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Comment on egusphere-2022-180

Anonymous Referee #1

Referee comment on "Strato-structural evolution of the deep-water Orange Basin: constraints from 3D reflection seismic data" by Nombuso G. Maduna et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-180-RC1, 2022

Using a set of 3D seismic reflection data this manuscript aims at constraining the strato-structural evolution of the transitional and compressional domains of a Late Cretaceous deep-water fold-and-thrust belt system and its influence of the overlying Cenozoic megasequence in the Orange Basin offshore SW Africa.

The writing, wording and phrasing very often is unclear. The authors often are not precise and leave room for misunderstandings. It, e.g., remain unclear what improvement in scientific knowledge will be achieved by the study. The authors cite a large amount of literature describing the tectonic setting and development (see, e.g., Seismic stratigraphy and Results and interpretation). The interpretation of sequences and stratigraphic markers is mainly a set of statements, there is no discussion. Most of the facts presented are cited from other studies – so what is new? This is really difficult to identify.

Furthermore, the authors are very focused on the tectonic influence on sedimentation and deposition. They large ignore the effect oceanic currents and water mass transport have on this. The authors only briefly venture in this direction but in an oversimplified way and they appear to lack an understanding of physical oceanography in general and of this region in particular.

The authors speak of high-resolution seismic reflection data. However, the data were collected with a sample rate of 2 ms (see Table 1, Nyquist frequency 250 Hz) and then resampled to 4 ms (see Table 2, Nyquist frequency 125 Hz). This certainly is not high-resolution! High-resolution seismic reflection data should be recoded at minimum 1 ms sample rate giving a Nyquist frequency of 500 Hz. And where is the use of lowering the vertical resolution of seismic data by resampling?

The paragraph on Seismic resolution is very simplistic and, in parts, wrong. A reputable reflection seismologist uses l/2 and not l/4 (or even l/8) to compute the vertical resolution as l/4 is a very theoretical value. I would like to see a spectrum of the seismic data since I seriously doubt that the dominant frequency observed is 50 Hz; please, provide evidence for this! Even so, the Fresnel Zone definitely is not half of the dominant wavelength but dependent on target depth! Please, see Yilmaz (2001), P. 1803. There the formula is given and states that . So, the Fresnel Zone is dependent on target depth z and the main frequency. So, even if we take the main frequency to be 48 m (serious doubts here) the Fresnel Zone in 1000 m water depth will be 155 m and in 2000 m water depth it will be 220 m. So, the assumptions of the authors of a constant Fresnel Zone of 24 m are wrong!

The data have undergone severe processing applying several deconvolution techniques, resampling, attenuations, and time variant scaling (see Table 2). This all may have enhanced the signal but it also destroyed the true reflection amplitude. Still, the authors interpret variations in reflection amplitude both laterally and vertically (e.g., line 176, 196, 212). This, however, is no longer possible.

There is more evidence that the authors are not on top of the seismic method. They have labelled the twoway travel time negative. That implies that the data have been recoded before the shots were trigger. This is nonsense (sorry for being so blunt). It is neither the habit to label depth in negative numbers. If you want to make clear that this is below seafloor, please use mbsf (metres below seafloor).

The authors use seismic attributes to aid their interpretation. They state that 'seismic attributes are designed through mathematical manipulation' but they are not designed but computed. And I hope the data have not been manipulated (even though the severe processing certainly manipulated the data).

Taking all this into account I have huge doubts about the interpretation of the seismic reflection data presented by the authors since they do not appear to have fully understood the concept and physics of the method or the possibilities and limits.

The interpretation of the data is presented before this is discussed. To me this appears awkward.

Little information is provided on the lithology. The authors just state that, e.g., a) the Albian surface represents a maximum flooding surface (how do they know this?) and b)is a shale detachment surface. This is just one example. What are the arguments for those interpretations? What knowledge is used and not presented?

Section 4.1.3 how does especially the younger Cenozoic tie in with other studies from this margin, e.g., (Hopkins and Cartwright, 2021; Weigelt and Uenzelmann-Neben, 2004,

2007a, b)? The discussion here is very centred on tectonically influenced sedimentation. It may be useful for the authors to broaden their reading.

Section 4.2.1 is very focused on complex attributes. In my opinion the authors got a bit carried away by the figures of the complex attributes and lost their feeling for resolution and other limits.

The whole discussion is a repetition of other people's work.

Section 5 to me does not show any advance in scientific knowledge.

It would be helpful if the described features all were marked in the figures.

The References needs checking. For several references journal name, etc is missing, some references are listed twice.

Jungslager, 199 is missing from References

Figure3: Seismic depth limit is supposed to mean maximum penetration? Annotate the seismic markers discussed in the text please with the numbers used in the text

Figure 4 is obsolete

Figure 5: How were the type of the faults and the throw direction identified? Certainly not based on Fig 5a, which is rather chaotic. Travel time can never be negative! Show coordinates

Figs. 6-8 TWT never is negative! Same applies to depth; if needed use mbsf

Figs 9-12 are extremely confusing and not helpful at all. Please, omit

Figure 13: How were the ages assigned? Where does the anticline in 13d originate? Cold water usually has a higher density than warm water (annotation 13e)

Line 40 The authors state that this margin is largely underexplored since there is only one well per 4000 km^2. This is meant regarding hydrocarbon exploration? Extensive sets of seismic reflection data have been collected along the margin and several scientific sites have been drilled. So, regarding the development of the passive continental margin a wealth of information is available.

