Reply to comment by J. R. Murphy on 'Q for short-period P waves: is it frequency dependent?' by A. Douglas

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Using network averaged spectra Murphy (1989) demonstrates that if it is assumed that the source functions of explosions at Pahute Mesa, Nevada Test Site (NTS) are as predicted by the Mueller–Murphy (M–M) source model then the average \( t^* \) at around 1 Hz for P waves radiated from the test site must be about 0.75 s. With this value of \( t^* \) Murphy (1989) estimates the best fitting M–M spectrum for each explosion studied, by adjusting \( A \) and \( \tau_w \); \( A \) and \( \tau_w \) being the amplitude and delay time of \( pP \) relative to \( P \) and \( c \) the wave speed for the material in which the explosion was fired. The absolute amplitudes of the theoretical spectra are obtained using a calibration factor estimated from the data. Murphy (1993) extends the analysis to explosions in granite at the Nevada, French Sahara and E. Kazakh test sites. For the French Sahara explosion \( t^* \) is assumed to be 0.75 s (Murphy's estimate for NTS explosions), and for the E. Kazakh explosion a \( t^* \) of 0.55 s is used. For the French Sahara and E. Kazakh explosions Murphy (1993) shows that by using the same calibration factor as for the NTS it is possible by varying \( A \) and \( \tau_w \) to fit the estimated average network spectra using the M–M granite source. Murphy (1993) states that the amplitudes and spectra for the largest NTS explosion in granite (PILE DRIVER) can also be predicted using the M–M model but these results are not shown.

The analyses carried out by Murphy (1989, 1993) have the attraction that they appear to be internally consistent. The fit between the observed and computed spectra are good within the passband (0.5–2.5 Hz) used and the estimated spectra are peaked, which is consistent with the presence of overshoot in the time domain as predicted by the M–M model. However, the results presented, to a large extent simply confirm what is already well established, that to obtain a fit between computed and observed P amplitudes (either in the time or frequency domain) using the M–M source it is necessary to assume that \( t^* \) at around 1 Hz for the Nevada and French Sahara sites is just under 1 s and for the E. Kazakh site is about 0.6 s. If these values are correct then the observations of energy in P signals at frequencies well above 1 Hz implies that \( t^* \) is frequency dependent and decreases as frequency increases. What Douglas (1992) does is to present evidence that these attenuation models are incompatible with deconvolved seismograms. Douglas (1992) argues that peaks in the spectrum such as those observed by Murphy (1989, 1993) are just what would be expected if too large a value of \( t^* \) was used in correcting for attenuation.

Murphy (1993) claims that the network averaged spectra are fully consistent with theoretical models. However, in estimating the observed spectra, \( A \) and \( \tau_w \) (and for the Pahute Mesa explosions \( c \)) are not set to theoretical values but are adjusted to obtain a best fit. [It is not clear from Murphy (1993) what variations if any were allowed in \( c \) in fitting the M–M source spectra to the estimated spectra for explosions in granite.] Now, there is a well substantiated inconsistency seen in both frequency-domain and time-domain studies between the apparent \( pP - P \) time (i.e. the apparent \( \tau_w \)) and \( \tau_w \), predicted from known depths of firing and wave speeds in the overburden. Thus for the PILE DRIVER explosion most estimates of \( \tau_w \) are about 0.75 s (Murphy does not give his estimate) and the P-wave speed in the NTS granite is about 5 km s\(^{-1}\). The estimated \( \tau_w \) thus implies a depth of burial of 1.75 km whereas the actual depth of burial is about 0.5 km. Similarly, published estimates of \( \tau_w \) for the French Sahara and E. Kazakh explosions including those of Murphy (1993) imply depths of firing for these explosions of about 2 km which seems likely to be a gross overestimate. (It has been assumed the P-wave speed in granite at these two sites is also around 5 km s\(^{-1}\).) So when Murphy (1993) states that the network averaged spectra are compatible with the theoretical spectra predicted by the M–M model and the appropriate \( t^* \) this would not be true for spectra computed using the theoretically predicted \( t^* \).

