Is ocean warming accelerating faster than thought? – An analysis of Cheng et al (2019), Science [1]

There are a number of statements in Cheng et al. (2019) 'How fast are the oceans warming?', ('the paper') that appear to be mistaken and/or potentially misleading. My analysis of these issues is followed by a reply from the paper's authors: Lijing Cheng, John Abraham, Zeke Hausfather and Kevin Trenberth.

Contrary to what the paper indicates:

- Contemporary estimates of the trend in 0–2000 m depth ocean heat content over 1971–2010 are closely in line with that assessed in the IPCC AR5 report five years ago
- Contemporary estimates of the trend in 0–2000 m depth ocean heat content over 2005–2017 are significantly (> 95% probability) smaller than the mean CMIP5 model simulation trend.

Ocean warming over 1971–2010 per IPCC AR5 and contemporary estimates

1. The paper states: "The warming is larger over the 1971–2010 period than reported in AR5. The OHC trend for the upper 2000 m in AR5 ranged from 0.20 to 0.32 Wm^{-2} during this period (*4*: *AR5*). The three more contemporary estimates that cover the same time period suggest a warming rate of 0.36 ± 0.05 (*6*: *Ishii*), 0.37 ± 0.04 (*10*: *Domingues*), and 0.39 ± 0.09 (*2*: *Cheng*) Wm^{-2}." [Numbered references in this article are to the same numbered references in the paper. The number is followed by the lead author's name, or AR5, where this aids clarity.]

2. AR5 (*4*) featured 0–700 m depth ocean heat content (OHC) 1971-2010 linear trend estimates from five studies, ranging from 0.15 to 0.27 Wm^{-2} of the Earth's surface. Adding the AR5 700–2000 m OHC 1971-2010 trend estimate of 0.09 Wm^{-2} brings the range up to 0.24 to 0.36 Wm^{-2}, not to 0.20 to 0.32 Wm^{-2} as stated. The warming rates plotted in Supplementary Figure S1 agree to my values, not to those stated in the paper.

3. Importantly, although AR5 featured several OHC trend estimates for 0–700 m depth, its assessment of the Earth's energy uptake (Section 3.2.3 and Box 3.1) used only the highest one (*10*: *Domingues*), adding the Levitus (*12*) 700–2000 m OHC trend to give a best estimate 0–2000 m warming rate over 1971–2010 of 0.36 Wm^{-2}. That rate is identical to one (*6*: *Ishii*) of the three more contemporary estimates given in the paper and extremely close to the other two of them – within the innermost one-third of their uncertainty ranges. See Figure 1, left hand section, and compare with the 'Updated OHC estimates compared with AR5' figure [Fig 2] in the paper. It is therefore misleading to claim that the warming is larger over the 1971–2010 period than reported in AR5.

4. Moreover, over the final decade covered by AR5, 2002–2011, the trend of the 0–2000 m OHC time series that AR5 adopted for its assessment, 0.60 Wm^{-2}, was noticeably higher than those for two of the three more contemporary estimated OHC datasets given in the paper (0.35 (*6*: *Ishii*) and 0.52 (*2*: *Cheng*) Wm^{-2}) and, unsurprisingly, almost identical to the third (*10*: *Domingues* + *12*: *Levitus*).

[1] The paper does not directly claim that ocean warming is accelerating faster than thought; that is the headline of The New York Times article about the paper.

