T. A. Moore

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Dear Tim
Please note your
substantial contribution to
the Ferm Saga. Didn't
have the paper back from
Krynine yet. Best my
best with Gary Glass
John.

Presentation of the Gilbert H. Cady Award to John C. Ferm

CITATION BY ROBERT EHRLICH

The Gilbert H. Cady Award is presented to John C. Ferm for his outstanding work on coal geology, which has spanned more than four decades and continues at an undiminished pace. He is largely responsible for the development of the fundamental understanding of the geometrical relations of the lithofacies within coal-bearing sequences. In order to do so, he has integrated outcrop, bore-hole, and mining data to yield precise three-dimensional stratigraphic syntheses in which centimeter-scale resolution is often crucial. The demands for copious high-quality information led him to develop one of the first computerized storage and retrieval systems for stratigraphic data. The database is the most complete record of stratigraphic variability of fluviomarine sequences in the world and is currently being used by petroleum companies for oil field modeling.

He developed and distributed to mining companies and others "core books" containing high-quality color photographs of all lithotypes in the sequence to standardize and speed up core description. Paleogeographic modeling that complements the work of stratal geometry is important in its own right, but has also proved useful in evaluating the validity of alternative correlations. Ferm's paleogeographic block diagrams have found their way into numerous textbooks.

His work has had a direct impact on improving coal exploration and development strategies. Stratigraphic analysis has improved mine safety by predicting areas prone to roof collapse. Many of his students are now prominent in the mining industry as well as in government and academia.

John Ferm's commitment to understanding the coal-bearing sequences began while in grammar school in Midland, Pennsylvania, where he was fascinated by plant fossils found in coal mine spoil piles. After flirtation with paleobotany, John was drawn into the orbit of P. D. Krynine and John Griffiths at Penn State, where no geological assumption was unchallenged, and the Socratic dialogue was the order of the day. The intellectual excitement of those days and the critical attitude to "common-sense" assumptions has remained with John throughout his career. The influence of the "Great Artist" Krynine and the "Great Quantifier" Griffiths has produced a happy synthesis in John.

The Gilbert H. Cady Award represents well-deserved recognition of the pioneering contributions of John Ferm in delineating and interpreting coal-bearing sequences; his contributions to applied as well as academic coal science; and his patience with and encouragement of students in stratigraphy, paleontology, petrology, and paleobotany.

RESPONSE BY JOHN C. FERM

To receive the Cady Award is both an honor and a surprise. The honor arises from my recollection of Dr. Cady, whom I met while I was a graduate student at the University of Illinois working under the supervision of Dr. Wanless. Dr. Cady appeared to me then as a formidable figure, who was not entirely in sympathy with some of Dr. Wanless' theories. Theories are a serious business with graduate students, whose heads are crammed full of them (but with very few facts), and all of this was a very important matter to me then. The next time I met him was on the front porch of my parents' home in a small steel mill town in western Pennsylvania. I was doing some work on the Allegheny Formation, and Dr. Cady and Bill Smith were looking for potentially minable Upper Freeport coal. Coal mining had left the area some time ago and I could be of little help, but it did give me some insight into his approach to a very practical question. Later I read some of Dr. Cady's Illinois work, and I have generated an image of a very rational person. As someone who has spent his career in the academy and government, I have come to treasure the quality of rationality, an attribute often absent in these institutions.

My surprise in receiving the Award arises from the fact that hell raisers are rarely thanked for their efforts, and I believe that I have raised more than my share of hell. I should first explain that by "hell raising," I do not mean random mischief or debauch but, rather, I am using the world "hell" in the sense of the ancient Greeks, whose orderly minds viewed hell as a disordered universe—or at least disordered from the point of view of the accepted ideas or concepts. In fact, the "hell" that I have raised has really amounted to alternative explanations for facts as they are known at the time.

One of my earliest experiences with this matter was testing the cyclothem hypothesis that I absorbed as a graduate student. One doesn't hear much about cyclothems these days, but they have returned recently in the disguise of "sequence" or "genetic stratigraphy." Now, however, they are burdened with a mound of what is said to be "scientific" terminology instead of the simple numeric designation for rock types of the classical cyclothem. The crucial feature of the cyclothem, as originally described, is the erosional contact at the base of the sandstone that is the bottom member of the cycle. This was said to be an unconformity of regional importance. When I first went into the field in the unreclaimed strip mines of western Pennsylvania, the concept worked like a charm; 40-ft-thick sandstones with scoured basal contacts overlay dark shales with marine fossils, and the dark shales overlay the coal. In addition, a thin coal bed and underclay could be seen overlying

the sandstone. With the coal overlain by marine shale and underlain by a sandstone with a scoured base, the cyclothem was nearly complete. Although one or two of the rock types were not there, the main elements were and all was well with the world.

