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The Continental Drift Controversy

By Henry R. Frankel
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The Continental Drift Controversy by Henry R. Frankel is a tetralogy beginning with Vol. I *Wegener and the Early Debate*, followed by Vol. II *Paleomagnetism and Confirmation of Drift*, Vol. III *Introduction of Seafloor Spreading* and concluding with Vol. IV *Evolution into Plate Tectonics*. In an earlier review I summarised Vols. I and II (*Preview*, 10.1071/PVv2013n163, pp. 28–30) and here I summarise Vols III and IV to give readers an idea of the vast breadth of content.

Vol. III is divided into six chapters covering (1) *Extension and reception of paleomagnetism/paleoclimatic support for mobilism: 1960–1966*, (2) *Reception of the paleomagnetism case for mobilism by several notable: 1957–1965*, (3) *Harry Hess develops seafloor spreading*, (4) *Another version of seafloor spreading: Robert Dietz*, (5) *The Pacific as seen from San Diego and Menard's changing views about the origin and evolution of the ocean floor*, and (6) *Fixism and Earth expansion at Lamont Geological Observatory*.

Vol. III begins by revisiting the Squantum Tillite anomaly. This Permian unit possessed low inclination palaeomagnetic directions, and without raising the spectre of low-latitude glaciation now confirmed for most of Precambrian time, back in the 1906s a 'tillite' with equatorial palaeomagnetic inclinations was seen as a glaring inconsistency in the palaeoclimate/palaeomagnetic consilience. Two advances changed this. Radiometric dating pushed the Squantum's age back to the early Carboniferous/Devonian and new sedimentological evidence from Bob Dott showed that its origin was more plausibly by gravity movement of rapidly deposited, volcanic-rich sediments and periodic resedimentation by turbidity currents, thus nothing to do with glacial deposition. The anomaly disappeared. Interestingly, Edward 'Teddy' Bullard, the British geophysicist, cast aspersions on the veracity of a number of 'tillites', but also had little regard for palaeoclimatology. Dott may have agreed with Bullard's first misgivings but would have told Bullard to stick to geophysics regarding the second.

The increasing acceptance of Continental Drift in the early 1960s renewed efforts to seek mechanisms bearing in mind that Wegener had emphasised the significance of isostatic equilibrium of continents decades earlier. Mass movement in the upper mantle must be a possibility for isostacy to be maintained. Mantle convection had been proposed by Arthur Holmes, and worked on further by Vening Meinesz and Harold Urey. Keith Runcorn took up the cudgels incorporating emerging information about ocean features. Runcorn took on Harold Jefferys, who steadfastly denied mobilism, by pointing out that elastic behaviour that describes seismic and nutation events of the Earth is incomplete when considering the long term behaviour of solids at high temperature exposed to shear stress, which allows steady creep (irreversible flow) to occur (p. 19). Some discussion follows on whether seismic discontinuities in the Mantle represented chemical changes (disallowing Mantle wide convection) or phase changes (allowing Mantle wide convection), the latter leading to convection of the scale thought to be required for Continental Drift. Keith Runcorn and Ron Girdler were the first to (independently) posit that the central magnetic anomaly over the Mid-Atlantic Ridge was caused by thermoremanent magnetisation rather than induced magnetisation (p. 25), implying rapid cooling of magma.

The 1962 anthology *Continental Drift*, which appeared 50 years after Wegener's theory appeared, and Gordon MacDonald's acerbic dissection is examined ('Continental drift has many appealing features…a favourite topic of pundits condescending to the lay public; it is a grandiose theory involving great changes…eminently suitable for a 'Wonders of Nature' series' (Vol. III, pp. 25,26). Also, under the spotlight is the 1963 Newcastle NATO conference organised by Runcorn (pp. 37–47). Harland's contributions linking mobilism, the Great Infra-Cambrian Ice Age and the Cambrian diaspora of life are discussed on pp. 47–52. New palaeomagnetic laboratories sprang up in Africa, with Ken Graham and Anton Hales at BPI, Johannesburg, and Ian Gough, Mike McElhinny, Dai Jones and Andrew Brock in Salisbury (now Harare), Rhodesia (now Zimbabwe). Neil Opdyke also joined Salisbury (after a post-doc at ANU) on an NSF research fellowship, the first awarded outside the USA. The plethora of new African results is discussed on pp. 85–92.

Ted Irving continued amassing palaeoclimate/palaeomagnetic evidence working with his PhD student, Jim Briden, and David Brown at ANU (Vol. III, pp. 92–109). In 1964 Irving published the first text on palaeomagnetism, *Palaeomagnetism and its Application to Geological and Geophysical* Problems (John Wiley & Sons, New York). Several other influential volumes came from UK symposia around this time. Neil Opdyke and Keith Runcorn continued working on palaeowind directions showing that mid-latitude 'trade winds' were the same back in the Palaeozoic as they are today, when dune fields were re-positioned according to palaeomagnetic declination and inclination.

