Review Comments by Dr. Ross McKitrick Submitted to the IPCC, Assessment Report 6, WG 1

Chapter 2: Changing state of the climate system	This section needs to include a brief summary of the rates of coverage of the world's oceans and the inherent data quality problems. Prior to WWII at least 50% of the ocean surface is completely unsampled, and the parts that are sampled have gaps in documentation that frequently make it uncertain how the measurements were taken (e.g. Hirahari et al. 2014). The discussion as currently written makes it sound as if the only problems with SST products are the integration of recent buoy data and a temporary postwar period of uncertainty regarding use of engine intake data.
Chapter 2: Changing state of the climate system	While the global average buoy differential is estimated to be 0.12 C, Kennedy et al point out it ranges + or - 0.17C depending on location.
Chapter 2: Changing state of the climate system	I find the discussion of urbanization inadequate. First if you are going to merely restate the AR5 conclusion then you have to quote it accurately, especially since AR5 went a small way to acknowledging a scandalous fabrication in AR4. In AR4 the Lead Authors invented evidence to dismiss a pair of papers finding evidence of correlation between warming rates and industrialization. They claimed "McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4), which exhibit large-scale coherence. Hence, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant." This claim that our results were insignificant was made up and was one of the topics subject to inquiry by the Muir Russell panel following the leak of Climategate emails which implicated Jones and Trenberth as having been "determined to keep [the papers] out of the IPCC report even if we have to redefine what the peer review literature is." Jones' only defence was that there is "no need to calculate a p-value for statements based on the laws of physics."

Chapter 2: Changing state of the climate system	Between AR4 and AR5 I published a pair of papers refuting the AR4 claim and another paper that had challenged our earlier work (McKitrick 2010, McKitrick & Nierenberg 2010). Later, AR5 (ch 2 p. 34) conceded the AR4 claim was groundless: "AR4 concluded that this correlation ceases to be statistically significant if one takes into account the fact that the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes but provided no explicit evidence for this overall assessment result." It then went on to concede that we had shown significant evidence for contamination of the surface record: "Subsequently McKitrick and Michaels (2007) concluded that about half the reported warming trend in global-average land surface air temperature in 1980–2002 resulted from local land-surface changes and faults in the observations. Schmidt (2009) undertook a quantitative analysis that supported AR4 conclusions that much of the reported correlation largely arose due to naturally occurring climate variability and model over-fitting and was not robust. Taking these factors into account, modified analyses by McKitrick (2010) and McKitrick and Nierenberg (2010) still yielded significant evidence for such contamination of the record."
Chapter 2: Changing state of the climate system	Thus, the conclusion of the matter as of AR5 was not that urbanization accounted for less than 10% of the global trend (itself a baseless claim that originated in Jones' 1990 Nature paper as an offhand conjecture in the conclusions, not a quantitative result in the body of the paper) but that previous papers had attributed as much as half the post-1980 land warming to data contamination, that the AR4 had claimed otherwise with no supporting evidence, and subsequent research had affirmed significant contamination of the surface record exists. If you are going to claim that nothing has been published since AR5 that changes the conclusion since AR5, that is the conclusion you must cite.
Chapter 2: Changing state of the climate system	However another important paper since AR5 is my 2013 Climatic Change paper [McKitrick, Ross R. (2013) Encompassing Tests of Socioeconomic Signals in Surface Climate Data. Climatic Change doi 10.1007/s10584-013-0793-5. Volume 120, Issue 1-2.], which was published only a couple of weeks after the deadline for inclusion in AR5. The IPCC has relied heavily in the past on Parker's argument that the similarity of warming trends between windy and calm nights refutes the claim that urbanization drives warming. Also other teams (including Berkeley) have relied on tests of warming differences between rural and urban locations to support the same view. In my CC paper I showed that these tests are poorly structured because they depend on unstated model restrictions and they can be shown to fail to find contamination patterns even in data sets where it is known a priori that contamination exists. I set up a statistical model which shows that the Parker-type windy/calm equivalence, and the rural/urban equivalence, can be shown to be testable restrictions in general regression models, and the restrictions are rejected on a couple of relevant data sets.