As I looked further, however, problems developed with the sandstone and its basal scoured contact. The sandstone was still present and it was overlain by a coal and underclay, but it was diminutive in thickness and *gradational* downward into a thick sequence of fossil-bearing siltstone and shale overlying a coal. It was, in fact, the top of what we now recognize as a "coarsening-upward sequence," and the cyclothem began to look more like Udden's western Illinois cycle and what the Brits had described as a cycle in their coal measures—and there was no unconformity. Gene Williams, with whom I was working at the time, argued that the unconformity was there but that it was in the underclay. That was OK, but it was not a cyclothem as it was in the books.

Borrowing liberally from our professors P. D. Krynine and John Griffiths, Gene and I put together a model that would include sandstones with and without a scoured surface at the base and coals grading laterally into ironstone beds with marine fossils, which is something that we had seen (Fig. 1). There were split coals on one end, of which we had seen several, and split marine limestones on the other, which we guessed at. We had a systematic pattern of land-sea distribution of rock types and a predictor of lateral variation. In a later, more elaborate form, this model became known as the "Allegheny duck."

If I had been wise and stayed in the strip mines in Jefferson and Clearfield Counties, Pennsylvania, the "Duck" would probably have survived longer, but later, at L.S.U., I supervised two studies, one by Vic Cavaroc in the so-called Allegheny rocks in central West Virginia and one by Bob Ehrlich in the Pottsville of the Warrior coal field of Alabama. The "Duck" could be painfully squeezed into Vic's West Virginia model, but it really didn't have much relevance to Bob's Alabama Pottsville. Something was wrong and, obviously, it wasn't with the rocks.

On the brighter side, results began to come in from a study of shallow cores drilled in West Bay, a shallow body of water on the lower reaches of the Mississippi Delta. Jim Coleman and Woody Gagliano were graduate students at the time and were working on a project to establish the shoreline of the delta during the past century. These cores, although composed of loose sand, mud, and some peaty muck, had sequences similar to what we were seeing in eastern Ohio and western Pennsylvania, and we convinced R. J. Russell, then director of the Coastal Studies Institute, that Jim Coleman ought to see the outcrops. So in a frenzied week of picture taking and all-night discussions, the Allegheny "Duck" was transformed into an Allegheny delta model with the West Bay data as a modern analogue. The sedimentary structures fit nicely, and, save for the fact that the coals were much too thick for the Mississippi peats, an upper and lower delta plain and alluvial plain facies could be recognized in the Allegheny rocks (Fig. 2). Later, comparing the rocks with the results of Miles Hayes and his students on modern coasts, the model developed a distinct shoreline edge, and the simple land-sea "Duck" became more specific with respect to sedimentary criteria and process inferences while retaining its basic geomorphic form.

As is often the case, the dissolution of the delta model for me began just at the peak of its acceptance. The state of Kentucky began a major road-building program and generated a series of enormous cuts extending north to south across the eastern coalfields. John Horne and Bruce Baganz were able to document some of the most beautiful deltaic sequences ever seen, but the predicted land-sea pattern of the "Duck" just wasn't here. Instead of the lower delta plain rocks passing laterally into the upper delta plain facies, they just got thicker and the fluvial sandstones popped in and out in no particular order. Something was seriously wrong. John Horne began to see hints of what this was, but it was Jim Staub and Jerry Weisenfluh, working with bore-hole and in-mine data in West Virginia and Alabama, who were able to deliver the final shot. Sequences that had been designated lower delta plain, upper delta plain, and alluvial plain were all there and the basic interpretations about process were correct, but one facies passed laterally into another with much greater rapidity

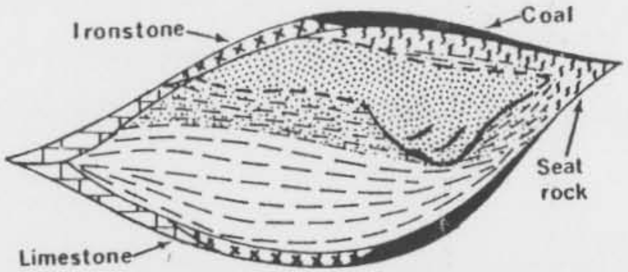


Figure 1. The first version of the "Duck" model based on surface mine outcrops in Jefferson and Clearfield Counties, Pennsylvania. As drawn, there is neither transgression nor regression. In a transgressive mode, marine limestones and ironstone extend entirely across the top of the model, and coal along the bottom. In a regressive mode, the distribution of the marine limestone/ironstone and coal is reversed. In either case, the character of intervening detrital rocks is modified accordingly. J. C. Ferm and E. G. Williams, American Association of Petroleum Geologists Bulletin, v. 47, p. 356–357, 1963.

