Comments on
The Canterbury Earthquake Sequence and Implications for Seismic Design Levels

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September 25, 2011

The primary issues in this report —The Canterbury Earthquake Sequence and Implications for Seismic Design Levels (hereafter referred to as CES)— relate to whether or not the Canterbury sequence (2010-2011) of earthquakes should lead to a modification of the national seismic hazard maps and zoning codes within New Zealand.

In this report there are four primary issues: 1) Is the lower limit currently used for the magnitude and distance of the floating earthquake in the background seismicity sufficient to guard against damage as observed in Christchurch? Are changes needed to the source model for New Zealand? 2) Should the current ground motion prediction equations (GMPEs) be modified? Do they account for differences in stress drops of earthquakes that occur rarely with those that occur on well-defined faults? Are near-source effects, such as directivity, accounted for? 3) What should be the requirements in design while rebuilding Christchurch over a period of 1-2 years, 5-10 years, more than 10 years. In particular, should time dependent processes, i.e., aftershocks, be considered in defining the design spectrum? 4) What will be the national implications of the Canterbury earthquakes?

1) Is the lower limit currently used for the magnitude and distance of the floating earthquake in the background seismicity sufficient to guard against damage as was observed in Christchurch?

A particular question that needs to be considered is the minimum magnitude and distance of the floating earthquake. Currently New Zealand uses a minimum magnitude Mw 6.5 at a distance of 20 km for the floating or random earthquake in the background seismicity. The ground motion is computed at the 84%. My own experience with critical structures such as dams is to consider Mw 6.5 at 15 km at the median ground motion as representative of a floating earthquake. Thus the 84% of a GMPE is a conservative estimate even though the distance of 20 km is slightly greater. One possibility is to reduce the distance to 10 km; this will approximately double the amplitude of the ground motion predicted by the GMPEs as developed for the western US (*Earthquake Spectra*, 2008). The PGA from McVerry et al. (2006) would increase by about 70% for a Mw 6.5 earthquake (Figure 8 in McVerry et al., 2006).

It is unclear to me how this change would affect the National Seismic Hazard Map. This will depend on how much of the seismicity (or seismic moment) is accounted for by known faults and how much is accounted for by background seismicity, in particular, the floating earthquake. It will certainly make a difference in the spectra for the population centers or other areas where this floating earthquake is applied. I think that the change in the reduction in the distance might be applied for more regions than the population centers. There are other critical elements of the infrastructure in New Zealand, for example, dams in the Southern Alps, power stations on the North Island, military bases, ports, etc. It may not be possible to limit the floating earthquake to population centers—a vague term because there has to be a definition of what constitutes a population center.

The other significant change to the source model is the use of time-dependent hazard. Clearly one must be cognizant of aftershocks following any earthquake, particularly large earthquakes such as Darfield where one could expect an aftershock about one magnitude less than the mainshock (Båth, 1965; Vere-Jones, 1969; Helmstetter and Sornette, 2003a).

2) Should the current ground motion prediction equations (GMPEs) be modified? Do they account for differences in stress drops of earthquakes that occur rarely with those that occur on well-defined faults? How would the GMPEs be modified to account for directivity, basin effects, buried ruptures?

The suggestion of modifying the GMPE seems to be motivated by the high accelerations observed in Christchurch, particularly those generated by the Feb. 22, 2011 Mw 6.1 earthquake[1]. In a paper by Segou and Kalkan (2011) the authors compare predictions of different GMPEs against the data from the Darfield and Christchurch earthquakes. (I presume they used Mw 7.1 for Darfield and Mw 6.3 for Christchurch.) Their results indicate that the GMPEs do much better at predicting PGA and SA at periods of 0.3 s, 1.0 s and 3.0 s for Darfield than they do for Christchurch. The amplitudes from Christchurch are generally larger than what would be predicted by the GMPEs.

The CES report suggests four possible factors for the larger than expected amplitudes: higher than normal stress drop, directivity, basin effects and rupture on a blind fault, i.e., the rupture does not break the surface. The most important effect appears to be a higher stress drop. A cursory spectral analysis (Brune, 1970) of the accelerograms implies a high stress drop around 15 MPa—a value used in the CES report for stress drop scaling of the GMPE.

However, the reasoning that underlies the higher stress drop is not convincing. Both the Darfield and Christchurch events are rare events and occur in basically the same crust. The results of Segou and Kalkan (2011), namely, the GMPE median predictions fit the SA and PGA for Darfield rather well, without any stress drop scaling. Likewise, Figure 3.5 shows that the New Zealand GMPE median (McVerry et al., 2006) generally overpredicts the observations. Likewise, the processed PGA for Darfield is well described by the New Zealand GMPE without stress drop scaling (Fry and Gerstenberger, 2011). The crust may be stronger in this region, but the general variability of stress drop (Allmann and Shearer, 2009) may be the cause of the difference.