Chapter 2: Changing state of the climate system	Another paper that proves the importance of urbanization over the land record (and which was not included in AR5) is [McKitrick, Ross R. and Lise Tole (2012) “Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods” Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9], in which we combined GCM-generated spatial warming patterns with observed socioeconomic growth measures which are omitted from climate models, and showed that the latter have very significant explanatory power over land even after controlling for the model-predicted patterns. We also showed that when the different explanatory groups are used in combination, we can almost always omit the climate model-generated pattern as insignificant, but we can never omit some of the socioeconomic measures.

Chapter 2: Changing state of the climate system	Therefore, if your position is that nothing has changed since AR5, that means you still take the view that AR4 made a groundless claim to dismiss evidence of surface data contamination and that the available analysis up to that point indicated the contamination is indeed significant. You cannot quote the 10% number since it has never been substantiated, even though the IPCC has repeatedly used it. In earlier AR's the citation to Jones 1990 was provided, which in addition to being long obsolete is a deception since that paper provides no support for the claim. The evidence that has emerged since AR5 further reinforces the view that the land surface record after 1980 has been significantly contaminated by urbanization, that this evidence is not rebutted by making static comparisons of windy/calm or rural/urban sites, and that a signficant fraction of the spatial warming pattern over land cannot be explained by climate models but can be explained by measures of socioeconomic development.
Chapter 2: Changing state of the climate system	Sources: McKitrick, Ross R. and Lise Tole (2012) “Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods” Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9; McKitrick, Ross R. and Patrick J. Michaels (2007) Quantifying the influence of anthropogenic surface processes and inhomogeneities on gridded global climate data. Journal of Geophysical Research-Atmospheres, 112, D24S09, doi:10.1029/2007JD008465. ; McKitrick, Ross R. (2010) Atmospheric Oscillations Do Not Explain the Temperature-Industrialization Correlation. Statistics Politics and Policy Vol 1. No. 1., July 2010. ; McKitrick, Ross R. and Nicolas Nierenberg (2010) Socioeconomic Patterns in Climate Data. Journal of Economic and Social Measurement, 35(3,4) pp. 149-175. DOI 10.3233/JEM-2010-0336.; McKitrick, Ross R. (2013) Encompassing Tests of Socioeconomic Signals in Surface Climate Data. Climatic Change doi 10.1007/s10584-013-0793-5. Volume 120, Issue 1-2.
Chapter 2: Changing state of the climate system	An OLS trend with an AR1 correction is quite inadequate here. Among other problems it contradicts claims in the AR5 attribution chapter that the surface temperature record is I(1). At the least you should use a more robust autocorrelation-consistent estimator such as Vogelsang and Franses (2005) Testing for Common Deterministic Trend Slopes, Journal of Econometrics 126 https://doi.org/10.1016/j.jeconom.2004.02.004.
Chapter 2: Changing state of the climate system	The warming rate at the 300hPa level over 1979-2012 from Sherwood and Nishant (2015) is not supported in other data products over longer intervals. McKitrick and Christy (McKitrick, Ross R and John Christy (2018) A Test of the Tropical 200-300mb Warming Rate in Climate Models. Earth and Space Science doi: 10.1029/2018EA000401) use RAOBCORE, RICH and RATPAC and find warming of 0.17 +/- .06 C/decade over 1958 to 2017, which drops to 0.14 +/- 0.12 C/decade allowing for a break point at 1979, neither of which is significantly different from the 0.14 near-surface rate.
