Fundamental university research in the geosciences and its application to the exploration and production of hydrocarbons

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It is not by accident that the Petroleum Science and Technology Institute (PSTI) has selected this department for the creation of a new chair in Petroleum Geoscience. In the last few years the approach of the department to the solution of problems in the earth sciences has evolved into a multidisciplinary one because it is recognized that some of these problems cannot be tackled any other way. In addition, a number of new problems have been approached with special application to the oil industry, particularly by new research staff who joined the department after the University Earth Science Review in 1989. The University of Edinburgh and PSTI saw an opportunity to build on this strength and create a new unit within the Department of Geology and Geophysics, called the Predictive Geoscience Research Unit, to focus primarily on the problems of exploration and production of hydrocarbons. The existing capability in this field was to be expanded, in collaboration with the British Geological Survey, by the addition of a Shear Wave Analysis Group, headed by Professor Stuart Crampin, and by the creation of the new chair in Petroleum Geoscience, funded by PSTI. The new professor of petroleum geoscience also heads the unit.

We have been in existence for less than three weeks and we still do not have a research plan for the coming five years. Nevertheless we can identify a number of fundamental problems that need to be worked on and I will talk about them later on.

Before I do that I want to say a little about four issues which, in the spirit of cooperation that exists here now, are easily taken for granted. The first is the integration of geology and geophysics. The second is the problem of academic preoccupation with pure science. The third is the depletion of nonrenewable hydrocarbons. The fourth is the question: What is predictive geoscience?

Integration of geology and geophysics. It is obvious to most earth scientists that our understanding of the Earth and its interior can progress only if there is cooperation between geologists and geophysicists. There is cooperation, but it has not always been so.

At the end of the 19th century, there was enormous disagreement between the physicists on one hand and the geologists and biologists on the other, about the age of the earth. The physicists were of the opinion that the earth could not be older than about 90 million years, and was probably not older than about 20 million years. The sedimentologists pointed to the great thicknesses of sediments and the current rates of deposition and showed that they needed much more time to accumulate the existing quantity of sediments. The paleontologists and biologists pointed to the evolution of animals over geological time and argued that it was impossible for these changes to have taken place in less than several hundreds of millions of years.

The physicists would not let them have more than 100 million years. They had two strong arguments. The first, applied by Kelvin, was that heat flow measurements show that the solid earth is cooling down, and would have been molten less than 100 million years ago. With better data he later revised this estimate down to 20 million years. The second, applied by Helmholtz, was that the sun could not have been in existence for more than about 25 million years. This argument was based on the sun’s rate of loss of energy and the theory that this energy is maintained mostly by gravitation; 25 million years ago the sun would have consisted of matter very widely dispersed and radiating very little heat.

The geologists and biologists said that they needed much more time, but they could not find fault with the arguments of the physicists. The physicists, though intrigued by the arguments of the geologists and biologists, could see no way out of the physical constraints imposed by their measurements and theoreies, and began to lose patience with the far less quantitative approach of the geologists and biologists. They stopped talking to each other.

After the discovery of radioactivity, the physicists, and Rutherford in particular, showed how this hitherto unknown source of energy now put the age of the...
Earth at about 5000 million years. This satisfied the geologists and biologists, but they were never quite able to stomach what they regarded as the conceit of the physicists.

Today’s geophysicists are really physicists who are interested in applying the methods of physics to understand the physical properties of the earth. They are trusted by geologists generally about as well as physicists are. My professor of geophysics, Sir Edward Bullard, was taught by Rutherford. He was fond of telling us that Rutherford said there were only two sciences: physics and stamp collecting.

In oil companies the need for the integration of geology and geophysics has always been recognized, but it has not always been easy to achieve. Real integration occurs, not because management insists on it, but because the problems demand it. In a little book published as recently as 1982, entitled Simple Seisimics, Nigel Anstey, a famous geophysicist, describes the work of an interpretation geophysicist struggling with his seismic data and trying to make geological sense out of it. When the difficulty is particularly abstruse, he says, “the resolution of the problem may even force him to make a major sacrifice for science, and to take a geologist to lunch.”

The line between exploration geologist and exploration geophysicist is disappearing. Nowadays, especially with the cutbacks in exploration and production staff in many major oil companies, both geologists and geophysicists try to increase their appeal to management by calling themselves “explorationists.”

Pure and applied science. There is a kind of academic arrogance which says that scientific research is not really “pure” if it is useful. It is then either technology or engineering in some derogatory sense. Even in The University of Edinburgh the Faculty of Science and Engineering has been in existence for only a few years. Before that it was the Faculty of Science.