Chapter 2 moves onto the conversion to mobilism of several 'notables' beginning with Beno Gutenburg (renown for making the first accurate estimate of the depth to the core-mantle boundary, and maybe also for the first military use of seismology detecting gun positions in the Great War). Actually Gutenburg was an early mobilist from the 1930s and embraced the new palaeomagnetic evidence wholeheartedly (p. 115). Vening Meinesz was a fixist but converted to mobilism in the 1960s. Meinesz believed mid-oceanic ridges were remnants of continents and rejected seafloor spreading before being persuaded by palaeomagnetic data of mobilism (p. 123). Gordon MacDonald continued to deny mantle convection (p. 129). The satellite gravity geoid was seen as evidence by MacDonald (and

Munk) as evidence for 'finite strength' (*sic*., do they mean effectively infinite strength?) of the mantle, while Runcorn sees it as evidence for convection (finite, yielding strength?), which is the current interpretation I believe (pp. 131–133). Jeffreys' incessant objections are comprehensively covered complete with the amusing exchange in Canberra where Jeffreys repeated his claim of a 15° gaping bight between Africa and South America after closing the South Atlantic. David Brown, who was then Chair of the Geology Department at Canberra University College, asked had Jeffreys read Carey's work which shows that the 3D fit, using the continental shelves (as per Wegener), was nearly perfect. Jeffreys replied 'I have never read Carey's papers, and I have no intention of doing so' (p. 141). Teddy Bullard pioneered work on heat flow that led him inexorably to convection, but strangely does not acknowledge Arthur Holmes's work in this area decades earlier. Bullard 'comes out' in 1963 as a mobilist. The similarity of continental and oceanic heat flow led to many red-herrings which confused Bullard earlier and had been seen as an argument against mobilism (of course it is now known oceanic heat flow is higher, especially at mid-oceanic ridges). Arthur Holmes's attitude to palaeomagnetism is detailed beginning p. 173; '…has brought about a major revolution in attitude… toward…continental drift'. 'Soviet paleomagnetists, notably Khramov and colleagues who in the 1950s, despite the predominance of fixism among Soviet geologists, made an important contribution to the paleomagnetic drift case based on their own observations and their knowledge of work internationally' (p. 179). Chapter 2 finishes with a 14-page tract on who believed what, when and the many false leads down dead ends.

Harry Hess (Princeton University) is generally recognised as the 'father' of seafloor spreading. His story from fixist to mobilist to proposing the mechanism which worked reads like a science fiction plot (Chapter 3). Hess's research began aboard submarines making gravity and bathymetry observations with Meinesz. Later Hess left observations to others (submarine, ship borne, airborne and satellite) and became a synthesiser. However, the seafloor spreading 'working model' he finally proposed was not handed to him on a plate, it was not joining the dots. Hess earlier rejected mantle convection because of the close correlation between gravity anomalies and topography, and he could not envisage how convection could be maintained for the lengths of time (100 – 200 My) predicated by the geology. There were many blind alleys before he found his way out of the maze, exhaustively recounted in a long tract on pp. 198–275. The clash between the Princeton/Scripps schools (Hess/Bob Fisher), which believed trenches to be convergent features, and the Lamont school (Maurice Ewing, Bruce Heezen and others), which interpreted trenches to be tensional features, is examined on pp. 254–271. Some evidence is presented (p. 236) that Sam Carey converted Hess to accepting palaeomagnetic data and mobilism, although it also seems plausible Hess was sufficiently resilient to fixist dogma that he came to his own conclusions.

The US took its time to turn on to mobilism but by the 1960s many US geologists were converting in droves. Chapter 4 is a longish (pp. 280–319) discourse on Robert Dietz, who was trained in photo interpretation and geomorphology, only ever wanted to study the lunar surface. Dietz worked for the US Navy Electronic Laboratory (NEL) and later the US Coastal & Geodetic Survey (USCGS). In 1946 Dietz went out on a limb proposing a meteoritic origin of the lunar craters. Dietz provided evidence from shatter cones (his specialty) that both the Vredefort Dome and the Sudbury Igneous Complex were of impact origin. He even suggested the Sudbury nickel was cosmogenic. Dietz coined the term 'astrobleme' and proposed that they caused ocean basins on Earth and were related to continental drift (1958, p. 288).

