 1990 CMS News] was taken at the Illinois State Geological Survey in the 1930’s, probably before I started working for him in October 1936. The picture was taken in the Old Turner House where he had an office and laboratories on the Boneyard Creek on South Goodwin, just south of the Ceramics Building.”

Omissions

The June issue of CMS News failed to acknowledge Don Scaife in the Thanks section. He and High Iron Photos were responsible for the fine photograph of Max Mortland.

Also omitted were thanks to Lelia Coyne for her advertisement.
Birthday Celebration

Walter Keller, the old mudslinger

Keller at Ninety

Born with the century and threatening to outlive it. Born when Henry Ford was still putting the finishing touches on his first handmade automobile. When the Wright brothers were still operating a bicycle shop in Dayton, Ohio. What incredible changes Walter Keller has seen in his lifetime! However, Walter is not an observer of change. He is a mover, a shaker, a contributor, and what incredible contributions he has made to this world of change!

Walter Keller is a teacher, an author, and a venerated institution in our profession. It’s unfortunate that bibliographic listings cannot convey more than a hint of the true significance of the basic documents or the significant contributions which the documents have made to the society in which we live. Keller’s bibliography crosses the boundaries of geology, mineralogy, soil science, geochemistry, electron microscopy, and others.

His early work in the ‘30’s and ‘40’s established the basic scientific approaches to the analysis of Missouri fireclays which are still the guiding principles of investigation. His work on sulfur additions to high alumina clay brought orders of magnitude additions to the reserves of refractory clay suitable for applications in steam boilers of naval vessels during the World War II period of critical raw material shortages. This contribution alone deserves any honor or recognition that his peers could bestow on him.

During the ‘50’s and ‘60’s, Walter Keller was one of the pioneers who converted clay mineralogy from an art to a science. Working with crude apparatus and minimal financial support, his research encompassed some of the earliest experimentation in clay mineral petrography, x-ray diffraction, differential thermal analysis, infrared radiation, and scanning electron microscopy. During this period, he also persuaded the A. P. Green Company to establish a series of earth science fellowships at the University of Missouri, the first of which was for seminal studies in the fledgling science of clay mineralogy.

Then in the ‘70’s, after a lifetime of solid contributions to science and having won acolade after acolade as an outstanding teacher, which after all was his life’s calling, Walter opened up a whole new world to his scientific colleagues by expanding upon his interest in electron microscopy and converting it from a device for producing interesting pictures to an analytical tool capable of shedding light on the origin and diagenesis of clays and other minerals. This work continues to the present, a time which finds Keller perhaps the world’s outstanding authority on the interpretation of scanning electron micrographs of clay. He is besieged by authors from around the world to add his interpretive genius and publishing credibility to their work.

There is no end to the Keller story. In the decade of the ‘80’s, Keller published almost 40 scientific papers, all of them worth reading. Surely, the decade of the ‘90’s will see the same devotion to the science he loves and the same willingness to share his genius with future generations.

The tallest trees cast the longest shadows. Walter Keller, a giant both physically and professionally, has cast shadows which will stretch forever. Happy Birthday, Walt. And many more of them.

Jack Burst
Rolla, Missouri

(Not: Keller celebrated his 90th birthday March 13, 1990.)

The Last Word

My official, finalizing (I hope) statement on the teaching-research question is: The bottom line on what counts in pedagogy is not how much teaching or versus research that the teacher puts out in class; it is the amount of learning that the student takes away! WDK
The Redoubtable Walter Keller

I first met the redoubtable Professor Walter Keller at the International Clay Conference in Madrid in June of 1972. I had read some of Walter’s publications, but was surprised to see that this internationally famous clay mineralogist had the physique of a football player, which I understand he really was in his student days. He had a bit more hair in 1972 than now, but otherwise his appearance has changed but little since then. Following that first meeting, Walter showed great tolerance in corresponding frequently with, and answering questions from, one who was then a rank amateur in clay science, and who, even now, can claim little more than semiprofessional status in the wonderful game of clays. Incidentally, Walter has classified clays as the nobility of the mineral kingdom, perhaps with just a bit of bias?

No one of my acquaintance has quite the same sense of humor as Dr. Keller. To my knowledge, his humorous comments never have been bitter or sarcastically biting, but rather show a keen affinity for the ironic. His humor is a curious mixture of down-home country boy and worldly sophisticate. When we humans behave in a rational, sensible manner, Keller agrees with the title *Homo sapiens*. If we display restraint and take pains to avoid violating the Good Planet Earth, he grants us the title *Homo disciplins*. But all too often Man’s actions earn him less felicitous titles, such as *Homo sap* or *Homo stupidens*.