In the earth sciences, it used to be very much frowned upon to be working on problems concerned with oil exploration and production. Those who stooped to pick up the scraps offered by the oil industry were regarded by their colleagues as belonging to an inferior caste. There are universities—not in Edinburgh of course—where this attitude is still prevalent. In some earth science departments this caste system is formalized, and the lower caste who work on oil industry problems are relegated to some inferior building or annex.

The desire to do pure research in the earth sciences is not restricted to academics of course. Learned papers are also produced by earth scientists in industry.

In 1976, I began work for the National Coal Board and shared an office with Mike Allen, a geologist. Together we coordinated the National Exploration Programme for new and existing coal mines: he did the drilling and I did the seismics. One day he received a call from an area geologist about the results of a borehole to prove an extension of a known coalfield. The area geologist was very excited about some fossils that had been found in the core and was planning to write a paper about them. Mike Allen was very patient with him and then, when had heard most of the story about the fossils, he very politely asked him: “Was there any coal?” It turned out there was a 10 m thick seam which the area geologist had almost forgotten to mention.

To a great extent the snobbery that is associated with “pure” earth science is eroding. One reason is that many higher caste earth scientists recognize that the oil industry has problems in exploration and production which are genuinely interesting in their own right. Another is that government funding in the earth sciences is insufficient to support the research aspirations of many pure earth scientists, and they see the oil industry as a convenient source of money.

The industry is only too aware of this. And it is also very aware of the academic arrogance that requires money for pure science. Many people in the industry have a well-founded fear that any money put into university research will have no direct benefit to the industry. Even if the academics have every intention to work on a problem that the industry needs to solve, the peer pressure to be “pure” is great, and the emphasis of the research often shifts to related problems that carry greater academic prestige, but have less direct relevance to the industry problem.

Even in the research departments of oil companies there is a tendency for some research to become more pure and less obviously relevant. Sometimes this cannot be avoided if the initial problem is to be solved. It takes a particularly good manager of research to know which is the best research for the company. Right now these managers are having a very difficult time with their manage
cements. In a number of oil companies, particularly in the United States, research is now being cut very severely. A number of my friends in research in different oil companies who have not yet lost their jobs have told me that they believe their companies will cease to do fundamental research in the future.

Even BP, with only Forties and the dwindling Alaskan North Slope as its major sources of oil, is cutting research. It is hard for explorationists to understand the rationale for these decisions. One of BP’s explorationists told me it appeared to him that BP was burning its bridges before it crossed them.

Research within oil companies is currently diminishing. However, there is a growing appreciation that research can be contracted out. This is obviously good news for universities. The bad news is that the people in charge of contract research money in oil companies want results. They are a hard-nosed lot and they are extremely good at detecting the weak points in research proposals.

In my experience, we cannot go to an oil company with a half-baked proposal. We have to know exactly what we plan to do, how we plan to do it, how long it will take, how much it will cost, and exactly which problems you have to solve along the way. We must present it only after very careful thought, and then be prepared for criticism. This is very likely to come, because it is very likely that our conception of the way things are done in the industry is incorrect. If there is really a good idea at the core of the proposal, and we are prepared to listen to the oil company representatives, and accept their criticism, we are then in a position to rewrite the proposal.

Oil men have no time to tell us their problems; we have to discover what they are, using whatever means are available. If our research proposals are criticized, it is probably not from a misunderstanding on the part of the oil company, but because there is something wrong in what we are trying to do, and we should learn from the criticism.

The diminishing reserves of fossil fuels. In these days of growing awareness of pollution and waste, we are all conscious that the reserves of oil, gas,
and coal are finite, and that we will one
day use them up, if we go on as we are.
Should we, then, go on as we are?
This question does not have a simple
yes or no answer. Obviously we cannot
keep on burning oil and coal hoping that
something will turn up. And we are not
doing this. Developments of other
forms of renewable energy are taking
place: foresters are finding how to grow
trees in less than 20 years, fuel can be
made from sugar, and so on. I believe
one of the biggest unsolved energy
problems is the storage of nuclear
waste. We do not know what to do with
it. We are not able to agree on places
where we can safely store it. Until that
problem is solved, the opposition to the
building of new nuclear power stations
will obviously be maintained.

The way we have squandered our
resources is a tragedy. For example,
only 10-15 percent of the coal in the
Northumberland and Durham coalfield
has been extracted, and now there is
almost nothing left; the remaining 85-
90 percent is still in the ground of
course, but it is in awkward-shaped
pieces which cannot be extracted with
present technology and current prices.
So the coal mines are closed down and
gradually fill with water, making re-
opening very difficult if not impossible.

The price of coal is determined by
the price of oil. No one has control over
the price of oil, and the industrialized
world is prepared to go to war to keep
that way. It can be argued that this is
the real reason for the Gulf War. We had
experienced enough inflation and un-
employment following the oil price
risers of 1973 and 1979 to know what
was at stake if Saddam Hussein was
allowed to control the oil reserves and
production from Iraq, Kuwait, and
Saudi Arabia, and we were simply not
prepared to let it happen.