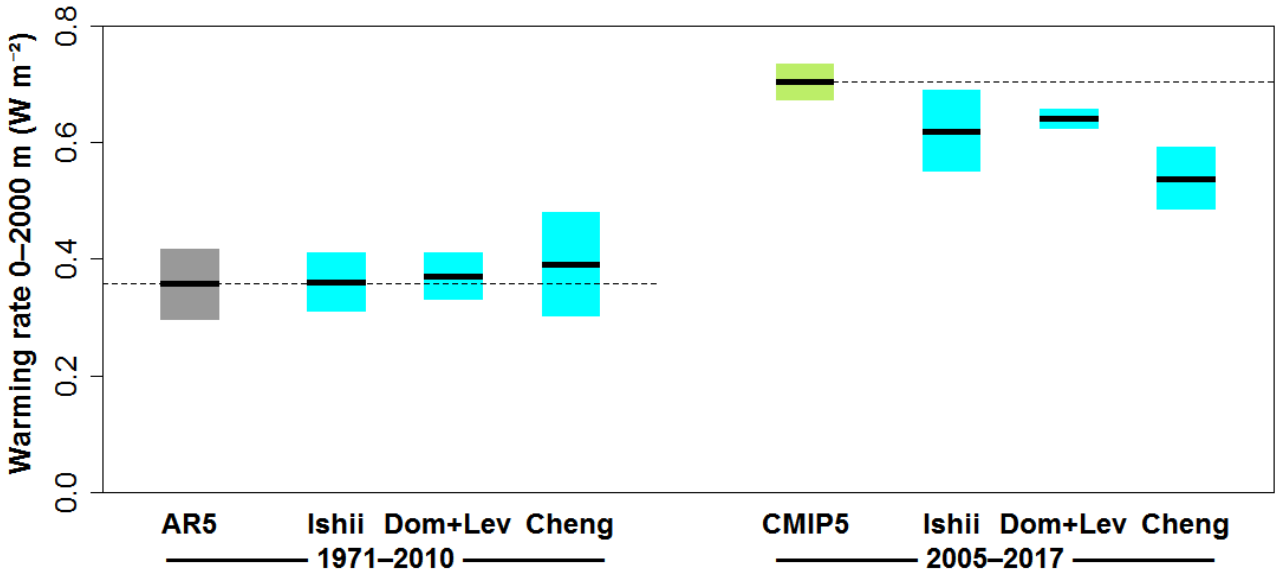


Figure 1: Updated 0–2000 m OHC linear trend estimates compared with AR5 and the CMIP5 mean. Error bars are 90% confidence intervals; black lines are means. Units relate to the Earth's entire surface area.

Ocean warming over 2005–2017 per CMIP5 models and contemporary estimates

5. The paper's 'Past and future ocean heat content changes' figure [Fig 1] caption states: "Annual observational OHC changes are consistent with each other and consistent with the ensemble means of the CMIP5 models for historical simulations pre-2005 and projections from 2005–2017, giving confidence in future projections to 2100 (RCP2.6 and RCP8.5)." This does not appear to be true for the linear trends of the annual values for the 2005–2017 projections, at least.

   The main text states: "Over this period (2005–2017) for the top 2000 m, the linear warming rate for the ensemble mean of the CMIP5 models is 0.68 ± 0.02 Wm^{-2}, whereas observations give rates of 0.54 ± 0.02 (*2*), 0.64 ± 0.02 (*10*), and 0.68 ± 0.60 (*11*) Wm^{-2}."

6. Five problems with this claim regarding 2005–2017 warming rates are:

   (i) the CMIP5 RCP2.6 and RCP8.5 projections top 2000 m OHC data archived for the paper shows an ensemble-mean linear warming rate over 2005–2017 of 0.70 ± 0.03 Wm^{-2}, not 0.68 ± 0.02 Wm^{-2}. The same is true when also including data from the third scenario used in the paper (RCP4.5).

   (ii) the underlying time series from which the third observational estimate is derived (Fig. 3.b in *11*: *Resplandy*) spans 1991–2016, and has a lower (and highly uncertain) linear trend from 2005 to 2016 (its final year) than the stated 0.68 Wm^{-2} (which is calculated over 1991–2016), so this estimate should be excluded;

   (iii) the statement inexplicably omits the Ishii et al. (*6*) observational data, which also have a lower estimated trend (0.62 ± 0.07 Wm^{-2}) than per CMIP5 over this period; and

   (iv) the uncertainty range for the Cheng (*2*) estimate appears to be seriously understated: I calculate that the estimate should be 0.54 ± 0.06 (rounding 0.055 up), not 0.54 ± 0.02.

   (v) adding the uncertainty ranges in quadrature, since CMIP5 and observational errors are independent, the CMIP5 ensemble mean trend is statistically inconsistent with the all three of these observational trend estimates (*2*: *Cheng*, *6*: *Ishii*, *10*: *Domingues*);

The right hand section of Figure 1 shows a corrected comparison of CMIP5 mean and observational 0–2000 m depth ocean warming rates over 2005–2017.

7. Although it is pointed out in the paper's Supplementary material that volcanic eruptions after 2000 have not been taken into account in CMIP5 models (with a minor effect on projected warming since then), it has been shown that when underestimation of other growth in other drivers of climate change is accounted for there is no overall bias in post-2000 CMIP5 model forcing growth (Outten et al. 2015).

Other issues

8. The straight black line in the 'Past and future ocean heat content changes' figure [Fig 1] for the Resplandy et al. (11) OHC estimate gives a misleading impression of close agreement with the three OHC time series based on *in situ* observations over 1991–2016: its trend uncertainty range is so large (0.08 to 1.28 Wm^{-2}) that the apparent close agreement is most likely due to chance.

9. The Press release for the paper claimed that 'If no actions are taken ("business as usual"), the upper ocean above 2000 meters will warm by 2020 ZetaJoules by 2081-2100", which is based on CMIP5 model RCP8.5 scenario simulations. That is misleading. RCP8.5 involves not only no actions (including those already carried out) being taken, but also emissions being unusually high for a business as usual scenario.[2]

Nicholas Lewis 21 January 2019

References

2. L. Cheng et al., Sci. Adv. 3, e1601545 (2017).

4. M. Rhein et al., in Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change, T. F. Stocker et al., Eds. (Cambridge Univ. Press, 2013), pp. 215–315.

6. M. Ishii et al., Sci. Online Lett. Atmos. 13, 163 (2017).

10. C. M. Domingues et al., Nature 453, 1090 (2008).

11. L. Resplandy et al., Nature 563, 105 (2018).

12. S. Levitus et al., Geophys. Res. Lett. 39, L10603 (2012).

Additional references

Cheng, L., Abraham, J., Hausfather, Z. and Trenberth, K.E. (2019). How fast are the oceans warming?. Science, 363(6423), pp.128-129.

