Instrumental Effects as seen by a Data Analyst

Gabi Laske

IGPP, Seismometry Seminar, Feb. 19, 2002; **DRAFT**

Disclaimer

For this talk, I represent the ordinary data analyst who sends requests to a ”data management center” (DMC). I represent the data analyst (or end-user) who, often blindly, uses what is conveniently provided, often not knowing just how much it takes to get a ground motion from out there in the field all the way onto my computer screen. I will show the bad things in a seismogram, not the good things. Naturally, I will point fingers and highlight bloopers that are happening to my fellow colleagues at ”network operations” (NO) and the DMCs. Nobody is perfect and bloopers happen but it is impolite to point at other peoples' bloopers in public. So, **to whom it may concern: I apologize!** Writing this summary of my talk, I also realize that some issues need a few additional remarks that I omitted in the talk by either forgetting to mention them, running out of time or being distracted by the recording camera. I will therefore include these remarks as footnotes.[1]

Data Availability and Selection

”The good must be put in the dish, the bad you may eat if you wish”

In this section, I discuss issues of data return. I would like to get an idea of how many data actually make it from the field into my publications. A network never runs at 100%, e.g. some stations fail to return continuous data.[2] Even though a station may produce nearly continuous data which are shipped by NO to the DMC, these data may not be suitable for a particular study. Probably the most restrictive demands on high–quality seismograms come from free–oscillation studies. Such studies typically require, among other things, 1) recordings from observatory–quality instruments, 2) continuous data over several days and 3) a high signal-to-noise ratio over this entire period. To assess data availability, I have gone through my database which includes records for 37 large earthquakes between 1986 and 2001. This database includes all records that are (or were) available at various DMCs (these include the IRIS-DMC, GEOSCOPE, sometimes in–house IDA, and lately GEOFON and the Northern California Earthquake Data Center). Prior to an analysis, we go through all records interactively and edit them (e.g. remove ”easy” spikes; flag short, gravely noisy sections in otherwise high-SNR records). We also decide whether a record is to be **deleted** (e.g. because of lack of data or lack of seismic signal) or should be **unloaded**, i.e. put on hold because it cannot be used in a free oscillation study but in some other study. For example, a typical free oscillation study requires record lengths of more than 50h (more than 80h when high–Q inner–core sensitive modes are examined). A moment tensor (MT) determination on the other hand, which I usually do for all great earthquakes, requires only 10h-long records.

[1] I should mention that I myself have been in the field many times to deployment temporary seismic sites, mainly in my former life as a travel–hungry student. So it is entirely possible that similar bloopers that created other people's nightmares have been caused by me.

[2] Possibilities of failure range from a failing sensor, failing electronic equipment, to failing data recording, and/or NO processing, or even a faulty ”transmission” between NO and DMC.

Figure 1 shows histograms of ”data fitness” for some of the 37 events (some are omitted because of space limitations).

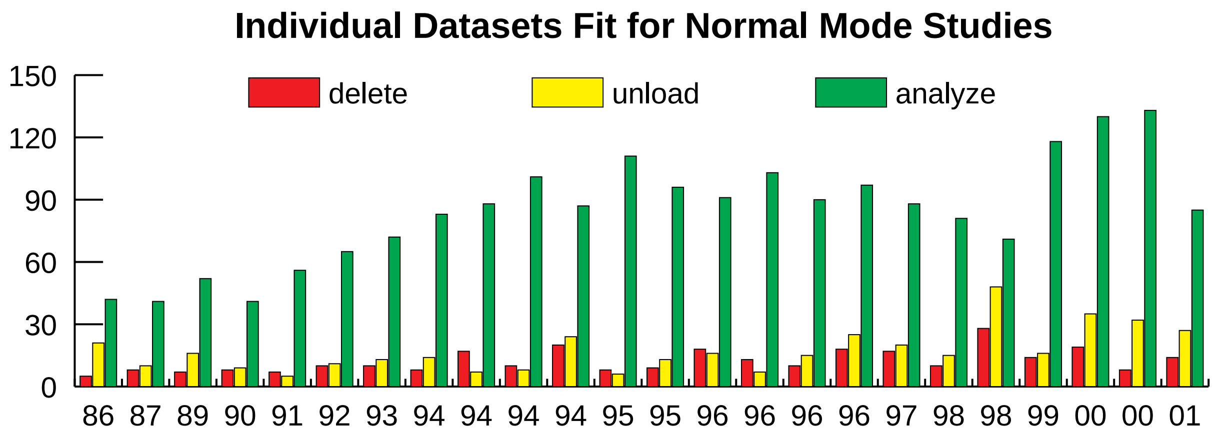


Figure 1 (*data-columns.eps*)

The total number of records has increased steadily until the mid-late 1990's. Of course, the number of available digitial recordings was much less for times before 1985 (typically 25 suitable records per event). I should add that our database includes old IDA (ID) as well as all global seismic networks (AS, SR, DW, HG, GT, IU, II, G, GE) and some regional networks (CD/IC, TS, MN, and lately BK and GE). The year 1993 approximately marks the time when the old networks were slowly phased out (e.g. ID, SR) or modernized and new GSN sites started to become available. A slight decline in Figure 1 at the end of the 1990ies is due to the fact that some networks (e.g. MN, GE) were not available at the time of my requests and I have not yet gone back to re-request the missing data. In the last few years, the total number of records (as well as the ”unloads”) increased. This is because my requests include TS (and now BK) who had been operating STS-1 and STS-2 in the past. Recently, TS joined TRINET and requests to the DMC are responded to by automatically providing all of the CI records (mostly STS-2 and CMG-3T), a lot of which are weeded out[3].

The fraction of discarded records depends somewhat on the earthquake. For example smaller ones are likely to produce less useful records. Overall, the fraction of discarded records is quite stable though. Of the 37 events I have looked at, 28 events happened between July 1993 (the ”upgrade year”) and June 2001. I calculate the ”typical data return rate” by averaging the histograms for these 28 events. The corresponding pie chart is shown in Figure 2. The fraction of data fit for a mode analysis can vary between 60 and 85% and is 73% on average. Considering that the ”up-time” of a network may typically be 80%, this implies that only 58% of the ground motion that is supposed to be recorded in the field make it into my papers. This is, perhaps, a suprisingly low number.

Just to provide a quick idea of what category ”delete” and ”unload” seismograms look like, here are some examples. I start with records from the great Arequipa earthquake on June 23, 2001, which is said to be the largest earthquake recorded within the last 30 years. Figure 3 shows a category ”delete” seismogram[4].

[3] There are some nice records though, e.g. PLM, even though this has an STS-2

[4] I start with II seismograms because these were the first available to me, thanks to Pete, and not because only II has bad examples!

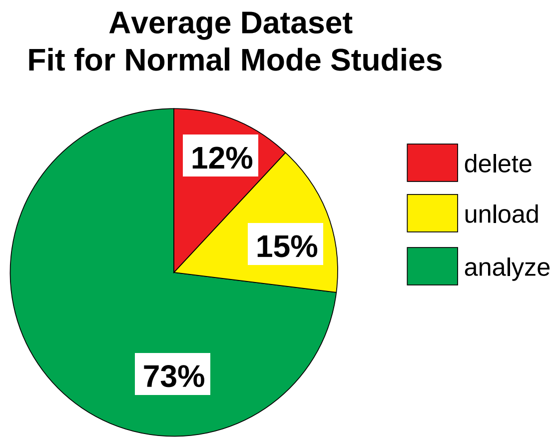


Figure 2 (*data-pie.eps*)

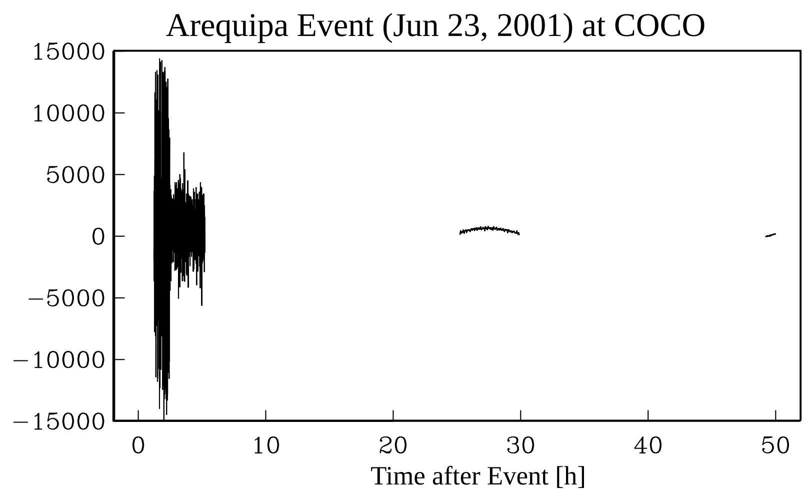


Figure 3 (*seis.coco.eps*) A category ”delete” seismogram.

Obviously, this seismogram has sections of missing data that are too large to make the record useful for any study. This kind of data sparseness can be found in any network, not just II! Other examples for category ”delete” seismograms are those with no obvious seismic signal (e.g. records of ”bit noise”) and when ”spikes” of any kind are larger than the seismic signal and occur too often to be ”edited out”.

Figure 4 shows an example for a category ”unload” seismogram. The first 4 hours of this seismogram have a good SNR (so this part could be used for a MT study), but then big spiky signals make the following 5 h useless for an analysis. As I said before, the June 23, 2001 Arequipa Earthquake in Southern Peru is the largest recent earthquake so the spikes shown here are huge! We also have to remind ourselves that gaps in the data alter the structure of a spectrum. Usually, small gaps in the data can be modelled in single record analyses. For example, to measure mode frequencies we can compare the spectrum of such a record with that of a synthetic one whose gap structure is identical to that in the data. Data gaps of the size shown here however, are very difficult to deal with, especially if they occur near the front end of the record. Due to their different gap structure, such records can not be used in stacking procedures, such as our techniques to calculate singlet and receiver strips. The particular

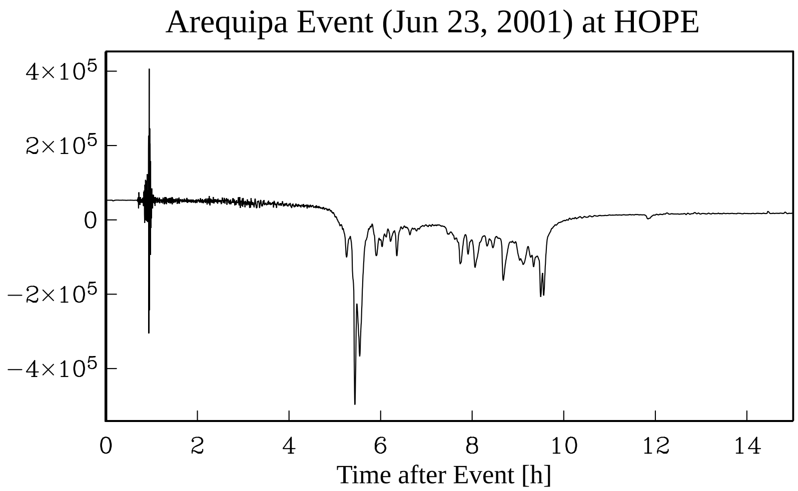


Figure 4 (*seis.hope.eps*) A category "unload" seismogram.

record shown here is an "unload" seismogram for a second reason. Figure 5 shows the full 3-component record and the north component has no seismic signal (the signal is non–zero but not seismic) so that the horizontal components cannot be rotated into radial and transverse directions.

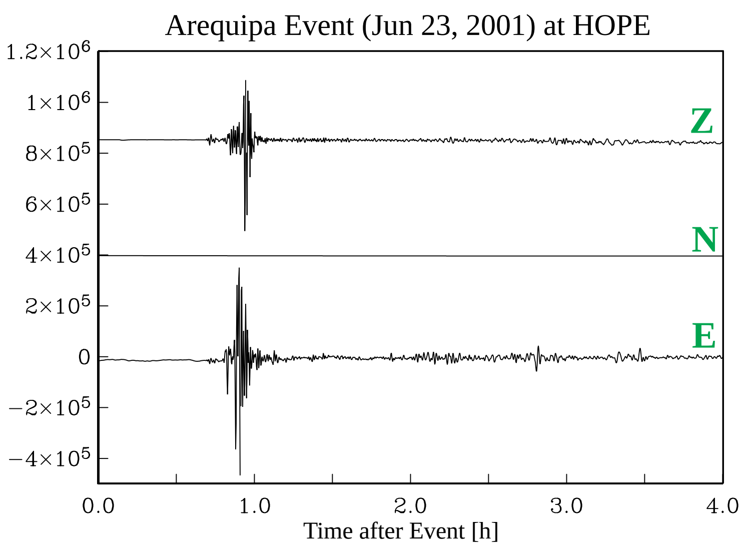


Figure 5 (*seis.hope-3c.eps*) The full 3–component record of Figure 4.

Figure 6a shows a seismogram that may be identified as an "unload" but perhaps only after inspecting its spectrum (Figure 6b). The time series appears to have a high SNR during the first 12h but a harmonic signal is clearly visible after that. The cause of this signal is unknown and could be caused "internally" (instrumental) or "externally" (environmental). Unfortunately for mode seismology, this signal has a period of around 39 min and is therefore located near the low end of the free oscillation band where the spectral amplitudes from seismic signals are typically quite small. Figure 6b confirms that the seismogram contains a complex harmonic signal near 39 min

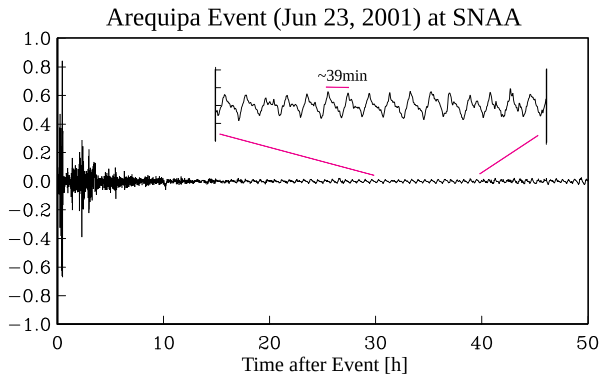


Figure 6a (*seis.snaa-all.eps*) A potential category ”unload” seismogram...