Some more recent Kellersisms:

“...and Bill Johns give D. Moore and Bob Reynolds an A for their book. It took me 40 years to learn piece-meal just some of that.”

“The Publish or Perish ‘era’ now is Grant Bucks or Perish. Mammon has displaced Athena in Academia.”

“Another way to become a multi-millionaire is to publish science books and charge three times what they should sell for.”

“Equilibrium chemistry is fine, but the varying kinetic states are probably most of what we see.”

“Where in the range of progressively more disorder does kaolinite end and Μhalloysite begin?”

“Definitions of minerals are idealized concepts, or maybe fence posts on which sagging wire mesh of most minerals are attached.”

“Even today, I cannot get our smart kaolin crystallographers to tell me where disordered kaolinite ends and halloysite begins.”

“In another 20 years you will find that your previously sharp memory is rounded on the edges—or missing entirely. I am finding it difficult to live long but not grow old.”

“Science advances by a gradual reduction of error.”

Walter says that 90 is really only 32 on the Celsius scale. What a remarkable man! On a personal note, I am very grateful to the Ol’ Perfessor for having taught me so much about these fascinating clay minerals, even though I never was one of his students. Walter, we all hope that you, old Missouri Tiger that you are, just keep on going strong forever!

Robert B. Hall
Lakewood, Colorado

Keller's Honors

Neil A. Miner Award, National Association Geology Teachers, 1967
Faculty-Alumni Award, University of Missouri-Columbia, 1968
Distinguished Faculty Award, University of Missouri-Columbia, 1969
Citation, Missouri State House of Representatives (not routine), 1970
Outstanding Educator of America Award, 1970-71
Distinguished Member, The Clay Minerals Society, 1971
Hal Williams Hardinge Award, Amer. Inst. Min. & Met. Engrs., 1979
Distinguished Member, Soc. Mining Engrs, AIME, 1979
Walter D. Keller Auditorium, Geology Bldg.-UMC, 1981
Wm. H. Twenhofel Award-Highest Award, SEPM, 1981
Award for Leadership and Support, Society of Environmental Geochemistry and Health, 1984
Doctor of Engineering (Honoris causa), Univ. MO-Rolla, 1988
Fifty-year Fellow, GSA, 1988

Who's Who in America
Who's Who in Engineering
American Men and Women of Science
Saltonstall Fellow, Harvard University
Gregory Fellow, Univ. Mo.
Phi Beta Kappa
Gamma Alpha
Sigma Gamma Epsilon
Alpha Chi Sigma
Walt Keller in Scotland

As part of the Sixth International Clay Conference held at Oxford in 1978, field trips were held in various parts of the UK, and I was responsible for organizing the excursion based in Aberdeen in the north-east of Scotland. As I scanned the list of participants immediately before the pre-conference trip, I began to be filled with a sense of trepidation. Some stiff climbs were involved, the weather promised to be foul, and my fear was that any clay mineralogists of advancing years might find the experience just too much to take. As far as I could see, the only participant falling into the veteran category was a certain professor from Missouri, and I decided that perhaps some diplomatic maneuvering might be in order.

On the day of the trip, there was a ferocious gale, and by the time we came to the point where a hill climb was required, the rain was drum-rolling against the side of the coach. I explained to the party the rather ordinary outcrop awaiting us at the top of the hill and pointed out that the climb would mean a soaking for sure, but that if anyone was particularly keen on altered basic igneous rocks, I was quite prepared to sacrifice my health and show them the outcrop. Everyone shook their heads and very sensibly remained firmly in their seats. With one exception! “OK,” said Walt, “Let’s go.” And with that, he got off the bus and, clambering up the hillside, disappeared into the storm. Needless to say, this shamed every other member of the party into following, and by the time we reached the summit—bedraggled, panting, and wet—there was our 78-year-old professor, fresh as a daisy, utterly indestructible, and attacking the outcrop with great enthusiasm.

Never again, I thought, will I make any assumptions about the physical or mental effects of age, particularly when applied to professors from Missouri. The lesson was brought home even more firmly when I later discovered that Professor Keller had spent his summer holidays in, of all places, the Himalayas in Nepal.

