

Interactive comment on “From widespread Mississippian to localized Pennsylvanian extension in central Spitsbergen, Svalbard” by Jean-Baptiste P. Koehl and Jhon M. Munoz-Barrera

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Dear Dr. Peace, Thank you very much for your input on the manuscript, it is highly appreciated. Here is our response to your comments. We hope the changes we implemented improve the shortcomings of the manuscript highlighted by your comments and suggestions. Please do not hesitate to contact us shall this not be the case for some comments.

1. Comments from Dr. Peace Comment 1: Wider implications of the study and comparison to other regions The authors present an exceptionally detailed examination of geological field observations, complemented by satellite data from a relatively small,

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isolated region. Although the approach and topic of the manuscript seem reasonable, I found that the relevance of the study, beyond that of the local geology, was not sufficiently outlined either in the introductory sections or later in the discussion. This is not to say that the work does not have such implications but that they are not currently described adequately. As such, I think this is probably a moderately easy, yet worthwhile, aspect to resolve as the study clearly has broader implications that would increase the appeal and usefulness to a wider group. Comment 2: Analysis of satellite imagery The figures showing the satellite data evidently bring a lot to the study in terms of extrapolating the field-based observations to infer more regional processes, and will no doubt be useful for addressing the point outlined above. However, minimal specifics regarding the satellite data (e.g., resolution or age) are provided in the methods section. For example, does all the data presented have the same specifications? In addition, no details are provided regarding the type of analysis or criteria used to interpret features on this data. Related to the latter point, I felt that better use of the satellite data could have been made by explicitly tying individual features identified in the field-based studies to specific features on the satellite data. If this type of ground truth investigation was undertaken it should be outlined more explicitly in the manuscript. Currently, I think that the lack of the information described in this point partially undermines the findings that are derived from this analysis. As such, I suggest expanding upon these aspects in the relevant sections, but particularly in the methods section. Comment 3: Description of the deformation - fault rock types The paper adequately describes the orientation and distribution of deformation sufficiently, both in the outcrop observations and also on the satellite data. However, the nature and categorisation of fault rocks could be better described. This is especially important in reactivation studies as the nature of fault rocks is an important line of evidence to evaluate such aspects. As such, I suggest attempting to better categorise the fault rocks, potentially using a scheme such as those outlined in Killick (2003) or Woodcock and Mort (2008). Comment 4: Referencing The reference list seems up to date and extensive. However, it currently contains three 'submitted' papers, in addition to an 'unpublished' internal report. I appreciate that much of

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this aspect is beyond the control of the authors. However, I was wondering if it is possible to cite some published work alongside these, perhaps even conference abstracts? For example, the EGU abstract Koehl et al. (2016) appears to address some of the themes in the present study. In addition, if the internal report can be made available online this would be beneficial. Hopefully during the time taken to review and revise the present paper some of the submitted papers will be accepted to alleviate this issue. Comment 5: Abstract – Currently the abstract is quite long and the scientific aims are not easily discernible. Perhaps the abstract can be restricted to the more salient points to assist with this. Comment 6: Line 13 – ‘central Spitsbergen’. For readers not familiar with the geography of Svalbard it might be helpful to say where this is e.g., offshore Northern Norway. Comment 7: Lines 27-29 – What are the terms in quotes taken from and are they necessary? Comment 8: Lines 35-39 – The last sentence of the abstract is currently very long. I suggest breaking this into smaller sentences to make it more poignant and easier to follow. This may assist with addressing the point above on the abstract length generally. Comment 9: Line 36 – ‘mildly reactivated’. In my opinion this phrase is ambiguous as it is not clear what would entail ‘mild’ reactivation compared to an event that could be considered more extensive reactivation. I therefore suggest rewording this in addition to the variants of it that appear throughout the manuscript such as ‘partially reactivated’ (line 666) and other occurrences (lines 292, 298, 317 and 658). With respect to ‘partially reactivated’ this is particularly ambiguous as it is unclear whether this is referring to selective structures being reactivated or whether the magnitude of reactivated fault movement is minimal. Please clarify appropriately. Comment 10: Lines 48-51 – I suggest referring to the location map (Figure 1). Comment 11: Geological setting – This section is particularly very well written, with the information mostly confined to only the most relevant points, whilst also being generally well organised. However, the authors may want to consider numbering the sections to make this part of the manuscript easier to follow. Comment 12: Line 145 – This sentence is currently a bit awkward to read. I suggest rewording. Comment 13: Line 160 – ‘Fourth and fifth’. This approach to denoting the points in this paragraph is difficult to follow. I sug-

