The Delft airgun experiment

Anton Ziolkowski*

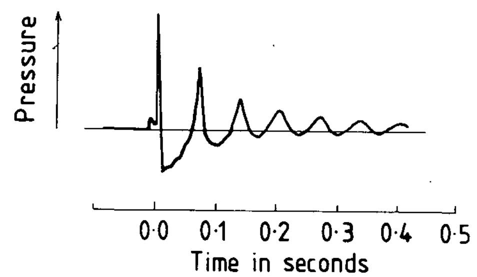

Fig. 1. The signature of a single airgun at a distance of 1 m. The gun volume was 0.8 l and was fired at a depth of 7.5 m and pressure of 135 bars.

In April and May 1983 the Geophysics Group of the Mining Department, Technische Hogeschool Delft (Delft University of Technology), carried out a seismic reflection experiment in the North Sea in cooperation with the Netherlands Organisation for Applied Scientific Research (DGV-TNO). The experiment, which cost in excess of £200 000, was sponsored by six major oil companies, three seismic contractors and DGV-TNO, who also managed the project. This article is a description of the experiment and an account of how it came to be performed.

In the present climate of economic recession, low oil prices and university cutbacks, it is remarkable that funding for such an expensive experiment was made available. I think it worthwhile, therefore, to include an explanation of how it was possible to create a consortium of companies willing to sponsor such a project, and to give details of the administrative background that enabled this technical cooperation to take place.

Seismic reflection with airguns

In an ideal world the perfect seismic source for exploration would create a sound wave that is a short, sharp impulse lasting for not more than a few milliseconds. A reflection of this impulsive wave from the interface between two different rock layers would be recorded as an impulse. The reflections of the impulsive wave from the top and bottom of a single layer would be recorded as two impulses arriving one after the other, the difference in arrival times being accounted for by the two-way travel time of the sound wave in the layer. The thinnest rock layer that could be resolved would be determined by the shortest resolvable time period between reflections multiplied by half the velocity of sound in the rock (the factor of 1/2 is necessary because the reflected sound wave must travel through the rock twice, of course). Thus the finest detail of the geology that can be resolved depends on the sharpness of the sound wave generated by the seismic source.

Considerable effort has been devoted to developing sound sources for seismic exploration on land and at sea. The most commonly used marine seismic source is the airgun, which produces a most unsuitable, long, oscillatory sound wave, generated by the oscillations of the high pressure air bubble emitted by the gun. A typical signal measured 1 m from an airgun is shown in Fig. 1.

*Afdeling der Mijnbouwkunde, Technische Hogeschool Delft, Postbus 5028, 2600 GA Delft, The Netherlands.

Despite this poor signal, the airgun has become the most widely used seismic source in marine seismic exploration for oil and gas because of its other attractions including its reliability, its very repeatable signal and its ability to be reloaded and fired within a second or two if necessary.

Two separate methods are currently in use to overcome the resolution problems caused by the poor signal shape. First, in data acquisition, arrays of airguns are used to create a sharper, more impulsive signal. Secondly, in processing, the recorded data are manipulated in a computer so that they more nearly resemble the data that *would* have been recorded if a perfect impulse had been generated at the source. This processing step is known as 'signature deconvolution'.

The use of airgun subarrays as a sound source was pioneered largely by Shell and has been adopted by nearly all the seismic contractors, with variations from contractor to contractor. An airgun array usually comprises a number of identical 'tuned' subarrays each consisting of about 6 or 7 individual airguns of different sizes spaced over a distance of about 20 m. Airguns of different sizes generate oscillating signals with different periods of oscillation. When all the guns are fired together, the initial or 'primary' peak of every gun signal occurs at the same time and this peak is reinforced by the array; the subsequent peaks or 'bubble pulses' in the different oscillating signals occur at different times and tend to cancel out. The 'tuning' of an airgun array consists of selecting the gun sizes and spaces such that the ratio of the primary peak to the later bubble pulses (the so-called 'primary-to-bubble ratio') is sufficiently large when all the guns are fired together. Figure 2 shows the measured far-field signature of such a 'tuned' airgun subarray including the reflection from the sea surface. This measurement was made 100 m vertically below the subarray in a Norwegian fjord with a water depth of 450 m. There are no significant sea bottom reflections in this measurement and this signal can be regarded as being the signal entering the sea floor.

The reflection of this signal from an interface between the rock layers will have approximately the same shape. The reflections from the top and bottom of a layer of oil-bearing sandstone 10 m thick with sound velocity of

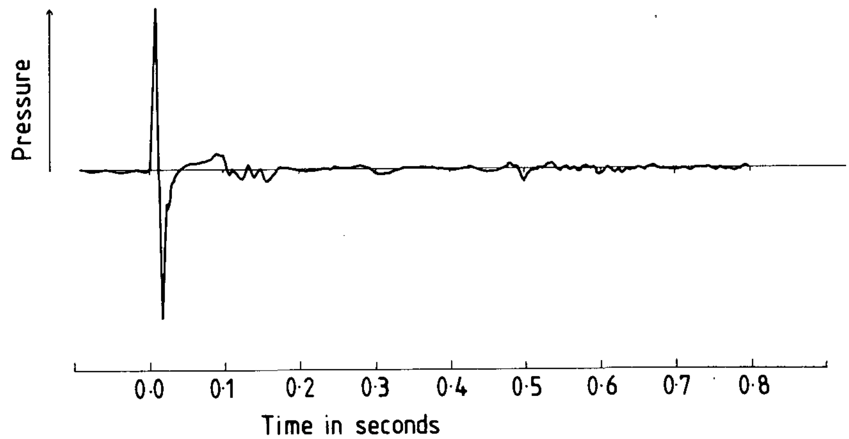

Fig. 2. The far field signature of an airgun array. Total duration of the signature is nearly 0.4 sec. (The event at about 0.5 sec is the reflection from the sea floor.)