So I believe it is fair to say that
market forces are currently controlling
the price of oil. I believe there are no
currently viable long term alternatives
to the use of oil and, while this is the
case, I firmly believe that earth sci-
entists have a duty to use their skills to find
the oil and to enable the oil to be pro-
duced in such a way as to leave as little
behind as possible.

What is predictive geoscience?

Every scientist should know something
about the philosophy of science. Geo-
science is earth sciences. But what is

science?

In 1734 David Hume published A
Treatise of Human Nature. In it he
proved that it was impossible to prove
that a scientific law was true. Take
Newton’s law of gravitation. Together
with Newton’s laws of motion, it ex-
plains all measurable planetary motion
and has been used to send men to the
moon and to get them back. We know it
works. We depend on it in our daily
lives. However, our experience of the
validity of Newton’s law of gravitation
is infinitesimal compared with the total
possible range of experiences in the
whole universe, and therefore the logi-
cal probability that Newton’s law is uni-
versally true is zero.

Hume was very uncomfortable with
this conclusion. If the logical probabili-
ty of a law of physics or any other
natural law is zero, how do we live from
day to day? Why do we think the sun
will rise tomorrow?

This remained a problem until the
work of Karl Popper. In 1919 Popper
was intrigued by three important theo-
ries: Marx’s theory of history, Freud’s
theory of psychoanalysis, and
Einstein’s theory of general relativity.
He felt that Einstein’s theory was some-
how “more scientific” than the other
two. What particularly concerned him
were two related problems. First, what
is it about Einstein’s theory that makes
it more scientific? Or, more generally,
what criterion separates a scientific the-
ory from a nonscientific one? And sec-
ond, why does it matter whether a the-
ory is scientific or not?

What most impressed Popper about
Einstein’s theory was Einstein’s own
clear statement that he would regard his
theory as untenable if it should fail in
certain crucial tests. Popper recognized
that Einstein’s attitude was entirely dif-
ferent from what he called the “dog-
matic attitude” of Marx, Freud, and
Adler. The dogmatic attitude constantly
claimed “verifications” for its theories;
the critical attitude was looking for cru-
cial tests which could refute the theory
tested, but could never establish it.

By the end of 1919 Popper had de-
cided that the scientific attitude was the
critical attitude. A few years later he
proposed his now famous demarcation
criterion for distinguishing between sci-
entific and nonscientific theories. A sci-
centific theory is formulated in such a
way that it deliberately puts itself at risk,
in the sense that it can be tested and
refuted. If a theory is not framed in this
way, then, according to Popper’s de-
marcation criterion, it is not a scientific
type. He agreed with Hume that we
cannot prove our theories are right. We
can only try to prove that they are

wrong.

This is a big point. The meaning of
the word “scientific” is a very emotive
issue. If you ask any researcher whether
his work is scientific, he will always tell
you it is. And yet much of so-called
science falls on the wrong side of
Popper’s line of demarcation. I know by
experience that you can become very
unpopular if you point to specific ex-
amples, so I shall not give any here.

Why does it matter whether a theory
is scientific or not? If we divide up the
theories we use into the two kinds which
are separated by Popper’s demarcation
criterion, we have on the one hand those
which are in principle testable, and on
the other those which are in principle
not testable. And let us, for the sake of
argument, agree with Popper to call the
first set of theories “scientific” and the
second set “nonscientific.”

Now whatever we do, whether it is
looking for oil, driving a car, or sending
men to the moon, we put our trust in
theories. When we send men to the
moon, we have to trust a lot of theories.
What is the basis of our trust? Well, of
course, we have tested our theories. The
theories we trust the most are the ones
which have survived the most rigorous
tests. Every time a theory survives a new
critical test, in which it stands at risk of
being refuted, it increases in value. If a
theory fails a critical test, we must dis-
card the theory. So this testability of a
scientific theory allows us to determine
how much we can trust the theory.

The theories in the other class have
not been tested and are untestable. We
are not able to find out how much we
can trust them.

In predictive geoscience we make
predictions based on our measurements
and our theories about the interior of the
earth. The theories we use to make the
measurements and to reveal the secrets
of the earth must be scientific in
Popper’s sense if they are to be trusted.

What we are trying to do is to under-
stand the earth. We do it by mapping it,
measuring it, taking rock samples, dril-
lng holes and looking in them, and so
on. In principle, our methods of investi-
gation are the same whether we are
looking for oil or whether we are look-
ing at the deep ocean floor, trying to
understand the forces of plate tectonics.
And in principle, therefore, the distinction between pure earth science and applied earth science is only imaginary. I know of one earth scientist whose intelligence is supposed to be purely imaginary; even if you square it, it doesn’t come out positive.

