Geophysical Prospecting 30, 155–165, 1982.

FURTHER THOUGHTS ON POPPERIAN GEOPHYSICS—THE EXAMPLE OF DECONVOLUTION*

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Abstract

Ziolkowski, A. 1982, Further Thoughts on Popperian Geophysics—The Example of Deconvolution, Geophysical Prospecting, 30, 155–165.

Popper's demarcation criterion should be applied to all our theories in geophysics to ensure that our science progresses. We must expose our theories to tests in which they stand some risk of being refuted. But if we have a theory which has no rivals it may be difficult in practice to devise a test in which the theory risks being refuted conclusively.

The example of the deconvolution problem for seismic data is considered for the case where the source wavelet is unknown. It is shown that all our existing theories of deconvolutions are not scientific in Popper's sense; they are statistical models. We cannot compare these models in a way that is independent of the geology, for each model requires the geology to have a different set of statistical properties. Even in our chosen geology it may be extremely difficult to determine the most applicable model and hence determine the "correct" deconvolution theory.

It is more scientific to attempt to solve the deconvolution problem (a) by finding the source wavelet first, deterministically, or (b) by trying to force the wavelet to be a spike—that is, by devising a "perfect" seismic source. A new method of seismic surveying, which has been proposed to tackle the deconvolution problem by the first of these approaches, is based on a theory which is open to refutation by a simple Popperian test. Since the theory makes no assumptions about the geology, the test has equal validity in any geology.

It pays to frame our theories in such a way that they may easily be put at risk. Only in this way will we establish whether we are on firm ground. The alternative is simply to take things on trust.

Introduction

In his Presidential Address to the 42nd meeting of the EAEG in Istanbul, Prof. Parasnis (1980) does a great service to geophysicists by leading us to the works of Karl Popper, a philosopher who has devoted so much of his life to understanding and defining the methods of scientific discovery. If we were more aware of Popper's contribution to the philosophy of science, we could possibly become better scientists

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0016–0825/82/0400–0155 $02.00

and be more successful geophysicists. I support the main thrust of Parasnis's argument and should like to follow it a little further.

But first there are two points in Parasnis's address which should be briefly discussed because they might confuse anyone not familiar with Popper—and, according to Parasnis (p. 667) that includes most geophysicists.

First, on p. 671, Parasnis says "The great divide between scientific creations and nonscientific ones like music, painting, poetry, etc. is . . . that scientific creations are the outcome of continual new formulation and systematic falsification of unambiguously posed theories, this being Popper's criterion of science, while nonscientific creations like painting or poetry are not theories and cannot therefore be refuted."

The distinction between science and works of art is not at issue. It is conceivable that we may have a problem in distinguishing between the two, but Popper is not concerned with this. The problem which Popper faced was: How do we distinguish between scientific theories and nonscientific ones?

Secondly, also on p. 671, Parasnis says "The scientific theories in disciplines like history, archaeology, literary criticism, etc. are (as a rule) not quantitatively testable, nor meant to be so, but this need not make them less ambiguous nor less scientific."

What is it, then, about the theories in these disciplines, that makes them scientific? Parasnis does not explain; but it is precisely the distinction between scientific and nonscientific theories which forms the very core of Popper's philosophy.

The remainder of this note is divided into two parts. The first part discusses Popper's demarcation criterion and questions a philosophical point made by Parasnis concerning the possibility of conclusive refutation of a scientific theory. The second part applies this criterion to theories of deconvolution with the result that they are found to fall into two classes: those which are scientific in Popper's sense and can be tested unambiguously; and those which must be regarded as models, rather than theories, and which cannot be refuted by a test. This second class includes all statistical deconvolution techniques which now form a standard part of normal seismic data processing.

Popper's Demarcation Criterion

What makes a theory scientific? When Popper faced this problem in 1919 he had in mind such theories as Einstein's theory of relativity, Marx's theory of history, and Freud's theory of psychoanalysis. He felt, as perhaps many people would, that Einstein's theory was somehow "more scientific" than the other two. But what made it more scientific? What was the criterion?

He concluded that what distinguishes a scientific theory from a nonscientific one is this: *a scientific theory* is formulated in such a way that it *deliberately puts itself at risk*, in the sense that it can be tested and may be refuted. If a theory is not framed in such a way that it stands some risk of being refuted by a test, or by new evidence, or by some future event, then, by Popper's demarcation criterion, it is not scientific. Such irrefutable theories may still be useful in shaping our thinking and our attitudes, but they should not be confused with science.