3000 m sec^{-1} will consist of two such signals arriving with a time separation of only 6.6 msec. Thus these signals will overlap considerably and the resolution of the 10 m layer will be a non-trivial task, especially when it is considered that there are many layers of rock between the sea floor and any oil-bearing sandstone.

If the shape of the signal is known from measurements, as this one is, it is now a simple matter to design a linear deconvolution filter in a computer to convert this known, complicated signal into a more impulsive one such as that shown in Fig. 3. The application of this filter to the recorded seismograms will then convert the recorded data into data which *would* have been recorded if the simple signal of Fig. 3 had been used instead of the complicated signal of Fig. 2. So the application of this deconvolution filter to the data should improve our ability to resolve details of the geology.

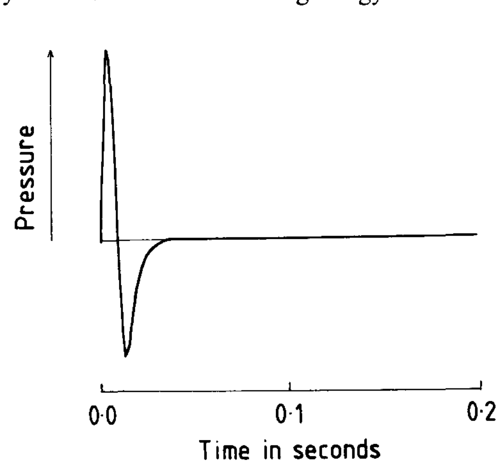

Fig. 3. The desired, more impulsive signature. Total duration, 0.038 sec.

The problems: source directivity, on-line source variations and airgun interactions

Directivity

Figure 4 shows the elements of a marine seismic reflection system. The vessel tows the airgun array and a hydrophone cable, say 3 km long, consisting of perhaps 100 or more groups of hydrophones at regular intervals. The sound wave generated by the airgun array propagates outwards in all directions. At each acoustic interface (between rock layers and at the sea surface and sea floor) some sound is reflected and some is refracted. Typical ray paths are indicated in Fig. 4. The sound received at any one hydrophone group arrives from many different directions. The arriving rays have also left the source in many different directions.

The array of airgun subarrays typically has overall dimensions about 60 m × 60 m. It is designed such that the signal has a high primary-to-bubble ratio in the vertical direction. Away from the vertical, the primary-to-bubble ratio decreases as a function of the angle to the vertical. The change in the shape of the sound wave is caused by changes in the interference pattern between the sound waves emitted by the airguns. In the vertical direction the primary pulses add together in phase, as intended. As the angle to the vertical increases, these primary pulses become increasingly out of phase, and the shape of the wave changes. This effect increases with frequency and also increases as the dimensions of the array increase. Typical variations in the shape of the emitted sound wave as a function of angle to the vertical are shown in Fig. 5.

The received seismogram at any one hydrophone group will therefore consist of a whole series of overlapping *non-identical* wavelets. The greater the variation in the angles of incidence, the greater the differences between the received wavelets. In modern seismic data processing, this effect of variation of signal shape with angle to the vertical is *ignored*, which results in not only a loss of resolution but also an increase in the noise level. The signature deconvolution filter is designed only for the vertically-travelling wavelet and depends entirely on the measured signature (Fig. 2) and on the desired output pulse (Fig. 3). Once the filter is designed, it will work successfully only on the correct wavelet. When the filter is applied to a seismogram

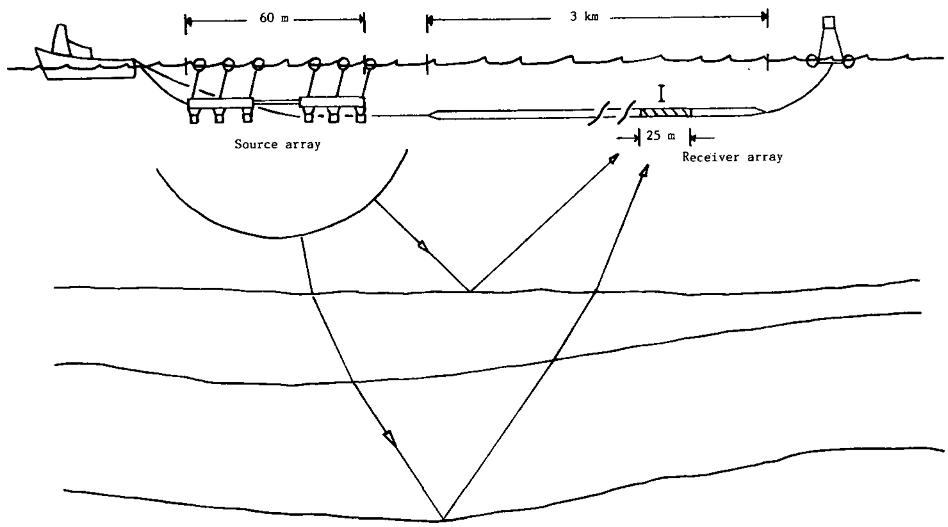

Fig. 4. The elements of a marine seismic reflection system. The path travelled by the reflected sound waves depends on the depth of the reflector. Reflections arrive at I at varying angles of incidence. They also leave the source at varying angles.