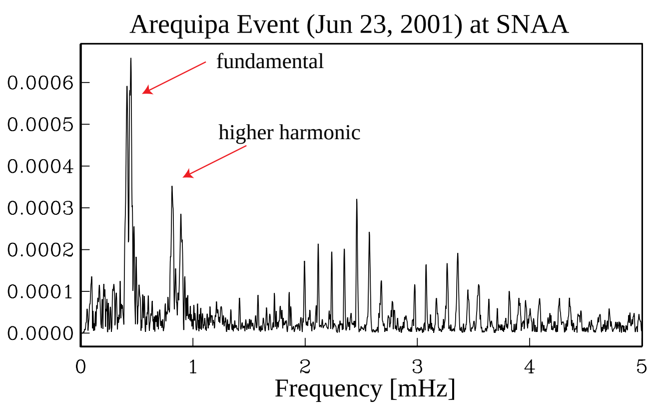


Figure 6b (*spec.snaa.eps*)... and its spectrum.

plus its first harmonic. This signal dwarfs the fundamental mode spectrum of the great Arequipa earthquake and we unload this time series (note though that the spectrum may be ok beyond about 2 mHz).

A tricky record that may pass as a ”to be analyzed” if not visually inspected is the one in Figure 7. The record looks noisy but ”normal” after automatic filtering. Of course, we do not have to be trained extraordinarily well to find that the negative values in the later parts of the time series are set to ”zero”. I am sure that NO experts can tell you immediately what instrumental defects can cause this type of phenomenon as well as the two-sided ”comb-like” structure in the first part of the time series. I as the end-user can only say: ”unload”.

Jeanne next door has to endure quite a few screams of disappointment from me. The next example shows why this happens. When you do this business for a while, you develop a ”personal relationship” with some of these sites. For one reason or another, they seem notorious for producing faulty records. So every time when an earthquake happens, you anxiously sift through the new files and you want so badly that this one site has a really nice record, just this one time! Such a station is NIL (Nilore/Pakistan)[5]. And for the Balleny Islands Event in 1998, this station produced a beautiful record (Figure 8a).

... for about 20h. What then happened ... nobody really knows (Figure 8b). The Balleny Islands event also

[5] **NEVER** name a station NIL!

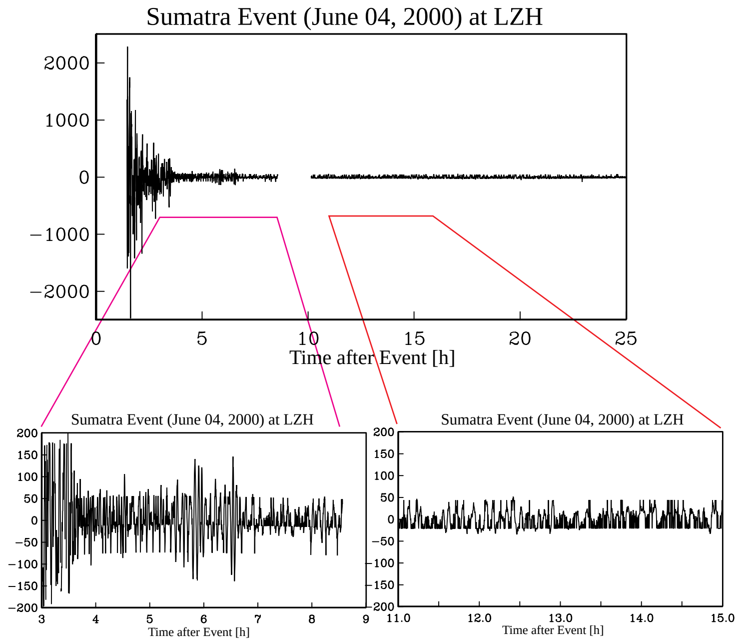


Figure 7 (*seis.lzh-all.eps*) An ”unload” seismogram hard to detect as such.

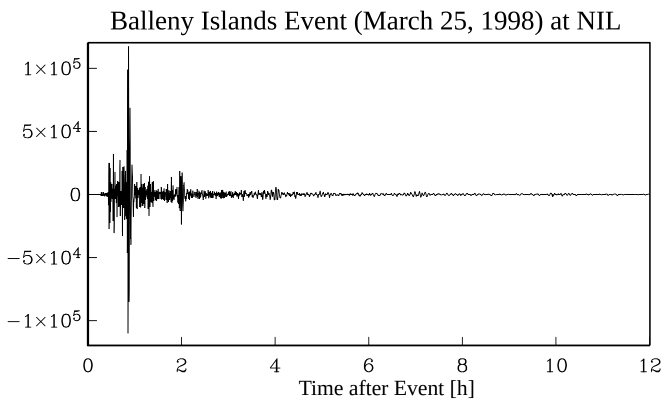


Figure 8a (*seis.nil.eps*) A high SNR seismogram at station Nilore/Pakistan.

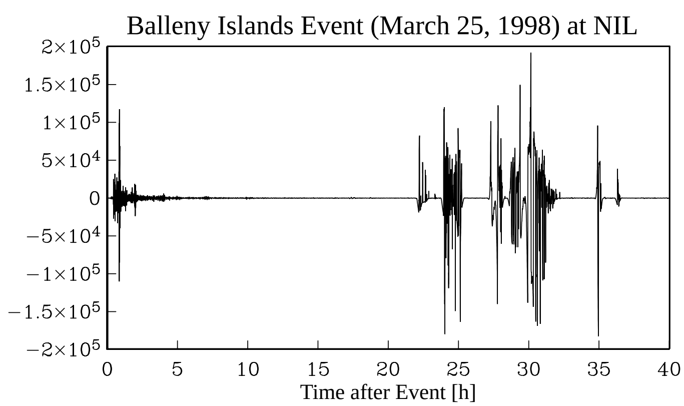


Figure 8b (*seis.nil1.eps*)...the same record 20 h later.

was a huge earthquake, but the spikes beginning about 23h after the earthquake, which get only worse as time goes on (not shown here), dwarf the seismic signal[6]. I should mention that this particular record is a borderline ”unload”. For some mode studies (e.g. high–frequency modes), 23h is close to Q-cycles of a mode which is the ideal record-length for this mode to be analyzed. For cases like NIL we typically keep the record in the database but flag everything after 23h. Our analysis algorithms then check for the amount of available data and decide if this is enough for a particular mode or not. I should also add that NIL has produced fine records in the past that are suitable for an MT study (only 10 h needed).

Figure 9 shows a quite annoying feature in a seismogram that is operator induced and needs to be edited before the filtering and removal of the instrument response (we need time series of ground acceleration).

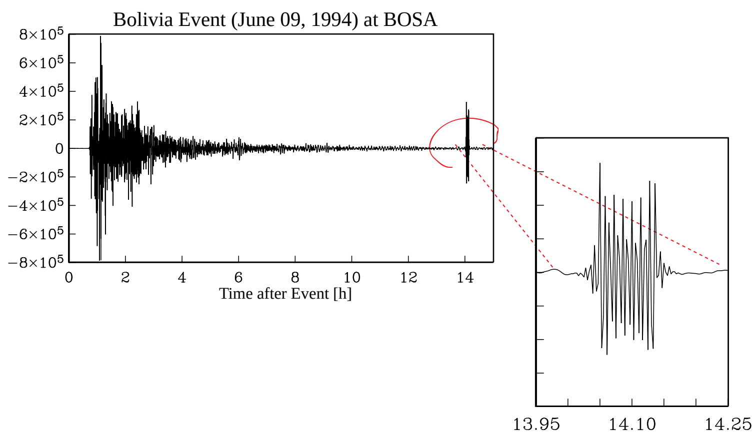


Figure 9 (*seis.boli-bosa-all.eps*) Calibration pulses at GTSN stations.

The little pulses that, to my knowledge, may or may not show up on a regular basis, are calibration pulses that are now pretty much unique to the GTSN network (GTSN, or GT, is run by AFTAC). The greatness of the 1994 Bolivia Earthquake emphasizes just how huge these calibration pulses can be. These pulses are a nighmare to deal with because no matter how you filter the time series before you remove the instrument response, these pulses always seem to be larger after this process. We usually end up flaging such pulses but we have to remind ourselves once again that this procedure alters the character of the spectrum. Luckily, the pulse shown here is pretty much the largest I have found during my quick search for an example before my talk. However, the danger of these pulses lies in the possibility that I don’t detect them during the editing process of my database when they occur early in the time series. In such cases they are likely hidden in the seismic signal but significantly alter the spectrum. [7]

[6] We do not know exactly what causes these spikes but we know that the problem is the sensor. Only the vertical component of the KS54000 seems to be doing these funny things and the problem is almost impossible to track down. You go through all the effort to take the sensor out; no spikes. You put the sensor back in; spikes.... The sensor will finally be replaced in the near future. In the meantime, only the data of the horizontal components are shipped to the DMC; Pete Davis, personal communication.

[7] Guy and Bob Woodward actually wrote a paper on how to model these calibration pulses for the old SRO stations (AS, SR). SRO used to have these pulses irregularly, then once a day and then once every 5th day after Guy complained to NO. I have not gone through ”my” calibration pulses systematically so I have no feel for these

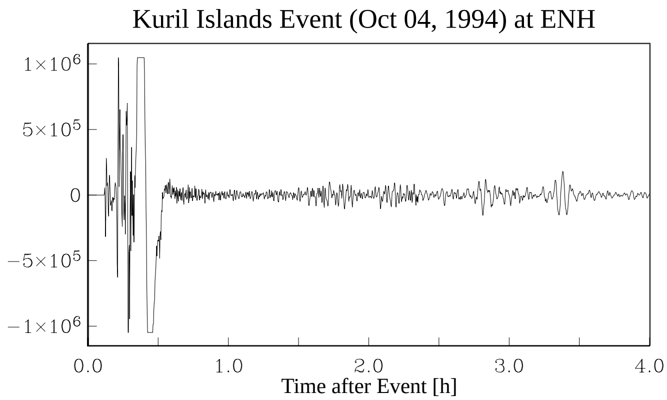


Figure 10 (*seis.nonlin-enh.eps*) A non–linear instrument response to a large R1 wave train.

The next figure (Figure 10) shows another instrument–induced problem in a time series. This is a relatively old recording on an instrument of the CDSN network that has recently been upgraded with the help of IRIS. The picture shows the instrument's non-linear response (with its typical long-period relaxation) to a very large R1 surface wave train. The Kuril Islands earthquake was only about 20 degrees away and was actually greater than the great Bolivia earthquake that happened earlier in the same year. Back in 1994, ENH had a 16-bit datalogger and its dynamic range was obviously not large enough to allow for the linear recording of strong seismic. signals[7a] In saying this, I simply assume that the sensor itself can handle such motion. I should add that this non-linearity in the seismogram does not affect my mode studies, since I typically start at least 1h after the event time. [8]

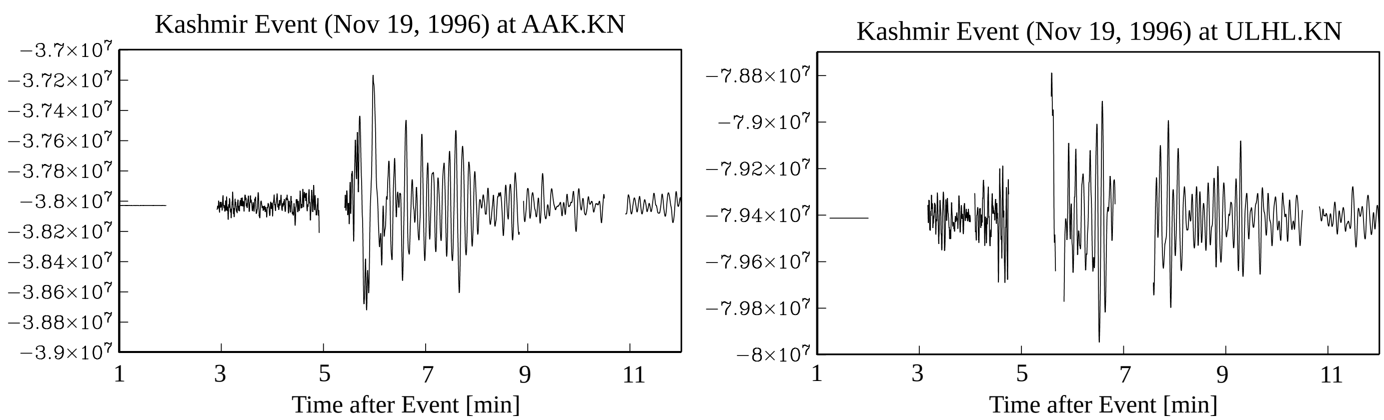


Figure 11 (*seis.kashmir-kn.eps*) Telemetry dropouts at a regional array.

The last example of records that cause my occasional screams leads us away from free–oscillation studies into the domain of surface wave studies. In these, I often use records from temporary or semi–temporary networks. Being

things and "edit them out".

[7a] Actually, Guy remarks that the feedback loop is the problem and not the 16-bit datalogger and I assume that he's right.

[8] ENH has an STS-1 and was an excellent producer of high-quality free-oscillation spectra. This site was "upgraded" in September 1997, presumably with a "better" datalogger (the sensor remained) and has sadly produced second-rate spectra ever since.

at IGPP, of course my first choice of such networks are Frank's. Figure 11 shows the 1996 Kashmir earthquake (Ms=7.1, Δ=800 km) at two KNET stations. The intermediate–period Rayleigh wave is clearly coming in. This is the waveform immediately after the hole for AAK but the long periods are probably spread out across the hole. I cannot use these records for a dispersion analysis, for obvious reasons. The holes in the time series are caused by telemetry dropouts that happened during the earlier days of Frank's networks.[9]

STS-1 or STS-2

"... that is the question!"