The Christchurch earthquake released a large amount of energy at shallow depth (Figure 3.8). The causative fault is close to the city. As noted in the CES report this combination seems to be the primary reason for the large amplitude ground motions. Directivity may have had an effect, but looking at the spatial variation in the corner frequencies for stations such as HVSC, LPCC, CCCC, PRPC, CMHS, I could not discern an obvious pattern to suggest a strong directivity

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[1] [The CES report often assigns this earthquake Mw 6.2, for example, Table 3.1 and elsewhere. Figure 4.6 and its caption indicate Mw 6.3 as does p. 49 and all references for papers submitted to Seismological Research Letters. On p. 1 and 9, the magnitude is given as 6.3 without being specific. Because GMPEs use Mw, it is a good idea to use that magnitude scale. The USGS, the Global CMT and the Earthquake Research Institute at the University of Tokyo all give Mw 6.1. I do not know where Mw 6.3 or Mw 6.2 has originated (It may have come from GeoNet using M_L 6.3.), but unless there is justification to ignore the results of the USGS, Global CMT and ERI, the Mw should be 6.1. Also, the Sept. 4, 2010 Darfield earthquake is assigned Mw 7.0 by the USGS, Global CMT and ERI. The CES gives it a Mw 7.1. The source for Mw 7.1 should be cited, perhaps Gledhill et al. (2011).]

effect (Tumarkin and Archuleta, 1994). A comparison of fault normal and fault parallel ground velocities may be more diagnostic if directivity played a major role in amplifying the ground motion in the CBD or elsewhere. Though there certainly would be some effects of directivity, which may be found by kinematic/dynamic modeling that reproduce the observed ground motion. However, directivity is already included in the construction of the GMPEs (closest distance to the fault implicitly accounts for directivity). The effect of explicitly accounting for directivity is about 2-20% reduction in the sigma of the GMPE (Spudich and Chiou, 2008). A 10% reduction in sigma reduces the uncertainty in a ground motion parameter by about 6%. The exact number is not so important as the fact that in construction of the GMPE, directivity has been taken into account.

The basin effects are fairly obvious in the pronounced oscillations observed at some stations, such as REHS, CHHC, and CCCC, for Darfield and Christchurch. The period of the surface waves is fairly long at 1.5 to 3 s. Though this may not be near the fundamental period of family houses or small buildings, the surface waves prolong the duration of shaking, adding more cycles to structures that may have been weakened and whose fundamental period would likely have shifted to longer period. Basin effects can be determined before a damaging earthquake by the use of permanent or portable seismometers that record local events or regional events. The relative strength of basin effects will depend on how a basin is illuminated by different earthquakes (Olsen, 2000). However, the dominant period(s) can be determined.

The differences in ground motion between ruptures that remain buried and those that break the free surface have been documented in recent earthquakes (Kagawa et al., 2004). The precise cause of this difference is not known (Dalguer et al., 2008). However, an increased stress drop is consistent with the physical explanation given by Dalguer et al. (2008).

The CES report often discusses stress drop derived from energy magnitude M_e (Choy and Boatwright, 1995). This magnitude is rarely used. It is based on computing seismic radiated energy E_S, which is not trivial. There is a lot of uncertainty in calculating E_S (Venkataraman, 2002; Pérez-Campos et al., 2003; Baltay et al., 2011). The ratio of radiated seismic energy to seismic moment times the shear modulus is known as the apparent stress $\left(E_S/M_0\right)\mu$. This measure of stress is less than or equal to half of the static stress drop. Thus measuring radiated energy becomes a means for estimating the stress drop (Savage and Wood, 1971). Although there are difficulties and uncertainties, it is more straightforward to use analysis of the Fourier amplitude spectrum, a la Brune (1970, 1971), to estimate the stress drop for the Canterbury earthquakes and past earthquakes.