M. J. Wilson
Aberdeen, Scotland

The Teachers' Teacher

The first time I met Walter Keller was at the First National Conference on Clays and Clay Technology at the University of California in July, 1952. At that meeting I asked him if he would come to Indiana University and give a lecture on flint clays at our Journal Club. He agreed, and he gave the usual enthusiastic, informative, and entertaining lecture which has become his trademark. His enthusiasm and humility have not changed over the long period that I have known him. A few years ago, after I returned to Indiana University from Georgia Kaolin Company, I was at a social function in Bloomington, and a woman asked me what I taught at Indiana University. I told her geology, and she replied that she went to the University of Missouri and that the best course and teacher she had had was in geology. I said, “I'll bet I can tell you who that was—Professor Walter Keller.” She said that was correct and that Dr. Keller was the most stimulating, informative, and entertaining professor she had had at the University. He has received many teaching awards, and from her comments, I certainly understand why.

Walt Keller and I were active participants in the IGCP project #23 on “Kaolin Correlation and Genesis.” This working group met in many places around the world, including England, France, West Germany, East Germany, Spain, Italy, Austria, Hungary, Czechoslovakia, Australia, Japan, and the United States. A major part of our activity was field trips visiting kaolin deposits. One of my memories of these interesting trips was that Professor Keller and Professor Chukrov from the Soviet Union would always sit in the front seat of the bus on opposite sides. Everyone left these seats for them, and I always tried to get close enough so I could overhear their discussions. Keller was always questioning Chukrov about geology, politics, freedom of expression, and many other topical issues. Their discussions were always most entertaining and sometimes became quite animated, but they were good friends.

Walt Keller has had a significant influence on me and my graduate students from his publications and particularly by his thoughtful questions and discussions of our research papers. One of his greatest attributes is his ability to make you think in a very clever and nice way. He is truly one of the giants in the field of clay mineralogy.

Haydn H. Murray
Bloomington, Indiana

My birthplace, North Kansas City, is in Clay County, Missouri; if it had not been named after our nationally renowned Henry Clay, it would have had to be named for glacial clay.

WDK
Keller's Tidy Office

The following excerpt from the UMC Geology Department newsletter (November '89) was contributed by the Chairman of the Department, Tom Freeman.

Jerry Yunker saw a recent account in the comic strip Shoe in which Perfesser couldn't find a ringing telephone on his desk for all the clutter. It called to Jerry's mind an event that occurred in our Department in the late '70's. While visiting with Doc Keller in his office, Jerry thought that he heard muted voices—perhaps a radio. He asked if there were a radio turned on somewhere, and Walter assured him that he didn't have a radio in his office. Not satisfied with that answer, Jerry began to pursue the sound, and Doc amiably joined him in the search. After rearranging a number of papers, posters, Pepsi bottles, and must-do notes, the two sleuths uncovered a television—fully lit with a game show broadcast by KOMU-TV.

Keller's Many Hats

Dr. Walter D. Keller is well known as a clay mineralogist, professor of geology, teacher, and researcher. It is not generally known that he was also a consulting geologist, a ceramicist, a clay miner, and prospector. In 1929, he worked for A. P. Green Fire Brick Company in the East Central Missouri Fire Clay District, developing and putting into operation a classification system for the miners. The system was used to mine and separate by hand, high aluminia clays (burleys and diaspore) into six categories ranging from 45% to 70% Al₂O₃.

In 1930, he received a degree in Ceramics from Missouri School of Mines and Metallurgy (now University of Missouri-Rolla) and continued to work during summers for A. P. Green in mining and research. He prospected for and leased land for drilling and mining. In research, he worked in the development of improved mixes for better quality fire clay and high alumina brick.

In 1936, he started and taught the first Sales School for A. P. Green in order to give sales personnel a better technical background in refractory raw materials, manufacture of refractory products, and their use and application in industry. After World War II, the sales school was expanded to a four-week course with Dr. Keller as school director. His sales schools are well remembered by a large number of A. P. Green executives who were new employees in the 30's and 40's. In the 1950's, he worked on refractory raw materials in the Pacific Northwest and did consulting work for A. P. Green de Mexicana in Mexico in the search for better quality raw materials.

In short, Dr. Keller is a well-rounded person—professor, teacher, advisor, researcher, consultant, ceramicist, prospector, clay miner, and friend.

Sue Wintsch
Bloomington, Indiana

James F. Westcott
Mexico, Missouri

Keller the Correspondent

During the eight years I managed the CMS office, nothing brought a smile more quickly than mail with Walt Keller's return address. Walt never sent a request, suggestion, or question to the Society office without adding a few lines of Keller wisdom or philosophy, or a simple appreciative insight on the rhythms of the season. From Walt I learned when the first frost settled on Missouri's pumpkins and what kind of winter was in store for the Midwest. I wouldn't be surprised if he could read his shadow for signs of spring.

