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Comment

# ***Interactive comment on “Possible signals of poleward surface ocean heat transport, of Arctic basal ice melt, and of the twentieth century solar maximum in the 1904–2012 Isle of Man daily timeseries” by J. B. Matthews and J. B. R. Matthews***

**Anonymous Referee #2**

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This submission makes interesting suggestions on surface and radiation controls on ocean heating, and interactions with Arctic ice. However, it needs at least major revision owing to internal inconsistencies and statements which need much better evidential support to be accepted by readers. It is also very long; the message would come through much better if content were better organised and focused on evidence and argument supporting the aims or message. The “Results” section has many statements “we suggest” which are often repeated rather than being properly supported by

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evidence. Such speculation should only be in the “Discussion”.

There are some important recurring points that need to be addressed.

A. Surely evaporation (and the associated loss of heat from the sea surface) depends on wind speed (e.g. page 50 lines 1 and 13). Almost all flux coefficients between ocean and atmosphere do, e.g. as “k” in Lorenz et al. (2010) *J. Geophys. Res. Atmospheres*, 115, D20118. “Wind chill” is a manifestation. Spray also seems to have been the subject of recent literature; if nothing else it increases the surface area for fluxes and is associated with strong winds.

B. Tropical and polar waters are supposed to arrive from west of Ireland via the North Channel. To do this they must cross a persistent poleward current along the continental slope, generally with temperature  $> 9^{\circ}\text{C}$ , salinity  $> 35.2$  (Souza et al., 2001, *Oceanologica Acta*, 24 (Suppl. 1) S63-S76) and winter mixing to several hundred metres.

C. There is not enough attention to the fact that properties of shallow water (whether a tropical or polar surface layer in transit or the water at Port Erin) are very sensitive to local conditions: heating and precipitation-evaporation. For 10m water (the depth at Port Erin?), a net heat flux  $100 \text{ W/m}^2$  produces a temperature change of  $1^{\circ}\text{C}$  in less than a week. 0.1 m of precipitation-evaporation (plausible in a month or two) produces a salinity change of more than 0.3 which is comparable with the range in figure 5a. Thus local effects on the UK shelf, in the North Channel and Irish Sea can obscure any tropical or polar “signature”.

The Introduction on pages 49-54 is very long with much emphasis on surface waters but no discussion of their loss of identity across the shelf to the Isle of Man, especially in winter when the whole shelf sea is vertically mixed. I suggest that there should be some sub-headings which would help to structure and focus the content; six pages is a lot to navigate without “signposts”.

Many of the detailed comments below are about unclear meaning owing to

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phrases/clauses which are too cryptic or not given sufficient context.

Detailed comments Page 48

Lines 19-20. “1959/1963 . . . event is consistent with . . . maximum 1923-2008” is implausible without much more explanation.

Line 27. “warming” means a change of temperature making the 93% very unclear. I think it is “added heat” that is meant. C.f. page 49 line 24 (which is a little clearer).

Page 49

Lines 1-2. What does “adaptively managed” mean?

Lines 7-8. “without reference to solar irradiance variations” is strange because the abstract does refer to them (page 49 lines 19-20), also page 67 line 7, page 68 lines 24, 28, page 69 line 16 etc. and even the heading of section 4.2!

Page 50

Line 5. The statement about precipitation increase is not given the proper context. It can be cold and raining, or hot and dry, even at sea. I think it refers to a climate sensitivity, e.g. as quoted in Lorenz et al. (2010) J. Geophys. Res. Atmospheres, 115, D20118.

Line 13. Correlation of what with wind speed? What range of wind speeds was tested?

Page 52 Lines 19-20. “sub-tropical and sub-polar buoyant surface water to mean land-fall . . . at  $\sim 54^\circ\text{N}$ ”. Surely the sub-tropical water must be south of the sub-polar water?

Page 53

Lines 20-25. It is unclear why this paragraph is here. Line 20 “fallacy that correlates” is expressed wrongly: the fallacy is that correlation implies cause and effect. And it is not clear how Sharples et al. exemplifies this; there is no explanation of what cause and effect is wrongly claimed. Most people would not regard 0.8 as a weak correlation

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(c.f. page 57 line 8, page 66 lines 14-16) – the maximum is 1 – and the significance depends on the number of independent data, not stated. What conclusions and who used inappropriate parameterisations of what?

Page 54

Line 13. What heat capacities have 3000x difference?

