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Reply to "Comment on 'Estimating the Maximum Magnitude of Induced Earthquakes in the Groningen Gas Field, the Netherlands' by Charles Vlek"

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Key Points

- Bommer et al. (2024) is a discussion and does not replace the current M_{max} distribution for Groningen.
- Vlek (2024b) contains several serious technical weaknesses that undermine any useful contribution.
- Vlek (2024b) repeats several points already made in our paper.

Abstract

In Bommer et al. (2024), we presented a critical review of the process through which the current logic-tree for the maximum magnitude used in seismic hazard and risk calculations for induced earthquakes in Groningen was obtained. The intention of the paper was to initiate a discussion for which our premise is that the current maximum magnitude distribution may be excessively conservative, impacting both the quantitative risk assessment and the public and regulatory perception of risk. In his Comment, Vlek (2024b) misrepresents our paper and makes inferences that have no basis in our article. He also puts forward numerous ideas, many of which have no connection to our paper and others of which seem to reflect statements that we make, despite having recently published a discursive paper on this very topic. There are several serious technical weaknesses in the Comment, which we explain in this Reply to minimize the confusion that the Comment could create.

We feel obliged to respond to Vlek (2024b) since his comment may create confusion in several regards. For the reader to fully understand our response, it is necessary to place it in context by briefing explaining the context and the sequence of events that preceded Vlek's comment on our paper discussing the maximum magnitude, M_{max}, of induced earthquakes in Groningen.

In developing a seismic hazard and risk analysis model for Groningen it was necessary to assess the possible range of M_{max} for the earthquakes caused by gas extraction. In view of the large uncertainty and potential controversy surrounding this issue, an international Expert Panel was convened in 2016 to undertake the assessment of M_{max}. In order to inform the Expert Panel's assessment, a workshop was held in which presentations were made by resource experts regarding the characteristics of both the gas field and the induced seismicity. There were also several proponent expert presentations by Earth scientists working on the question of M_{max} in Groningen (Bommer and van Elk, 2017). Six years later, with abundant new data available, the Expert Panel was reconvened and a second workshop organized in June 2022, with the updated information being presented, as in 2016, by several resource experts. Once again, the workshop included proponent expert presentations by a number of Earth scientists (a few of whom had participated in the first workshop). Dr. Charles Vlek, emeritus professor of environmental psychology and decision theory, was also invited as a proponent expert in the light of his multiple public statements on M_{max} for Groningen, which were generally based on the statistics of the earthquake catalogue. Vlek (2018) proposed that M_{max} was on the order of M_L ~4.5 as well as commenting on the M_{max} logic-tree proposed by the Expert Panel in 2016. Vlek (2019a) discussed M_{max} in terms of the largest earthquake for which a single M_L ~4.5 event could be expected in a defined future period, noting that this estimate would be expected to drop to M_L ~4.0 in view of the reductions in gas production levels. Vlek (2019a) also discussed the 2016 M_{max} logic-tree. Vlek (2019b) proposed an M_{max} of M_L ~4.0, once again on the basis of the recurrence statistics of the observed seismicity.

In our paper (Bommer et al., 2024) we discussed issues related to the 2022 Expert Panel assessment of M_{max}. This discussion was motivated by the panel's own criticisms of the process that did not allow sufficient time for a thorough assessment of the available data and models. We also highlighted a divergence between the proponent expert models and that proposed by the Expert Panel, with the latter being significantly more conservative. In parallel, consideration has been given to potential conservatism in routine assessments of M_{max} for natural seismicity and the way this may have influenced M_{max} evaluations for induced earthquakes (Bommer and Verdon, 2024). Our two main observations were as follows: (1) there are several reasons why the triggering of tectonic earthquakes in Groningen, which would necessarily initiate at shallow depth and propagate downwards into the Carboniferous, is highly unlikely; and (2) the maximum faults ruptures of induced (anthropogenically driven) events, which will be confined to the Rotliegend sandstone reservoir, will be constrained by the reservoir thickness and limiting aspect ratios. These conclusions led us to propose that M_{max} should lie between M_L 3.6 (the largest event observed to date) and an upper limit that is very unlikely to exceed M_L 4.5. We consciously resisted, however, proposing a new M_{max} logic-tree (no weights were given) since we believe that this should be generated through a suitable multiple-expert assessment. The official M_{max} distribution being used in the seismic hazard and risk modeling for Groningen is that proposed in 2022 by the Expert Panel.