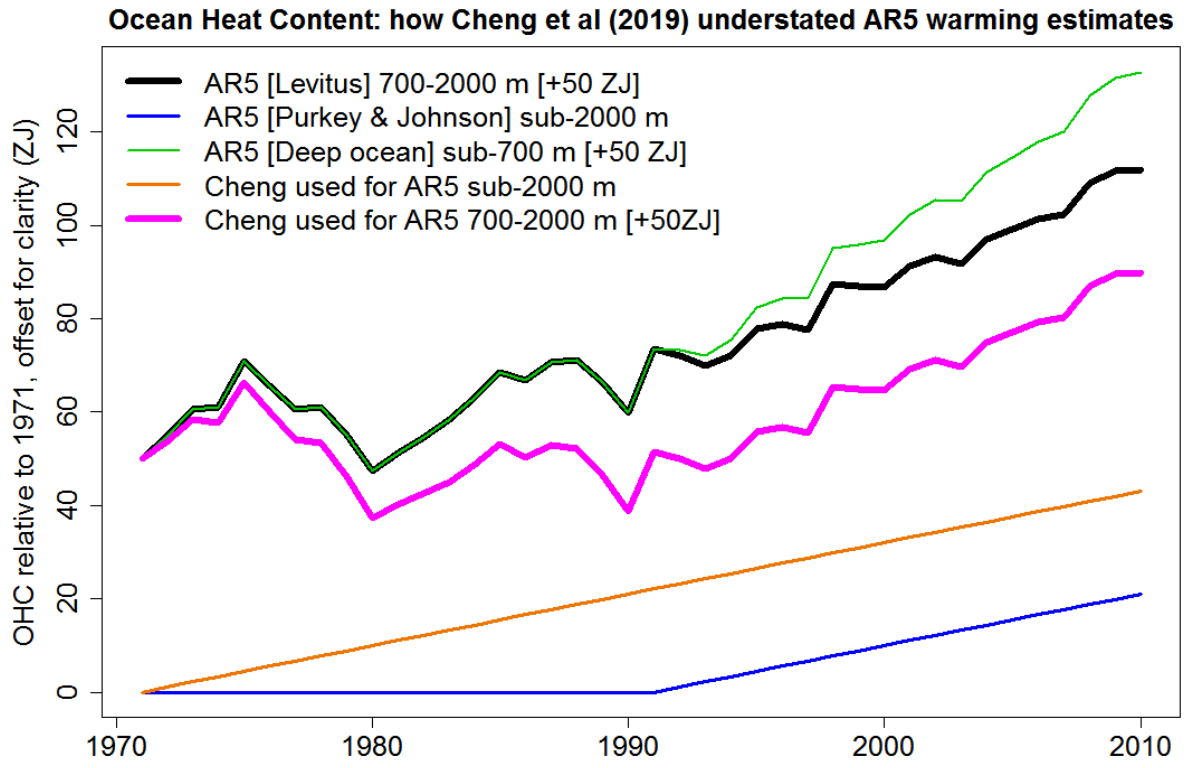
Outten, S., P. Thorne, I. Bethke, and Ø. Seland (2015), Investigating the recent apparent hiatus in surface temperature increases: 1. Construction of two 30-member Earth System Model ensembles, J. Geophys. Res. Atmos., 120, doi:10.1002/2015JD023859

[2] As the source paper (Riahi, K., et al., 2011: RCP 8.5–A scenario of comparatively high greenhouse gas emissions. DOI 10.1007/s10584-011-0149-y) states: "RCP8.5 combines assumptions about high population and relatively slow income growth with modest rates of technological change and energy intensity improvements, leading in the long term to high energy demand and GHG emissions in absence of climate change policies.", and that "[RCP] 8.5 corresponds to a high greenhouse gas emissions pathway compared to the scenario literature". As Riahi et al. (2011) make clear, the assumed energy intensity improvement rates are only about half the historical average while middling world GDP growth is assumed, leading to coal use increasing almost 10 fold by 2100.

Addendum: Update 22 January 2019 in reaction to tweets by co-author Zeke Hausfather

The authors seem still not to understand that their Figure 2 AR5 0–2000 m warming rates are mathematically wrong, while those I calculate are correct, not merely a different approach. Perhaps when they see it graphically they will admit their error. The below plot shows how the AR5 Box 3.1 1971-2010 Deep ocean (sub-700 m) OHC values (green line) were made up from Levitus 700–2000 m OHC data (black line) plus warming of the sub-2000 m ocean at a rate of zero until 1991 and then 35 TW from 1992 on (blue line).

Cheng et al. deducted from the AR5 sub-700 m OHC (green line) an assumed sub-2000 m warming at a rate of 35 TW *throughout* 1971–2010 (orange line) to reach their estimate of AR5 700-2000 m warming (magenta line). As can be seen, their 'AR5' 700-2000 m OHC estimate (magenta line) has a substantially lower linear trend (warming rate) than the actual AR5 700-2000 m OHC estimate (black line), which is what my calculation uses.