Page 55

Lines 1-12. The aims are given here. However, it is unclear how the Introduction content up to this point relates to the stated aim. Section 1.1 first paragraph. The relevance is not clear, I suggest deleting it.

Page 56 The main paragraph here is mostly not necessary to make the point that “sea level has been relatively unchanged for at least the last ~ 4000yr”. One relevant citation rather than much “anecdotal” evidence would be enough.

Page 57 Section 1.2. It is not clear how this paragraph contributes to the stated aims.

Page 59

Lines 9-10. Horsburgh and Hill (2003) held salinity constant at 34.0 so no statement could be made about “brackish” water.

Line 25 – page 60 line 8. Most papers using good data do not say so much about such factors leading to poor quality.

Page 60

Lines 24-28. Surely these temperature gradients cast doubt on the value of Port Erin for representing the temperature of water arriving west of Ireland.

Page 62

Lines 6-8. It is unclear why these particular boxes (especially the last two) are “of interest here”.

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Line 9. This heading only applies to the first four lines of the three paragraphs in section 1.5.

Page 64

Lines 12-14. What was the reason for choosing these particular sub-intervals of 1904-2012? For example, 1939/1940 is a local minimum in the time-series, and its choice therefore has the effect of reducing the warming in the first period and reducing the cooling of the middle period.

Line 22. “is expected” is not a “Result”.

Lines 24-26 and page 65 line 1. This plotting shows exactly the same information as the normal centred method, merely displaced in timing.

Page 65

Lines 5-6. I do not understand “remove deviations from statistical means”.

Line 24 should refer to table 2a.

Page 66 Lines 3-4. “We suggest”! There has not been an argument that Cypris should represent the ocean as a whole. C.f. page 69 line 16.

Lines 14-16. Here correlation is used to argue cause and effect! The Gulf Stream and prevailing winds are not suggested by the value 0.8 but by our additional knowledge.

Lines 23-28. Do these refer to Ronaldsway?

Page 67.

Line 13. Solar radiation may be the largest but is not the only source of ocean heat. Rain and condensation can add heat. Anyway, why say anything about this here?

Lines 14-19. This is more than necessary about sunspots. Just one reference for time series from 1900 will do.

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Lines 21-27. Predictions are not relevant; is correlation with land temperatures relevant? Lines 25-26 suggest sunspots may be irrelevant. Better to omit all this.

Page 68

Lines 12-14. This text refers to 12 months (1959) deriving from 5 months (September 1957 to January 1958). If “equatorial” is precise, then any additional radiation from sunspots may be offset by the sun not being overhead in winter, hence not maximum irradiance. What is the evidence for ~ 22 months transit from “equatorial” to PEMBS? This has to include the transit from 54°N west of Ireland to PEMBS.

Lines 20-21. “tropical warm water” is stated as a fact but has not been demonstrated.

Lines 22-23. What “tropical water maximum”?

Page 69

Line 1. The maximum radiation anomaly was earlier than October 1959 according to page 68. I think this means PEMBS temperature.

Lines 7-8. “Peak irradiance” surely only in the southern hemisphere.

Line 10. “21-22 months” is now a circular argument since it was needed earlier, c.f. page 68 Lines 13-15. “We suggest”!

Line 16. If the ocean was warming during 1940-1986 then clearly PEMBS (which cooled) does not represent the ocean during this period.

Page 70

Line 10. Is there a reference for expected transit times for Baltic water?

Line 23. Is there evidence (a reference) for “stratified”, “polar”. Stratification is very unlikely in mid-winter under such strong winds unless there is local river input.

Line 24. What “shore”.

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Lines 25-26. I don't think Port Erin can "explain" what happened in the southern North Sea and English Channel. Cause and effect?

Page 71

Lines 5-7. It is not made clear what bearing 1959 heating has on 1963 winds.

Lines 14-16. There is nothing special about the fall to 15 March 2011 in figure 4a. The 1917 fall in early March is much more striking.

Lines 21-24. I cannot follow the logic. No salinity values are adduced to test the "freshwater surface pool" theory. Then "the buoyant cool plume" is stated as a fact in lines 23-24 although buoyant and cool imply fresh. And what is the evidence for tropical (salt) water underneath? Is this meant to apply at Port Erin in only  $\sim 10$  m water depth?

Page 72

Line 8. Only one 21st century cold winter is discussed. This should not be generalised.

Lines 20-22. These minima timings are reversed from the statements on page 71 lines 15-16 and page 72 lines 8-10 for extreme cold winters.