This seminar was arranged for several reasons. We all know that we are running out of observatory-quality seismometers. The STS-1 are no longer built and the KS54000 probably not either[10]. The question arises of what should we do in case there is no 'next generation' observatory–quality sensor within the near future. Could we do with the next best thing (STS-2/CMG-3T)? This question is difficult to address but does need to be discussed. If we want to records ultra–low frequency free oscillation, we likely cannot replace the STS-1 with STS-2s and the fear is that the retirement of our last STS-1 will coincide with the ultimate great earthquake that we have been waiting for for 40 years (unlikely but possible).

In Figure 12, I show you what a collection of spectra should look like. The spectra on the left hand side are 11 of the 14 spectra of the GEOSCOPE global seismic network that were available to me immediately after the great Arequipa Earthquake last June. This earthquake was big enough to excite the gravest free oscillations and mode ${}_0S_3$ should easily be visible on the spectra. This is indeed the case for quite a few spectra but the majority of all spectra I have so far is too noisy for ${}_0S_3$ to have a high SNR. The GEOSCOPE spectra are actually exceptionally good – though II also has exceptionally good spectra which I don't show here (e.g. BFO, PFO and SUR). On some individual spectra, we can even see the elusive ${}_0S_2$ (e.g. SSB, and also BFO.II) which has been seen for perhaps a handful earthquakes in the last 30 years. These are indeed observatory–quality spectra which I would like to see much more often (and we should, given the magnitude of this earthquake and the quality of the instruments). We all know too well that the quality of a seismogram does not depend on the quality of an instrument alone, and that perhaps the environment plays the dominant role. The panel on the right hand side of Figure 12 is a collection of spectra that I received after a data request to the German GEOFON DMC. With the exception of KMBO (a global GEOFON station near Nairobi) and ISP (a Mednet/GEOFON station in Turkey), the quality of these spectra is quite a bit poorer than that of the GEOSCOPE network. I'd like to draw your attention however to the spectra with a red circle. Some of these have an excellent SNR down to about 0.5 mHz (e.g. IBBN, RGN, but also WLF, and even PSZ). All these sites have STS-2 sensors and, in fact, should not have a spectrum like this. Figure 13 shows the velocity responses for SSB (the GEOSCOPE site having a STS-1) and RGN (the GRSN site having a STS-2). The response for RGN rolls off much faster at low frequencies than that for SSB. Below 3mHz, the response of the STS-2 is less than that of the STS-1 by almost a factor 10 (compare RGN adjusted, which is shifted downward for better comparison).

I should say that none of the STS-2 sites are part of the global GEOFON network (e.g. RGN is a station of the German Regional Seismic Network) but are provided in response to a generic request to the DMC. These

[9] I am not exactly sure I understand how these networks are run. Presumably, there is no recording medium involved, so nobody has to go and change tapes at each site. The signals are instead sent by radio to some recording center. Radio has hick–ups ... Gabi loses Rayleigh waves.

[10] And, of course, Erhard is spending his sabbatical here, so what better time could we possibly have this seminar?

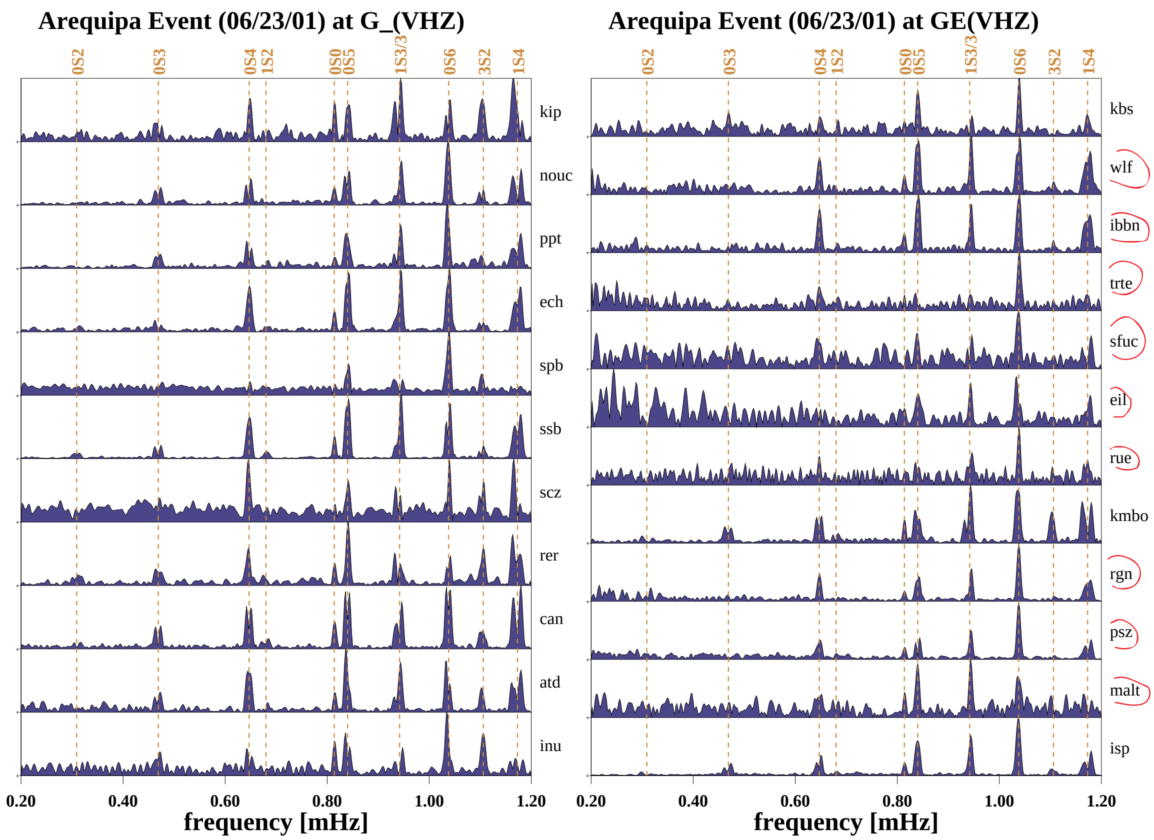


Figure 12 (*specs.peru.eps*) Observatory–quality ultra–low frequency free oscillation spectra

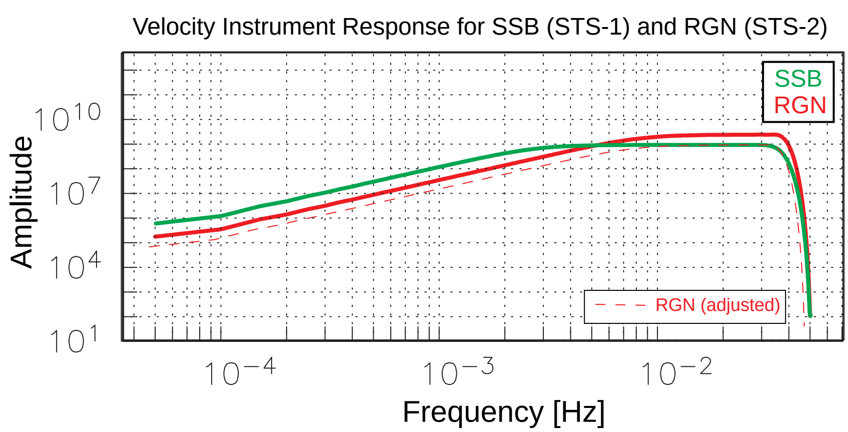


Figure 13 (*resps.ssb-rgn.eps*)

spectra are truely spectacular and we know that, at some sites, they are consistently so. For example, Ruedi Widmer-Schnidrig used the spectra at RGN (among other GRSN spectra) to study the continuous free oscillations of the Earth.[11] The question arises of what makes these sites so special. Have the NOs done anything unusual to the installation to achieve such a high SNR? RGN (Rügen, Germany) is an island in the Baltic Sea and to my

[11] Note however, that none of the STS-2 sites has a ${}_0S_3$ which is probably truely burried in the instrument noise.

knowledge, it is just a pile of sediments, as is the larger part of Northern Germany. Erhard Wielandt, who is present at this seminar, comments that this is a miracle that ”we do not understand”. There is nothing special about the installation, no special vault but just a ”hole in the ground”, with no special shielding[12]. To get to my point, it seems that though this is not the rule (plenty of experience with IU and CI/TS sites) in some cases, the STS-2 produces spectacular free–oscillation spectra. We have numerous GSN sites, with either an STS-1 or a KS54000, that perform only poorly in the very-long period band. Some are identified as consistently bad, i.e. I have never seen a high-quality spectrum and there is no prospect of improvement[13]. These are often important istallations for other purposes (e.g. CTBT sites or excellent body wave sites) and perhaps we should discuss whether the installation of an observatory quality sensor makes a lot of sense or whether a STS-2 would do and the replaced STS-1 could be used at a ”better” site.

The Effects of Calibration ...

”... on Arrival Angles; Shear Wave Splitting ... and and and...”

I am probably expected to talk about sensor orientations and the like, so I’ll briefly recall how I ”detected” some of the horizontal component misalignment at GSN sites.

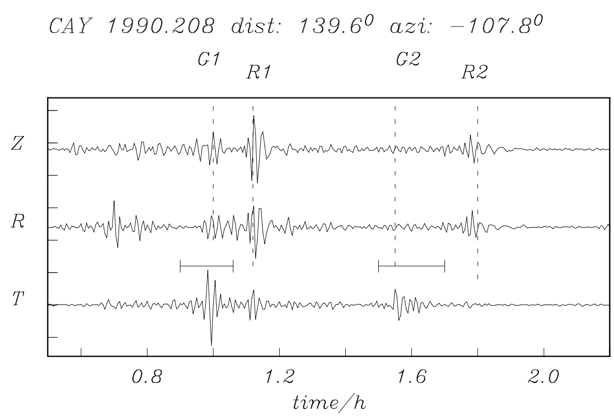


Figure 14 (*seis.3-comp.eps*) A typical 3–component surface wave seismogram.

Figure 14 shows a typical 3-component seismogram of the first two Rayleigh (R1, R2) and Love (G1, G2) wave trains. On a laterally homogeneous Earth, Rayleigh waves are visible only on the Z and R components, while Love waves are visible only on the T component. We see quite clearly a wave packet on the T component arriving at the R1 arrival time. The fact that R2 and G2 do not show up on the ”wrong” component is strong evidence that a faulty component calibration cannot be the cause for the anomalous R1 signal (the signal of G1 on the Z and R components could be G1 but could also be Rayleigh wave overtones that arrive prior to R1). These waveform anomalies are due to lateral refraction of surface waves in a heterogeneous Earth, away from the source–receiver

[12] My own conversation with Winfried Hanka, the executive director at GEOFON, revealed that RGN made use of the ”Wielandt-shielding”, though I do not know what this means. Winfried wrote a report on this for IRIS and the online version is found at http://www.gfz-potsdam.de/geofon/manual

[13] I will not mention these sites but I am happy to discuss these in private. Some of these sites are known to have environmental noise problems for about 99% of the time that will likely remain.

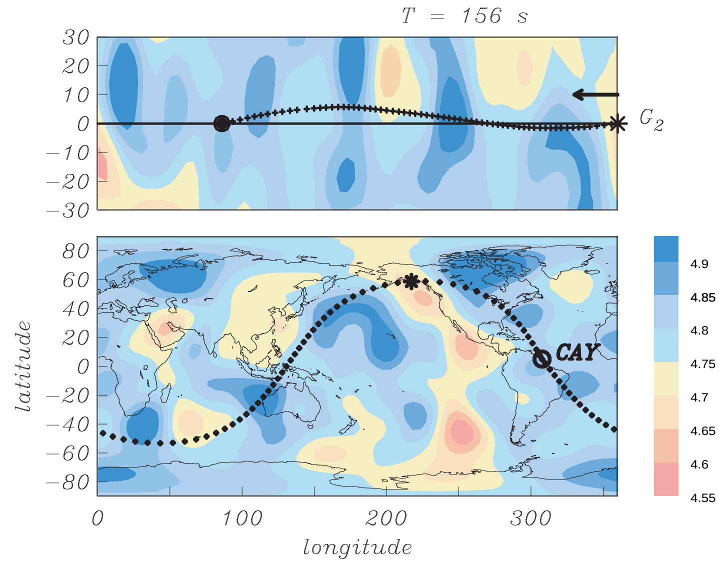


Figure 15 (*ray-map.eps*. A true ray on a laterally heterogeneous Earth. The top panel shows the structure along the source–receiver great circle which has been rotated to be along the equator; the asterisk is the source, the dot is the receiver.

great circle. The wave packets then arrive at the recording station at an angle (see Figure 15 for the concept) which can be measured on the three-component seismogram, as function of wave type and frequency. Using a linear theoretical framework, these data can be interpreted in terms of laterally heterogeneous structure, as function of frequency (i.e. a global phase velocity map). Before talking about problems, let's first have a look at the data. For each station, I typically plot arrival angles in rose diagrams (see Figure 16 for the concept).

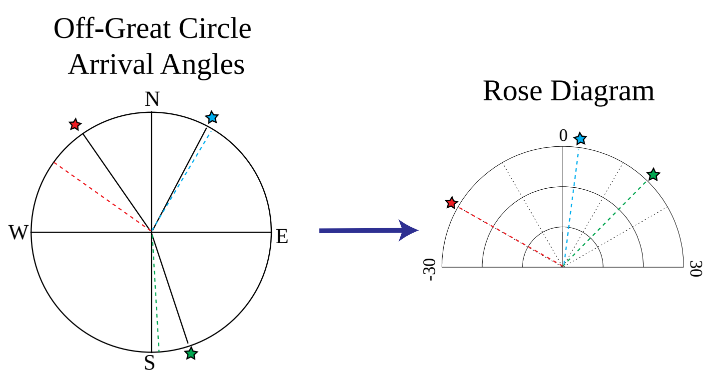


Figure 16 (*pol.example.eps*). Concept figure for the rose diagrams, in which I present my measured arrival angles. The left panel shows the coordinate system of a seismometer and back azimuths of three earthquakes (stars). The dashed lines mark the "measured" arrival direction for each of these earthquakes (the black lines mark the great circle direction). The right panel shows a rose diagram in which each arrival angle is plotted with respect to its great circle direction (which is always the 0°-line). In this presentation and scale, arrival angles range from -30 to 30°.