The proponent experts at the second workshop were invited, and indeed, encouraged to publish their models in the scientific literature, and several papers were produced (Boitz et al., 2024; Dempsey and Suckale, 2023; Muntendam-Bos and Grobbe, 2022; Zöller and Hainzl, 2023). Professor Vlek also published his model (Vlek, 2023) but the description of his own proposal for M_{max} constitutes a very small part of that article, which mainly focuses on presenting, narrating and discussing the 2016 and 2022 Expert Panel assessments. We find it surprising that Vlek has submitted a Comment on our article in which he presents several ideas that were not expressed in

Vlek (2023). Indeed, in many regards, Vlek (2024b) seems to be more of a vehicle to present additional and new opinions (some of which seem to be adopted from our own paper) rather than a comment directly on our paper. We do not believe that it would be worthwhile to address every point made by Vlek (2024b), but in the following we briefly demonstrate several technical deficiencies in the work, highlight some misunderstandings or misrepresentations of our article, and note where it seems that Vlek is simply repeating our observations as his own. Vlek (2024b) also discusses risk communication and community engagement issues, neither of which were part of the paper on which he is commenting, hence we ignore those elements in his comment.

As already noted, the intention of Bommer et al. (2024) was to initiate a discussion and we have not proposed an M_{max} distribution to replace the 2022 model proposed by the Expert Panel. Vlek (2024b) misleadingly attempts to dramatize the situation by implying that the Expert Panel assessment has been abandoned, which is not the case.

There are several aspects of Vlek's work on this topic that do not align with standard practice in seismology. For example, in all his publications, Vlek's estimates of M_{max} are always derived from recurrence relationships, with *b*-values determined through least squares regression, which is not valid for such cumulative data hence standard practice being to use maximum likelihood fitting (e.g., Weichert, 1980). The recurrence relationships are then extrapolated to a rate of one event in a selected forecasting period. That this is his understanding of M_{max}, which would be an expected rather than truncating maximum magnitude (see, for example, Bommer and Verdon, 2024), seems to be confirmed by challenging us to provide the recurrence rates for the M_{max} values we mention, even though the assessment of rates and maximum values can, and usually should, be completely independent. Physics seem not to enter into the analyses in Vlek's publications, but Vlek (2024b) then challenges the use of a frequentist approach by asking rhetorical questions regarding wagers

we might make with the regulator regarding future seismicity patterns. To engage in this debate, starting from such erroneous premises, would only generate more unhelpful confusion.

Vlek (2024b) goes on to declare that "*for practical risk communication the decisional meaning and function of 'logic tree'.....need further clarification – against the background of a logical, frequentist and/or degree of belief interpretation of probability*". While there is ongoing debate about the specific meaning and interpretation of logic-tree weights in the literature (e.g., Bommer, 2012 and references therein), the logic-tree concept is well understood and widely used within the seismic hazard and risk community, whereas Professor Vlek appears to have a very different view of what constitutes a logic tree (Figure 1). His own 'logic tree' for M_{max} includes several magnitudes smaller than the largest earthquake that has already occurred in the field.

We also have concerns regarding his Figure 2, which presents values of peak ground acceleration (PGA) and peak ground velocity (PGV) plotted against magnitude, through which a quadratic curve is fitted in the range of the Groningen data (M_L 1.8 to 3.6) and then extrapolated to estimate PGA and PGV values for earthquakes as large as magnitude 5. The plotted PGA and PGV values are from a table on the Nederland Rapid Raw Strong Motion Portal of the Dutch seismological service at KNMI (*Koninklijk Nederlands Meteorologisch Instituut*; https://rdsa.knmi.nl/rrsm/), which reports maximum PGA and PGV values for earthquakes of $M_L \geq 2$ since 2013, to which Vlek (2024b) added the M_L 3.6 Huizinge event of August 2012. Vlek (2024b) identifies 55 of the 58 listed earthquakes on the portal as being from Groningen, whereas zooming out to the full map display shows that six of the earthquakes are clearly not within the Groningen field. The freely available database of Groningen ground motions presented by Ntinalexis et al. (2023) includes a few earlier events, which expand the available database. Figure 2 shows the magnitude-distance distribution of combined data, from which it can be seen that these peak motions were recorded over a range of distances and not, as Vlek (2024b) states, "*in the vicinity of the earthquake*