It follows that Cheng et al.'s Figure 2 values for AR5 0-2000 m warming rates over 1971-2010, which add the trend of the magenta line to the 0-700 m warming rate, are understated by the difference between the trends of the black and the magenta lines.

Honest scientists, unlike activists, are prepared to admit and correct factual mistakes in their papers, whether or not they alter its primary conclusions. I expect that Cheng et al. will accordingly submit a correction to Science to substitute correctly calculated 1971–2010 upper 2000 m AR5 OHC trend values for their erroneous values.

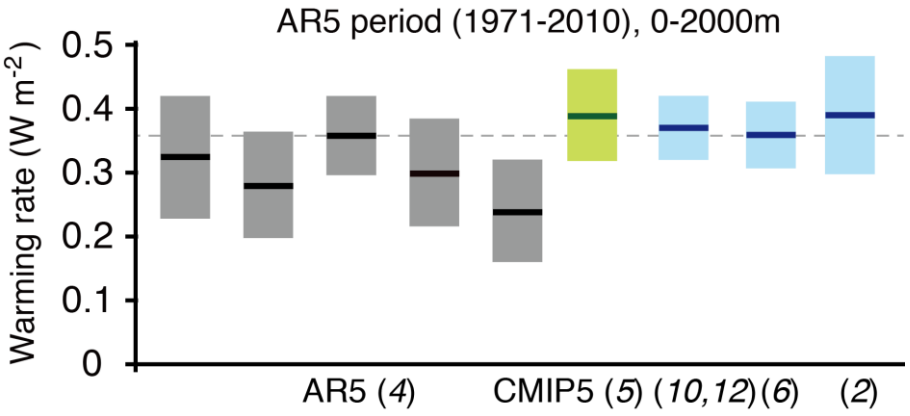
Lijing Cheng has asked me to post also the following reply from the paper's authors to my critique. I am pleased to do so and I thank him for providing it. I have replaced interspersed text extracted from my article with paragraph number references. The authors' responses are shown in blue. I have appended my comments, shown in red, on a number of them.

Paragraphs 1 and 2

Some questions have been raised concerning the numbers in our article (…) and indeed there is an inconsistency between a value in the supplementary material and the main text. It relates to the use of linear trends and how to assign a change over various periods. For longer time intervals, a linear trend is not a good fit to the data and use of that to assign a change can be misleading. In the IPCC AR5, below 700m depth, it is stated that "the heating below 700 m is 62 TW for 1971-2010". They also state "For the ocean from 2000 m to bottom, a uniform rate of energy gain of 35 [6 to 61] TW from warming rates centred on 1992–2005 (Purkey and Johnson, 2010) is applied from 1992 to 2011, with no warming below 2000 m assumed prior to 1992." Hence the difference for the 700 to 2000 m layer is 62 -35 = 27 TW. This is 0.05 W m^{-2} and is what was used in the main text to produce the numbers quoted. However, if instead one takes the 2 flat lines below 2000 m and subtracts from the actual values, and then fits a linear trend, the implied change is closer to 45 TW which gives the 0.09 W m^{-2} plotted in Fig. S1. If the latter is used instead, then the change from the old AR5 values to the newer OHC values is somewhat reduced (see figure below). The increase is up to 40% over the prior IPCC estimates, and the average is 24%. This exercise was prompted by a comment by Nic Lewis who we thank, and it highlights the uncertainty in actual trends and their use to depict changes. The conclusions in our Perspective remain sound. If the alternative analysis method proposed by Nic Lewis is used, the change is not quite as dramatic as implied in some of the associated press releases.

Based on this:

- While there is an inconsistency that is not discussed between Fig. 2 and Fig S1, it reflects the uncertainties in previous OHC estimates and the associated methods. In particular, some values before 1980 or so are erratic (high values in the 1960s) and a linear trend is not a good fit to the time series.
- All of our key points are still valid: (1) the best estimates are collectively higher than the 5 estimates featured in AR5 (0-63% higher). (2). And the best estimates are more consistent with each other (0.36/0/37/0.39 Wm^{-2} than 0.24~0.36 Wm^{-2} in AR5). (3). Model ensemble means are higher than 5 estimates featured in AR5 (0.39 Wm^{-2}, 8-63% higher) and consistent with new/updated observations.
- AR5-Box 3.1 used the strongest estimate without backup literature, we state this in the supplement (also read our replies below). So our study justifies the choice in Box 3.1 as we discussed in supplement. The new estimates could be 0-8% stronger than the selected estimate by AR5-Box 3.1 for 1971-2010. But we didn't make claims regarding Box 3.1 in Science article, so this is not an issue.
- This would be an adjusted Fig. 2 (plot below) if we used the different value, the key messages do not change:



Additionally, the Domingues value for 0-700m should have extremely large error bars: all of the values prior to 1970 are much higher than from 1970 to 1980 in AR5, (see AR5 Figure 3.2; given also below) and hence the trends for that estimate are extremely dependent on the period used. Whether that value was used or not in AR5 (and we state it was), the AR5 message is that they really didn't know the value at all well, and now we do.