Figure 17 shows arrival angles for 6.4 mHz Love waves I measured at 4 GSN stations (PPT, NNA, AAK and PFO). The angles in a typical dataset vary between ±10° and cluster around 0°. Sometimes, the data cluster around other angles (e.g. around -5° at PPT). In principle, if the data coverage is azimuthally highly uneven, this "shift in the mean value" can be caused by structure. For well-sampling data, this is somewhat unlikely and more so if this

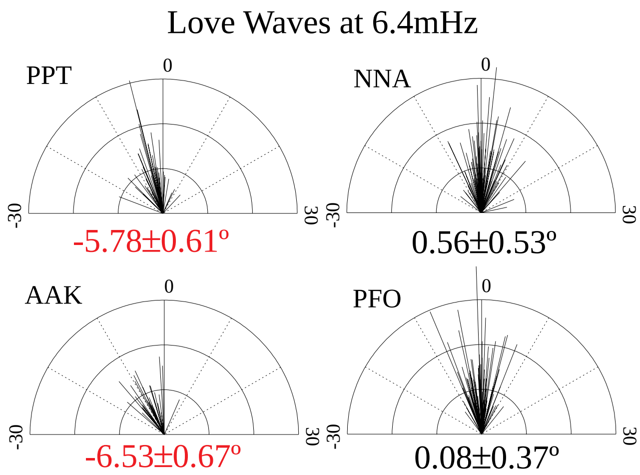


Figure 17 (*pol.gsn.eps*). Rose diagrams and ”Apparent North” for 4 GSN stations.

shift is seen at different frequencies and for both wave types that sample 3D structure very differently. There are two important issues I need to point out: 1) the signal in the arrival angle dataset is relatively small. Predictions using published phase velocity maps can be as high as 15° but are usually smaller than 5°. 2) Arrival angle measurements depend strongly on instrument calibration and component misorientation. As far as instrumental effects are concerned, the most straight forward effect to account for is a component misorientation. We assume that the calibration of both horizontal components is given accurately in the DATALESS SEED volumes at the DMC (the files we extract the instrument responses from). We further assume that both horizontal components are orthogonal. A misorientation at each station can then be included as additional unknown in a non-linear joint inversion for structure, where 2-3 iterations typically lead to convergence. To obtain the component misalignment, I perform such inversions for typically 3 frequencies for both wave types and then average the results. I elaborate on this because I think that my technique (perhaps in contrast to others) is probably the most reliable to distinguish between structural and instrumental effects. The numbers for my 4 example stations are given beneath the rose diagrams as ”Apparent North”. A negative value implies that the system is rotated clockwise with respect to true (geographic) North. Figure 17 implies that the components at stations PPT and AAK are significantly misoriented. This figure is somewhat historical as I, being a fresh postdoc at Scripps, showed it at the EGS 1994 meeting in Grenoble, France. This talk probably got the most feedback of all my talks ever, and I was instantly (in)famous, because I asserted that an observatory–quality station (PPT) was in error. Only now do I understand how bold I was back then. Amazingly enough, only a few months later, the NOs at GEOSCOPE confirmed that PPT was off by 5°(pretty close to what I had predicted). An error was also confirmed at AAK though the misrotation there was more complicated. The NOs checked the station and found that only the North component was misaligned (by 6°) but not the East component. The fact that I predicted 6° is probably a coincidence (as I had assumed orthogonal components) but the point is that I was still able to detect orientation problems at this site.[14]

[14] A question from Mark Zumberge prompts the discussion of whether my determinations of ”Apparent North” are affected by calibration uncertainties, or in other words, how well does the calibration have to be known? This question is actually non-trivial to address. Calibration problems of individual horizontal components are probably not easy to detect by my analysis. Back-of-the-envelope calculations show that if one of the two components is off by a factor of two, individual arrival angles can be affected by as much as 20°. This, of course, is much bigger than the signal due to Earth structure. But the average around which the values cluster (and hence suggest a misrotation of the sensor package) may actually stay the same, depending on the azimuthal data coverage. So the

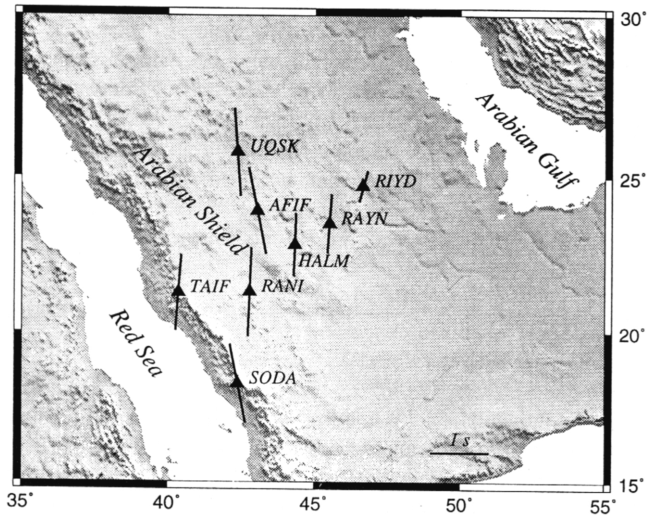


Figure 18 (*sw-splitting.eps*) A shear wave splitting study for the SAUDI data.

So how does a component misorientation affect science? Well, it does affect arrival angles but I think that I can account for this in my inversions. Other studies that are more sensitive to bias caused by such effects, because there are typically only few measurements, are shear–wave splitting studies. These studies typically do not account for component misalignment before interpreting shear–wave splitting in terms of seismic anisotropy (and ultimately mantle flow within the Earth). Figure 18 shows an example for Saudi Arabia. This is a shear–wave splitting study done for the Saudi Seismic Broadband Network that was operational from late 95 through early 97. The direction of fast shear–wave velocity is usually coherent across the array and approximately follows absolute plate motion. Two outliers are AFIF and SODA. I am not recalling the details of this publication but the temptation is great to iterate on why these two sites have different directions.

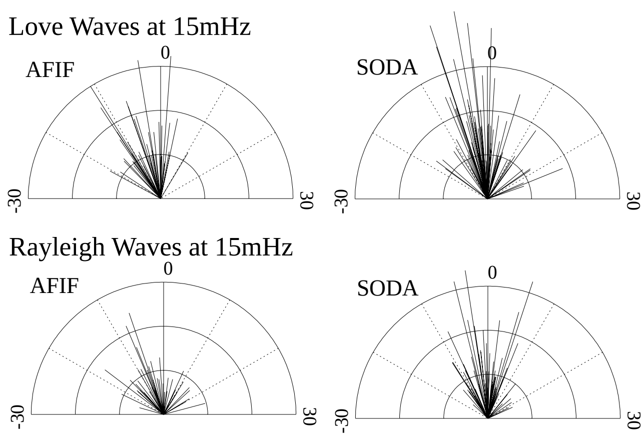


Figure 19 (*pol.saudi.eps*). Arrival angle data at two sites of the temporary Saudi Arabian Seismic Broadband Network. Of the 8 sites, at least two have small but significant component misalignment: apparent North at AFIF is $-4.22 \pm 0.66°$ and at SODA is $-2.72 \pm 0.54°$.

This study was done about two years before I had the time to check the orientation of the sensors. Figure 19

trade–off may actually be quite small. If one of the two components is off by 10%, then the effects on individual arrival angles can still be as high as 3° (bad news for me) but probably won't affect the mean (so my misrotation values are probably reliable estimates).

shows my data for both stations for Rayleigh and Love waves at 15 mHz. For the Saudi Array, I did my analysis at slightly higher frequencies because that array has STS-2 sensors and may be noisy at longer periods. The data seem to cluster at a small negative angle and my inversions confirm this. The misrotation is quite small and can by no means account for all the anomalous signal in the shear–wave splitting study but the coincidence is curious and speaks for itself. My friend Rob Mellors who installed the instruments confirmed that the magnetic declination was about 2° and so a correction in the wrong direction might be embarrassing but conceivable[15].

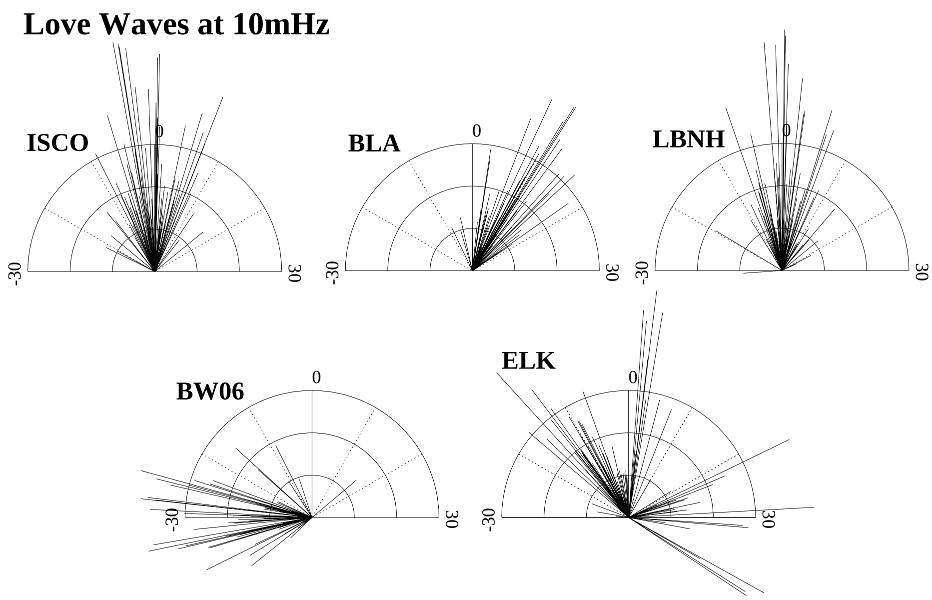


Figure 20a (*pol.usnsn.eps*) Arrival angle data at 5 USNSN stations.

My last example serves as a case study for the proposed USArray which will have a permanent component as well as a large flexible array component (”Bigfoot”) both datasets of which I would be very interested in. A few years ago, the IRIS DMC made the data of the USNSN easily available and these data are now part of our database. At the time when I analyzed these data, about 18 months worth of data were available, which is the expected life time of a USArray ”Bigfoot” site.

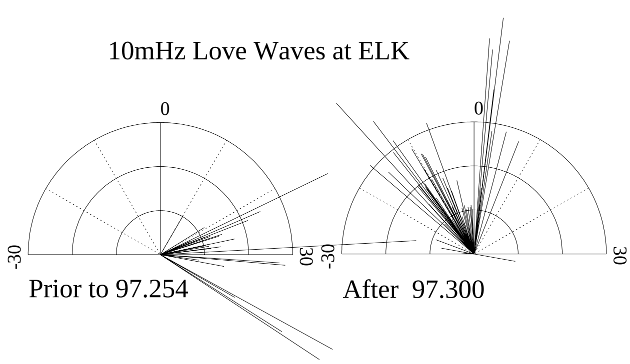


Figure 20b (*pol.elk.eps*) Time dependence of arrival angle data at station ELK.

15 I have yet to check if this goes in the right direction!

The USNSN has 28 sites for which I measured arrival angles (Figure 20 shows some examples) and my results are quite depressing. While some sites display the typical rose diagram (e.g. ISCO, LBNH), other sites clearly have orientation problems (BLA, BW06, ELK). The arrival angles at station BW06 are so extreme that I had to change my computer code to be able to analyze this station. A particularly interesting, and difficult, site is ELK for which the data seem to cluster around at least two different means. It turns out that the data prior to julian day 254 in 1997 cluster around a large positive value, while they cluster around a slightly negative value after day 300. So, I have it black and white that somebody must have visited ELK in the fall of 1997 and done something to the installation. Actions like these are particularly devastating for an ”end user” since none of this is reported by NO.

Apparent North

BLA	9.45 ± 0.82	
BW06	-26.41 ± 0.62	
CEH	9.72 ± 1.34	
ELK	31.80 ± 0.71	before 97.254
	-3.93 ± 0.56	after 97.300
GOGA	7.67 ± 0.97	
MCWV	5.90 ± 1.20	
MIAR	5.74 ± 1.19	
WMOK	-17.69 ± 1.03	
WVOR	-18.38 ± 0.90	

9 of 28 more than 5 degrees
4 of 28 more than 10 degrees

Figure 21 (*pol.table.eps*) Gross deviations of ”Apparent North” from true North at USNSN.

A summary of the orientation status of the USNSN is given in Figure 21. Nine out of 28 stations have an orientation error of more than 5°. This is a third of the network! And four out of 28 station have an orientation error of more that 10°. This is **unacceptable** for the proposed USArray[16].

Dataloggers

or ”The Subleties in a Seismogram”

I had prepared this section for the talk but then cut it out because I ran out of time. This is about a subtle signal we had detected in the data some time ago but didn’t know what its cause might be. It later turned out to be related to round-off problems in the filtering process of the II network’s Mark7 datalogger. The problem has recently been detected conclusively when the Mark8 datalogger had a test run at PFO and the Harvard group noticed that the noise level of the Mark7 datastream was higher than that of the Mark8 datastream.

Figure 22a shows the great Balleny Islands earthquake in 1998 recorded that the Black Forest Observatory, BFO. This is a beautiful record for free oscillation studies for which the very small event about 100h after the Balleny Island event is neglected (or edited out) (this is a Ms=5.6 earthquake on Mar29 north of Ascension Islands, 53° away from BFO). Ruedi Widmer-Schnidrig noticed for this event, as for others, that wave forms of such small events sometimes look ”peculiar” on the VH data stream, with a long–period signal at the back–end of the

[16] Duncan Agnew remarks that the numbers that occur suspiciously often are close to 15 and 30°. 15° is close to the declination within the U.S., so one has to wonder ....