hypocenter". The analysis by Vlek (2024b) completely ignores the effect of distance even though empirical models for PGV in Groningen show an amplitude decay of 60% within 4 km of the epicenter and of 90% within 10 km (Bommer et al., 2022). Consequently, the physical meaning of the predicted variable in the simple equations Vlek (2024b) derives from these data is not at all clear. These are not maximum ground motions, just the largest motion that happened to be recorded in each earthquake, which is highly dependent on the network configuration (and it should be noted that the highest amplitudes are not always recorded at the closest station, and the largest PGA and PGV values do not always belong to the same recording).

In addition to the three earthquakes Vlek (2024b) includes from outside Groningen, we note that the peak amplitudes reported by KNMI are obtained after standardized processing of the recordings with a bandpass filter between 0.05 and 40 Hz, whereas the records in the Ntinalexis et al. (2022) database were individually processed using procedures especially developed for this purpose (Edwards and Ntinalexis, 2021). The PGA and PGV values are therefore slightly different.

The extrapolation of the quadratic function that is then fitted through these maximum ground-motion values to estimate maximum (recorded) PGA and PGV values that might be generated by larger earthquakes, has absolutely no physical basis. Moreover, it ignores the potential for strongly nonlinear site response under higher shaking levels, given the low shear-wave velocities of the near-surface deposits in the region (Kruiver et al., 2017). Given that ground-motion models have been developed for Groningen that are applicable over a wide range of magnitudes (and capture the epistemic uncertainty associated with such extrapolations beyond the data) and also include nonlinear site response (Rodriguez-Marek et al., 2017; Bommer et al., 2022), we fail to see any value in these so-called sensitivity analyses.

The analysis of PGA and PGV described above constitutes what Vlek (2024) as a necessary sensitivity analysis to explore the consequences of the M_{max} distribution. This is justified in Vlek (2024b) by misrepresenting a statement from Bommer and Scherbaum (2008), which referred to using sensitivity analyses in order to avoid complicated logic-tree structures in which some nodes or branches exert little or no impact on the resulting hazard estimates (and hence unnecessarily increase the computational burden). This is quite different from allowing the hazard or risk consequences to influence the selection of M_{max} values and their associated weights; we firmly believe that estimates of the maximum magnitude should be made on the basis of the available seismological, geological and geomechanical data, without being influenced in any way by the hazard and risk implications of the resulting values. We also need to point out that in Bommer et al. (2024), we did perform sensitivity analyses (in our Fig. 8) to show the impact of the 2022 update of the 2016 M_{max} distribution on seismic risk estimates, but we limited the analyses to these two cases for the simple reason that in our discussion paper we did not make a definitive proposal for a new M_{max} distribution.

Vlek (2024) goes on to even speculate about the impact of a modified, and less conservative, M_{max} distribution on the need for building strengthening programs in Groningen, stating that "*such a high-magnitude projection of PGA and PGV may be utilized to estimate possible (further) building damage and fatality risk*" (without explaining how these deterministic ground motions would be used in risk analyses that could be compared with probabilistic Dutch safety norms). While the conclusion regarding a diminished need for seismic strengthening in the region may well be valid, we feel it is irresponsible to make such pronouncements in the absence of a firmly defined and agreed M_{max} distribution. Indeed, as Professor Vlek knows well, a comprehensive seismic risk assessment model has been developed for Groningen that facilitates robust examination of the impact of modifications such as a revised M_{max} logic-tree (e.g., van Elk et al., 2019 and references therein on the exposure and vulnerability models). The need for careful analyses prior to proposing

policy changes has not stopped Professor Vlek from making the pronouncements in the Dutch press that "*the earthquake threat has been greatly reduced and further home reinforcement hardly seems necessary*" (Vlek, 2024a; Google translation). In that opinion piece, Vlek cites our paper as the basis for these declarations, notwithstanding the fact that we did not propose a definitive new M_{max} distribution and much less did we make any statements regarding the implications for seismic risk estimation. If the M_{max} distribution is ever formally updated, then we would, of course, conduct new sensitivity analyses, similar to those in Figure 8 of Bommer et al. (2024), and only then could conclusions be drawn regarding the impact on the seismic risk in Groningen. It is unthinkable, however, that risk mitigation measures would be modified or abandoned on the basis of Figure 2 in Vlek (2024b).

