United States Department of State

*Bureau of Oceans and International Environmental and Scientific Affairs*

*Washington, D.C. 20520*

November 15, 1995

Sir John Houghton
Dr. L.G. Meira Filho
IPCC Working Group I
c/o Technical Support Unit
The Hadley Centre
Meteorological Office
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Bracknell RG12 2SY
United Kingdom

Dear Sirs:

Attached please find the United States' comments on the text of the Summary for Policymakers of the IPCC Working Group I Second Assessment Report.

The revised Summary for Policymakers is very substantially improved over the earlier versions we have seen. There remain, however, a number of critical issues that must still be addressed in order to assure that the SPM is accurate and reflects the current state of the science. Per your letter of 19 October, 1995, the United States has provided a detailed, line-by-line set of comments which we would like to see included in the collated materials circulated to delegates at the Madrid session.

While we provide detailed comments in our attachment, we would like to highlight the following general concerns with respect to the document:

- The major new result in this assessment is the incorporation of the effects of aerosols on global climate change into analyses of greenhouse gas forcing. In spite of some of our earlier comments on this matter, the present draft still does not present this critical new work in a satisfactory manner.

  For example, the inclusion of greenhouse gas only projections is misleading and confusing. The USG believes that the projections described in the SPM should all be for

greenhouse gases and aerosol -- and not for greenhouse gases only. A possible approach would be to eliminate the twinned (scenario and "constant aerosol") figures, and provide a summary of how the "no aerosol" or "constant aerosol" results differ from these scenario results (and the 1990/1992 findings) in a brief clause or parenthetical phrase at appropriate points in the text.

There also appears to be a significant misunderstanding regarding the IPCC scenarios for sulfur dioxide emissions. It is important to clarify that IS92 emissions scenarios already presume significant controls of sulfate aerosols motivated by local air pollution concerns, with such controls reducing sulfur emissions from coal-fired electricity generation by 90 percent or more in all regions by 2100. The existing text gives the impression that sulfur dioxide concentrations are unconstrained by the IPCC scenarios. This must be corrected.

- The discussion of the relationship between cumulative emissions and concentrations needs clarification. The present text does not include the richness of the stabilization studies regarding the timing of emissions, what the implications are for after 2100, etc.

- It is critical that this assessment base its results on the most-up-to-date, published and generally available information. Unfortunately, some of the more interesting new data contained in the underlying chapters are entirely absent from the SPM -- in particular, additional text should be added to reflect the new information from Chapters 9 and 10. Another example where information is not entirely updated is the global carbon budget for the 1990's; the SPM text still reflects information from the 1980's instead of our most recent understanding.

- In the U.S. view, the SPM, wherever possible, should be made more quantitative. Qualitative descriptions of information may not only be misleading, but do not provide the full flavor of the detail in the underlying material.

- Finally, in comparing the text in the SPM and in the chapters, we have noted several inconsistencies, including some between different sections of the chapters. In keeping with past practice in WG I, it is essential that the chapters not be finalized prior to the completion of discussions at the IPCC WG I plenary in Madrid, and that chapter authors be prevailed upon to modify their text in an appropriate manner following discussion in Madrid.

The United States appreciates the enormous time and effort that has gone into the preparation of this Report. Through our comments, we hope to assure that the final product is not only accurate, but of value to the policy community. We look forward to our discussion on this report in Madrid, and to a final product that reflects well on the IPCC and its enormous efforts to produce what is certain to become a widely read and frequently cited document.

Sincerely,

Day Mount
Deputy Assistant Secretary, Acting
Environment and Development

Attachments:
As Stated

cc: Dr. Bruce Callander

# U. S. Government Specific Comments on the Draft IPCC WG I Summary for Policymakers

**Prepared by the U. S. Delegation for consideration at the IPCC WG I Plenary**

**For Submission on November 15, 1995**

General Comment on Model Simulation Projections Presented in the SPM

The repeated references to results from "no aerosol" (NA) and "constant aerosol" (CA) model runs in the October 9 draft of the WG I SPM are highly problematical in several respects. While modelers may run different types of cases, the WG I SPM should not depart from discussion of the emissions scenarios provided by the relevant experts. The explicit or implicit rationales for doing so provided in the present draft report are all unacceptable. First, the present draft does not show awareness of the scenario content. For example, the repeated assertions that NA and CA scenarios are justified by the need to address local pollution problems (e.g. acid rain), ignore the fact that IS92 emissions scenarios already presume significant controls of sulfate aerosols -- for example, 90 percent or greater removal from electric generation in all parts of the world by 2100. This is why energy-related sulfate aerosol emissions in IS92a increase by a factor of less than two between 1990 and 2100, a period over which coal use increases by a factor of almost seven. Even ignoring the controls already implicit in the scenario, the application of the local pollution argument appears highly selective. The same argument could be used even more convincingly to lower projections of fossil fuel use to reduce emissions of precursors of tropospheric ozone (smog), incidentally reducing carbon dioxide emissions significantly below the levels presented in the IS92 scenarios. That this did not appear to be a major concern of the authors suggests a greater interest in making adjustments that raise rather than reduce anticipated forcing. The need to avoid this perception is self-evident.

A second rationale used to justify the presentation of results for NA and CA scenarios, that aerosol forcing is highly uncertain and poorly understood, is inconsistent with the importance of aerosol forcing to the recent results on pattern matching that are featured prominently in the draft report. We clearly cannot use aerosol forcing as the trigger of our smoking gun, and then make a generalized appeal to uncertainty to exclude these effects from the forward-looking modeling analysis.

A final (implicit) rationale for the presentation of NA results is that aerosol forcing, although important, cannot be incorporated in the full set of climate models. This rationale ignores the purpose of the Summary for Policymakers, which is to provide policymakers with relevant information. Information that a particular region of the world gets drier in a model with no aerosol effects is not useful to policymakers if results in a world with aerosols -- the one we actually live in -- are significantly different. Where improved understanding of the importance of aerosols has rendered certain findings obsolete and irrelevant, it is better to leave these results out of the SPM than to mislead policymakers by including them. It is worth remembering that the purpose of the SPM is to inform policymakers, not to demonstrate how hard the scientific community is working or how smart we are.

While we recommend specific changes in the SPM to account for this apparent misconception on the scenarios assumptions for sulfur dioxide, it is likely that changes may need to be made in the chapters to accommodate this information.

emission reductions followed. Relatively higher emission rates early in the period would have to be compensated by relatively lower rates throughout the remainder of the period, and conversely." Also: "Further, in future centuries, cumulative emissions would need to be even lower than in the twenty-first century to prevent concentrations from rising above their stabilized levels."