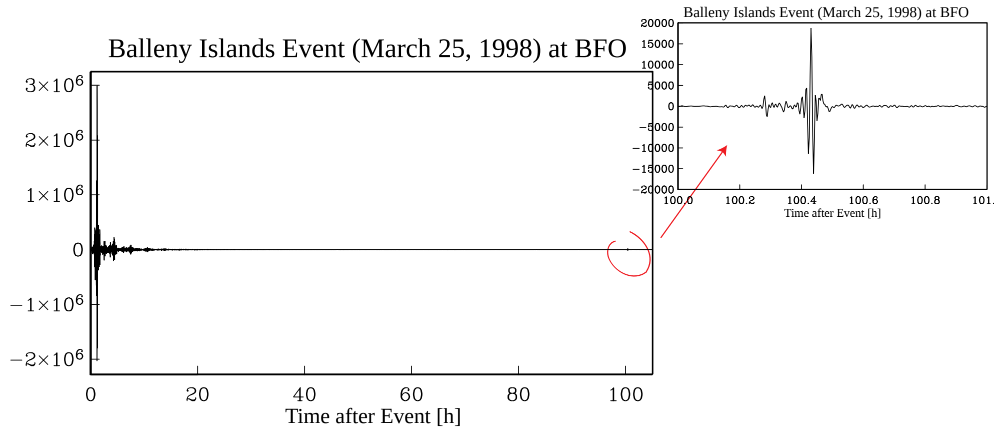


Figure 22a (*mark7.eps*) The Balleny Islands Earthquake recorded at BFO with a high SNR.

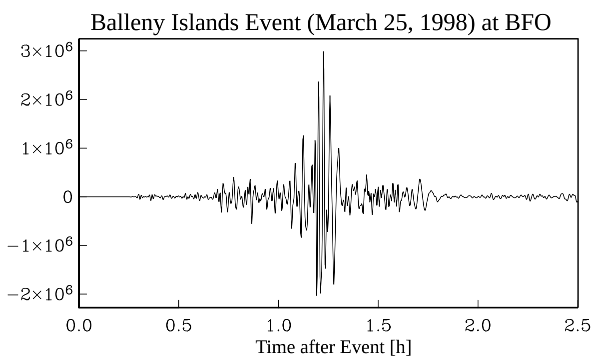


Figure 22b (*seis.ball2-bfo.eps*). The first Rayleigh wave train of the Balleny Islands earthquake on the VH data stream of BFO. The signal is ”on scale” and shows no non-linearities.

Rayleigh wave train. This looks suspiciously like a non-linear instrument effect. Of course, this doesn't make sense, if we remind ourselves that the much larger Balleny event did not show such a signal (Figure 22b).

Pete and I discussed what the cause for such a long–period signal might be and to me this waveform looked actually quite normal. Depending on the size of the earthquake, on how the seismogram is filtered and on the Earth structure the wave is travelling through, the long-period Rayleigh wave may very well arrive at the back end of the wave train. I tested this with synthetic seismograms and a comparison was inconclusive (i.e. I was basically right).

A disturbing fact however was that the waveforms of the VH channel and the LH channel looked different (Figure 22c). It looked like the VH channel was missing some of the high–frequency content at the back-end of the wave trains. This was very disturbing and pointed toward a problem with the datalogger, after we had ruled out any problems with filtering on our computers[17]. The great difficulty of this problem to pin down was that such

[17] Though this should not be the case, it is remotely possible that my filtering of the LH channel did not remove enough of the short-period signal to compare it to the VH data. The purpose of my figure is to illustrate what we saw in the data, and not to quantify the effects.

Balleny Islands Event (March 25, 1998) at BFO

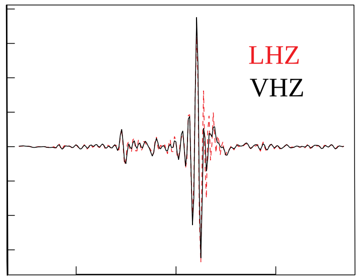


Figure 22c (*seis.ball4-bfo.eps*).

waveforms were sort of transient. They did not occur on ALL Mark7 recordings but they did seem to occur primarily on II records. Perhaps this only occurred at particular stations and consistantly so, which I do not recall. It turned out that indeed only small signals were affected and occurred primarily when the original data stream had a significant DC offset. The problem was a round–off error in the filtering from the original 100 Hz data down to BH, LH and VH channels. The VH channel was the worst affected since it underwent the most (imprecise) filtering. The NOs have succeeded in reconstructing the signals as they should have looked like and are currently replacing the faulty records at the DMC. It is not really clear just how much of my research is affected by this. I personally do not look at such small events on the VH channels and it is not clear to me how much a dispersion measurement is affected by this filtering error (though amplitude/attenuation research might be another story, as are noise studies).

Timing Problems

”a thing of the past?”

During this seminar series, we have been discussing the issue of timing several times and we repeatedly agreed that timing ”is not a problem anymore” in the era of GPS. We also agreed, however, that there are still rare, isolated cases, e.g. when the GPS receiver loses the signal and the internal clock starts to run freely. A question that needs to be discussed in our ”What’s next” session is what should we do in these cases. Should we let the clock run freely and record the time with a second clock so that data analysts have a chance to take care of it or should the NOs resynchronize the time (I vote for the latter)? The questions I will address today: Are there any timing problems nowadays? Can we detect them? How severe are these timing problems? And what effects do these have on our research?

How do we know that a station has timing problems? We all know that this is a tough question to address. If we pick a travel time and compare it to a prediction using a model, then measured time differences can have many causes. Foremost among these are 3D structure (what we want to know), source location errors, errors in the data analysis (e.g. picking inaccuracies), interference effects from other seismic (or whatever) signals, and lastly instrumental effects (the one we are talking about here). Instrumental timing errors of only a few seconds cannot be detected in a raw dataset. A reliable way to detect timing errors is to compare co–located instruments. There is no such case that two independent observatory quality instruments occupy the same site[18] but there are two

[18] though an increasing amount of GSN sites have secondary sensors, presumably with their own datalogger

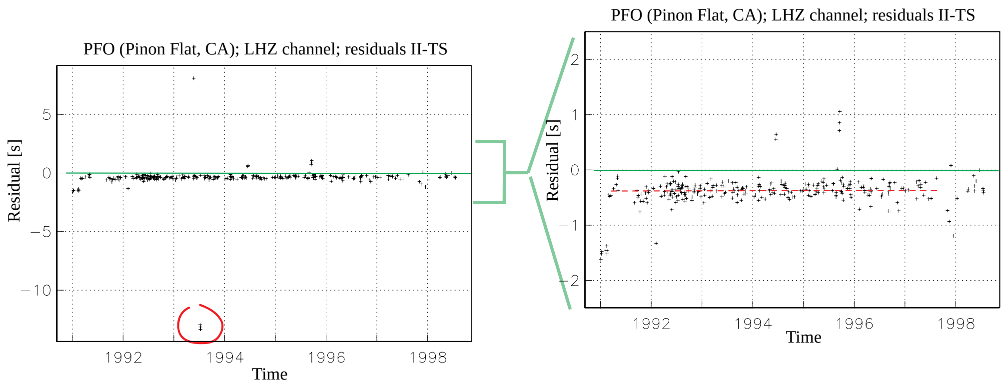


Figure 23 (*timing.pfo.eps*). The relative timing between the II and TS dataloggers at PFO.

GSN sites that have two dataloggers connected to the same sensor, PFO and KIP. After removing instrumental effects, the times picked from the two data streams at each site should be identical.

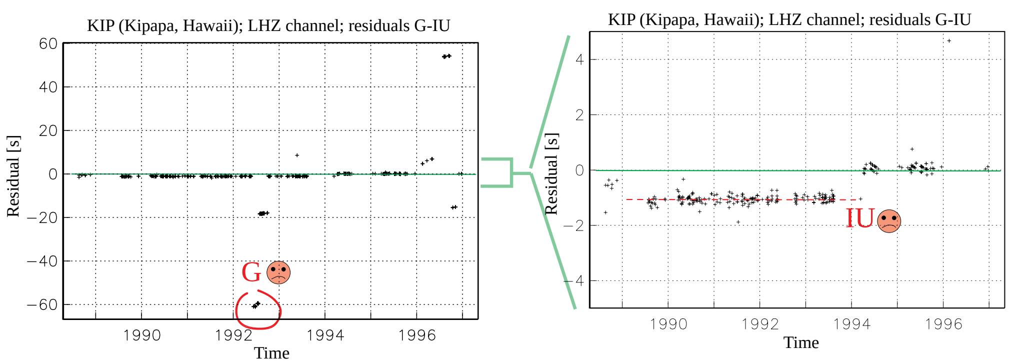


Figure 24 (*timing.kip.eps*). The relative timing between the G and IU dataloggers at KIP.

The figures I will show you come from Guy's body wave travel time work. Figure 23 shows the differences of P travel times picked at the two LHZ data streams at station PFO (one is from the II data logger, the other one from TS). There was a temporary problem with the TS response in the DATALESS SEED volume at the DMC the data for which are omitted here. A significant difference of about 13 s occurred in the middle of 1993 for a short time (successive days are consistent). I can't tell you right now which of the two dataloggers is at fault here but it would be obvious if we checked Guy's list. Apart from this we notice an almost constant offset between the two data streams of about 0.4 s where the TS stream is delayed. The travel time picking accuracy is 0.1-0.2 s so this difference is just resolvable. We do not know the cause of this but we can say that the two dataloggers have a time difference of about 0.4 s.

At station KIP (Kipapa, Hawaii), a similar setup exists with a GEOSCOPE datalogger and a IU datalogger hooked up to a STS-1 (Figure 24). There the difference between the timing of the two dataloggers is much larger. For some time in 1992, the GEOSCOPE timing was advanced by 1 min, and later by about 20 sec[19]. Apart from other gross excursions (and I don't recall which data stream has the timing error), there is also a constant offset between G and IU for the years 1990 through 1994. We find that, in this case, the IU data stream is late by 1 s. The cause of 1 s timing errors are manifold. I know from experience in field experiments, that some systems synchronize to the next higher second, so can be off by a second. I don't know if observatory quality systems have the same problem but I guess it is conceivable. Another timing problem of the order of 1 s is the handling of leap seconds. I am told that they occur quite often and that different NOs handle these in different ways.

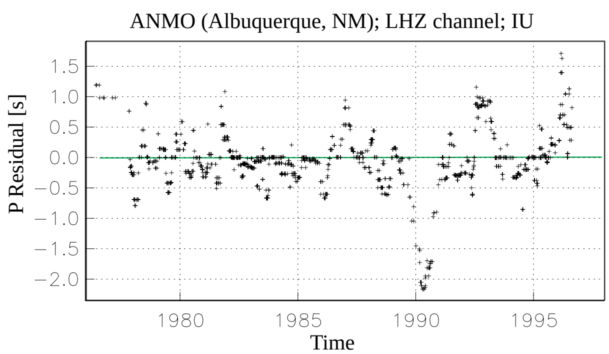


Figure 25 (*timing.anmo.eps*). Absolute timing errors at station ANMO.

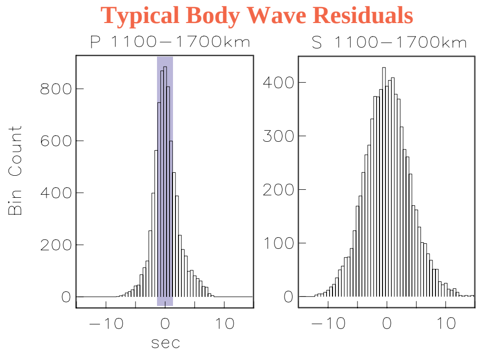
Unfortunately (or fortunately!), PFO and KIP are the only sites where two data loggers are hooked up to the same sensor so we cannot perform similar comparisons for other sites[20]. On the other hand, we are indeed able to get an idea of the timing from the absolute body wave picks. Figure 25 shows the absolute time residuals at station ANMO. For this figure, travel times have been calculated for a 3D earth and subtracted from the measured time[21]. The residuals shown in this figure are due to timing errors at the station, if the trends in Figure 25 are identical to the trends found for corresponding S picks (not shown). Anything not identical cannot be due to timing, unless there is a differential timing between the 3 components of the instrument (the P picks come from a vertical components, while the S picks come from a transverse component). Figure 25 suggests 1 s timing errors at ANMO for a short while in 1982, 1987 and 1993 and about 2 s timing errors in 1990 and 1997. As mentioned before timing problems in mutliples of 1 s are, unfortunately, probably very common.

How relevant is a timing error of 1 s? Figure 26 shows typical histograms for P and S travel time residuals relative to PREM. Any deviation from 0 s is regarded as due to 3D structure (in our inversions, we actually do allow for some of this signal to come from earthquake location errors). The signal in the P dataset is much smaller than in the S dataset. The variance in the P dataset is about 1 s (marked by the purple bar), i.e. much of the signal of the 3D Earth is only of the order of 1 s (or smaller)! The table on the right hand side summarizes this. The signal in S from 3D structure is larger than that from earthquake mislocation and noise (picking errors, timing errors etc)

[19] I can confirm this with my independent mode study. I analyzed a Fiji Islands earthquake occurring on July 11 (julian day 193). I had the GEOSCOPE and IU VHZ components and, in addition, the old IDA gravimeter (ID.VGZ). Since I am using data sampled at 40 s, my determinations are much less precise than Guy's but I obtained a difference between VGZ and IU.VHZ of -4 s and between G.VHZ and IU.VHZ of -61 s which confirms Guy's value.

[20] Though Frank Vernon points out that we could compare GSN data streams to his own, e.g. PFO and AAK.

[21] the actual procedure is a little more elaborate but the details are irrelevant for this talk



Contributions to the Standard Deviation of a Typical Summary Ray in the Body Wave Datasets (in seconds)

Data Set	Location σ_x	3D Structure σ_{3D}	Noise (LP) σ_N
P	0.9	1.0	0.9
S	2.0	3.0	1.3

* (mis) location effect important for P!

Figure 26 (*timing.body.eps*). Typical body wave travel time anomalies and their causes.

but all these signals are of the same size for P! Hence, 1 s timing errors are really not acceptable and we need to strive to do better than this.