I was apparently not the first he tutored in the correct spelling of Columbia (var. North American). Judging from the way he covered ground on the CMS field trips I've attended, he'll be educating and delighting Jo Eberl and the entire Society with his grace and wit for years to come.

Sue Wintsch
Bloomington, Indiana
Keller on Teaching

Most of my teaching "career" was in the days before Mammon displaced Athena in the universities. In earliest days, academic recognition was given to teachers for the number of class-hours and/or student-hours taught, i.e., for quality teaching to students including freshman classes. When enrollments in the University increased so much that classes were too large for "one-on-one" discussion (more than 50 students in a class), I devised my "Big 8" scheme to personalize students individually in large classes. This "teaching device" worked even more effectively than expected, in the auditorium here named for me. To explain auditorium, if you are a good student of Latin, you know audit means hear, and ortum, a place to, but for a modern student, the word is split before the 'i,' audio means to hear, and toro, the bull.

The "Teaching" era was followed by one on "Publish or Perish" (when administrators counted the number of published titles), so I published a little; now evidence of pedagogic and scholarly ability is measured by administrators adding on a hand calculator the thousands of grand dollars pulled in for the school, which then "skims off overhead charges" that can be used to pay higher salaries to administrators, so the bised, disillusioned faculty says (Mammon has displaced Athena).

UMC has been kind to me, still providing me with an office 20 years after retirement, although I have been on small NSF grants that paid "overhead" during 15 of those years. The CMS has proved to all of us that the whole is greater and stronger than the sum of its individual parts and members.

Keller's Students

Although Dr. Keller had many students, he preferred to mention only those who have been involved in clay-related work, either as students or during their careers. Those students are: John F. Bursi, C. P. Ding, Robert F. Hanson, Wen H. Huang, Togwell A. Jackson, Arthur L. Reesman, Nick M. Saum, Maynard Slaughter, Joe Snowden, and James F. Westcott.

Australia, continued from page 2

planning an interesting social program, field trips through Australia and New Zealand, and also the framework for a diverse technical program for the 1993 Conference. When they have tidied up the details, the participation of the active clay mineralogists from all over the world will then ensure a successful Tenth International Clay Conference. On behalf of the organising committees, I would encourage the members of The Clay Minerals Society to come Down Under in '93 and experience Adelaide's charms for themselves.

Jock Churchman
CSIRO Division of Soils, Glen Osmond Adelaide, South Australia

Teaching Papers

Space prohibits us from printing the texts of several of Keller's papers on teaching. They are available, however, from Dr. Walter D. Keller, 305 Geology Building, University of Missouri, Columbia, MO 65211, or from the CMS Office (for a $3.00 handling fee).

The titles include "Refrain from Teaching that will Hinder or Deprive Students from Learning," "On Teaching and Learning," "Teaching Geology One to Non-Science Majors in College," "Device for Student Personal Participation in a Large Lecture Class" (The Big Eight), and "Fundamental Entities in Geology." All have been published in the *Journal of Geological Education*.

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Ask the Clay Doctor
(Not a real doctor)

Dear Clay Doctor: How can one distinguish between a clay mineralogist, a zeolite mineralogist, and a geologist?
Nanometer Reader, Cincinnati

Dear Clay Doctor: Do you see any important new developments in instrumentation for studying clays?
Microman, Santa Barbara

Dear Nanometer Reader: They can be distinguished simply by asking them what they study. However, the real challenge is to distinguish them at a distance by their body language. A clay mineralogist usually cocks his head to one side, holds one hand at about eye level, palm down, and points to the edge of it with the other hand. A zeolite mineralogist bows his head and holds both hands at about chest level, as though grasping a softball. A geologist can be spotted because he holds a beer can in one hand, and waves the other hand wildly in the air.

Dear Clay Lifer: I have heard it said that clays support agriculture. Could you comment on the saponite content of the soils of the New England Maple Groves? Is it higher than that of other clay minerals? Does it vary seasonally?
Clay Lifer, Alcatraz

Dear Clay Lifer: Was this question planted by a carbonate petrologist who is trying to lead clay scientists astray by encouraging them to get lost in words? If the saponite content of soils is related to the flow of sap in maple trees, then does illite make you sick? Is dickite an aphrodisiac? Does hectorite swagger about and kill Greeks? Hardly. Clay scientists need to remain focused on the important questions, such as whether halloysite is endellite.

The Clay Doctor is available for consultation. Please send contributions to CMS News.

