Author reply to reviewer and community feedback to "Stress field orientation controls fault leakage at a natural CO_2 reservoir" by Johannes M. Miocic et al.

Reviewer comments are in *italics*, while author replies are in normal font.

Reviewer 1 (Alan Morris)

Reviewer Comment	Author replies	Changes to the manuscript
Scientific significance: Does the manuscript represent a substantial contribution to scientific progress within the scope of Solid Earth (substantial new concepts, ideas, methods, or data)? Yes, I rank this manuscript Excellent (1). This work represents an innovative look at a natural example of stress-state-enhanced fault and fracture permeability. Although the approaches used in the paper are not new, the careful application to a real-world example using an interesting and compelling dataset is an extremely valuable contribution to both the underlying science and possible technical uses addressing a globally significant problem – storage and sequestration of CO2. *Scientific quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Yes, I rank this manuscript Excellent (1). See above for the first question. The manuscript is very straightforward, clearly written and alternative interpretations are discussed. The referencing of previous work is comprehensive and appropriate.* *Presentation quality: Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)? Yes, I rank this manuscript Excellent (1). Use of English is good, style is concise, and order is logical. It is one of the most fluent*	We thank the reviewer for their constructive review, kind words and the assessment that our work is valuable for the community it is aimed at. This is appreciated and we have addressed their remarks and suggestions in the revised manuscript which has subsequently improved in quality.	

manuscripts I have reviewed. I have made several minor word-change suggestions throughout the manuscript and these are contained in the pdf that I have uploaded with my review.		
Line 41: Contrastingly,		This has been changed accordingly in the revised manuscript.
Line 50: tectonically		This has been changed accordingly in the revised manuscript.
Line 55: extent		This has been changed accordingly in the revised manuscript.
Line 80: overlie		This has been changed accordingly in the revised manuscript.
Line 83: include		This has been changed accordingly in the revised manuscript.
Line 87: bordered		This has been changed accordingly in the revised manuscript.
Line 108: fluids migrating from ; surface:		This has been changed accordingly in the revised manuscript.
Line 110: The St. Johns Dome travertine		This has been changed accordingly in the revised manuscript.
Line 114: bounded by		This has been changed accordingly in the revised manuscript.
Line 117: trace are		This has been changed accordingly in the revised manuscript.
Line 120: permeable conduit through		This has been changed accordingly in the revised manuscript.
Line 124: extent		This has been changed accordingly in the revised manuscript.
Line 217: supported		This has been changed accordingly in the revised manuscript.
Figure 4: I suggest using a different symbol, or simply changing this to "black dots", the crosses are not resolvable	We thank the reviewer for their comment and have changed the figure/caption	See revised Figure 4 (now Figure 6).

on the steroplots. This is not indicated on the stereoplots, from the map and text it would seem that the portion of the fault with the more NWesterly strike is also the NW fault tip. It would be good to annotate the stereoplots.	accordingly. The symbol used has been changed to black dots as requested in the revised manuscript.We have also annotated the NW section of the fault as suggested.	
Line 224: WNW		This has been changed accordingly in the revised manuscript.
Line 231: from failure towards its SE tip		This has been changed accordingly in the revised manuscript.
Line 232: similar to stress field B		This has been changed accordingly in the revised manuscript.
Line 245: extent		This has been changed accordingly in the revised manuscript.
Figure 5: Again, the fault facet poles are not resolvable as crosses in the Mohr circle plots.	We have changed this as with the resvised figure 4.	See revised Figure 5 (now Figure 7).
Figure 7: fault slip tendency		This has been changed accordingly in the revised manuscript.
Line 280: trending		This has been changed accordingly in the revised manuscript.
Line 287: tip of near critically stressed faults		This has been changed accordingly in the revised manuscript.
Line 288: I guess it could ultimately be the mantle, but do you mean magma?	We thank the reviewer for this comment which similarly has also been raised by reviewer 2. Detailed noble gas and stale carbon isotope analysis of the gas contained within the reservoir has clearly shown a mantle source to be the origin (Gilfillan et al., 2008, 2009). We thus believe that it is reasonable to suggest mantle as the source for the CO2 within the reservoir, even though magma will have formed the migration pathway. The magma of the	No changes to the manuscript.

	Springerville field is suggested to be sourced from a low velocity, partially melted mantle (Priewitsch et al., 2014).	
Line 289: near critically stressed faults		This has been changed accordingly in the revised manuscript.
Line 293: CO_2 or other fluids		This has been changed accordingly in the revised manuscript
Line 296: We recommend to select areas		This has been changed accordingly in the revised manuscript

Reviewer 2 (Johnathon Osmond)

Reviewer comments	Author replies	Changes to the manuscript
My review comments for manuscript se-2020-12 written by Miocic and others are described herein. The authors present a body of work set on addressing how stress field orientation influences natural leakage along faults by combining geomechanical modelling of a particular fault bounding a natural subsurface CO2 accumulation with outcrop information. Although the methods and framework are not novel, the main idea communicated by the authors is that CO2 leakage occurred mainly along the northern tip of the analyzed fault, which coincides with the location of travertine deposits as well as areas of high fault slip tendency and fracture stability modeled along the fault zone using three potential stress fields derived from regional measurements. The	We would like to thank the reviewer for their positive feedback and very detailed constructive review which has significantly improved the quality of our manuscript. We have addressed their comments and suggestions below.	

authors go on to discuss the importance of valid stress orientation parameters in relation to CO2 storage site modeling and evaluation. The key contribution that this manuscript provides is a strong correlation between unfavorable stress orientation and field evidence of CO_2 leakage, where areas of leakage occur where the modeled fault approaches geomechanical failure under a plausible stress state. In general, the quality of the manuscript is high, often demonstrating appropriate levels of scientific thought, valid conclusions, adequate visual aids, and clear language. Section lengths are of appropriate length and the number of figures is satisfactory. Minor revisions to the manuscript are suggested and pertain mainly to providing more details and discussion with respect to the modelling parameters and the significance of the results in a geological context, as well as with revising minor items.		
Overall, the authors do well to address most of the related subjects at a sufficient detail. Among others, these include considerations of regional stress orientation variability, timing of travertine deposition and leakage, and the lack of in situ stress measurements. However, several shortcomings were noted, but not limited to: *- Outcrop information (travertine mounds) provides a critical link between the geomechanical modeling and the observed leakage phenomena. What is lacking is the*	We strongly agree that field observations on fault orientation and fault rock properties would strengthen our manuscript. However, during two field seasons where we mapped and sampled the travertine deposits and geology in the area, outcrops of the actual fault were not encountered. This is likely due to (1) the overprint by both the with late Pleistocene gravels of the Richville formation and the (massive) travertine deposits which cover parts of the fault by several meters and (2) strong erosion along the Little Colorado River. This	Line 124: the <u>buried</u> Coyote Wash Fault

presentation of field observations to build and strengthen the validity of the author's models with respect to the fault parameters. Based on the text, it does not appear that the authors contributed or incorporated any new or direct field data. For instance, fault orientation measurements and fault rock observations from outcrop were not mentioned, but would bring confidence in the modeling parameters chosen by the authors. Perhaps this could be a possible avenue of future research.	suggests that there has not been (vertical) movement of the Coyote Wash Fault in the last ~400 ka based on our previous travertine dating work (Miocic et al 2019). Future research should include additional field work, in particular in the southern area of the Coyote Wash Fault where there are no travertine deposits and the fault is located further away from the Little Colorado River. The fault trace may be found to outcrop. in that area. However, access restrictions due to the electric power station mean we were unable to conduct field work in that area yet. We are also involved in a collaboration with Prof. Steve Nelson of Brigham Young University, Utah, to complete shallow geophysical surveys to improve the understanding of the geometry of the fault in the near-surface, which aims to address this uncertainty.	
- The parameters used to build the fault model heavily influence the results shown in figures 4, 5, and 6. Fault geometry (strike and dip) can have a dramatic impact on geomechanical modelling results, and at no point do the authors state them or describe why those values were used for building the fault. Also, no details about the fault mesh or grid were given to the reader. In most modeling programs, points on stereo plots or Mohr diagrams are derived from orientations either extracted from the 3D surface of the fault at each of its unit faces (mesh or grid) or unit vertices. Along with	We thank the reviewer for their comment and agree that we have not sufficiently reported on the properties of the 3D model and that reasoning for why we choose certain values was missing in the original manuscript. We have added several new sections to the revised manuscript addressing the reviewer's points as well as updating the respective figures.	Line 173ff: A 3D geological model of the St. Johns Dome was built based on published geological maps (Embid, 2009; Sirrine, 1958), well data from 37 exploration and production wells available from the Arizona oil and gas conservation commission (well logs, horizon markers) as well as previously published reservoir horizon map and markers (Rauzi, 1999) using Move™. Between wells a constant stratigraphic thickness was assumed and for the fault a dip of 70° was estimated, based on previous works (Embid, 2009, Rauzi, 1999) and a 3D dip-domain construction (Fernandez et al.,