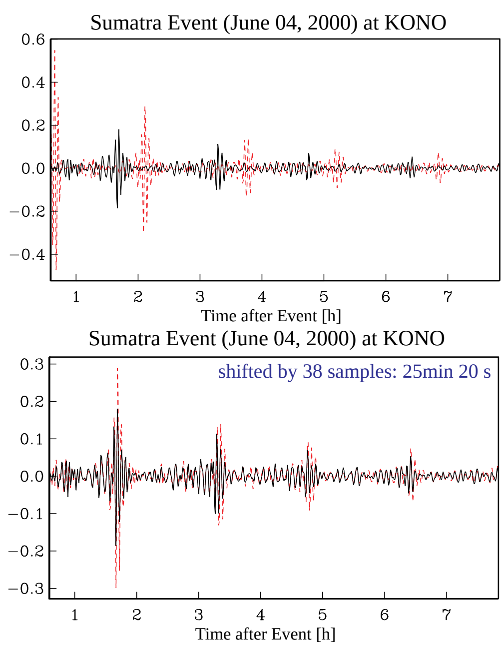


Figure 28 (*timing.kono.eps*). Data (black) and synthetic seismograms (red) for the great 2000 Sumatra Earthquake, before and after shifting the synthetic.

In one of his recent papers, Guy listed gross timing errors (5 s or more) in a table (Figure 27). Note that the timing errors at stations ATD, ENH, GNI, PAS and SBA occur in 1994 and are therefore to be regarded "recent". You may wonder if such gross timing errors were common 8 years ago but not today. Figure 28 shows a recent timing error

Station	Time interval
ANMO	1985/150–1985/157
ANTO	1992/90–1994/187
ATD	1994/243–1994/298
BNG	1992/311–1993/122
CCM*	1993/99–1993/110
CHTO	1978/184–1978/217
COL*	1993/20–1993/264
COR	1990/55–1990/74
CRZF	1989/232–1989/256
ENH	1994/205–1994/289
GDH	1992/195–1992/237
GNI	1994/46–1994/51; 1994/100–1994/366
GRFO	1989/162–1990/347
HIA	1987/118–1987/129; 1987/145–1987/158
HRV	1988/53–1988/70
HYB	1989/203–1989/302
INU	1990/203–1990/222; 1992/2–1992/73
KEV	1982/246–1982/249; 1986/120–1986/143
KIP[b]	1992/161–1992/272
KMI	1987/118–1987/129
MBO	1991/292–1993/79
MAJO	1985/282–1985/296
PAS*	1988/103–1988/143; 1988/282–1988/293; 1992/13–1992/20; 1994/281
RER	1991/361–1992/58
SBA	1994/266–1994/366
SCP	1984/264–1985/83; 1987/327–1988/69; 1988/258–1988/297; 1988/359–1989/26
SNZO	1983/277–1983/304; 1992/181–1992/242; 1992/326–1993/74
TAU	1988/183–1988/209; 1992/274–1992/344; 1993/265–1993/289
TUC*	1992/166–1992/252
ZOBO	1985/110–1985/135; 1993/252–1993/264

Table 1. Intervals of erratic timing deduced from the analysis of the temporal behavior of S and P residuals through the end of 1994. The dates are inclusive, e.g. for ZOBO all picks from 1985/110 up to and including 1985/135 have been deleted. Entries marked by an asterisk are assumed to be exact 1 minute timing errors (or multiples thereof) and have been corrected. The entry for KIP refers to the GEOSCOPE data stream. Note that start and end days correspond to days when we have measurements and so may not exactly reflect the duration of the timing problem.

Figure 27 (*table1.eps*).

that Guy would have never found because it wouldn't have fit on his screen. I calculated synthetic seismograms for the great Sumatra earthquake on June04, 2000. The synthetic record at station KONO is significantly delayed (the data record has the first Rayleigh wave train missing). The two time series need a relative shift of 25 min 20 s to be aligned so it is clear that KONO has a truely spectacular timing error of 25 min (give or take 20 s–as I said earlier, I cannot pin down timing errors that accurately)!!

One concluding remark of this section is in order. We need to talk about the "time dependence of timing errors". "We" (actually Guy) had realized quite some time ago, that G and IU had, at certain times, a timing difference of pretty much exactly 0.5 s (which was corrected before making Figure 24). Contacting both NOs, we received two ascertainments that the "other" datalogger was at fault. It later turned out that it was the GEOSCOPE logger and if you request data of this period nowadays, you may find that both streams are actually in phase. What happened in this case is that GEOSCOPE corrected their timing a posteriori. This post–processing phenomenon is not network dependent and could happen to any station. This does not appear to be such a big deal but the implication of this is absolutely **huge**! For example, let say I requested a data stream of the June 1994 Bolivia earthquake at station ANMO in July 1994 and found a timing error of 120 s (just an assumption!). I then warn my colleague Göran Ekström at Harvard who has requested the same data in 2000. He may ask me if my computer codes are working right, because he finds no timing error at ANMO for June 9, 1994. You know why: the timing error has been fixed at the DMC in the meantime. This brings us back to Guy's table. He invested tremendous effort and time to detect and report these timing errors but only 3 years down from now, his table may not match with the data then stored at the DMC. Depressing, isn't it? So what should we do abut this problem? There is the suggestion that we should not touch the data but provide accurate "station histories" at the DMCs (I think I would endorse this idea). This leaves the really blind data analysts who have never heard of station histories (unfortunately probably the majority of us) in the dark. Some say that the data should be corrected. This would cause the "expert" data analysts (such as Guy) who typically store large databases **major** headaches. I cannot count how often we refurbished our database with updated requests to the DMC and initially unknowingly mixed un-corrected and corrected data. The rectification of this mix-up costs us a huge amount of valuable time.

Our Friends at the Data Managment Centers

This section I had also prepared for the talk but omitted due to time constraints. I do find it important however to point out that a data enduser does not only have to worry about "instrumental effects" created on the way between the seismic site and NO. A data enduser also has to deal with "instrumental effects" that are created after the data left NO. The latter may only learn about this when the DMC sends (seemingly unjustified) complaint about a station or time period to NO.

Let me briefly review the usual path of ground motion from the field onto my computer screen, from the enduser's point of view:

- ground motion is recorded by instrument in some format
- record is shipped to NO
- NO checks record and decides whether it's worthy of distributing
- NO puts this record into a SEED file and sends it to the DMC
- Gabi sends breq_fast request to DMC
- DMC assembles "Gabi's" SEED file from data stored on jukebox
- Gabi ftps her SEED file from DMC disk onto her own
- Gabi uses RDSEED (provided by DMC) to read the SEED file and converts record into own format (from SEED to SAC to GFS format)
- Gabi edits raw time series on screen
- Gabi extracts instrument response from SEED or DATALESS SEED volume using EVALRESP (provided by DMC) and corrects time series
- Gabi plots time series on screen and analyzes it

And here trouble usually starts for the data analyst. I've been looking at global seismic data provided by the DMC for about 10 years now (I looked at BFO and GEOSCOPE data for much longer than that). During this time I've survived about a ga-zillion software upgrades at the DMC, each single one of which caused some disturbance in my data processing. Some of the changes are really minor and easy to fix but can cost the data analyst an

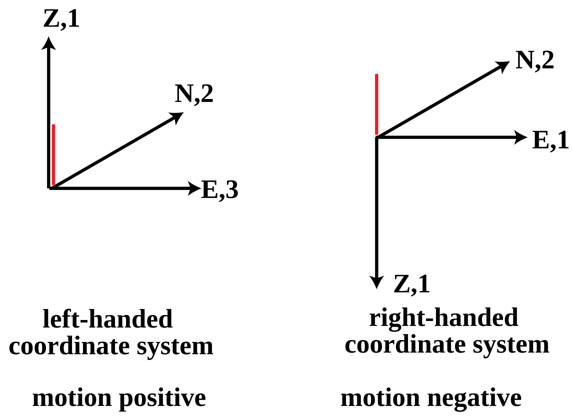


Figure 29 (*dip.definition.eps*). The definition of the dip in different coordinate systems.

disproportional amount of time.

For example, one of the early ”upgrades” of RDSEED was to provide the option of automatic polarity checks and of flipping records as they are read out. Some instruments have true polarity-flips (e.g. ERM.II) which need to be corrected. A major headache, however, was to combine data from different networks. A fundamental issue to agree upon is the coordinate system in which a seismic motion is recorded (Figure 29). We all know that the coordinate system is ZNE but there is, amazingly enough, a disagreement over what Z actually is, i.e. what is positive. The coordinate system in seismology has positive Z going up (in this case ZNE is a left–handed coordinate system). Unfortunately, it is conventional to use a right–handed coordinate system and in this case, Z is going down. Choose NOs out of each of these two groups and the confusion about the sign of the dip of Z is perfect. Of course, the actual motion recorded by the instrument is always the same but the definition of the dip now becomes NO-dependent. I don’t remember exactly who was using what but, just for the record, GEOSCOPE **used to** use a different convention (the same as old IDA/ID) than everybody else. Having the polarity check in RDSEED switched on, this routine would then flip all GEOSCOPE data during the reading process, which was exactly the wrong thing to do because now all vertical motion in the GEOSCOPE records had the wrong sign. So in the busy days when Guy, Junho, Harold and I myself had their fingers (and their own heads) involved in the reading process, our database was a huge mess. I think, in the meantime, ID ”died out” and GEOSCOPE was persuaded to change their convention so we all seem to be consistent now.[22]

Another ”problem” is that the folks at the DMC (have to) strive to make RDSEED become more universal. For example, recent versions are supposed to be used for GSN and PASSCAL data alike. A consequence of these upgrades is that the input parameter list constantly changes. I never use RDSEED in interactive screen mode but in c-shell script files (batch mode) and I don’t have to say more. The lifetime of my c-shell script files is far less than various versions of Microsoft Windows.

A very sad and actually quite grave issue is ”data aging”. This should not happen but great seismograms that were available at the DMC only a few years ago get lost. I started free oscillation studies relatively late, after my fellow postdoc Junho Um had left. This was several years after the 1994 Bolivia earthquake the data collection for which I was not involved in. When I joined in, the raw data were no longer on disk available and it was only approximately known what was done to the data that are stored in the database. Trying to be consistent with

[22] ... and the red blobs in our models really are red and not blue!

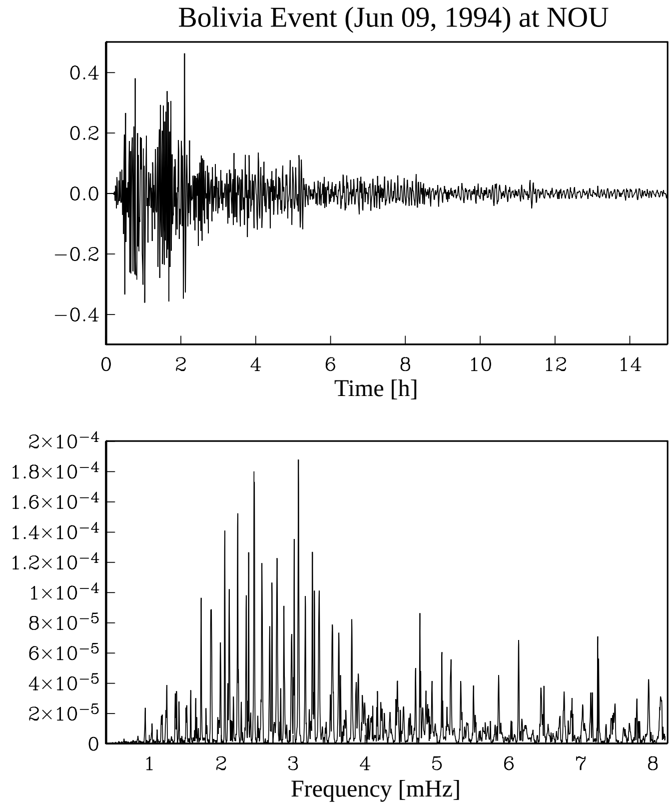


Figure 30 (*seis.boli-nou.eps*) A historical record: the seismogram at NOU is no longer available at the DMC.

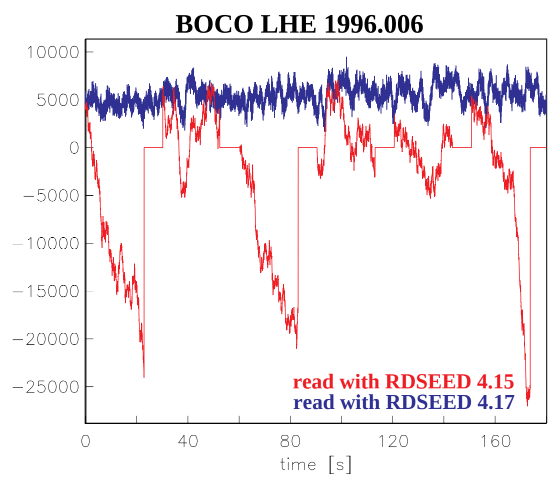


Figure 31 (*rdseed.boco.eps*). A SEED file read with different versions of RDSEED.

the data processing, I started from scratch and re-requesting the data from the DMCs. This exercise was quite sobering. Some records (I do not recall which ones these were) now had huge holes while the old ”processed” data on Guy’s disk were continuous. This implies that parts of seismograms were lost between 1994 and 1999, within only 5 years! Figure 30 shows a record (and its spectrum) that is historical, meaning that it is no longer available. My requests to the DMC does not return any data for station NOU. This spectrum is one of the 10 best we have for the Bolivia earthquake!

Another sad and grave story is that SEED files and various versions of RDSEED become incompatible and data practically unrecoverable. Figure 31 shows an example of this. At some point during the past few years, we decided to refurbish our database. We requested several years worth of data at the DMC read them with the SEED reader and put them on a big disk. The data could (and still can) then be used by whoever needs them. In summer of 2000, I decided to look at data recorded in North America and had a look at our LH database. I noticed that, in a certain time interval, seismograms at BOCO didn't look right (red trace). This also happened for other stations, at other times. Sometimes it happens that a SEED file gets corrupted due to a disk error on our disk or an undetected error during ftp from the DMC to us. So in order to track this problem down, I re-ordered the data from the DMC. The new SEED file however had the same problem, so we knew that the original SEED file did not ”age” on our disks. During the reading process, I got an error message:

> *Steim 2 Decompression Sample Count Error. Expected 1808, counted 1600. Lost values will be Padded with Zeroes.*

I then realized accidentally that the DMC was providing an updated version of RDSEED. Reading the SEED file with the new update (version 4.17 instead of 4.15) gave the blue trace in Figure 31. Obviously the ”new” SEED files were not down–ward compatible and this change was made without our knowledge. We are supposedly on a user-email list at the DMC but we never receive emails about software upgrades and the DMC web site also does not announce updates (perhaps somewhere 20,000 levels down but I could not find it). I don't have to tell you that it cost us major time to debug our database, a problem we had repeatedly pointed out to the DMC.

