Replies to Referee #2

It is unfortunate that referee #2 was unfamiliar with the physical wavelet methodology used to find physical laws for the precursory phenomena of impending large EQ events. Therefore referee #2 has some difficulty understanding their relations to the seismogenic processes of impending large EQ events observed by seismologists as stated in section 5.

First, I would like to reply to your main objection by using the same reference stated in the reply to referee #1's section (a), which discusses deterministic chaos on the observed seismicity as in Fig. 3a. I would also like to place my replies following each of your some other minor points.

This work is not related to any of those statistical analyses suggested for proving the predictability. I may only say the following:

Reference (Takeda, F., The precursory fault width formation and critical stress state of impending large earthquakes: The observation and deterministic forecasting; AGU, Fall Meeting 2009, NH13A-1126, http://adsabs.harvard.edu/abs/2009AGUFMNH13A1126T), has the largest Lyapunov (Ly) exponents calculated by using the same time series as $d\,(c,\ m)$ in Fig. 3a, as listed in a table below. The reference shows that the largest exponents of $d\,(c,\ m)$ in the property space are all positive, statistically distinct from those surrogated by randomly shuffling only the event index. The t-test (for six surrogated data sets) with the confidence level of 99.9 % suggests that the seismicity of $d\,(c,\ m)$ is a deterministic chaos so that one may have a deterministic model for the prediction. However, there are three large EQs and one EQ swarm as in Fig. 3a (see also Fig. 2a). Since the Hurst exponents of each $d\,(c,\ m)$, which are also listed below, suggest that each has a long memory of large evets, the seismicity after main events is deterministic chaos. If one can find the same result before main events, one may be then able to make a physical model for the EQ predations. This evaluation of the deterministic chaos is difficult for any tectonically active region where large EQ evets often occur.

The statistical quantiles in the reference are as follows.

c	Largest Ly exponent	t-test 99.9% level	Hust Exponent
LAT	0.331 ± 0.028	0.410～0.451	0.84
LON	0.459 ± 0.032	0.497～0.551	0.77
DEP	0.335 ± 0.026	0.341～0.453	0.76
INT	0.241 ± 0.024	0.247～0.327	0.78
MAG	0.155 ± 0.019	0.156～0.199	0.58

One may note that the well-known Hurst exponents are 0.78 ±0.09 and 0.5 for many natural systems and Brownian motion, respectively.

Physical wavelets are fundamentally different as stated in the text (see also the replies to referee #1). I would like to make very simple statements here. The well-known wavelet analyses cannot obtain

velocity (momentum) and acceleration (force) from non-differentiable time series. Physical wavelets can find the same results obtained by the well-known wavelet analyses (Takeda, 1994). Similarly, time delay embedding cannot construct displacement-momentum or displacement-force phase spaces.

Some other minor points.
Pag.8 Lines 8-9. "The NCI(m, 2s) is proportional to seismic activity. If it is large, the activity is quiet.."
From the second sentence it seems that NCI(m,2s) is inversely proportional to seismic activity.
Reply to this:
We discuss if the activity is quiet (seismic quiescence has large *INT*) or active (small *INT*). In this sense, large or small $NCI(m, 2s)$ is quiet or active (or noisy), respectively.

Pag.8 Lines 15-16 "The AMR's in the large region generally start a few days before a large event occurs somewhere in the region as well as before a large aftershock occurs (Takeda, 2015)." Generally AMR does not start a few days before a large event but starts months or even years before (Mignan et al., 2007).
Reply to this:
The tool of (Mignan et al., 2007) to detect AMR is completely different from our tool to monitor the state of strain energy density expressed by NCI(m,2s) and NCD(m,2s). If the observational window of AMR is different, the start of AMR is different.
As an example of this, we have demonstrated that the observed periodicity of seismicity in section 5.1 will depend on the size of seismic region (in section 4.6), the observational tool and its width of time window.
As for this and my first reply stated above, a great mentor and seismologist Keiiti Aki, had commented in his email sent to me as follows:
Jan 27, 2005
Dear Takeda-san:
I finished reading through all the documents you sent me, and I now feel that our accidental meetings might have been planned by someone in the heaven.
Your successful accomplishment as an engineering consultant, your background as a physicist and your humanitarian wish to mitigate the earthquake disaster meet everything I expect for someone to practice the future earthquake prediction research. As I explained in my extended abstract of a talk at an international meeting on "Imaging Technology" held in Sendai last November, which I have asked Anshu Jin to mail to you, I believe that the earthquake prediction is not an academic problem, but an engineering application of Seismology. The problem involves three elements; physics, nature and society (the three most beautiful things human beings experienced in this world). I think we need someone like you to solve this problem.
I attended an international meeting in Spain in October, 2004, celebrating the centennial