The reason that it matters whether a theory is scientific or not is that any theory may be regarded as "the truth" at the time, until it has been shown to be false. Theories which consistently pass tests in which they are put seriously at risk obviously gain credence. But so also do theories which are supported by propaganda. The propaganda may not be obvious. It may seem to be pure, rational, scientific reasoning. However, we should beware of theories which have never seriously been put at risk. Popper himself has vigorously applied his criterion to attack what he regards as very dangerous theories, particularly Marxism, which are believed to have some sound basis in science, but which have, in reality, been formulated in such a way that they are insured against falsification. In other words, they cannot be put at risk in any way, and therefore you either believe them or you do not; either way, they have nothing to do with science (see, for example, "The Poverty of Historicism" and "The Open Society and Its Enemies", especially chapter 25: "Has History any Meaning?").

Parasnis is quite right, therefore, to urge us not to try to *prove* theories (which is an impossible task in any case), but to try to *disprove* them. If the theory passes the test, it increases in value. If it fails, we need to find a new theory.

At the end of his address, Parasnis finishes on an optimistic note, with the statement "it is possible to *conclusively* disprove or falsify a scientific theory" (my emphasis). Is this statement correct? What do we regard as sufficient refutation to justify discarding a scientific theory?

Imre Lakatos, a colleague of Popper's, showed (1970) that the problem with Popper's criterion in practice is to find a valid test, particularly when there are no rival theories. Thus, although a theory may have been refuted time and again, these refutations are not recognized as such until perhaps much later, when they come to be cited as "crucial experiments". Until then the existing theory is augmented with auxiliary hypotheses which "explain" the observations which do not fit.

Thomas Kuhn goes further than this in his "The Structure of Scientific Revolutions" (1962), in which he describes the process by which a scientific community will hold on to theories which explain some, but not all, of their observations, until a new theory or discovery comes along to alter their thinking. Until this happens they are imprisoned in a kind of mental trap, which Kuhn calls "a paradigm". It is a property of this trap that the prisoners do not know they are imprisoned.

Lakato's point was anticipated to some extent by Popper (1960, p. 241) who says that a scientific theory "must have new and testable consequences (preferably consequences of a *new kind*); it must lead to the prediction of phenomena which have not so far been observed". Without this indispensable requirement, it will always be possible to patch up the theory with ad hoc hypotheses, which are designed to explain the observations which do not fit the theory. However, unless the additional hypotheses have new and testable consequences, they do not strengthen the theory or extend scientific knowledge. In other words, they add nothing.

Popper has very little patience with Kuhn's concept of paradigms, which he has dubbed "The Myth of the Framework". He argues (1970, p. 56) "I do admit that at any time we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But we are prisoners in a Pickwickian sense: if we try, we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one, and we can at any moment break out of it again."

He is therefore an optimist about this. But, further, he regards Kuhn's thesis as dangerous (1970, pp. 56 and 57): "The Myth of the Framework is, in our time, the central bulwark of irrationalism . . . it simply exaggerates a difficulty into an impossibility . . . . Thus in science as distinct from theory, a critical comparison of the competing theories, of the competing frameworks, is always possible. And the denial of this possibility is a mistake."

But what happens when the theory has no competitors? Popper points out that there are *degrees of testability* (1960, p. 256): "Some theories expose themselves to possible refutations more boldly than others . . . . A theory which is more precise and more easily refutable than another will also be the more interesting one . . . . But it is better testable, *for we can make our tests more precise and more severe.* And if it stands up to severe tests, it will be better confirmed, or better attested, by these tests."

If it fails, we must discard the theory. But what do we put in its place if we do not have another theory? Before we discard the theory we are likely to make sure the test has been carried out properly. In fact, the test will come under as much scrutiny as the theory, especially if there are no rival theories. And if we find nothing wrong with the test, but cannot see exactly *why* the theory has failed—although we can see from the test exactly *how* it has failed—we should, logically, discard the theory. In practice we will worry over this state of affairs, lose sleep over it, and be in two minds over it, until we solve the problem. But until that happens, or until a rival theory comes along, we will hang on to the original theory with its faults which we do not quite understand. Whether we are in one of Kuhn's paradigms, or whether we recognize this difficulty and struggle to break out of our framework, depends, Popper would argue, on us.