Nic Lewis comments:

a) Their arguments justifying their deduction of 35 TW from the AR5 1971–2010 linear trend below-700 m ocean heating rate to give their 700–2000 m layer heating rate of 27 TW (0.053 Wm^{-2}) make no sense. AR5 arrived at its sub-700 m deep OHC time series (plotted in Box 3.1 Figure 1) by adding, from 1992 onwards, { (year – 1991) * 1.10 ZJ } to its estimate of 700–2000 m depth OHC [1.10 ZJ / year = 35 TW].

The only correct way to derive the AR5 700–2000 m depth OHC time series is to take its sub-700 m OHC time series and reverse out this addition, which is what I did. Cheng et al.'s method gives the wrong 1971–2010 rate of 700–2000 m depth ocean heating irrespective of whether this is measured by a linear trend or otherwise.

b) Their explanation of the inconsistency between their Fig 2 and their Fig S1 conflicts with the facts. They imply in the supplementary material that for both figures the warming rates are linear trends from an ordinary least squares (OLS) fit. Whether or not an OLS fit is ideal is irrelevant; it is what AR5 did and is what Cheng et al. indicated they did. I have verified that their Fig S1 estimates agree to OLS fits to their data. It is undeniable that the AR5 warming rates plotted in their Fig 2 are erroneous.

c) Numerical simulations using strongly autocorrelated random errors confirm that the 1971–2010 trend uncertainty for the Domingues 0–700 m OHC trend stated in AR5, which is incorporated in the AR5 0–2000 m trend uncertainty plotted in my Fig 1, appears to adequately reflect the large uncertainty that AR5 showed the Domingues estimates as having in pre-Argo years (which dominates the uncertainty shown in Box 3.1 Fig 1).

Paragraph 3

Please read our supplement, we fully describe the whole story as follows "IPCC-AR5 (*1*) featured five estimates for OHC within 0-700m including Levitus et al. (*2*) (LEV), Ishii et al. (*3*) (ISH), Domingues et al. (*4*) (DOM), Palmer et al. (*5*) (PAL), Smith and Murphy (*6*) (SMT), one estimate for 700-2000m: Levitus et al. (*2*) (LEV) and one estimate below 2000 m: Purkey and Johnson (*7*) (PG). For the Earth's energy budget inventory (Box 3.1 in Ref. (*1*)) and other places, DOM, LEV and PG are used for 0-700m, 700-2000m, and below 2000m respectively. Among the five 0-700m OHC estimates in AR5, the minimum yields an ocean warming of 74 [43 to 105] TW (SMT) within 1971-

2010, which is almost half of the maximum, with a rate of OHC change of 137 [120 to 154] TW (DOM). If all of five estimates are treated equally, a huge error bar has to be put in the final OHC estimate, downplaying the reliability of OHC records.

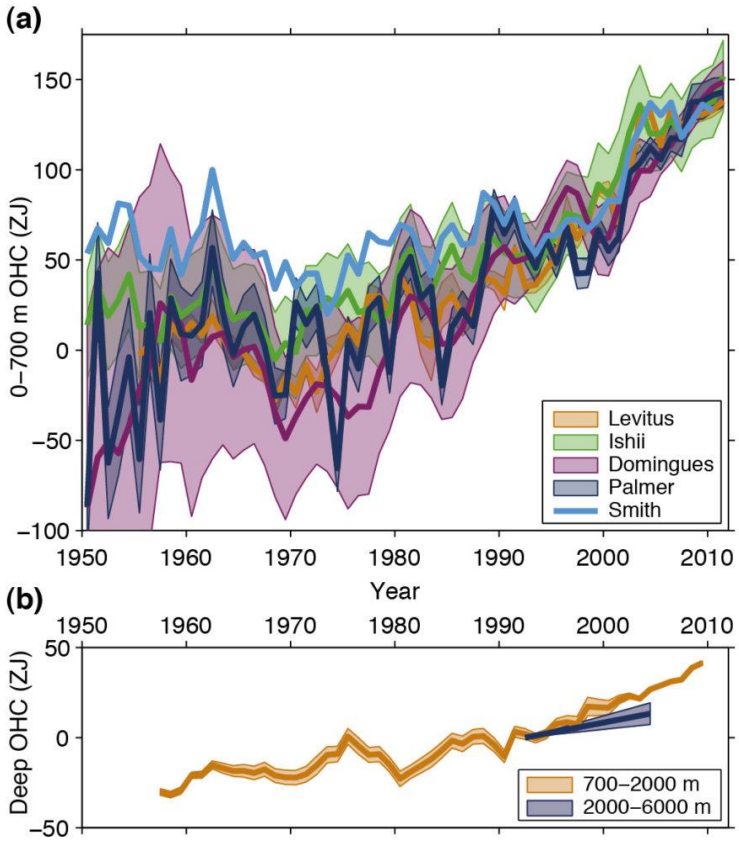
AR5 chose the DOM estimate to assess Earth's energy budget, rather than any others or an ensemble mean of the five featured estimates by stating:

> "*Generally the smaller trends are for estimates that assume zero anomalies in areas of sparse data, as expected for that choice, which will tend to reduce trends and variability. Hence the assessment of the Earth's energy uptake (Box 3.1) employs a global UOHC estimate (Domingues et al., 2008) chosen because it fills in sparsely sampled areas and estimates uncertainties using a statistical analysis of ocean variability patterns.*".