Later Dietz also coined the term 'seafloor spreading'. Dietz's ideas on seafloor spreading were published in the popular press October, 1961, while Hess (November, 1961) had been induced to switch from publishing in *The Sea* to Runcorn's forthcoming book on *Continental Drift*. Dietz never claimed priority over Hess although the order of publishing may seem he had a right, notwithstanding he by-passed peer review. Dietz graciously added a note in proof clearing the air (p. 312).

Chapter 5 (pp. 320–357) documents a productive period in the development of seafloor spreading as a self-consistent hypothesis. Henry Menard went to work with Dietz at NEL as a photo interpreter and later joined Scripps, San Diego. Menard discovered seafloor fracture zones in 1953 and later showed that they were nearly parallel, and almost great circles, in the western Pacific. In 1958 Menard is so close yet so far from putting it all together. Instead he opts to accept convection and a mobile seafloor, but remains a continental fixist (p. 337). Menard took the retrograde step of proposing that mid-oceanic ridges were sunken isthmuses that once provided corridors for flora and fauna to pass along. I often think had I worked harder during my PhD years my thesis could have been so much better, but spare a thought for 'Bill' (Menard), if only he had been more open to mobilism. Menard had witnessed a fellow young scientist being torn apart by a crusty old fixist who had had the temerity to ask after a talk how his ideas fitted in with continental drift (p. 322). The event might have left an indelible scar on Menard but for his collaboration with Dietz. When magnetic anomalies in the north-eastern Pacific were shown to be offset parallel to Menard's fracture zones, he gave up his fixist notions (pp. 338–346). Other workers thought the magnetic anomalies showed the seafloor to be rigid, or blocky, and the congruent continents reflected this rigidity, so there was no way that continents had ploughed through the seafloor. The solution to this impasse would be an important advance in geophysics (p. 342).

The final chapter (Chapter 6, pp. 358–434) revolves around the ideas of Maurice Ewing and Bruce Heezen at the Lamont Geological Observatory. These include Ewing's fixist stance, until everyone else converted so he followed in 1967, and Heezen's support for, and later retraction of, Earth expansion. Ironically, or tellingly, it was under Ewing's stewardship that Lamont workers amassed the data that brought an end to fixism. I say tellingly, because a great research director does not micro-manage and gives researcher the freedom to either 'hang' or 'glorify' themselves. Despite Ewing's leanings he did not try to intervene at the individual level.

Moving onto Vol. IV, this is divided into seven chapters covering (1) *Reception of competing views of seafloor evolution, 1961–1962*, (2) *The origin of marine magnetic anomalies, 1958–1963*, (3) *Disagreements over continental drift, ocean floor evolution, and mantle convection continue, 1963–1965*, (4) *Further work on the Vine-Matthew hypothesis, transform faults, and seafloor evolution, 1965*, (5) *Continuing disagreement over the Vine-Matthew* *hypothesis, transform faults, and seafloor evolution, 1965*, (6) *Resolution of the continental drift controversy, and (7) The birth of plate tectonics.*

While by 1960 the palaeomagnetic evidence that continents had drifted was undeniable, there was such a gap in knowledge of the seafloor that it was not possible to construct a robust model that included the role of the seafloor. The scene was set for some momentous discoveries of the secrets of the oceanic realms. Throughout these volumes some characters are the stars (Irving, Creer, Opdyke and belatedly, Runcorn, etc.) fixed in the firmament, unchanging, their stories reappear in almost every chapter, so interrelated were their activities, while other are like planets and their stories wander, but at crucial stages they align with brilliance and add an important element to the development of ideas. Tuzo Wilson was a bit like a planet. Wilson was the giant of Canadian geophysics who nevertheless held onto fixism until 1961 (p. 37). Earlier, Wilson championed contractionism, continental growth by accretion, as could be interpreted from photo interpretation of Precambrian cratons of Canada, Australia and Africa, and geosynclinal theories with island arcs evolving into mountain belts. The contraction idea held that the outer 70 km 'skin' of Earth had finished cooling and contracting, but from 70 km to 700 km the 'husk' was still contracting and the 'kernel' below 700 km was yet to begin cooling and contracting. Thus, the 'skin' was in compression and the 'husk' in tension. The compressed 'skin' accommodated the growing space problem by up-down displacement along arcuate normal faults, explaining trenches. These ideas must have been elegant, if not compelling in their day, although I cannot see how the still cooling 'husk' can contract more that the already cool 'skin'. Once a mobilist, Wilson was joining up features across the Atlantic like the Great Glen Fault in Scotland with the Cabot Fault in Canada, and making spectacular prognostications faster than anyone. Wilson, of course, is remembered for his idea of transform faults which is fully covered in Chapter 4. This was one of the keys to understanding seafloor spreading as a kinematic model.

