Comment on "Total Probability Theorem Versus Shakeability: A Comparison between Two Seismic-Hazard Approaches Used in Central Asia" by D. Bindi and S. Parolai

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INTRODUCTION

The recent paper of D. Bindi and S. Parolai (Bindi and Parolai, 2015; hereafter referred to as BP15) considers discrepancies between variants of seismic-hazard estimates for central Asia. BP15 notes that for this territory the levels of hazard estimated in Ulomov *et al.* (1999; hereafter referred to as Ul99) during the construction of the Global Seismic Hazard Assessment Program (GSHAP) map shows a clear mismatch when compared with similar maps for neighboring areas: hazard estimates of Ul99 are systematically larger. Indeed, when Zhang *et al.* (1999) who were responsible for aggregating probabilistic seismic-hazard assessment (PSHA) maps for various territories of Asia into the single final map tried to combine the component maps, the only way to incorporate the results of Ul99 was to decrease its peak ground acceleration (PGA) values by ~30%. BP15 discusses the assumption that this problem could be caused by deficiencies of Riznichenko's hazard estimation procedure, believed to be used in the calculating of Ul99 for GSHAP. The first deficiency under discussion is the implicit assumption of Riznichenko (1965) that the level of shaking, when considered as a function of magnitude M and distance r, can be treated in deterministic style, with no allowance for the scatter of individual observations with respect to an assumed mean relationship. Originally, Riznichenko expressed
3 the level of shaking in terms of macroseismic intensity I, but most of the further discussion is applicable to cases when the level of shaking is expressed in any amplitude measure, further generically denoted A. The need to account for the scatter of I or A is evident (see e.g., Keilis-Borok *et al.*, 1973). BP15 comes to the conclusion that their initial guess is untrue: the possible assumption of zero scatter of the $I(M, r)$ relationship would result in a negative bias of hazard estimates, not in a positive one, as actually occurred. Another possible cause of the discussed discrepancy is a certain difference between hazard calculation procedures of Riznichenko and Cornell. Both these observations of BP15 seem to be relevant and need not be commented on.

The reason for the present communication is different. BP15 supposes that in the preparation of the segment of the GSHAP map that covers northern Eurasia (NEA), including central Asia, Riznichenko's methodology and/or algorithms were applied. The actual procedures were radically different. I am a member of the research team that produced the NEA segment of GSHAP, and my responsibility in the team was just to design the algorithms and engine code for hazard calculation. Thus, I am in the right position to tell the true story, and also to discuss other possibilities that might cause the mentioned misfit between the Ul99 result for NEA and similar results for adjacent regions.

The NEA hazard map of Ul99 was produced by the team of Valentin Ulomov (see Ul99 for details). In parallel with GSHAP project, Ulomov's team produced a similar hazard map, this one in terms of I, which was implemented as the General Seismic Zoning (GSZ, Russian abbreviation is OSR) map of Russia. This map, labeled OSR-97, was part of the Russian building code until 2015. The concept of the hazard calculation engine used in these projects is modeled after Gusev and Shumilina (1995). The employed model for single-event effect of a finite-earthquake source is modeled after Gusev and Shumilina (2000). The calculation engine proper, labeled PRB, was designed by Gusev and Pavlov (see Shumilina *et al.*, 2000), and Ul99 for short description of PRB methodology and algorithms. To clarify radical differences as well as similarities of the employed algorithms with those of Riznichenko (1995 and 4
later work), I consider it relevant to list here the key features of the PRB package.

COMPARING THE PROCEDURE USED IN ULOMOV ET AL. (1999) TO RIZNICHENKO'S APPROACH

1. *Macroseismic intensity as a hazard parameter.* Hazard level is specified by macroseismic intensity, not amplitude (like PGA). This particular mode was requested by Russian engineering authority (Gosstroi/Rosstroi) that eventually incorporates a PSHA/GSZ map into the building code. In Russia, it is an engineer who takes the design I value, IT,