Chapter 2: Changing state of the climate system	Again an AR1 correction was inadequate a decade ago and is unacceptable now. Additionally, the presentation in this table is confusing. If you have estimated warming trends then they should be presented in a uniform format, such as C/decade. The 3 columns show, respectively, C/49 years, C/39 years and C/19 years, making it difficult to compare.

Chapter 2: Changing state of the climate system	Once again use of an AR1 correction here is woefully inadequate. No one would seriously try to use that in a hydrology application. If you don't want to compute a persistence-robust measure (such as Cohn, Timothy A. and Harry F. Lins (2005) Nature's Style: Naturally Trendy. Geophysical Research Letters 32, https://doi.org/10.1029/2005GL024476.) or use a HAC-robust estimator then don't show confidence intervals at all since AR1 is seriously biased downwards in precip and hydrology applications.
Chapter 3: Human influence on the climate system	The assumption necessary for using climate models in D&A is stronger than what is stated here. D&A regressions use the piControl run to generate a pre-whitening operator which, to be valid, requires that the climate model generates not only accurate location-specific time series variances but all cross-sectional covariances as well, and that the expectation of the D&A regression residuals is independent of the product of the piControl covariance and the signal vectors. The way the opening sentence is phrased makes it sound like the model need only generate a reasonable univariate time series variance.
Chapter 3: Human influence on the climate system	I appreciate the note of caution in this paragraph about the list of positive detection findings perhaps being overstated. One of the strong assumptions in D&A methods is that climate models omit nothing relevant for explaining patterns of climate change. In McKitrick, Ross R. and Lise Tole (2012) "Evaluating Explanatory Models of the Spatial Pattern of Surface Climate Trends using Model Selection and Bayesian Averaging Methods" Climate Dynamics, 2012, DOI: 10.1007/s00382-012-1418-9 we showed that the spatial pattern of warming trends over land cannot be explained without including measures of urbanization-related land use change which are not included in models. Once they are included most climate models had no explanatory power over land. I have also done an extensive re-examination of the Allen&Tett optimal detection procedure which is under revision for resubmission at JClim (I don't know the procedure for sharing submission-stage materials with LA's but contact me if you'd like to see it). For 20 years the field has relied on the Allen&Tett Residual Consistency Test, TLS estimation and the claim that P-weighting of the regression model satisfies the Gauss-Markov conditions. I show that these are all invalid claims. The RCT is uninformative as a test of regression misspecification, TLS imparts an upward bias rather than correcting downward bias (the case where it corrects attenuation bias doesn't apply in signal detection regression) and Allen&Tett were mistaken in their presentation of the Gauss-Markov conditions. Standard econometrics tests show that several types of specification error commonly exist in D&A regressions and their remediation substantially weakens attribution results.

Chapter 3: Human influence on the climate system	The summary of optimal signal detection methods makes no mention of the common practise of filtering climate data by "projection onto spherical harmonics". For a revision of a paper submitted to J Clim I have been asked to take account of this step, since in its absence the conventional attribution results weaken substantially and the reviewers insist that filtering using spatial harmonics is an integral part of the methodology. The trouble is that the method has (as far as I can see) never been explained in print, and recent papers simply refer the readers in a chain back to papers in the late 1990s which in turn refer to an unpublished 1972 Danish technical report. AR5 only briefly and tangentially alluded to it. There are no papers that provide sensitivity analyses on a with/without basis (I corresponded with Gareth Jones and while he thought he had once seen one he couldn't recall what it was). My impression is that filtering via spherical harmonics is a "secret sauce" that boosts the significance of results but also introduces bias in the process, that its usage has not adequately been debated and for some reason authors have not been asked to explain their methods or to report on sensitivity of results to its usage. My suggestion is that this section should highlight this matter and caution the reader that a lot of the results to be discussed appear to depend on an arbitrary filtering step and may not be robust to its removal.