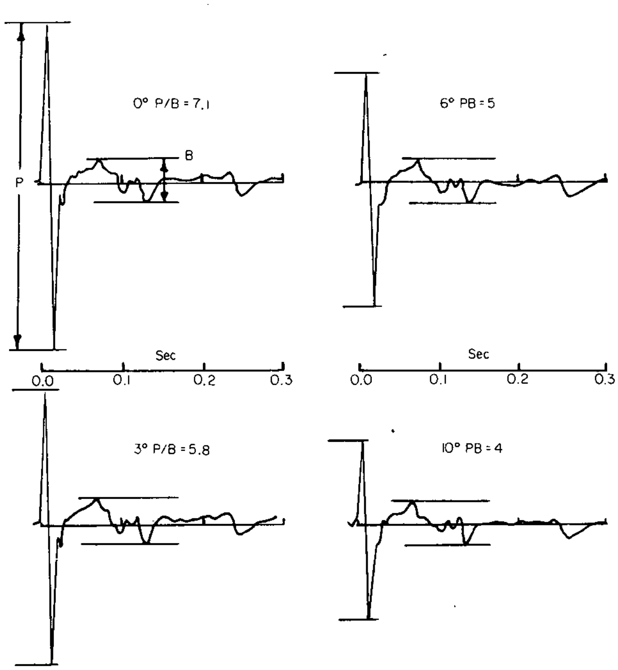

Fig. 5. Variation of source wavelet with angle of incidence. The signal lasts about 0.3 sec and has a large peak P at the beginning, followed by smaller oscillations known as 'bubble pulses'. The ratio of P to the largest bubble pulse B is known as the 'peak-to-bubble' ratio and is a measure of the sharpness of the signal. The signal shape, particularly the P/B ratio, varies significantly with angle of incidence.

consisting of a mixture of wavelets, the increase in resolution is less than expected: the more the real, non-vertically incident wavelet differs from the expected vertical incidence wavelet, the less precise is the deconvolution and the more noise is added to the data. This 'processing noise' is predominantly high frequency, because the errors are caused by the directivity of the airgun array, a problem which increases with increasing frequency.

The results of normal modern data acquisition and processing with airguns are therefore always a little disappointing. The final seismic section looks noisier than one would hope, especially at high frequencies. To make the section look better, the high frequencies must be filtered out, with a consequent loss in resolution.

To attack this problem it is necessary, first, to be able to specify the shape of the propagating wave as a function of the angle of incidence, and secondly, to be able to separate the different angles of incidence from each other in processing in order to deconvolve each wavelet separately. The first part of the problem is essentially a problem in data acquisition; the second part is essentially a processing problem using the information from the first part.

On-line variations in source performance

Of more immediate concern to most of the sponsors of our project was a separate problem: the variations in source performance in production seismic surveying. The signal that is measured 100 m below an airgun subarray in the peace and quiet of a Norwegian fjord may not be the same as that generated by the same subarray when being towed at 5 knots on a rough day in the North Sea, when all the guns may not even be at the same depth. How do we know what the signal is in production? Very often things go wrong: one of the airguns (usually the biggest) stops working. Should we stop production, at great expense, while we repair the gun? Should we carry on shooting, knowing that we must be generating a different source wave, which must be compensated for in processing? What we should like is some sort of production monitoring system that will be able to tell us exactly how much the source wavefield is varying from shot to shot (every 25 m, say) throughout the survey.

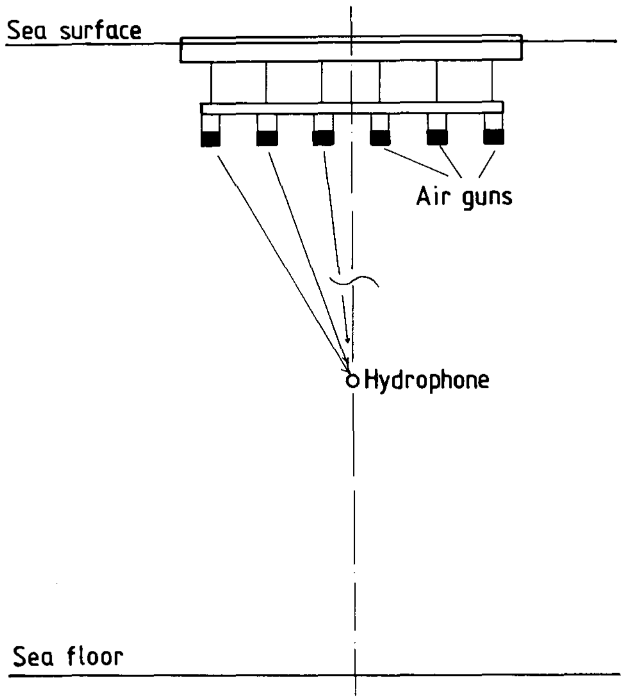

Fig. 6. The measurement of the far field signature of an airgun array. The hydrophone must be sufficiently far away that differences in travel paths from the different guns to the hydrophone are small compared with a wavelength. There must also be a sufficient depth of water beneath the hydrophone to ensure that the reflection from the sea floor does not interfere with the measurement of the downgoing wave.