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from the sheet of the official GSZ map for the appropriate return period T. He then converts it into the design PGA value by the formula $\log_{10}\mathrm{PGA}[g] = 0.301I - 3.107$ implemented in the text of the code, then converts PGA to pseudospectral acceleration (PSA), all this with certain account for local geology. When producing the NEA segment of GSHAP, the IT $(T = 475\ \mathrm{yr}) = I_{475}$ values were converted to PGA by assumedly more realistic formula (Ul99)

$$\log_{10}\mathrm{PGA}(\mathrm{m/s}^2) = 0.333I - 2.222. \qquad (1)$$

For each event of the simulated earthquake catalog used in hazard calculation (through a Monte Carlo scheme, see below), and for each receiver, the expected value of I is predicted assuming its sufficiently close correlation with the maximum value of Fourier spectrum of an accelerogram (FSA). Conceptually, using FSA as a predictor of I is similar to the use of Arias intensity. This approach permits the calculation of idealized intensity fields of a finite earthquake source on the basis of the theory of incoherently radiating source (Gusev, 1983; see also Singh *et al.*, 5 1989; Ohno *et al.*, 1992). The scatter of the empirical FSA–I regression is comparable or more tight than similar scatter with respect to PGA (Chernov, 1985). An important advantage of this approach is that damage accumulation that occurs in a structure during strong motion is implicitly accounted for. Thus, the PGA value converted from an I value picked from a hazard map represents an effective-load estimate. In this way, the differences are taken into account between damage potential of, for example, two earthquakes with equal PGA values, and different strong-motion durations. These may be equal to, say, 1 or 30 s, when generated by two earthquakes with magnitudes M 5.5 or 8.5, respectively. See Gusev (2002) for detailed justification of the use of I for hazard specification. Originally, Riznchenko also used I as the hazard parameter, later he preferred PSA.

2. *Hazard calculation by Monte Carlo.* When one tries to take into account finiteness of sources and many other details of real seismicity, the common way of calculation of return period, through numerical integration, becomes prohibitively entangled. Instead, somewhat slower Monte Carlo 6 method (Shapira, 1993) was used: a synthetic event catalog of a sufficient duration, like 10^6 yr, is generated; and shaking statistics at each receiver (i.e., at a node of the hazard calculation grid) is accumulated, to derive the Monte Carlo estimate of return period. Riznichenko, like Cornell, calculated return period using numerical integration.
3. *Finite sources.* To calculate, for a receiver, the value of I produced by an individual earthquake, its source is treated as a planar radiator represented as a grid of point subsources. In calculation for a particular receiver, the size of a cell of the subsource grid is automatically adjusted to be appreciably less than the hypocentral distance to the nearest subsource. Accounting for source finiteness is a critical issue when one wishes to simulate effects of M_w 8–9 sources (in Tien Shan, Sayan Mountains, or Kurile–Kamchatka), with their sizes up to 400 km. Riznichenko used point sources, with I field determined by some fixed $I(M, r)$ function.
4. *Accounting for the empirical scatter of effect.* To the mean value of I found by the above procedure, a random term was added that simulated real scatter, on the order of one I degree. Riznichenko assumed zero scatter; this deficiency is discussed in BP15.
5. *Basic magnitude scale.* Riznichenko used the Soviet *K-class* magnitude scale (Bormann *et al.*, 2013); this mode created 7 a number of problems for large earthquakes. For them, the *K-class* scale, like M_L, becomes poorly defined. In PRB, the M_w scale is used as the basic one.
6. *Nonlinear, "humpy"* log*N(M) relationships.* Magnitude statistics was permitted to deviate broadly from the Gutenberg–Richter (GR) law, thus permitting to emulate observed statistics of seismicity. On a noncumulative $N(M)$ plot, such deviations often form a hump just below the largest possible magnitude value for a region (Bath, 1976; Stirling 8 *et al.*, 1994). The presence of such a hump was found to 9 be a ubiquitous feature of all studied regions of NEA (Ul99). (This fact is often regarded as a manifestation of the characteristic-earthquake behavior.) One can measure the height of the hump by the ratio of the observed recurrence value for the top of the hump to its GR-predicted value, calculated, for the same magnitude, by extrapolation of the linear $\log N(M)$ trend from the small-to-moderate magnitude range up to the vicinity of the discussed peak. The actual height of the hump (see fig. 4 in Ul99) had the value 4–5 for central Asia; its maximum, of 5–6, was found for Kurile–Kamchatka. Riznichenko assumed GR statistics and used the GR formula for extrapolation of event rate from moderate-to-high magnitudes. By and large, some contribution of this factor to the discussed mismatch of PSHA estimates is possible, but can hardly be significant.
7. *Smoothing of seismicity map based on the assumption of uniform geology.* To get rid of bull's eyes on hazard maps, Gusev and Shumilina (1995) proposed to smear historical seismicity uniformly over as large linear (or areal) source region as possible, on the condition that this region can be treated as having geologically uniform character. This approach was followed in general (though maybe was somewhat overdone) when preparing the seismicity description database for the calculation of OSR-97 or NEA GSHAP maps. Riznichenko described seismicity by smoothed epicenter density (seismic activity); and his density estimates were often based on short-term (10–20 yr) small-to-moderate seismicity levels. Estimates of this kind are too sensitive to short-term space–time fluctuations of background seismicity. Also, geological information was mostly put aside. These are commonly recognized deficiencies of the original Riznichenko's approach; thus, the set of hazard maps produced by his team was mostly ignored in the construction of the practical GSZ map of USSR of 1978 (SR-78).