the number of points shown in figures 4 and 5, the authors could provide details about the modeled surface (mesh or cell size) and method of value extraction from the fault surface in Figure 6. This would provide the reader and future workers some idea of how to repeat the workflow and obtain similar results.		2008) of the intersection of the fault trace with the 1/3 arc-second DEM of the 3D elevation programme of the USGS. The modelled fault has 6635faces constructed as triangles from 3525 vertices. The current gas-water-contact is at 1494 m above sea level (Rauzi, 1999) and is assumed to be horizontal. Due to lack of pressure data, a hydrostatic pressure gradient is assumed (0.0105 MPa/m). Geomechanical analysis of the model was conducted with industry standard software (Move™ and TrapTester®). Updated Figure 4 and 5 (now figure 6 and 7), new figure (figure 5).
Fault rock type is discussed and taken into consideration in the geomechanical modeling. Despite this effort, the authors do not provide an explanation as to why they interpreted the primary fault rock type to be either phyllosilicate (PFFR's, i.e., Fig. 2 from Yielding et al., 2010) or clay smears rather than other possible fault rock or fracture corridor types (such as some sort of deformation bands). The authors go on to assign values to geomechanical properties used in the fault model based on the interpretation of the possible fault rocks. The authors again fail to provide reasoning behind their decisions. To resolve this, the authors could provide reasoning behind their interpretation of the fault rock types (perhaps based on calculated burial	The reviewer is correct to point out that the properties of the fault rock incorporated into the geomechanical modelling are critical to the outcome of the modelling. We have added a section to the revised manuscript in which we detail our reasoning for choosing the used fault rock types and properties.	Line 183ff: As no outcropping fault rocks were available, the Shale Gouge Ratio (Yielding et al., 1997) was used as a fault rock proxy. SGR was calculated from a V_{shale} log of well 10-29-31, which was calculated from the gamma ray log assuming a linear response (Asquith and Krygowski, 2004). As this method only works for siliciclastic rocks, zonal V_{shale} values for evaporitic sequences ((70% shale content for anhydrite, 55% shale for carbonates were assumed, expecting rapid fault sealing for these lithologies (Pluymakers and Spiers, 2014) or low permeability fault rocks (Michie et al., 2018)) were used. Resulting SGR values indicate a high potential of phyllosilicate rich fault rocks (Fig. 5). To emphasise the

depth and throw or outcrop observations) and associated modeling parameters (maybe with references).		uncertainty regarding the fault rock composition, two different fault rocks were used for F_s calculations: clay smear (cohesion C=0.5 MPa, coefficient of internal friction μ=0.45) and phyllosilicate (C=0,5, μ=0.6) with rock strength values from the TrapTester® internal database.
The stratigraphic section contains both siliciclastic and carbonate units, which have been displaced by the Coyote Wash Fault where the CO2 column is interpreted to be trapped along. Siliciclastic fault rocks were discussed in detail by the authors, while carbonate fault rocks could also be discussed or at least mentioned in the manuscript.	We thank the reviewer for their comment on carbonate fault rocks. While there are several non-siliciclastic horizons within the faulted stratigraphy we have focused very much on siliciclastic rocks in our modelling approach. This is mainly because the chosen modelling software (TrapTester) has no predictive tools for combining carbonate fault rocks and siliciclastic fault rocks – which is understandable as the (permeability) evolution of carbonate fault rocks is not very well understood as of yet. The main carbonate unit (San Andres Limestone) at the St. Johns Dome is finely crystalline and of low permeability with only local fractures, acting as a seal. We have added a section to the revised manuscript addressing this comment.	*Line 191ff:* Note that for modelling purposes we assume a siliciclastic sequence, however the stratigraphic sequence also contains ~15 % carbonate and evaporitic rocks (Fig. 3) which may have locally significant influence on the fault rock strength.
- The authors state that they have generated a 3D geomodel based on several sources of outcrop and subsurface data, and it is evident that the model has guided many of their interpretations and discussion points throughout the length of the manuscript. Aside from the 3D fault model in Figure 6, however, there is no presentation of the 3D model by the	We thank the reviewer for their suggestion and have added additional information to figure 2 in the revised manuscript to visually aid the reader.	As suggested we have added structural contours of the top reservoir horizon to the geological map (Fig. 2 in the revised manuscript).

authors to aid the reader while reviewing their descriptions and discussion points. Aside from simply assisting the reader and showcasing the model, providing visuals from the 3D geomodel could be used to support arguments made by the authors. For instance, the authors mention several significant structural levels inside the closure of the Cedar Mesa Anticline with no visual aid, such as with the maximum closure height (300 m) or the additional fault-limited capacity based on the geomechanical model results (up to 160 m of additional CO2 than its current state). The authors go on to suggest that travertine deposits outside the current areal extent of the CO2 accumulation may have once been located with it when the structure contained more CO_2 before leakage. This is stated without demonstrating this to the reader visually. A suggestion is to include more of the 3D model in the current array of figures. This could be in the form of structural contours for the top of the reservoir overlain in Figure 2 or a new map or 3D view of the model.		
With respect to the history of the faults in the area, it is perplexing that shortening in the Laramide created a normal fault. Reverse sense of movement would be exhibited in that case. Seems that this is more of a Basin and Range or Rio Grande Rift type structure, where extension lead to normal faulting and a footwall rebound anticline. Interpretations from the cited literature suggest inversion of a Laramide	We thank the reviewer for their suggestion and have revised this part of the geological setting in the revised manuscript.	Line 85ff: The fault is thought to be related to Paleogene Laramide compressional tectonics which led to monoclinal folding of the Phanerozoic strata and the reactivation of older basement structures such as the Coyote Wash Fault on the Colorado Plateau (Marshak et al., 2000). The normal displacement of the fault suggests an inversion of the reverse fault related to the

reverse fault into a normal fault later in the Tertiary during Basin and Range/Rio Grande Rift events. With that in mind, this detail should be reflected in the text.		Basin and Range extension starting in the Early Miocene and continuing in the Pliocene as evident from displacement of Pliocene basalt flows (Embid, 2009).
SHmax measurement locations were cross-checked with their mapped pattern in Figure 2. The pattern for the set of 8 points appears to agree with what is shown in Figure 2 except for point ID 4 (SHmax=61 deg, Connor et al., 1992), which plots much further south than what is indicated in Figure 2. This means that either the mapped location in Figure 2 is correct or the location form Table 1 is correct. Point coordinates were then checked based on their sources. WSM points from Heidbach et al. (2016) were validated, but those from Connor et al. (1992) were not. It was not clear from Connor et al. (1992) where the authors of se-2020-12 obtained both their SHmax azimuths and coordinate locations in Table 1. Could it be that the data was sourced from an alternative data repository? Two suggestions to the authors here include addressing the discrepancy between Table 1 and Figure 2 locations for point ID 4, and describing the origin/location of the measurements derived from Connor et al. (1992) in greater detail.	We thank the reviewer for this detailed verification of our work. Based on their suggestions we have included an updated version of Figure 2 in the revised manuscript where georeferencing errors are fixed and additional data from Connor et al (1992) are included. We also updated table 1 to show the correct coordinates.	New and updated Figure 2 Line 210f: Note that the maximum horizontal stress (SH_{max}) from Connor et al. (1992) is based on vent clusters linearly aligned with lengths of 11 to 20 km length (Fig. 2) and that table 1 lists them as point measurements at the centre of the cluster.
- The authors propose three potential stress fields for their model (A most-likely, B least-likely, and C-intermediate likely cases). To main a logical order, perhaps cases B and C could	We thank the reviewer for their suggestion which we have implemented in the revised version of the manuscript.	We have changed the order of stress fields throughout the manuscript: Old order was most likely, least likely, and intermediate likely.

be switched throughout the manuscript so that B is the intermediate case and C is the least-likely case?		New order is most likely, intermediate likely and least likely (also called A, B C).
- The location of travertine deposits is clearly provided in Figure 2. However, the authors make mention to individual deposits with not enough details for the reader to understand which deposit(s) the authors are referring to exactly. Moreover, no attempt is made to communicate the different age of the travertine deposits on the map, which is important for discussing the logic behind interpreting the timing and mechanisms of the CO2 leakage events. It is suggested that the authors either provide a way to distinguish between individual travertine deposits (by location and possibly age) within the text or the figures.	We have added a new figure (Fig 4 in the revised manuscript) to highlight the temporal evolution of CO2 leakage events as indicated by the travertine deposits.	New Figure (Figure 4 in the revised manuscript). Line 133f: In addition to the occurrences along the northeast tip of the Coyote Wash Fault (cluster A), travertine mounds follow the trace of the Buttes Fault over a distance of more than 7 km (cluster). Travertine mounds are also found northeast of the present-day extent of the CO_2 reservoir, with no clear link to other structural elements (cluster C). It is notable that there are no indications for fluid migration in the southern half of the reservoir.
- Although the authors make a logical case for leakage in the northern parts of the study area being related to the stress field acting on the unfavorably oriented Coyote Wash Fault, the possibility exists that CO2 point sources could have been located towards the northern half of the study area and influenced the pattern of travertines observed today? Some discussion on this could be fruitful.	A detailed study of the noble gas and stable carbon isotope geochemistry of surface water springs in the region showed that there is a direct link to the CO_2 contained in the natural reservoir below, indicating that there is only a single source of CO_2 in the area. Hence, the surface point sources of travertines, which are the individual springs, are linked to the deeper reservoir through leakage up faults and fractures, and the concentration of them in the northern area is more likely to be the result of greater degree of fracturing at the fault tip, than any different point source of CO_2.	No changes to the manuscript.