Possibly one's first thought is to tow a hydrophone below the airgun array to monitor the source wavefield. A moment's consideration will show that this is likely to be a fruitless exercise. Consider Fig. 6. First of all, the hydrophone cannot be too close to the airgun array because there will then be differences in travel path length from different parts of the array to the hydrophone. It must be in the 'far field' of the array, typically 100 m away. Secondly, there must be enough depth of water below the monitor hydrophone to prevent the echo from the sea bottom contaminating the measurement. Typically, there should be at least 200 m of water between the hydrophone and the sea floor. Thus, for any meaningful measurement to be made, the sea must be at least 300 m deep. This automatically excludes all continental shelf areas where most offshore oil and gas is found and can (at current prices) be exploited. Thirdly, it is not a simple task to tow a hydrophone 100 m below the airgun array and know exactly where it is all the time. A special positioning system in a frequency range above the seismic range must be used. In practice, any towing device capable of keeping the measuring equipment at a depth of 100 m at a speed of 5 knots will tend to wander from side to side, which will cause the measured signal to vary. It will always be impossible to separate the variations caused by the movement of the hydrophone from the variations caused by fluctuations in source performance. In other words, even with all this effort, the source cannot be monitored effectively in this way.

Amazingly, a few years ago quite a lot of money was spent in trying to monitor the 'far field' signature of airgun arrays by towing hydrophones below the array even in surveys on the continental shelf. This effort has now been abandoned because no improvement in data quality was obtained when the hydrophone measurements were used in signature deconvolution. In view of the above discussion the reasons for this are plain.

Airgun interactions

Another approach which has failed is to superimpose the signatures from individual airguns. The idea behind this is that the wavefield generated by an array of airguns is simply the sum of the wavefields generated by the individual guns. Therefore one could measure the wavefield generated by each gun fired separately and add up the individual recordings to construct the field that is obtained when all the guns are fired together. Whenever this hypothesis has been tested, it has been found to be wrong. The sum of the wavefields of the guns fired separately is *not* equal to the wavefield generated by all the guns firing together.

It is found that the behaviour of each gun is affected by the other guns. The closer the guns are to each other the more noticeable this mutual interaction becomes. It can be shown by a simple conservation of energy argument that this interaction is a frequency-dependent effect and is important whenever the separation between guns is small compared with a wavelength (Ziolkowski *et al.* 1982). In practice this means that, for a given separation between guns, the effect increases as the frequency decreases. For most airgun subarrays in use to-day, these mutual interaction effects are significant and cannot be ignored.

Three new methods

The problems which are encountered in the use of airgun arrays are a direct consequence of the long, oscillating signal of the airgun and the belief that, to use airguns effectively, it is essential to fire the guns together in a subarray to produce a vertically-travelling far-field signature which has a high primary-to-bubble ratio.

In my own work on airguns, I have never felt that the primary-to-bubble ratio approach was more than a brute force way to tackle the problem. I have been looking for something more elegant. Between 1977 and 1981 I had a spate of ideas on these problems which were triggered by discussions with fellow geophysicists.

The scaling law

The first of these ideas I shared with Bill Lerwill of Seismograph Service Ltd one spring lunch time in 1977 in a country pub in Kent over a pint of beer. We had noticed from measurements of dynamite records on land that the frequency content of the pulse changed with the size of the dynamite charge. Some early theoretical work (especially Sharpe 1942), indicated that the *shape* of the pulse was not changing—only the timescale and amplitude scale were changing. Friends of mine at Massachusetts Institute of Technology had noticed the same thing with underground nuclear explosions (Frasier and North 1978). This scaling law could be phrased another way: the shape of the pulse generated by explosive is independent of the *size* of the explosive; it depends only on the rock in which the explosive is detonated. If two explosive charges of different sizes are detonated under the same conditions in the same rock, the pulses they generate, $s_1(t)$ and $s_2(t)$, will have the same shape and will be related by some scale factor α:

$$s_2(t) = \alpha s_1(t/\alpha) \qquad (1)$$

where t is time and the scale factor α is the cube root of the ratio of the mass of the second explosive to the mass of the first.

We suddenly realised that this information could be used in seismic reflection surveys. Suppose the sequence of reflections from a seismic experiment conducted with a perfect impulsive source is $g(t)$. With a real seismic source with some (unknown) signature $s_1(t)$ every impulsive reflection is represented by the wavelet $s_1(t)$. The received seismogram $x_1(t)$ is a series of overlapping wavelets which we can write as the convolution of the wavelet $s_1(t)$ with the sequence $g(t)$:

$$x_1(t) = s_1(t)*g(t) \qquad (2)$$

where the asterisk* denotes convolution.

This is one equation with two unknowns. If we want to separate the geology $g(t)$ from the seismogram $x_1(t)$ we must know the wavelet $s_1(t)$. Our idea was *to repeat the seismic experiment in the same place using a different scaled version of the original source.* Because the experiment is in the same place, the reflection $g(t)$ is the same; but with a different source, the recorded seismogram $x_2(t)$ is different:

$$x_2(t) = s_2(t)*g(t). \qquad (3)$$

Providing the scaling factor α is known, we are left with three equations (1)–(3) and three unknowns $s_1(t)$, $s_2(t)$ and $g(t)$. Thus, without measuring the outgoing wavefield directly, we can still determine the signatures and the reflection sequence from the two separate seismic experiments and the scaling law.