In this way, the "conservative error" of many estimates has been identified in AR5 but not supported by the literature. Since AR5, many studies have been looked into this issue either directly or indirectly (*8-13*) and several new/revised estimates are available, and are chosen by our study.

For OHC within 0-700m, the new CHG and ISH estimates are consistent with DOM (Figure S1). The three estimates are collectively higher than LEV/ISH/PAL/SMT featured in AR5 (Figure S1). Therefore, the progress after AR5 justifies the choice of DOM in AR5 for OHC 0-700m."

We note that the AR5 featured five different OHC estimates available at the time in the main body of their assessment and the main figure (Fig. 3.2), shown below. We feel that this justifies comparing newer OHC estimates to all five series, rather than just the Domingues series that the AR5 chose to highlight.



Additionally, when the 700-2000m values from Levitus are used (as discussed above), recent records still show 0% to 8% more warming over the 1971-2010 period than the AR5 Domingues value: 0.36 ± 0.05 (Ishii), 0.37 ± 0.04 (Domingues+Levitus), and 0.39 ± 0.09 (Cheng) compared to the old Domingues value of 0.36 Wm^{-2}.

Incidentally, since we have this figure here: note the big bump in Domingues in the top panel in the 1950s and 60s. Also note the bump in the 1970s in the 700 to 2000 m layer.

Nic Lewis comments:

None of these points affects what I say in my article. The paper says " These recent observation-based OHC estimates show highly consistent changes since the late 1950s (see the figure). The warming is larger over the 1971–2010 period than reported in AR5. The OHC trend for the upper 2000 m in AR5 ranged from 0.20 to 0.32 W m−2 during this period (4)." Since the figure referred to shows only 0–2000 m OHC, it is implicit that " The warming is larger over the 1971–2010 period" in the next sentence refers to warming in the 0–2000 m ocean layer. AR5 only featured 0–700 m OHC dataset other than Domingues when discussing warming of that ocean layer; it did not use any of them to estimate warming over 0–2000 m .

Paragraph 4

The period from 2002-2011 seems somewhat arbitrary, and we chose to focus on the 1971-2010 period as it was the one specifically highlighted in the AR5. We would expect greater agreement between older and newer estimates of OHC changes after around 2005 (when Argo data begins being available), as corrections of XBT measurements and better spatial interpolation approaches – which were the primary changes made to newer OHC datasets – matter much more prior to the early 2000s. And there is a better agreement after 2005, Johnson et al. 2018 BAMS state of climate show this already. We do give updated values for 2005-2017 (Argo period) for comparison with CMIP5.

Further, we could see the time series plot similar to AR5-Fig.3.2 below, the new time series apparently show better consistency than AR5-Fig. 3.2 among estimates.

