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Comments by Jan Šílený on the PhD thesis "Limitations of first P-wave onset method of full seismic moment tensor inversion and impact on effective application in anthropogenic seismicity studies" by Anna Tymińska

The subject of the thesis by Anna Tymińska, MSc is highly topical. It is situated somewhere on the border between science and application, in particular between earthquake seismology and the seismology adapted to induced earthquake investigation. This aspect makes it very important for the practice. Of course, in the home institute of the applicant by far it is not an isolated effort. The Institute of Geophysics of the Polish Academy of Science is known for this streaming for decades. It recent years this tradition was even strengthened by initiation and enforcing of an activity towards investigation and mitigation of anthropogenic hazard. The widely based activity, now within a broad international cooperation, is roofed by the EPOS project. International team of researchers and IT specialists forms one of the interest groups of the EPOS, namely the Thematic Core Service Anthropogenic Hazard (TCS AH), and initiated numerous links to partners in industry. Thanks to this partnership, the flagship of the TCS AH, the research-educational platform EPISODES possesses a wealth of 45 datasets collected worldwide about seismicity induced by various types of anthropogenic activity, which are complemented by related technological data. As such, they represent invaluable data to research how the induced anthropogenic seismicity is driven. The Applicant herself benefits from the EPISODES by gaining precious data from four sites for the research about the topic of her PhD thesis.

I have read the thesis with a great interest, because the topic is very close my own field of research. The study is elaborated, the computations are performed by using the HybridMT package by Kwiatek et al 2016, doi.org/10.1785/0220150251). Undoubtedly there is a lot of "hidden" work in the waveform pre-processing, amplitude picking and group evaluation of clusters of events. Interesting and for practice useful results have been obtained about resolution of the moment tensors (MT), treated in various degrees of their constraining from full MT across deviatoric MT to a double couple (DC) relevant to a shear-slip source, in dependence of noise contamination, and geometry of the observation (quality of focal sphere coverage) expressed by varying the event depth. Realizing the crucial role of the focal sphere coverage quality, insightful tests were performed concerning complementing seismological data, which may be sparse in particular setups, by high-rate geodetic observations , HR GNSS. The output of the tests is the understanding of advantages and limits of the geodetic data contribution towards enhancing the quality of the MT retrieval. Last but not least, taking into account particular configurations of the networks monitoring the anthropogenic seismicity, analysis was performed elucidating the contribution of the intermediate part of the wavefield in addition to the traditionally processed far field term, and resulting modification of the mechanisms obtained. In particular cases, the effect may be surprising.

In the following sections there are the comments to individual points of the PhD thesis, sorted among major items which should be discussed during the defense of the dissertation, and minor points mentioned just to make the treatise clear and accurate for, e.g., a later usage. Within the quotation marks there are the sentences from the dissertation which are just discussed.

Major points in the comments about the study reviewed

Description of the results of the synthetic tests of noise bootstrap (p.23, Fig.9) is rather brief. The fault plane solution marked by the blue line seems to be the same for all the test sites and the source setups. Is it just a plot of the theoretical model or a plot of the bootstrap solutions, i.e. N solutions yielded by the individual jackknife trials, which are all the same within the thickness of the line? If the latter is relevant, it is puzzling that practically there is no effect of removing each single station apart from the nearest one for the LGCD configuration and full MT setup for the inverse task, where there is a substantial difference in the DC orientation. Anyway, the DC orientation is just one part of the solution in case of the full MT and deviatoric MT setups, what about the MT decomposition: was there somewhere a non-zero content of the non-DC part of the resolved MT? I guess that it would be very improbable, thus I assume the nodal lines and zones of compression displayed are relevant either to the theoretical model (the pure DC) or the DC part of the resolved MT. Also I am puzzled that there is no effect of removing, e.g., the station S10 in the Song Tranh site, the station just positioning the dilatation quadrant. (The station codes are seen very poorly in Fig.9, they are too small even in extreme zooming . In any case they are different from codes introduced for the stations in Fig,8.)