The figures in the manuscript are generally of good quality. While some minor adjustments could be made to Figure 2, several mapping errors were recognized after trying to georeferenced the map using the coordinate grid along its border. This was evident after plotting the SHmax points provided in Table 1 (aside from point ID 4 mentioned above), as the points on the map did not line up with the plotted points using the coordinates (see attached images for this review). WSM SHmax coordinates in Table 1 were verified with Heidbach et al. (2016), while points from Connor et al. (1992) could not be verified by reviewing the cited publication. If the Figure 2 map is georeferenced based on the WSM points, the map is distorted and the location of features (like LymanLake) do not align with satellite imagery. The same can be said if the Figure 2 map is georeferenced based on the Connor et al. (1992) points. The geographic features do align with satellite imagery if the map is georeferenced to the well locations (I obtained the well locations using ArcMap online data searching Arizona oil and gas wells). However, even though Lyman Lake and other features are aligned, the SHmax points on the map still don't match the points plotted using Table 1 coordinates. Since all the data should agree, it is suggested that the coordinates along the outside of the Figure 2 map and the Table 1 SHmax coordinates are reviewed compared to the geography from the satellite imagery and the well locations. Any errors or	We thank the reviewer for their suggestion and very detailed review of the figure. Unfortunately, as the reviewer highlights, there was a georeferencing error in the original figure for which we apologise. To correct this error we have now redrawn the figure in the revised manuscript and have also included additional data as suggested by the reviewer.	Updated and improved figure 2 in the revised manuscript Updated table 1 in the revised manuscript.

reference system discrepancies should be corrected in the manuscript. Furthermore, it is suggested that additional culture data, such as state boundaries or highways, are added to the map.		
All comments, suggestions, and corrections are compiled in the PDF document accompanying this review. Aside from the issues described above, most comments and suggested corrections were rather minor and are deemed easily addressed. Spelling mistakes and typographical errors were noted, but did not distract from the flow of the text. Time was put into correcting the format of the items in the reference list to match the SE style. On occasion, suggestions were made as an attempt to improve clarity or flow of the text. Minor suggestions of a similar nature were also made for figures. Finally, I would like to take the time to thank the subject editor and authors for the opportunity to review this manuscript. This concludes my frist review of the se-2020-12	We thank the reviewer for their thorough review and have addressed their comments and suggestions below.	
Title: controls on fault		This has been changed accordingly in the revised manuscript.
Line 11: deposits above		This has been changed accordingly in the revised manuscript.
Line 13: Here, we combine		This has been changed accordingly in the revised manuscript.
Line 15: stress fields and two interpreted fault rock types		This has been changed accordingly in the revised manuscript.
Line 16: existing in a fault damage zone and around a fault tip.		This has been changed accordingly in the revised manuscript.

Line 17: have controlled CO_2 leakage	Groundwater measurements as well as small scale travertine deposition at springs suggest that leakage is still ongoing. Thus we use “control”.	No changes.
Line 18: in situ ; “complex” A bit subjective. I would consider a different term.		This has been changed accordingly in the revised manuscript.
Line 21: I think there could be some more subsurface examples of gas chimneys, etc. cited here.	We thank the reviewer for their comment but do not see how citing subsurface examples of gas chimneys would be helpful for the introduction of this surface leakage site.	
Line 24: space between sources and (Alcalde..)		This has been changed accordingly in the revised manuscript.
Line 25: Might be misleading. How do you mean engineered? I assume you mean the integrity of the subsurface trap and seal, however, these are not engineered, They are naturally occurring. The only engineered part is the well(s) drilled through it.	We thank the reviewer for their suggestion. The term “engineered carbon storage site” is well accepted in the literature to describe a man-made subsurface CO_2 storage site. Furthermore, the engineered nature of CO_2 storage operations controls the pressure that the reservoir is subjected to, and the total volume of CO_2 injected for storage – both of these factors will strongly influence the integrity of the subsurface trap and seal.	No changes.
Line 27: Comma after e.g		This has been changed accordingly in the revised manuscript.
Line 33/34: Do you mean local stress field?	Here, as described the mechanics of faulting are implied: how the mechanical properties of the host rock, fault rock and the type of faulting influence the fault zone geometry, permeability etc. Naturally, the stress field plays a significant role.	No changes

Line 37: This section seems to only be talking about siliciclastics, what about carbonates, especially since the stratigraphic section contains some? Perhaps be more specific throughout the manuscript about this.	The reviewer is correct to point this out, we have adopted the section in the revised manuscript to highlight that we focus on siliciclastics as the majority of the stratigraphic column is siliciclastic. Additionally, predictive algorithms of how fault rocks are formed in carbonate rocks similarly to SGR in siliciclastics do, to our knowledge, not exist. Fault rock behaviour and sealing potential in mixed siliciclastic and carbonate sequences are indeed a field needing further research.	Line 36: In a widely used simple conceptual model for fault zones in siliciclastic rocks…
Line 39: The text over this topic seems incomplete, what about juxtaposition of low permeability units against a reservoir? Does fault rock matter then from a lateral migration standpoint? The accumulation could be trapped this way.	A similar comment has been raised by a community comment to our manuscript and we have added juxtaposition sealing as a lateral seal to this section. For the Coyote Wash Fault however, juxtaposition sealing only plays a role at high throws in the centre of the fault, and not in the critical areas where CO_2 migration is observed. We have added an Allan Diagram of the Fault as well as an SGR calculation to visualise the fault rock seals.	Line 41f: Lateral fluid migration across the fault zone is thus controlled (1) by the permeability and continuity of the fault gouge/rock within the fault core(s), which is dependent on the host rock composition, shear strain and faulting mechanism, as well as (2) the juxtaposition of strata across the fault (Yielding et al., 1997). Figure 5 in the revised manuscript.
Line 40: Also, structural diagenetic processes, such as cementation of fractures.	We have added this to the revised manuscript.	Line 40: if not diagenetically cemented
Line 41: Do you have a reference in mind here? It seems like this and the next sentence could benefit from citations instead of putting them all in the sentence starting on line 42.	In light of this valid suggestion, we have distributed the references in this section more evenly in the revised manuscript.	
Line 43: composition and continuity of fault gouges, as well		A comma has been added to the revised manuscript.

Line 46: fracture or deformation band networks (compaction bands are a type of deformation band , probably better to stick with the general term since your references don't distinguish between the different types)		This has been changed accordingly in the revised manuscript.
Line 50/51: If... then what? Consider rewriting this sentence		A comma has been added to the revised manuscript.
Line 52: Could use a reference.	The revised manuscript now has references for this point.	Line 54f: or fracture stability (Handin et al., 1963; Terzaghi, 1923) can be used to assess the potential of vertical fluid flow.
Line 67: How were fault rock types chosen and how were fault parameters determined (e.g., dip angle) for this work?	This is addressed in the methods part of the revised manuscript.	Line 183ff: As no outcropping fault rocks were available, the Shale Gouge Ratio (Yielding et al., 1997) was used as a fault rock proxy. SGR was calculated from a V_{shale} log of well 10-29-31, which was calculated from the gamma ray log assuming a linear response (Asquith and Krygowski, 2004). As this method only works for siliciclastic rocks, zonal V_{shale} values for evaporitic sequences (1% shale content assumed) were used. Resulting SGR values indicate a high potential of phyllosilicate rich fault rocks (Fig. 5). To emphasise the uncertainty regarding the fault rock composition, two different fault rocks were used for F_s calculations: clay smear (cohesion C=0.5 MPa, coefficient of internal friction μ=0.45) and phyllosilicate (C=0,5, μ=0.6) with rock strength values from the TrapTester® internal database. Line 173ff: A 3D geological model of the St. Johns Dome was built based on published

		geological maps (Embid, 2009; Sirrine, 1958), well data from 37 exploration and production wells available from the Arizona oil and gas conservation commission (well logs, horizon markers) as well as previously published reservoir horizon map and markers (Rauzi, 1999) using Move™. Between wells a constant stratigraphic thickness was assumed and for the fault a dip of 70° was estimated, based on previous works (Embid, 2009, Rauzi, 1999) and a 3D dip-domain construction (Fernandez et al., 2008) of the intersection of the fault trace with the 1/3 arc-second DEM of the 3D elevation programme of the USGS.
Line 68: space beteen 10^10 and m3		This has been changed accordingly in the revised manuscript.
Line 69: southeastern		This has been changed accordingly in the revised manuscript.
Line 72: Is there an active CO2 seep currently at the study area? Springs are there, but not not active in a similar way to something like Crystal Geyser. Change if not deemed active. I don't think it changes your story.	There are active CO_2 seeps at Salado Springs, even though they are smaller in volume than adjacent older travertine mounds. Crystal Geyser as a man-made CO_2 spring is not comparable to natural CO_2 seeps where degassing occurs quietly and unspectacularly. As previously cited, Gilfillan et al., 2011 found evidence for a geochemical link between dissolved noble gases contained in the water in the springs and those contained in the CO_2 stored in the deeper reservoir, and this link has been verified by further study of the groundwater chemistry by Keating at al., 2014	

Line 73: How so? If you mean perpendicular to the hinge of the anticline (limb to limb, west to east), Figure 7 makes it look symmetrical, I suggest adjusting the word choice here or adjusting the illustration of the anticline in Figure 7	The anticline has been described as asymmetrical by previous authors (Rauzi, 1999, Moore et al., 2005), however the reviewer is correct that it is not really an asymmetrical anticline and we assume that the term has been used as the western limb is more steeply dipping than the eastern limb.	Line 77: a broad, northwest-trending anticline
Line 73: How steep is it? Any outcrop evidence for this? How do you know it's steep? What angle do you use to model this later? These details are missing.	We thank the reviewer for their comment. Indeed, outcrop information on the angle of the fault is missing as there are no suitable outcrops. The fault has been classified as steep by previous works (Rauzi, 1999), and most faults in the area seem to have dip angles of 70° or steeper (Embid, 2009). We have added what angle we are using to the methods section of the revised manuscript.	Line 174ff: Between wells a constant stratigraphic thickness was assumed and for the fault a dip of 70° was estimated, based on previous works (Embid, 2009, Rauzi, 1999) and a 3D dip-domain construction (Fernandez et al., 2008) of the intersection of the fault trace with the 1/3 arc-second DEM of the 3D elevation programme of the USGS.
Line 74: EIther the wells produce from it or they don't. Is there a way to check this to be more certain. Also, doesn't this suggest the fault is creating a trap via juxtaposition of the reservoir onto downdropped caprock units?	There are limited numbers of wells on the western (downthrown) side of the fault (four) of which one has encountered CO_2 in the reservoir interval, while the other three have not. The well with CO_2 is spatially quite far from the fault (10 km) but located on a local high while wells closer to the fault are dry. This suggests that there may be across fault leakage with some CO_2 accumulating at the downthrown side of the fault. However, as the CO_2 may also have migrated vertically from beneath the Springerville Volcanic field the situation is not very clear. Thus we decide to use "the fault seems to form the western boundary" also in the revised version of the manuscript.	No changes.