Chapter 3: Human influence on the climate system	Given that the general tone of the summary sections is that confidence in attribution has increased since AR5 I think this paragraph should outline more clearly that attempts to make attribution to GHG's distinctly from aerosols has gone in the opposite direction, namely the detection experiments often fail. Jones et al (2016a) didn't simply note the range of results, they found that when the anthropogenic forcing signal is separated into greenhouse gas and other effects including aerosols, the greenhouse gas signal was detected in only 8 of 15 cases (each case being based on using one of 15 climate models over the 1910 to 2005 interval) and varied widely in magnitude across models, that the influence of other anthropogenic effects was detected in only 5 of 15 cases, and that aerosol forcing effects were detected in only 7 cases. They noted that their results add to some other recent studies showing “little consistency in the magnitude of the scaled greenhouse gas warming across a sample of CMIP5 models” (Jones et al. p. 6980) and specificaly called into question the credibiltiy of claiming GHG detection when the model failed to etect another signal that should be just as clear. I know that practitioners have been quick to blame this on "signal degeneracy" and assume that a bit more data or some fancy ad hoc statistical methods will resolve it, but that doesn't square with the discussion in Ch 7 on ECS estimation, which takes the position that the spatial pattern of aerosols much more precisely known now. I think it is more likely that D&A has been relying on the circa 1999 Allen-Tett-Stott ad hoc regression methods that have serious robustness problems and that are not used ANYWHERE outside of climatology for well-known reasons. Given the size of the data sets involved, there should be no difficulty distinguishing GHG and aerosol signals if the influence were as significant as has been claimed, and the effect magnitudes should not jump around so much from study to study.

Chapter 3: Human influence on the climate system	Why does this section only refer to post-1979? McKitrick and Vogelang 2014 ("HAC-Robust Trend Comparisons Among Climate Series with Possible Level Shifts" Environmetrics DOI: 10.1002/env.2294) looked at the nature of the warming of the tropical troposphere over 1958 to 2012 and found the observations were best represented by a step-function with a break in the late 1970s and no significant trend on either side, whereas the models show a smooth trend with no break. Thus not only did the models overstate warming in the tropical troposphere (as pointed out in 3.3.1.2) but they characterize its time profile differently, which goes against the idea that anthropogenic forcings were dominant over the whole period. The Santer references in this section look at relatively short time intervals and don't address the 1970s step change.
Chapter 3: Human influence on the climate system	What is "stochastic uncertainty"? It sounds like a redundancy--like saying stochastic randomness. When you say they "quantified the stochastic uncertainty" does that mean they estimated a variance?
Chapter 3: Human influence on the climate system	Griffin and Anchukaitis (2014) found the 2012-2014 California drought was exceptional in the paleo record, but the change in precipitation was not--it was an ambiguous finding. Christy and McKitrick (Assessing Changes in US Regional Precipitation on Multiple Time Scales Journal of Hydrology vol. 578 Nov 2019, https://doi.org/10.1016/j.jhydrol.2019.124074) show evidence that recent precipitation and drought in western and eastern US records is within natural variability measured either in daily records over 150 years or annual paleclimate records back 2000 years.
Chapter 3: Human influence on the climate system	The discussion does not adequately convey what Figure 3.6 shows. The Figure shows that there is almost no consistency in D&A results from one model to the next. In the 2-way diagram regression coefficients (top left) there are no 2 model outputs that yield the same pair of inferences about ANT and NAT. The lower panels don't seem to connect to the upper panels since all the variabiltiy vanishes. In the top right diagram, switching to a 3-way attribution, namely all the authors do is separate GHG from non-GHG, mainly aerosols, which should have a distinct pattern, yet the results scatter all over the place and become completely incoherent. Again no pair of models gives the same results and MIROC6 explodes (my guess is they are using TLS regression and the coefficients are going to zero but TLS crashes near zero). The top-right panel cannot be described by saying "all models are consistent in attributing most of simulated warming to anthropogenic influences" because there is no consistency among the models. The cautions in Jones et al. 2016a should be noted here: "it is then legitimate to question the confidence of the magnitude of the attributed greenhouse gas warming when another important forcing factor with known strong radiative effects is not detected at the same time. As other anthropogenic influences are not robustly detected, is the factor not important for twentieth century temperature changes? Are there errors or biases in the other anthropogenic response patterns? Are other important factors not being included? Or is the detection analysis methodology flawed?" (p.6980) My opinion based on very extensive reading is that the latter is at the root of the problem.

