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CONCEPTUAL REVOLUTIONS AND GEOLOGICAL EDUCATION

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ABSTRACT

Important new ideas in geology mostly originate *in spite of* the educational system rather than because of it, but at least nowadays they arise in the universities rather than elsewhere (though not always in the obvious disciplines). New ideas seem to get through the system faster nowadays than in the past, through this depends largely on the internal organization of the departments concerned. The essential thing for a forward-looking organization open to new ideas is an atmosphere of interest and enthusiasm for the subject and a respect for the man with unconventional ideas.

Charles Darwin would never have been awarded a research council grant in the latter part of the 20th century. And, certainly, with his qualifications, he would never have got the job on the "Beagle" had he been applying for it today. That illustrates my first point in considering the great conceptual revolutions of the past and their relationship to formal geological education. Important new ideas *have* not and *do* not arise by way of a conventional education. It is commonplace to speak of William Smith, the canal engineer and Hugh Miller, the quarryman. But Hutton was a doctor turned farmer, Murchison was an old soldier, Buckland and Sedgwick were clerics, Lapworth was a school-teacher, Mantell and Rowe were doctors and Baron Cuvier was a biologist turned statesman (and was nearly President of France). And, of course, Wegener—the originator of our second great revolution in the earth sciences—was an astronomer turned meteorologist (and balloonist). As Tony Hallam wrote recently of Wegener (1975) "to geologists of the time Wegener was an outsider .... Today .... we can see that his position was an advantage because he had no stake in preserving the conventional viewpoint".

Hardly any of the men who were responsible for the early conceptual revolutions in our subject had a conventional geological education. And do not let us forget that geology was being taught in our universities way back in the 18th century. Werner started teaching it in 1775 at Freiburg, where the famous Bergakademie had been founded 10 years before. John Walker started geology at Edinburgh in 1779. In other words the conceptual revolutions came about *in spite of* the teaching, not because of it! Perhaps this is not peculiar to geology. After all, we are always told that Einstein couldn't do sums.

Nowadays, of course, we live in a different world, where people's leisure is a valuable commodity to be exploited, and it is far rarer for the intelligent man of leisure to theorize about the fundamentals of science and for the scientist himself to move from field to field. We excuse ourselves on the grounds of the need for fantastically expensive equipment, but once the data had been collected and published the basic ideas of plate tectonics, for example, were very simple and did not need costly ironmongery. The same is true with regard to the revolution we are now perhaps going through in palaeontology with regard to evolution.

The important new ideas in recent years, from sabkhas to sea-floor spreading, all seem to have come from universities (though not always from the obvious departments or disciplines). Certainly they come from universities rather than from industry or from government institutes. This is not a criticism of the latter, new ideas are not their business, but it is an important consideration when we discuss the place and purpose of universities in the present scheme of things.

This is why I always shudder when universities and higher education are spoken of in the same breath as though they were synonymous. Perish the thought. If we in the universities accept the doctrine that universities are merely organizations for educating the next generation, then certainly the people who hold the purse strings will be only too glad to take us at our word. There must be somewhere to produce new ideas. If we cannot now depend on the wealthy dilettante then we must rely on the universities or we all stagnate. Much research is futile, extravagant, wrongly motivated. But who is in a position to dogmatise what is worthwhile and what is not? What could be more ludicrously academic than

studying extinct microscopic organisms? The Victorians used to mount them in fanciful patterns. Yet what branch of "soft rock" geology has proved more economically valuable?

It is then worth considering the university environment in which the new ideas blossom. I have made a private list of countries and have gone round my friends in all aspects of geology and asked them "Have any important new ideas in your subject come out of country X or country Y?" Almost invariably the answers have been the same, as if I had only asked myself the question about my own specialities. The same countries figure high on everyone's list. We are partly influenced by language, no doubt, but in general I would say that among the rich countries that can afford luxuries such as academic research, the more rigid the system the less likely is it to produce something that is really new. The new ideas come from establishments that favour the original mind, the way-out thinker, the "odd-ball", the eccentric.

But I would not wish it to be thought that I am allowing my political prejudices (whatever they may be) to show. There are authoritarian professors and directors all over the world. What important contributions have been made in recent years by the old autocrats? No doubt they made them once, to get where they are today, but I can think of very little thereafter. What is more, in the departments of the old pattern, where *the* professor gives all (or nearly all) the lectures and is "second only to God Almighty", there is also very little emanating from his oppressed underlings Probably he doesn't allow it! It is surely significant that all the important British contributions to plate tectonics came from young lecturers and not from elderly professors. As Darwin once exclaimed: "What a good thing it would be if every scientific man was to die when sixty years old, as afterwards he would be sure to oppose all new doctrines".

