

Discussion summary

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1 INTRODUCTION

International experiments have been conducted at Turkey Flat, California and Ashigara Valley, Japan to systematically compare and evaluate current methods for predicting the effects of surface geology on seismic motion. The objectives of this Special Theme Session are to review the results of the two experiments, discuss lessons learned from these experiments, and discuss future directions for the international experiment.

2 SUMMARY OF EXPERIMENTS

The special theme session began with presentations on both the Turkey Flat and Ashigara Valley experiments. Material is presented elsewhere in this report on the Special Theme Session from each author. Only summary comments and discussions are recorded here.

The test sites were chosen according to the following factors:

1. There was a reasonable likelihood of recording strong shaking in the short term,
2. Geologic site conditions are relatively well defined,
3. Conditions are similar to those found at some typical urban/industrial sites, and
4. The sites are within and complementary to other existing arrays.

Basic elements of the experiments include:

1. Comparing geotechnical measurements and models,
2. Making blind predictions of weak and strong ground motion from earthquakes and comparing prediction models, and

3. Comparing results of predictions using a standard geotechnical model and a preferred model.

Some general comments and observations concerning the experiments are indicated below:

1. There is considerable difficulty making reliable geotechnical measurement of the site leading to uncertainty in model parameters. This suggests a future focus on geotechnical issues (e.g., reliable characterization of the site) rather than more complex modeling.
2. There is a need to upgrade instruments to 16-bit A/D from 12-bit A/D to improve the dynamic range of the measurements.
3. "SHAKE"-type analyses lead to overestimated damping; this seems to be *inconsistent* with observed predictions
4. The grouping of results appears systematic e.g., "SHAKE"-type codes lead to similar results. This hypothesis has not been tested.
5. The levels of acceleration were in micro-g range, where the damping in rock is approx. 1% and for soil approx. 7-9%. It was emphasized that a distinction must be drawn between lab and field results on damping.
6. The Ashigara test site is rather complex. Velocity inversion is present which leads to opportunity for individual layer response rather than whole deposit, and measurements being made across a discontinuity.
7. For both comparison purposes or calculations for a specific site, a major problem faced is variability in the input. Methods of analysis are considered better than the experiments suggest. Indeed, centrifuge analyses, where there is

greater control over the experiment, yield better results.

8. A one-dimensional analysis is limited in that full information on waves contributing to surface motion is not available: There is certain to be surface waves at this site, e.g., Mexico city showed effects of surface waves.
9. A two-dimensional analysis *should* produce better results, but did not. The reasons for this are not yet understood.
10. The complexity of the test sites, particularly Ashigara Valley, is thought by some to unnecessarily penalize the ability to do site-specific studies.
11. More attention needs to be given to input and what is legitimate site geometry/topography.
12. Treasure Island (San Francisco) represents a good site to model, yet differences of 100% are observed between predictions and observations.
13. Further study is needed to determine whether or not period lengthening due to pore pressures is a factor in the measured ground motion.
14. Only six of the participants in the Turkey Flat experiment gave estimates of variability associated with their predictions (i.e., made an attempt to quantify the uncertainty). Further studies of this aspect are needed.

3 SUMMARY OF PANEL AND GENERAL DISCUSSIONS

The Panel consisted of Bard (chair), Donovan, Liam-Finn, Iai, Tucker, Irikura and Paolucci.

Tucker began with a discussion of the California Division of Mines and Geology's (CDMG) motivation for the experiment. The advantages and disadvantages of blind tests were not seriously considered at the initiation of the project: It just seemed like a good idea. CDMG was given the responsibility of estimating the expected shaking near the sites of public structures (e.g., prisons), which placed it in a difficult position given the state of the art and its ability to assess methods of prediction. It was felt that a comparison/test such as this would serve the public interest. However, there are clearly now more questions than when process started. It appears that accuracy of geotechnical properties are a significant weak point. Coordination with the Ashigara experiment could have been better (in the geotechnical context in particular.)