Position Sought
Innovative, teamwork-oriented, mineralogist (PhD August 1990) seeks a challenging multidisciplinary position. Special interest in application of clay mineralogy in industry (petroleum, ceramics), agriculture, environmental or geotechnical engineering. Experience in XRD, XRF, EM, AA, analysis of interstratified clay minerals and smectite, quantitative clay minerals analysis, geotechnical techniques, and computer applications. Familiarity with DTA and HPLC. Available immediately and willing to relocate and travel. For further details, please call or write Hameed Ullah Malik, University of Hawaii, Sherman Lab., 1910 East-West Road, Honolulu, Hawaii 96822. Phone (808) 956-8149. Fax: (808) 956-6539. Will be attending the Clay Minerals Society 27th annual meeting at Columbia, Missouri. Reference: Dr. Rollin C. Jones, Phone: (808) 956-8149.

"Position Sought" Policy: CMS News will try to print "Position Sought" advertising as a service to its members at no charge. In the event of a space shortage or an ad that runs as large as a quarter of a page, we may have to charge up to $40.00. Send ads to the CMS Office, P.O. Box 12210, Boulder, CO 80303.
Feats of Clay

Ray Ferrell recently returned from a year at the University of Berne, working with Karl Ramseyer and Albert Matter on projects relating to petrography of clays and diagenesis. He also visited Fritz Madsen at ETH in Zurich and Bernard Kübler at Neuchatel.

Bill Johns is spending the year on sabbatical at the University of Pittsburgh.

Rod Tettenhorst recently returned from a month in Malaysia where, working at the Nuclear Energy Lab of the International Atomic Energy Agency, he helped evaluate Malaysia's clay resources to see if ball clays from northeastern Malaysia have potential in the ceramic industry.

Dewey Moore was married in June to Shelley Roberts, an educational anthropologist.

Please help us recognize our members' achievements by sending in announcements of awards, grants, promotions, and new positions, as well as marriages, births, retirements, and other relevant passages.

New Members

We welcome the following members who have recently joined The Clay Minerals Society.

Dr. Prem Bhat
P.O. Box 2562
Baltimore, MD 21215

Ms. Jennifer J. Carter
1900 Powell St., 12th Floor
Emeryville, CA 94608

Mr. George C. Christidis
University of Leicester
Department of Geology
University Road
Leicester LE1 7RH, England

Miss Maureen Tischler
P.O. Box 2562
Baltimore, MD 21215

Mr. David Finkelstein
203 Woodcliff Road
Newton, MA 02161

Dr. John T. Haynes
10429 Haywood Drive
Silver Spring, MD 20902

Dr. Mingchou Lee
241 Wedgewood Drive
Lincoln, Nebraska 68510

Mr. T. Michael McDonald
P.O. Box 7682
Shreveport, LA 71137

Mr. Michael S. Ruth
653 Grain Court West
Millersville, MD 21108

Mr. William N. Pizzolato
NLU Department of Geosciences
700 University Avenue
Monroe, Louisiana 71209

Mr. Frank G. Serfin
31 Livingston Drive
Peabody, MA 01960

Dr. Shigeru Suzuki
C-204, 1150 Takeda, Kagamino-cho
Tama-shi, Tokyo, Japan

Dr. Willy A. Vlaene
Fysisch-Chemische Geologie
Celestijnenlaan 200 C
B-3030 Heverlee, Belgium

Dr. Baruch Weingarten
509 Puffin Village
Amherst, MA 01002

Mr. Pushan Zhang
Department of Agronomy
Purdue University
West Lafayette, IN 47907

Dr. Zhihong Zhou
OSHR, P.O. Box 8330, Station F
Edmonton, Alberta, Canada

X-Ray Diffraction by Disordered Lamellar Structures

by V. A. Drits and C. Tchoubar


New methods for the determination of the nature, proportion, and distribution of structural defects in microcrystallized lamellar systems. Mathematical formalism is illustrated by various examples, allowing this method to be applied easily to other solids with lamellar or pseudo-lamellar structures.

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Commentary

The Discovery of the Structure of the Clay Minerals

Linus Pauling, winner of two Nobel prizes, was awarded the Nobel prize in chemistry in 1954 for his work on the nature of the chemical bond. He received the Nobel peace prize in 1962 for his efforts to ban atomic bomb tests. These efforts included a nationally televised debate with Edward Teller, and a letter, signed by over 9000 scientists from 44 countries, that he presented to the Secretary General of the United Nations in 1958. Pauling also established the foundation of clay science by his discovery of the phyllosilicate structures in 1929-1930. Next year Dr. Pauling will celebrate his 90th birthday.