Until we solve the problem and find out *why* our theory leads us to predict something which appears to be false, we are stuck, as Lakatos has pointed out. We may never solve the problem. The theory will remain in serious doubt, but will not have been conclusively falsified. To say that it *is* possible to falsify it conclusively is metaphysical. It is equivalent to claiming that we *will* solve the problem of why the theory makes a false prediction, which means that we are able to replace it with a better theory. Yet there is nothing inevitable about any of this; this is only something which, as scientists, we should be striving for. In my view, Parasnis is quite right to urge us to strive for it, but he is too optimistic in claiming that we will always succeed.

As geophysicists, we are part scientists and part engineers. We apply our science to solve practical problems, but of course we do not always succeed. We make mistakes, and we try to learn from them, so as to avoid them in the future. If, for example, after much surveying and analysis of the geophysical data, we decide to drill an oil well which turns out to be bone dry, we try to find out where we went wrong. We recognize that the geological model we have constructed from the geophysical data, using our theories, is wrong. We therefore analyse both our data and our theories to find our mistakes. How deep does this analysis go? Probably, due to limitations of time, it does not go as far as we would like; on the other hand, perhaps it does not go as far as it should, for another reason. Perhaps we are sometimes reluctant to question basics—for reasons which Kuhn would regard as quite natural.

As in all sciences there are steps in the theory which are regarded as firm, but which will in due course surely be shown to be quite erroneous. This has happened many times before. Indeed, unless it does happen the science will not progress. So we know that some of what we do is likely to be wrong, but we are completely lost if we question everything at once. Obviously, we must try to question each step in turn and, according to Popper, put the theories at risk in some way.

The Application of Popper's Demarcation Criterion to Theories of Deconvolution

Statistical techniques

Consider the theories of deconvolution. This is an area which has been a very fruitful one for research over the last 20 years or so, but there is no single method, or theory, which is guaranteed to work on every piece of data. Which theory is the best? Which is the one which has been put most at risk and passed the tests?

We are bound to say that some methods work better on some data, some work better on others and some hardly work at all. The one we *use* every day, almost without question, is that of Robinson (1957), extended by Peacock and Treitel (1969) and applied in practice using the Wiener–Levinson algorithm (Levinson 1947).

We know, by experience, that for some kinds of data (for example, explosive land data with a short-period peg-leg multiple problem) it can do a superb job. On these kinds of data the premises of the theory fit the data well. On other kinds of data, where the premises clearly do not fit, it does not seem to damage the data very noticeably. In other words, it works if it can. It is robust. As engineers we value its robustness, but as scientists we are looking for something which will work even better—a theory in which the premises fit the data more of the time, and which still has this important robustness. Hence the constant stream of papers on deconvolution.

The basic problem that these theories of deconvolution try to solve is the extraction of the earth impulse response $g(t)$ from the reflection seismogram $x(t)$, uncontaminated by the source far field wavelet $s(t)$. In the absence of noise, these three quantities are related by the convolution equation

$$x(t) = s(t) * g(t) \tag{1}$$

where the asterisk ($*$) denotes convolution, and the convolution arises as a direct consequence of the assumption that the propagation of seismic waves through the earth is according to the theory of linear elasticity. The theories of deconvolution aim at an accurate estimate of $g(t)$, given $x(t)$, even when $s(t)$ is unknown.

In addition to the extraction of the wavelet, there is also the problem of the prediction and subtraction of multiples. Since the kind of filter which tries to perform this operation for us—namely the prediction-error filter—is designed and applied in exactly the same way as the source wavelet deconvolution filter, this suppression of multiples is regarded as part of the deconvolution process. This implies that the impulse response $g(t)$ is regarded as the convolution of a multiple sequence $m(t)$ with a primary sequence $p(t)$:

$$g(t) = m(t) * p(t). \tag{2}$$

In this equation $p(t)$ is simply the impulse response without the multiples, and contains all primary reflections from the three-dimensional earth below the source and receiver, as well as any diffractions and refractions that occur within the time window.

