Open Model of Probabilistic Seismic Hazard Assessment for the Indian Subcontinent

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Abstract

Open models encourage peer review and collaboration; open models can be built upon.

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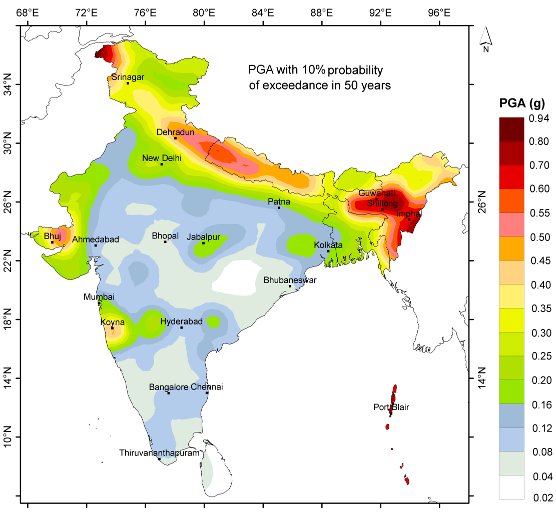

Figure 1: Hazard map of India

1 Introduction

1.1 Overview

1.2 Background

We'll be adapting the model of Nath and Thingbaijam (2012) to the OpenQuake platform.

2 Model development

2.1 Seismogenic Sources

Thingbaijam and Nath (2011)

2.2 Ground-Motion Prediction

Sharma et al. (2009) points out that the decay rate of PGA for shallow India-Bangladesh and deep India-Burma border events have different distance scaling. The former leads to the necessity of a GMPE specific to the Shillong plateau Nath et al. (2012) while the latter means interface subduction events need to be treated differently.

Issues encountered while implementing GMPE logic tree:

- Layer 4 depth range of 180-300 km is significantly deeper than deepest events used in regression for ATBO03 (100 km), LILE08 (161 km), ZHAO06 (120 km) and GUPT10 (148 km) are specified for. YCSH97 only included events to 229 km. KANN06 is specified to 200 km depth, but is only used for interface events (layer 2).
- Assumed and Andaman-Sumatra subduction missing from Figure 3.
- Why is Youngs (1997) not used in the subduction interfaces?

Table 1: Ground shaking intensity models. Models newly implemented in OpenQuake as part of the current work are indicated.

Code	New	Reference
AKBO10		Akkar and Bommer (2010)
BOAT08		Boore and Atkinson (2008)
CABO08		Campbell and Bozorgnia (2008)
ZHAO06		Zhao et al. (2006)
ATBO03		Atkinson and Boore (2003)
ATMA09		Atkinson and Macias (2009)
LILE08		Lin and Lee (2008)
YCSH97		Youngs et al. (1997)
TORO02		Toro (2002)
ATBO06		Atkinson and Boore (2006)
CAMP03		Campbell (2003)
SDBK09	✓	Sharma et al. (2009)
NTMN12	✓	Nath et al. (2012)
GUPT10	✓	Gupta (2010)
KNMF06	✓	Kanno et al. (2006)
RAIY07	✓	Raghukanth and Iyengar (2007)

- Should the Japan/Cascadia distinction not also be used for interface subduction with Atkinson & Boore (2003)?
- Nath and Thingbaijam (2012) doesn't seem to me to follow the recommendations of Nath and Thingbaijam (2011) as far as having two subduction intra-slab sub-regions: the former uses Indo-Myanmar and Himalayas while the latter recommends Indo-Myanmar and Hindukush. Nath and Thingbaijam (2012) is followed strictly for for phase 1.
- Assignment of source mechanism (normal or not, matters in shallowest layer only) is tricky. Dip cannot be used to distinguish normal and reverse subduction because the subduction interface angle is not known. different GMPEs use different rake thresholds; a threshold of 30° was chosen, consistent with Boore and Atkinson (2008); Campbell and Bozorgnia (2008) but not Zhao et al. (2006).