I have been interested in the clay minerals for nearly eighty years, and I was pleased when Patricia Jo Eberl wrote to me, asking me to write an account of the discovery of their structure.

My interest in minerals began in 1913, when I was 12 years old, a year before it shifted to chemistry. At that point I collected a few minerals and read books on mineralogy. Then, in the fall of 1922, a couple of months after I had entered the Division of Chemistry and Chemical Engineering at the California Institute of Technology as a graduate student and had been taught x-ray crystallography by Roscoe Gilkey Dickinson, the first person to have obtained a Ph.D. degree from the California Institute of Technology (1920), I determined with Dickinson the crystal structure of a mineral, molybdenite. This mineral was interesting as the first one to be found in which a metal atom, with ligancy 6, is surrounded by atoms at the corners of a trigonal prism, rather than at the corners of an octahedron.

The x-ray-diffraction method of determining the structures of crystals was a marvelous method. It was not then very powerful, however; nevertheless during the period around 1922, many crystal structures, the simpler ones, were discovered and thoroughly investigated. For example, Sterling B. Hendricks and I made a careful redetermination of the structure of hematite and corundum, that had been investigated earlier by W. L. Bragg and Pauling (after Sir Lawrence Bragg), who when he was a student had discovered the Bragg equation. Hendricks was my first graduate student. The x-ray laboratory of the California Institute of Technology, which had been set up in 1917, was turned over to me by Dickinson in 1924. By 1927 I had become impatient, as a result of having had to abandon the study of many minerals and other inorganic crystals because of the limited power of x-ray crystallography, at that time, to locate the atoms. Bragg had in 1926, in his effort to determine the structures of some silicate minerals, formulated the hypothesis that in these crystals the structure was often to some extent determined by having the large anions of oxygen arranged in cubic close packing or hexagonal close packing, with the metal ions in the interstices. I had the idea that the use of auxiliary information of this sort could make the x-ray technique more powerful. From studying the known structures of two forms of titanium dioxide, rutile and anatase, I recognized that they were similar in a remarkable way. In each structure there are octahedra of six oxygen ions around a titanium ion. (At that time I overemphasized the ionic character of bonds in the oxide minerals.) Rutile each octahedron shares two edges with adjacent octahedra, and in anatase each octahedron shares four edges with adjacent octahedra. I surmised that in brookite, the third form of titanium dioxide, there would also be octahedra, with each octahedron sharing three edges with adjacent octahedra, and I formulated two structures satisfying this hypothesis, and with all of the octahedra in each structure crystallographically equivalent.

My second graduate student, James Holmes Sturdivant (Ph.D. 1928),

continued on next page
Pauling, continued

made x-ray photographs of brookite and found that the dimensions of the orthorhombic unit agreed reasonably well with those that I had predicted from the interatomic distances in rutile and anatase, in which the shared edges of the octahedra are shortened to about 2.50 Å from the average value of 2.8 Å, and that the intensities of the diffraction maxima were in reasonable agreement with those predicted for one of the two structures, which is now accepted as the structure of brookite. I also used the idea, based on the ionic radii that I had published in the Journal of the American Chemical Society in 1927, that in topaz, Al₂SiO₄F₂, there would be AlO₄F₂ octahedra and SiO₄ tetrahedra, and in this way was able to locate the atoms in this orthorhombic crystal.

In 1929, after having studied some other minerals and applied this method of predicting their structures and then checking by comparison with the x-ray data, I published two papers on a set of principles determining the structure of complex ionic crystals. One of these rules is the Valence Rule. The valence of a cation is divided equally among the bonds to the surrounding anions, and the sum of the bond strengths of the bonds to each anion should be close to its negative valence, usually within one quarter of a valence unit. In the papers I started the argument by mentioning Bragg’s use of the idea that the oxygen (and fluorine) ions are often arranged in a close-packed structure, but it turned out that for many silicates this arrangement does not occur, whereas the principles of the coordination theory are satisfied.

At that time, 1929, I became interested in the structure of mica, and a few months later, of the chlorites and the clay minerals. I had become interested in mica when I was 12 years old, and had studied the large grains of mica in samples of granite that I had collected, and had also observed that sheets of mica were used as windows in the wood-burning stove in the house in which I had lived with my parents and my two sisters. I read a paper that Mauguin had published in 1927, in which he gave the dimensions a = 5.17 Å, b = 8.94 Å, c = 20.01 Å, with b = 96° for the monoclinic (pseudo-hexagonal) unit of structure of muscovite. I also made Laue photographs and rotation

Many of my early x-ray studies of minerals were made with specimens from the Oppenheimer collection, and I still take pleasure in examining some of the more striking specimens.

photographs of a beautiful blue-green translucent specimen of fuchsite, a variety of muscovite containing some chromium, and verified Mauguin’s dimensions.