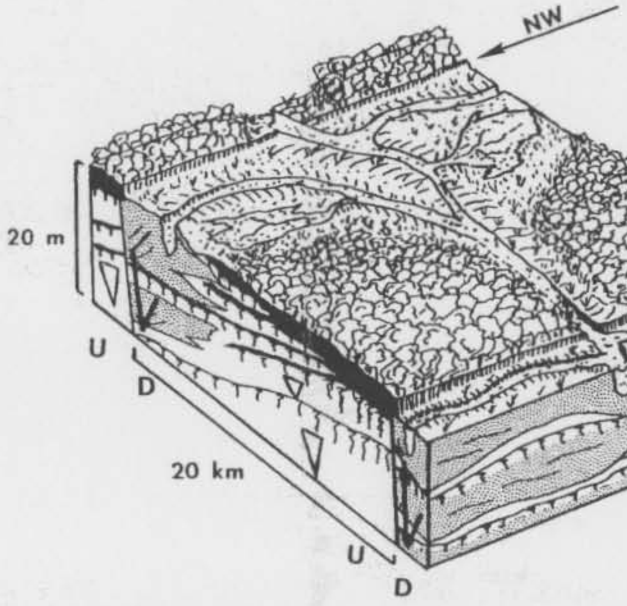


Figure 3. Effect of deep-seated faulting on contemporaneous accumulation of thick coal and thick sandstone. Thick coal accumulates on topographically elevated surfaces on the upthrown sides of fault blocks. Waterborne sands are deposited on topographically lower, downthrown sides. Note that structure effects are not included in Figures 1 and 2, in which depositional control is purely geomorphic. J. C. Ferm and G. A. Weisenfluh, International Journal of Coal Geology, v. 12, p. 259–292, 1989.

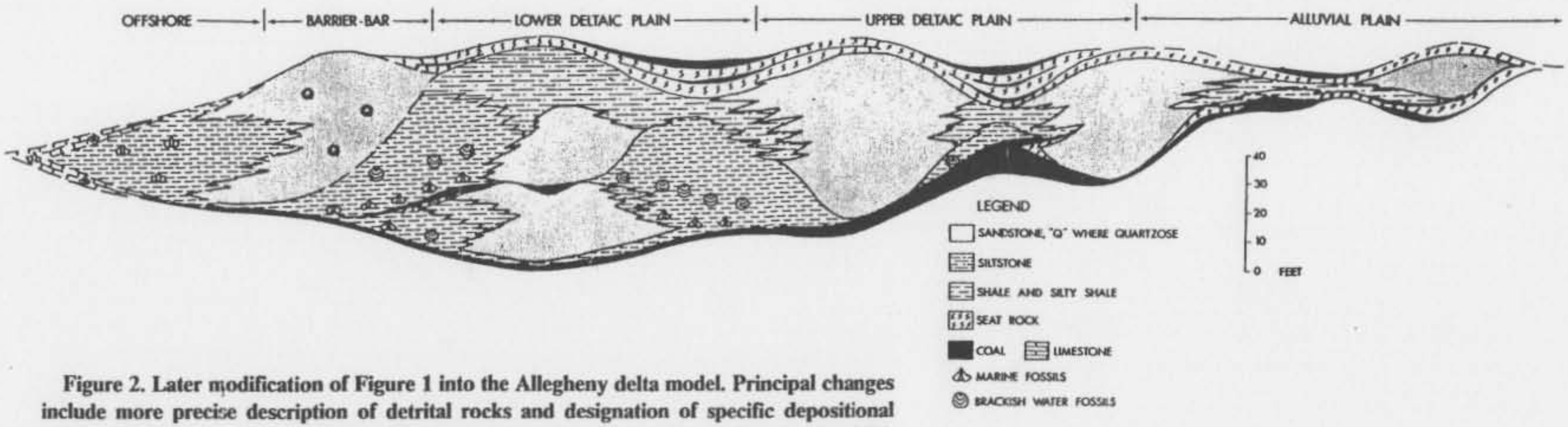
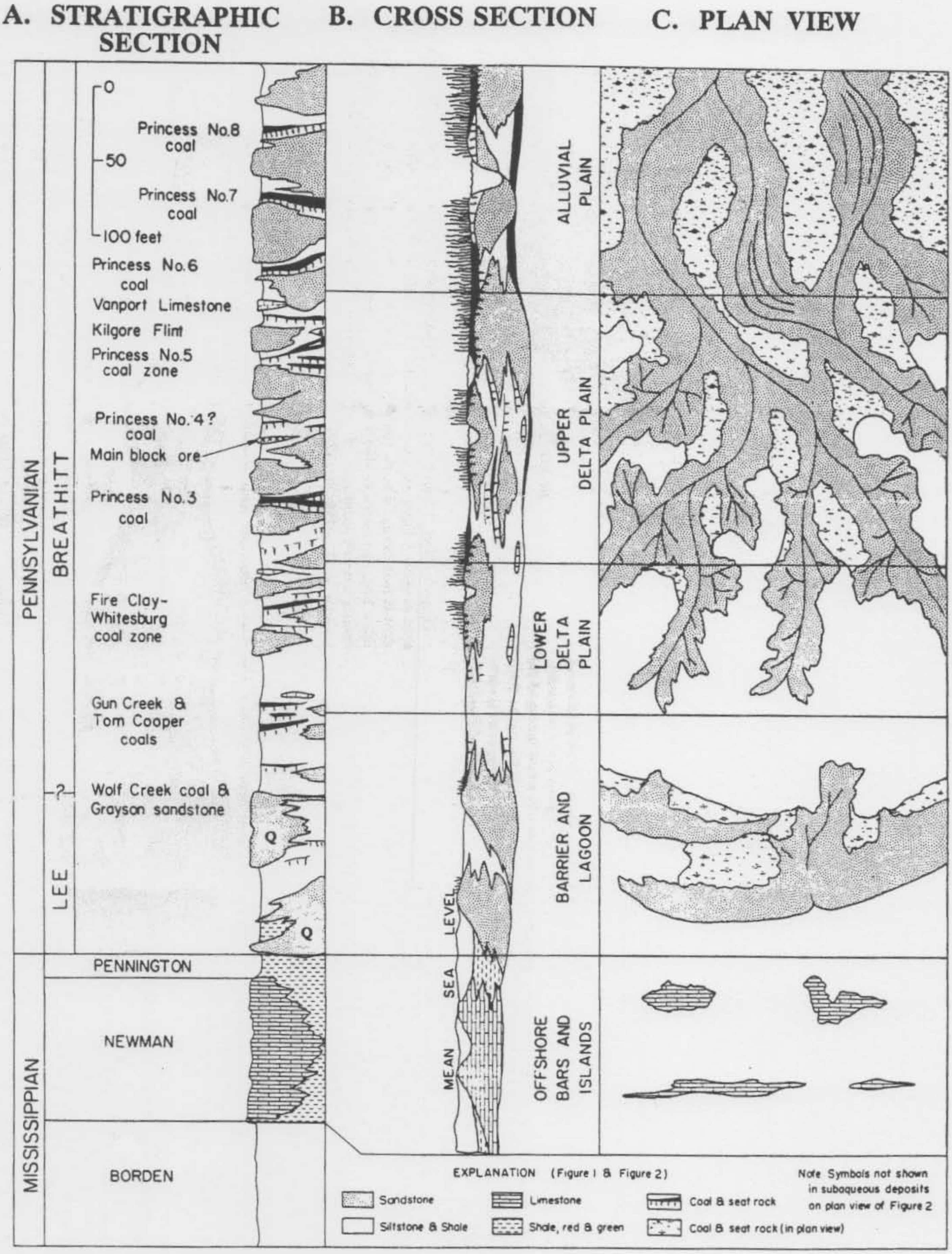