Lines 44/45 what is the scientific importance of this?

Line 46 what is 'early' 2D seismic data

Lines 48/49 already said in line 46

Lines 58/59 'an in-depth examination of the transitional domain from a buried DWFTB system' – from the DWFTB to what? Confusing

Line 93 this is a very confusing sentence. Rephrase

Line 102 this is only true until the onset of the Benguela Current in the Miocene. Please, study the right literature

Line 117/118 why was only a subset of the 3D survey used for this study? No argument given

Line 125 why were the data resampled to 4 ms? That reduced the vertical resolution by half!

Line 129 the velocity model used for depth conversion is rather crude. How was this derived? What is the reason for not using velocities derived during velocity analysis?

Line 151 'local deviation of the seismic signal' – from what?

Line 170 'often always' – which one?

Line 248ff how was constraining the ages carried out? And here it is stated that age assignment was difficult, but in the previous paragraphs ages were used very confidently…. It really would be nice to see a correlation of seismic data with ages and lithology at a well.

Line 268 how do you know the Oligocene unconformity was formed subaerially?

Line 270 or other mass transport?

Lines 278-280 this cannot be seen in the figure

Lines 280-282 see, e.g., (Weigelt and Uenzelmann-Neben, 2004)

Line 285 TWT can never be negative!

Lines 289-290 it is unclear how this detailed interpretation was derived

Line 292 'as explained previously' – this was not explained but stated

Lines 294-296 and what is the importance of this?

Line 305 it is impossible to see this in the somewhat chaotic Figs 9 and 12

Line 310 to me the spoon-shaped feature looks over-interpreted

Line 314 in which sequence?

Line 321’ deviation from the normal trend’ – what is the normal trend?

Line 326 the authors cannot resolved metre-scaled displacements! The resolution of the seismic data does not allow this.

Line 329 mounded and chaotic geometry is not necessarily a sign for turbidites. There are other mass transport deposits

lines 335-337 this is no discussion!

Lines 341-343 repetition from lines 335/336

Lines 351-353 what does this mean? Confusing

Lines 372-394 this is all other people’s work but the manuscript rests on this. How were the ages identified?

Lines 417-420 there is no anticline in Fig. 7

Lines 439-442 all this is based on already published studies

Line 454 the canyon in Fig. 8 is definitely not fault controlled

Line 463 there are plenty of more recent studies based on higher quality data than Dingle et al., 1983

Line 465ff this whole discussion is focused on tectonic sealevel variations. How does the onset of Antarctic glaciation tie in, how the variability in Antarctic ice-sheet thickness and size, the variability in the location of the Southern Ocean frontal systems?

Line473 which existing planes of weakness? What caused them?

Line 484 it is interesting to see that the authors cite a paper from the Brazilian margin. There, the oceanographic system is quite different to the SW African. Why not cite papers which dealt with the SW African margin?

Line 480ff it appears to be assumed that deposition is mostly fault controlled. That is not the case for SW Africa, where upwelling, NADW, AABW and the Benguela Current significantly influence sediment transport and deposition.

Lines 490ff the ocean offshore SW Africa is strongly stratified, which results in baroclinic/geostrophic flow. Internal waves are not needed for this. Here, the authors think too complicated. I seriously doubt that tidal movements will affect deposition/erosion in 2000 m water depth. Tidal current further act not slope parallel.

Line 492 what is erosional undercutting?

Line 494 differences in temperature/salinity generally cause geostrophic flow, not only parallel to the slope

Lines 497-501 those erosional features may have been formed by AABW or NADW, not only by upwelling. Also see (Weigelt and Uenzelmann-Neben, 2004)

Line 501 and due to variability in the glaciation of Antarctica!

Line 502 in what water depth?

Lines 503-505 this is NADW!

Line509 a CDS show high sedimentation rates!

Line 512 it is not true that erosional features of CDSs have been poorly studied

Lines 513-514 e.g., (Weigelt and Uenzelmann-Neben, 2004)

Lines 549-552 I do not agree. Most of the following was already known previoulsy

Hopkins, A., Cartwright, J., 2021. Large scale excavation of outer shelf sediments by bottom currents during the Late Miocene in the SE Atlantic. Geo-Marine Letters 41, 33.

Weigelt, E., Uenzelmann-Neben, G., 2004. Sediment deposits in the Cape Basin: Indications for shifting ocean currents? AAPG Bulletin 88, 765-780.

Weigelt, E., Uenzelmann-Neben, G., 2007a. Early Pliocene change of deposition style in the Cape Basin, southeastern Atlantic. Geological Society of America Bulletins 119, 1004-1013.

Weigelt, E., Uenzelmann-Neben, G., 2007b. Orbital forced cyclycity of reflector strength in the seismic records of the Cape Basin. Geophysical Research Letters 34.