Despite a shortage on the description (see the Minor comments), Fig.10 is nicely instructive about the depth dependence of the focal sphere coverage. The essential information carried by the Figure is the station distribution related to individual setups, where the enormous role of the velocity model applied is nicely demonstrated. For instance, for Rudna mine and source depth 500m and 800m, all the stations apart from the closest one TRBC are projected along a circle, indicating the same take-off angle for all the ray paths. This is the feature of a head wave, the wave of the order one in the ray theory, traveling just below a slow/fast velocity interface (sin(take-off)=v_{above}/v_{below}). For the depth 500m it is the case of the velocity dscontinuity at or below this depth (the scale of the velocity profile in Fig.5 is not detailed enough for a precise location, unfortunately), and even greater discontinuity around the depth 800m. It is instructive to check the position of the projected closest station TRBC: for the depth 800m it is NW, which means the ray leaves the source downwards, while for 500m it is projected SE, indicating the ray going upwards from the focus. Thus, below 800m there should be a greater velocity gradient than at 500m, which seems to agree with the velocity profile in Fig.5. The rest of the stations however behave differently. All of them are projected to a circle, its radius being smaller for 500m than for 800m. Then, the low/high velocity contrast ruling the ray-tracing of the head waves should be greater at 500m than at 800m. The discrepancy should be resolved by inspecting the velocity profile in more detail than that provided in Fig.5, and checking the rays for the stations discussed. A similar effect can be seen for Bogdanka mine at 2km, evidently related to a large velocity discontinuity in this depth (Fig.5). From deep hypocenters the rays go upwards: the greater depth, the smaller take-off . Here the waves correspond to zero order rays. For Bogdanka and Song Tranh, stations are projected near the outer margin of the focal sphere, which indicates the rays are close to horizontal direction when leaving the source. It nicely fits to the velocity model in Bogdanka, where there is a steep increase in shallow part of the profile. This is however not the case of Song Tranh, where the model is homogeneous up to about 17km. Thus the rays should go straight towards the station, i.e. the take-off should be less than 90 degs. It is however true that the extent of the network is very large and even the straight-line rays will be sub-horizontal. When analyzing the situations relevant to individual cases in Fig.10, it would be necessary to see the rays.

In noise experiment in the LGCD a bad behavior of the inversion for the depth of 1km is mentioned (p.26) – both the crack and anticrack solutions appear at the same time. I guess it is a consequence of the complex velocity model, where there is a deep low velocity channel below 1km, which certainly makes the ray tracing and the Green's function evaluation sensitive a lot to particular source-station configuration. In this context a question arises about the confidence of such a distinct feature of the velocity profile – is the channel so deep indeed?, are its boundaries so sharp?, is it existing across the whole zone? etc. In other words, any feature of the velocity/attenuation model which is "singular" should be examined very carefully whether it is not artificial but genuine, otherwise it can yield complexities which are mere artifacts of the improper modeling. Another question is if the low velocity channel, which is deep but rather thin, is seen in the frequencies processed; to answer the question, a look at the waveforms would be helpful. Is the unwelcome behavior at the depth 1km expressed also in the RMS values, in particular, are they higher at 1km compared to the other depth levels assuming the same noise?

"Mechanisms comprising about 50% of double-couple component located at 5 km and 8 km seemed less sensitive to noise contamination." Apart from Bogdanka, it is seen (though in a less pronounced pattern) at Rudna. It is a mysterious feature, have you a hypothesis for that?

Based on the pattern of the resolved MT decomposition, I doubt about the theoretical mechanisms applied in the noise experiments. According to Fig.1, the theoretical source used to genera the synthetics is the combination of a DC and tensile crack (TC) with a varying content of these components from pure DC up to pure TC. In the experiments, two types of the DC are chosen, a normal faulting and a reverse faulting. Browsing results of the experiments with a low noise (where a good resolution of the theoretical mechanism can be anticipated), regardless of the site, in the depth levels yielding a fairly good coverage of the focal sphere (5km and 8km for Rudna and Bogdanka mines, more than 5km for the Vietnamese sites), it is the TC which is complementing the normal faulting DC, and an anticrack which is complementing the reverse faulting DC. My question is if my assumption is correct and if yes, what was the reason of selecting just these combinations.