Figure 2. Later modification of Figure 1 into the Allegheny delta model. Principal changes include more precise description of detrital rocks and designation of specific depositional settings based on a modern Mississippi Delta analogue. J. C. Ferm, Compte Rendu, Band III, Septième Congrès International de Stratigraphie et de Geologie du Carbonifère, p. 9–25, 1974.



A. STRATIGRAPHIC SECTION

B. CROSS SECTION

C. PLAN VIEW

Figure 4. General stratigraphic section and its environmental interpretation. A, on the left, is the stratigraphic section in the northern part of the eastern Kentucky coalfield. This section is much thinner than in other parts of the coalfield, but the presence of thick lithic arenites in the upper part of the coal-bearing sequence and quartz arenites (Q) in the lower part is typical of the region. B consists of an environmental interpretation of the left-hand column. C is a plan view of the center column. This interpretation shows the Carboniferous succession as reflecting a prograding sequence with offshore lime and silicate muds separated from fluvio-deltaic deposits by shoreline quartz arenites. J. C. Ferm and others, Carboniferous depositional environments in northeastern Kentucky, Kentucky Geological Survey, 30 p., 1971.

than the delta model would allow and the pattern would be repeated again and again at 6- to 10-mile intervals. A model was there, but it wasn't pure land and sea. As it has turned out, there is now evidence for deep-seated structural control during sedimentation, which appears to be the governing mechanism (Fig. 3). As one of the graduate students put it, "After 20 years, Ferm finally discovers structural geology." I suppose that it is better late than never.

In connection with the highway work along U.S. 23 in eastern Kentucky, another bit of hell was rising along I-64 where it crosses the strike of the Coal Measures. This problem arose for me many years before when I was working with the U.S. Geological Survey and was learning about the Pottsville sequence of eastern Kentucky. In the northern half of this area, the lower part of the sequence was made up of thick quartz arenites with thin coals and shales. This was overlain by coal-bearing fluvio-deltaic rocks in which the sandstones were lithic arenites that became more abundant in the upper part of the section (Fig. 4A). How to explain this? There were thick sandstones at the top and bottom, but the lower sandstones were much more quartzose than the upper ones.