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Line 3
Suggested Change: Add sentence: "This forcing is not yet well characterized, but is estimated to be about [10%] increment of the radiative forcing of the long-lived greenhouse gases." The purpose is to make this more quantitative.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Lines 7-8
Suggested Change: Replace "give rise to a negative forcing over particular regions" by "have led to a direct negative forcing of about 0.5 W/m**2, indirect negative forcing is comparable or even larger. While the forcing is focused in particular regions and subcontinental areas, the negative forcing can have continental to hemispheric scale effects on climate patterns."

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Lines 10-13
Suggested Change: On line 10, insert "compensating" before "depletion". Replace sentence on lines 11-13 with the following: "Further, because of control measures taken under the Montreal Protocol and its amendments, growth rates in atmospheric concentrations of CFCs and some other halocarbons have slowed substantially and in some cases actually declined. The direct and indirect radiative effects of these compounds are expected to begin declining slowly over the next decade."

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Line 22
Suggested Change: Delete line, in that it duplicates lines 24-25, and is less clear.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Line 23
Suggested Change: Insert "Over land" before "night-time"

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Line 24
Suggested Change: Insert "1991" before" Mt. Pinatubo"

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Lines 29-30
Suggested Change: Replace with the following: "Global sea level has risen by between 10 and 25 cm over the last 100 years. Approximately 4 to 12 cm of this rise may currently be attributed to thermal expansion of the ocean water and to the measured melting of small glaciers resulting from rising global temperatures. Early observations of the Greenland and Antarctic ice sheets are inadequate to provide a baseline for determining how much of the remaining rise may have resulted from anthropogenic climate change." This statement is more reflective of our understanding as included on pages SPM-22-23 and more quantitative.

Specific Comments

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Line 6
Suggested Change: After "1860" add "when instrumental records began" so as to indicate why the date is given.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Line 11
Suggested Change: The text should mention that the term "radiative forcing" (and other terms put in italics, are defined in the Glossary. Other terms defined in the Glossary should be similarly treated.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Lines-17-19
Suggested Change: Change to: "The growth rates of CO2, CH4, and N2O concentrations were low during the late 1980s and early 1990s. While this apparently natural variation is not yet fully explained, recent data indicate that the growth rates are currently comparable to those averaged over the 1980s." Rationale: Adding concentrations helps to make clear that the text is not referring to emissions. It is not really clear what anomalous rates of rise are. Given large interannual fluctuations of CO2, and the 5-year downturn anomaly, it is premature to claim that a 1-year upswing is a return to a "long-term" trend.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Lines 20-21
Suggested Change: To make the statement more quantitative, rewrite the first sentence to: "Increases in the concentrations of CO2, CH4, N2O, and CFCs since 1850 have increased the radiative forcing by about 2.5 W/m**2." The CO2 radiative forcing of "about 70%" of the total for greenhouse gases disagrees with Chapter 2, which states 64%. It is essential here to clarify for what period the comparison occurs (e.g., since preindustrial, the 1980s, out to 2050, etc.); "currently" is not sufficiently defining. Given Figure 2.4 relates these factors, it would seem helpful for there to be consistency.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Lines 23-25
Suggested Change: Delete; the sentence is unnecessary in this very brief section.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Lines 27-29
Suggested Change: Place a period after "centuries" and then rewrite the second sentence: "The atmospheric CO2 concentration would reach about 550 ppmv by the end of the 21st century and an ultimate equilibrium concentration of about [550 ppmv?] (approximately twice the preindustrial concentration of 280 ppmv)."

Name: Robert Watson Country: USA
Executive Summary, Page SPM.1, Lines 30-40
Suggested Change: The US will draft language to insert here and offer it at the IPCC plenary. Our concern is that this text does not include the richness of what can and has been learned from the stabilisation studies. We expect to draw language from the IPCC 1994 report. Ideas that we expect to cover include: cumulative emissions, timing of emissions, emission paths, etc. We want both qualitative and quantitative points to be made. As possible suggested sentences regarding the qualitative points: "Stabilizing atmospheric concentrations of CO2 at a preselected level depends more on the cumulative emissions released over the time period required to achieve stabilization than on the exact pathway of

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 31
Suggested Change: After "increase" insert "by a further [x to y] degrees" in order to provide some indication of amount.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 24
Suggested Change: Change "2.0" to "2" in order not to imply more precision than is appropriate and that is included elsewhere in the paragraph

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Lines 33-34
Suggested Change: It is essential here to make clear if the projected sea level change is the portion due to thermal expansion and melting only, or if and how the part of the observed sea level change that is not now explained is extrapolated.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 36, 38, and 40
Suggested Change: Replace "by 2100" with "from the present to 2100" to make clear that this is not since the preindustrial period.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 40
Suggested Change: Change "continue to rise" to "rise by even greater amounts" to provide a sense of magnitude of the extended change. It would also be very helpful to quantify the range of potential future increases.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Lines 6-15
Suggested Change: Delete bullet point. Rationale: the purpose of the SPM is to provide information relevant to policymakers. Information about regional results in a no-aerosol world (a thought experiment by modelers rather than a simulation of a scenario developed by relevant experts) is not useful if the results do not carry over into the actual scenarios modelers were given to work with. Parts of this material that do carry over should be incorporated in the discussion on lines 16-20, which itself should be moved up to follow line 5. The material in Lines 12-15 should be revised to be for the actual case with aerosols.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Lines 16-17
Suggested Change: Revise and extend bullet as below, then move it to line 5. Rationale: See discussion for SPM-4 lines 6-15 and general comments. This fix avoids an independent discussion of GHG-only results that would be misleading for policymakers. Recommended revisions:

- The cooling effect of aerosols is not a simple offset to the warming effect of greenhouse gases, but significantly affects the sub-continental patterns of climate change. Model results for thought experiments that do not consider the role of aerosols, including greater surface warming over land than sea, a maximum surface warming in high northern latitudes in winter, and little surface warming over the Arctic in summer, and increased precipitation and soil moisture in winter at high latitudes in winter, may not all hold once the effects of aerosols are taken into account. For example, models ..... (continue with lines 18-19).

Name: Robert Watson Country: USA
Executive Summary, Page SPM.2, Lines 35
Suggested Change: Replace "behaviour of the" with "persistence of the naturally occurring". As an additional point, the text on page SPM.23 refers to unusual behavior since 1977, especially since 1989; the question of whether the 1977 date should be used in the ES should be considered.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Lines 3-11
Suggested Change: This text is not fully consistent with the rest of the SPM and various parts of Chapter 8; because this is such a new and important aspect of the report, we believe particular care must be taken. We believe the text here, with some clarification, does represent current understanding as contained in the body of the chapter, but that the executive summary and concluding sections of the chapter may need to be revised. We suggest the following text for these lines:

- Observed global warming over the past 100 years has increased the global average temperature to levels higher than have occurred in the past 600 years (and probably significantly longer) as a result of natural variability. There is increasing evidence that changes in the latitude-longitude temperature pattern over the last century and changes in the latitude-vertical temperature pattern over the past several decades result from forcings by greenhouse gases, sulphate aerosols, and stratospheric ozone depletion rather than from natural fluctuations. Taken together, these results indicate a detectable human influence on global climate.
- The human-induced component of climate change is consistent with a warming of several tenths of a degree over the last century. Our ability to quantify further the human effect on climate is currently limited by uncertainties in key factors, including longer-term natural variability and the time evolving patterns of radiative forcing by greenhouse gases, aerosols, and other human influences.

