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

by D. Bindi and S. Parolai

INTRODUCTION

The authors would like to thank Gusev (2016; hereafter referred to as Gu16) for his interest in our work (Bindi and Parolai, 2015; hereafter referred to as BP15) and for providing additional information about the methodology followed to prepare the hazard map for northern Eurasia included in the Global Seismic Hazard Assessment Program (GSHAP) model (Ulomov and the GSHAP region 7 Working Group, 1999, hereafter referred to as Ul99; Zhang *et al.*, 1999).

The main interest of BP15 was to compare two probabilistic seismic-hazard analysis approaches, namely the Riznichenko (1965) and the Cornell (1968, 1971) methods, which differ in the way they account for the aleatoric uncertainty. Although Gu16 agrees with the main conclusions of BP15 ("Another possible cause of the discussed discrepancy is certain difference between hazard calculation procedures of Riznichenko and Cornell. Both these observations of BP15 seem to be relevant, and need not be commented."), he raises some criticisms against the discussions developed in BP15 regarding the GSHAP overestimation of the hazard level in central Asia ("Both assertments of BP15: that Ul99 hazard estimates are substantially exaggerated, and that this alleged fact is related to the use of Riznichenko (1965) procedures are incorrect."). Here, it is worth remembering that, first, the overestimation of the Ul99 results is not an original statement of BP15, but, as also mentioned by Gu16, it was originally observed by Zhang *et al.* (1999) when compiling the GSHAP map, and, second, the Riznichenko approach followed in preparing the hazard map for northern Eurasia was a working hypothesis tested in BP15, not an absolute statement. Indeed, this hypothesis was inspired directly by Ul99 ("We calculate seismic hazard using an algorithm and software code designed by Gusev, Pavlov and Shumilina, following the principles of Riznichenko (1965) and Cornell (1968).").

Considering the main outcome of their study, BP15 concluded that it was not plausible to assume that the Riznichenko approach was the methodology used in Ul99 or, at least, the usage of the Riznichenko approach was not the reason for the overestimation ("We conclude that the overestimation of the hazard for some of the former Soviet countries observed in previous studies (e.g., Zhang *et al.*, 1999) cannot be related to the implementation of the shakeability concept."). We think that, in light of these clarifications, the criticisms of Gu16 are not justified. Indeed, the author of Gu16, who was also the main developer of the methodology applied in Ul99, confirmed the conclusions of BP15 about the application of the Riznichenko approach in Ul99.

Although what was written above would permit us to conclude our comments here, we would, however, like to take the opportunity to discuss other general aspects raised in Gu16, being sure that this would help in better understanding the discrepancies between the models obtained in the past when compared with the most recent ones.

In Gu16, an alternative hypothesis for the overestimation of the Ul99 model is formulated. In particular, it is suggested that the different way used to account for the site amplification effects in Ul99 with respect to the surrounding areas (Zhang *et al.*, 1999) could be the reason for the observed overestimation. Please note that we are aware that it is not, strictly speaking, correct to discuss overestimation, but for the sake of simplicity we use this term to indicate that the model was predicting hazard higher than the other ones. As also discussed in Gu16, the Ul99 model was computed for macroseismic intensity and later converted to peak ground acceleration (PGA) but without considering the large uncertainties affecting such conversion relationships. This is clear from the procedure suggested in the Russian normative indicated by GU16, where a conversion from macroseismic intensity to PGA and in turn to spectral acceleration is proposed. Although the GSHAP model was prepared for rock, Gu16 stated that the Ul99 map should be considered for medium ground conditions, and the common practice in the Soviet Union was to decrease