One clue was provided by the lateral relationships of the quartz arenites where it could be shown that they interfingered with shales and lithic arenites of the overlying coal-bearing rocks. This could be interpreted as evidence for two sources—quartz detritus from one direction and lithic detritus from the other—which is what Gene Williams and I had proposed for Kittanning rocks in western Pennsylvania. There was no real point in looking at the rocks *under* the quartz arenites because an unconformity was said to separate them from abundantly fossiliferous red and green shales and thick limestones which were totally different from the overlying quartz arenites and coal measures.

Another clue emerged one day when, by the merest chance, I was driving along a small, rural road that closely overlay the quartz arenites, and there, lo and behold, were fossiliferous red and green shales and thin limestones *overlying* the quartz arenites. Remembering my lesson from school that said that sequential repetition of different rock types spelled interfingering and not unconformity, I began to wonder about this unconformity at the base of the quartz arenites. Unfortunately a search of nearby areas showed few outcrops, and the problem had to be set aside.

The solution came many years later when the Kentucky highway improvement began and I-64 was generating large and laterally continuous cuts only a few miles away from the place where the initial observations had been made. At that time, John Horne was a new post-doctoral student looking for a problem, and we thought that this was just the one for him. He was newly arrived from the University of Illinois and was quite convinced about the regional unconformity at the base of the quartz arenites and, therefore, was well suited to approach this problem. Neither John nor I knew much about limestones, so we called in Jon Swinchatt to help us out. When they finished their work, it was reasonably evident that the quartz arenites represented shoreline sand bodies and the carbonates were beautiful examples of carbonate islands and shoals (Fig. 5). At that point, the Carboniferous succession became clear. It was a normal prograding sequence with offshore carbonate islands surrounded by red and green mud, overlain by quartzose shoreline sands which, in turn, were overlain by and interfingered with coal-bearing fluvio-deltaic sediments (Figs. 4B and 4C).

There was a big problem, however. The thick limestones and red and green shales were said to be "Mississippian" in age and the Coal Measures "Pennsylvanian," and if the prograding model was correct, parts of the "Mississippian" rocks were the same age as parts of the "Pennsylvanian." If you think you have seen hell, you have really seen nothing compared to the reception that these results enjoyed. This controversy is still not resolved. My biggest disappointment in this case was that despite all the protests, no one to my knowledge actually repeated the observations to confirm or deny the facts. I was obviously not dealing with traditional science, which involves repeated experiments by different observers.

In the meantime, other things were happening. I had always been interested in southern West Virginia where, on outcrop, there were no quartz arenites separating the lithic arenites of the Coal Measures from the underlying fossiliferous red and green shale. In the subsurface, only a short distance away, however, the quartz arenites interfingered with and replaced the Coal Measures of the outcrop. A search for outcrops yielded disappointing results, and it became clear that subsurface information was required. Because this was a major coal producing area, it was obvious that the drill-hole records from the coal companies would be the major source of information. It has taken a long time to accumulate enough information to solve this problem, but Jim Staub has developed a summary which shows the Coal Measures of the Pocahontas and New River Formations interfingering with the red and green shales of the underlying Mauch Chunk Formation which is said to be of Mississippian age (Fig. 6). The relationships are similar to those on I-64, but their scale is much more grand.

One of the reasons that it has taken so long to reach these results in West Virginia is that we were diverted to another problem. It all began while Malcolm Galloway and I were waiting in the drafting room of the Westmoreland Coal Company office in Tams, West Virginia, to see the chief engineer. The walls were covered with mine maps. I had seen mine maps before, but had never looked very closely. With time on our hands, Malcolm and I began to examine the maps in detail—and what a surprise! At every tunnel intersection (about 60 ft apart), the elevation at the base of the seam was recorded and, at spacings of about 100 ft, the character and thickness of the seam were given. The terminology was simple—coal, bone, and rock—but all the thickness variation in each rock type was documented for miles and miles underground. Data like this had accumulated over several lifetimes of engineers and surveyors and were just sitting there unused. This, plus drill-hole records surrounding the mine where the coal was thin, completely documented the character of the coal body. Needless to say, the coal bodies described using these data did not resemble the very regular pattern shown on conventional correlation diagrams. Seams would split or thin, and benches would abruptly depart from the bottom or top of the seam. An opportunity to learn