The crystal of fuchsite had been given to me, along with about a thousand other mineral specimens, in 1928, by my friend J. Robert Oppenheimer, who had obtained them, mainly by purchase from dealers, when he was a boy. Oppenheimer’s first published paper, written when he was about 16 years old, was in the field of mineralogy. He later got his bachelor’s degree in chemistry from Harvard University and then a Ph.D. in physics from Göttingen. Many of my early x-ray studies of minerals were made with specimens from the Oppenheimer collection, and I still take pleasure in examining some of the many striking specimens.

I recognized at once that the layers clearly indicated to be present in mica by the pronounced basal cleavage contained close-packed layers of oxygen atoms, and that the dimensions were similar to octahedral layers in hydargillite and brucite and also tetrahedral layers in beta-tridymite and beta-crystobalite, the dimensions for hydargillite and the two forms of silica tending to be equal to those for the mica sheets to within about two percent. With the rules about the structure of complex ionic crystals as a guide, the structure of mica could at once be formulated as consisting of a layer of aluminum octahedra condensed with two layers of silicon tetrahedra, one on each side, with these triple layers superimposed with potassium ions in between. Calculation of the intensities of the x-ray diffraction maxima out to the 18th order from the basal plane gave results agreeing well with the observed intensities, so that there was little doubt that this structure was correct for mica. I pointed out in my paper, which was communicated to the National Academy of Sciences on January 16, 1930, and published a month later (February issue, Proc. Nat. Acad. Sci. 16, 123-129, 1930) that clintonite, a brittle mica, has a similar structure, with the triple layers held together by calcium ions instead of potassium ions, and that the correspondingly stronger forces bring the layers closer together, the separation of adjacent layers being 9.5 to 9.6 Å in place of the value of 9.9 to 10.1 Å for the micas. I also pointed out that talc and pyrophyllite have the same structure, but with the layers electrically neutral, and held together only by stray electrical forces. As a result these crystals are very soft, feeling soapy to the touch, whereas to separate the layers in mica, it is necessary to break the bonds of the univalent potassium ions, so that the micas are not so soft, thin plates being sufficiently elastic to straighten out after being bent, and that the separation of layers in the brittle micas involves breaking the stronger bonds of biphosphatic calcium ions, these minerals then being harder and brittle instead of elastic, but still showing perfect basal cleavage. I also mentioned the significance of the sequence of hardness continued on next page
Pauling, continued

in relation to the strength of the bonds: talc and pyrophyllite, 1-2 on the Mohs scale, the micas, 2-3, and the brittle micas, 3-5-6.

I then made Laue photographs and oscillation photographs of specimens of penninite and clinoclore, and found a monoclinic unit of structure with $a = 5.2-5.3\ \AA, \ b = 9.2-9.3\ \AA, \ c = 14.3-14.4\ \AA$, and monoclinic angle 96° 50 minutes. It was clear from the dimensions and the pronounced basal cleavage that the chlorites consisted of layers somewhat similar to those found in mica. At first I tried to formulate a single layer made of two octahedral and two tetrahedral layers, but I soon recognized that there are layers similar to the mica layers, with, however, layers similar to the brucite or hydargillite layers, but with a positive electrical charge interspersed between them, in place of the potassium ions in mica. I then comunicated a paper to the Proceedings of the National Academy of Sciences on July 9, 1930, while my wife and I and our eldest son, Linus Jr. (then five years old) were in Europe. This paper was published two months later (Proc. Natl. Acad. Sci. USA, 16, 578-582, September 1930), with the title "The Structure of Chlorites." There was good agreement between the calculated intensities of x-ray maxima out to the 26th order from the basal plane and the observed intensities.

In this paper I also proposed a structure for kaolinite, consisting of an octahedral layer with a silicon tetrahedral layer on only one side. I also mentioned that with this unsymmetrical layer there would be a tendency for the layer to curve, one face becoming concave and the other convex, and that this tendency would in general not be overcome by the relatively weak forces operated between adjacent layers. I did not predict that jelly roll structures of clay minerals would be found (and perhaps already had been reported at that time; I am not sure about when they were discovered), but I used the argument that unsymmetrical layers probably would be curved, and only in some clay minerals, kaolinite, would the tendency to curve be overcome by the forces between layers. I also discussed briefly the possibility that a clay mineral similar to chlorite, but with a neutral brucite layer, might exist, and I suggested the possibility that more complex minerals might be discovered, with alteration between the mica structure and the chlorite structure.