It would also help greatly if the figures and captions relating to this point were made self-consistent. Adding the figure showing the geographic pattern of changes matching model expectations would seem to be particularly important.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 13
Suggested Change: Concerning the presentation of temperature projections, it is particularly confusing to have concentration changes referenced to preindustrial levels and temperature and sea level changes referenced to 1990. An additional confusion is that preindustrial was used as the reference in the 1990 IPCC report. We recommend that this section include the change referenced to 1990 and the reference to preindustrial values (in parentheses).

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Lines 16-19
Suggested Change: Replace with: "Through understanding of the global carbon cycle and atmospheric chemistry and radiation, these emissions can be used to project atmospheric concentrations of greenhouse gases and aerosols and the perturbations of natural radiative forcing. Climate models can then be used to develop projections of future climate." This rewrite clarifies better the sequence of what is done and what tools and knowledge is used.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.3, Line 20
Suggested Change: After "simulations" insert "of current and past climate" to indicate why confidence is increasing.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 21-23
Suggested Change: This seems a conclusion that should be included much earlier in the text, for example page SPM.2, line 14.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Lines 24-25
Suggested Change: These lines should be moved to before the discussion on regional patterns (i.e., to before line 12). Also, on line 24 change "tends to lead to an increase in" to "tends to increase the frequency of"

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 26
Suggested Change: To make a more assertive sentence, change first part of sentence to: "There will be a more vigorous hydrological cycle with warmer temperatures; this will translate into..."

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 31
Suggested Change: After this line it would be appropriate to add lines 32-39 of page 9.1 in Chapter 9 to indicate some of the important findings from that chapter. Thus, we suggest adding the following text:

- With slow climatic change, shifts in the competitive balance among species might occur subtly, with minor effects on terrestrial carbon storage. With rapid climate change, direct impacts on growth and survival of particular types of plants could cause dieback and carbon loss before better adapted types become established. this possible asymmetry of terrestrial carbon loss and accumulation under rapid climate change has led to concern that climate-induced transient vegetation changes could release CO2 into the atmosphere, counteracting the biosphere's capacity to take up CO2. The magnitude of this feedback is highly uncertain, but it could be near zero or, with low probability, as much as 200 GtC over the next one to two centuries. The more rapid the climate change, the greater the probability of a large transient carbon release.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 33
Suggested Change: Change "the climate" to "the interglacial climate" to indicate why this period is chosen.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 39
Suggested Change: Change "emissions" to "emissions and biogeochemical cycling" to indicate broader range of uncertainties to be considered.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 40
Suggested Change: Change "clouds" to "clouds, oceans, vegetation, and surface-atmosphere coupling" to include the need to improve projections of the rate of climate change and of the regional patterns of climate change.

Name: Robert Watson Country: USA
Executive Summary, Page SPM.4, Line 41
Suggested Change: Change "climate" to "the atmosphere, oceans, cryosphere, land surface, and biosphere" in that these are what is observed, with climate being the result of a time integral of the observations.

Name: Robert Watson Country: USA
Section 1, Page SPM.5, Lines 1-2
Suggested Change: Change to read "was first established in 1988 to assess..." We are unaware of establishment and re-establishment, and this confusion is not necessary.

Name: Robert Watson Country: USA
Section 1, Page SPM.5, Lines 22-32
Suggested Change: It is important to revise this text to be more quantitative about the statements in earlier IPCC assessments. In particular, it would be helpful to provide numbers for the BaU scenario on lines 22-26 and the ozone depletion effects on lines 30-32.

Name: Robert Watson Country: USA
Section 1, Page SPM.5, Line 35
Suggested Change: The phrase "pathways" only applies to carbon dioxide and not all greenhouse gases. This should be corrected.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 3
Suggested Change: The indication that "continental scale" results are presented is not really borne out in the assessment. This statement should accordingly be modified.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Lines 22-23
Suggested Change: This sentence needs clarification. It would better read: "As a result, more of the infrared radiation emitted from the surface will be absorbed, and this absorption will occur at lower levels in the atmosphere, creating in more back radiation to the surface. In addition, radiation emitted from the atmosphere to space will occur at higher altitudes; because these levels are initially colder, warming must occur until temperatures are warm enough to emit sufficient radiation to balance the incoming solar radiation."

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 26
Suggested Change: Here, "millions" should be "billions"

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 31
Suggested Change: Replace "sulphur-containing gases" by sulphur dioxide" as this is by far the dominant sulphurous gas.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 35
Suggested Change: Insert "(days to weeks)" after "lifetimes" and "decades to centuries" after "gases" to provide a clearer indication of the importance of the difference.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 37
Suggested Change: Insert "primarily sulphur dioxide)" after "gases" to indicate this is the primary gas of interest.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 38
Suggested Change: Insert "(i.e., a few years)" after "transitory" to indicate what is meant.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 40
Suggested Change: Replace "fluctuate" by "varies" to indicate more correctly that these are not random variations.

Name: Robert Watson Country: USA
Section 1, Page SPM.6, Line 39
Suggested Change: Insert "over periods of a few years" after "atmosphere" in order to indicate time period involved.

Name: Robert Watson Country: USA
Section 1, Page SPM.7, Lines 6-8
Suggested Change: For clarity, rewrite start of sentence to: "These will be accompanied by changes in..." and end sentence after "regimes)."

Name: Robert Watson Country: USA
Section 1, Page SPM.7, Lines 10-11
Suggested Change: Replace "man-made" with "anthropogenic"

Name: Robert Watson Country: USA
Section 2, Page SPM.7, Line 26
Suggested Change: Replace "understanding of the role of aerosols." by "representation of the role of aerosols in climate simulations." This is the real accomplishment.

Name: Robert Watson Country: USA
Section 2, Page SPM.7, Line 29
Suggested Change: Insert "Globally averaged" at the start of the sentence for clarity.

Name: Robert Watson Country: USA
Section 2, Page SPM.7, Line 30
Suggested Change: Delete "very largely" as there are no significant non-anthropogenic factors known to have played a role in the increase.