Table 1
475-yr Seismic Hazard in Intensity Scale I_{475} and Observed Intensities for Four Capital Cities of Central Asia

City	Hazard Estimate I_{475} after					Observed I Value
16	Bindi *et al.* (2012) emp.[*]	Bindi *et al.* (2012) calc.[†]	Ul99 orig[‡]	GSHAP Map, Rock[§]	Uh15 A/F/W[‖]	Maximum since 1870[#]
Almaty	9	9.4	9.2	*8.8*	7.7/6.2/7.3	9
Bishkek	8.5	8.4	8.8	*8.6*	7.2/6.3/7.3	7.5
Dushanbe	8	8.4	8.5	*8.3*	7.5/8.0/7.1	6.5[**]
Tashkent	8	8.1	8.3	*7.9*	6.9/6.5/6.8	7
Average	8.38	8.58	8.70	*8.40*	7.32/6.75/7.12	–

All values refer to the medium ground, except when the opposite is stated. GSHAP, Global Seismic Hazard Assessment Program; Ul99, Ulomov *et al.* (1999); Uh15, Ullah *et al.* (2015).

[*] Back-interpolated from the smoothed and partly extrapolated macroseismic recurrence data shown as black dots in figure 6 of Bindi *et al.* (2012); these are based on Nurmagambetov *et al.* (1999).

[†] Back-interpolated from the results of hazard calculations shown as gray dots in figure 6 of Bindi *et al.* (2012).

[‡] Based on the PGA_{475} value as picked from the original map of Ul99 for NEA; for conversion to I_{475} see text. 17

[§] Similar value based on PGA_{475} value as picked from the Asia or global GSHAP map; this column is the only one that refers to rock ground; numbers are italicized.

[‖] Calculated by Uh15, picked from their figure 19; three methods were used, coded here as A/F/W.

[#] Observed maximum intensity values since 1870, picked, for each city, as the height of the corresponding tallest red stem in figure 3 of Bindi *et al.* (2012).

[**] As given in table 1 of Negmatullaev *et al.* (1999).

The above list shows that the differences between the Gusev and Shumilina (1995) methodology and PRB program, on one side, and Riznichenko's methodology and algorithms, on another side, are in fact dramatic. It is also seen that there is no evident cause for a systematic overestimation of hazard levels in NEA. It should be noted however that the applicability of item (1) to central Asia is no more than a reasonable guess.

BP15 found some deficiencies in the procedure described by Riznichenko (1965), and associated the shakeability term coined in this article with this particular procedure. In my opinion, the real achievement of Riznichenko is of a conceptual kind. He was the first to introduce a new function of a location, which he labeled "shakeability," and proposed to calculate it from parameters of seismicity surrounding this location. Shakeability means local shaking rate (identical to "MARE" of BP15), and it plays the key role in the description of seismic hazard. Indeed, let shakeability (or MARE) be $B(A|x) = 1/T$, in which T is the return period for exceedance events $A' > A$ at location x. Then, seismic hazard A_T for the fixed return period T is merely the value of inverse function $A_T = B^{-1}(T|x)$. In short, seismic hazard is the inverse function with respect to shakeability. The concept of shakeability is much more important than a particular algorithm for its calculation, which may have imperfections.