Page 73

Lines 7-8 seem to contradict the page 68 emphasis on extra equatorial or tropical heating in 1957/59, and lines 16-18.

Lines 16-17. "solar maximum radiation, two orders of magnitude greater . . .". Meaning 100 times greater?? Is this a reference to extra sunspots by more than two standard deviations?

Page 74

Lines 3-5. Does this low salinity really happen every 8th November – implausible? The anomaly can be mostly explained by the very low value on 8th November 2005 (Figure 5b).

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Lines 6-7. Why “tropical” rather than local freshwater? I agree with “travel times . . . less than one year” but this contradicts the 21-22 months (page 69 line 10). A local source seems likely because otherwise the fresh anomaly has too much time to be mixed away.

Page 75

Lines 15-16. These salinities exceeding 36 are implausible; such values have not otherwise been seen around the UK. Such dense water could not remain near the surface while in transit across the shelf. Could there have been local evaporation (c.f. line 18)?

Lines 18-29. These sentences do not appear to be connected with each other. What point is being made?

Page 76. The first two paragraphs should be confined to the aims of the submission. I don't see the relevance of the sentence on lines 17-19 or the third paragraph.

Pages 77-78 This is a lot about Arctic ice melt, which could probably be reduced to what is needed by a few sentences with references. The “Relationship” in the title is to what? This seems to be missing.

Page 78

Line 8. I don't understand “Heat formerly used in the annual ice melt is now available” since on page 77 lines 23-24 “The annual ice melt . . . increased”.

Page 79

Lines 6-7. The periods 1958-1970, 1993-1997 don't seem to relate to what has been said.

Lines 11-25. I don't see the relevance of most of this.

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Lines 8-10. The relevance of this sentence is unclear – delete? If retained, “rise < 1°C because of the meltwaters” needs more explanation.

Lines 10-12. “. . suggest . . as hypothesised.” Is this progress?

Lines 15-16. “The North Atlantic Ocean is less saline than the North Pacific” is not generally true, it depends which sub-areas are compared.

Lines 12, 13, 19. Without access to Matthews and Matthews (2013; at the time of writing this) it is unclear what is meant by “1/3 (or 2/3) of ocean heat”. Are these fractions of the radiation arriving at the sea surface?

Lines 19-21. “The circulation . . . suggests . . .” What is the logic for “suggests”? Suggests what – what do “this” and “It” refer to?

Page 81

Line 3. No heating “from basal ice melt” which takes up heat – delete this phrase.

Line 4. Irradiance in polar regions is not “weak” in mid-summer.

Line 7. Warm tropical water was “suggested” not “found”.

Line 9. “gyre period is about ~ 20 months.” This was an inference only. If there is independent evidence please give a reference.

Line 12. “10°C”. Is this an annual mean – do seasonal variations in surface temperature affect this value?

Line 14. I do not understand “ocean conveyor brine separation”.

Lines 15-16. This lack of effect of solar radiation needs to be reconciled with the claimed effects on page 68 etc.

Lines 20-29. What is concluded from this information in relation to the aims here?

Page 82 The last paragraph of section 4.2 and first paragraph of section 4.3 (to line 25)

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are not obviously relevant to the aims here – could be omitted. Especially the sentence lines 4-5.

Page 83.

Lines 8-12, and the sentences lines 18-21 and 23-26 don't seem to be relevant to the aims here.

Lines 14-16. These sentences should be merged with page 81 line 14 and the merger located where it relates to the rest of the discussion. “salinity and temperature have equal and opposite effects” needs more qualification – it does not generally apply at PEMBS.

Page 84.

Line 2. The European North Atlantic is not global: “temperature” not “AGW”. “rising” rather than “increasing”.

Line 13. “eastern sides” would be clearer than “west coasts”.

Lines 25-28. These do not obviously add to what is said on lines 21-24; merge or omit.

Page 85

Line 2. “Arctic thermodynamic processes” is vague and unclear – please be more specific.

Lines 6-7. “inferred” or “suggested” not “observed”.

Line 11. The minima were December 2010 and March 15 2011, not March/April.

Lines 15 and 19. “heat uptake” not “AGW”.

Lines 16-17. “rapid rise” not “rapidly increasing trend”.

Page 86

Line 1. As page 78 line 8.

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Line 16. As page 49 lines 1-2.

Pages 100, 101 Tables 1a and 1b could be merged. Please explain why the period trends differ from the mean  $\Delta T$ .

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