It is an interesting feature of the noise experiments that for the depth 0.5km only in Rudna the crack/anticrack is well identified in the normal/reverse faulting setup. For this depth, just Rudna displays a very inconvenient station distribution along the focal sphere. In other sites, there is a complete ambiguity concerning the crack/anticrack resolution. The explanation is unknown, at least to me.

Intermediate field assumption, p.44 bottom, Fig.25 The mismatch of the calculated amplitudes with the observed ones for the July 2020 event is puzzling, for all stations the synthetics are too big. The Author argues with the fact, that "Additionally, the mechanism of this event is strongly non-shearing, meanwhile, theoretical amplitudes are calculated for the DC mechanism." OK, it may be the reason for part of the mismatch. It is the matter of fact that the simplification of the synthetics in considering the DC instead of the complete MT was not needed in principle, instead u^{IP} and u^{FP} from eqs.(8) and (9), respectively, the 4th and 2nd term from (4.29) in Aki &Richards (2002) could be used. Similar alarming mismatch however appears for some events from Song Tranh in Fig.26: while the match for event 2024-06-12 is reasonable, the synthetics for 2015-04-05 are everywhere too low, and for 2015-05-17 they are too high. I would expect that, as a result of fitting synthetic amplitudes to the data in the course of inversion, some of the data point will be matched better, some worse, but the synthetics will "in average" approach the data, it shouldn't happen that all are too low or too high, because if the RMS or another similar measure is used as the criterion of the match within the inverse procedure, for the cases just mentioned it does not go towards the minimum for sure. Here I either do not understand the setup of the inverse task properly, or I have to be suspicious about the proper functioning of the inverse procedure used. In L_2 norm (least squares), big data items are matched preferentially. This is just not the case for event 2019-01-29 and 2019-03-09. The default pattern may be affected by a selective weighting of individual stations, was this the case here?

HR-GNSS application (Chap.3.2 and p.52): spectral amplitudes are used instead of the 1st motion amplitude. This is however a different inverse task – "average" mechanism vs. the mechanism relevant to the very beginning of the rupturing. Moreover, adding the sign to the spectral amplitude may be a delicate choice indeed.

Minor comments about the study reviewed

Abstract, 1st paragraph: „Relevant to correct MT solution are azimuthal coverage and noise, but the distinct influence that factors on MT solution is undetermined." The sentence is rather obscure.

p.4, par.2 „Unfortunately, with a detailed model, location uncertainty will result in the possibility of large changes in modelling the seismic waveform." The message is expressed somewhat unluckily: The detailed model should yield a small location error, provided it represents a proper approximation of the real properties of the medium. Here the Author probably points the case when the model is detailed but incorrect.

p.4, par. "...(Stierle et al., 2014). Detailed synthetic tests for small events and local network conditions are necessary." Paper by Stirle et al. is OK as for the topic, but not much relevant due to the scale employed. We did such tests in a small scale for Soultz (Pageoph, vol.171, No.10, 2014, DOI 10.1007/s00024-013-0750-2).

p.6 „A simple MT inversion method is the first P-wave onset." I am not convinced this is entirely OK. P-wave onsets, i.e. first signs, are not informative enough to retrieve an MT unless the network is extremely dense (e.g., Hyashida et al., Non-double-couple micro-earthquakes...via...hyperdense seismic observations, GRL 2020, doi.org/10.1029/2019GL084841). Which particular approach have you in mind?

p.8 „If the force couple acting in opposite directions and the force's arms are shifted the moment of force appears." The sentence is not clear. The force couple itself yields a moment of force, which shifting is mentioned here? Next sentence is formulated unluckily as well („...the perpendicular force couple appears"); simply the couples have to be balanced.

p.8 „The double couple mechanism represents the shearing mechanism on a fault." The sentence in inexact a bit: this happens on a planar fault in an isotropic medium only.

p.8 „The shearing movement forces in earthquake source can cause other mechanisms if occurred simultaneously in different directions, such as volume changes or crack opening." The wording is obscure a bit, in fact the ISO component and a tensile crack need dipoles without moment. Next sentence („Regardless...") is formulated unluckily again, simply seismic waves are radiated and the source mechanism affects the radiation.