Chapter 4: Future global climate: scenario-based projections and near-term information	There has been a lot of discussion about the misuse of RCP8.5 and it should be mentioned in this chapter. Examples: Hausfather & Peters Nature https://www.nature.com/articles/d41586-020-00177-3 Burgess et al https://osf.io/preprints/socarxiv/ahsxw/ The issue is that RCP8.5 is not at all a "business-as-usual" (no mitigation) scenario, so it should not be used as a contrast to "mitigation" scenarios.
Chapter 4: Future global climate: scenario-based projections and near-term information	More generally, historical comparison of the range of emission/concentration scenarios to observed CO2 accumulation shows that the "no mitigation" scenario is rght at the low end of the projection range. On this see Figure S4 in the supplement to [Hausfather, Z., Drake, H., Abbott, T. and Schmidt, G. (2019) Evaluating the performance of past climate model projections. Geophysical Research Letters doi: 10.1029/2019GL085378] It's a really remarkable graph and you should look at it, here is the link: https://agupubs.onlinelibrary.wiley.com/action/downloadSupplement?doi=10.1029%2F2019GL085378&file=grl59922-sup-0001-2019GL085378-SI.docx. It contrasts the CO2 concentration forecasts from a wide range of past IPCC assessments (and other studies) against the observed concentrations. Since the 1970s through to today, there have regularly appeared batches of forecasts that spread upwards like a series of sideways-V patterns, reflecting high, medium and low projections. The observations have always tracked the low end of the V. Since they accumulated over an interval during which there was no mitigation policy, the historical implication is that the "no mitigation" scenario is at the low end of the various ranges put forward, going right up to the present.
Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	It is disconcerting that in this chapter you have arbitrarily changed to a new measurement product for global surface temperature, namely using air temperature rather than the customary combined air-SST products. If it is now the IPCC view that air/SST products should never be used you had better get the other chapter teams to rewrite their sections, otherwise it reads like you have cherry-picked the available temperature products to bump up the ECS range. Also, having decided to use surface air temperature, why confine your data to the surface? The chapter makes little or no use of tropospheric temperature products, sonde or satellite, which are associated with low TCS estimates: [Christy, J.R. and McNider, R.T. (2017). Satellite bulk tropospheric temperatures as a metric for climate sensitivity. Asia-Pacific Journal of Atmospheric Science 53(4) 511-518 DOI:10.1007/s13143-017-0070-z https://link.springer.com/article/10.1007/s13143-017-0070-z]

Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	I find it difficult to reconcile the uncertainty and conflicting nature of the information here with your claim of High Confidence. Specifically, you are trying to make a case that the findings of Lewis and Curry 2018, Otto et al 2013, Skeie et al (2014) and others (most of which you don't discuss) who find a low ECS can be set aside because the feedback parameter $-\alpha$ will change over time to become $-\alpha+\alpha'$ and $\alpha' > 0$. Specifically your argument is: "There is high confidence that radiative feedbacks will become less-negative in the future ($\alpha' > 0$) owing to the fact that historical warming has shown relatively more warming in key negative feedback regions (e.g., western tropical Pacific Ocean) and less warming in key positive feedback regions (eastern tropical Pacific Ocean and Southern Ocean) than is projected in the near-equilibrium response to abrupt4xCO2....implying that the true ECS will be larger than the effective ECS inferred from historical warming." Paraphrasing, you are confident the models are right, namely that ECS is high, because the historically-observed warming gradient between the western and eastern tropical Pacific runs opposite to what models predict should have happened, therefore it will happen in the future, therefore the gradient will