Name: Robert Watson Country: USA
Section 2, Page SPM.8, Table 2.1
Suggested Change: The excess carbon budget (Table 2.1) continues to be summarized in terms of 1980s information rather than updated to reflect estimates of more current rates. Based on summaries contained in this table, some terms of the balance equation can (and should) be updated to reflect new information of the 1990s. Because they have not been updated, some of the excess budget information is not consistent with the latest state of the science. While we know there are difficulties in doing this, those terms that can should be updated (either using current values or including a longer time period); showing such seemingly old data seems unfortunate. In the table, (a) row 1 could be updated (to about 6.1 GtC/yr), this would lead to an update of line 3; (b) WG II (Chapter 24) includes an estimate of regrowth that should be used to update this entry: (c) in that separate estimates

(variously from models and observations) have been made for at least some of these terms (and they are mentioned in the present text), they should be separately listed in an updated table; and (d) if we have listed all known sources and sinks, then we could have a line added for any remainder that would explicitly indicate that this is how it was calculated rather than having plausibly known items included as is now the case for row 7.

Name: Robert Watson Country: USA
Section 2, Page SPM.8, Lines 8-10
Suggested Change: It is our impression that the unusual persistence of the El Nino is also a possible contributing factor. If this is so, it should be mentioned.

Name: Robert Watson Country: USA
Section 2, Page SPM.8, Lines 18-19
Suggested Change: Is it true that forest accumulation of carbon is only going on outside the tropics?

Name: Robert Watson Country: USA
Section 2, Page SPM.8, Lines 20-23
Suggested Change: This sentence gives quantitative estimates for some processes, but not for others, and thus it lacks balance. The sentence also ignores the comprehensive and careful assessment of the literature contained in Chapter 24 of WG II (see section 24.2.2, including Table 24-1) on this issue. The Chapter 24 analysis makes it clear that their best estimate of 0.7+/-0.2 GtC/yr for the global sink flux in mid- and high-latitude forests (reduced in WG I to 0.5+/-0.5 for unstated reasons) includes the combined effects of regrowth, N-fertilization, CO2 fertilization, and climatic effects, as this estimate relies on measured results form national forest inventories (the residual of 1.6+/-0.4 GtC/yr for low latitude forests is a modeled result from a single model). thus, the presentation in these sentences is confusing with regard to what the data indicate for the carbon flux of mid- and high-latitude forests, including soils to 1 meter depth. WG I authors would have to show that mid- and high-latitude non-forest biota could account for the large carbon fluxes suggested by their modeling results, and they have not done this. Until these differences between the WG I and WG II authors are resolved, this text should be deleted and replaced with the following thoughts from Chapter 9 (page 9.1, lines 6-18): "However, direct observations to establish the processes responsible for this carbon storage are generally not available. The cumulative consequences of these several processes must be taken into account when evaluating the future state of the atmosphere."

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Line 7
Suggested Change: Insert "transfer into" before "the deeper layers" to indicate that it is not the condition of the deeper layers that is critical, but the transfer processes.

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Line 8
Suggested Change: Change "50-70%" to "50-70% of that remaining" to clarify what is meant (presuming this is what is meant--if it is not, then further clarifications are needed). Overall, wording needs to be clarified.

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Lines 17-18
Suggested Change: On line 17, replace "lost" with "released" and on line 18 replace "restore" with "sequester" in order to be more consistent with conventional terminology.

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Lines 20-24
Suggested Change: The point of the paragraph about the role of marine biota needs to be strengthened. the first sentence should be replaced by: "Marine biota play a critical role in depressing the atmospheric carbon dioxide concentration significantly below its chemical equilibrium state. Because the marine biotic system is also responsive to climate change, feedback cycles couple the climate and atmospheric chemistry." Additional specific changes include, on line 20, add "to the surface ocean" after "supply". In order not to imply that this could be an overwhelming effect, the sentence on lines 23-24 might be changed to "Improving understanding of such processes may help to resolve some of the discrepancies in biogeochemical models, and changes in such processes in the future may lead to adjustments in the projections of CO2 and other trace gases."

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Line 26
Suggested Change: Change "There is intense interest at present in" to "A key question at present is" to better indicate the limited potential for this approach.

Name: Robert Watson Country: USA
Section 2, Page SPM.9, Line 38
Suggested Change: Change "1721" to "1720" in recognition of limitations in accuracy of measurement network, etc.

Name: Robert Watson Country: USA
Section 2, Page SPM.10, Line 4
Suggested Change: Change "Carbon" to Carbon-14" to be more specific.

Name: Robert Watson Country: USA
Section 2, Page SPM.10, Lines 7-8
Suggested Change: This sentence on methane emissions from natural wetlands should be made consistent with WG II results.

Name: Robert Watson Country: USA
Section 2, Page SPM.10, Line 23
Suggested Change: Table 2.2(a) needs to be updated to include the 1990s. The particular change that would occur is for the rate of atmospheric increase. The number here does not match the statement given on page SPM.19, line 31, where a smaller number is used based on recent rates of increase. This inconsistency can only be corrected by updating the table.

Name: Robert Watson Country: USA
Section 2, Page SPM.12, Table 2.3
Suggested Change: The row of the table concerning the recent rate of concentration change can and must be updated to include the 1990s in order to convey the correct message. As examples of the inconsistency, page SPM.19, line 30 says that methane has been increasing at only 8 ppbv/yr, far below 13; the fluorocarbon rates of increase are totally wrong for the present and give the wrong impression to the reader. A range of annual CO2 increase rates should be given or the title of the row should be changed to "Decadal average rate of concentration change" but then the ranges have to be corrected. It would seem that in updating the table the points in the asterisked note on lines 8-10 could be incorporated into the table itself, which would then mean that ranges of annual increases in concentration are included for all species. Overall, it is very disappointing that this table has not been updated, and we believe it is now misleading.

Name: Robert Watson Country: USA
Section 2, Page SPM.12, Line 12
Suggested Change: In that SF6 is discussed in this section and it is not a halocarbon, the title of the section should be changed to "Halocarbons and other halogenated compounds"

Name: Robert Watson Country: USA
Section 2, Page SPM.12, Lines 15-17
Suggested Change: The sentence does not seem to make sense. Further, in that HCFCs do not significantly perturb the ozone layer, the phrase "(CFCs and HCFCs)" should only refer to CFCs. This would also clarify the next sentence in that the growth rates of HCFCs have not fallen, but are climbing rapidly.

Name: Robert Watson Country: USA
Section 2, Page SPM.12, Line 17
Suggested Change: The reference should be to Figure 2.3, not Figure 2.2.

Name: Robert Watson Country: USA
Section 2, Page SPM.13, Line 25
Suggested Change: The altitude at which the ozone has increased needs to be indicated--is it a specific altitude of the tropospheric vertical average.

Name: Robert Watson Country: USA
Section 2, Page SPM.14, Lines 11-39
Suggested Change: The section on aerosols needs to be made comparably specific and quantitative to the sections describing greenhouse gases. Thus, there should be discussions of sources, sinks, trends, other aerosol types (i.e., organics, nitrates, dust), etc. in order to provide a fuller understanding of how aerosols contribute to this and other environmental issues.