p.8, eq (1) There is a mess in the indexing at the end: $G_{ij}*M_{ij} \rightarrow G_{ij,k}*M_{jk}$

p.8 "..., a CLVD for uniaxial compression or tension,..." To be exact, this is not true: the CLVD is not such a simple force system, and it is not a force representation for the uniaxial compression/tension. Rather it is an artificial force construction for the purpose of the most widely used MT decomposition – a complement to DC to constitute the deviatoric part of the MT.

p.8 "Such decomposition is used because of its usefulness for physical interpretation..." That's true, however an important point is that the DC and CLVD involved possess the same tension/compressive axis, otherwise a nonsense decomposition can appear, see Julian et al., Rev Geophys 36:525–549. The source description using the tensile crack (Vavryčuk 2001) remains within the decomposition into ISO, DC and CLVD.

p.9 „Except for the information about the components,..." Unlucky wording: the components themselves define the MT, and the MT can be further processed towards determining its principle axes, to which the fault planes are related. Stress retrieval is the next level of the MT processing. Within the paragraph just discussed, the Author starts with an MT, however the sentence mentioning the perpendicular nodal planes deals already with a DC, and it is not indicated in explicit.

p.9 „For comparison of many mechanisms or quick mechanism components valuation,..." These plots are designed first of all to visualize the MT decomposition into the individual „elementary mechanisms", i.e. the isotropic part, the DC and the „rest" – the CLVD. The source plot in Fig.1 is commonly called the Hudson plot, Vavryčuk named his alternative to Hudson plot a diamond plot.

p.10 "In the time domain the G is nx6 matrix..." What is described here is the inversion of a priori picked amplitudes, where the time dependence of all the quantities involved is already stripped off. Keeping the dependence of time, G is $n*n_s$x6 matrix, with n denoting number of points within the seismogram, etc.

p.11 "There are relative moment tensor determination methods where the need for Green's function computation is removed. However, the reference mechanism is needed, ..." More precisely, the need for evaluation of complete Green's function is removed, a necessity for the ray-tracing (or, the determination of the take-off angles) remains. Of course it is much less demanding concerning the details of the velocity/attenuation model then. Also not all relative methods of the MT retrieval demand for a reference mechanism.

p.11 "The Bayesian approach for moment tensor inversion is also applied in some cases, however requires knowledge about input parameter errors and gives high uncertainty for non-DC components." The Bayesian approach is a higher level of the inversion task incorporating also errors in addition to the inverse problem solution itself. The amount of uncertainty of the resolution of individual MT parts depends of the setup of the particular inverse task. Non-DC components are often rather uncertain, but this feature is not universal.

p.12 "For all types of results six independent elements of moment tensor..." For a deviatoric and a DC solution all six MT elements are not independent.

p.13 "The percentage of isotropic, CLVD and DC components depends on a tensile angle as shown in Figure 3." Yes, primarily of the tensile angle (standardly called the slope angle), but also of the material properties expressed, e.g., in the ratio of Lamé constants λ/μ.

p.14 The reading would be more friendly if the factor *x* in eq.(4) were described at least shortly (why just one third of it appears there). From the eq.(4) it follows that the noise contamination (marked by the same percentage) is different for each amplitude/station: higher for bigger amplitudes and vice versa. The question may arise whether this definition is relevant to a reality: if there is a background noise within a zone/mine, it affects weak signal more than the strong ones. The definition of the noise contamination affects results of the synthetic experiments in 3.1 essentially, of course.

p.15 "The above equation describes the displacement caused by the double-couple source." The equation (5) is general, the specification of the mechanism is ruled by defining the term $R^{(N,I,F)}$ for the particular source.

p.15 "This research is based on a Haskell model with progressive slip along the fault..." The Haskell model assumes a propagation of the rupture along a finite-extent fault with a particular rupture speed, here the source remains to be a point, and only the concept of the ramp time function from the Haskell model is taken. It is a clever idea, as thanks to (7) the F and I terms of (5) can be treated jointly, and merely amplitudes instead of whole the waveforms can be processed. The notation should be uniform (Δt=T), i.e. the same symbol in (7) and in Fig.4 should be used.

p.23, Fig.9 The naming bootstrap/jackknife for the test performed should be unified. To my knowledge, jackknife is the test with removal of some of the data points, bootstrap is an experiment with removing some data and completing the data set by repeating others. Then, the tests performed here should be called jackknife experiments.