Beware of great men! They are often the death of good science. I remember a (very liberal) French professor saying of geologists in another country: "They have too many popes". That is it exactly. De la Beche (as Director-General of the British Geological Survey) wrote to H. C. Sorby: "You are working at slaty cleavage! You have no business to do that. We have settled that question at this place thoroughly, and you have no business to go into it any more", (Higham 1963). Fortunately Sorby ignored the great man. I sometimes marvel that the really great men of our subject, such as Sorby & Holmes, ever became professors of geology!

Think how long the geologists of some countries have resisted the idea of nappes, of major lateral displacements, and think how recklessly the geologists of other countries have carried them quite unnecessarily, to almost every corner of the earth and to every orogenic belt. We have been so much "brain-washed" in Britain by the new ideas that we take lateral compression at the continental margins for granted and laugh at those who think only in terms of up and down. Yet now W. S. Pitcher has demonstrated clearly that tension and uplift dominate the Andes, which are supposedly on one of the classic plate margins of the world.

We must then consider how long it takes the important new concepts to penetrate the educational system. For the older revolutions it is very difficult to estimate, though it might be possible by studying collections of old examination papers (if they still exist).

Let us just consider a few examples. Agassiz and his ideas of continental glaciation received bitter opposition from geologists as eminent as Murchison. They were first proposed in 1840, but did not receive general acceptance before about 1852. W. Nicol provided the first thin sections (of fossil wood) to be published by H. Witham in 1831, but it was 25 years before that remarkable man Henry Sorby saw the potential of the method and published his first paper on thin sections of minerals in 1858. Evolution, with its impact on religious sensibilities, took even longer. It is not really mentioned in the history of the Geological Society of London published in 1907, in which Darwin only appears as a geologist. Undoubtedly, it only broke through into British geological syllabuses as a result of a few classic papers, notably Rowe's work on *Micraster* (1899), Carruthers on *Zaphrentis* (1910) and Trueman on *Ostrea-Gryphaea* (1922). Certainly it figures in Swansea examination papers from the graduation of our first honours graduates in 1923, but then Trueman was our first professor.

However, two years later, in 1925, there was held the famous evolution trial at Dayton, Tennessee, when a high-school teacher was found guilty of teaching a doctrine that was forbidden under state law. As late as the early 50's I had a student at an extramural class in London who came to hear about evolution because it was prohibited in her home country. Even today there is strong opposition to the teaching of evolution in schools by some fundamentalist groups in California.

Once the ideas man has produced the exciting new concept what happens to it? The two main bottlenecks in my experience are firstly the problem of publication and secondly the problem of reading everything important that is published. This is really another discussion, but I wonder if many like myself rely more on being sent the relevant reprints rather than on ploughing through the annual pyramid of paper. This is fine for the specialist and the specialist topic but what about the great new ideas? I know I was well behind my younger colleagues in catching up on plate tectonics (and probably haven't yet done so), but how to find the time? Such is the pressure of time that I have often found ideas

coming to me from the literature via my research students who have more time to read.

But in my experience, once the active-minded university lecturer reads or hears of the exciting new idea then it goes into his next lecture, sometimes the same day. I remember Deryck Laming bringing up the idea of a 40° rotation of Britain from the evidence solely of wind directions in the Permian, only later confirmed by the palaeomagnetists. An undergraduate complained to me shortly afterwards that he had been told about it by three different lecturers.

From advanced undergraduate courses the idea moves down to elementary courses and then to the schools. Some time after this it seems to get to industry and the government institutes, but perhaps I am being unfair and certainly the I.G.S. is to be congratulated on the speed with which plate tectonics was put on show in their splendid "The Story of the Earth" exhibit. They were more sluggish about geosynclines. It amused me to see the American concepts of "eugeosynclines" and "miogeosynclines" being forced on us, in effect, by the Russians, through the medium of the tectonic map of Europe. But I wonder how long it would have taken us in Britain to start teaching geosynclines at all if it had not been for a single presidential address by O. T. Jones (1938). Certainly the word "geosyncline" did not appear in Swansea examination papers before then, although the word had been coined by Dana way back in 1873. If I may digress for a moment, it is amazing how quickly our viewpoint on the importance of things changes with new ideas. I recently gave a lecture on early Palaeozoic events in western Europe and it was only after I finished that I realised that I had made no mention at all of the classic country of Sedgwick and Murchison. Then it came to me that my omission was not really of any great importance. But what would my predecessors have said, to whom the details of the Nant Pig Mudstone and the Gasworks Sandstone were almost Holy Writ?