The combination of equations (1) and (2) yields

$$x(t) = s(t) * m(t) * p(t). \tag{3}$$

Some of the theories of deconvolution aim to solve equation (1) for $g(t)$; others try to solve equation (3) for $p(t)$. In the remainder of this note we consider only the problem of removing the source wavelet. The problem of multiples is seen as a separate problem.

> It is easy to show that the multiple sequence $m_1(t - t_1)$ which follows the arrival of a primary event at time t_1 cannot be the same as the multiple sequence $m_2(t - t_2)$ which follows the arrival of a primary event at time t_2. In fact, *every* primary in the sequence $p(t)$ is followed by a *different* sequence of multiples. It follows that it is wrong to describe the earth impulse response $g(t)$ as the convolution of a primary sequence $p(t)$ with a multiple sequence $m(t)$. There *is* no multiple sequence $m(t)$ which is the same for all primaries. Therefore $m(t)$ cannot be deconvoluted from $g(t)$.

We still have one equation and two unknowns. And, since we have band-limited data, there is an infinite number of different solutions. That is, we can find an infinite number of different combinations of $s(t)$ and $g(t)$ which yield our observable seismic response $x(t)$. The extraction of $g(t)$ is thus nontrivial. All the theories of deconvolution we apply (prediction-error filtering, Kalman filtering, homomorphic deconvolution, minimum-entropy deconvolution, parsimonious deconvolution, etc.) are all *statistical models*. They make assumptions about the statistical properties of $g(t)$, and they also have to make some assumption about the phase of $s(t)$. *In other words, they all assume something about the statistical properties of $g(t)$ in order to find out what $g(t)$ is.* The applicability of any one of these models thus depends very much on the geology. If the assumptions do not fit, the deconvolution will be poor. We see immediately that we cannot compare these different models in any way that is independent of the statistics of the geology. And since we do not know the statistics of the geology in advance (we are trying to reveal the geology with our seismic processing), we do not know which method will give the best results until we try them all. This procedure must be adopted in every new area. As new theories of deconvolution (or statistical models of the seismogram) are proposed, the number of possible processing trials increases.

When we have done all these trials, how should we compare the results? Do we have any clear-cut tests which reveal in an unambiguous way which method is the best? If we look at what we do, we find that we need to have geology of a certain kind (for instance, with an uncomformity) in order to make comparisons of the results. If the geology does not present us with an obvious feature, we are likely to find that any comparison of the results of these processing trials is subjective. We discern differences, but are not able to tell which, if any, is nearer to the truth. Any non-subjective comparison of the results is geology-dependent and, logically, would require us to know the geology in advance.

Instead of comparing the results we could always compare the premises of the different theories. But, again, we would find that we would have to know a lot about the geology—in order to check the validity of the statistical assumptions—before we can make any non-subjective comparisons.

Thus, whether we try to compare the premises or the conclusions of these theories, we must already know the geology in some detail. If we try to use these theories to elucidate the finer details of the geology, we must treat the results with at least some scepticism.

Let us summarize the steps in the argument so far: (1) We need to deconvolve the data in order to determine the geology in more detail. (2) Unfortunately we have one equation and two unknowns. Therefore we know before we start that we cannot guarantee to solve it (if the result of multiplying *a* by *b* is 20, what is *b*?). (3) We resort to statistics and we get an answer (we know by experience that *a* is much more likely to be equal to *4* than to *5* or *10* or *2*, from which it follows that *b* is equal to *5*). We can always force out an answer by using one of these techniques. Each technique gives us a different answer, as it should. (4) We have no way of knowing which, *if any*, of these answers is close to the truth, and we do not know what we mean by "close to the truth". (5) Furthermore, we cannot conclusively refute any of these techniques, the number of which we can invent (and fill the literature with) being unlimited. All our data examples are different, and there is an infinite number of them. Any number of examples which show that a statistical deconvolution technique has failed does not amount to a water-tight case for rejection of the technique (we cannot rule out the possibility that it may work on some future data set). Equally, any number of examples which show that a statistical deconvolution technique has been successful do not amount to a water-tight case that it will be equally successful on the next piece of data. (6) Since none of these theories, or models, as we must call them, can ever be refuted, and are in fact insured against refutation by their very nature, we must conclude that they are nonscientific, by application of Popper's demarcation criterion.