p.23 "Only removal of the TRBC station in Legnica-Głogów Copper District, the closest to the event, resulted in a reversed fault type for events shallower than 1 km." I don't see a reversed fault type in any of the experiments, only a change of the strike due to removal of the TRBC in the test at the depth 800m.

p.24, Fig.10 Similarly, in Fig.10 it is not described what is the fault plane solution depicted for individual sites and depths: the theoretical model generating the synthetic data, or a solution of the inverse task.

p.25, Fig.11c The regular pattern of the curve Kagan angle (named rotation angle in the thesis) vs. DC percentage obviously is a uniform decrease. This however happens for the depth levels 1km and 2km only. Shallow events reside in the part of the velocity profile with a complex pattern, thus it shouldn't be a surprise to observe a complicated shape of the curve. Deep events should however behave regularly, but they do not. The argument (p.26 top) "Deeper events exceeded station-event distance which results in instability of non-DC mechanisms" is not convincing because what is important is the station coverage along the focal sphere, and it remains good (Fig.10, Rudna mine, depth 5 and 8km). A more relevant explanation should be searched for.

p.29 Bogdanka site: At 0.5km, the failure in the resolution crack/anticrack, alias normal/reverse faulting, seems to be very similar to the LGCD site and the depth 1km. And again there is a low velocity channel below this depth level, though not so deep as at the LGCD. The poor focal sphere coverage for the depth 2km mentioned is evidently the consequence of the low/high velocity interface at 2km, originating the head waves as the first arrivals, which possess the same take-off angle. The stability tests are really fairly good for the other depth levels (Fig.11d). On the other hand, the resolution of the MT decomposition is very poor for both the normal and reverse theoretical source models (Figs.14 and 15). For the depth less that 1km the normal/reverse faulting type is not resolved at all (there is about the same amount of + and – linear vector dipole LVD. Moreover, at 0.5km the DC source is not identified at all!

"For deeper events instability occurs." This concerns the orientation only (Fig.11d), concerning the decomposition the instability is reduced just on the contrary – the DC/nonDC contents is resolved very well at 5km and 8km (Figs.14,15).

A note concerning the terminology: The Author defines instability as a difference in the orientation of the fault plane solution obtained from the unconstrained MT and the deviatoric MT. This is entirely OK for mechanisms which are prevailingly shear, those with a minority of the DC component may yield very unconfident nodal planes orientation. Here, rather than the fault planes, the T or P principal axes should be checked for the stability.

p.35 "The focal coverage changes smoothly with slight changes in the velocity model." The velocity model was fixed, to my understanding.

'Solutions (*in Lai Chau*) are more stable than in the case of Song Tranh (Figure 11c).' Comparing Fig.11b and 11a (not 11c) may support the sentence, it is however illogical, as the geometry of observation is much worse in Lai Chau than in Song Tranh. The reason?

"Interestingly, for a noise level of 10%, results tended to converge for shallow events." Convergency to what have you in mind? Both the magenta points (DC source) and red points (non-DC) tend to follow the lines across whole the graph.

The scatter of the MT decomposition in the noisy experiments is similar in both the Vietnamese sites, which is puzzling taken into account the fact that the geometry of observation is much worse in Lai Chau than in Song Tranh.

The most puzzling feature is however the ambiguity between tensile crack and anticrack in the noise experiments for shallow events. Even with a low noise, the DC source is not resolved but spread from crack to anticrack. This is hard to understand, because in a low noise contamination the first signs are mostly not affected. At the same time, tensile crack and anticrack yield just opposite signs, equal towards all directions.

p.38,39 Combining data from in principle different instruments (seismometers vs. GNSS receivers) surely demands for a thorough restitution of the signals, in order to work with pure ground motion. At least a brief description in addition to the reference (Kudlacik et al 2021)would be helpful and friendly towards the reader.