Figure 3.16 encapsulates much of the difficulty in trying to find a single approach that will reduce the risk. Suppose that we take the USGS Mw of 7.0 for Sept. 4, 2010; 6.1 Feb. 22, 2011; 6.0 for June 13, 2011. The distance from the CBD to the Mw 6.1 and Mw 6.0 events is nearly the same; the magnitude difference would lead to less than 10% difference in spectral accelerations for periods less than 1.5 s. However, the observations (Figure 3.16) indicate almost a 100% difference. On the other hand, a factor of two is about what the median plus one sigma (GMPE) will produce. While the peak accelerations are nearly the same for the Mw 6.0 and Mw 7.0 earthquakes, the Mw 6.0 response generally exceeds the Mw 7.0 for periods between 0.75 s and

2.0 s. This may be due to the way that the different faulting excited the basin. I also compared, approximately, the response spectrum in Figure 3.16 with the response spectrum in McVerry et al. (2006) shown in their Figure 18. It is not quite a true comparison because McVerry et al. are computing the response for a reverse fault at 30 km. Still, looking at the two, the McVerry et al. response looks very close to that shown as the average on Figure 3.16, suggesting that the GMPE in New Zealand is capturing the expected ground motion.

To be conservative, it might be prudent to use a higher stress drop for the random background earthquake. Except for basin effects, it would ameliorate the ground motion predictions for rare events in accounting for the effects of stress drop, directivity and buried ruptures. If that were coupled with bringing the distance to 10 km, the net effect would be similar to using the median plus two-sigma ground motion for an earthquake with Mw 6.5 at 20 km. That might be more conservative than necessary.

3) What should be the requirements in design while rebuilding Christchurch over a period of 1-2 years, 5-10 years, more than 10 years. In particular, should time dependent processes, i.e., aftershocks, be considered in defining the design spectrum?

With Omori's law (the number of earthquakes is inversely proportional to the time since the mainshock) and Båth's law (expect at least one aftershock with a magnitude about 1.2 magnitude units less than the mainshock), there is every reason to consider the effect of aftershocks following any earthquake of Mw ≥ 6.5. Exactly what seismicity model(s) to use is subjective. The ETAS model (e.g., Ogata, 1988; Helmstetter and Sornette, 2003b; Werner et al., 2011; Zhuang et al., 2011), which has been well tested (SCEC Collaboratory for the Study of Earthquake Predictability), is one that should be considered. I don't know that ETAS would change the basic result in Figure 4.2, namely, that the rate at which the probability changes is not constant in time. Figure 4.2 shows that the construction in the next year or so would be subject to a higher probability of a significant earthquake than what would happen over the period 2013-2020 with a return to a background rate beyond about 2020. The concept of the rate changing is valid. But it is model dependent. The rates do not say what to expect for the size of the next event. Given Båth's law, one would say that the expected large aftershock has occurred. There always remains the possibility that either the Darfield or Christchurch earthquake is a precursor to something larger.

I found Figure 4.4 confusing. The labeling of the two spectra was left off. (It is found on Figure 3 of Gerstenberger et al., 2011). I was surprised that the Z=0.3 spectrum would be considered satisfactory. The 10% in 50 year spectrum exceeds the Z=0.3 spectrum for all periods less than about 0.33. With the basic idea of a 3-storey building having a fundamental period around 0.3 s, and a 2-storey building around 0.2 s and 1-storey around 0.1, it seems that a lot of residential housing and apartments are at risk. Perhaps a discussion with the authors would enlighten me as to why most family homes would be all right under this design. It doesn't appear that is the case.

The design spectra are all anchored to the period 0.5 s. Yet, all of the figures showing SA as a function of distance (Figures 3.7, 3.10 and 3.11) for three Canterbury earthquakes were plotted for a period of 1.0 s. These plots are not very useful when trying to infer whether the Canterbury earthquakes were worse than expected. Similarly, Figure 4.4 does not have the original design spectrum, Z=0.22 for comparison with the proposed Z=0.3 spectrum.

It is good to see (comparing Figure 4.7 with the Z=0.3 spectrum in Figure 4.4) that even a Mw 8.2 on the Alpine Fault should not cause much of a problem for Christchurch at any period.

It appears that the Feb. 22, 2011 earthquake has provided the most severe test one would expect to use for modifying the response spectrum criterion for Christchurch. As mentioned earlier, the average response spectrum for the CBD is at the median plus one sigma level of ground motion. The closest distance (Joyner-Boore distance) to the projection of the fault onto surface is about 4-7 km. I have not calculated what the response spectrum of an earthquake with Mw 6.5 at 10 km would look like, but I have to wonder if it would be much larger than what was already observed. It is hard to know if there is another fault within 5-7 km of the CBD that is large enough to host a Mw 6.5 earthquake.

4) What will be the national implications of the Canterbury earthquakes?