Some fundamental problems. Within the Predictive Geoscience Research Unit we have formulated some goals. As we work together, we will no doubt refine them. Currently we have four goals:

• To understand the factors influencing the origin and character of hydrocarbon reservoirs.
• To develop and apply techniques to improve reservoir characterization and the prediction of characterization of reservoirs.
• To determine the physical and chemical factors that affect the economic development of hydrocarbon reservoirs, and the means by which these factors can be predicted.

1 To disseminate the results of our research to industry.

These are very general goals. To someone with no special knowledge of the oil business, they probably read as if they were written by a committee of civil servants. I shall try to illustrate what we are trying to do with some examples.

First, we have to find the oil field. Consider the North Sea. How do we explore it and find oil fields? Without geophysics, the only way to find them is by drilling holes at a cost of several million pounds per hole. The fields are a few kilometers across. Even the biggest field, Forties, is not more than 30 km long. In order to stand a good chance of finding these fields systematically by drilling, you would have to drill a grid of holes at, say, 5 km centers. This would be prohibitively expensive and has never been considered.

Nearly all offshore fields are found by the seismic reflection method and on today’s modem, large vessels, about a megabyte of information is digitally recorded every 10 seconds during normal operations. If the vessel is using several multichannel cables and performing a survey that covers several thousand kilometers, the data volume can run to hundreds of gigabytes.

We now come to one of the fundamental problems. The processing of this huge amount of seismic data depends on the theory of wave propagation and the theory of time series analysis. The earth is a linear system and we can treat it as a black box with an input, the bang made by the source, and an output, the received signal at a given hydrophone. If we know the output and the input, we can get the earth response. Normally only the output is measured. The input is usually estimated from the data. I think it should be measured. This Can be done, but it is not.

There are two reasons. First, before we were able to invent methods to measure the input, which occurred only within the last 10 years, the whole industry of wavelet estimation grew up within the seismic industry. Everyone is generally satisfied with the results and they see no good reason to change. Secondly, the way the whole contracting system is set up, it is very difficult to change things. The change will only take place if the oil companies see an advantage. It someone gets better results by changing, the other companies will follow. What is required is a crucial experiment to show that the change will pay for itself.

This kind of information is not important when we are simply trying to find the main geologic structure: the faults, salt domes, unconformities, etc. But it is critical when we try to extract the density and velocity contrasts at layer interfaces. The point is to find out how these contrasts vary laterally, because these vary when hydrocarbons are present. We want to get this information directly from the data. If we can do this, we should be able to predict what these contrasts should be at the well. This would form the basis of a crucial experiment to compare these alternative methods. In fact, even to tie seismic data to well logs can still be a major problem.

One new method we hope to investigate in the near future is a combination of seismic and electromagnetic methods. In some places the variation in porosity can be too small to detect by seismic methods, but big enough to make a difference in production. In one reservoir I know of, porosity varies laterally from 3-20 percent, and these variations are almost impossible to see on the seismic data. If the porosity is over 12 percent, the oil can be produced; if below 12 percent, it cannot. We need another technique. Now the electrical conductivity can be very sensitive to these lateral variations, depending on the fluid content. We should therefore use seismic methods to pin down the geologic structure, and electromagnetic methods to define lateral variations in electrical conductivity, calibrated and tied-in with the seismic data at boreholes. We are beginning a three-year project in collaboration with companies and universities in France and Germany to establish the viability of this approach.

A further development called crosswell seismology will allow us to investigate reservoir structure in an existing oil field from the actual wells. This is an exciting new field of exploration seismology in which the technology is in its infancy. The reason we need to do this is that the resolution of seismic data obtained at the Earth’s surface is not good enough. The problem is that the smallest things we can see from the surface have dimensions of about tens of meters. We cannot see them any sharper because of the noise at the surface and the attenuation of high-frequency energy. From the wells we can get much closer to the target. This has two advantages: first, the noise level is about 1000 times smaller than at the surface, and secondly, the attenuation is much less.

In a crosswell experiment conducted in 1990-funded by Norwegian oil companies and carried out by the Norwegian company Seres at a site in the Groningen gas field in the Netherlands, provided by NAM—we were able to demonstrate that this new method is able to improve the resolution by a factor of 20 and will allow us to see things on the scale we need.

There are many developments to be made in this field. For example, there is no production downhole equipment available to the industry to obtain the required data, and the methods of processing these kinds of data are in their infancy.

Several major new projects related to the exploration and production are already going on in this department. The results are changing current understanding of reservoir behavior and are likely to affect the way we go about trying to produce the oil more efficiently in the future.

There is, though, an awful lot to do and I am very much looking forward to doing it at the Predictive Geoscience Research Unit.