Another key to understanding seafloor spreading was the origin of marine magnetic anomalies (Chapter 2, pp. 62–147). In 1962 the Cambridge University marine geophysics group, headed by Drummond Matthews, acquired data from a new marine magnetic survey over the Carlsberg Ridge in the Indian Ocean. Frederick Vine, Matthew's student, suggested a way to simultaneously explain the pronounced magnetic anomaly over the axis of the ridges, and the symmetrical magnetic stripes of highs and lows either side. Spreading from the mid-ocean ridges, while the geomagnetic field polarity irregularly flipped, is obvious in hindsight. This scheme became famously known as the Vine-Matthews hypothesis. However, we humans cannot do things simply as the intriguing tale of Lawrence Morley's shows. (p. 124). Morley, Geological Survey of Canada, had arrived at similar conclusions as the Vine-Matthews hypothesis in 1962, but in 1963 had two papers rejected, one by *Nature* and the second by *JGR*. One *JGR* reviewer wrote, 'This is the sort of thing you would talk about at a cocktail party' (p. 137). Frankel devotes some space (p. 139–141) to why Vine's and Matthews' manuscript was accepted by *Nature* while Morley's was not, but does not descend to the level to suggest the former were from Cambridge while the latter from the colonies. I will not stoop to such temptation either. Vine's and Matthews' paper includes original data and computer modelling that most probably gave it the edge if an editor was weighing up between the two. One possibility Frankel does not discuss is that an editor's decision to accept at least one of them would be enhanced if two papers turned up with the same solution to such a controversial topic of the day. This may be especially so with *Nature* continually on the lookout for papers at the forefront. If the Vine-Matthews paper was submitted alone perhaps it would have been rejected out of hand. But, imagine the CI for the Vine-Matthews paper. John Sclater, then a geophysics PhD student at Cambridge, said the 'tea room' was 'surprised that *Nature* published what we considered idle speculation' (p. 140), not far from the *JGR* comment on Morley's manuscript.

Like Einstein's Special Relativity, if he had not published when he did, there were others with manuscripts ready. The palaeomagnetic group at Salisbury in Rhodesia (Gough, McElhinny and Opdyke) immediately thought of reversals on viewing the seafloor striped anomalies albeit after Gough returned from Scripps late 1962 (pp. 141, 142). Another was PhD student Geoff Dickson at Lamont (MSc Sydney University 1962, p. 144), who was familiar with reverse polarity remanence having worked on Tertiary igneous rocks in the Sydney Basin. Chapters 3 (p. 202) records Manic Talwani's assessment of the Vine-Matthew's hypothesis stating that 'less startling' explanations are possible, so not everyone at Lamont was predisposed to new ideas. Apart from Heezen, no one at Lamont was a mobilist until Neil Opdyke arrived 1963 (p. 440). Generally, the Vine-Matthews hypothesis was accepted rapidly by marine geologists and geophysicists (p. 431).

Chapter 3 to 6 continue in this vein documenting every thrust and parry between the heavy lifters and bickering amongst lesser mortals, until we arrive at Chapter 7, 'The Birth of Plate Tectonics' (pp. 437–616). For the remainder of my space I will attempt to succinctly summarise how the hypotheses of continental drift and seafloor spreading were fused into The Plate Tectonic Theory. In the 1960s, one by one, all the major Earth science schools became mobilists.

The serendipitous discovery of subducting slabs by Jack Oliver and Bryan Isacks at Lamont (pp. 438–456), and their conversion to mobilism, is well worth reading. Oliver was a fixist and sent his PhD student, Isacks, to Fiji in 1964 with some seismometers to see what deep earthquakes were all about; no hypothesis testing, just pure curiosity. There are amusing asides like the British colonials in Fiji attempting to thwart the 'Yank' from receiving any 'freebie' logistical support. This was circumvented by a 'Kiwi' in the met office who apparently had reason ('Pommy b…d') and so Isacks got the assistance he needed and collected some excellent data. After analysing the data, it was clear that a high Q rigid oceanic crust-like layer (at least like what was known about the North Atlantic then) was diving westward into the mantle between Tonga and Fiji. Seismic waves from deep earthquakes beneath Fiji arrived at Tonga with low loss of amplitude. Lamont seismologists were sold on subduction and seafloor spreading.