The Canterbury earthquakes have demonstrated once again that the unexpected earthquake can have profound effects when it occurs very near a populated area: for example, 1971 San Fernando, 1988 Spitak, 1994 Northridge, 1995 Kobe, 2003 Bam, etc. As noted in the CES report, the principal factor for the damage in and around Christchurch from the Feb. 22, 2011 earthquake was the combination of the proximity of the fault to the built environment and the high stress drop associated with the earthquake.

There is an undercurrent throughout the CES report that the ground motion model for New Zealand was insufficient, especially as applied to the Feb. 22, 2011 and June 13, 2011 events. “Once we take account of earthquake magnitude and distance, we see that the earthquakes of 22 February and 13 June 2011 both produced higher levels of shaking than expected compared to the average New Zealand model at distances of less than 10 km from the fault (Figures 3.10, and 3.15).” p. 43. The statement is true in that the ground motions are compared to the average. However, in looking at these two figures, the spectral values are entirely consistent with the 84% and the 95% levels. Of the 11 data points, 8 are within ±1 sigma and all 11 are within ±2 sigma of the mean. Admittedly the data points are not symmetric about the mean, but who would design for the median minus one sigma?

The argument I find most convincing is that rare events might represent a population of earthquakes with higher than average stress drops. If one couples that thought with the other argument presented, namely, it is exceedingly difficult to define all faults that might produce a Mw 6.0-6.3 earthquake, then some of the proposed objectives in Section 5 are better supported than others. I thought the identification of regions with both low deformation rates and a strong crust (p.45) is a good means of regionalizing the problem (Brune and Thatcher, 2002). In regions where this condition is met, the question will be in specifying the random earthquake. It is likely to dominate the hazard.

I think that making M_{cutoff} ~Mw7.2 everywhere may reduce the hazard in the regions with low deformation rates. My naïve reasoning is that if M_{cutoff} were smaller, more earthquakes with Mw

6.0-6.3 would have to occur to take up the deformation rate. As a consequence, these earthquakes will sample the tails of the ground motion distributions, which would lead to more extremes. I completely agree with the statement that specifying M_{cutoff} needs some careful thought about its impact on the NSHM.

While I wholeheartedly support reexamining the GMPE given all the new strong motion data since 1996, I don't know if that is going to lead to a radical change. Expecting that median stress drop will have a major effect is not obvious. Stress drops have a rather stable median value —4.0 MPa worldwide—but with a huge scatter 0.3 – 50 MPa (Allmann and Shearer, 2009). Oth et al. (2010) have shown that crustal earthquakes in Japan have a median stress drop of 1.1 MPa, but subcrustal earthquakes have a median of 9.2 MPa. (Allmann and Shearer use Madariaga's corner frequency-radius relation, while Oth and others use Brune's. Madariaga's relation produces a stress drop that is 5.5 times larger than Brune's.) Rather than computing Me, about which I have many reservations, I would suggest examining stress drops for the earthquakes used in developing the GMPE. The auxiliary material with Allmann and Shearer (2009) has the data set used for their 1759 earthquakes between 1990-2007, $M \geq 5.5$. There is a good chance that many of the earthquakes used by McVerry et al. (2006) are included as will be those after 1996. For those stress drops not included, I would look at the accelerograms first and apply a Brune-type analysis to determine the stress drops. Looking at the literature, it is obvious that stress drop is highly variable. A median stress drop will not be sufficient; one will need the sigma. It is not obvious how that will work into the uncertainty in the GMPE, but it is worth investigating.

Let me comment briefly on use of the energy magnitude Me. The measurement does not lead directly to stress drop. One has to measure the radiated energy. The ratio of radiated energy to seismic moment (multiplied by the source shear modulus) gives apparent stress. Computing radiated energy has a large uncertainty, factor of 10 between regional and teleseismic estimates (Singh and Ordaz, 1994)—though this can be reduced with corrections for attenuation, site amplification, directivity, etc. (e.g., Pérez-Campos et al. 2003; Venkataraman, 2002). Apparent stress is not the stress drop used by Atkinson and Boore (2006). There is a large variability allowed for converting apparent stress to stress drop. That is on top of the variability in computing apparent stress. One paper with a very good discussion of some of the issues is Singh and Ordaz (1994). In Appendix B of Singh and Ordaz (1994), the plot of the fraction of energy versus normalized frequency points out much of the difficulties with obtaining radiated energy. One has to integrate the spectrum out to frequencies that is six times the corner frequency to get 80% of the energy. The velocity structure has to include the attenuation structure. As shown by Oth et al. (2011) in analysis of more than 67,000 S-wave records of earthquakes in Japan, the attenuation can vary from region to region. In short, calculating radiated energy is not straightforward, nor is relating the apparent stress to stress drop. It is more straightforward to compute stress drop a la Brune (1970) or use the root-mean-square of the S-wave acceleration (McGuire and Hanks, 1980).