Name: Robert Watson Country: USA
Section 2, Page SPM.14, Line 22
Suggested Change: Delete "upper troposphere and" as the lifetime of aerosols in the upper troposphere is quite short.

Name: Robert Watson Country: USA
Section 2, Page SPM.14, Line 26
Suggested Change: Insert "to a few years" after "months" to provide a more accurate estimate of the duration of stratospheric aerosols.

Name: Robert Watson Country: USA
Section 2, Page SPM.14, Lines 26-28
Suggested Change: This sentence needs to be rewritten to include mention of aerosols from biomass burning and organic aerosols from forests, all of which may be more important that desert dust.

Name: Robert Watson Country: USA
Section 2, Page SPM.14, Line 31
Suggested Change: Delete "knowledge of", the forcing depends on the physical properties. Estimates of the radiative forcing depend on knowledge of the properties.

Name: Robert Watson Country: USA
Section 2, Page SPM.15, Lines 2-7
Suggested Change: That tropospheric ozone will also be a forcing to consider in the future also needs to be mentioned.

Name: Robert Watson Country: USA
Section 2, Page SPM.15, Line 15
Suggested Change: The value given in IPCC (1994) should be given here for comparison.

Name: Robert Watson Country: USA
Section 2, Page SPM.15, Line 30
Suggested Change: Insert "central estimate" for "value" to indicate that this choice was made only because it is near the center of the range.

Name: Robert Watson Country: USA
Section 2, Page SPM.16, Line 29
Suggested Change: Replace "re-calibration, resulting" by "re-calibration for methyl chloroform and on an improved representation of the carbon cycle, which lowers the additional forcing from incremental emissions of the CO2 reference gas from values assumed in developing the 1990 GWPs. This results..." Rationale: The discussion here should be more explicit in noting that consideration of the operation of the carbon cycle on marginal carbon emissions, which lowers the forcing effect of incremental carbon dioxide emissions, is the primary reason for the change in GWPs.

Name: Robert Watson Country: USA
Section 2, Page SPM.16, Line 31
Suggested Change: Insert the following language adapted from the 1994 IPCC Report on Radiative Forcing "The GWPs presented in Table 2.5 were calculated on the assumption that the present background atmospheric concentration remains constant indefinitely. An assumption of increasing CO2 concentrations, such as occurs in all of the IPCC emissions scenarios (see Figure 2.5(b)), would lower the additional forcing of incremental CO2 emissions, thereby increasing the GWPs of other gases relative to CO2." Rationale: Because GWPs are a tool for policy tradeoffs, policymakers should know that "exchange rates" for different gases depend on background concentrations, and that concentrations move with scenarios.

Name: Robert Watson Country: USA
Section 2, Page SPM.18, Line 15
Suggested Change: It is generally inconsistent to reference GHG concentrations to preindustrial but temperature changes to 1990. here, the increase from the present should also be indicated.

Name: Robert Watson Country: USA
Section 2, Page SPM.18, Line 22
Suggested Change: After "projections" insert "of temperature and sea level" to indicate is being referred to.

Name: Robert Watson Country: USA
Section 2, Page SPM.18, Lines 25-26
Suggested Change: Delete sentence. Rationale: See general comments. The statement is gratuitous. It is not clear what evidence there is to suggest that emissions scenarios for sulfate aerosols are more "uncertain" than those for other gases. The role of sulfates in total forcing appears to be well-established, and features prominently in this report (discussions of Pinatubo effect and pattern matching). We cannot focus on the solidity of aerosol results for reaching significant new conclusions in the "pattern matching" area, and then turn around and emphasize unreliability and uncertainty when we are looking forward.

Name: Robert Watson Country: USA
Section 2, Page SPM.18, Lines 40-41
Suggested Change: The question should be rephrased. As stated, it seems to be asking for suggestions of policy and technology instruments. A possible rephrasing would be: "how would stabilizing greenhouse gases constrain emissions in the future?"

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Lines 11-13
Suggested Change: The discussion needs to be clarified. Although the text refers to changes in carbon budgets as underlying the difference between Table 2.6 and Table 2 in the 1994 Report executive summary, the carbon budget as outlined in Table 2.1 is identical to that presented in Table 1 of the 1994 report--the change that is being referred to is apparently present in the models, and not in these estimates. Thus, on lines 12-13, replace "to account for the revised carbon budget for the 1980s (see Table 2.1)" with "with updated carbon cycle models"

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Lines 6-20 and Table 2.6 on Page SPM.20
Suggested Change: The present discussion and Table 2.6 imply the existence of a 1:1 relationship between cumulative emissions and concentrations without regard to emissions timing. This is not the case and the discussion and table need to be revised to indicate this so that decision-makers do not focus on a particular number. Ways to do this include revising the text, using ranges in the table, and/or indicating that the numbers are only valid to, perhaps, +/-15% or whatever is the case. With regard to a possible text revision, it would be appropriate to insert the following new sentences on line 17: "While the results in the table are illustrative, the relationships between cumulative emissions and concentrations are sensitive to the timing of the emissions. In particular, increased front-loading of the emissions profile raises the amount of cumulative emissions consistent with a given concentration objective."

With regard to Table 2.6 and in line with the previous comment and the mention of the 1994 calculations in the text, Table 2.6 should include a column for the cumulative emissions levels actually reported in the 1994 IPCC report, and a clear explanation for the changes in the estimates (e.g., going from results from an ensemble of models to the results of one model, changes in the single model, shifting of emissions profiles in time. The text also needs to make clear, as indicated above, what the reasons are for the differences using the two profiles and that while the new profiles may allow greater emissions in the 21st century, they would imply lower emissions in the 22nd century. In addition in Table 2.6, the provision of point estimates rather than ranges seems inconsistent with the significant uncertainty in the terrestrial carbon sink, which is exactly the same uncertainty presented in the 1994 Report.

Either early next week or for the meeting, we will prepare specific language to suggest.

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Lines 25 and Table 2.6
Suggested Change: The table on emission for the stabilisation case needs to include a second column to reflect the cumulative emissions allowed for the 22nd century, as changing the emissions profiles borrows emissions from the next century. This would be particularly important in differentiating the 750 and 1000 ppmv cases.

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Line 26
Suggested Change: The phrase "mid-range carbon cycle model" needs to be better explained. It is a carbon cycle model whose results fall in the middle of the range of a suite of plausible models.

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Line 30
Suggested Change: As indicated in the comment on Table 2.3, this text is not easily reconcilable with the value for annual rate of increase in the earlier table.

Name: Robert Watson Country: USA
Section 2, Page SPM.19, Lines 38-41
Suggested Change: Revise sentence to read as follows: "The IS92 scenarios assume that significant controls of sulfate aerosols emissions will be implemented throughout the world to address local environmental problems such as acid rain. Implementation of more or less stringent controls than assumed would affect the amount of sulfate aerosols emitted to the atmosphere." Rationale: The present language implies that the IS92 scenarios don't reflect the application of emissions control technology. They do. There is also a perceived imbalance in the failure to consider the effect of "local pollution goals" on emissions of carbon dioxide.