As regards the similarity of the results of the two- and three-dimensional models, the structure is complex, most likely requiring 3-D modeling. However, given the uncertainty in geotechnical parameters, it is likely that the 2-D models have not been given adequate parameters.

Liam-Finn referred to some of the comments in his earlier discussion. He noted that Turkey Flat is a simple site and Ashigara is complex. Also, field variation exists and indeed may be unresolvable. He encouraged the use of centrifuge modeling for verifying method of analysis and estimating the reliability of site-specific responses. The chosen sites test reliability of site-specific studies under better than normal conditions. He added that efforts should be made to combine a more formal reliability-type analysis with FEM analyses. For example, extant analyses could be repeated with more careful attention paid to modeling the uncertainty in the parameters and the models, and its effects on the prediction. This type of approach would serve to validate our ability to generate reliable predictions under realistic conditions.

Donovan referred to an old Spanish earthquake document, with a quote that seemed to be appropriate: "Drowning in information; starving in knowledge." Turkey Flat is not an ideal site, but was the only one available in the area. It is not a soft soil site. He suggested an instrument on rock on both sides of valley. It was noted that the transfer functions showed evidence of free-surface reflection (factor of 2 amplification.) When one looks at the Ashigara predictions, it is questionable as to whether predictions are good or not. His feeling is that the one-dimensional approach is a good way to go. The results show some promise, with the range of the answers falling well within one order of magnitude. Further information can be gained from Ashigara, but it would take a lot of work.

Paolucci noted that small variations in S-wave velocity profile strongly affected results i.e., in amplitude and structure of spectrum. He noted that the selection of the input motion for the model (i.e., which "baseroack" record) produced not much difference in the range 0 to 4 Hz, but elsewhere significant differences in the transfer function were evident. He also noted that two-dimensional results were similar to one-dimensional results. The role of internal soil damping and frequency is important. He believes that hysteretic damping models are better at high frequency (where PGA occurs) while linear damping models are better at low frequency. He also pointed out that commonly used spectral *shapes* are not so

bad, even if amplitudes require more work.

Irikura posed a number of important questions. What is the goal of the test? For example, in the ratio of KS2/KD2, should we be aiming for a factor of 2? Is that a reasonable goal? Clearly there is significant scatter even for one input motion; caution in comparison is advised. What is the effect of incoming wave directionality? Do similar angles produce less scatter? Scatter would appear to be a 2-D/3-D effect, but why is there so much? Indeed, there is some question as to whether the so-called "standard" model is actually standard. Did participants in fact use different models? This was considered a major question to be resolved. Donovan later noted that the standard model still left some scope for variation by the modeler.

Iai noted that most of what has been learned has been discussed. The major goal (of this meeting) is to decide where to go from here. What items are important, and how should they be prioritized.

Bard proposed a "grid" to focus the remainder of the discussion. The grid consisted of the following elements:

1. What has been learned?
2. What remains to be learned? For example:
 - geotechnical measurements
 - appropriate input motion
 - geometrical effects
 - nonlinear effects
 - computational methods (e.g., 1-D scatter)
3. What may be done with the Turkey Flat and Ashigara Valley results?
4. Should the instrumentation be modified for future strong motion events at Turkey Flat and Ashigara Valley?
5. Suggestions for new international experiments

In response to item 1., Stepp noted that great variability exists in standard practice; that is important in itself. There is clear indication that material property determination is important. There is some question as to whether lab and site tests are comparable: Indications are that they are not. It is necessary to perform whole waveform analyses in the field to determine low-strain properties. In the Ashigara Valley and Turkey Flat experiments, 1-D modeling does a good job; 2-D/3-D modeling offers no significant improvement. However, we know that

2-D and 3-D effects will be important in some cases – where is that line? Stepp endorsed item 3, indicating that we must make stronger use of existing information.