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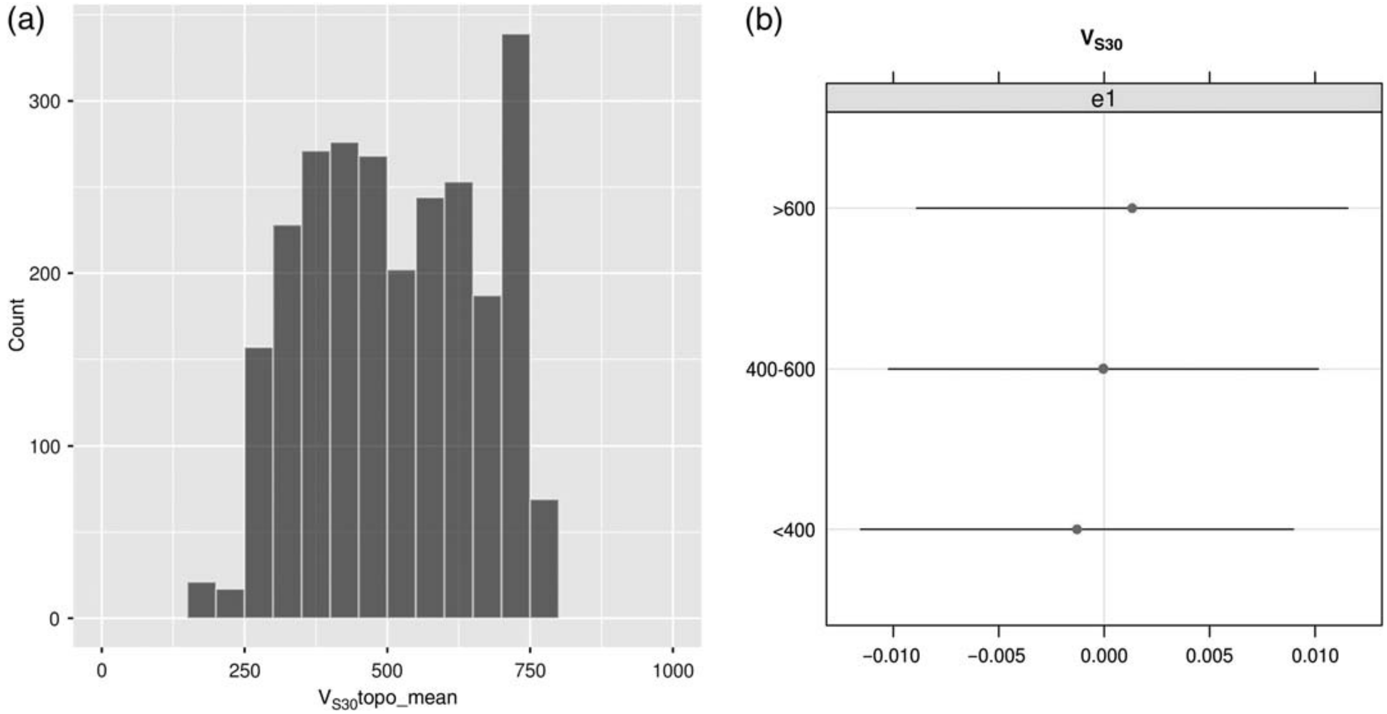

▲ **Figure 1.** (a) Distribution of the V_{S30} values (in m/s) derived from the topographic grading for intensity assignments in central Asia. (b) Random effects ρ_k depending on V_{S30} intervals (see equation 1).

the values by one intensity unit when moving from medium ground to rock. Although this procedure is (1) not based on clear definitions of site conditions and (2) the intensity is not related to an instrumental and local measurement of the ground shaking to verify whether this procedure is appropriate at least as a first approximation, we investigate the role of site effects in the intensity assignments available for central Asia.

In general, we can discuss the impact of site effects on macroseismic intensity from two different points of view. The first one is the relation between the variation of ground shaking due to site effects and the effect on the macroseismic intensity. In fact, because the site effects related to the surficial geology can amplify the ground motion for specific frequency ranges and extend their duration, they can also have an impact on the spatial distribution of the intensities, for example, at the urban spatial scale. In central Asia, Bindi, Mayfield, *et al.* (2011) found that the amplification functions for Bishkek (the capital of Kyrgyzstan) obtained through the standard spectral ratio techniques lead to macroseismic increments as high as two degrees when included in a scenario study prepared for the Issyk-Ata fault. The amplification levels and the spatial variability of the site effects observed in Bishkek (e.g., Parolai *et al.*, 2010; Ullah *et al.*, 2012) are indeed a common feature for most capitals of central Asia (e.g., Pilz *et al.*, 2013, 2015), and the site effects deserve special attention when developing strategies for risk mitigation in this region (e.g., Pittore *et al.*, 2014). To account for the influence of the local geology on the intensity distribution, in the former Soviet Union the so-called seismic microzoning (SMZ; e.g., Nurmanganbetov *et al.*, 1999) was developed, in which the local site conditions (including topography and groundwater level) were spatially categorized to assign intensity increments for normative purposes.