There is no question that directivity strongly affects the amplitudes of the ground motion, particularly for periods contributing to the peak ground velocity. As mentioned earlier, the construction of the GMPE will likely account for most of the effects of directivity. If one allows that the Darfield earthquake probably had a significant directivity with rupture on the Greendale fault aimed directly towards Christchurch, the GMPE prediction (Figure 3.5) overpredicts all of the data observed in Christchurch. I agree with the statement "directivity may be accounted for by an appropriate increase in variability of expected ground motions." (p. 53). There may be special, site-specific cases where directivity would be applied, but for the NSHM, I would not know how to include directivity without almost double-counting its effect already built into the GMPE.

There is nothing unusual about vertical motions having more high frequency content than the horizontals and with larger amplitudes (e.g., Brady et al., 1980). Given the widespread liquefaction, particularly associated with the February 22 event, it is not surprising to observe vertical accelerations larger than the horizontals. This would be expected if one has nonlinear soil response—the shear modulus is reduced but Young's modulus is not as affected—though the shallow water table may have increased the amplification of the vertical motion (Yang and Sato, 2000). Near-source recordings of nuclear explosions (e.g., Hutchings et al., 2005) show extreme levels of vertical acceleration that probably would not exist if nonlinear soil response were as effective for the verticals as it is on the horizontals.

One of the most critical issues will be the setting of the lower bound for earthquake design. I have referred to this, perhaps mistakenly, as the random or floating earthquake. On p. 54, "The minimum Z factor is based on the 84th percentile motions from a magnitude 6.5 earthquake at 20 km distance." The magnitude Mw 6.5 seems appropriate. Using the 84% everywhere is conservative. One possibility is to reduce the distance. As described on the first page of this report, reducing the distance by a factor of two (from 20 km to 10 km) multiplies the design spectrum by about the same factor. To reiterate the one-sigma uncertainty in the GMPEs is about a factor of two. Thus use of the 84th percentile has already allowed for about one factor of two. One way to judge if lower bound is already appropriate is to look at Figure 3.11—assuming this applies equally well to different periods. The spectral acceleration of the 84th percentile at 20 km underpredicts 10 of the 11 measurements for distances less than 10 km; the spectral acceleration of the 84th percentile at 10 km underpredicts 8 of the 11 measurements for distances less than 10 km. The GMPE shown in Figure 3.11 is presumably for a Mw 6.2 earthquake. An empirical estimate for Mw 6.5 could only raise the spectral level and thus capture more of the data.

The Z factor is also modified by a magnitude-weighted factor described in Appendix 5. I was not able for find the reference Kennedy et al. (1984). This magnitude weighting is new to me. As described it reduces all of the spectral values for periods less than or equal to 0.5 s for any earthquake with Mw less than 7.5. Given that the NZ design spectrum is scaled by the value at 0.5 s, this magnitude weighting effectively reduces the design spectrum for most earthquakes in New Zealand. (Because this is the first time I have seen magnitude weighting applied to the spectrum, it may be ignorance on my part that I am thinking it reduces the spectrum.) With only one study supporting its use for structures, not liquefaction for which it was originally proposed, the use of magnitude weighting should be investigated more thoroughly.

As I understand it, the Z factor is effectively increased by using the 84th percentile ground motions for a Mw 6.5 at 20 km. Then the Z factor is effectively reduced by about the same amount by using magnitude weighting. The former I can understand; the latter I do not.

The Canterbury earthquakes have provided an impetus to reevaluate the methods by which seismic hazard and seismic design are calculated for New Zealand. In itself this is a good idea. There is a positive and negative (I might find a better word.) element to this analysis. The positive is that one will have to consider new data and processes that were less well known 10 to 15 years ago. Certainly considering the rare event is an important development. The negative is that one can be biased by the data from the rare event. For me it is a philosophical question that has a practical outcome. How does one handle the statistics of a heavy-tailed function? The mean or median reflects the ordinary event—the event for which we have pretty good statistics. The tail of the function, the rare event, is not captured by the description of the mean or median unless one goes to three sigma or more. I think by considering the physical situation, such as variability in stress drops for different regions, local geological conditions (basins, water tables, etc.), and making good use of smaller earthquakes to constrain attenuation, basin-edge generated waves, and stress drops one can make better estimates of the ground motion from future earthquakes—both those near the median and those in the ends of the tails. Many of the ideas expressed in this CES report are steps in that direction.

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