Line 75: boundary of the		This has been changed accordingly in the revised manuscript.
Line 75: former		This has been changed accordingly in the revised manuscript.
Line 75: Measurements from outcrop to be certain of this, or modeling?	This is based on offset in Mesozoic strata on both sides of the fault as observed in the field by Embid (2009) and Sirrine (1958).	
Line 75: What kind of gas? CO2? If not, add some more text on this field.	Yes, CO_2.	Line 79: former commercially exploited St. Johns Dome CO_2 gas field.
Line 77ff: How did shortening in the Laramide create a normal fault? I would expect a reverse sense of movement in that case. Seems to me that this is more of a Basin and Range or Rio Grande Rift type structure, where extension lead to normal faulting and a footwall rebound anticline. A more complex, but valid, interpretation would be inversion of a Laramide reverse fault into a normal fault later in the Tertiary during Basin and Range/Rio Grande Rift events, which your reference explains well on a regional level. With that in mind, please add this detail to the text here, and readers familiar with the regional geologic history will be satisfied.	We thank the reviewer for their suggestion and have revised this part of the geological setting in the revised manuscript.	Line 81ff: The fault is thought to be related to Paleogene Laramide compressional tectonics which led to monoclinal folding of the Phanerozoic strata and the reactivation of older basement structures such as the Coyote Wash Fault on the Colorado Plateau (Marshak et al., 2000). The normal displacement of the fault suggests an inversion of the reverse fault related to the Basin and Range extension starting in the Early Miocene and continuing in the Pliocene as evident from displacement of Pliocene basalt flows (Embid, 2009).
Line 79: Again, if limestone is present, probably need to mention carbonate fault rocks (damage zones and cores) in the earlier sections of the manuscript. I may be important to distinguish that these may be mix sliciclastic and carbonate rocks in the stratigraphic column, which has implications on fault rock types.	We have addressed this in the revised manuscript.	Line 35: … fault zones in siliciclastic rocks… Line 182: Note that for modelling purposes we assume a siliciclastic sequence, however the stratigraphic sequence also contains ~15 % carbonate and evaporitic rocks (Fig. 3) which may have locally significant influence on the fault rock strength.
Line 80: No comma, dash not hypen		This has been changed accordingly in the revised manuscript.

Line 83: This should all be in order of oldest to youngest		This has been changed accordingly in the revised manuscript.
Line 85: and the reservoir not filled to spill		This has been changed accordingly in the revised manuscript.
Line 87: bordered ; Basin		This has been changed accordingly in the revised manuscript.
Line 88: en dash, not hyphen		This has been changed accordingly in the revised manuscript.
Line 89: 1958 according to the UT Austin Library.; I think this needs a reference.	The Arizona Geological Survey lists it as 1956, we still have changed it to 1958. We have merged the two sentences in the revised manuscript.	Line 95: The basaltic volcanic field consists of more than 400 individual vents and related flows, with the oldest volcanic activity dating back to around 9 Ma and the youngest flows, which can be found 8 km northwest of Springerville, to about 0.3 Ma (Condit et al., 1993; Condit and Connor, 1996).
Line 90: of about		This has been changed accordingly in the revised manuscript.
Line 94: basement and volcanic rocks?	There is no evidence of volcanic rocks in the subsurface.	
Figure 1: igneous rocks (should be lower case). The location of mapped Cenozoic igneous rocks seems a little incomplete. Where did the original data come from? USGS? Luedke and Smith, 1978? The maps from the cited literature does not cover it all. 2005 was a while ago, I think this map could be up.	We thank the reviewer for their suggestion and have added the appropriate citation to the figure caption (Aldrich and Laughlin, 1984). There may have been a more recent mapping of the late Cenozoic volcanic rocks on the Colorado Plateau, but as this map is only for illustration and orientation purposes we believe that it is not necessary to update it to include all volcanic rocks on the Colorado Plateau. We thus have updated the figure caption accordingly.	See Fig. 1 in the revised manuscript.
Line 98: Figure		This has been changed accordingly in the revised manuscript.

Figure 2: No fault here?	The occurrence of this travertine cluster is not completely understood. Based on field mapping there is no fault, however the two orientations found within the cluster (WSW-ENE and NW-SE) are similar to the two main faulting directions in the area. Additionally, the travertines in this area are among the oldest (outside U-Th dating range, so >500 ka). More research is needed to understand this cluster.	See updated Figure 2 in the revised manuscript.
All labels could be larger, State boundary, highway, or other culture would be very useful.	This has been changed accordingly in the revised manuscript	
This CO2 accumulation outline in the HW suggests some juxtaposition sealing, but you make no mention of it.	See our comment above. There is one well located more than 10km away from the fault which has CO2 showings. Outline of the CO2 field in that area is based on the structural contour of the GWC.	
Expl./prod. well (lower case), Should be SHmax (with max in subscript). Add year to stress map vintage (2016) should also have deg in upper corner of symbol like the WSM symbol, Should be SHmax (with max in subscript)., al., (needs a comma or 1992 in parentheses)		This has been changed accordingly in the revised manuscript.
There are four notable problems with this figure, some of which stem from the same issue. I find that these must be addressed so that the information can easily be used by other researchers. Locations must agree with each other. *1.) The map itself cannot be georeferenced properly using the coordinates provided, meaning they are incorrectly labeled.*	We thank the reviewer for their detailed review. Unfortunately, the map indeed was not properly georeferenced/using multiple coordinate systems. This has been addressed in the revised manuscript.	See updated Figure 2.

Geographical features in the map do not match up, among other things.		
2.) The location of Hmax points has several issues. I mapped the points based on coordinates in Table 1. WSM locations match as reported by Heidbach et al., 2016. Their generalized map pattern compared to Figure 2 checks out. However, the problems with the coordinate labels for Figure 2 place the map in such a location that the WSM points on the map don't match the location of the points in Table 1. For Connor et al., 1992 Hmax points, their pattern does not match the FIgure 2 mapping pattern when plotted using the provided coordinates in the figure for the map and the points in Table 1. Other than all of the points being shifted, similarly to what I described for the WSM points, point ID 4 (61 deg) is either too far north in the map, or the Table 1 coordinate is wrong. Another thing is that I cannot find the original list of coordinates in Connor et al., 1992 or subsequent publications. Their maps and figures (their Fig. 2, Fig. 5, and Table 2) contain some information, but it is not clear how their data was extracted and presented by you. I see one point that could potentially be linked to their data (your ID 4, their 061 I) and could match, but Connor and others do not include exact coordinates for these measurements. No other measurements listed by you seem to match the data in their Table 2 or maps. Is there another source for this information somewhere (i.e., a database for	Due to the georeferencing error most of the coordinates were misaligned. Additionally, we have added how we use the data provided by Connor et al. (1992) to get a Hmax azimuth location to the methods part of the paper.	Line 210: Note that the maximum horizontal stress (SH_{max}) from Connor et al. (1992) is based on vent clusters linearly aligned with lengths of 11 to 20 km length (Fig. 2) and that table 1 lists them as point measurements at the centre of the cluster.

these measurements from Connor et al., 1992)? I find this confusing and concerning.		
3.) It appears that if the map is georeferenced base on the location and pattern of mapped wellbores, the features in the map (like Lyman Lake) are located properly. However, this is not the case Hmax points for either the WSM or Connor et al., 1992 points.	This has been addressed in the revised manuscript and was the result of poor georeferencing.	See updated Figure 2.
4.) The scale bar is wrong with any georeferencing of the map I attempted. If the wells were used to georeference the map, the length of the bar is only about 17 km. Maybe there is some difference between the with the coordinate system or projection I'm using compared to you? I use WGS1984 with a UTM projection. See attached screenshots.	This has been addressed in the revised manuscript and was the result of poor georeferencing.	See updated Figure 2.
(SHmax), subscript max *World Stress Map (I think this is a proper noun)* *(1992)*		This has been changed accordingly in the revised manuscript.
Line 106: shown ; CO2 accumulations occur within		This has been changed accordingly in the revised manuscript.
Line 107: Expression and timing of fluid flow		This has been changed accordingly in the revised manuscript.
Line 108: This sentence is a bit long for a introductory sentence. Consider breaking it up into two at the end of line 108		Line 118f: The travertine deposits at the St. Johns Dome are an expression of CO_2-charged fluids migrating from the subsurface to the surface. Travertine formation occurs when CO_2-rich fluids outgas CO_2 as they migrate upwards to shallower depths and lower pressure, resulting in $CaCO_3$ supersaturation and carbonate precipitation.
Line 112: Unites States		This has been changed to North America in the revised manuscript.