change, therefore ECS will go up. But isn't it also possible that the models simply get the gradient wrong? That is the argument in Seager et al. (2019), nature.com/articles/s41558-019-0505-x, who say "State-of-the-art climate models predict that rising GHGs reduce the west-to-east warm-to-cool sea surface temperature gradient across the equatorial Pacific. In nature, however, the gradient has strengthened in recent decades as GHG concentrations have risen sharply. This stark discrepancy between models and observations has troubled the climate research community for two decades... erroneous warming in state-of-the-art models is a consequence of the cold bias of their equatorial cold tongues. The failure of state-of-the-art models to capture the correct response introduces critical error into their projections of climate change in the many regions sensitive to tropical Pacific sea surface temperatures." They find that the historically-observed warming gradient is actually consistent with rising GHG levels, which implies it is not going to reverse. Consequently your argument in this section, upon which your main chapter conclusion rests, namely that α will start rising any day now so we'll assume it has already happened, is not only at odds with historical evidence but is based on projections of the Pacific temperature gradient from models now believed to be erroneous.
Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	Further on the discussion of the tropics, you are basing your conjectures about future increases in ECS on the ability of models to represent the tropical climate accurately. But Chapter 2 acknowledges that models don't get the tropical troposphere correct, they systematically over-estimate warming trends there. Many papers have pointed this out. In McKitrick and Christy (2019) which AR6 Ch2 cites, we show that every run from every model in CMIP5 over-predicts warming in the 200-300 hPa layer where the feedback effect is supposed to be strongest, and in most cases the discrepancies are large and statistically significant. Yet here you are making statements with High Confidence that rely on models' ability to characterize accurately the feedback effect over the tropics. See McKitrick, Ross R and John Christy (2018) A Test of the Tropical 200-300mb Warming Rate in Climate Models. Earth and Space Science doi: 10.1029/2018EA000401.

Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	Pages 82-83 is the part of the chapter where you discuss estimates of α' which are critical to your case for revising ECS upwards. You cite Lewis and Curry 2018 and Dong et al (submitted) for values around α' =0.05, which are very small, and Andrews et al (2019) for α' =0.23, and Dessler et al (2018) model simulations that say α' can vary naturally by 0.5. Then on page 83 you discuss climate model simulations showing α' =0.6 and then on page 84 lines 43 to 46 you conclude "Thus, α' is estimated to be in the range 0.0–1.0 but with a low confidence in the upper end of this range." You are over-privileging model simulations here. i think an assessment that really conveys the situation for a reader would show more clearly that if α' =0.05 or 0.06 then the ECS estimates based on historical energy balance estimates will look like those in Lewis and Curry and similar papers, but if α' is closer to 0.5 or 1.0 then ECS will go up in the future, and this issue can't yet be decided. You can't ask readers to take a position based on an assumption that climate models provide accurate forecasts of climate features they have inaccurately represented in the past. Commenting on a different but related issue (aerosol forcing) and whether to privilege model projections over observations, Stevens and Fielder (https://journals.ametsoc.org/doi/10.1175/JCLI-D-17-0034.1) said "Surely after decades of satellite measurements, countless field experiments, and numerous finescale modeling studies that have repeatedly highlighted basic deficiencies in the ability of comprehensive climate models to represent processes contributing to atmospheric aerosol forcing, it is time to give up on the fantasy that somehow their output can be accepted at face value." This warning applies here too.
Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	I haven't looked at the papers cited herein but having published several papers comparing models to observations in the tropical troposphere using satellite and balloon data I cannot see how inferring ECS based on which models match observations better would give you anything other than a preference for the lowest-possible ECS value. The observed warming from 1958 onwards (balloons) or from 1979 onwards (satellites) is at the bottom of the range for models following observed forcings. See most recently McKitrick, Ross R and John Christy (2018) A Test of the Tropical 200-300mb Warming Rate in Climate Models. Earth and Space Science doi: 10.1029/2018EA000401. We have a new paper under review (this link might work for the preprint 10.1002/essoar.10503288.1) showing that of 38 CMIP6 models we tested, all 38 over-predict warming not only in the tropical troposphere but globally as well, and using an "emergent constraint" type of analysis comparing model ECS to global tropospheric warming, only the models with the very lowest ECS are in the range of possibility.
Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	Your reliance on pattern effects regarding aerosols doesn't seem consistent with the recent developments in the Detection&Attribution literature, summarized in Figure 3.6 but also shown in, for instance Jones et al JGR 2016, that when the anthropogenic forcing signal is divided into GHG and Other (chiefly aerosols) detection results more or less fall to pieces. In Jones et al they could only detect the GHG signal in 8 out of 15 cases (each case representing signal vectors from a unique climate model) and the influence of other forcings including aerosols could only be detected in 5 out of 15 cases; they also mentioned a few other papers indicating "little consistency in the magnitude of the scaled greenhouse gas warming across a sample of CMIP5 models". Yet here you are placing a great deal of weight on the ability of climate models to explain and simulate spatial patterns of responses to aerosol forcings.

Chapter 7: The Earth's energy budget, climate feedbacks, and climate sensitivity	I think you should mention that this is the 2nd time the IPCC has raised the likely lower bound of ECS from 1.5 to 2C, and the last time it was subsequently lowered again to 1.5 as new evidence emerged. Groupthink is only one of the cognitive biases you need to consider. In IPCC circles a big problem is conflict of interest: asking people to assess their own work versus that of those who disagree with them, and not being troubled by the fact that the LA's always conclude in their own favour. You have cited a number of papers that place the center of the distribution of ECS values in the 1.5-2.0K range (and you could have cited many more), which implies a substantial part of the distribution lies below 1.5, yet you say it is "virtually certain" it can't be below 1.5. The papers that place the center of the distribution in the 1.5-2.0K range are based on historical observations. Your "virtual certainty" is based on climate model projections that a longstanding temperature gradient over the Pacific will eventually reverse. That is not grounds for virtual certainty, especially when there is evidence (Seager et al. (2019), nature.com/articles/s41558-019-0505-x) that the historically-observed gradient is consistent with rising GHG forcing and the models that have said otherwise are erroneous. It would be more accurate to say something like "If the feedback parameter rises in the future then the feedback parameter will be higher than it is presently." But it is not permissible to add "therefore it is virtually certain no part of the ECS distribution is below 1.5K." Alternatively, if you had a lot of empirical papers showing the entire ECS distribution was > 1.5K and none that showed otherwise, you could claim very high confidence (I'd still be reluctant to claim virtual certainty on anything where you are measuring a weakly-defined physical variable on noisy data) that ECS>1.5K, but you are not in that situation. If your "virtual certainty" is based on the fact that you took a poll around the office and the opinion was unanimous, then re-read your own cautions about the dangers of groupthink.
Chapter 11: Weather and climate extreme events in a changing climate	Trends in precipitation daily extremes in the US are sensitive to the time scale examined. In McKitrick, Ross R. and John Christy (2019) Assessing Changes in US Regional Precipitation on Multiple Time Scales Journal of Hydrology vol. 578 Nov 2019, https://doi.org/10.1016/j.jhydrol.2019.124074 we examined 2,000 year drought proxies and 150 year-long daily precip records from the US Pacific and Southeast region. On the long time scales there were no significant trends, and the apparent upward trend using a 100 year 20th-century sample doesn't hold up either when the sample is extended back to the 1800s (because there were intervals in the mid-1800s with very high extreme precip levels) or when the sample is confined to the post-1970 interval during which the signs of the trends reverse.