Fig.20 There is a mess a bit about the GNSS stations employed: KOMR, TRBC and TRN2 are named in the text, but in Fig.20 some others are listed (TRZB, TARN, LES1). The Figure 20 caption is not communicative sufficiently: please describe better the data sets employed (e.g., what it means "no data from co-located SM"? GNSS data only?). Labels are too small, on paper it is impossible to read them without a magnifying glass, on the screen the font size is insufficient as well (zooming does not help as the resolution of the graphs in the PDF is too low).

p.40 "...rotation angle, indicative of solution quality" The Kagan angle measuring the difference of fault plane solutions relevant to the full MT and the deviatoric MT is not always a good criterion for the solution quality assessment. In particular, if the MT is close to a CLVD, the DC component represents a marginal part of the complete MT, and its fault plane solution (odal planes) may be slanted markedly. Then, the Kagan angle may be high even if the MT solution is fairly stable.

"...unsuitable for roof collapse scenarios..." It is a popular idea that the CLVD is a suitable model for a roof collapse, however it is not true entirely. The CLVD is not a physical phenomenon (or does not have directly a physical analog) like the DC and ISO, it is a mathematical construction invented for decomposing the full MT into ISO, DC and "something". It does not describe displacement but forces, that's at first, and further there is not only a single force couple involved but two perpendicular couples in addition, which makes it far from a similarity with a roof collapse. The roof collapse equivalent in terms of forces and force couples was derived by Malovichko (Equivalent Point Sources for Seismogenic Processes in Mines, RaSiM Proceedings)

p.42 „To determine the limitations of the application HR-GNSS data synthetic maximum amplitudes were calculated." The synthetic experiment (p.42 and 43) explains an interesting feature of the velocity model constructed for the LGCD, nevertheless I don't understand much its correspondence to the application of the HR GNSS data: just a scaling of seismological and GNSS data?

p.44 „The first peak duration was taken from existing data - moment tensors calculated by first P-wave amplitude inversion." The sentence is unclear. I expect to determine it as the average pulse width just by inspecting the waveform data.

Fig.24 It would be more illustrative to see the plots with the same scale of the axis Intermediate/Far Field Ratio. Here the reader has to read the numbers on the vertical axis to realize which line is steeper.

p.50, paragraph 2 "Solution quality determination is relevant for calculating MT for anthropogenic phenomena with substantial non-DC components." Correctly declared that a big DC part is necessary for applying the criterion, which was not noted earlier. On the other hand, the following sentence "Synthetic tests on MT inversion indicated increasing instability with a rising contribution of non-double-couple mechanism components" is a trivial statement: for a DC: the instability is here defined for the DC only and when it disappears, the concept of instability is irrelevant, see also the comment ad p.29. "Instability in solutions was observed especially if the depth of the event was greater than the biggest event-station distance for all networks,..." This is the evident consequence of deteriorating the focal sphere coverage with increased source depth, worth mentioned in the Discussion.

'Significant for stability seems to be stations on the focal sphere evenly distributed in a circle near the edge.' I am suggesting to assess the particular configuration as *sufficient* for stability, rather than *significant*.

p.50, last paragraph The logic of the paragraph is messy a bit. 3rd sentence: for all 4 networks, stations over whole focal sphere and at least one opposed polarity needed. 4th sentence: the same for the LGCD, so why is there the preceding "however"? 5th sentence: it is saying the same as the 3rd one, infact. Last sentence: it repeats what we have read in the 3rd one. Last sentence could be healed by changing the wording "also depends" by "primarily depends".

p.51 "In the simple model of Lai Chau, focal coverage changes gradually with velocity variations however, the azimuthal gap is visible. " Focal coverage changes with focal depth, the velocity model is fixed. The azimuthal gap is naturally visible for all of them, because it persists in any 1-D model, and can be reduced/removed by incorporating a suitable lateral inhomogeneity only.

p.51, paragraph 2 'To get a stable solution the *reverse arrangement* of stations on the focal sphere was required, which apparently excludes obtaining a stable solution with certain components.' Unclear to me, similarly the next sentence as well.

p.51, paragraph 3 "Compared to the test results from the Rudna mine, it is essential to examine the influence of a detailed near-surface velocity model to determine if its inclusion can enhance the quality of MT solutions." The logic of the sentence is not convincing: In the MT retrieval, the GF applied should be as exact as possible. If the velocity structure is determined reliably, it should be incorporated into the procedure. Its reliability should however be assessed carefully in order to prevent construction of fictitious "ghost structures". For instance, at the Rudna mine, are the low velocity channels really a regional feature or are they rather local phenomena? This affects a lot ray tracing amplitude evaluation, focal sphere coverage, and at the end the MT retrieval itself.