Line 114: Springs but no bubbles. Does this actually mean the fault is leaking CO2? I find this term tricky.	There is active travertine formation (although small scale) so the fault is still leaking CO_2.	
Line 115: (Embid, 2009)		This has been changed accordingly in the revised manuscript.
Line 115: No indication of these in Figure 2.	The groundwater analyses were not part of this study but carried out by Moore et al (2005) and Gilfillan et al. (2011). We thus do not believe it to be necessary to include the groundwater wells sampled by these authors in our map.	
Line 117: Not necessarily mantle. Could just be deep Precambrian or from volcanic intrusions.	The noble gas isotopic signature is clearly showing a mantel source.	
Line 120: permeable zone through		This has been changed to conduit in the revised manuscript.
Line 123: mounds follow the trace ; Was there any attempt to run the same analysis on the Buttes Fault as you did for the Coyote Wash Fault? Some text on why this wasn't done should be added	We thank the reviewer for their suggestion. While a similar analysis on the Buttes fault would be very useful, the limited information on the fault (dip, displacement) makes it much less constrained than the Coyote Wash Fault. We are currently cooperating with scientists from the Brigham Young University, Utah, to use geophysical methods to constrain the nature of the fault. Thus a detailed fault analysis will hopefully be done in the future.	We have included the following text in the revised manuscript: Line 133ff: …travertine mounds follow the trace of the Buttes Fault, of which the subsurface extend is not well constrained, over a distance of more than 7 km (cluster B).
Line 124: northeast ; link to other		This has been changed accordingly in the revised manuscript.
Line 126: Which mounds? All?	We have added a new figure to the revised manuscript (Figure 4) to visualise the dated travertines and their age distributions.	See new Figure 4 in the revised manuscript.
Line 130: seep events interpreted	In order to produce a travertine deposit there must be a surface spring with degassing CO_2. As such we believe that travertines represent	

	surface seeps of CO2 and that there is no room for interpretation.	
Line 132: I would reword this. The recharge may have been episodic, rather than continuous. I think this word draws too closely to continuously; Needs a reference		We have changed this in the revised manuscript to "constantly or regularly". We have added a reference in the revised manuscript (Miocic et al., 2019a).
Line 133: by the travertine deposits; –, en dash		This has been changed accordingly in the revised manuscript.
Line 133: leaked (this Miocic et al., 2019a calculation is not for what is happening now, but calculated from 420 Ma and the total closure volume, not a calculation from what is happening now, more like until now)		This has been changed accordingly in the revised manuscript.
Line 134: This point seems weak. Wouln't CCS be risky if you inject, especially near the fault? Your results say so. Pressure control is important, also how much capacity is left at the site? Also, the evidence of leakage suggests this could happen more over time. Why risk it?	We thank the reviewer for their comment. It certainly depends on how risk is defined. We state that from the climate change mitigation point of view, where a reservoir is allowed to leak CO_2 to the surface as long as it is less than a fraction of the overall stored volume, the reservoir is a suitable storage site. Politically no leakage may be the goal, in which case the reservoir would be an unacceptable risk. This is why we have not changed this section in the revised manuscript.	
Line 134: location	See comment above.	
Line 136: has occurred	As there is still active travertine formation the leakage is ongoing. See Priewisch et al., 2014 and Embid, 2009.	
Line 137: What are stable? The faults or the pathways? Maybe a poor word choice considering it is also used to describe faults from a geomechanical standpoint. Also not clear what you mean by spatially.	We thank the reviewer for pointing this out and have changed the wording in the revised manuscript to "spatially fixed".	Line 155f: These observations illustrate that fluid migration at the St. Johns Dome occurs along fault zones and once migration pathways have been established they are spatially fixed for long periods (>100 ka).

Line 137: fault-controlled		This has been changed accordingly in the revised manuscript.
Line 138: or "a few thousand years"		This has been changed accordingly in the revised manuscript.
Line 139: fault movement		This has been changed accordingly in the revised manuscript.
Line 140: in Italy		This has been changed accordingly in the revised manuscript.
Line 141: Again, seems like poor word choice here.		This has been changed accordingly in the revised manuscript.
Line 141: My point exactly about taking out the last sentence of the previous paragraph.		This has been changed accordingly in the revised manuscript.
Line 145: Perhaps subscript is better for s?		This has been changed accordingly in the revised manuscript.
Line 149: leave out, faults are usually not planes		This has been changed accordingly in the revised manuscript.
Line 151: Perhaps subscript is better for this s too?		This has been changed accordingly in the revised manuscript.
Line 154: How did you constrain the geometry of the fault zone? I assume the strike was contstined by the fault trace on published maps at the very least, but were there any outcrop measurements to constrain it further. Also how was dip angle constrained? Was it a 60 degree assumption or are there outcrop measurements? I view these as important parameters and aspect of your model since fault stability is sensitive to fault orientation in a particular stress field. It is safe to assume the fault is one large fault or a linkage of smaller ones?	We thank the reviewer for their comment. Indeed, the dip of the fault is critical for the geomechanical modelling. Unfortunately, there are no outcrops on which the dip of the fault could be measured. We used a 70° dip based on previous works – however we did also conduct a 3D dip domain reconstruction of the dip based on the fault trace and a DEM. While the DEM is not of the highest resolution and the dip domain construction does not work along most parts of the fault trace, in several areas a fault dip between 65 and 75° could be constructed. We have added this to the manuscript accordingly.	Line 176f: Between wells a constant stratigraphic thickness was assumed and for the fault a dip of 70° was estimated, based on previous works (Embid, 2009, Rauzi, 1999) and a 3D dip-domain construction (Fernandez et al., 2008) of the intersection of the fault trace with the 1/3 arc-second DEM of the 3D elevation programme of the USGS.

References?	We have added references to the text accordingly.	Line 173f: A 3D geological model of the St. Johns Dome was built based on published geological maps (Embid, 2009; Sirrine, 1958), well data from 37 exploration and production wells available from the Arizona oil and gas conservation commission (well logs, horizon markers) as well as previously published reservoir horizon map and markers (Rauzi, 1999).
Hydrocarbon or CO2?	The first exploration wells were drilled for hydrocarbons, with the later and majority for CO_2. We do not think that it matters to the reader as to whether they were drilled for hydrocarbons or CO_2.	
Line 155: References?	We have added references to the text accordingly.	See Line 170f above.
Line 158: These fault rock types have fairly clear definitions that should be noted somehwere in the manuscript. The main difference between them being depth of burial. How do you substantiate these assumptions/predictions? Was field work done? In other words, why did you choose these in particular? If no field work was carried out, how did you determine these fault rocks for modeling? This very important. Why not consider deformation bands as fault rock type since you mentioned them already? Could you also provide information about depth of burial over geologic time? Were the carbonate-rich layers taken into consideration? The choice in fault rock affects the result of the modelling greatly	We thank the reviewer for this suggestion and have added reasoning of why and how fault rock types were chosen to the revised manuscript. As discussed above, there are no outcrops available at which fault rocks (or dip of the fault) can be analysed. However, due to the high content of shale and silt within the stratigraphic column, phyllosilicate-rich fault rocks are the most likely. Calculations on the Coyote Wash Fault show high to very high Shale Gouge Ratios, indicating that there is a high potential of phyllosilicate fault rocks. One shortcoming is that the used approach focuses on siliciclastic fault rocks even though there are small amounts of carbonates and evaporitic layers (<15 %) within the	Line 187ff: As there are no outcropping fault rocks were available, the Shale Gouge Ratio (Yielding et al., 1997) was used as a fault rock proxy. SGR was calculated from a Vshale log of well 10-29-31, which was calculated from the gamma ray log assuming a linear response (Asquith and Krygowski, 2004). As this method only works for siliciclastic rocks, zonal Vshale values for evaporitic sequences (1% shale content assumed) were used. Resulting SGR values indicate a high potential of phyllosilicate rich fault rocks (Fig. 5). To emphasise the uncertainty regarding the fault rock composition, two different fault rocks were used for Fs calculations: clay smear (cohesion C=0.5 MPa, coefficient of internal friction μ=0.45) and phyllosilicate (C=0,5, μ=0.6) with

	stratigraphic column. However, as far as we are aware, predictive tools similar to SGR which predict fault rock types for siliciclastics are not available for mixed lithologies. We have highlighted this in the revised manuscript. We have commented on the burial history further below.	rock strength values from the TrapTester® internal database. Note that for modelling purposes we assume a siliciclastic sequence, however the stratigraphic sequence also contains ~15 % carbonate and evaporitic rocks (Fig. 3) which may have locally significant influence on the fault rock strength.
Clay Smear		This has been changed accordingly in the revised manuscript.
C=0.5 MPa, not C = 0.5 MPa, or change similar occurances in manuscript.		
Can provide your reseasoning for using these values? References or experimental data? Seem arbitrary without some defined basis for the values, although they are actually reasonable	These values are from the TrapTester internal database, which is based on literature and oil and gas industry internal data. This has been indicated in the revised manuscript.	See Line 187ff above.
Line 159: the current, no hypen		This has been changed accordingly in the revised manuscript.
Line 160: World Stress Map, not world stress map?		This has been changed accordingly in the revised manuscript.
Line 162: Similar to 2.1, is the use of 3.1 necessary without additional sub-sections?	We feel that this section is different from the previous section as it is discussing input data for the geomechanical modelling and as such should have a different heading.	
Line 163: within the greater Basin and Range		This has been changed accordingly in the revised manuscript.
Line 165: World Stress Map		This has been changed accordingly in the revised manuscript.
Line 166: I would switch these around		This has been changed accordingly in the revised manuscript.