Name: Robert Watson Country: USA
Section 3, Page SPM.20, Line 14
Suggested Change: Given that much of the warming is said to have occurred at night, is there any way to say how much of the 0.3 to 0.6 D change occurred during the day or the night? If so, this would be helpful.

Name: Robert Watson Country: USA
Section 3, Page SPM.21, Lines 1-3
Suggested Change: The size of the changes (a few tenths of a degree in the troposphere and larger in the stratosphere) should be specified here.

Name: Robert Watson Country: USA
Section 3, Page SPM.21, Lines 10-11
Suggested Change: It would help to indicate if the change in the cloud amount and in the change in temperature range provide a rough quantitative match. What are the changes?

Name: Robert Watson Country: USA
Section 3, Page SPM.21, Lines 15-22
Suggested Change: This paragraph needs to be made more quantitative. About how much were the changes, etc.?

Name: Robert Watson Country: USA
Section 3, Page SPM.21, Line 25: The text needs to indicate that "coldest ever observed" only refers back to the period of record, which goes back only about 30-40 years. the present text is an overstatement.

Name: Robert Watson Country: USA
Section 3, Page SPM.21, Lines 38-39
Suggested Change: For clarification, we suggest inserting "period" after "transition" and replacing "(the last 10,000 years, known as the Holocene)" by "that began about 10,000 years ago"

Name: Robert Watson Country: USA
Section 3, Page SPM.22, Lines 13-15
Suggested Change: The description of what is shown on Figure 3.4 is not particularly transparent. That the 1% increase is continuing after 1960 is not at all obvious. That it is appropriate to take a trend over such a record, with its variations and apparent long term increase and decrease is not apparent. A much more thoughtful commentary is needed.

Name: Robert Watson Country: USA
Section 3, Page SPM.22, Line 40
Suggested Change: Insert "measured" before "retreat" to make clear that the discrepancy later discussed is mainly due to a lack of measurements.

Name: Robert Watson Country: USA
Section 3, Page SPM.23, Line 6
Suggested Change: It would help to clarify the significance of this uncertainty by adding a phrase at the end of the sentence: ", because there are insufficient data about these ice sheets from 100 years ago."

Name: Robert Watson Country: USA
Section 3, Page SPM.23, Line 21
Suggested Change: The term "extratropical cyclones" should be explained; this could be done by inserting the phrase "(e.g., major winter storms)" or they should be defined in the Glossary.

Name: Robert Watson Country: USA
Section 3, Page SPM.23, Line 24
Suggested Change: Insert "warm phase" before "episodes" to indicate what has been unusual.

Name: Robert Watson Country: USA
Section 4, Page SPM.24, Lines 19-20
Suggested Change: The accuracy of weather forecasts seems overstated by implying that detailed evolution can be forecast out to ten days. On line 20, replace "determine the detailed" by "simulate the"

Name: Robert Watson Country: USA
Section 4, Page SPM.24, Line 40
Suggested Change: Change "box" to "energy balance" in order to make the naming of this model consistent throughout this report.

Name: Robert Watson Country: USA
Section 3, Page SPM.25, Lines 4-7
Suggested Change: this description seems quite dated. Have not coupled atmosphere-ocean GCMs also been run to near equilibrium? Should not the text also make the point that these more realistic models also give results in this range? Would it not be appropriate to provide some table or diagram indicating the clustering of such model results within the range given?

Name: Robert Watson Country: USA
Section 3, Page SPM.25, Line 15
Suggested Change: Two general additions are needed to the box. Text should be added to indicate which models (even what types of GCMs) are being used for the results to be presented here. The second essential addition is to describe upwelling-diffusion models and

their calibration to the GCMs in that these models are used to interpolate GCM results to the actual scenarios.

Name: Robert Watson Country: USA
Section 4, Page SPM.26, Line 1
Suggested Change: Because it is unclear how "the best climate models" are selected, change to "the most comprehensive ocean/atmosphere models"

Name: Robert Watson Country: USA
Section 3, Page SPM.26, Lines 13-15
Suggested Change: It is not clear why this point is made here. It seems to disrupt the flow from the first point to the third. The text should perhaps be moved to the attribution section.

Name: Robert Watson Country: USA
Section 3, Page SPM.27, Line 1
Suggested Change: Insert "surface" before "temperature" for clarification

Name: Robert Watson Country: USA
Section 3, Page SPM.22, Lines 13-16
Suggested Change: This sentence should be written in a more positive tone, in that GCMs are now doing quite well at simulating many aspects of ENSOs. This phrasing seems to require perfection before admitting the successes. suggested rewrite: "Current coupled ocean-atmosphere model simulations are capturing, although still not fully, many aspects of ENSO events and the observed interannual variations in the atmosphere associated with these events."

Name: Robert Watson Country: USA
Section 3, Page SPM.27, Lines 30-31
Suggested Change: We are concerned that, if flux corrections are varied on a monthly or seasonal basis, claiming that the coupled models reproduce simulate the seasonal cycle is equivalent to saying that the adjustment process is working. Are the flux corrections based on annual average or seasonal values?

Name: Robert Watson Country: USA
Section 3, Page SPM.28, Line 6
Suggested Change: Given continuing limitations in our understanding of feedback processes and our inability to pin down the sensitivity from detection and attribution studies, we would recommend changing "of 2.5 C" to "near the middle of this range."

Name: Robert Watson Country: USA
Section 3, Page SPM.29, Lines 4-5
Suggested Change: While we concur with the first part of the sentence, we are confused by the explanation. In previous reports, the explanation for the North Atlantic not warming as rapidly as other areas has been the deeper mixing of the heat. It would seem that a retarded thermohaline circulation would lead to shallower mixing and a tendency to greater warming, just the opposite of what is said. We also understand that changes in cloud cover in the models, or maybe even sulfate aerosol increases, might be factors. We believe that a fuller explanation is needed.