We thought that the scaling law would work even better with airguns in water than with dynamite on land. An indication of the scaling of the airgun signatures is given in Fig. 7. Our ideas were worked out together with Derek March and Lloyd Peardon of Seismograph Service Ltd and published (Ziolkowski *et al.* 1980). I extended the idea to two dimensions with the help of a discussion with Andrew Stacey of the British National Oil Corporation (Ziolkowski 1980). This allowed the concept to be applied to two-dimensional airgun arrays as illustrated in Fig. 8. In production, the two scaled arrays are towed one behind the other with the array separation equal to the shot point interval so that the second array always fires in the same place as the first one.

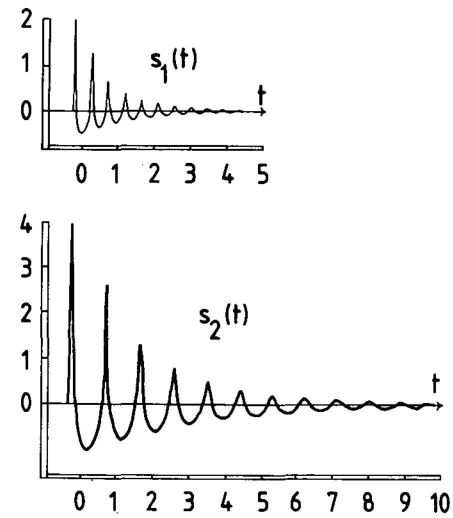

Fig. 7. Scaling of airgun signatures. The second signal $s_2(t)$ has the same shape as the first signature $s_1(t)$; however, the amplitude and time-scales differ by a factor of 2. This is a consequence of the ratio of gun volumes being equal to 8.

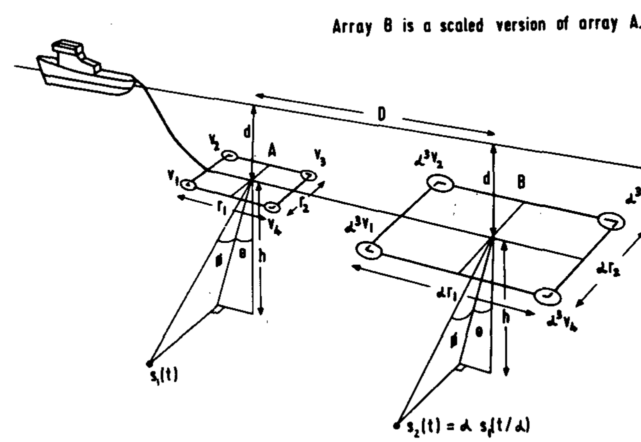

Fig. 8. Operation of scaled arrays A and B which are fired alternately. The spacing D is equal to the pop-interval to ensure that the arrays are fired once in the same place.

It was pointed out to me by Geoff King of the Department of Geodesy and Geophysics, University of Cambridge, that the whole theory on which this method is based is open to refutation (see Ziolkowski 1982) if a third scaled array is used. From the first two arrays we obtain estimates $\hat{s}_1(t)$, $\hat{s}_2(t)$ and $\hat{g}(t)$ of the two different signatures and the impulse response of the earth. The third array will produce a third signature $s_3(t)$ which should be related to $s_1(t)$ by a second scaling factor β:

$$s_3(t) = \beta s_1(t/\beta) \qquad (4)$$

and the received signal $x_3(t)$ will be

$$x_3(t) = s_3(t)*g(t). \qquad (5)$$

Since we already have an estimate of $s_1(t)$ we may obtain an estimate of $s_3(t)$ from equation (4):

$$\hat{s}_3(t) = \beta\hat{s}_1(t/\beta). \quad (6)$$

and since we already have an estimate of $g(t)$, we can *predict the seismogram* we would get with the third scaled array:

$$\hat{x}_3(t) = \hat{s}_3(t)*\hat{g}(t). \quad (7)$$

Our test would simply consist of comparing this prediction $\hat{x}_3(t)$ with the measurement.

The machine gun array

In August 1980 Paul Stoffa (of Lamont-Doherty Geological Observatory) and I were discussing the problem of airgun interaction over a glass of sherry. We agreed that once the shape of the far field signature was known, it did not matter whether it had a high primary-to-bubble ratio or not; it could still be deconvolved into some short desirable pulse. After all, we said to ourselves, even Vibroseis† works. So why do we have to fire the guns all together? If you fire them one after the other you stretch out the signal, but that is not an insuperable problem and it eliminates the interactions. Furthermore, when the guns are fired one at a time, each source is very small and the 'far field' hydrophone can be put very close—say 1 m from the gun. Thus the *far field signature* from an array can be measured with *near field hydrophones,* provided that the guns fire one after the other, such that the signal from each gun passes the hydrophone before the next gun fires. In practice one needs to have a hydrophone 1 m away from each gun. The signals from these near field hydrophones must be recorded on tape.

Because the hydrophones are so close to the guns, the signal is very strong. The reflections from the sea floor and even from the sea surface have much further to travel than the direct signal and are therefore much weaker—to the point where they can be neglected in the measurement. In other words, this is a method which would work on the continental shelf. The far field signature of the array is determined from near field measurements, but the guns cannot be fired together. Thus the primary-to-bubble ratio philosophy is totally rejected in this method. We presented this idea at the Offshore Technology Conference in Houston in 1981 and published it in a final version nearly two years later (Stoffa and Ziolkowski 1983).

The interaction method

In September 1980 I met Tor Haugland who was the Technical Director of a newly formed Norwegian seismic contracting company: Seismic Profilers A/S. He invited me to come on board his new vessel, the *Nina Profiler,* in the spring of 1981 when it would be doing airgun trials before beginning production seismic surveying. He thought that there would be time to test both the scaling law method and the machine gun array. (At that time we did not call it that. The nickname was coined a year later by Steve Levy of Seismic Profilers.) The new vessel would have a new towing system for the airgun array and would have the near field hydrophones built in as standard available equipment. This was new.