It will be argued that the application of these techniques definitely enhances the data and, in general, we like the results. I agree. But it is still worth pointing out that (a) as scientists, we should be worried that we have no means of testing whether we are doing the right thing to the data and (b) as engineers, we sidestep this problem by giving an interpreter the responsibility for choosing the deconvolution operator from among a number of different operators, none of which (as far as anyone knows) may be correct. The criteria for making this choice are subjective. Only if the geology presents us with an obvious feature can they be objective.

Deterministic deconvolution techniques

To solve equation (1) without making guesses about the statistical properties of $g(t)$, it is obviously necessary (a) to know $s(t)$; or (b) to reduce $s(t)$ in the field to a spike, with the aim that, within the bandwidth of interest, there will be no difference between $x(t)$ and $g(t)$. With this second approach the implication is that no further deconvolution is required. Both of these approaches are more scientific, in the Popperian sense, than any of the statistical methods mentioned above. Their premises may be tested by making simple measurements which are completely independent of the geology.

Solutions of the type (a) include the Maxipulse process (which is interesting in that it exploits the special properties of the pressure field around a point source to find the far field wavelet from a near field measurement—see, for example, Ziolkowski (1980), appendix 2); attempts to measure the far field signature of an air gun array with a deep tow hydrophone (Nooteboom 1978 and Hubbard 1978); and of course, Vibroseis.

Solutions of the type (b) include all attempts to create the "perfect" seismic source. At sea typical examples are implosive devices such as Vaporchoc and water guns, and also explosive devices such as Flexotir which use physical apparatus to suppress bubble pulses. In this category we can also include Vibroseis, for the Vibroseis signature, after correlation with the correct reference sweep, can be made as sharp as we want, provided the Vibroseis sweep and signal-to-noise bandwidth are broad enough (Lerwill 1979, 1981; see also the paper "Resolution, bandwidth, and money" by N. Anstey, 50th meeting of the SEG, Houston 1980).

In practice, whatever approach we use, the data are never quite as good as we would like, and we find that we could still do with some improvement in resolution. At this point we often resort to one of the statistical deconvolution techniques in the hope that we will get this improvement. It *is* just a hope. I do not supply any outside information about $s(t)$, this processing step is no more scientific than before. This is not to say that it will not work, of course. It may. We just have to try it and see, as before. We may be lucky in that our geology may happen to have statistical properties close to those we assume it has. If it does not, the deconvolution will not work. And the problem for us, given that none of these statistical models is a theory which can be put at risk, is to determine whether it *has* worked or not.

Another example of a deterministic deconvolution technique is a method of seismic surveying proposed by Ziolkowski et al. (1980), in the class of solutions (a) above. This method permits the data to be deconvoluted for $g(t)$ without making any assumptions about the statistics of $g(t)$. The method allows $s(t)$ to be found first, but without making direct measurements of the wavelet. The theory is open to refutation by a simple Popperian test.

The basis of the method is to do the seismic experiment twice in the same place using two different seismic sources, one being a scaled version of the other. In the absence of noise the two seismograms we obtain can be expressed as

$$x_1(t) = s_1(t) * g(t), \tag{4}$$

$$x_2(t) = s_2(t) * g(t), \tag{5}$$

in which $g(t)$ is the same in both equations because the experiments are conducted in the same place. The two seismic wavelets $s_1(t)$ and $s_2(t)$ are related to the scaling-law equation

$$s_2(t) = \alpha s_1(t/\alpha), \tag{6}$$

where α is the scale factor. The three equations (4), (5) and (6) can be solved for the three unknowns $s_1(t)$, $s_2(t)$ and $g(t)$.

A simple test of the theory would be to repeat the experiment one more time in the same place using a third seismic source, different from the other two, but still a scaled version of the original source. This will yield the seismogram $x_3(t)$.

Now, without knowledge of the scaling law, we would not know exactly what to expect $x_3(t)$ to be like. But, since we can arrange to know the scale factor, we can calculate the new far field wavelet $s_3(t)$ from $s'_1(t)$, which is our estimate of $s_1(t)$ derived from the measurable seismograms $x_1(t)$ and $x_2(t)$ using our theory. We predict $s_3(t)$ from $s'_1(t)$ using the scaling law, as follows:

$$s_3(t) = \beta s'_1(t/\beta), \tag{7}$$

where β is the scale factor. Using our theory we will already have found $g'(t)$, our estimate of $g(t)$, and therefore, using the convolutional model, *we would predict the seismogram*

$$x'_3(t) = s_3(t) * g'(t). \tag{8}$$

Within the limits of applicability of the convolutional model, and within the signal-to-noise ratio available, $x'_3(t)$ should be the same as $x_3(t)$ if our theory is correct. If $x'_3(t)$ and $x_3(t)$ are not equal within these limits, there is something wrong with the theory.