THE PROBABLE CAUSE OF MISFIT FOUND BY ZHANG ET AL. (1999)

It seems relevant to consider other possible causes of misfit between the values of A_{475} on the map of Ul99 and on the adjacent maps. One cause may be long-term seismicity fluctuations. This factor is well known but it is not tamed until now. This question is relevant for central Asia in particular, where short powerful bursts of high-magnitude seismicity were observed for the last 150 years (Almaty area, 1870–1912; Kyzyl-Kum, 1976–1984). Different approaches to incorporation of such data into the Procrustean bed of Poisson process is quite possibly a source of error when comparing the results of two methodologies, applied to the same territory or to two adjacent territories.

However, there is another, trivial, but, in my opinion, most probable possibility for the misfit in question, irrelevant to any methodology of PSHA calculation. Any PSHA map is, normally, tied to a particular ground type, like B/C boundary or engineering rock. Originally, the GSHAP project was aimed at estimating A_{475} on rock. The calculations of Ul99 were always tied to medium ground. No trace of required ground correction/reduction can either be found in Ul99 or in Zhang *et al.* (1999). What is the value of the required adjustment? The Soviet/Russian tradition is to subtract one degree of I when passing from medium to rock ground; taken literally, and using item (1), this would reduce PGA estimates by $10^{0.33} = 2.15$ times. This approach is, however, hardly adequate when working with PGA: various sources predict much smaller reduction, if any. For the case in question, from medium to rock ground, common values of reduction factors are in the 1.25–1.6 range. Therefore, the empirical reduction factor of ≈ 1.4 used by Zhang *et al.* (1999) looks completely adequate.

This viewpoint is supported by similar discrepancy between variants of a PSHA map for Caucasus. As a test study within the GSHAP project, a number of such maps were created by various research groups by various methodologies (Balassanian *et al.*, 1999). A comparative analysis of these maps is highly relevant when the stability of PSHA estimates is discussed. From the map produced by Balassanian's own team using the standard SeisRisk III software and ground-motion **10** prediction equation from WUSA, one can pick $\mathrm{PGA}_{475} \approx 3.5\ \mathrm{m{\cdot}s^{-2}}$ for Yerevan and $\approx 2.5\ \mathrm{m{\cdot}s^{-2}}$ for Tbilisi; whereas the map from Ulomov's team gives, correspondingly, $I_{475} = 9$ and ≈ 8.5 that reduces to $\mathrm{PGA}_{475} = 6\ \mathrm{m{\cdot}s^{-2}}$ and $\approx 4\ \mathrm{m{\cdot}s^{-2}}$ using equation (1). Also, the results of the Molchan– **11** Keilis-Borok team, also given in (Balssanyan *et al.*, 1999), show approximately the same values $I_{475} = 9$ and ≈ 8.5, despite using the approach completely differently from that of Ulomov's team. The medium to rock ground-motion conversion seemingly was not performed in both cases and resulted in evident misfit with the results of Balassanian's team. If ground corrections were applied, the results of all three discussed studies would quite agree.

Until other possibilities are investigated, I believe that the possible lack of conversion of the Ul99 map from medium to rock ground is the most probable source of the discrepancy found by Zhang *et al.* (1999) and discussed by BP15. Taking this a probable fact in consideration, one can believe that after ground-type correction the NEA hazard map of Ul99 could be stitched together with adjacent maps without any border problem, demonstrating, in essence, the general consistency of PSHA approach for different areas of Asia.

COMPARISON WITH HISTORICAL DATA

BP15 claim that in addition to the discussed misfit found by Zhang *et al.* (1999), "recent PSHA performed in terms of macroseismic intensity (e.g., Bindi *et al.*, 2012) also observed such overestimation." Differences between two calculated hazard maps (as well as coincidence) do not mean much. To be really sound, hazard calculations must fit observed shaking rates (where possible). One can base on empirical recurrence of I values for four capitals of central Asia as cited in Bindi *et al.* (2012, their fig. 6, black dots). Back interpolation of these produces the estimates of I_{475} shown in column 1 of Table 1. In column 2, the results of hazard calculations of Bindi *et al.* (2012) are also given, taken from same figure 6 (gray dots). The differences between columns 1 and 2 are small: column 2 is $\approx 0.2I$ degree above column 1 on the average. This means that in this hazard study, the calculated hazard values approximately matched the observed ones.