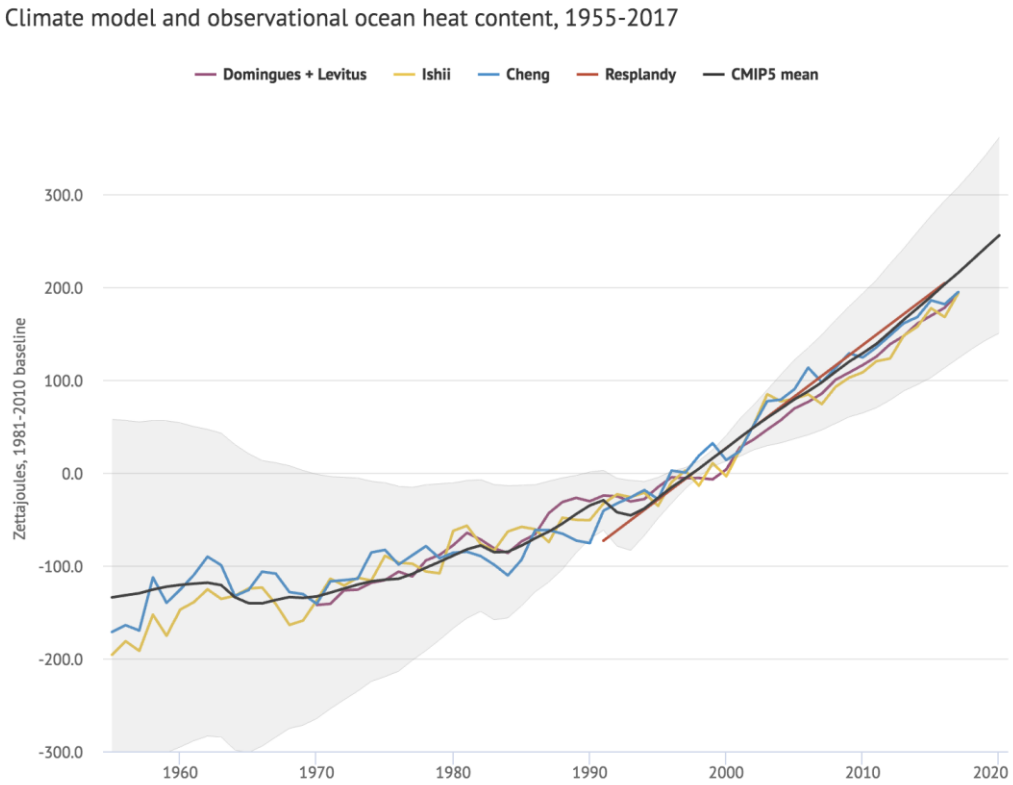


Figure. Times series of OHC 0-2000m for the four best estimates compared with CMIP5 model ensemble mean and two-sigma model spread.

Nic Lewis comments:

My paragraph 4 is simply an observation; it does not claim to point to any mistake in the paper. Nor does it bear on my point that it is misleading to claim that the warming is larger over the 1971–2010 period than reported in AR5.

Paragraph 5

First, 2005-2017 is a short period, there are many uncertainties: 1) There are short-term variability in the time series (i.e. Interannual variability such as ENSO) and uncertainty in observations, these can impact the trend calculation in a short period within 2005-2017; 2) CMIP5 models do not contain natural variability in phase with actual natural variability, and 3) do not contain realistic forcings after 2005. We discuss this in some detail in supplement:

> "We show in the main text that over the period of 2005-2017, the linear warming rate for the ensemble mean of the CMIP5 models is 0.68±0.02 W m^{-2}, slightly larger than the observations (ranging from 0.54±0.02 to 0.64±0.02). Many studies, including Gleckler et al. (13) and Schmidt et al. (16) have shown that the volcanic eruptions after 2000 have not been taken into account in CMIP5 models. Taking this into account, the Multi-Model-Average of CMIP5 simulations will be more consistent with observations during the recent decade (13)."

Gleckler et al. 2016 explicitly addressed the volcano impacts in ocean heat content comparison between model and observations, after Outton et al. 2015, they suggested a correction for a global volcanic aerosol forcing since 2000 of 0.19±0.09 Wm^{-2}.

Nic Lewis comments:

None of this is relevant to my point that the claim in the caption to Fig 1 of the paper that "Annual observational OHC changes are consistent with each other and consistent with the ensemble means of the CMIP5 models for historical simulations pre-2005 and projections from 2005–2017" is contradicted by the differences in the linear trends of the data involved over 2005–2017, having regard to the trend uncertainty ranges.

Paragraph 6

(i) As we point out in the supplementary materials (figure caption) "CMIP5 results (historical runs from 1971 to 2005 and RCP4.5 from 2006 to 2010) are indicated by the green bar". Using CMIP5 historical + RCP4.5 runs gives us 0.68 ± 0.02 Wm−2. We could have been clearer in the main paper which RCP runs were shown in the trends comparison part of the figure; we did in earlier drafts of the article but it was cut at the suggestion of the editors at Science to shorten/simplify the figure caption.

(ii) Resplandy explicitly state in their paper that the trustable estimate is the linear trend, rather than annual values, because the O2 and CO2 changes on annual scales are not primarily driven by temperature. Hence we only use their linear trend (the revised version shown in Real Climate).

(iii) Earlier drafts of the paper did include the Ishii estimate, though it was omitted from the final version due to length constraints as it fell between the 0.54 (Cheng) and 0.64 (Dom/Lev) instrumental estimates noted. We should have made this clearer (e.g. mention that instrumental estimates range from 0.54 to 0.64), although its exclusion here does not impact any of our conclusions. As the 0.62 Ishii estimate is closer to the Dom/Lev than Cheng, its inclusion would make the overall range of instrumental estimates seem closer to CMIP5 over this period.

(iv) We used the error calculation presented in Foster and Rahmstorf 2011, which takes accounts of the autocorrelations in a time series.

OHC_errorbar=1.65*OHC_se*sqrt(v)

Where:

- OHC_se is standard error using OLS.
- v=1+2*p1/(1-q)
- p1=OHC_autocorrelation(2)
- q= OHC_autocorrelation(3) / OHC_autocorrelation(2)
- OHC_autocorrelation is the autocorrelation of the time series.

Using this method, we can replicate the error bar provided by AR5, so it should be nearly identical to AR5 method.
We also get 0.06 uncertainty range for Cheng (2) if simply using OLS method. But this does not impact the comparison between new/updated observations and model.

(v) The four new/updated best estimates are 0.54, 0.62, 0.64, and 0.68 Wm^{-2}. The CMIP5 historical + RCP4.5 model ensemble mean is 0.68 Wm-2. If we focus on instrumental estimates (and exclude Resplandy et al given its large uncertainties), the CMIP5 models are a bit higher than observations during the Argo era, although, as we discuss in the Supplementary Materials and our previous replies, mismatches between projected and observed forcings in the forecast period are expected to give differences over this period.