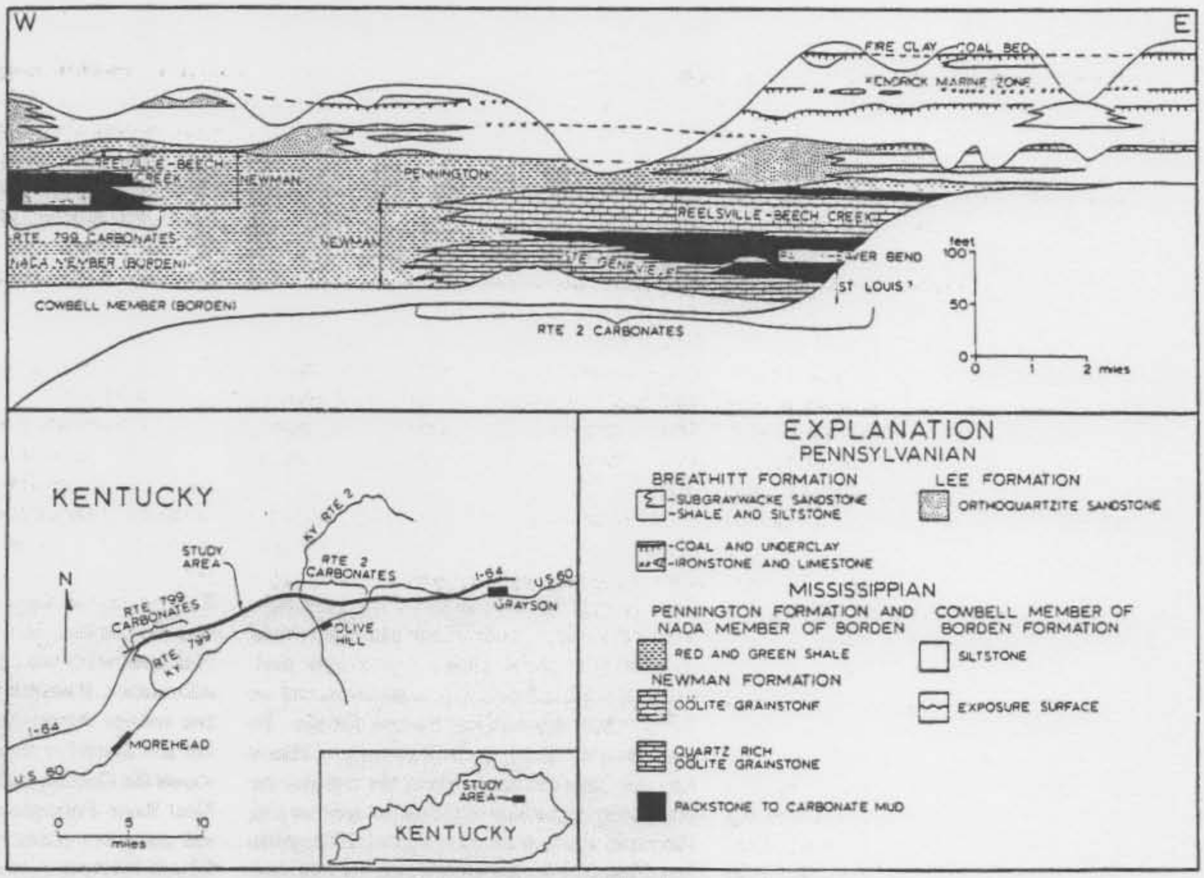


Figure 5. Cross section of Carboniferous rocks exposed along Interstate Highway 64 for 30 miles westward from Grayson, Kentucky. This section shows the areal distribution of offshore carbonate and silicate muds, shoreline quartz arenites and fluvio-deltaic lithic arenites, mud rocks, and coal beds. Superposition of these facies arises from westward progradation. John C. Horne, John C. Ferm, and Jonathan P. Swinchatt, Geological Society of America Special Paper 148, p. 97–114, 1974.

