Reply to "Comment on 'Influence of Focal Mechanism in Probabilistic Seismic Hazard Analysis' by Vincenzo Convertito and André Herrero," by F. O. Strasser, V. Montaldo, J. Douglas, and J. J. Bommer

by V. Convertito* and A. Herrero

We thank F. O. Strasser, V. Montaldo, J. Douglas, and J. J. Bommer for the interest they have shown in our article (Convertito and Herrero, 2004). Strasser *et al.* (2006) present a critical comment of our work arguing that the solution proposed by Bommer *et al.* (2003) is a better solution. Note that the authors are nearly the same in both article and comment, except for V. Montaldo. Because this brief article is a reply, we will focus on the arguments directly concerning our article.

The main objection supported by Strasser *et al.* (2006) is that the method we proposed is not appropriate to "style-of-faulting" correction. We completely agree with this assertion because it is simply not the scope of our article. We speak about "focal mechanism" intended as radiation pattern and nothing else. This point is clearly stated in the introduction of Convertito and Herrero (2004): "in this article we consider that the focal mechanism influence is only expressed by radiation pattern changes. In particular we do not consider any tectonic influence, stress drop variation or dynamic effects." The style-of-faulting parameter, even if its identity is blurred (e.g., Bommer *et al.*, 2003), is an empirical definition of a complex set of physical conditions including the tectonic regime, the medium behavior, rock mechanics, rupture dynamics, and so on. In our opinion, the style of faulting is simply too complex to be used directly in our approach. Because the scope of our article is to show how it is possible to insert inside the main equation of probabilistic seismic-hazard analysis (PSHA; e.g., Cornell, 1968), simple physical parameters of the seismic source, that is, how it is possible to integrate deterministic parameters inside a probabilistic approach, we have chosen a small target, limiting ourselves only to the radiation pattern. We believe that the same approach can be used to insert many other parameters of the seismic source inside PSHA by using only a theoretical approach such as the fault strike (which has already been shown by Convertito, 2004), the directivity and stress drop.

The second important argumentation is that a method based on regression (i.e., Bommer *et al.*, 2003) is better than the method we propose. Once again we agree with Strasser *et al.* (2006) and this is clearly stated in the conclusion of our article: "when an attenuation law, including a faulting style parameter, is available for a given region, the use of this attenuation law gives a more reliable estimate of the hazard than the one obtained using the corrective coefficient we propose in this article."

Reply to the Focal Mechanism Comment

Strasser *et al.* (2006) start their comment on this topic with the assertion that both methods are identical. This assertion is mainly unfounded, in our opinion, simply because our article is not on style of faulting. The misunderstanding comes from a problem of point of view. Strasser *et al.* (2006) have a phenomenological approach and we have a theoretical point of view. We do not want to reduce the style of faulting or focal mechanism to radiation pattern. We only state that the theory says that a double couple with a particular focal mechanism has a radiation pattern, that is, speaking about a theoretical radiation pattern (e.g., Aki and Richards, 1980, p. 115). And this radiation pattern effect can be included in a theoretical way inside the equation of PSHA. It is true that it does not take into account stress drop, directivity, and so on, and it is surely not the response to the whole problem of style of faulting. But it is not its scope, as it was stated in Convertito and Herrero (2004). If the theory, that is, the representation of the seismic source by a double couple, is valid, it is then possible to take into account only for this particular physical effect inside the probabilistic approach.

In fact, we believe that the key to improve PSHA is to introduce *a priori* knowledge, that is, to insert some deterministic information into the probabilistic approach. Only in this way will it be possible to reduce the standard deviation, especially within the attenuation law, and to refine the seismic-hazard map. A simple phenomenological approach, that is, based only on the regression approach, is limited because many parameters depend on each other. It is currently difficult to separate their influence, mainly because of

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the small databases. If the style-of-faulting parameter depends on the tectonic regime and local context, the number of regions where the actual databases are sufficiently complete to consider this parameter are few if any. In our opinion, the theoretical approach may be an alternative. The assertion made in the comment, "radiation effects are also difficult to decouple from dynamic effects such as directivity," illustrated the phenomenological way of thinking of its authors.

Moreover, we agree that the result of our figure 3 is at odds with the result shown in the figure 3 of Bommer *et al.* (2003), the reason being again that we are not talking about the same thing. The study of Bommer *et al.* (2003), due to their regression approach, is limited to the definition of a rough style of faulting; meanwhile we speak about the radiation pattern. The behavior of these two effects are surely not identical and even if the radiation pattern is a part of the style of faulting, we believe that a comparison between them is meaningless.

The second criticism made by Strasser *et al.* (2006) on this section is very interesting, because it deals with the assumptions we made to insert the corrective coefficient into the equation. A multiplicative corrective coefficient by definition must be equal to 1 in absence of *a priori* information. In our definition, no *a priori* information means that all mechanisms are possible. Thus, this is the minimal state of information. It is true that this specific point is a very strong assumption and may be considered as a drawback of the method. But we are convinced that no alternative is available. The only solution for a different normalization coefficient may be when, first, we know in advance all the radiation patterns of the earthquake used for the predictive equation and, second, they are representative of the seismic activity considered. We believe that such information is not easy to achieve (we are speaking about a good knowledge of the radiation pattern and not the style of faulting). But even if it is possible, in this case, there is no need to use the method we have proposed because it would imply a number of data sufficient to realize regression. Thus, to be able to include our approach in the PSHA, we decided to impose this very strong assumption. In a certain point of view, such type of assumption is necessary, whatever the method you use. If you use a method based on regression, you implicitly assume that your database is representative but you cannot be sure. This problem comes from the probability density function, which has to be normalized.