Vlek (2024b) also opines on the application of the SSHAC (Senior Seismic Hazard Analysis Committee) procedures, which is surprising given that the Expert Panel (Coppersmith et al., 2022) and Bommer et al. (2024) discuss at some length both how the workshops intended to follow the SSHAC principles and how they were not achieved in practice. Moreover, chair of the Expert Panel was a member of the original Senior Seismic Hazard Committee and both he and the lead author of our paper were contributing author to the SSHAC guidelines (USNRC, 2012; 2018) and have played leading roles in many SSHAC projects for seismic hazard assessment both in evaluator/integrator and peer review roles. Vlek's (2024b) description of the participatory peer review in a SSHAC study is simply incorrect, and we are at a loss to understand his motivation in using a simple quote from a document by the regulatory body, SodM (*Staatstoezicht op de Mijnen* or State Division of Mines) to question the fact that the Dutch authorities did not approve the adoption of a SSHAC process for the full seismic hazard and risk assessment (see Bommer, 2022). Everything else stated on this topic simply echoes our paper.

In closing the main body of his comment, Vlek (2024b) poses a question to us about why there should be any interest in M_{max} "*if it would hardly affect seismic hazard and risk*"; since he does not substantiate the premise of this question (we certainly made no such assertion in our paper), it does not warrant a response. His concluding remarks are speculative and make numerous unsubstantiated assumptions about the future actions of stakeholders in Groningen, after once again misrepresenting our discussion paper as the new and official NAM policy with regards to M_{max}. His closing statement that the magnitude M_L 3.6 of the 2012 Huizinge earthquake might never be exceeded is a point made so clearly in our own paper that the reader must wonder what is the contribution of Vlek (2024b) if his punchline is simply to echo our own statements? Why was this point not made in Vlek (2023)?

We refrain from commenting on Vlek's proposal for how seismic risk in Groningen should be discussed and communicated, other than to note that we do not disagree at all with such broad participation in such discussions. However, it is vital that the quantitative risk assessments underpinning such discussions are robust and defensible, and these assessments must be made by appropriately qualified experts. As an environmental psychologist, Professor Vlek no doubt has much to offer to such discussions, but his insistence on applying simple statistics without physical considerations or constraints to contribute to quantitative seismic hazard and risk analyses can only cause confusion.

We believe that our decision to invite Professor Vlek to participate in the second M_{max} workshop was consistent with the SSHAC principle of considering all available data and models. Following the workshop, he published an overview of both the 2016 and 2022 workshops (despite not having been present at the first and only for part of the second) in Vlek (2023). Despite having had the opportunity therein to present his views, he is now presenting a long discussion with several ideas that were not included in Vlek (2023) or any of his earlier publications. Before publishing Bommer

et al. (2024), we circulated Figure 10 from that paper to all the proponent experts asking them to check that we were correctly representing their models, which we felt was important since this is the information that the Expert Panel would have received. All the proponents responded to confirm that we had correctly represented their M_{max} estimates, but Professor Vlek asked that we replace his single estimate with a weighted distribution for which there was absolutely no basis in his workshop presentation (and which also was surprisingly close to the range of values we indicated might be the outcome of a revised assessment).

Vlek (2018; 2019a, b) has expressed views on M_{max} for Groningen and, on this basis, he was invited to participate as a proponent in the M_{max} workshop. Subsequently, he has published more extensive commentary on this topic in Vlek (2023) and has added the extensive commentary in Vlek (2024b), which we have shown to include several inconsistencies and technical weaknesses. It is both remarkable and regrettable that enormous investment in data acquisition and risk modeling had very limited impact on the policy decisions regarding the future of the Groningen field (Bommer, 2022). The seriously flawed attempts of Vlek to contribute to the scientific discussion of the maximum magnitude of induced earthquakes and the impacts on seismic hazard and risk do nothing to address this unfortunate situation – and very possibly make it worse.