Issued encountered while implementing GMPEs:
Sharma et al. (2009)

- lacks a M^2 term Cotton et al. (2006)
- does not define rock vs. soil

Raghukanth and Iyengar (2007)

- typographical errors in coefficient tables: grossest error fixed, 3 other errors causing approximately 10% error not fixed
- actually defines 4 different models: must assume that for all of peninsular India was used by Nath and Thingbaijam (2012), not one of those for sub-regions.

2.3 Logic Tree Weights

Anbazhagan et al. (2015) seem to be proposing different weights for different regions based on single events in those regions. An extreme example is to define different weights for Anjar, 1956 and Bhuj , 2001 earthquakes even though the epicentres and depths were very close together. In contrast Nath and Thingbaijam (2011) compute LLH for 7 regions (using 38 events total) and state that, "individual events do not have significant number of observations to support a viable ranking basis."

Anbazhagan et al. (2015) seem to misuse the concept of data support index (DSI) (Delavaud et al., 2012) by setting weights to zero when the DSI is negative. The threshold is arbitrary and is chosen without discussion. As Delavaud et al. (2012) point out "more important than the sign of the DSI is the difference of DSI between two models."

Both Anbazhagan et al. (2015) and Nath and Thingbaijam (2011) rely on estimating ground motions from macroseismic intensity. I'm sure it is a matter of low seismicity and lack of instrumentation, but I'm still surprised. I would expect the catalogue for peninsular India to be complete for 20 years to magnitude 5 so that one could thus get 10 well-recorded events, at least. There is significant additional (aleatory and epistemic) variability in mapping EMS to PGA which must obscure the true performance of the GMPEs. Perhaps this is part of why Anbazhagan et al. (2015) and Nath and Thingbaijam (2011) arrive at such different LLH scores and rankings for the same events (Anbazhagan et al., 2015, Table 5). It would be interesting to compare the results of LLHs computed using EMS inferred from digitized intensity maps to those computed using instrumental PGA for at least a few events since 1990. Nath and Thingbaijam (2011) take a step towards this by looking at the scatter in their mapping of PGA to EMS but it's not quite the same.

Many authors (Scherbaum et al., 2009; Nath and Thingbaijam, 2011; Delavaud et al., 2012; Anbazhagan et al., 2015) seem unduly interested in "ranking", i.e. constructing an ordered list of GMPEs. This is not a horse race. Scherbaum et al. (2009) suggests a way to turn an LLH score into a logic-tree weight and the formula does not require ranking. Furthermore, in constructing a logic tree one must include factors outside the performance-based scoring, for example an assessment of whether the set is "mutually exclusive and collectively exhaustive" (Bommer and Scherbaum, 2008). For me the question of ranking is just "noise" which obscures more important questions.

The mutual exclusivity requirement means, to me, that models should be omitted which are redundant in the sense of being too similar to other models in terms of the methodology of their construction, especially if that means they make similar predictions and have similar limitations as a result. For example the exclusion of models which have been superseded (Cotton et al., 2006) can be seen as an application of the requirement that models be mutually exclusive. Another example would be, for a GMPE logic tree intended for the Indian subcontinent, to omit a model such as Hwang and Huo (1997) in favour of Atkinson and Boore (2006) since both are based on stochastic simulation in Eastern North America.

The collective exhaustiveness requirement means, is trickier. It is this requirement which pushes hazard modellers to seek out and evaluate more and complementary types of models. Thus models with broad data support from other regions complement models with poor data support from the target region. Stochastic models supplement data-driven models. Models with different functional forms, distance or magnitude ranges can complement each other.

The process of developing a logic tree to assess epistemic uncertainty is thus a dialectical one. Mutual exclusivity and collective exhaustiveness comprise opposing forces which must be exerted alternately and in tandem.

[Now apply these principles to move forward from Nath and Thingbaijam (2012)!]

3 Hazard results

3.1 Verification

3.2 Sensitivity

4 Conclusions

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