The third criticism is based on the comparison we made with empirical data. This comparison is aimed at verifying that the application of the corrective coefficient does not provide unrealistic estimate of the peak ground acceleration values. The use of the difference as indicator of goodness-of-fit is due to the limited number of data points ($N \leq 30$). In fact, if the sample dimension is less than 30, from a statistical point of view, it is not correct to assume a Gaussian distribution for the data and the use of a standard deviation is misleading.

We agree with the conclusion of Strasser *et al.* (2006) that the improvements of the estimate is more evident for normal faults than strike slip or reverse. This is a consequence of choosing an attenuation law (e.g., Sabetta and Pugliese, 1987) retrieved on a database that prevalently includes normal mechanisms. Thus, for mechanisms other than normal, the median estimate of the uncorrected peak ground motion is far from the real one and would require a more complicated correction formulation. However, as can be seen from panels e and f of the figures 4 through 7 from Convertito and Herrero (2004), even the attenuation laws retrieved from a larger and complete database in terms of fault mechanisms (e.g., Boore *et al.* 1997; Campbell, 1997) suffer from the same limitation in the estimation of near-fault peak ground motion.

Note that the effect of the theoretical corrective coefficient provides estimates similar to those provided by the attenuation laws that take into account for style of faulting explicitly. This observation suggests the importance of the radiation pattern effects inside the style-of-faulting effect.

PSHA Comment

Their first argument is the existence of a misfit between the result of a not-yet-available study and the application we have made. The scope of our application was only demonstrative. We are quite sure that the values we have fixed are not the right ones because it will require a very serious study on the repartition of the different types of mechanisms inside seismic zones. Moreover, the zonation we have used is only a part of the old official ZS4 zonation for Italy and does not correspond to the one used today, used also in the not-yet-published article. We then wonder how it is possible to compare both studies. In our opinion this argumentation is the perfect illustration of the misinterpretation of our article by Strasser *et al.* (2006). They speak about new building code and new hazard map of Italy. We speak about research and how to introduce radiation pattern into a PSHA equation.

It is also obvious that we have selected very particular distribution of focal mechanism to present a clear example. The effect of the PSHA variation due to the radiation pattern is certainly not controlled by the very narrow Gaussian distribution we have chosen but by a much more complicated function. However, the definition of the distribution shape of the focal mechanism in the different zones of the ZS4 was not the scope of our article.

Concerning the dependency between the parameters, it is more a criticism of the limitation of the classical PSHA method itself rather than a criticism of our approach. In fact, if the concept of seismogenic zone, intended as a region having uniform seismic potential (Reiter, 1990), is accepted, then earthquakes can occur anywhere within a zone with any focal mechanism. On the other hand, when information about the kinematics of the main seismogenic sources becomes available with the details claimed by Strasser *at al.* (2006), it will be unnecessary to define source zones and

better results will be obtained using a hazard analysis based on single faults. In our opinion, however, this degree of knowledge is not yet available everywhere, especially not for the Italian peninsula and especially not for Southern Apennines. Moreover, because of the theoretical formulation of the method we proposed, information about the most probable location of the main seismogenic sources, if available, can be included in the classical PSHA approach by simply modifying the formulation of the $f_{\delta\lambda}(\delta,\lambda)$ probability density function to take into account for further variable dependence. Thus, our method is very flexible and may be adapted to many input configurations.

The comment of Strasser *et al.* (2006) on the dependency of the dip and the rake of the fault on the magnitude may be easily included in our approach if the data are available and reliable.

Conclusion

We consider the whole comment of Strasser *et al.* (2005) the fruit of a deep misunderstanding of what we have suggested in Convertito and Herrero (2004). We agree with Strasser *et al.* (2006) that it is important to take into account the influence of different source effects in strong ground motion and hazard, but we disagree on its applicability. We believe that the application of a too much sophisticated definition of the hazard inside a generic hazard map, for instance, at a national scale, is not wise because the required knowledge is far too large to ensure completeness; the multiplication of parameters and the use of a logic tree prevents a good quantification of the errors (difficulty to guarantee a sufficient exploration of the tree and significant difficulty to obtain a deaggregation for all the parameters); it is impossible to use the result for detailed local studies, the reference map is still too complex; the lack of transparency, a checking of the computation from independent research groups being nearly impossible, thus generates an awkward monopoly status.

Refining the hazard map must be done carefully on a limited target. In Convertito and Herrero (2004), we attempted to show a simple solution to merge deterministic information into a probabilistic approach, taking care of the self-consistency of the approach to be able to use classical deaggregation techniques. This method will not refine the result for the whole style-of-faulting effect but only for the radiation pattern, which is an important component of this effect.

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