The trials took place in March 1981. I was very impressed by the quality of the near field signatures we could record. The change in recorded signal caused by interactions was clearly observable. Also on board were Les Hatton and Gregg Parkes of Merlin Geophysical. Les was of the opinion that we would have three alternative methods to the normal, high primary-to-bubble ratio approach. First, the scaling-law method, secondly, the machine-gun array, and thirdly, the interaction method. 'What interaction method?' I asked. Les argued that we would get so much data from the airgun measurements that we would sort out the interaction problems. My initial reaction to this optimism was to argue that that was the problem which everyone had wanted to solve for years, and there was no reason why we would solve it during these airgun trials. I was wrong and Les was right.

I had spent some time over the previous two years working out how interaction could be thought of as a time-dependent modulation on the pressure surrounding the airgun bubble which could be treated as a point at the wavelengths of interest. I was therefore able to write down the effect of interaction as an equation and show what a near field hydrophone was measuring. We then wrote down what all of the seven near field hydrophones would measure when all of the seven guns in a subarray fire simultaneously. Staring us in the face were seven linear equations with seven unknowns which, Les declared, would be 'no problem' to solve. Conceptually at least, we had solved the problem. All we needed was a sufficient number of near field hydrophones with known sensitivities in a known geometrical relationship with the airguns. This was available to us on board the *Nina Profiler.*

During the rest of the time available for the trials, various operational problems prevented us from making more than a tentative stab at testing the other two methods. Despite this disappointment, the trials left us feeling optimistic, especially as we now thought we could solve the problem of interaction between airguns, and had collected enough data to test our method.

We were able to analyse the data fairly quickly. Gregg Parkes wrote a program in a few days to solve the seven equations for the seven unknowns and we found that our method seemed to work. We presented our results at two international meetings and were able to publish the method and results fairly quickly (Ziolkowski *et al.* 1982). Further details of the method were published a year or so later (Parkes *et al.* 1984).

†Trademark of Continental Oil Company.

A test of the methods

Do any of these ideas work? Are they superior to the current method pioneered by Shell in Rijswijk? Naturally I wanted to find out. From 1980 to 1982 I was working full-time on these problems. I was able to obtain five days for tests in December 1981. The tests were to be carried out on board the *Nina Profiler* and were to consist of two profiles over an oilfield in the North Sea, each profile to be shot with the normal method and then repeated with each of the new methods. In the scaling law profile the third array was to be included to allow the test to be self-contained. The normal and interaction methods could be combined, because the interaction method can be applied under normal conditions, without any modifications, provided there are a sufficient number of near field hydrophones whose responses can be recorded when the guns fire. Thus the two profiles were to be shot three times each and the results were to be compared.

At the end of the five days we had managed to shoot one of the lines once only—with the normal method. Bad weather and failure of new equipment prevented more work being done. These tests had been very expensive, but we thought we knew what the problems would be in trying to do them again. We had also been able to confirm that the interaction method seemed to work very well. The results of this test are published in Chapter 7 of Ziolkowski (1984).

The proposal

When I came to the Mining Department of Delft University of Technology in May 1982, I was still eager to do the experiment, of course. I was supported in this by Seismic Profilers and Merlin Geophysical. Andreas Nordvoll, Managing Director of Seismic Profilers, agreed to make one of his seismic vessels available for the experiment and offered to pay one day's ship time towards it. Peter Taylor, Managing Director of Merlin Geophysical, offered to do the data processing free of charge.

I estimated that £200000 would be needed to pay for the experiment, of which about 90% would be for ship time. I thought £25000 was approximately the most any sponsor would contribute towards the project. Therefore I needed a minimum of eight sponsors.

I told Nico de Voogd of TNO about the idea, hoping he would persuade TNO to sponsor the project. He and Frans Walter, the Director of DGV-TNO, proposed instead that TNO manage the project as their contribution to the sponsorship. I agreed immediately which, in retrospect, turned out to be an even better idea than I realised at the time.

In July 1982 we wrote a proposal entitled 'Proposal for an airgun experiment by DGV-TNO and Delft University of Technology'. The proposal outlined the experiment and gave the theory behind each method, quoting either the publications or the papers in press.

Participation in the experiment was proposed on the following basis. Each participating company had to contribute £25000, with a minimum of eight participants contributing before the experiment could be undertaken. Each participant would receive copies of the field data, processed data, seismic sections, and a full report of the details of the data acquisition and processing. The responsibility for technical control of the data acquisition and processing was mine, the responsibility for the management and operation of contracts lay with DGV-TNO. All companies interested in the prospect were to indicate this before 15 September 1982, whilst final commitments would be received up to 31 December 1982.

For my part, I needed approval first from the Board of the Mining Department and then from the University. The Mining Department gave their approval very graciously. The University Legal Department required a contract with TNO and insisted that the University should not have any financial or legal obligations under the terms of the contract. In other words, the University would not allow me to do the experiment unless a satisfactory contract was agreed such that TNO would assume all financial and legal liabilities, and that there would be no liability to the University. Surprisingly, TNO was still interested in taking the management role.