The second point of view is related to the identification of a possible imprint left by the site effects in the macroseismic surveys data compiled after an earthquake. This task is certainly not straightforward because the intensity assignments are not based on ground-motion measurements (i.e., they are not instrumental information but are based on descriptive assessments), they are not pointwise (the observed effects are averaged over areas, often one assignment per village is provided), they employ many scales (e.g., Medvedev–Sponheuer–Karnik [MSK], modified Mercalli intensity [MMI]), and the assignments are not systematically corrected for possible bias due to the effects of vulnerability (see, e.g., the requirements of the European Macroseismic Scale 1998 [EMS-98 scale]). This difficulty is very well exemplified by the intensity prediction equations (IPEs) developed by Allen *et al.* (2012) for active crustal regions. In that study, the authors attempted to develop an IPE model that also included site effects, estimated by using the topographic gradient as a proxy for the site effects. Although a discussion about the suitability of the topographic gradient as proxy for the site effects is beyond the aim of the present work, the effort of Allen and coauthors resulted in no significant (statistically speaking) improvement in the median residuals computed for the model including the topographic gradient with respect to the original one that did not include it (see their fig. 12). Considering the statement of Gu16 about the role of site effects, we also investigated the influence of site effects in deriving a ground-motion prediction equation specifically for central Asia. We started with the intensity assignments for 66 earthquakes considered by Bindi, Parolai, *et al.* (2011) to derive an IPE for central Asia without including any specific parameter in the regression that accounts for site effects. We used a random-effect approach (Stafford, 2014; Bates *et al.*, 2015; Kotha *et al.*, 2016) in which the site effects are included as a random effect on the offset. In particular, the site effects are described as categories based on V_{S30} (i.e., V_{S30} intervals), in which V_{S30} at each location of the assignment of macroseismic intensity is estimated from the topographic gradient (M. Pittore, personal comm., 2016) following Wald and Allen (2007). Again, discussions about the suitability of V_{S30} for describing site effects are well beyond the aims of the present work; nevertheless, we believe it can provide first-order indications of the influence of the shallow geology on macroseismic intensities. For the parametric model describing the median prediction, we followed Ullah *et al.* (2015) as described in the equation

$$I = e_1 + e_2 M + e_3 \log \sqrt{\frac{R^2}{h^2} + 1} + e_4(\sqrt{R^2 + h^2} - h) + e_5 \log(h) + \varepsilon + \rho_k, \quad (1)$$

in which M is the magnitude (considering the M_{LH} scale, see Bindi, Parolai, *et al.*, 2011), R is the epicentral distance, h is the depth, ε is the residual distribution, ρ_k the random effects on the offset parameter e_1, with $k = 1, \dots, N$, in which N is the number of the considered V_{S30} categories. The random effects ρ_k describe the systematic deviations with respect to the median prediction of each group of observations relevant to any considered V_{S30} class. The distribution of the V_{S30} values estimated from the topographic gradient is shown in Figure 1a. The V_{S30} values mainly span the interval 250–750 m/s, covering classes B and partially C of the Eurocode 8 (classes C and D of National Earthquake Hazards Reduction Program [NEHRP]), with an almost uniform distribution. Although we calibrated model (1) by performing regressions with different categorizations of V_{S30}, all the results showed that the random effects ρ_k are not significantly different from zero. Figure 1b shows this for the case $k = 3$ (i.e., $V_{S30} < 400$; $400 \le V_{S30} < 600$; $V_{S30} \ge 600$ m/s), in which all the three random effects are not significantly different from zero, and the standard deviation of $\rho (= 0.005)$ is negligible with respect to the standard deviation of $\varepsilon (= 0.75)$.