p.54 In the approach the boxcar function is essential, but it does not imply that the finite source model is applied (e.g., where there is the rupture velocity?). A finite source time function can be originated by other features, e.g., a low pass filtering near the hypocenter.

p.54, 1st paragraph The shear source assumption is important in estimation of the "strength" of the intermediate and far field radiation (eqs.11-13), but the assessment far filed – intermediate field is legitimate for other source types as well.

2nd paragraph Calculated amplitudes vs. observed ones display a clear increase with the event size (moment magnitude). The Author hypothesizes that this may be due to assuming only the far field during the evaluation of seismic moment/magnitude. Another hypothesis may consist in neglecting the source finiteness: the larger event, the bigger the error to consider it as a point. Another factor may be the simplified assumption about the radiation (described by a constant factor instead of assuming the directional radiation pattern). This is an important point, why the correct evaluation of radiated amplitudes was not performed, when the mechanisms of the events selected were known? Apart from these mentioned, the Author suggests additional factor s with a potential influence for the effect discussed: erroneous location, complexity in radiation within the Haskell model. It may true, nevertheless the increase of the curve in Fig.29 is so clearly pronounced that the reason must be straightforward, in my opinion.

p.56 Non-physical solution are mentioned in context of the synthetic experiments, the Author however does not define them. From the context there is obvious that as nonphysical those solutions are considered, which result in the decomposition into ISO and CLVD of opposite signs. The same signs indicate the solutions, which behave similarly to a crack – tensile or the opposite one. It is a question whether this naming is sound: for instance, explosions are not "crack-like" sources, but undoubtedly they are physical.

"Solutions were unstable when events were located on velocity layer boundaries" In my opinion, to much extent this is a mathematical trouble and artifact of the velocity model definition. The boundaries are not sharp but rather transition zones, thus the discontinuity itself may be not real but merely an approximation of the reality in the model. If they are sharp, probably they are not a feature across whole the zone but they are rather local phenomena. In this case also they are not strictly horizontal, but they are parts of a 3-D inhomogeneity. In summary, the needn't be as sharp in reality and may be not so harming the GF evaluation as it often happens.

"Moreover, instability was particularly noted when the event depth exceeded the maximum event-station distance." The configuration generally yields a poor focal sphere coverage. The coverage deserves to be mentioned in explicit as a prerequisite of a success in the MT retrieval.

Paragraph 3 "The Lai Chau and Song Tranh networks set up for monitoring artificial reservoirs, provide stable and reliable MT inversion" Please define in explicit what it means *stable* and *reliable*.

"While the location of an event on the network edge may affect the component results, *it does not compromise the overall stability of the solution*." Unclear to me.

Last paragraph "Deeper events presented *correct* components solutions however, events with non-DC components proved *unstable* at these depths." To my understanding, the stability/instability relates to the difference in orientation of the DC part related to full MT and deviatoric MT, respectively. The decomposition is however just a product of processing of the MT components. Is the decomposition wrong if the component are correct?

Typos

p.10	last-square problem → least-square problém
p.16, eq.(11)	$...2(\cos 2\theta \cos\theta\ \theta...$ → $...2(\cos 2\theta \cos\phi\ \theta...$
p.43	LGOM → LGCD
p.50	contrary to the isotropic velocity model → contrary to the homogeneous velocity model
p.56	unfavourable incidence angles resulting in poor focal coverage → unfavourable také-off angles resulting in poor focal coverage.

Conclusion

I have studied the PhD dissertation by Anna Tymińska, MSc in detail and my overall opinion is positive. Thus, I can declare that

- she demonstrated a sufficient level of the general theoretical knowledge in the discipline and the ability to independently carry out scientific research;
- to my knowledge, her solutions of the problems solved in the PhD dissertation are original and worth of constituting the contents of a PhD dissertation; and
- I fully support her admission to the public defense of the PhD dissertation.

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