The next triumph was Dan McKenzie's (Cambridge/Scripps) who by 1967 had solved his perceived problems with mid-ocean ridges (p. 456–469). One problem was the heat flow was too low for the ridges to be sites of upwardly convecting limbs, as per Hess's seafloor spreading model. The second problem was Antarctica and Africa are essentially surrounded by ridges and McKenzie reasoned that seafloor spreading and stable convection in such a scheme were inconsistent. Thus McKenzie challenged Hess's model and proposed passive fracturing at mid-ocean ridges without any mantle root, a lower geothermal gradient and consequently normal thickness crust at ridges. I do not think this is current thinking but there are a lot more data now. Again there are amusing asides whereby McKenzie, who liked the company of geologists who were the reason for him becoming an earth scientist, states geophysicists are 'like geologists, but more intelligent' (p. 458). Perhaps I meant bemusing.

Once the scales fell from Lamont's eyes they worked furiously to catch up (pp. 469–474). Lamont completely redeemed itself in 1967 with four astonishing paper in *JGR* on seafloor spreading in the major oceans. Everyone should read this remarkable set of papers. Jim Heirtzler divided his team into four groups who digitised all the data they had (way ahead of Scripps and Woods Hole), which meant each team could access all date easily and quickly. One of the enduring outcomes of this was the extension of the polarity reversal time scale back to nearly 80 Ma based on the steady spreading in the South Atlantic.

Jason Morgan (Princeton with Hess) made the next splash, and it was the big one – Plate Tectonics (p. 474–494). In early 1967 Morgan worked on cartographically mapping fracture zones, starting in the eastern Pacific. His naval navigation skills on spherical surfaces had alerted him to the fact that Menard's 'almost great circle' fracture zones were actually small circles (the central fracture is very close to a great circle but those either side depart in the opposite sense from each other). By determining the intersection of great circles (perpendicular) to the small circles fractures Morgan defined Euler poles for each oceanic plate. In April 1967 Morgan presented plate tectonic replete with Euler poles and the three classes of boundaries between plates, trenches, transforms and triple-junctions at the Spring AGU meeting, and later submitted a paper to *JGR*. Next Dan McKenzie, who was unaware of Morgan's work, and Bob Parker both now at Scripps, independently discovered their version of plate tectonics (p. 499) using slip vectors along transforms and Euler poles determined by the intersection of great circles perpendicular to these vectors. McKenzie and Parker's paper was submitted to *Nature* in November 1967 just before Morgan was notified his paper was accepted by *JGR* for publication March 1968, pending minor revisions. McKenzie finally finds out about Morgan's paper via Menard and Morgan's much earlier AGU presentation. McKenzie did not know about Morgan's AGU talk because Morgan substituted his plate tectonic talk, understandably, instead of the one described in his abstract and McKenzie had left AGU beforehand. In an act of gallantry McKenzie and Parker make an effort to delay their publication in *Nature*. In late December *Nature* replies to McKenzie 'We must regret… already appeared…December 30th… one of the penalties of dealing with a really rapid journal'! Later McKenzie and Morgan meet and while some might think Morgan would have a right to be annoyed, they decide to write a joint paper on the evolution of triple-junctions (p. 505). The last 100 pages or so is filled with detailing the differences between McKenzie's and Morgan's versions of Plate Tectonic, Isacks discovery of the cause of deep earthquakes, Le Pichon closing the loop showing relative motions of plates to be consistent with their Euler rotations around the globe, the integration of seismology with plate tectonics and details of the evolution of triple junctions among other things.

Common throughout this series is the conflict and disagreement over many aspects of Continental Drift, Seafloor Spreading and the Plate Tectonic model. The reconciliation between Dietz and Hess, and later between McKenzie and Morgan stand out as beacons of integrity. We see that once unshackled from the conventions of the day, and free of the stigma of heresy, the combined intelligence of a community quickly sorts the gems from the dross. It is one of the triumphs of humanity, afflicted with the human condition that it is, that it has nevertheless developed the enterprise called the scientific method to guide us as a community like a pathfinder to overcome the entanglement of the many falsehoods and misleading notions held by individuals, to arrive at a closer and closer approach to the truth, satisfying an increasing number of observations as it does, until predictions can be made at which point hypotheses graduate to theories.

As for Vols I and II there is a profusion of quotes, citations and notes at the end of each chapter packed with extras for specialists and non-specialists alike. The occurrence of only a few blemishes throughout demonstrates, in general, excellent proofreading. The exceptions include: in the *Introduction* to Vol. I (p. xxi) fracture zones 'were found to be not small, but great circles' should be 'were found to be small, not great circles'; did Opdyke go to Lamont early (Vol. II, p. 92) or late 1963 (Vol. IV, p. 441)?; the indexing in Vol. III is sometimes inaccurate, e.g., Vine-Matthews is actually p. 428 not 431; Vol. IV, p. 465 should read anomalously 'low heat flow', rather than 'high heat flow'. There is also a quote of Munk's and MacDonald's that appears twice within a few pages in Vol. II, on pp. 394 and 398, but overall these volumes are high quality.