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gest changing it. Comment 14: Line 166 – Are the phrases in quotes directly from the reference in this sentence? This is currently not clear in the manuscript. Comment 15: Line 176 – ‘thick Pennsylvanian sedimentary strata’. If possible state the thickness of these sediments. Comment 16: Line 207 – Suggest removing the word ‘these’ to make the sentence flow better. Comment 17: Lines 208-209 – It is not clear whether the observations are from this study or those referenced in the sentence. This should be clarified. If both this present study and the previous work make the same observation this should be made clearer. Comment 18: Line 242 – Consider replacing ‘there’ with ‘here’. Comment 19: Line 245 – ‘the hereby described grey sandstone’. This phrase is quite awkward to read. I suggest rewording. Comment 20: Line 250 – Suggest removing the word ‘rather’ to make the sentence flow better. Comment 21: Lines 267-268 – ‘we propose that the hereby-described red-bed sedimentary succession is part of the Hultberget Formation’. The readability of this sentence could be improved. I suggest something like: ‘we propose that the red-bed sedimentary succession described herein is part of the Hultberget Formation’. Comment 22: Line 275 – ‘non-cohesive fault-rock’. In line with the second major point outlined above I suggest better characterising this the fault rock. Comment 23: Line 285 – ‘high angle’. If possible, I suggest stating how steep the ‘high angle’ fault is. Comment 24: Line 287 – ‘cataclasite’. Here, terminology related to fault rocks is used. I suggest doing this elsewhere in the manuscript. Comment 25: Line 292 – ‘during Cenozoic transpression’. When reading the manuscript I did not feel that the evidence leading to this interpretation was adequately provided. Specifically, what is the time constraint leading to this interpretation? Comment 26: Line 315 – ‘is made of’. Consider replacing with ‘comprises’ to help the sentence flow better. Comment 27: Lines 349-350 – ‘are believed to have been eroded or never deposited’. It is not clear if this is a finding of this study or previous work. I suggest clarifying. Comment 28: Lines 359-360 – ‘which we interpreted as steep brittle faults’. This is an example of the ambiguity outlined in 3rd main point above. In particular, was any attempt made to directly tie the interpretation of the satellite data to actual field observations such as this? If so I suggest stating it more clearly here and elsewhere in

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the manuscript. Comment 29: Line 367 – ‘dolerite dykes’. Has an age of these dykes been obtained? If so it would be helpful to state it here. Comment 30: Lines 437-432 – In these opening sentences of the section numerous lines of evidence ‘in favour of Mississippian syn-sedimentary extensional brittle faulting’ are presented as one very long sentence. It is therefore quite difficult to follow due to the large amount of information contained, and I suggest either numbering the lines of evidence or separating this into multiple sentences. Comment 31: Line 520 – ‘c.’ not ‘ca.’ when not referring to ages or times. Comment 32: Line 521 – As previous. Comment 33: Lines 625-665 – The conclusions section contains many long sentences, with each concluding point comprising one such statement. I suggest shortening the sentences to make the conclusions easier to read and more poignant. Comment 34: Line 631 – Add ‘s’ after ‘suggest’. Comment 35: Line 634 – ‘of the Hultberget Formation, thus suggesting’. I suggest breaking this long sentence into two smaller ones by concluding the first after ‘Formation’ and replacing ‘thus’ with ‘This’. If this is accepted, then ‘suggesting’ needs to change to ‘suggests’ in the second statement. Comment 36: Line 656 – This sentence is incomplete and ends at the word ‘which’. Comment 37: Line 663 – ‘gently dipping’. Is it possible to state which way they are dipping? Comment 38: Line 838 – ‘Geochemistry’ is spelt incorrectly. Comment 39: Figure 1A – Scale is missing. Comment 40: Figure 1B – The white areas on the map are not on the key. Comment 41: Figure 2 – The green and brown colours on the stratigraphic column do not appear on the key. Comment 42: Figure 3 – Although the caption states ‘The photographs are approximately one kilometer wide’ I think more accurate measurement of the scale of the images is required as they are clearly not all the same dimensions. Comment 43: Figure 4 – The dip markers on the figure are quite problematic to see and on the key a white line in a black box is shown (fault core boundary). However, this does not appear on the figure. Comment 44: Figure 5 – the field photographs require scale and orientation. Comment 45: Figure 6 – This figure contains two types of yellow line. Are these showing different features? If so this is not clear in the figure. Also the statement in the caption that ‘The outcrop is approximately 10–15 m wide’ is a bit ambiguous as