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Declaration of Competing Interests

Bommer and Zoback have acted as independent consultants for a variety of organizations including hydrocarbon operating companies, including Shell and NAM, and governmental organizations on issues pertaining to induced seismicity; van Elk is an employee of NAM. The views expressed herein

reflect the independent professional judgements of all three authors and neither NAM nor Shell has exerted any influence on the authors' conclusions.

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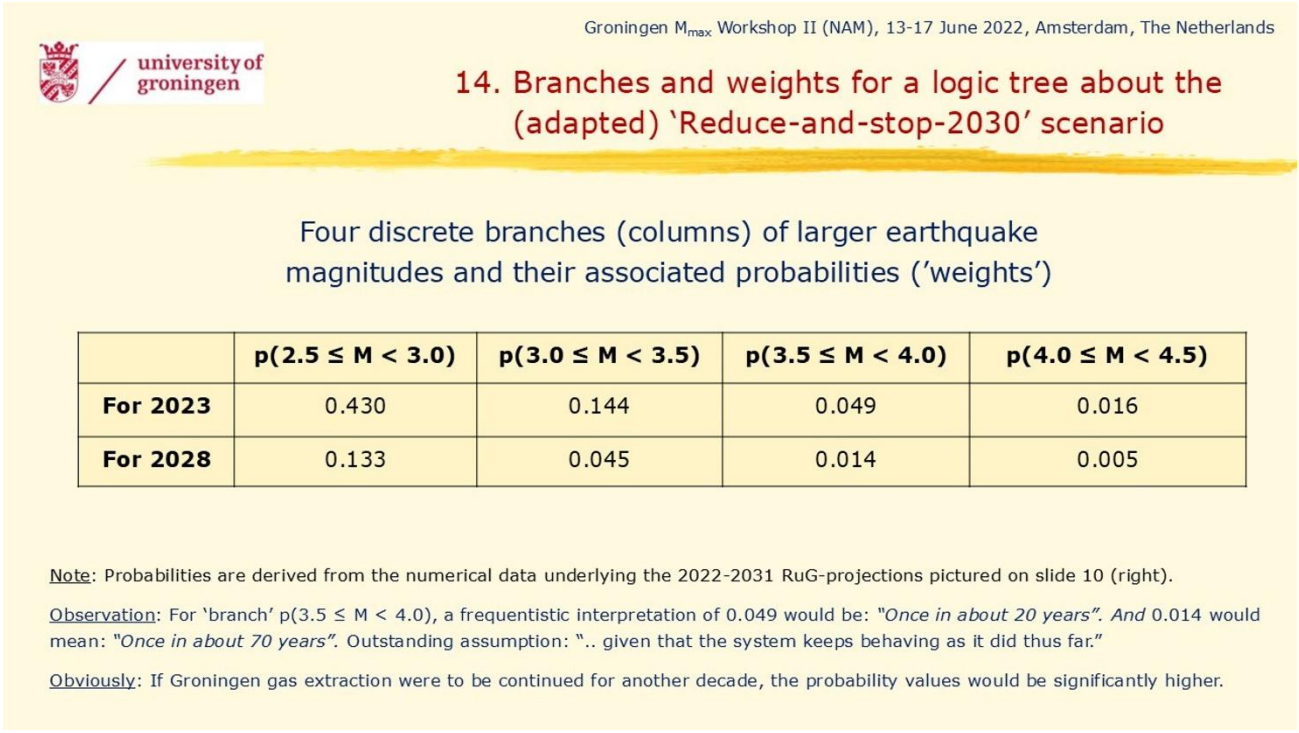
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	p(2.5 ≤ M < 3.0)	p(3.0 ≤ M < 3.5)	p(3.5 ≤ M < 4.0)	p(4.0 ≤ M < 4.5)
For 2023	0.430	0.144	0.049	0.016
For 2028	0.133	0.045	0.014	0.005

Figure 1. Slide from Professor Vlek's presentation at the 2022 Groningen M_{max} workshop, which purports to show logic-tree weights (NAM, 2022). If the different magnitude intervals represent the branches for where M_{max} will lie in each forecast period, the tabulated 'weights' should sum to unity, as an absolute minimum requirement.