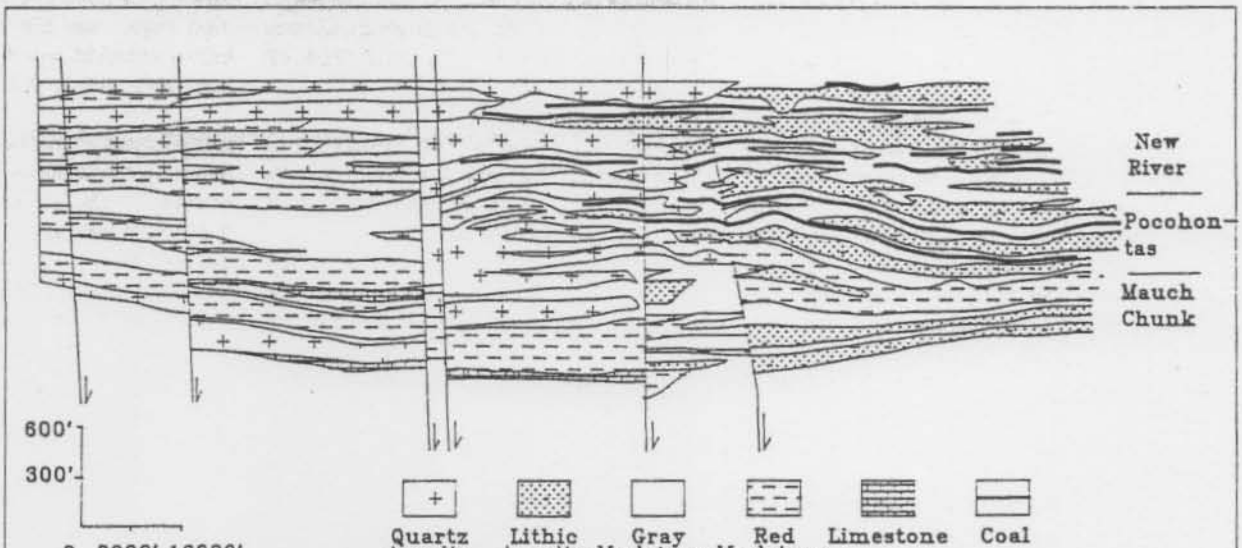


Figure 6. Cross section of Carboniferous rocks westward from Beckley, West Virginia, based on oil test records and logs of continuous cores for coal exploration. Note that the relationships between fluvio-deltaic coals, shoreline quartz arenites, and red offshore mudstones are similar to those in Figure 5 but that thicknesses here are much greater, presumably due to greater subsidence. Generalized from a personal communication from J. R. Staub, 1991.

about coal beds in detail and to integrate these data into regional rock patterns presented an entirely new suite of problems.

Simultaneously, another opportunity came along by way of a study of roof falls in the Pocahontas #3 seam under the auspices of the U.S. Bureau of Mines, with Noel Moebs of the Pittsburgh office overseeing the work. This allowed us not only to learn about roof falls but also allowed access to the mines and made us more familiar with the geologic problems faced daily in coal-mine operations. Throughout this period, the Westmoreland Coal Company continued to provide guidance and material help, and their assistance is gratefully acknowledged here.

There were two principal results of this work. First, we found that geologists could make real contributions to the operations part of the coal industry. In many cases, geologists had been restricted to reserve calculation or property acquisition, but it became clear that they could also offer major contributions in day-to-day problems of seam thickness variation, roof and floor control, and coal quality, because the scale was much smaller than in most geological work and much greater precision was required. The opportunity was there to find out about coal and associated rocks and to apply this knowledge.

Later, as we became acquainted with the work of some coal company geologists, another important result became evident. These geologists, although they had substantial resources to carry out their work, were very much constrained by time limits. Assignments were generally short term and, although they could make some very important observations, they could not linger over them and had to move on to the next project. Except for the personal knowledge that they gained, the information was lost to the scientific community. In contrast, data of this quality are simply not available to scientists, but it is their job to pursue ideas and write up results. It is my opinion that a major contribution to coal science and coal geology could be made by the development of relationships between company geologists and those in the scientific community. Such a mutual relationship could yield a substantial body of information to science at large, and any new material found could be quickly fed back into industry where it could be applied. This would require some organization, but it would be well worth the effort.

All of this effort working toward a definition of minable coal deposits came to roost when the Jet Propulsion Laboratory asked us to estimate coal reserves of the entire United States, including Alaska, in 1 year. This was a *real* change in scale, but it allowed a re-evaluation of methods used in reserve-resource estimation. I had worked on the eastern Kentucky reserve estimate by the U.S. Geological Survey and clearly remembered tracing coal outcrops, isopaching thickness, and drawing arcs that were supposed to represent the confidence in the estimate. At the time, I asked about the basis for this method and was assured that it had been "all worked out" years ago. Comparing the data that we had used to produce the Kentucky tonnage estimate with that I had seen in the coal companies, I really began to worry. With what probability could correlations be established in a rigorous sense, and what sort of confidence limits could be placed on the tonnage estimates? Quite aside from time constraints, these standard techniques could not be justified in our JPL study, and we therefore devised a statistical technique based on one used early in this century by M. R. Campbell. The level of precision was low, but at least additional data could measurably improve the quality of the estimate. This was no more than a start, however, and, of all topics in coal geology, I regard this as the greatest piece of unfinished business.

From determining the shape of coal bodies based on mine maps and drill-hole data to determining the character of the coal itself would seem to be a short step, but I had always avoided it. From what little contact I had with the subject matter, the terminology seemed formidable, and experts argued among themselves about recognition of coal components. I had begun to compare this with P. D. Krynine's definition of stratigraphy as "the complete triumph of terminology over facts and common sense." A try was necessary, however, and when Joan Esterle turned up, I suggested that she study the petrology of a seam in southeastern Kentucky. I also passed along the sage advice that it was common knowledge that one bench of coal would differ greatly from the other, and hence, the bench should be the basis for sampling. After a week underground, she came back and told me that this was nonsense (actually very much worse than nonsense) and that there was more variation both vertically and horizontally *within* benches than *between* them. This spelled death in the afternoon for the bench sampling idea and raised a still unresolved question of recognizing levels of maximum homogeneity within benches. So much for common knowledge in coal sampling.

The next step in examining the coal was sample preparation, which, I was told, consisted of grinding the coal to very small particles and mounting them in plastic pellets. For a person trained in sedimentary petrology, this sounded a little odd—we don't grind up sandstones for microscopic observation—but I was willing to go along. Then Tim Moore dropped his bomb. Ground samples of two coals yielded the same petrographic results but different grindabilities. Small etched blocks (as per Ron Stanton) of the same two coals, however, showed big differences in the *size of the constituent organic* particles, and these differences could be related to grindability. In addition, we found that the composition of the particles was clearly related to their size. Now here was something that I could understand—composition could predict size, and size/composition could predict grindability. Out went the ground coal pellets, and we started over. Subsequently Tim has done more experiments and has come up with some nice polymodal size distributions that point to a new direction in characterization and sampling. For me, the woods of coal petrology are still deep and dark, but there seems to be a beginning for the way out.