Line 170: Might brackets be better on the inside instead of having parentheses within parentheses? SE may have a system		This has been changed accordingly in the revised manuscript.
Lin1 171: as based on the World Stress Map		This has been changed accordingly in the revised manuscript.
Line 175: Tab. 1		This has been changed accordingly in the revised manuscript.
Line 176: stress field		This has been changed accordingly in the revised manuscript.
Line 177: switch C and B	We thank the reviewer for their comment and have changed the cases B and C throughout the revised manuscript.	
Line 178: solitary		This has been changed accordingly in the revised manuscript.
Line 179: most- (A), intermediate- (B) and least-likely (add hyphens, the order of B and C should be swtiched to be more logical, also I feel that the phrase 'intermediate-likely' is very awkward sounding and would consider revising it)		This has been changed accordingly in the revised manuscript. Intermediate-likely has been replaced with moderately-likely.
Table 1: Stress Map, 5 & 6, 7 & 8, not 5&6, 7&8, subscript Shmax and Shmin		This has been changed accordingly in the revised manuscript.
Might want to change the order of these so reflect B and C being switched. See comments for Table 2. If the final SE tables aren't reformatted, the line thickness and colors in both tables 1 and 2 are inconsistent. I say keep them all the same thickness and color		This has been changed accordingly in the revised manuscript.
Heidbach et al., (replace all in the table to maintain parallel structure with Connor et al., 1992)		This has been changed accordingly in the revised manuscript

Table 2: I suggest swtiching the intermediate and least fields here and in the text as it seems more logical		This has been changed accordingly in the revised manuscript
Line 190: One significant discussion topic that is missing here is about the fault rock type with respect to its implications on fault stability analysis. *Interpretation/determination/prediction of the fault rock influences the modeling parameters chosen, and has a major impact on the results. I view that addressing this is vital for this disucssion to be complete.*	We have expanded the fault rock section of the methods chapter which clearly indicates why we chose the according fault rock types.	Line 175ff (see above).
Line 194: Or maybe 'failure'? 'slipping'? Again, the clusters of B and C should be switched. Seems strange to describe these as A, C , and B here. Make sure to go through tables and other text to adjust all.		This has been changed accordingly throughout the revised manuscript.
Line 195: Again, age and burial depth of the rocks matter. How does this change from north to south along the fault? Looks like most of the travertine is located on top of older Triassic rocks, with potentially greater burial depths and possibly with fault rocks falling into the phyllosilicate category. Also, the largest of the travertine deposits seems to be located directly north of a preserved basalt mass. Could it be baffeling and directing the co2 to the north? Could there be a reason why the basalt is still there in relation to leaking CO2?	We thank the reviewer for their comment on burial history as well as overburden. Indeed, the burial history is important when it comes to the reconstruction of stress history and likely fault rocks. However, there are no reconstructions of the burial history available (i.e. vitrinite reflectance data) for the study area. Regional studies suggest an uplift of 2-3 km of the southern Colorado Plateau during the last ~50 Ma, but we do not believe that there is a strong gradient within the St. Johns Dome area – i.e. we assume a similar uplift (and burial) rate for the whole area. As the reservoir itself is of Permian age, it will have seen the same burial history throughout the field. The whole reservoir is covered by Triassic rocks, which in	

	the SE part of the reservoir are covered by (thin) Jurassic and younger sediments. The irregular erosion is due to the incision of the Little Colorado River during the last few million years and we believe this has not a significant influence on the uplift/burial history. Certainly, the lava flows act as some kind of flow baffle as they are likely of lower permeability than the sedimentary rocks. However, they do not seem to control the fluid flow from underneath: If CO_2 were to migrate vertically into the sedimentary rock layers underneath the lava flows (e.g. the one next to Lyman Lake), it would migrate upwards along the Cedar Mesa anticline and should surface to the SE of the basaltic lava flow where it intersects with the anticline. This is not the case, instead CO_2 occurs on the down-flow side of the lava flow. This highlights that migration occurs along the damage zone of the Coyote Wash fault and not through the sedimentary rock sequence – as documented in our manuscript.	
Line 197: Seems like you've moved towards using abbreviated directions compared to earlier in the manuscript. Consider being more consistent throughout.	We have changed this where we feel it does not negatively affect the readability of the manuscript.	
(Saldo Springs)?		This has been changed accordingly in the revised manuscript.
Line 205: What is lacking is a structure map or figure showing the geomodel for the reader to verify these details. For instance, the extent of these potential CO2 columns is not indicated	We have updated figure 2 in the revised manuscript illustrating the geomodel with the top reservoir horizon.	See revised figure 2.

anyway in your figures. Perhaps this could be shown in the map in Figure 2 as structural contours of the top reservoir or in FIgure 3 if that figure was split in two. I think this would add value to the manuscript since some of your results are so dependant on the geomodel and first few figures are only updated (or recycled) versions of previously published information. No geomodel from this study area has been presented before.		
Fault instead of reservoir		This has been changed accordingly in the revised manuscript.
Line 208: Where are these in Figure 2? Saldo Springs or further NE? It's hard to keep track of which deposits you are talking about. I see four groups of travertine deposits in Figure 2. One definitely inside the current CO2 accumulation (along the Cedar Mesa Anticline), two on the edge (Saldo Springs and Buttes Fault), and outside to the NE. Are the Saldo Springs and Buttes Fault clusters considered to be outside the current CO2 accumulation extent? To help the reader understand the distribution of these older travertines compared to younger ones, can you state whether there are older travertines (of similar age to the outside ones, not sure which they are, Saldo Springs?) located within the current extent of the CO_2 accumulation, and where they are on Figure 2 (e.g., along the Cedar Mesa Anticline)? In other words, are there only younger travertines inside the current CO2 accumulation extent and older travertines throughout all study area	We thank the reviewer for their comment. We have added an additional figure to the revised manuscript (Figure 4 in the revised manuscript) where we show the travertine clusters and their ages with respect to the reservoir extent. Additionally, we have added information to the “Expression and timing of fluid flow” section to highlight where these travertine deposits are found.	New Figure (Fig. 4), Line 129ff: In addition to the occurrences along the northeast tip of the Coyote Wash Fault (cluster A), travertine mounds follow the trace of the Buttes Fault, of which the subsurface extend is not well constrained, over a distance of more than 7 km (cluster B). Travertine mounds are also found northeast of the present-day extent of the CO_2 reservoir, with no clear link to other structural elements (cluster C).

locations? These observations have implications towards the lifetimes of these leakage points and to where the faults are leaking. Is it generally at the tips of the faults? Potentially something to add.		
What about the possibility of other but extinct point sources of the gas in these locations? Couldn't the location of the travertines just be attributed to CO2 sources located in the northern half of the study area? Also, is there a possibility that there was some partitioned migration through the overlying stratigraphy away from the fault contributing to other travertine deposits?	We have addressed the reviewers comment on point sources above.	
Something your geomodel could tell us is whether or not you think the CO2 would reach the location of these older travertines if another 160 m of column were added to substantiate your hypothesis. I think this would be worth adding to increase the value of the discussion.	We have updated figure 2 in the revised manuscript to illustrate the 3D model.	
Line 211: Do you have any thoughts on how the original accumulation got so big before leaking and how it continued to leak despite the pressure reducing along the Coyote Wash Fault, even with CO2 charge occurring over the lifespan of the travertine deposits? *Your interpretation of the different travertine ages implies that there were multiple episodes of charge and leakage. Correct? The leakage has been episodic then. Does fault valve theory apply here? Is there more evidence for fault healing or strain hardening along the Coyote*	We thank the reviewer for their comment. This is indeed one of the questions we are still looking to answer. Currently our preferred theory is the following (similar to the reviewers): The reservoir slowly filled and the initial reservoir had a gas column ~150m higher then the current one. This triggered leakage along the Coyote Wash Fault, the Buttes Fault and an unidentified fault in the NE (where the oldest travertine deposits can be found [>500 ka]). Subsequently, the reservoir shrank with more or less continuous leakage occurring along the	We have not added our unproven hypotheses to the revised manuscript as there is no clear understanding of this issue as of yet. We hope that future work on the St. Johns Dome will address this.

Wash Fault? My feeling is that the accumulation was originally large, then the first leakage event reduced the stability of fault and smaller pressure increases were necessary to initiate leakage again. There must be some reasons why the reservoir is ~100 m smaller, even with sufficient charge.	critically stressed faults. One option is that there were massive leakage events which released pressure within the reservoir to have sealing faults again (i.e. fault valving). Another option is that once a leakage pathway was created due to overpressure and geomechanical instability it stays open even if the fault is not critically stressed anymore – what exactly could keep the fluid pathways open is not clear yet. More detailed analyses of the travertine ages could help to shed some light on whether there were pulses of leakage or continous leakage – something which is planned in the future. Excavations of the faults could help to understand their properties – this could be future work following the ongoing geophysical work at the site.	
Line 215: Might not be strictly continuous, but the period of episodicity could be short. Furthermore, do you think there is a point source or area of higher CO2 charge along the fault or in the area of the dome? Such could play a role in travertine locations and fault perturbation. Again, safer to say from more local features, like the volcanic intrusions.	This is an interesting idea raised by the reviewer. Unfortunately, there is no way to test how the continous or episodically the influx of CO_2 into the reservoir has been – other than dating the travertine deposits on surface which however may not be concurrent. The infux of CO_2 into the reservoir has been suggested to occur through the highly fractured granite. There are no indications of point sources and we believe that the filling of the reservoir occurs over a larger area by flow through the fractured subsurface – and that the Coyote Wash Fault may form one of these pathways.	