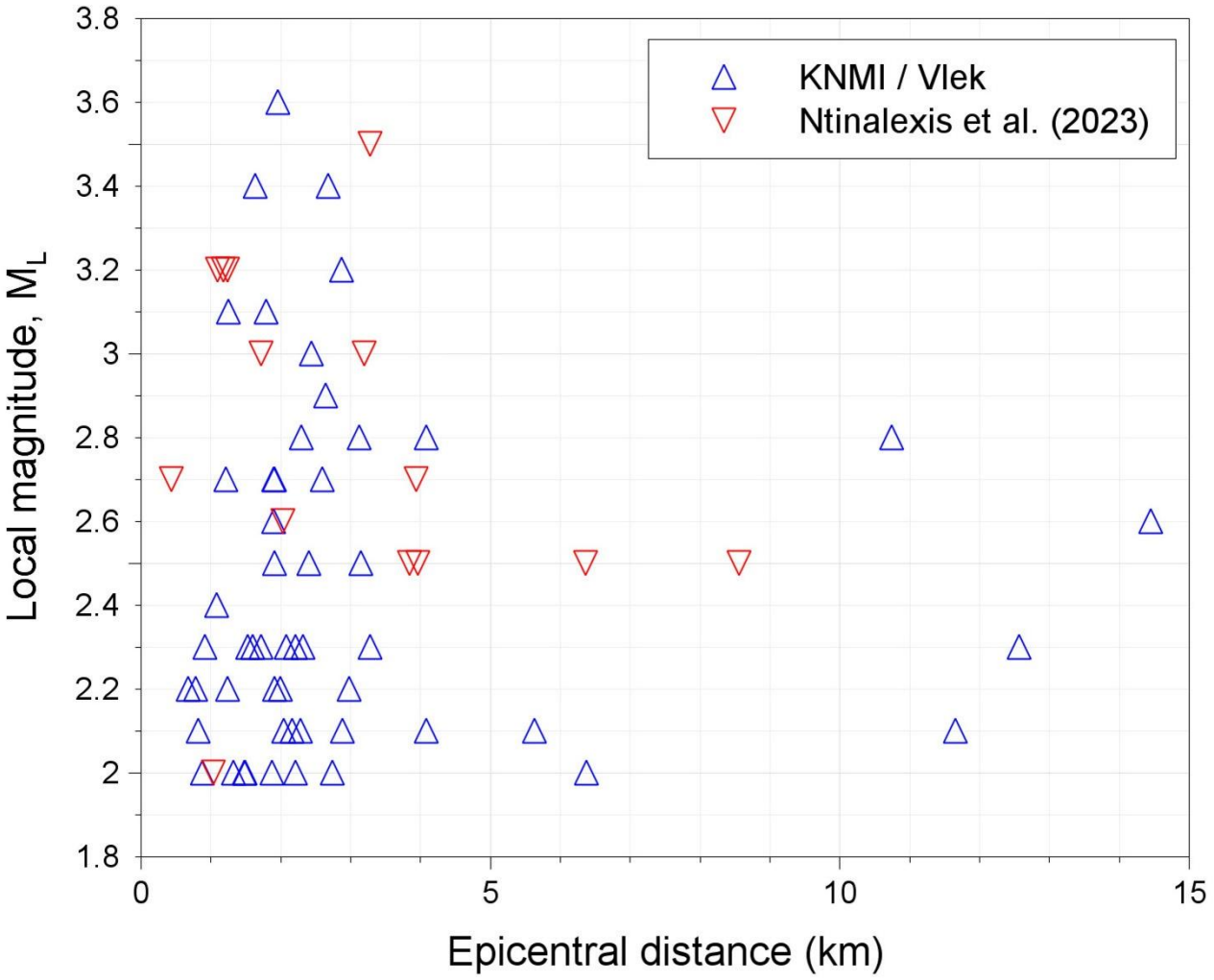


Figure 2. Magnitude-distance distribution of the maximum PGA data used by Vlek (2024b) and the additional events in the Ntinalexis et al. (2022) database, excluding from the KNMI database the events that occurred outside of the Groningen gas field. The distribution for maximum PGV is similar but not identical since the largest PGA and PGV values in many cases correspond to recordings at different stations.