It now seems to me to be odd that I should have published the mica paper without mentioning talc and pyrophyllite in the title, and the chlorite paper without mentioning kaolinite in the title. Also, each of these papers ends with the statement that a detailed account of the investigation would be published in the Zeitschrift für Kristallographie, and in fact no such detailed account was published. I made many more x-ray photographs of specimens of micas and chlorites, and had my graduate student Jack Sherman make many such photographs. This work was never completed, however, partially because Jack Sherman soon became tired of the experimental work and began making quantum mechanical calculations with me, and I also became much involved during 1930 and later years in working on the quantum mechanics of the chemical bond and on a new method that we were starting to use in our laboratory, the determination of the structure of gas molecules by the diffraction of electrons. It was, of course, poor judgment on my part to say that detailed discussions would be published later.

My first graduate student, Sterling Hendricks, after he left Pasadena, carried out a number of investigations of the micas and the chlorites, as well as of other minerals. Jack Sherman continued to make calculations, and his x-ray studies of the micas maintains his only effort in this field (never published). I, however, together with my students and associates, made many more studies of the crystal structure of minerals, and I have retained my interest in this field up to the present time. In fact, my most recent mineral paper, published together with my son-in-law Barclay Lamb (American Mineralogist, 62, 817-821, 1982), is on the crystal structure of lithiophorite, which is a clay mineral. The structure that we assigned to lithiophorite, $\text{Al}_q\text{Li}_m\text{Mn}_n\text{O}_{3q}$, involves alternating brucite (octahedral) layers of two kinds. One layer has the composition $\text{Al}_q\text{Li}$(OH)$_m$, with one octahedron in 21 vacant, and the other layer has the composition $\text{Mn}^{2+}\text{Mn}^{2+}\text{O}_q$. The hexagonal unit cell has $a = 13.37\ \AA$ and $c = 28.20\ \AA$, space group $P6_3$. The determination of this structure involved the application of structural principles in a somewhat new way, which might be useful in the consideration of other complex clay minerals. The new way consists in consideration of transfer of charge through hydrogen bonds in relation to the electroneutrality principle.

At the present time my work in x-ray and electron diffraction by crystals relates to intermetallic compounds, especially the so-called quasicrystals, and the structures of metals under high pressure. I may, however, get interested in the clay minerals again, since I remember how much excitement and pleasure I had in 1929 and early 1930 when I was working on the micas, chlorites, and related substances.

Linus Pauling
Palo Alto, California
Books available from the CMS

The Clay Minerals Society offers a number of publications at a significant discount to its members. Below is a list of available books and slide sets. To order, please send the required amount plus $2.00 postage per book, in U.S. funds, to The Clay Minerals Society, P. O. Box 12210, Boulder, CO 80303. Call 303-444-6405 with questions.

Crystal Structures of Clay Minerals and Their X-ray Identification, Min. Soc., Monogr. 5, Brindley & Brown, editors, $70.00
Chemistry of Clays and Clay Minerals, Min. Soc., Monogr. 6, Newman, editor, $87.50
Both of the above, package price, $135.00
Serpentines, Chlorites, and Other Non-Mica Phyllosilicates, MSA Review Series, Bailey, editor, $18.00
Clay Minerals for Petroleum Geologists & Engineers, SEPM Short Course, Estinger & Pevear, editors, $28.00
X-Ray Diffraction and the Identification and Analysis of Clay Minerals, Moore & Reynolds, Jr., $22.00
Modern Powder Diffraction, MSA Review Series, Bish and Post, editors, $20.00
Mica Polytyle Slide Set, Prepared by Audrey C. Rule for the CMS, $15.00
Crystallography Slide Set, Symmetry Drawings of the Seventeen Plane Groups, Prepared by Audrey C. Rule for the CMS, $32.00
Quantitative Mineral Analysis of Clays, CMS Workshop Lectures: Volume 1, Pevear & Mumpton, editors, $14.00

CMS Membership Application

To join the CMS, please send a copy of the application below, along with the required dues in U. S. funds, to The Clay Minerals Society, P.O. Box 12210, Boulder, CO 80303. Subscribing members receive Clays and Clay Minerals, the journal of the CMS.

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