We had worked out how to minimise the financial liability before sending out the proposal. Nico de Voogd suggested that we must know how much the experiment was going to cost. If we had enough money for 10 days ship time and we did not complete the experiment in 10 days, for whatever reason, what could we tell the sponsors? I telephoned Tor Haugland, who proposed that we agree a price of £180000 for the data acquisition part regardless of the length of time it would take. That is, if it took three days, Seismic Profilers would make a big profit; if it took 1 month they would make an enormous loss.

I agreed. I thought it unlikely that we would do it in less than about 10 days. The timing of the experiment should be in the spring of 1983, the exact period to be chosen by Seismic Profilers, who would try to choose a time when business was slack but when the weather would be good. (This is not an easy choice. All the oil companies want to do their work when the risk of bad weather is least, and book up the seismic vessels accordingly. Thus the slack periods are at times when the weather is bad.)

Selling the proposal

Nico de Voogd suggested that the proposal would perhaps be better understood by potential sponsors if we gave them an oral presentation. Accordingly, in advance of mailing the proposal to the 50 or so potential sponsors, we sent them each a telex to tell them the proposal was coming and to offer to give all interested companies a presentation at the then forthcoming meeting of the Society of Exploration Geophysicists in Dallas in October 1982. By September 15 about 30 companies had indicated their interest in attending this presentation. We therefore hired a room and made our preparations.

Before the Dallas meeting, Peter O'Brien, Head of Geophysical Research at British Petroleum, invited Nico and me to come to London at BP's expense to give them an individual presentation. This we were delighted to do, of course. Without committing themselves to sponsor the project, BP suggested that, if they did they would like the line to intersect one of their wells, possibly in the Norwegian sector of the North Sea.

This would make the experiment much better. We could then compare the seismic data with synthetic seismograms computed from the well logs, and could therefore determine which of the methods yielded results which were closest to the 'truth'.

We originally expected most of the sponsors to be American companies and we wanted to choose a location which would be interesting to them. (The location would have no effect on the experiment, of course, since the experiment would be testing methods, which should be independent of location.) We had therefore suggested the Gulf of Mexico in our proposal.

At the Dallas presentation on Sunday 16 October 1982 we mentioned BP's suggestion, which was warmly received by everyone. Most of the 60 or so people who attended the presentation were deeply interested and had a far greater understanding of the new methods than I had expected. They also had concerns about issues which we had not really considered. Several additional points emerged:

(1) Those who might commit themselves to the experiment demanded that there be a meeting to discuss and agree all the specifications for the acquisition of the data in the experiment.
(2) If there were more than the minimum number of sponsors there should be a reduction in cost of sponsorship.
(3) The experiment should include measurements of the far field signature not only in the vertical direction, but also away from the vertical..
(4) Sponsors should receive a discount on any royalties charged on licences for the use of these methods (which are the subject of patent applications) and should be able to buy the necessary computer programs at reduced cost.

We agreed to the first point and said we would draft a detailed specification in good time ahead of the discussion for all committed sponsors. We also agreed to the second point, but said we would have to think about the details and come back later with a proposal on reduced sponsorship fees. We argued that the far field signature experiment was outside the scope of the proposal, but we were prepared to do it if the costs were borne by the sponsors, and if the sponsors proposed exactly how to do it. (This experiment had never been performed before by any of the companies we talked to.) We further argued that our interaction theory had stood at risk in the signature tests we had conducted in Norway; the theory survived the tests, as the results shown in our paper and presented in our proposal confirmed. The angular-dependent measurements did not put the theory at greater risk; they were simply more awkward and more expensive to make.

The problem of royalties and computer programs is very involved. The patent rights to the inventions were shared by a number of companies. Some of the computer programs I had developed were not my property and could not be released without permission of the owners. We promised to explain exactly what the position was on all relevant patents and computer programs. We realised that unless we made this quite clear we would have no sponsors.

By 31 December 1982 we had the following commitments: Amoco, BP, Britoil, Chevron, Mobil, Statoil, Merlin Geophysical, Seismic Profilers, Western Geophysical and TNO. This was just enough. BP and Statoil had also provided a well in the Norwegian sector of the North Sea that could be used for the experiment.

Publication

I was anxious to be able to publish the results, but the problem was this: if I published the results, what would be the benefit of participation to any of the sponsors? Tor Haugland suggested to me that the value of the data far exceeded the cost of sponsorship. The sponsors would be getting good value for money already: the point was to restrict the access to the data; in other words, to make it impossible for non-sponsors to gain access to the data.

We therefore decided that all the sponsors and Delft University would have copies of the field data and the processed results; and that all sponsors and Delft University would be free to publish the results after the final report had been issued, provided all participants acknowledged the contributions of all other participants. We also decided that none of the participants could give away or sell its copy of the data. This formula was accepted by all the sponsors before the contracts were drafted.

The contracts

TNO had to draw up all the contracts. There were separate contracts between TNO and each of the sponsors, a contract between TNO and the University for my technical input, and a contract between TNO and Seismic Profilers for the acquisition of the data. Negotiations between TNO's contracts branch and the legal departments of the sponsors were fairly lengthy and were taking place right up until the experiment began.

When the time came to do the experiment, not every sponsor had committed his share of the funding, although telexes were supposedly on their way. I wish to express my thanks to TNO and to Nico de Voogd in particular, for shouldering the responsibility and for taking the big financial risk to go ahead as planned. In fact, all the committed sponsors paid, as they promised they would.

The technical specifications of the experiment

We proposed to the sponsors that we hold a meeting in London on 3 March 1983 to discuss the technical specifications. BP had kindly agreed to provide a conference room for the discussion. All the sponsors sent at least one representative who was, in nearly every case, the man responsible for persuading his company's management to sponsor the project.