I would like to conclude with a few comments on what has been the origin of all this "hell raising." The first is rank opportunism. We all carry around some geological concepts, but occasions arise when we can really put them to the test. For example, the unreclaimed strip mines in Pennsylvania permitted a test of the cyclothem concept and demonstrated the possibility of a better-fitting alternative hypothesis. The newly constructed I-64 highway cuts allowed for generation of alternative hypotheses concerning "Mississippian" and "Pennsylvanian" rocks. The U.S. 23 and Kentucky Route 15 data allowed for the testing of the Allegheny models. The coal company drill holes and mine maps allowed us to see very precisely the nature of coal beds. Were these data not available, there would have been no "hell raising."

More important are the people with whom I have worked. These are mostly graduate students, and, in every case, I have tried to let them develop their own direction of interest. In this way, I learn from them and they pursue their work with greater enthusiasm. Bob Ehrlich very early showed an interest in statistical treatment of data, and following his logic allowed me to learn something new. I still regard Vic Cavaroc's model in central West Virginia as a classic. John Horne was probably the best field man that I have ever known, and his work led to massive contributions to my own thinking. Bob Melton was convinced that the only way to manipulate core-hole data was with computers, and he began the first generation of our computer programs. Jerry Weisenfluh and Jim Staub showed me how much information could be gained by careful underground mine observations, and Joan Esterle and Tim Moore dragged me kicking and screaming into coal petrology. Bob Hook and Glen Merrill even taught me at least something about fossils. This list goes on and on, and I cannot do justice to it with limited time; I want to make the point that without a hard-working and aggressive cohort of student colleagues, all of the "hell raising" would not have been possible.

Finally I want to acknowledge my teachers,

P. D. Krynine and John Griffiths, who were first class "hell raisers," and T. C. Chamberlain, who developed the notion of multiple working hypotheses. Chamberlain's cooly reasoned thesis about alternative explanations was carried out in spades by Krynine and Griffiths. Although Krynine could confront hypotheses with real rigor, his long suit was hypothesis generation. Griffiths, on the other hand, could propose some very reasonable concepts, but he really shone in testing every hypothesis in sight. In such a cauldron, the idea of multiple hypotheses and rigorous testing become thoroughly ingrained. Personally, I have not been able to carry out the multiple hypothesis concept very well. I seem to be able to handle only two at a time—generally an existing one, mine or someone else's, and an alternative that at least at the time, seems to better fit the facts. The main effort has always been the continual testing of any existing hypothesis until major cracks appear, then reassembling the surviving pieces into an alternative until it, too, fails. Presumably we will arrive at something nearer the truth with each reconstruction.

I should stop now, but I want to assure you that the "hell raising" is not over. For example, we have a very strange rock in the Appalachian region called "flint clay." It is not at all common, but its properties make it very distinctive. It is very hard, fine grained, and brittle, and it is in many cases associated with coal beds. Bill Bragonier at the Rochester and Pittsburgh Coal Company has recently provided a summary of what is known about this rock and how it was formed. Because of its high kaolinite content, some authors believe it to be an *in situ* residual product of intense weathering, whereas others believe that it is a transported residual material. More recently, a volcanic origin has been proposed based on the presence of beta quartz, sanidine, and similar high-temperature minerals.

Not long ago, Steve Moshier, one of our junior faculty, who is a carbonate petrologist and who had never *heard* of flint clay, told me about some strange chert that he had found associated with marine limestones and coals in the western Kentucky coal field. The strange part was that the so-called chert had a very high kaolinite content, and the rock consisted mainly of fine-grained chert and kaolinite. This strange rock turned out to be a flint clay, and it was now clear why at least these flint clays are so hard. With this information at hand, I began to look around for the chemical background that would precipitate both kaolinite and chert. The outcome was pretty exciting, so I called Bob Ehrlich, who was doing some related work (bacteria devouring feldspar). Bob told me to look up the results of some Danes, who were studying chert nodules in the Cretaceous chalk and who had found that the chert was, in fact, trydimite. *Trydimite*?! I tried to recall the phase diagram—certainly high temperature. There it was in chalk, however, with the nearest igneous rock miles away. Then I began to think of the beta quartz in flint clay and the development of authigenic feldspars in sediments. Could they be albite or sanidine? Then I remembered that the phase diagrams really did not tell much about the pH or Eh background of the reactions. I felt a slight trembling of the floor, and the temperature of the room was distinctly warmer. Was there a little bit of hell just below the surface, ready to come out? Well, we will see.

With that, I really will close with many sincere thanks for this Award and your patience in listening to me.