These volumes should be read by all geoscientists serious about understanding how we have come to learn the inner workings of our planet. Maybe many cannot afford to furnish their own libraries with them but institutional and university libraries should all acquire copies. Hopefully in time they will be available online or as CDs at reasonable prices for all.

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Recording noise

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I recently unearthed a report prepared for Delhi Petroleum in 1982 that documented a seismic experiment designed to obtain information about source generated linear noise. In 1982 we spent considerable effort testing various acquisition parameters so we could minimise noise before it was recorded. The purpose of these 'noise tests', commonly recorded when a crew moved into a new area, was to determine noise characteristics such as frequency and wavelength so that acquisition parameters could be selected. Parameters were selected that maximised the signal while minimising the strength of the unwanted linear noise. In contrast, my most recent experience with onshore acquisition involved recording everything – noise and data – with enough sampling to allow the noise to be removed in the processing sequence.

Figure 1 shows a typical noise analysis display. In this case 24 channels were laid out in a closely spaced receiver spread (3.125 m spacing) and a number of source positions were used in a walk away fashion to simulate a single 312 channel spread. The close spacing of each receiver allowed the noise to be recorded without spatial aliasing so that the wavelength and frequency could be determined and acquisition parameters designed that would attenuate the coherent noise. The main attack on noise was the receiver group array, which summed the output of each element (geophone) of the array so that horizontally propagating noise was attenuated while the vertical propagating reflections were not affected. Other parameters that could be altered to minimise noise were the low cut filter and the near and far trace offset.

Figure 2 is a shot record from a 1982 survey in the Eromanga Basin. The coherent noise is apparent but aliased, as a result processing options to remove it were limited. Often it was simply excised along with any useful data by applying an inner and outer trace mute. In contrast however, the 2006 record (Figure 3) has finely sampled the noise to avoid spatial aliasing and the processing algorithms can successfully reduce it (Shiju *et al*. 2008). Table 1 compares some of the acquisition parameters used in 1982 with those of the 2006 survey.

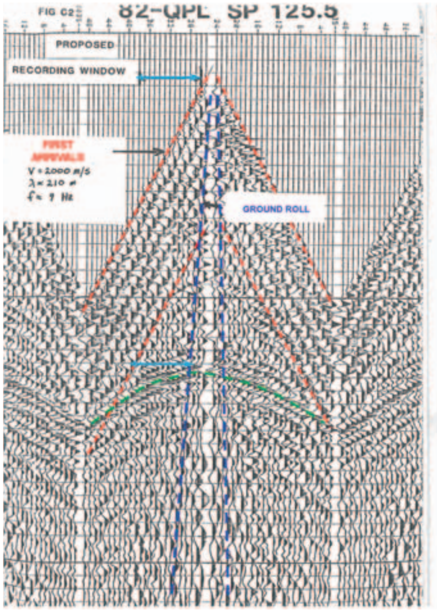

Fig. 2. *Shot record: 1982. Annotations (light blue) indicate the survey parameters were selected to avoid recording the high amplitude noise. Reflections at the target are shown in green.*

When did this change to recording noise rather than signal occur and what has changed to drive this move?

I suspect the change occurred when enough channels were available to adequately sample and record the noise trains so that they could be filtered. My guess is that in Australia this occurred in the late 1990s.

Channel count. The major difference is channel count. In 1982 a good seismic crew had 48 channels (24 either side of the source point) so a wide group interval was used to obtain sufficiently long far offsets. In 2006 the onshore crew I used had thousands of channels, which enabled the receiver interval to be reduced and still retain the long maximum offset (Note: the 2006 survey was initially designed with an 8 m group interval but this was revised to 10 m for operational reasons). The channel count has increased almost 100 times and allowed a closer receiver group interval. This close receiver spacing in turn leads to the use of single elements or bunched groups rather than long arrays with the benefit that distortion of the wavelet is minimised. Figure 4 is a graph that shows the channel count