Iai suggested that there are too many problems involved: This is a "well-posed" problem, yet still obtaining meaningful results is difficult. A well-organized blind prediction experiment is necessary where it may be simpler to learn.

Petrovsky noted that at Ochrid Lake in Macedonia in the 1980's 77 array instruments were deployed in a three-dimensional array. These instruments recorded a number of events in the range M4.5 – 5.5, with PGA's in the range 20-35cm/s². These, however, are still insufficient to give an idea of the modification of frequency content by sites. Two problems are being mixed: consideration of topography effects and modeling of soil properties (linear and nonlinear). Are we in a position to handle this yet? Petrovsky felt that we must look to a number of experiments, otherwise we must stick to highly speculative methods of analysis.

Bard pointed out that differences are not in computational methods alone since in some cases the same model and methodology led to different results. Differences between predictions and observations can also be attributed to bad geotechnical surveys or the use of bad inputs (or insufficient input data.)

Petrovsky again questioned why the differences occurred if the same model was used. Participants should use the standard model first to understand the differences.

Li-li Xie suggested that while progress had been made, there remain uncertainties in many aspects. More experiments are necessary, and we can do much better. We must ascertain the source of the scatter in the results: How much is from incorrect model, data, etc.?

Petilakis commented that he was not disappointed. He felt we found what we expected given the set up of the experiments, our knowledge, etc. He emphasized again that *models* are different even if *codes* are the same. Modeling strategies differ from person to person, with the "standard model" depending strongly on the "personality" of the modeler. He also noted that many 2-D models are really effectively 1-D models. An important caveat: Statistics mean nothing in this case, as we are comparing different models. He proposed that we examine the best models, and try to understand why they worked so well. He also noted the difference between practical and theoretical issues: We need to know what

is an appropriate code and model to use *in practice*, setting aside some of the details of the theoretical issues.

Finn asked whether the choice of velocity in the models is optional? If so, this is perhaps an important area for further discussion.

Tucker suggested that a volunteer is needed to do a detailed shakedown and analysis of the models. This is likely to be a fundable project.

Heidebrecht suggested that a first stage would be for those who have data to identify groups with similar methodology but different results in order to try to figure out where the differences are. It is clear that we expect some variation, but what is the order of these variations and what is their source?

Petrovsky reiterated that he has some results available, and that continued collaboration between the USGS and (the former) Yugoslavia is desirable.

Celebi proposed a new site: Erzincan, Turkey. The topological and geological setting is unique. It is a "deep basin," not as large as Mexico (15-20 km by 40-50km). The town of Erzincan is at an elevation of 1200 meters. Adjacent mountains rise to 4000 meters. The water table is quite high. The geology is quite well defined, with steep mountains north and south. The fault line is well defined also, and well-defined gaps exist representing sources for future events. There is already 1 SMA in the basin (deployed before the earthquake.)

Petrovsky asked about the possibility of conducting an experiment in the basin of Mexico City. There is a detailed SMA network in Mexico City which forms a good microzonation base. In addition, there is the Ochrid Lake three-dimensional array. We should start new experiments now. Data collected from strong ground motion arrays in the past have proven very useful. The data furnished by these new experiments is likely to be of equivalent utility.

Shakal suggested some California sites where one might perform experiments on sites with poor soils (S4). He indicated that the frequencies are lower, and it is the lower frequencies where the damage occurs.

4 CONCLUDING REMARKS

The following are major factors to be considered in any future experiments on the effects of surface geology on ground motion:

- There is a need to move toward a probability-based approach to capture and model the uncertainty in the predictions;

- New sites must be carefully selected to avoid the difficulties associated with the complexity of the Ashigara Valley and Turkey Flat sites; and

- Experiments must be carefully designed to isolate and capture the desired characteristics.

It was noted that a "simpler" site or the use of "simpler" models does not necessarily mean that the modeling and prediction task will be any easier.