Figure 4: What is the number of fault measurements and how were they derived? Also, the stereo plots indicate the fault dip was about 75 degrees. Is this true? The fault parameters must be shared with the reader	We have updated the figure in the revised manuscript and have added sections on how the fault parameters to the revised manusript.	Figure 6 in the revised manuscript. Line 176ff: for the fault a dip of 70° was estimated, based on previous works (Embid, 2009, Rauzi, 1999) and a 3D dip-domain construction (Fernandez et al., 2008) of the intersection of the fault trace with the 1/3 arc-second DEM of the 3D elevation programme of the USGS.
Line 219: Very difficult to see. Also, are these extracted from the grids/mesh triangles/verticies? More information about the model is needed for the reader to know where these values are coming from. For instance, how big were the cells and why was that resolution chosen?	We have added the needed information to the revised manuscript and have changed the figures accordingly.	Line 174: The modelled fault has 6635 faces constructed from 3525 vertices.
Line 220: Switch C and B		This has been changed accordingly in the revised manuscript.
Line 221: Where? I don't see these indicated anyway in this figure.		We have added the NW fault tip to the Figure in the revised manuscript.
Line 226: A& B?		This has been changed accordingly in the revised manuscript.
Line 228: 10's of		This has been changed accordingly in the revised manuscript.
Line 229: Evidence could have been eroded from the record, though	This is unlikely as the NW area of the St. Johns Dome has seen deeper erosion over the last ~500 ka (Embid, 2009) and there travertines older than 500ka have not been eroded	
Line 232: delete after comma.		This has been changed accordingly in the revised manuscript.
Line 234: Vertical migration of fluids through fault and fracture		This has been changed accordingly in the revised manuscript.
Line 235: Ogata et al is missing in references		This has been changed accordingly in the revised manuscript.

Line 237: , particularly at their northeastern tips.		This has been changed accordingly in the revised manuscript.
Line 238: fracture networks along the anticline structure		This has been changed accordingly in the revised manuscript.
Structural contours of the top reservoir surface would give the reader an idea of the geometry of the anticline	We have added a figure of the structural contours of the top reservoir horizon	See Figure 2 in the revised manuscript.
Line 239: Just as an observation, the tip of the Coyote Wash Fault is in the vicinity of a large basaltic body east of Lyman Lake. Has this igneous body acted as a barrier to CO2 migration since there are no travertines mapped on the basalts (as far as I can tell by the map) and the large travertine deposits are located north of it? Might be worth mentioning.	We have addressed this in an earlier comment above (Line 195).	
Line 242: pathways observed		This has been changed accordingly in the revised manuscript.
Line 243: Do you mean here or in general. Please clarify.		This has been changed accordingly in the revised manuscript.
Line 244: But by how much do you propose? This suggestion may have good intention, but one is then mapping (interpreting) beyond observation without constraints. Since you have not provided a deeper discussion on this topic, consider changing the language to be even more suggestive than it is stated here. Or, discuss further if you wish.		Changed "should" to "could" in the revised manuscript.
Line 246: Again, please provide text as to why this fault was not modeled somewhere in the manuscript		See comment above.
Line 246: Buttes Fault; Provide the reader more information about this fault to substantiate this		Line 288: Similarly, faults with low displacement such as the Buttes Fault, for which significant

claim. What is the current maximum normal displacement along the Buttes Fault? 30 m (Coyote Wash Fault) is detectable with seimsic, so I'm assuming it will be lower than this.		fault related leakage has been recorded but is thought to have a maximum displacement of <25 m, may not be detectable on seismic data.
Line 247: considered (How can you identify these, for instance, with seismic data? "considered		This has been changed accordingly in the revised manuscript.
Line 248: Ensuring that small faults are mapped or extending fault tips is not a product of having a good understanding of any particular storage site on its own. Assuming all geologists are equipped properly, a good understanding of the geology is dependent on the data availability, quality, and resolution. Observations are made from the data and it is interpreted. Although I see the point you are trying to make (an interesting one about interpretation philosophy), your conclusion comes off as a plea for one to extend their interpretations further without providing any caution or suggesting a limit to it. I'm not sure this conclusion is strong enough as is. Additionally, you have not alluded to this subject in your abstract or introduction. I suggest you to reworded the sentence or left it out since the sentence starting on line 282 is more appropriate.	We have added some more information on how we envisage to include fault tips and small faults into the geological model in the revised manuscript.	Line 289f: This highlights the need for a good structural understanding of any geological storage site to ensure that fault tips and small faults are considered and incorporated, possibly as an additional uncertainty parameter, into the geological model.
Figure 5: Switch, see above comments on these. stress, not stress (change throughout entire figure)		This has been changed accordingly in the revised manuscript.
Line 250: Still a bit difficult to see these. Could they be shown differently? Perhaps make bigger by swtiching the axes to make the figure	We thank the reviewer for their suggestion and have changed the layout of the figure accordingly.	See Figure 7 in the revised manuscript.

skinnier (fault rock on top and stress field on side).		
Figure 6: clay smear *Is this change in Fs because of a change in fault orientation here?* *I recommend labeling these fault cutoffs by their stratigraphic surface* *You have indicated vertical exaggeration, but why not put a vertical scale instead? It would be clearer for the reader to interpret the scale*	We have added a new figure to the revised manuscript (Fig. 5 in the revised manuscript), which highlights the fault cutoffs and SGR calculations. Additionally, we have added vertical scales to the figure.	New Figure 5 and updated Figure 8 in the revised manuscript.
Line 254ff: clay smear, switch stress fields		This has been changed accordingly in the revised manuscript.
Figure 7: Label as A. *Couldn't CO2 be coming from the basalt too since it is the material that is degassing rather than the crystallyne basement? It can be coming from the basement too.* *Label as C.* *Consider using another color, such as blue for the CO2 migration arrows and accumulation. Green is typically used for oil in the petroleum industry.*		This has been changed accordingly in the revised manuscript. The figure has been amended accordingly. See Figure 9 in the revised manuscript.
Line 259: has been (can you prove it's ongoing?)		This has been changed accordingly in the revised manuscript.
Line 263: Figure, I think readers would prefer a vertical scale in the diagram instead. Is there some reason in opting for this?		This has been changed accordingly in the revised manuscript.
Line 265: in situ		This has been changed accordingly in the revised manuscript.
Line 266: e.g.,		This has been changed accordingly in the revised manuscript.

Line 269: plausible instead of potential; I believe there may be a few recent publications related topic		This has been changed accordingly in the revised manuscript.
Line 271: This was not undertaken in this study. Although your point is entirely valid, maybe this should be left out to keep in line with your study or expressed in some other way.	We believe that this is an important point to raise and that is should be part of the discussion.	
Line 278: area is also located *Line 282: complex regional setting*		
Line 283: thorough site selection criteria, CO_2 instead of fluid; What about adequate data?	We have changed good to thorough and have added "adequate data" to the revised manuscript. However, we believe that this comment is also true for other types of fluid storage and not only CO_2 and thus we have kept the term fluid storage in the revised manuscript.	Line 329f: .. thorough site selection criteria for engineered fluid storage sites and adequate geological data to ensure that only reservoirs with well understood structural frameworks are chosen.
Line 284: Frameworks		See above.
Line 285: geological CO_2 storage	We believe that several of the lessons learned from this natural analogue for CO_2 storage also apply to other types of subsurface fluid storage and thus kept the original heading in the revised manuscript.	
Line 287: near-critically stressed	This has been changed accordingly in the revised manuscript.	
Line 288: Reword based on comments, Not necessarily	We have changed the wording in the revised manuscript. However, noble gas isotopes clearly show a mantle source.	Line 332f: We propose that regular filling of the reservoir with CO_2 from mantle sources increased the pore pressure within the reservoir and further reduced the stability of near critically stressed faults

Line 290: I think CO2 storage here seems risky given your results and narrative. Moreover, I'm not really seeing why this is being mentioned much. Details and discussion are too few. Maybe more discussion would help. However, the site is onshore, which is often logistically unattractive. Also, capacity seems too low given the injected CO2 would never sustain a superciritcal phase because of the reservoir depth (P-T conditions).	We thank the reviewer for their comment. The emphasis here is on the term "for climate mitigation" as discussed in a comment above. We do not say that CO_2 storage should take place at St. Johns, but that even though there is leakage, this would not render it a poor storage site on a climatic basis. Onshore sites can be quite attractive as there often is short transport associated. E.g. there is a coal fired power plant located at the St. Johns Dome. The volumes of CO_2 that could theoretically be stored at the St. Johns Dome are rather large (100s to 1000s of Mt CO_2) - even at the shallow reservoir depth.	
Line 291: Impede physically, operationally, or socially? Unclear. Cross out last part of sentence: This seems way off-topic with the rest of the manuscript. Better to keep out		This has been changed accordingly in the revised manuscript.
Line 293: Same as above line		This has been changed to "other fluids" in the revised manuscript.
Line 298: This seems a bit regressive. I suggest leaving this out since this thesis is focused on CO2 and other authors have made this point in the past.		This has been changed to other fluids in the revised manuscript.
Line 301: Travertines, yes, but I also see a tremendous value in studying the fault rocks in outcrop to better constrain the models. Don't you? A primary shortcoming of this manuscript the lack of outcrop data and outcrop-based modeling	We very much agree with the reviewer. Outcrop based data would very much improve the modelling results – unfortunately we (and previous workers) were unable to locate suitable outcrops.	
part of the faults		This has been changed accordingly in the revised manuscript.