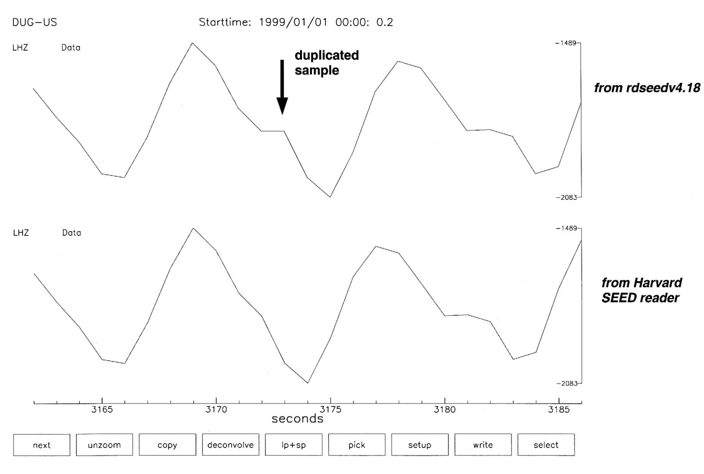


Figure 32 (*usnsn.timing.eps*) A timing error introduced by RDSEED.

The non–communication between the DMC and endusers is a really big problem. I seem to be one of the few who actually does provide feedback to the DMC and with an odd smirk in their face they now refer to me as ”not her again”. My colleague Göran Ekström also is one of the bugging kind. About two days before the last Fall AGU meeting, he asked me whether I found any timing problems with the USNSN data....two days before AGU!

Since I had only looked at long period surface wave data, I was not in the position to say anything conclusive. He sent me a figure (Figure 32) that contained enough material to panic. Using his own SEED reader, Göran got a different time series for USNSN data than when using the latest version of RDSEED (4.18) (distributed by the DMC). The DMC reader seemed to have put in extra samples, delaying the following data stream by 1 s. The source of the error was identified during a lengthy search and discussion process. Data are stored in data blocks, and a request to the DMC usually spans several of these data blocks. NO at USNSN write the data blocks in a slightly non–standard way. Successive data blocks begin with the sample that the last data block ends with and RDSEED interprets this as being two separate data points, duplicating the one datum when data blocks are strung together. We do not know how often this occurs in our database but this incidence prompts yet another major refurbishing process. If these duplications occur more often than once per event I can redo about 10,000 hand–picked dispersion measurements.

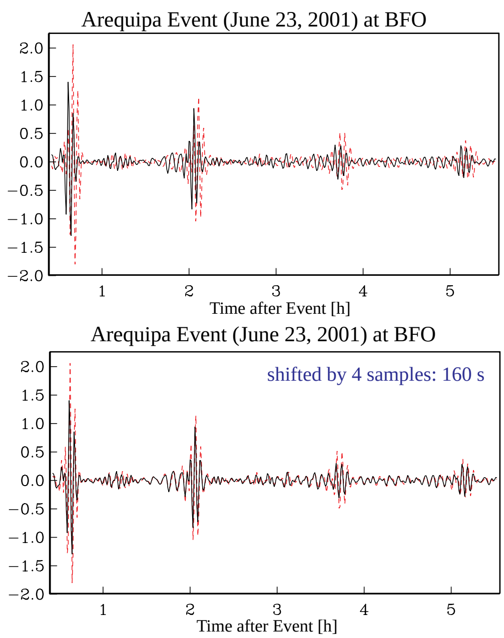


Figure 33 (*evalresp.eps*). A timing error introduced by EVALRESP.

RDSEED is not the only software that potentially alters data. I spent a good part of my last summer to help the DMC debugging EVALRESP. The cause of all trouble was the great Arequipa earthquake in June. For ”interesting” earthquakes I routinely gather all VH data I can get a hold of to determine a source mechanism. There are some interesting cases when the USGS publishes a mechanism that is very different from the Harvard quick CMT. More than once, I also got a different MT that was very close to a later revision of the Harvard CMT. We usually update instrument responses from the DATALESS SEED volumes only once a year so for these ”special events” I read the responses from the new SEED files. The Arequipa event quickly turned into a major headache when many of my synthetic seismograms were shifted relative to the data by large amounts. An example is shown in Figure 33 where the synthetic has to be shifted by almost 3 minutes (!) to match the data. We initially assumed that complicated source processes (e.g. a long source duration) could be the cause of this. The fact that not all records were shifted led to wild speculations about never before seen directivity effects, until I noticed that this time shift was network dependent. IU stations had no shift, GEOSCOPE stations were 120 s off, II stations 160 s and BK stations even more. The mistake I made here was that I made certain that I used the latest versions of RDSEED and EVALRESP. Had I used an old version of EVALRESP, I would have had no problems. The communication with the DMC was lengthy and frustrating but we finally identified several bugs in EVALRESP (v.3.2.18). Just to give you a flavor of the types of things that happened, a beta version during the debugging process would run fine on a PC at the DMC and under CDE on my Sun Ultra 5 but not under OpenWindows. Such incidences usually prompt my correspondents to conclude that the fault is to be sought on my end and not theirs, which of course is not the case. The debugging process at the DMC ended with the release of version 3.2.19 and the conclusion that this can only be a preliminary fix. The ultimate culprits, according to our friends at the DMC, were the NOs. The problem is that different NOs use a header parameter called ”group delay” in different ways, or not at all, and older versions simply ignored this (which was correct in most cases for GSN). Version 3.2.18 reads this group delay[23] and puts it into the responses. I cannot stress often enough that the DMCs (especially the IRIS DMC) needs to work on improving the communication with all NOs (not just IU) and endusers.

Surface Waves and the Ocean Bottom

”... to boldly go ...”

With this section, I want to give you an outlook of where I would like to go with my research on regional scale. One of the reasons why we have this seminar is to discuss global seismometry which faces an uncertain future, because the STS-1 are no longer built. But regional seismology also harbors issues that are yet to be resolved.

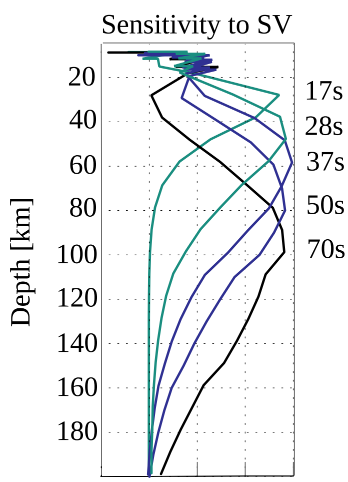


Figure 34 (*swell.kernels.eps*). Sensitivity of Rayleigh wavaes to structure at depth.

One of many things I would like to know before I retire is how the Hawaiian hotspot works. To find this out, we need a nice tomographic image of the lithosphere–asthenosphere system of the Hawaiian Swell which can only be achieved by using OBSs. The standard body wave tomography provides a fairly good picture of the deeper upper mantle (i.e. between 200 and 600 km, depending on the aperture of the recording array). For example, the ICEMELT experiment illuminated the deeper Iceland Plume. To image the upper 200 km however, we need

[23] that it shouldn’t read, because all NO except IU do not seem to be knowing about this ... Some NO also don’t know about the software upgrades and use the same version of EVALRESP as we do or even older...COMMUNICATION!

either regional seismicity or surface waves. Figure 34 shows how Rayleigh waves sample shear velocity with depth, as function of period. Only 70 s Rayleigh waves have significant sensitivity to structure below 140 km and are necessary to reliably resolve structure below 80 km. It is therefore essential to measure dispersion at these periods and longer. Here exactly lies the problem for an "OBS" experiment.

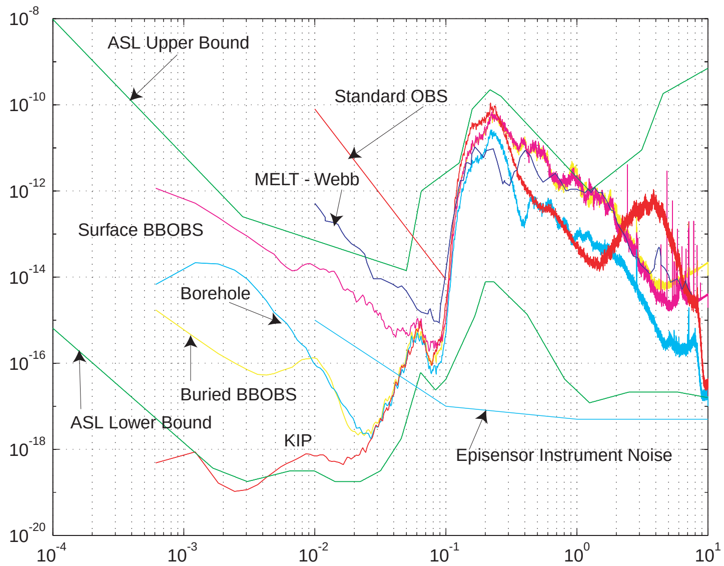


Figure 35 (*swell.all-resp.eps*) Ambient noise curves at OSN-1 (by Vernon and Orcutt).

I routinely measure dispersion at station KIP (Kipapa on Oahu, Hawaii). For reasons that are not entirely understood this site has an exceptionally low noise curve at long periods (Figure 35) but this is what I'd like to see for all stations. In the oceans, the low–noise band is limited due to inteference effects from long–period infragravity waves beyond about 80 s. The data collected in the OSN1 pilot experiment from February through June 1998 confirm just this. In the frequency–range I am interested in (this side of the micro–seismic band), the noise curve of any seismometer in the ocean (e.g. the OSN1 KS5400 marked "borehole") is higher than that of KIP. The cheaper buried BBOBS (broad-band ocean bottom seismometer, that used a Guralp CMG-3T) has a noise curve similar to that of the KS54000 but buried BBOBSs, deployed in a 60–instrument experiment are unaffordable (even surface BBOBSs are too expensive). Standard OBSs are not suitable to record my surface waves because the instrument noise is too high. Even the Webb–instruments of the MELT experiment had a noise curve that is higher than the ASL upper bound beyond about 40 s. It is therefore not surprising that in the MELT experiment Rayleigh wave dispersion could be measured up to 50 s but not beyond. For my tomographic image of the Hawaiian Swell we clearly want to do better than this.

An option is to take advantange of the ground–water coupling at the ocean floor and measure pressure changes in the water caused by the passing Rayleigh waves, rather than ground motion. Measuring pressure changes rather than ground acceleration has the advantage that the noise in the pressure signal is lower than that in acceleration, at periods shorter than about 27 s (Figure 36). The difference in noise is significant for periods shorter than 20 s. At longer periods however, the noise is higher in pressure prompting doubts about the feasibility to measure surface wave dispersion with high precision.

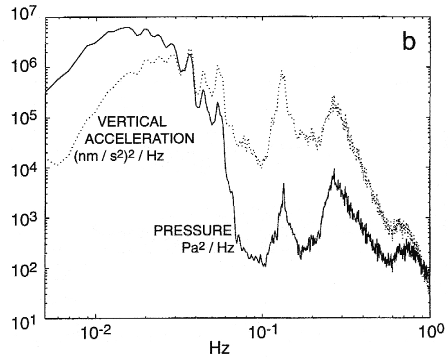


Figure 36 (*swell.acc-press.eps*) Acceleration and pressure noise in 1 km depth, off CA coast (by Webb, 1998).

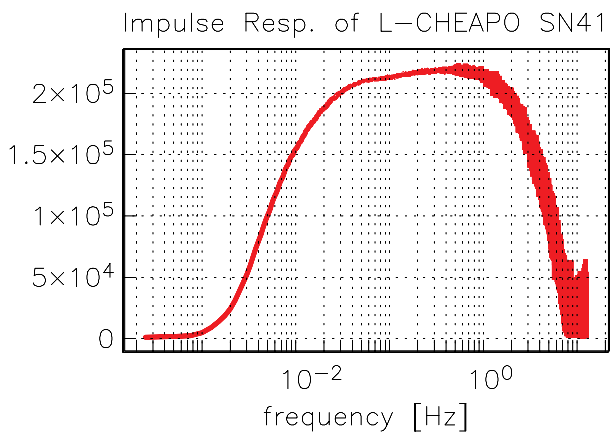


Figure 37 (*swell.dp-resp.eps*) Impulse response of the SWELL L–CHEAPO instruments (using a DPG sensor).

In our SWELL pilot exeriment near Hawaii in 1997/1998 we therefore wanted to investigate just this. The impulse responses of our L–CHEAPOs had not been measured before but had instead been taken from spec sheets as for most OBS studies the instrument response is a minor issue. I initially didn't make friends in our OBS team with my persistent inquiries about the responses but they made our engineer Paul Zimmer sufficiently nervous. He sat down with our student Harm van Avendonk and performed a calibration test. This exercise probably saved my experiment because it turned out that the responses were nowhere near what we expected them to be. Paul changed a few resistors (this is magic to me) and managed to expand the response to the period range I was interested in (and we had initially thought we had) (Figure 37).

So thanks to Paul Zimmer, Cris Hollinshead, Dave Willoughby and numerous cruise helpers, we collected textbook waveforms (Figures 38 and 39) and I could measure dispersion in an unprecedented period range (sometimes between 17 and 90 s!). A closer look at the record section however reveals one trace as being rather noisy (trace 7). In fact, it is so noisy that the S arrival cannot be made out. This brings us to the implied title of my talk, "instrumental effects which inhibit my analyses". Trace 7 suffers from harmonic noise that we observed quite often at the SWELL instruments but that was previously unknown to us because these instruments never before recorded long–period seismic signals. The noise was confined to a very narrow frequency band (Figure 40) and we could usually also see the first 2 harmonics.