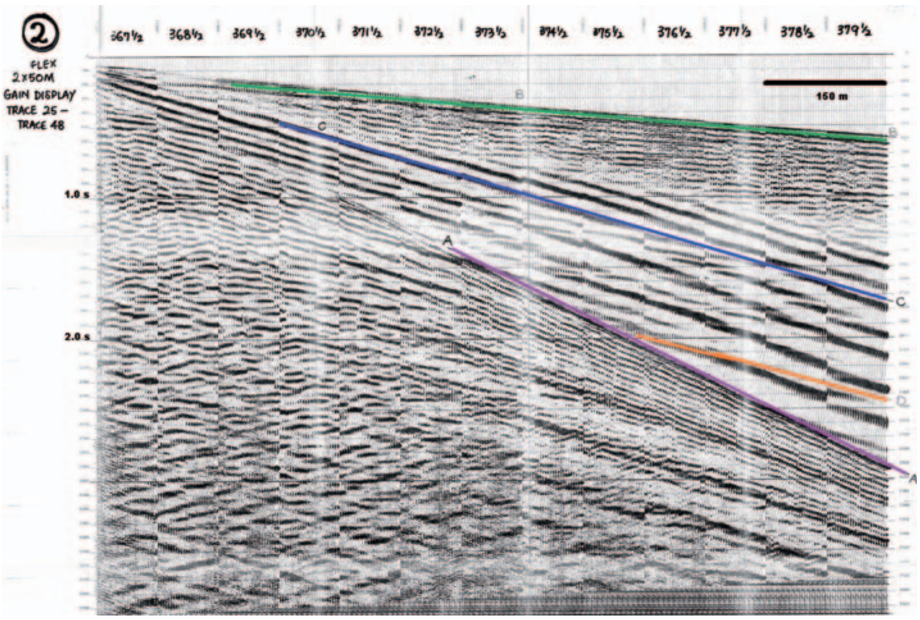

Fig. 1. *Noise test: one of the composite records from the 1982 Breakfast Creek–NAC seismic survey. On this panel four separate noise trains have been identified: (A) airblast; (B) first arrivals/refractions; (C,D) labelled ground roll in the past.*

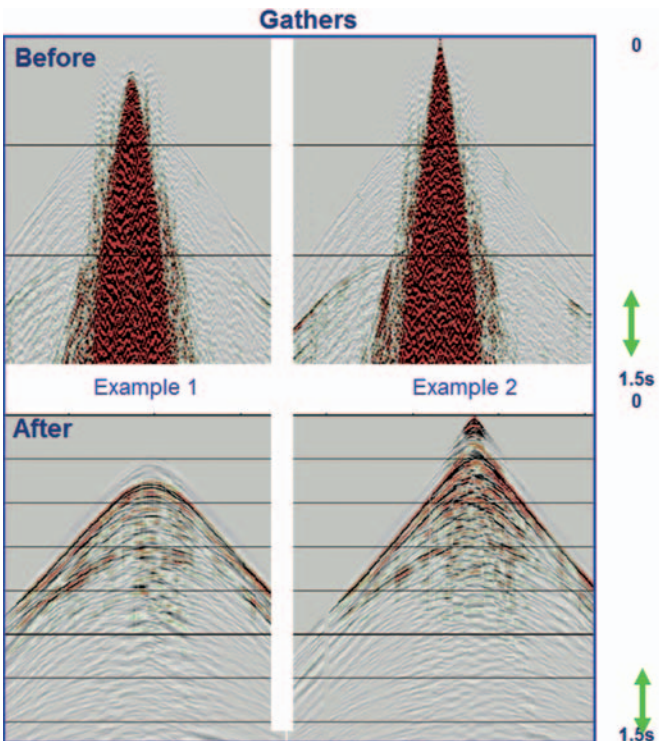

Fig. 3. *Noise attenuation using modern processing on the 2006 survey (from Shiju* et al. *2008).*

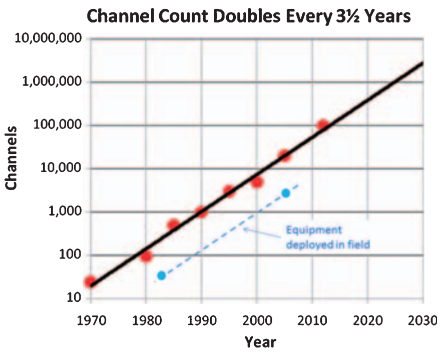

Fig. 4. *Channel count doubles every 3.5 years. Field equipment lags the curve as illustrated by the two blue points.*

Table 1. Acquisition parameters comparison (major differences shown in italics)

	1982 (2D)	2006 (3D)
Recording		
No. of data channels	*48*	*4320*
Sample rate	2 ms	2 ms
Record length	4 s	4 s
Acquisition filter	8–125 Hz	OUT-OUT (+ antialias filter)
Source	Single hole – 2.5kg Anzite@12.5m	1 vibrator 8–110 Hz 1 x 8 s sweep
Source spacing	*150 m*	*10 m*
Source line spacing	–	180 m
Receiver		
Group interval	*75 m*	*10 m*
Receiver line spacing	–	150 m, 10 line swath
Geophones/group	12	12
Group array	*Linear 12 @ 6 m spacing*	*12 in 2 m circle*
Near trace offset	*188 m*	*~5 m*
Far trace offset	1988	–
Nominal fold	12	60

increasing with time in a seismic version of Moore's Law. (The new generation Schlumberger recording system has a channel count of 150 000.) Practically the number of channels has now increased to a level where management of all the cabling is becoming an imposition and wireless technology is providing a viable alternative.