Similarly, "continental drift" first appeared in Swansea examination papers in 1928 and "modern views on the permanence of ocean basins" came up a year earlier, clearly following Wegener's classic work. I certainly learned a lot about it as a student in the late 40's, though always with the sad rejoinder at the end that the geophysicists said it was impossible.

Plate tectonics seem to have got into the syllabus almost immediately. Within a year or two of the first important papers it had permeated through the courses in all but the most rigid departments and was into the schools almost as quickly.

I think I should pay tribute at this point to the "Open University" which has certainly done a tremendous amount in Britain to speed up the spread of new concepts in geology, especially those of plate tectonics, both by its activities and its publications.

We are, perhaps, rather too smug about plate tectonics and how quickly the ideas have penetrated the educational system all over the world (I have even seen a Chinese textbook with the familiar diagram of the descending subduction zone). But have we been so quick about other important new ideas? What about astroblemes? Now that we know from the moon, Mars and its satellites, Mercury and probably Venus (in spite of its thick atmosphere) that meteoritic impact craters are a general feature of the Solar System, how do we escape the conclusion that they must be generally distributed on this planet also, especially in the crustal rocks that were formed before we had an atmosphere? About a hundred have now been recognized around the world, but most are still strongly disputed. How many of us consider them as an unavoidable influence on our stratigraphical record and teach them in our courses? Even at the lively Department of Geology at Laurentian University which sits right on the Sudbury astrobleme (perhaps the most spectacular, and certainly the most economically valuable, of all such craters on earth) it was 5 years after the recognition of this as an astrobleme before it was included in the courses by a new young lecturer.

What about finite element analysis? This is a mathematical method which has been around for years in civil engineering and has an immense potential in geology. Now Professor Olek Zienkiewicz at Swansea has a N.E.R.C. research student working on its relevance to plate tectonics, in a Civil Engineering department.

Of course the only alternative to the old autocratic professor is some form of democracy within teaching institutes. But democracy takes time. If all the staff and perhaps all the students as well are to have their say, then the most trivial decision can take an age and none of us gets any work done. There is a lot to be said for benevolent dictators, so long as there is a mechanism for removing them when they cease to be benevolent. After all, why is it only the universities that have to be democratic in this respect?

Besides the prejudices of the academic head, there are also the prejudices of the publishers of journals. Look at the resistance to Nancy Kirk's new ideas about the mode of life of graptolites. I don't know whether she is right or wrong, but like Voltaire I would fight to the death for he right to publish her ideas. In the end she had to publish them through a special publication produced by her department at Aberystwyth.

It has given me great pleasure as Editor-in-Chief of the international journal "Palaeogeography, Palaeoclimatology and Palaeoccology"—to push through an exciting new paper by Terence Hughes suggesting that

the Cordilleran belt only moved across the Pacific to join North America in Mesozoic times. I don't know if he is right, though it makes sense of a lot of things that have puzzled me, but I am sure this is an idea that should be considered.

If I may air my own prejudices here, it seems to me that there is a basically new approach in geology, which is sometimes called "neocatastrophism", and which is not yet regarded as really respectable. To me the whole trend of interpretation, from turbidity currents to the collision of plate-carried continents, is towards the rare event rather than the dull continuum. Similarly in palaeontology, to use the jargon of S. J. Gould (1971) "phyletic gradualism" has been replaced by "punctuated equilibria". In other words, palaeontologists are no longer thinking of general, slow continuous change in species, but rather of a continuing state of quiescence which is interrupted periodically by sudden, sharp "happenings".

When I tried to preach this sort of attitude in my book "The Nature of the Stratigraphical Record" (Ager 1973) I found myself the recipient of everything from violent condemnation (of course quite unjustified!) to ecstatic praise. But I do not yet see many signs of these attitudes penetrating the educational system.

But what is it fundamentally that speeds up or delays the dissemination of new ideas through the educational system and in particular through the universities? In the long run it is a matter of deep heart-searching and often dispute within departments when it comes to the allocation of time in timetables. When I was a student, geology consisted of stratigraphy, palaeontology and petrology (the last largely consisting of a forgotten specialzation called "micropetrology"). There are still university departments and other geological organizations in Britain which hardly yet recognize subjects such as "sedimentology". The introduction of new subjects and new ideas could only be done at the expense of others and inevitably stratigraphy (in the classical sense), taxonomic palaeontology and petrography have been the main sufferers.