Figure 38 (*swell.lcheapo.eps*) An L–CHEAPO during deployment in the SWELL experiment.

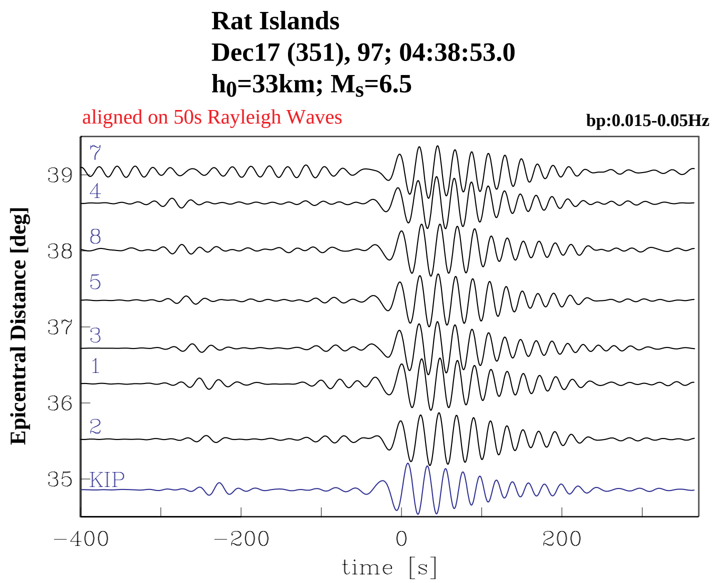
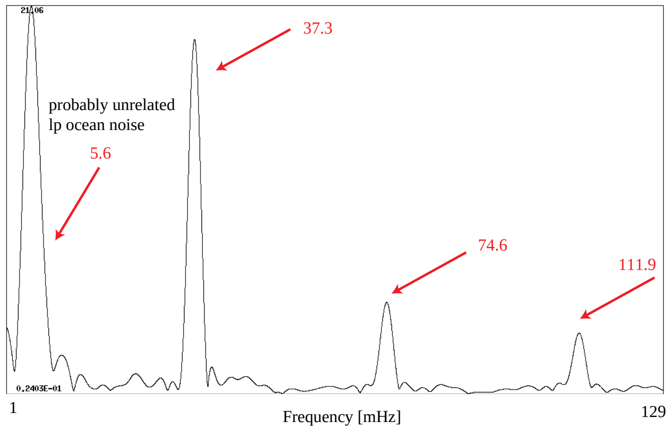


Figure 39 (*swell.seismogram.eps*) An earthquake recorded at the SWELL pilot array. For comparison, the trace at nearby GSN station KIP is also shown.

The properties of this noise were quite interesting. It was different at each site and extremely intermittent and appeared to be very rare at the deep sites. Figure 41 shows the time–dependence at site 2 for the first few days of operation. The instrument was in the water for about two hours or so and recorded a small earthquake (the blue horizontal streak at time 106, 20:00 – the other blue streaks every 6h are put in by me to mark the time and are not in the time series!).After the earthquake, this station was very quiet for about a day and 6 h, then the harmonic noise started and lasted for about 6 h before it stopped and picked up after another 14 h (day 108),



Station 3 (KAKU): "noise" before Colombian event Sept. 2., 1997

Figure 40 (*spec3.Colo.eps*) The harmonic noise recorded at SWELL site 3.

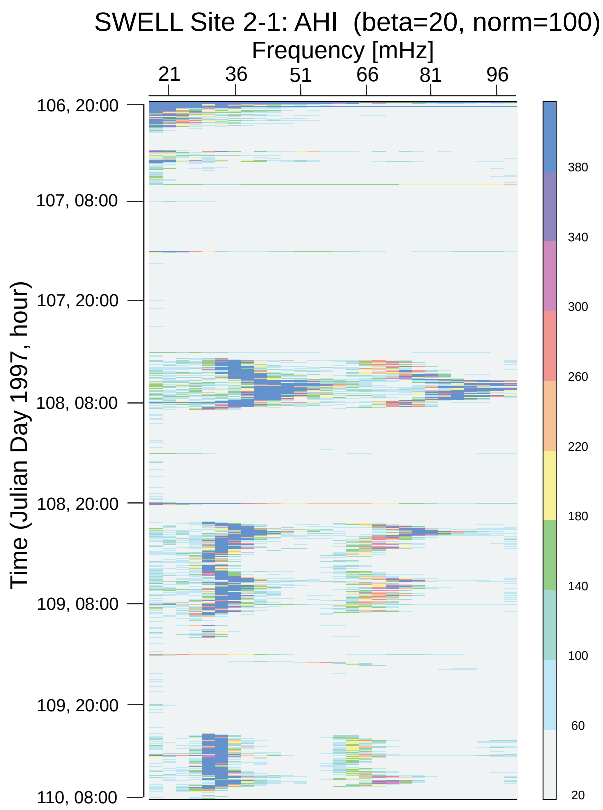


Figure 41 (*swell.ahi-gabo.eps*) The time–dependence of the harmonic noise at SWELL site 2.

at lower frequencies. In each episode, the noise increased in frequency (and so did the higher harmonics) and amplitude and then decreased in both.

To illustrate the variability of the signal, Figure 42 shows the noise at SWELL site 6. The first harmonic is much less obvious than at site 2 (and the fundamental is at slightly higher frequencies). The horizontal streak before time 110, 00:00 is almost certainly also noise because site 2 was quiet at this time (so this is not an earthquake).

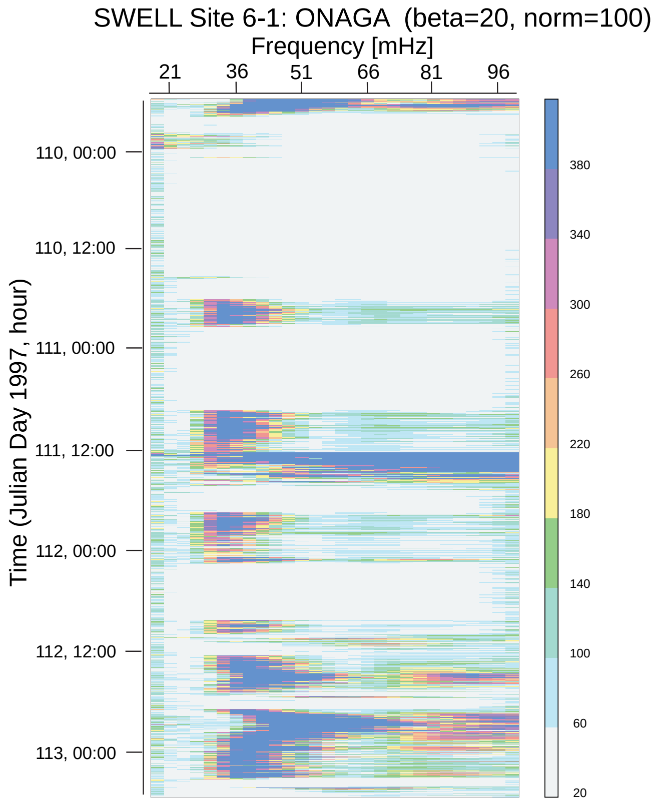


Figure 42 (*swell.onag-gabo.eps*) The time–dependence of the harmonic noise at SWELL site 6.

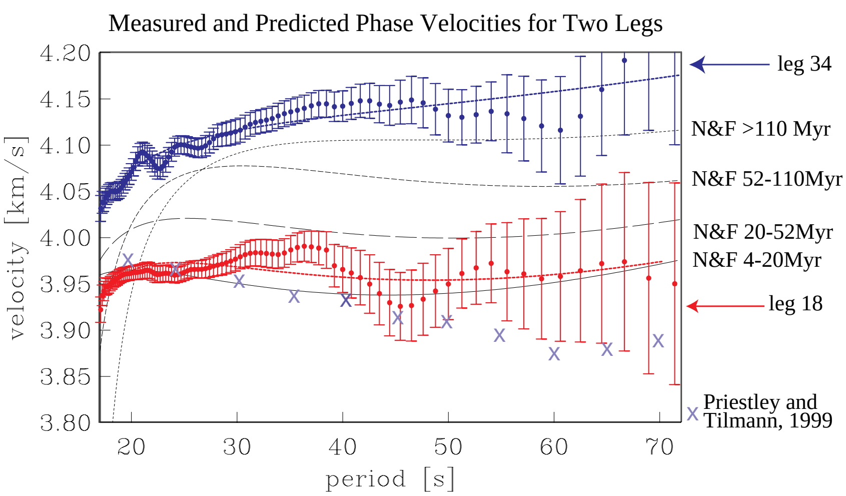


Figure 43 (*swell.disps.eps*) Two interstation dispersion curves measured at the SWELL pilot array. Leg 18 was close to Big Island on the Swell, while leg 34 was in the deep ocean off the Swell.

The horizontal streak at time 111, 12:00 is a large earthquake in the Santa Cruz Islands region to the southwest.

That this noise could potentially affect my dispersion measurements is evident in Figure 43 which shows inter–station dispersion curves. The little bump in the leg–18 dispersion curve near 45 s is unphysical and could have been caused by the contamination from harmonic noise (though other causes are possible). I have not gone back to verify this for every dispersion curve and I also don't really know what would be the best way to correct for this. Since this is a pilot experiment, I decided to let the results be as "raw" as possible.

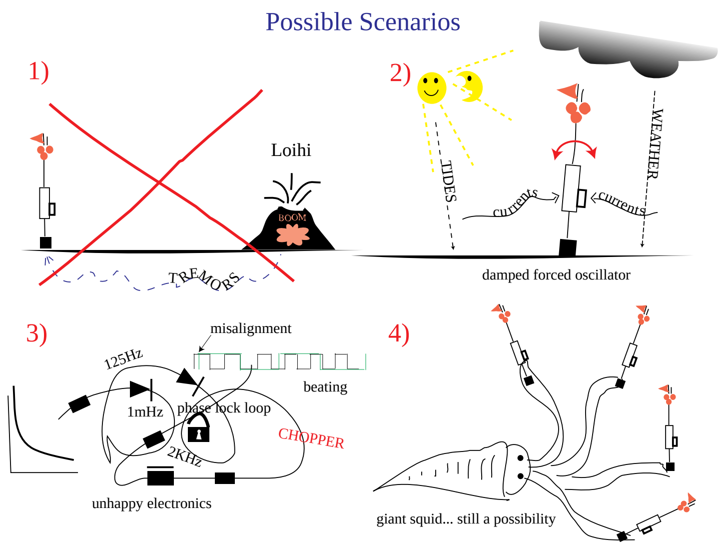


Figure 44 (*swell.scenarios.eps*) Possible Scenarios for the harmonic noise at the SWELL pilot array.

The search for the possible origin of the harmonic noise was quite an entertaining exercise. Of course, the first guess was seismic signals. Unfortunately, the widely favored cause by tremors from the Hawaiian volcanoes had to be ruled out quickly because the signal was so different between stations. It was curious however, that the deepest site off the Swell seemed to have been the quietest. Since the instrument package was more than 3 m long, it was conceivable that deep–ocean currents could force oscillations on the package. But only very few people warmed up to this theory. Spahr Webb suggested internal problems in the instrument package, i.e. that the clock driving the sensor could beat with the clock driving the electronics if they were not synchronized. This was identified by John Orcutt as a grave mistake ("never put two clocks into one instrument") and the new generation LC2000 now has only one clock. Well, if we go out again with the new instruments, and we still record the noise, then we have to search for other reasons.

Food for Thoughts

I would like to conclude this talk with one last issue that we may want to discuss in our final "what's next" session in March. The SWELL pilot experiment was regarded in the community as only partially successful, which I take personal but cannot do much about. The reason for this is, oddly enough, that we recorded the waveforms on pressure sensors and not on existing seismometers. I already discussed the reasons why we did not go out with the currently available OBSs. Had we done so, I would not have the high–quality dispersion curves I have today. Going out with pressure sensors however, so my proposal reviewers say, has the disadvantage that we record only Rayleigh waves and these with only one component. With pressure sensors, we cannot record the particle motion of surface waves, which also carries useful constraints on the structure along the source–receiver ray path. Neither can we record Love waves which provide complementary resolution of structure at depth. To my knowledge, only two Love waves have ever been recorded with current OBSs. One was a large regional event during the MELT experiment (less than 20° away). The other one was the 1999 Hector Mine earthquake recorded on Leroy Dorman's OBSs off Costa Rica, also only about 20° away. I would say that these are the most expensive Love wave records we have and we have to remind ourselves that surface wave dispersion of relatively large earthquakes recorded at a distance less than 20° are very difficult to analyse. The trade-offs of this (increasingly political) issue are obvious. We proved with our pilot study that we are able to obtain Rayleigh wave dispersion curves with our L-CHEAPOs in an unprecedented period range. The full period range is essential to resolve structure reliably at depths below 80 km. Proposal reviewers are reluctant to recommend funding of DPG seismic experiments because of the highly specialized approach of using Rayleigh waves in passive seismic experiments on the ocean floor (no matter how successful SWELL pilot was). Using only DPGs, we cannot measure shear–wave splitting and receiver functions, the standard observables which are much more widely (over)interpreted than surface waves. Neither can we measure particle motion or Love waves. Current OBSs are probably good enough to measure shear–wave splitting but the sensors that record both Rayleigh and Love waves in a useful period range probably have yet to be built. As a surface wave seismologist interested to obtain a tomographic image of the Hawaiian Swell, I would rather have an excellent collection of wide–band Rayleigh waves than a mediocre (short–period) one and one additional Love wave, but my proposals would not get funded if I expressed this. In return, I cannot really regard a proposal that promises to resolve structure at 150 km with 50 s Rayleigh waves as fundable, so we are sort of stuck in the review process. The OBS community needs a seismic sensor that is capable of giving us both excellent Rayleigh and Love waves though I am personally a little sceptical about recording Love waves in the oceans with a high SNR. In the meantime, while we are waiting for the ultimate sensor to appear on the horizon, I will not go out to sea without a DPG and the equipment being fine–tuned to record long periods.

Acknowledgments. This talk was a contribution to our "Seismometry" seminar which was held during Erhard Wielandt's sabbatical visit to IGPP.