Nic Lewis comments:

(i) No indication is given in the paper or the supplementary material that "the linear warming rate for the ensemble mean of the CMIP5 models" for the top 2000 m over 2005–2017 referred only to projections based on the RCP4.5 scenario. Although the authors were unlucky to have an editor who was more concerned with presentation than scientific content, they, not the editor, are ultimately responsible for the paper.
The caption to their Fig 1, which states that annual observed OHC changes are consistent with the ensemble means of the CMIP5 models, shows projections based on the RCP2.6 and RCP8.5 scenarios. The relevant 2005–2017 trends for those scenarios are respectively 0.70 and 0.71 Wm^{-2}.

(ii) This supports my point that the 2005–2017 trend estimatable from the (revised) Resplandy data is highly uncertain. The fact that the estimated 1991–2016 Resplandy trend is somewhat less uncertain (at 0.68 ± 0.60 Wm^{-2}) does not justify treating it as also being the 2005–2017 trend. The fact is that the information available from the Resplandy data is so imprecise that it adds almost nothing to knowledge about ocean warming trends over 2005–2017.

(iii) Noted. IMO this issue illustrates a problem with publishing papers in Science and similar 'high profile' journals.

(iv) I also used the error calculation presented in Foster and Rahmstorf 2011. I estimated the relevant autocorrelations over 2005–2017, since that was the period over which the trend was being estimated. They were insignificantly negative for the Cheng data, so no correction to the OLS standard error estimate of 0.0332 was appropriate. Multiplying this by 1.65 gives a 5–95% uncertainty range of, rounded up, ±0.06. The authors appear to agree with this value. I cannot understand how a correction for autocorrelation could possibly reduce the uncertainty range by a factor of three in these circumstances. The paper's ±0.02 uncertainty range for the Cheng 0–2000 m 2005–2017 trend seems clearly wrong.

(v) Using data only from the RCP4.5 scenario simulations, giving an ensemble mean lower than that forRCP2.6 only, for RCP8.5 only, and for all three scenarios combined, appears to be unjustified (even if had been disclosed).

Paragraph 7

Gleckler et al. 2016/Santer et al. 2014 (cited in our supplement) explicitly addressed the volcano impacts, after Outton et al. 2015, see our previous reply.
Also:
Schmidt, A., Mills, M. J., Ghan, S., Gregory, J. M., Allan, R. P., Andrews, T., et al. (2018). Volcanic radiative forcing from 1979 to 2015. Journal of Geophysical Research: Atmospheres, 123. https://doi.org/10.1029/2018JD028776

Nic Lewis comments:

This is irrelevant. Outten et al 2015 also included the omission in CMIP5 models of the impacts of recent volcanic eruptions, but found that it was fully offset by the net impact of misestimation of recent changes in other forcings.

Paragraph 8

We agree that the uncertainties in the revised Resplandy estimate are quite large, as we note when including them in the paper over the 2005-2017 Argo period (0.68 ± 0.60 Wm^{-2}). Unfortunately showing the error bars of all the underlying observational series in main text figure as well as those of the climate models would have made it unreadable, and the fact that the Resplandy estimate does not extend back to 1971 means that it is left out of the "Updated OHC estimates compared with AR5" portion of the figure that does show individual series uncertainties. However, Resplandy et al does provide a novel approach to estimating ocean heat content, and we think their median estimate was worth showing alongside the three updated instrumental datasets, even if (unlike the other three datasets) Resplandy's uncertainties are so large that they limit the claims that can be made regarding agreement with climate models.

Paragraph 9

We agree it is generally better to include the full definition of RCP8.5 to avoid any confusion, but in a press release for the general public we had to simplify. This does not impact the message in the published Science article. We note that there is an ongoing debate within the energy modeling and climate science community regarding RCP8.5 and the extent to which it represents a "business as usual" outcome, and that this is shifting with the availability of the SSP scenarios and the inclusion of a 7 w/m^2 forcing scenario in CMIP6. However, references to RCP8.5 as "business as usual" in the published literature are quite common, and the original paper presenting the RCP8.5 scenario (Riahi et al: https://link.springer.com/article/10.1007/s10584-011-0149-y) explicitly refers to it as "a high-emission business as usual scenario".

Closing statement by the paper's authors:

1. Thanks for the critique, an alternative set of values could be used in Fig.2 in our calculation for 700-2000m OHC in AR5. But the uncertainties are large in those early years.
2. We believe that all of our conclusions are still valid:
   - After significant progress since AR5, the best OHC estimates show stronger warming than estimates featured in AR5 (0~63%), and they are also more consistent with each other.
   - The models are consistent with the best OHC estimates for the 1971-2010 period. While models are warming slightly faster than most of the observational records during the 2005-2017 period, this is expected because the volcanic aerosol effects are not fully included.

Nic Lewis closing comment:

I thank the authors for their constructive response. I concur that OHC uncertainties are large in the early years of the 1971–2010 period.

None of the authors' responses refute any of my criticisms concerning factual errors and misleading statements in the paper.

In particular, presenting my method of calculating AR5 0–2000 m warming rates over 1971–2010 as alternative to their method is like claiming that calculating 4 – 2 = 1 is an alternative to calculating 4 – 2 = 2.