Name: Robert Watson Country: USA
Section 4, Page SPM.29, Lines 19-33
Suggested Change: The existing text is almost entirely biogeophysically oriented, and ignores the important biogeochemical feedbacks between climate and ecosystems. In that this draft of the SPM is relatively weak in its linking the physical climate systems with the

longer-term biospheric feedbacks to them, this subsection is the most logical place to correct this problem. The suggested substitute paragraphs summarize important work in chapter 9, which is otherwise ignored. The suggested replacement text (drawing from the existing text and Chapter 9) is as follows:

> Terrestrial ecosystems and climate are closely coupled. Changes in climate and the $CO_2$ concentration of the atmosphere cause changes in the structure and function of terrestrial ecosystems. In turn, changes in the structure and function of terrestrial ecosystems influence the climate system through biogeochemical processes that involve the land-atmosphere exchanges of radiatively-active gases such as $CO_2$, $CH_4$ and $N_2O$, and changes in biogeophysical processes that involve water and energy exchanges.
>
> The current generation of GCMs attempts to model some of these processes, but not all. Biogeophysical land surface schemes used in current GCMs may be more sophisticated than in IPCC(1990), but the disparity between models in their simulation of soil moisture and surface heat and moisture fluxes has not been reduced. Confidence in calculation of regional projections of soil moisture changes in response to greenhouse gas and aerosol forcing remains low.
>
> Changes in the composition and structure of ecosystems are capable of modifying surface climate and the overlying atmosphere by altering the exchange of water and energy between the land surface and atmosphere. For example, forests spreading into tundra in a warmer world would absorb relatively more solar energy, thereby increasing warming.
>
> Changes in the functioning of ecosystems are capable of appreciably altering the concentration of greenhouse gases in the atmosphere. Preliminary ecosystem modeling results suggest that a doubled $CO_2$ concentration combined with warmed climate could result in removing in excess of 200 GtC from the atmosphere in an equilibrium situation. On the other hand, a temporary release to the atmosphere of a similar quantity of carbon could result from ecosystem disturbances in the transient condition before equilibrium is established. A decrease in stomatal conductance induced by higher CO2 concentrations would produce profound changes in local, regional, continental, and global hydrological cycles. Methane and nitrous oxide fluxes from ecosystems are highly dependent upon their moisture status, so are very responsive to changing climate.
>
> Such ecosystem feedbacks are subject to additional modification by non-climate related land use changes, such as deforestation. Most coupled ocean-atmosphere models used for climate change studies do not yet include such interactions between ecosystem structure and functioning and climate.

Name: Robert Watson Country: USA
Section 3, Page SPM.30, Line 26
Suggested Change: the phrase "noise in the observations" should be changed to "the natural variability evident in the observations" in order to make clear that it is not instrument noise that is causing the difficulty (although we realize that an inadequate network may also be a factor in contributing noise to the observations, we believe that natural variability is the primary factor).

Name: Robert Watson Country: USA
Section 3, Page SPM.31, Line 5
Suggested Change: We believe it is important in this section to indicate that, on a global basis, the model simulations of GHGs and aerosols are consistent with the general range of model sensitivity (i.e., 1.5 to 4.5 C). This has been done in the past with a diagram that compares the observed record with model projections for various sensitivities. Figure 6.3 in Chapter 6 provides such a comparison for one GCM run, and Figure 6.5 may also be

useful, but what is needed in the SPM is a diagram based on the upwelling diffusion model results. Such a diagram would serve as a complement to the pattern results.

Name: Robert Watson Country: USA
Section 3, Page SPM.31, Line 30
Suggested Change: We are confused by the word "their". Is this referring to limitations in the treatment of aerosol effects (poor understanding of aerosol radiative properties, poor treatment of scavenging, no indirect forcing, non-moving aerosols, etc.) or to model neglect of other potentially important forcings (e.g., stratospheric and tropospheric ozone, dust, land use change, etc.). We believe both interpretations contribute to the concerns, but are not sure this will be understood. Rewriting of the sentence is needed by the authors to make themselves more clear.

Name: Robert Watson Country: USA
Section 3, Page SPM.31, Line 17-38
Suggested Change: We believe that the detection and attribution issue is so important that an additional figure showing the horizontal pattern of observed and modeled changes is needed. Both the Santer-Penner and the Tom Karl figures generally accomplish this. We have suggested other figures to remove, so believe this addition can be accommodated.

Name: Robert Watson Country: USA
Section 5, Page SPM.31, Line 37
Suggested Change: Replace "The pattern correspondence is generally" by "For most seasons, the pattern correspondence is" to make clear what is actually the case.

Name: Robert Watson Country: USA
Section 5, Page SPM.32, Line 13
Suggested Change: We suggest retaining the following paragraph from the 10 August 1995 draft: The apparent reason for dropping this paragraph was that scientists should not be concerned about short-time-period disagreements between modeling results and data. While this is true, this is a summary for policy makers. As such, most policy makers have, or will be, confronted with this apparent anomaly. We would be seriously misguided to delete this short assessment of the science pertaining to a prominently-used objection to public concern with global climate change. The text to be added back in is as follows:
One apparent inconsistency between observed changes and model predictions that has received considerable attention recently is the satellite-based observation of a slight cooling in the low- to mid-troposphere since 1979. Recent studies show that this inconsistency is more apparent than real. When the satellite data are corrected for volcanic and ENSO effects and are compared with rates of temperature change in model experiments with combined forcing by $CO_2$ and sulphate aerosols, the satellite data and model predictions are in reasonable agreement.

Name: Robert Watson Country: USA
Section 6, Page SPM.32, Line 37 to Page SPM.33, Line 1
Suggested Change: Delete two sentences. Rationale: Same problem here as SPM.19, Lines 38-41. Contrary to the implication of the draft text, the IS92 scenarios already assume sulfate aerosol control motivated by local air pollution concerns. We know of no analysis indicating that this is more "uncertain" than any other part of the scenarios. Even if it is that uncertainty is two-sided rather than one sided. Finally, the leap from an assertion of uncertainty to the arbitrary assumption of a constant aerosol effect is not justified (or justifiable). The uncertainty is two sided, not one sided. Moreover, the same logic applies to other "local" environmental problems associated with energy use itself, such as urban smog and particulate matter. Logical consistency with the approach taken for aerosols

would require that CO2 emissions be arbitrarily adjusted down. It is better to stay out of this game entirely.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Line 1
Suggested Change: Delete "probably". With high confidence this statement is true without this equivocation.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 5-10
Suggested Change: Delete to end of paragraph after "sensitivity." Rationale: Same problem here as SPM.19, Lines 38-41 -- contrary to the implication of the draft text, the IS92 scenarios already assume sulfate aerosol control motivated by local air pollution concerns.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 21-22
Suggested Change: Delete sentence as it is not one of the IPCC scenarios.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 22-23
Suggested Change: Delete "both with and without changing aerosols" and Figure 6.1(b). Revise Figure caption accordingly. The case without aerosols is not of use to policymakers.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 25-26
Suggested Change: Delete ", it can be seen in Figure 6.2 that". Delete Figure 6.2

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 30-31
Suggested Change: Delete. Also modify Figure 6.3 and caption to eliminate "constant aerosol" cases.

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Line 33
Suggested Change: Correct spelling of "Global"

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Line 35
Suggested Change: Delete "both with and without changes in aerosol concentrations."

Name: Robert Watson Country: USA
Section 6, Page SPM.34, Lines 40-41
Suggested Change: Delete sentence and Figure 6.4(b). Modify figure caption.