Line 312: Ogata et al., 2014 must be added.		This has been changed accordingly in the revised manuscript.

Short comment 1 (Mark Mulrooney)

Reviewer Comment	Author replies	Changes to the manuscript
	We would like to thank Mark Mulrooney for his short comment which has helped to improve our manuscript.	
Line 21: Perhaps mention fault valving theory and how pore pressure is such a fundamental control on reservoir integrity.	We thank the reviewer for their comment and have added this to the introduction of the revised paper.	Line 54f: This so called fault-valve behaviour, where faults act as highly permeable pathways for fluid discharge, is particularly likely for faults that remain active while unfavourably oriented for reactivation within the prevailing stress field (Sibson, 1990). Geomechanical parameters such as slip tendency (Morris et al., 1996) or fracture stability (Handin et al., 1963; Terzaghi, 1923) can be used to assess the potential of vertical fluid flow. The latter considers pore pressure which is a critical parameter controlling reservoir integrity not only with regards to fault weakening (Hickman et al., 1995) but also with respect to the integrity of the caprock (Caillet, 1993; Sibson, 2003).

Line 24: Space before (Alcalde)		This has been changed accordingly in the revised manuscript.
Line 29: "In the Vicinity is a bit vague". Probably better to be more exact here, i.e., " intersecting or bounding the storage formation" Perhaps shorten and merge with the following sentence.		This has been changed accordingly in the revised manuscript.
Line 33: vague, Probably worth mentioning cross-fault juxtaposition as a control.		This has been changed accordingly in the revised manuscript.
Line 35: A bit overly simplified ... the most abundant DZ features can be litho-controlled ... can have deformation bands, veins, stylolites, secondary slip surface ect. Perhaps using a more general term like "secondary structural discontinuities" rather than "fractures" is more inclusive.		This has been changed accordingly in the revised manuscript.
Line 40: cross-fault juxtaposition?		This has been changed accordingly in the revised manuscript.
Line 41: Could you simply say "fracture permeability"?		This has been changed accordingly in the revised manuscript.
Line 45: cross-fault juxtaposition? burial/uplift history ...chemical and mechanical cementation ... in situ stresses		This has been changed accordingly in the revised manuscript.
Line 51: pore pressure, vertical fluid flow		This has been changed accordingly in the revised manuscript.
Line 55: extent		This has been changed accordingly in the revised manuscript.
Line 64: At this site,		This has been changed accordingly in the revised manuscript.
Line 61: Missing a research statement here, give the reader a reason to keep reading	This has been changed accordingly in the revised manuscript.	Line 69f: We show that leakage locations are controlled by the orientation of the reservoir bounding fault with respect to the regional stress field.

Line 70: between?		This has been changed accordingly in the revised manuscript.
Line 73: intersected		This has been changed accordingly in the revised manuscript.
Line 75: Has this been mentioned earlier in the text? .. if not, it perhaps it should be, or at least name the field here		This has been changed accordingly in the revised manuscript.
Line 80: "in gas state"		This has been changed accordingly in the revised manuscript.
Line 81: laterally or vertically?		This has been changed accordingly in the revised manuscript.
Line 83: Opposite order would make more sense		This has been changed accordingly in the revised manuscript.
Line 84: Which consists		This has been changed accordingly in the revised manuscript.
Line 86: hyphenate filled-to-spill. "outcrops primarily consist of ... ; age of volcanic rocks?		This has been changed accordingly in the revised manuscript.
Line 87: northwest, bordered		This has been changed accordingly in the revised manuscript.
Line 98: necessary when already labeled?		This has been changed accordingly in the revised manuscript.
Line 103: not necessary to have the ref. in both the legend and the figure caption		This has been changed accordingly in the revised manuscript.
Line 105: Generalized.. or just omit		This has been changed accordingly in the revised manuscript.
Line 107: Long sentence, I suggest starting with " Travertine formation occurs when ...." then in a second sentence you can state, "As such, the travertine deposits at the St. Johns Dome are an expression of ....." or similar.		This has been changed accordingly in the revised manuscript.
Line 112: Better to use a geographical term, i.e., North America?		This has been changed accordingly in the revised manuscript.

Line 117: taken/collected; trace are influenced		This has been changed accordingly in the revised manuscript.
Line 121: "suggesting instead that faults have controlled localized fluid flow"		This has been changed accordingly in the revised manuscript.
Line 129: Perhaps remind the reader.		This has been changed accordingly in the revised manuscript.
Line 131: Volumetric		This has been changed accordingly in the revised manuscript.
Line 134: Can you state what the storage capacity is?	We have added this information to the revised manuscript.	Line 153: of the reservoir volume (1900 Mt CO_2)
Line 141: Spatially and temporally,	No comma is needed here.	This has been changed accordingly in the revised manuscript.
Line 142: "herein" or "below		This has been changed accordingly in the revised manuscript.
Line 143: No mention of pore pressure regime. I would also like to know the resolution of the fault surfaces in the geomodel, i.e., the size and geometry of each vertice that makes up the fault upon which you drape fault stability and slip stability .	Unfortunately, no pressure data is available and thus hydrostatic pressure was assumed. We have tried to calculate pore fluid pressures by using the density of drilling fluids – however due to the shallow nature of the reservoir there was not much variation drilling fluid density and we were unable to create pressure profiles for water and gas legs. We have added the information on the geomodel and fault to the revised manuscript.	We have added the following to the revised manuscript: Line 180: Due to lack of pressure data, a hydrostatic pressure gradient is assumed (0.0105 MPa/m). Line 178: The modelled fault has 6635 faces constructed as triangles from 3525 vertices.
Line 152: Cohesion and the angle of internal friction/coefficient of static friction ... as stated below.		This has been changed accordingly in the revised manuscript.
Line 159: For the stress fields no in situ measurements were available		This has been changed accordingly in the revised manuscript.
Line 165: reference figure 2		This has been changed accordingly in the revised manuscript.

Line 166: (Table 1: Connor et a.)		This has been changed accordingly in the revised manuscript.
Line 167: the maximum horizontal stress		This has been changed accordingly in the revised manuscript.
Line 168: of the reservoir the Shmax orientation		This has been changed accordingly in the revised manuscript.
Line 171: Different styles to denote hierarchy?		This has been changed accordingly in the revised manuscript.
Line 193: What is the significants of an incremental change in this ... i.e., can the Ts number be calibrated to something like Critical perturbation pressure?	We thank the reviewer for their comment. As T_s is the ratio of shear stress to normal stress an incremental increase in Ts indicates that shear stress is increasing compared to normal stress – and thus the fault is closer to failure. There is likely a way to calibrate Ts against critical peturbation pressure (similar to Chiaramonte et al., 2008) – however that would take away the simplicity of the Ts approach.	
Line 205: filled-to-spill		This has been changed accordingly in the revised manuscript.
Line 219: the mesh is strange for a stereonet, .. looks more like a rose diagram; Can't actually see that these are crosses - Can you explain the distribution (stripe-like)? .. also what is each black cross representing? - presumably individual vertices of the fault?	We have updated the figure to be more clear in the revised manuscript.	See figure 6 in the revised manuscript.
Line 250: You have presented Ts results as stereonets and Fracture stability results as Mohr circles, and colour drapes on fault planes - is there a reason for not being more consistent with visualisation of the results, and if so could it be stated in the methodology.	We thank the author for their comment. While Ts is a ratio which does not translate easily to column heights Fs can be directly translated to column heights – but also makes assumptions about the fault rock properties. However, both approaches are important (as are others not	Line 193ff: T_s results are presented using stereonets as this here the reader can readily visualise how changes in the stress field orientation would influence fault stability while F_s results are presented on a Mohr circle as this allows a direct visualisation of how much the pore pressure needs to change to force

	used in the manuscript) to identify critically stressed areas of a fault. Using the stereonet to illustrate Ts has the advantage that one can easily see how changes in stress field orientation would influence the fault (i.e would it move more into "red" areas?) which we believe is important in this case as there are some uncertainties associated with the stress fields. Fs plotted on a Mohr circle directly allows to visualise by how much the pore pressure would need to be increased to force the fault into failure. Supporting this with colour drapes on a fault plane allows to visualise where on the fault these critical areas are.	different parts of the fault into failure. It also allows the reader to see how changes in fault rock strength could change the pore pressure needed for fault failure.
Line 256: Usually called "cut-off lines" or "Fault-horizon intersection lines" Further, there is no mention in the Methodology how these were computed.	Cut-off-lines were calculated by projecting the dip direction of horizon triangles/faces within a patch of 200m onto the fault (e.g. Yielding & Freeman, 2016).	We have changed this in the revised manuscript. Line 179f: Cut-off-lines were created on the fault surface by extracting the dip from a 200 m wide patch of the horizon of interest on either side of the fault and projecting this along the dip-direction until it intersects with the fault (Yielding and Freeman, 2016).
Line 289: I find the duration of leakage events to be the most curious - the Sibson fault valve theory considers leakage to be episodic, and once pressures relax, the faults seal again. Perhaps the reservoir charge is at an incredible rate?	This is indeed a curious situation and one of the reasons we have undertaken this study. One possibility could be that the very shallow nature of the fault and leakage pathways allows them to not be closed in times of relatively low pressure? If the reservoir is charged at rates which would fill the reservoir within short time-scales with CO_2 (~1000 Mt assuming that it	

	never completely empties) there would be an incredible overpressure building up.	