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it is not entirely clear which parts of the field photo are considered ‘outcrop’. Comment 46: Figure 7A – The lines marked on here are extremely thin and unlikely to be easily visible at publication scale. Comment 47: Figure 7A-C – Scales needed. Comment 48: Figure 8 – Scales and orientation need to be provided for all subfigures. Comment 49: Figure 9 – Same as previous comment. Comment 50: Figure 10 caption – ‘c.’ not ‘ca.’ when not referring to ages or times. Comment 51: Figure 11 – The text on the figure is very small and unlikely to be easily visible at publication scale. Comment 52: References Killick, A.M., 2003, Fault rock classification: An aid to structural interpretation in mine and exploration geology: *South African Journal of Geology*, v. 106, no. 4, p. 395–402, doi: 10.2113/106.4.395. Koehl, J., Tveranger, J., Osmundsen, P.T., Braathen, A., Taule, C., and Collombin, M., 2016, Fault-growth deposit in a Carboniferous rift-basin: the Billefjorden Trough, Svalbard: *Geophysical Research Abstracts*, v. 18, p. 7131. Perron, P., Guiraud, M., Vennin, E., Moretti, I., Portier, É., Laetitia, L.P., and Konaté, M., 2018, Influence of basement heterogeneity on the architecture of low subsidence rate Paleozoic intracratonic basins (Ahnet and Mouydir basins, Central Sahara): *Solid Earth Discussions*, doi: 10.5194/se-2018-50. Phillips, T.B., Jackson, C.A., Bell, R.E., and Duffy, O.B., 2018, Oblique reactivation of lithosphere-scale lineaments controls rift physiography – The upper crustal expression of the Sorgenfrei-Tornquist Zone, offshore southern Norway: *Solid Earth*, v. 9, p. 403–429, doi: 10.5194/se-9-403-2018. Woodcock, N.H., and Mort, K.M., 2008, Classification of fault breccias and related fault rocks: *Geological Magazine Rapid Communication*, v. 145, p. 435–440, doi:10.1017/S0016756808004883.

2. Author’s response Comment 1: agreed. Comment 2: agreed. Comment 3: agreed. We now use the classification of Woodcock and Mort (2008). Comment 4: agreed. The internal report is already available on the main author’s ResearchGate webpage upon request. However, the suggested abstract by Koehl et al. (2016) does complement any of the submitted papers. Comment 5: agreed. Comment 6: agreed. However, “offshore northern Norway” is quite confusing for readers that are actually familiar with the study area. Comment 7: agreed, they are not necessary in the abstract and can

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be described at a later stage, in the discussion. Comment 8: agreed. Comment 9: disagreed. The term “mildly” refers to the magnitude of movement along the reactivated structures, which is relatively small compared to km-scale offsets along large faults (e.g., the Billefjorden Fault Zone) in the study area. Furthermore, the term is clarified line 441 where it is followed by “with little or no upwards propagation”. Comment 10: agreed. Comment 11: agreed. Comment 12: agreed. Comment 13: disagreed. The introductory sentence of the paragraph stipulates that the paragraph is dealing with five different formations, and we therefore believe that the use of “first”, “second”, etc. appropriate to this paragraph. Comment 14: yes, the phrases in between quotation marks are directly from the associated publication. The manuscript even specify in which figure of the referred publication one may find the terms in quotation marks: “Gawthorpe and Leeder, 2000, their fig. 3”. Comment 15: agreed. Comment 16: agreed. Comment 17: the first sentence lines 207–208 refers to literature data, while the second sentence shows that the gneissic foliation described in the literature can be observed on satellite images. Comment 18: agreed. Comment 19: agreed. Comment 20: agreed. Comment 21: agreed. Comment 22: agreed. Comment 23: agreed. Comment 24: agreed. Comment 25: there is no major post-Mississippian contractional–transpressional tectonic event recorded in Spitsbergen other than an episode of Cenozoic transpression. Thus, it is natural to infer that any contractional structure or reactivation might have formed during Cenozoic transpression. Comment 26: agreed. Comment 27: agreed. Comment 28: agreed. Comment 29: agreed. Comment 30: agreed. Comment 31: agreed. However, the examples lines 520 and 521 should remain as “ca.” since they are referring to ages. Comment 32: see response to comment 31. Comment 33: disagreed. The present manuscript addresses a very specific issue (initiation of extension in Mississippian times, not in Early Pennsylvanian) and the authors need to be very specific in their conclusions in order to make their findings clear for all specialists and maximize the impact of the paper on future research. Comment 34: disagreed. Two arguments “suggest” this: the extensional growth strata and the change of contact type between the two formations. Comment 35: agreed. Comment 36: agreed. Comment

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37: the dip of the décollements varies as that of Carboniferous strata in the area, i.e., from SW to SE and from NW to NE. The authors believe that this information is not relevant to include to the conclusion and would rather overload a conclusion already crowded with specific points. Comment 38: agreed. Comment 39: agreed. Comment 40: agreed. Comment 41: agreed. Comment 42: agreed. Comment 43: agreed. Comment 44: disagreed. All four figures in figure 5 already contain scales and do not need orientation since they do not show oriented structures. Comment 45: agreed. However, the distinction between dotted and dashed yellow lines is made in the figure caption. Comment 46: agreed. Comment 47: agreed. Comment 48: agreed. Comment 49: agreed. Comment 50: agreed. Comment 51: agreed. Comment 52: agreed. However, the authors do not understand the suggestion of the work by Phillips et al. (2018) and Perron et al. (2018) to the reference list, although the authors are familiar with the suggested works. Perhaps the referee could specify the aim and the place he may find appropriate to add these references.