In this test, the theory runs a serious risk of being refuted. The test is independent of the geology because the theory makes no assumptions about the geology. We note that the scaling law, a premise of the theory, may also be tested independently. Thus, the theory may be put at risk either through its predictive power or through its premises.

It may be worth emphasizing that this is not an argument claiming that the scaling-law deconvolution technique is bullet-proof. It is exactly the opposite. The scaling-law technique can clearly be set up as a target to be shot at very easily. Perhaps it will disintegrate on the impact of the first shot. Perhaps it will survive. The outcome, for the purpose of *this* paper, is irrelevant. The point is that the theory is framed in such a way that it stands at risk of being refuted by a test. It is scientific in Popper's sense.

This note would be more exciting if it reported the result of such a test, especially if the theory were refuted. But perhaps the results would indicate that the theory had survived. In that case the test would most probably have to be repeated by an independent person, to carry any validity, for there must always be doubt about an author's impartiality where his own pet ideas are concerned.

Conclusions

Parasnis (1980) is quite right to urge us not to try to prove our theories (which is logically impossible in any case), but to try to disprove them. Only in this way will we be able to progress, and learn from our mistakes.

Popper's demarcation criterion allows us to distinguish between scientific theories, which stand some risk of being falsified under test, and nonscientific theories which are insured against falsification. If a scientific theory has no rivals, however, it may not be possible to refute it conclusively as Lakatos (1970) has argued, for the test will come under as much suspicion as the theory itself.

Whether we progress or not depends on our identifying the false premises in our theories, and replacing them with better ones. It therefore *pays us* to frame our theories in such a way that they may be put at risk. Psychologically this is hard to do. Therefore we need to make a conscious effort to do it. If we avoid this, and frame our theories in such a way that they are insured against falsification—which is the path we have taken in developing statistical deconvolution techniques—we then cease to do science. This may be no bad thing, but it is not science, in Popper's sense, and we are then put in the position that we must take things on trust.

If we wish to minimize the number of things we have to take on trust, and establish the firm ground, we must apply Popper's demarcation criterion and ask whether the theories we use have survived tests in which they have seriously been put at risk.

Acknowledgments

In the preparation of this note, I have benefited enormously from the discussions I have had with friends. These have prompted much rewriting and augmentation of the original idea, with the unfortunate result that each draft has been a little longer than the previous one. For this I apologize to Sally Bryson of The British National Oil Corporation who typed nearly all the drafts.

Any clarity which does emerge, however, is due to the advice and criticism of the following friends: Dave Brown and Ken Larner of Western Geophysical Company of America; Kate Crowley of the Economics Dept., Massachusetts Institute of Technology; Neil Goulty of the Geophysics Dept., University of Durham; Leslie Hatton of Merlin Geophysical Co.; Bill Lerwill of Seismograph Service (England) Ltd.; Lloyd Peardon and Andrew Stacey of The British National Oil Corporation; Elio Pooggiagliolmi of Entec; and Chris Walker of Geosource. I wish to thank the anonymous reviewer who pointed out that the second part of the paper was hazy, and thus gave me a further opportunity to try to be more clear. The errors which (doubtless) remain are my own work.

I owe a special debt to Geoff King of the Geophysics Department, University of Cambridge, who proposed more than three years ago, that the scaling law deconvolution theory could be put at risk through its ability to predict a seismogram.

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Editor's Note

This is the first paper in a speculative vein ever to appear in the refereed part of Geophysical Prospecting (Presidential addresses are, of course, not refereed). The decision to accept it for publication has by no means been unanimous, and the editor in particular has hesitated lest a precedent might be set that changes the character of our journal irrevocably. In the end the argument that around the manuscript a lively discussion among experienced exploration geophysicists had developed carried conviction: such discussions should be conducted in the open.

It might be that exploration geophysics has matured so much that some introspection and soul searching is indicated. Should this indeed be the case it would follow that our official journal—since it reflects our professional thinking—must open its pages to this and similar contributions.