BP had agreed with Statoil on a particular well in the Norwegian Sector of the North Sea. The well was chosen for the quality of the logs which were excellent. The target was deep which meant that we had to ensure that we had good resolution down to over 3000 m. The specification was drafted by me with considerable help from Wietze Eckhardt, one of my students, who did a large number of computer calculations to help me to determine the optimum depth of the hydrophone cable. His computer plots were included in the specification.

At the meeting, which began at 14.00, there was initially considerable disagreement about the exact parameters to be used, but, by the end of the afternoon, complete agreement had been reached on everything. (This may tell us something about the differences between industrial scientists and lawyers.) The additional experiment to measure the far field signature in all directions had by this time been fully worked out. It involved two separate navigation systems, one shore-based to locate the ship and airguns accurately, the other a transponder system placed on the bottom of the fjord to locate the position of deep hydrophones suspended from digital sonobuoys. The cost of this experiment was estimated at £150000. Only three sponsors had been really interested in this experiment, but, at this point, they regarded the cost as prohibitive.

Two fresh considerations were raised at the meeting. First, the normal navigation would not be good enough; it would have to be augmented by additional systems. The reason was that we wished to shoot the seismic lines in exactly the same place. We expected to see differences between the lines and we wanted to be sure that these differences were caused by the different methods of acquisition, rather than by changes in location of the line. We agreed that we could obtain the required degree of accuracy for another £10000.

Secondly, we needed another technical person on board to check for quality control. Although I would be on board, there were many details of the operation of the equipment which were quite unknown to me, and I felt that everyone's interest would be served if an independent observer were to be present too. Two of the oil company sponsors said that, between them, they would be able to provide someone to do this task and to write a report. This was acceptable to all the other sponsors. The spirit of cooperation between the sponsors was very impressive.

The experiment

On 13 April 1983 Tor Haugland called me to say that the vessel, the *Liv Profiler* (sister ship of the *Nina Profiler*) would be ready to leave for our experiment from Stavanger on Sunday 17 April. TNO were represented by Teake Stavenga and I was accompanied by three students: Wietze Eckhardt, Albert Holtslag and Kees van der Schans, all of whom would be working on the data for their final-year Masters degree theses, as well as supporting the project. Neither of the two oil companies was able to find anyone to provide the quality control because all suitable personnel were committed on other vessels. Quality control was thus the responsibility of Tor Haugland and myself.

The first two days at sea were spent steaming to the location, getting the hydrophone cable balanced and the equipment ready, and trying to get the new Syledis navigation equipment to work. The weather became worse and the Syledis could not be made to work. Then, as further gales were forecast, we received a telex to say that the vessel was required within the next two days. In view of the failure of the Syledis navigation equipment, the prospect of further bad weather and the lack of an oil company representative for quality control, we agreed to try again after the Shell survey was completed. This was the third time in two years that I had failed to do the experiment.

The survey was completed very quickly, and on 7 May I flew to Stavanger to join the ship again. This time TNO were unable to send a representative and I felt I could not justify inviting Wietze, Kees and Albert again, so I went alone. Tor Haugland was also unable to go, but one of the oil companies was able to send a representative to do the quality control.

This time we were lucky: the weather was fair and the Syledis gave no problems. We worked steadily, most people working up to 18 hours every day—especially the airgun crew. I believe the party chief, Chris Poundal, slept for fewer than 10 hours during the six days that we worked. The final part of the experiment, the test of the scaling law, was completed at 10.30 on the morning of Friday 13 May. A fine day.

Processing the data

The data processing required special care and Merlin Geophysical assigned the project to Alek Gorski, a senior research analyst with whom I have now been working very closely. The details of the source wavefield determination, both from the near field hydrophones and by the use of the scaling law, were to be provided by the Geophysics Group of the Mining Department. Wietze Eckhardt, Albert Holtslag and Kees van der Schans have each put in several months of work and written many computer programs in this effort. Kees has also computed the offset-dependent synthetic seismogram from the well logs.

The data were shot in reasonably fine weather and are of excellent quality. The errors in the navigation were very small: each line was shot in the same place within generally less than 5 m of the planned position. This error is less than half the distance between two adjacent common mid-points. Differences between the data are therefore attributable only to the differences in the acquisition parameters.

Finally, all the processing methods seem to work; the only problem we have failed to solve to our satisfaction is the deconvolution of the angular-dependent source wavefield, although we expect to be able to make some improvement, eventually, over the conventional one-dimensional approach. Ultimately, of course, the correct thing to do is a full-scale inversion of the data which should be possible one day, given the well-control and the control on the source wavefield.

For the project, we are limiting ourselves to conventional processing. The final sections were anticipated by April 1984 enabling us to produce our final report to the sponsors and to publish our results.

Final word

It has been a far from trivial task to do the experiment. The processing problems have also been far tougher than I expected. Having a nice theory on paper is one thing; making it work on real data is quite another.

Of course it has been worth it, especially to the Geophysics Group of the Mining Department. We now have a very high quality data set which has cost more than £200000 to obtain and which may now be used for education and research within the Department. We have confirmed that it is possible to obtain field data of high quality with the new methods, and so the experiment has been a success. The verdict on each of the new methods has yet to be made and will be made by the sponsors. The successful carrying out of the project has been the result of a lot of effort by a lot of people. Perhaps the most important factor in making it happen is that the people who promised to help have kept their promises. I thank them all from the bottom of my heart.

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