It is worth noting that because the distribution of velocities shown in Figure 1 do not sample class B of NERHP or class A of Eurocode8, we cannot discuss the impact of site effects for higher-velocity site classes. However, because the GSHAP map was compiled for generic (or engineering) rock conditions, corresponding to the boundary between B and C classes of NEHRP (i.e., $V_{S30} = 760$ m/s), the absence of any significant correlation between macroseismic intensities and site classes for low velocities (where the larger site amplifications are expected to occur) does not support the expectation of intensity increments as large as one degree between the generic soil condition and the generic rock, as mentioned by Gu16. Although a deeper analysis could be performed to assess the impact of site effects on intensity assignments, we think that the results of our regression analysis suggest that the hypothesis of Gu16 relating the overestimation of Ul99 to the site condition is not sound.

CONCLUSIONS

In summary, we think that we have clarified that the conclusions of BS15 about the methodology followed in Ul99 are indeed consistent with the information provided by Gu16. On the other hand, we explained our different position about the role of site effects within the framework of an IPE. We think that the procedure followed to convert the Ul99 map from intensity to PGA makes it very difficult to understand the sources of the disagreement between the hazard levels observed by Zhang *et al.* (1999). Gu16 gave us the possibility to access information about the methodology followed to derive the hazard map for northern Eurasia. To discuss the methodology of Gu16 would move this article far from its original aim. In any case, we found the methodology interesting, in particular the approach of mixing physical simulation and empirical models that, although embedded in a different methodological framework, resembles parallel approaches developed in western countries (e.g., Boore, 2003, and the references cited therein). However, there are topics that would deserve a deeper discussion, for example, the usage of a spectral attenuation-like model directly in the time-domain simulation without a discussion of the duration issue, as well as of its physical suitability, or the lack of discussion about the uncertainties affecting the obtained ground-motion estimates (see Gusev and Shumilina, 2000), but, due to restriction of space and our reply to Gu16 not being the most appropriate avenue for these topics, we leave this for future discussions.

Finally, Gu16 proposed a comparison of the different hazard models. Although we refrain here from touching such a delicate issue that had been discussed in several works (e.g., Albarello and D'Amico, 2008; Beauval *et al.*, 2008; Mucciarelli *et al.*, 2008), we need to note that using observations collected over a short period of time to evaluate the hazard models (e.g., observed maximum intensity) could be misleading (e.g., Iervolino, 2013) and, in particular, lead to an incorrect understanding of the probabilistic seismic-hazard results. Moreover, for future studies, the reliability of the large intensities assigned in central Asia to some historical earthquakes should be re-evaluated, because, considering the adobe, rubble-stone, or masonry houses typical of the area, the MSK (and other scales) saturates at intensity >7, following the example of the study performed by Ambraseys and Bilham (2012) for the 1911 Sarez earthquake.

We now close our reply answering the comment about the usage of macroseismic intensity as a design parameter mentioned by Gu16. Following Luco and Cornell (2007), macroseismic intensity is not an engineering demand parameter because it is, by definition, the damage observed in a certain fraction of the total number of buildings at given locations as a result of one earthquake; thus, it is not applicable to a specific building type. Indeed, state-of-the-art earthquake engineering, as it is conveyed in codes, recognizes that structural design required for response is more efficiently related to ground-motion intensity with respect to macroseismic intensity, and all modern engineering demand-versus-intensity relationships are based on ground-motion intensity measure. This state-of-the-art approach is based on the concept that it is more effective to relate response to shaking at the structure site with respect to heterogeneous damage to a heterogeneous building stock over a wide area, as would be implied by the use of macroseismic intensity.

We hope, considering the impressive increase of the availability of strong-motion data, that this approach would, in the future, also be followed in countries for which, at the moment, the lack of strong-motion recordings seems to hamper the introduction into seismic normatives the use of ground-motion parameters directly.

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