Of course there has been resistance, not entirely unjustified, by the traditionalists who see the standards of scholarship falling. "Students can't recognize fossils and minerals any more". It is easy to poke fun, but will aknowledge of plate tectonics be more or less valuable than a knowledge of ore minerals to a man who is going to spend his life in the mining industry? Who are we training? I shudder to think of any student of mine going out into the world (like one very good American graduate I know) never having heard of belemnites. Referring once again to Sudbury, because of a recent visit, how many of our students would have recognized the nickel minerals in the cutting being made for the Canadian Pacific Railway near Sudbury?

Even the most exhilarating flights of geological fancy depend on much basic knowledge. Many might think it superfluous to tell students of British stratigraphy about that trivial little patch of Lenham Beds high on the North Downs, yet these are an essential ingredient in any theorizing about the later geological history of this part of Europe. If I may do a little advertising, it seems to me that Dick Owen of Swansea is a superb example of the combination of the old and the new approach. He took plate tectonics and all the recent findings on the continental shelf directly into his teaching, but instead of airy-fairy theorizing about the geological evolution of Europe as some of us would do, he showed how these new ideas related to and made more sense of his detailed knowledge of the stratigraphy of Wales. And of course he did it with his usual Welsh fervour!

Again it is autocracy that delays progress in the dissemination of new ideas just as it does in their original flowering. I am all for the young lecturers being given their heads even if they do at times expect too much of their students and lose sight of the wood because of all those beautiful trees. H. H. Read used to say "let me get at them first and then you can teach them what you like". I think the elderly professor (providing he is not *too* out of date) is probably best used teaching the first years and the new young lecturer teaching the advanced courses. They will both do least harm that way!

In a sense this is moving towards the American system where, in effect, every teacher is his own department. Each "professor"—in the American sense—teaches his own courses quite independently of everyone else, he sets and marks his own exams, he gets his own research money and equipment, he often employs his own secretarial and research staff and in some cases he even pays his own telephone bill. The mobility of American university teachers has a lot to be said for it when compared with our system in which we are appointed soon after puberty, and stay there until retirement but for the gamble of a chair. Personally, I think that all senior appointments should be automatically advertised and perhaps be only for five years at a time. Our system does too much for the stick-in-the-mud (and the AUT doesn't help). The American system has its drawbacks, of course. The teaching of a subject lacks integration, the student doesn't belong anywhere and a great deal of time as a post-graduate student has to be spent filling in the gaps left by inadequate undergraduate coverage.

The head of a department is then literally just the chairman, and it's usually a very unpopular job. The department frequently lacks direction and continuity. But it can be a very good system for training both the so-called "ordinary" geologist and the future "ideas"

man. The main danger, perhaps, is the power of the individual teacher. With no constraint by external examiner or even by his own colleagues, the individual teacher can be the greatest totalitarian of all. What worries me most about the American type system is the tremendous pressure towards conformity. The class grade follows you about in your "transcript" for the rest of your life, so there is a great tendency to regurgitate what you are told and to expect the clear factual answer.

I once asked a question in an American university examination something to the effect "You can put down a deep borehole wherever you like in North America. Where would you put it and why?" After that exam four students came up and asked what was the correct answer to that question! That is not the spirit to encourage if we want to foster independent thinking. If an authoritarian professor doesn't like (or doesn't read about) a new idea, he can keep it out of his courses for 40 years.

It all comes back to who we are trying to teach. It we think we are teaching the next generation of Darwins and Einsteins we'd best give up—they won't come up that way—and if they do it will be in spite of us not because of us. If we think we are training the next generation of geological technologists, then we'd best scrap the fancy stuff and stick to Woods and Rutley. If we think (as most of us seem to do) that we are teaching the next generation of researchers and university teachers, then O. K. roll out the latest theories, but I'm not sure that we are really doing our job that way.

Personally, I stick to the old-fashioned belief that the only things worth learning are those which we teach ourselves. Then the answer is simple. Our first priority in the universities should be to provide the right atmosphere of interest and enthusiasm for the subject. Everything else will follow from that. Let the students read for themselves, think for themselves and speak for themselves. Let our young lecturers do likewise. Then we shall have the speediest possible dissemination of new ideas, even if some of them are wrong.

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