3. Changes implemented Comment 1: addition of a sentence on the broader implications of the present study on the hydrocarbon exploration, geodynamics, and margin architecture at the end of paragraph 1 in the introduction: “The present local study has broader regional implications, especially regarding the geodynamic setting of Arctic regions in the Mississippian (contraction versus extension versus tectonic quiescence?), the architecture and geometry of the Barents Sea and west Spitsbergen margins (Mississippian basins?), and may affect our understanding of the distribution of Mississippian coal-bearing hydrocarbon source rock in the Barents Sea” lines 60–64. Comment 2: addition of “In addition, fault surfaces and escarpments in the field were tied to map-view lineaments on satellite images that matched their trend and location (Figure 4). Critical factors used in the interpretation of geological features on satellite images in inaccessible areas include existing literature (e.g., N–S-trending gneissic foliation in basement rocks east and southeast of the field area was evidenced by multiple works, including notably Harland et al., 1966 and Witt-Nilsson et al., 1998), the geological database at svalbardkartet.npolar.no, and similarities with fault-related escarpments

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tied to actual brittle faults in the field area (Figure 4). Glacial features were segregated from ductile and brittle structures and fabrics using satellite images and scientific literature on recent and past glacial flow. Satellite images used in the present study are from 2011 and have a horizontal resolution of 40 cm” to the method chapter. Comment 3: addition of “fine-grained,” line 275; “i.e., fault gouge; Woodcock and Mort, 2008;” line 275; “dominantly fine-grained cohesive fault-rock (i.e., meso- to ultra-cataclasite; Woodcock and Mort, 2008)” lines 287–288; “(meso- to ultra-)” line 1064; Reference to Woodcock and Mort (2008) to the reference list. Comment 4: Added Reference to Bergh et al. (2014), Koehl (2018), and Klitzke et al. (2018) as complements to Bergh et al. (submitted), Koehl et al. (submitted), and Klitzke et al. (submitted) respectively. Comment 5: deletion of one sentence and several phrases. Comment 6: added “Svalbard” line 14. Comment 7: deleted terms in quotation marks. Comment 8: the sentence was shortened. Comment 9: no change. Comment 10: added “figure 1” line 49. Comment 11: added numbering to Geological setting sub-chapters. Comment 12: sentence split into two and partially rewritten. Comment 13: no change. Comment 14: no change. Comment 15: added “tens (hundreds?) of meters” line 181. Comment 16: implemented suggested change. Comment 17: no change. Comment 18: replaced “there” by “at this location”. Comment 19: deleted “hereby described”. Comment 20: deleted “rather”. Comment 21: implemented suggested change. Comment 22: implemented suggested change. See answer to comment 3. Comment 23: added “(> 70°)”. Comment 24: implemented suggested change. See answer to comment 3. Comment 25: no change. Comment 26: implemented suggested change. Comment 27: added reference to Harland et al. (1974). Comment 28: see response to comment 2. Also added “based on their similarities with fault-related lineaments in the field area (Figure 4) and their obliquity to the dominant N–S-trending ductile fabrics and structures (Harland et al., 1966; Balashov et al., 1993; Witt-Nilsson et al., 1998; Johansson and Gee, 1999)” lines 377–379. Comment 29: deletion of “in Mississippian times (Visean; Lippard and Prestvik, 1997)” line 386, and addition of “Mississippian (Visean; Lippard and Prestvik, 1997)” line 387. Comment 30: addition of numbers ahead of each ev-

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idence. Comment 31: replaced “ca.” by “approximately” where needed in main text. Comment 32: none. Comment 33: none. Comment 34: none. Comment 35: implemented suggested changes. Comment 36: deletion of “which” and addition of “.”. Comment 37: none. Comment 38: implemented suggested change. Comment 39: implemented suggested change. Comment 40: addition of “Areas shaded in white represent glaciers” to the figure caption. Comment 41: implemented suggested change. Comment 42: addition of a common scale on figure 3a. Comment 43: implemented suggested change. Comment 44: none. Comment 45: added scale to the figure. Comment 46: implemented suggested change. Comment 47: implemented suggested changes. Comment 48: implemented suggested changes. Comment 49: implemented suggested changes. Comment 50: implemented suggested change. Comment 51: increased the size of all text in the figure. Comment 52: addition of the Woodcock and Mort (2008) reference to the reference list.

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