Turning now to the deconvolution results. Murphy (1993) argues that because the results of averaging the amplitude spectra across many stations are consistent with the M–M model, the deconvolution results which are available from only a few stations and are inconsistent with the model should be given little weight. Murphy (1993) points to the variability between the stations that are available, as evidence of unreliability and suggests that inconsistencies in the attenuation corrected estimates may arise because of errors in the phase of the attenuation operators used for the deconvolution. However, the information from deconvolved seismograms and other time-domain studies can not be ignored just because it shows that some P seismograms do not conform to the model derived from average amplitude spectra. Seismograms do contain information that is not used in frequency domain studies—i.e. phase information—and which has to be accounted for. The usual way of including phase information in the past has been by modelling SP seismograms but in doing this it is often found that it is only possible to fit the source model by using \( pP - P \) times greater than predicted from the known depth and wave speed in the overburden, and at least for NTS and Amchitka Island explosions, by reducing the \( pP \) amplitude below that predicted assuming elastic reflection at the free surface and adding a later positive arrival (referred to below as \( A_p \)) which is usually attributed to the effects of spalling of the free surface (see for example Bache 1982). It is because of
such evidence that explosion $P$ seismograms depart markedly from what would be expected for a point compression source in a layered elastic medium that leads Douglas, Marshall & Young (1987) to argue that direct interpretation of broad-band seismograms—corrected for anelastic attenuation if possible—may be a more efficient way of determining the seismic source function of explosions than modelling using theoretical sources.

When BB seismograms are analysed even without attenuation correction for stations where site effects are a minimum they show that the simple $P$ & $pP$ model of the explosion source is often inadequate. For example for large yield explosions, such as MILROW and CANNIKIN fired at Amchitka Island (Douglas et al. 1987), and BOXCAR fired at the NTS (Douglas 1991), $A$, is about as large as $P$. (Indeed the signal from MILROW recorded at EKA consists principally of two positive pulses direct $P$ and $A$, with little evidence of $pP$..) The results obtained for CANNIKIN come from three stations one each in India, Canada and the UK and for BOXCAR from a station in the US, one in Canada and a third in the UK. With similar effects seen over such wide geographical regions it seems unlikely that the departures from the simple $P$ & $pP$ model are due to station effects. Consider also the EKA seismograms presented by Douglas (1992) of the E. Kazakh explosions of 1968 June 19 and 1971 June 30. Even without any correction for attenuation the initial $P$ pulse seems to have a significantly higher corner frequency than apparent $pP$ and later arrivals. For the 1968 June 19 explosion there is also evidence of an $A$, pulse following $pP$ which appears to contain less high frequency energy than direct $P$. If it is true that $P$ & $pP$ (and $A$, where present) do have different spectra then the frequency domain techniques such as those used by Murphy (1989, 1993) would not give a true estimate of the spectra of $P$.

At the present stage in the estimation and interpretation of deconvolved seismograms the most sensible way to proceed seems to be to concentrate on understanding those seismograms recorded at high quality stations where station effects are small (and ideally can be attenuated by array processing) and which appear to show clear source pulses. The seismograms presented by Douglas (1992) are intended to be examples of such seismograms. Of course, both the frequency domain analysis of Murphy (1989, 1993) and time domain analysis of Douglas (1992) would be invalid particularly for explosions in granite if true $pP$—that is $pP$ generated by elastic reflection at the free surface—makes a significant contribution to explosion seismograms. What the analyses of Murphy (1989, 1993) and Douglas (1993) identify is the apparent $pP$ which often arrives much later than expected for true $pP$. For the explosions in granite discussed by Murphy (1993) true $pP$ would be expected to arrive about 0.2 s after $P$. The two arrivals $P$ & $pP$ would thus merge. In such a situation further corrections would need to be applied to the spectra of Murphy (1993) to allow for true $pP$, and the initial pulse which Douglas (1992) interprets as $P$ would then be $P + pP$.

In view of the many uncertainties in interpreting even the simplest $P$ seismograms from underground explosions it seems to be premature to conclude, as Murphy (1993) does, that: the M–M source can account for all important features of signals; that the results are consistent with theory; and that consequently observations that are inconsistent with the M–M source—such as evidence of trapezoidal source pulses—are artifacts of the deconvolution process.

REFERENCES