anniversary of an old observatory. Don Turcotte was there and gave a talk concluding that the seismicity is a chaotic noise. I started my talk saying “I accept that completely as phenomena originating from the brittle part. Our data are dominated by the events from the brittle part. We need to find faint signals from the ductile part which can be modeled deterministically."
Here I recognize some difference between you and me. You are characterizing the phenomena as "deterministic chaos”, opposing the view of Turcotte's group. It seems to me that there is no need for this conflict, if you consider that the physical system is not just the brittle part but includes the ductile part. I learned it from my experience with an active volcano as described in my Trieste lecture note, which again I asked Anshu to mail to you.
Thank you for an exciting time I had since reading your mails.
With best regards,
Kei Aki

Jan 27, 2005
Dear Takeda - san:
My excitement continues from reading your paper.
First, you do not seem to be bothered by the 60 - event periodicity, attributing it to some process at the brittle-ductile transition zone. Seismologists would react with the suspicion that some artifact in analysis causing it, and discredit your finer interpretation as your imagination. I am amazed in your confidence as a physicist that such fluctuation can be expected as a physical phenomenon. Personally I believe that this periodicity is real, indicating a clear departure of the process involved from the self-similarity, possibly due to the unique size of the fractures in the brittle part of the lithosphere (a few hundred meters to about a km) that I have proposed since the 1989 JGR paper with Anshu Jin. There are numerous observations supporting the existence of such a unique length as I described in my Trieste lecture note, but I still cannot prove it. For example, as you find in the fluctuation of coda Q and N(Mc) in California by Jin and Aki (as quoted in my 2004 EPS paper), we saw a periodicity of about 10 years. The fluctuations in these parameters in other areas are usually several years, much longer than what you showed in your figures. So there must be some artifact in the apparent periodicity that needs to be clarified before convincing seismologists about their physical reality.
Secondly, your distinction of CQT (T for Tottori) and CQK (K for Kobe) is extremely interesting because the high resolution map of coda Q obtained from the 1000 Hi-net stations and the map of N(Mc) from the JMA data both obtained recently by Anshu also identify the two areas not only as anomalous, but also in distinctly different ways. I have not digested fully these observations, but I feel that both you and Anshu are detecting the common phenomenon through different windows. Would you two exchange papers and start communicating each other? There is not much time left, because Anshu must quit her position at NIED at the end of March, as I mentioned in my earlier mail.
Have you read the extended abstract of my paper titled --A perspective on engineering application of

seismology-- presented at an international symposium organized by the Society of Exploration Geophysicists (SEG), Japan, which I asked Anshu to mail a copy? I have a feeling that my dream about the future of earthquake prediction described in that paper may be realized by you. Perhaps that was the intension of someone in the heaven who arranged several accidental meetings between you and me!
With best regards,
Kei

As for the periodicity of several to 10 years mentioned above, we have clarified in section 5.1 that our periodicity of about 2 years (about 600 days) becomes 8 years if we use his observational time window.

In the references some articles are not easily accessible.
For instance: Takeda, F. and Okada, S.: Time Series Analysis with Physical Wavelets, 20 http://adsabs.harvard.edu/abs/2001APS.MARX23005T, 2001. when I attempt to reach this document I have the following message: No valid abstract selected for retrieval or not yet indexed in ADS
Reply to this:
I appreciate your complete checkups of references. It is a typo. Two dots should be after APS.
http://adsabs.harvard.edu/abs/2001APS..MARX23005T

In addition all below references are in Japanese:
TEC21 website: Crustal movement that caused the 2011 M9 Event,
http://www.tec21.jp/g_eq_tohoku_crust_m.htm, 2017a.
TEC21 website: The 2011 M9 Event and Earthquake Prediction,
http://www.tec21.jp/News_EQ_forecasting_j.htm, 2017b.
TEC21 website: Cycle of strain energy density accumulation,
http://www.tec21.jp/critical_cycles.htm, 2017c.
TEC21 website: Predictions and Diagnostics–Industrial Systems,
http://www.tec21.jp/Indust_sys_j.htm, 2017d.
TEC21website: Precursors and Predictions,
http://www.tec21.jp/pr_CQK_CQT_model_1.htm, 2017e.
Reply to this:
They are all from my patents most of which are in Japanese. Their English translations are under way by the Japanese Patent office. Google may translate them for you. I am also writing a few English drafts to be submitted to some geophysical journals.