Also, in column 3 the I_{475} values are given recovered from the PGA_{475} values shown on the NEA hazard map of Ul99 (the original one). For the adequate PGA_{475} to I_{475} conversion needed for this recovery, the inverse function with respect to equation (1) was used. One can see that I_{475} values of Ul99 are also comparable with observations: Column 3 is $0.3I$ degree above column 1 on the average. The difference is hardly significant. The above-cited statement of BP15 about overestimation of hazard by Ul99 is scarcely supported by this comparison. One can also notice that the differences between the results of two hazard studies are also quite minor on the average.

In column 4, I values are given recovered from PGA_{475} presented on the final GSHAP map for Asia (Zhang *et al.*, 1999), obtained using the above-described conversion procedure. The latter map is the adjusted version of the Ul99 one (represented by column 3), claimed by Zhang *et al.* (1999) as downscaled by about 30%. As seen from the comparison of columns 3 and 4, the converted Asia (and global) GSHAP I_{475} values are somewhat below those on the original Ul99 map, as expected; the average difference is about $0.3I$ degrees. It should be mentioned that the maps in question are not sufficiently detailed, and the converted I_{475} values may be somewhat inaccurate, but hardly more than by 0.2°–0.3°.

The average difference of $0.3I$ degrees between columns 1 and 3 can be translated into ≈25% larger PGA amplitudes predicted by Ul99 as compared to observations. This difference would be preserved if the data of both columns were reduced to rock ground. This possible bias is within the real accuracy of entire GSHAP calculations. It is not significant statistically; in my opinion, it can be treated as marginal even if real. At any rate, it should not be automatically propagated to entire NEA part of the GSHAP map until checkups over larger territories are done.

BP15 cite Ullah *et al.* (2015, hereafter referred to as Uh15) as another study that allegedly also revealed overestimation of hazard in Ul99. In this case, there is indeed a gross disagreement between predictions of I_{475} produced by Ul99 or Bindi *et al.* (2012), on one side (columns 2 and 3), and by each of the three techniques used in Uh15, given in column 5. Taking average over three techniques one sees that column 5 gives estimates 1.4–$1.6I$ degree below those of columns 2 or 3. Of course, this fact indicates a real difference, equivalent to about three times in terms of PGA_{475}; but it does not indicate an overestimation of real hazard by Ul99. The comparison with macroseismic parameters in column 1 shows that the estimates of Ul99 or Bindi *et al.* (2012) fit these parameters considerably well; whereas those of Uh15 are clearly below it. However, as follows from Nurmagambetov *et al.* (1999), to derive the numbers presented in column 1, not fully reliable procedures were employed that included smoothing, and most doubtful, extrapolation in some cases, of the observed intensity recurrence data. More certain conclusions can be derived from the comparison of 475-yr hazard estimates directly against maximum known intensities. With this aim, I cite in column 6 these intensities at the same four locations in question during the last 150 yr. In three cases out of four, the actual maximum shaking level during less than one-third of the return period (150 yr against 475 yr) exceeded each of the three variants of I_{475} values calculated by Uh15. To summarize, the comparison with observations shows that the hazard estimates of Ul99 are, by and large, realistic, whereas those of Uh15 seem to underestimate real hazard. Of course, the entire analysis carried out here is no more than exploratory as it is based on only four locations.

CONCLUSIONS

1. Based on limited data for four locations at state capitals, the hazard map of Ul99 predicts, for medium ground, realistic or maybe marginally exaggerated hazard estimates for central Asia, and probably for NEA as a whole, in terms of PGA$_{475}$ or I_{475}. The exaggeration, if real, may be on the order of 25% in terms of PGA$_{475}$. This is supported by direct comparison of predicted I_{475} with macroseismic statistics. Both assertions of BP15: that Ul99 hazard estimates are substantially exaggerated, and that this alleged fact is related to the use of Riznichenko (1965) procedures seem to be incorrect. However, the Ul99 hazard map does exaggerate hazard if applied to the case of rock ground.
2. Based on the same limited evidence, the GSHAP hazard map provides realistic or maybe marginally (+25%) exaggerated hazard estimates for central Asia, and probably for NEA as a whole, in terms of effective PGA$_{475}$ for rock ground. The discrepancy between the Ul99 hazard map and maps compiled independently for adjacent regions, which was found and amended by Zhang *et al.* (1999), was probably caused by the missed conversion of the Ul99 map from the medium-ground reference to the rock-ground reference.

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