Dynamic Range has also improved. The 1980s instruments incorporated a 14 bit analogue to digital converter, which was adequate but there were substantial benefits in using receiver arrays to attenuate the high amplitude noise such as ground roll. Recording instruments now use 24 bit sampling, which enables the full waveform to be recorded without losing the subtle amplitude variations of weak reflections.

Processing algorithms have developed and can now remove noise (Figure 3) if it is adequately sampled. This requires closely spaced, effectively point receivers to ensure noise trains are not aliased or distorted. The noise-reducing algorithms are applied pre-stack so improvements in computing performance have also been a benefit. When properly sampled it is apparent that rather than being purely noise the unwanted energy is contained in a number of noise cones or diffractions, which propagate from scattering points in the near surface and can be effectively predicted and removed.

Compare the 2006 shot records (Figure 3) with those from the 1982 survey (Figure 2). The processing filters applied to the modern records have removed most of the noise and reflections are apparent across the gather. In contrast, the old record has significant noise that was best removed by muting or selecting an offset range between the noise trains, i.e. the noise affected areas were avoided but the offset range was limited.

Is there a practical limit to the number of channels?

Maybe the seismic acquisition contractors can answer this, but if there is a limit then this limit is also increasing. With wireless technology replacing cables and new designs reducing weight and power consumption per channel a million channels is a distinct possibility.

Reference

Shiju, J., Bowyer, G., and Micenko, M. 2008. *Mangala Field High Density 3D*. Proceedings SPG 7th International Conference and Exposition, Hyderabad 2008

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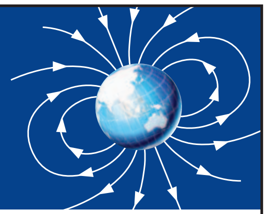

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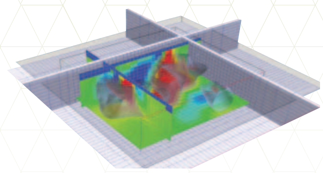

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21–26 Jul	IEEE GRSS International Geoscience and Remote Sensing Symposium (IGARSS) http://www.igarss2013.org	Melbourne	Australia
August			**2013**
9 Aug 11 Aug 16 Aug	SEG Distinguished Instructor Short Course 2013 David H. Johnston, ExxonMobil: Making a difference with 4D: practical applications of time-lapse seismic data http://www.seg.org/disc	Perth Melbourne Brisbane	Australia Australia Australia
11–14 Aug	ASEG-PESA 2013: 23rd International Geophysical Conference and Exhibition http://www.aseg-pesa2013.com.au/	Melbourne	Australia
September			**2013**
8–11 Sep	Near Surface Geoscience 2013 http://www.eage.org	Bochum	Germany
30 Sep–4 Oct	Sustainable Earth Sciences 2013: Technologies for Sustainable Use of the Deep Sub-surface http://www.eage.org/events/index.php?eventid=960&Opendivs=s3	Pau	France
October			**2013**
6–11 Oct	SAGA 13th Biennial Conference and 6th international AEM 2013 http://www.saga-aem2013.co.za/	Mpumalanga	South Africa
7–10 Oct	7th Congress of the Balkan Geophysical Society http://www.eage.org	Tirana	Albania
November			**2013**
18–20 Nov	The 11th SEGJ International Symposium: Geophysics for establishing a sustainable secure society http://www.segj.org/is/11th/	Yokohama	Japan
24–27 Nov	Second International Conference on Engineering Geophysics http://www.eage.org	Al Ain	UAE
January			**2014**
20–22 Jan	The 7th International Petroleum Technology Conference (IPTC) http://www.iptcnet.org/2014/doha/	Doha	Qatar
February			**2014**
25–27 Feb	SPE/EAGE European Unconventional Resources Conference and Exhibition http://www.eage.org/index.php?evp=1979	Vienna	Austria
March			**2014**
9–12 Mar	GEO 2014: 11th Middle East Geosciences Conference and Exhibition http://www.geo2014.com/	Manama	Kingdom of Bahrain
April			**2014**
7–10 Apr	The 6th Saint Petersburg International Conference & Exhibition http://www.eage.org/index.php?evp=1979	Saint Petersburg	Russia
June			**2014**
16–19 Jun	76th EAGE Conference and Exhibition incorporating SPE EUROPEC 2014 http://www.eage.org	Amsterdam	The Netherlands

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