Name: Robert Watson Country: USA
Section 6, Page SPM.35, Lines 7-8
Suggested Change: Delete "and about 15 cm to 110 cm when they are neglected." Modify Figure 6.5 to eliminate "CA" cases. Rationale for all of above: Per general comments and specific points above, de-emphasize "constant aerosol" cases. Note that CA cases are especially inappropriate for sea-level rise, which reflect an integral of temperature change.

Name: Robert Watson Country: USA
Section 6, Page SPM.35, Line 27
Suggested Change: The first point should refer to the changes in the projections of CFC concentrations. The point could read: "The accelerated phaseout of CFCs as a result of the projected implementation of the Montreal Protocol on Substances that Deplete the Ozone Layer and subsequent amendments has significantly reduced the projected radiative forcing in the next century and therefore reduced the projected temperature change, even though not affecting the model estimated sensitivity to a doubled CO2 concentration."

Name: Robert Watson Country: USA
Section 6, Page SPM.36, Lines 2-6
Suggested Change: Delete. Rationale: For reasons outlined in the general comments, this rationale for the relevance of "no aerosol" model results regarding regional level climate changes cannot be accepted. Aerosols, which are included in the scenarios at highly controlled levels in the long run, are important to regional distribution of impacts in the long run as well as the short run. Given the importance of aerosols, it is much better to limit reporting available results that correspond to the emissions scenarios than to report results that apply to an arbitrary aerosol-free world. To do otherwise risks supplying policymakers with information that is known to be misleading -- see earlier comments on SPM-4.

Name: Robert Watson Country: USA
Section 6, Page SPM.37, Lines 7-8
Suggested Change: The last sentence is ambiguous and poorly worded. Does this conclusion apply everywhere, including continental interiors, or is it a zonal average?

Name: Robert Watson Country: USA
Section 6, Page SPM.37, Lines 37-38
Suggested Change: Suggest re-wording the parenthetical statement to read, "(the type of model that gives more reliable information on a regional scale)". The present text would invalidate nearly all of the impact assessment work reported by IPCC W/G 2, which strongly relied upon equilibrium AGCM modeling results.

Name: Robert Watson Country: USA
Section 6, Page SPM.38, Lines 29-31: SPM-35, Lines 22-24
Suggested Change: The statement here should address the net change in exposure to droughts and floods. It is unclear whether the present draft statement is meant to imply such a change, or merely a redistribution of droughts and floods with an uncertain net effect on human and natural systems. It is important to distinguish a change in spatial distribution from a change in net incidence.

Name: Robert Watson Country: USA
Section 6, Page SPM.39, Line 22. Eliminate constant aerosol case from Figure 6.9.
Rationale: Same as previous comments on this point.

Name: Robert Watson Country: USA
Section 6, Page SPM.40, Line 19
Suggested Change: Insert "the hydrological cycle" after "forcing," and replace "release" with "and methane cycling" The additions are of equal potential importance as the other examples.

Name: Robert Watson Country: USA
Glossary, Page SPM.41, Line 36-37
Suggested Change: Delete the sentence: "It is generally not possible clearly to make attribution between these causes." The statement is superfluous to a definition of climate change; and hopefully, the thought it expresses will not be a constraint on the science of tomorrow. The statement is also inconsistent with the rest of the SPM regarding detection and attribution.

Name: Robert Watson Country: USA
Glossary, Page SPM.43, Line 8
Suggested Change: The term "stabilization scenario" should be included in the glossary if it is ultimately used in the text.

Name: Robert Watson Country: USA
Glossary, Page SPM.43, Line 9
Suggested Change: Use the same formulation for describing the upper bound of the troposphere as is used to describe the lower bound of the stratosphere.

Name: Robert Watson Country: USA
Figures, Page SPM.44, Line 1 (Figures)
Suggested Change: As noted in the general comments, the discussion and figures should focus on the results for the actual IS92 scenarios. No Aerosol (NA) and constant aerosol (CA) should be discussed only parenthetically, if at all. The twinned ( IS92 scenarios and "constant aerosol") figures should be eliminated, because they convey a misleading impression of equal status between the IS92 scenarios and arbitrary excursions with revised aerosol loadings. The general comments above suggest that this is highly inappropriate. Only brief clauses or parenthetical phrases should be included at a small number of appropriate points in the text indicating how CA and NA results would differ from the IS92 scenario results.

Name: Robert Watson Country: USA
Figures, Page SPM.46, Line 18
Suggested Change: Insert "June 1991" before "eruption" to indicate relevance of figures provided.

**Chapter Coordinator Comments on Chapters**
**IPCC WG I Draft of the Summary for Policymakers**

(Nov. 15, 1995)

Chapter 1: The Climate System: An Overview

V. Krishnamurthy: In the new version of Chapter 1, most of our previous concerns which were expressed in the general comments have not been addressed. However, among the modifications suggested in the specific comments, about 40% have been implemented. Because of not addressing our major concerns, the flow of the chapter still has rough transitions. The discussion in the introductory section on Climate and Climate System still lacks coherence and the definitions are presented in a disorganized manner. The section on greenhouse effect (1.2.2) should have included a discussion on aerosols. The section on climate models (1.6.1) is still inadequate without the discussion on different types of models used, why they are used and when they are used. However, the chapter contains a good improvement in the discussion of climate response which is now presented as a separate section (1.4). This section has a better and expanded discussion on the feedbacks and the newly included subsections on the role the oceans and the role of land have provided an appropriate and needed discussion. The last subsection in now more appropriately titled as climate projections instead of climate prediction and includes a few minor modifications which seem to be quite appropriate.

Chapter 2: Radiative Forcing of Climate Change

Roger Dahlman: Section 2.1 on "CO2 and the Carbon Cycle" is somewhat improved. It contains the most recently reported data on atmospheric CO2 measurements, and also recently published analysis of sink estimates for the excess CO2. Except for a few sections (described below), the conveyed information seems to be state of the art, writing style is effective, and there are scientific foundations for interpretations. The August draft is much more coherent than the earlier April draft, and it reads as a stand-alone document. The excess carbon budget (Table 2.1) continues to be summarized in terms of 1980s information rather than updated to reflect estimates for the 1990s. Based on summaries contained in this table, some terms of the balance equation can (and should) be updated to reflect new information of the 1990s, but have not been, and therefore some of the excess budget information is not consistent with the latest state of knowledge. Criticisms of the April draft concerning this issue still apply to the August revision. As Wigley points out in another report (CGER IO18-95), the terrestrial sink term is significantly larger when component central values (i.e., for CO2 fertilization, N-stimulation of growth, and climate effects) are summed to give 2.35 GtC/yr (this quantity is also supported by the Ciais et al. analysis) than when the sink is estimated by difference (i.e., 1.3 GtC/yr from line 7 of Table 2.1) as is the practice of IPCC Chapter 2. Also, the well-estimated emissions term (i.e., line 1) becomes 6.1 GtC/yr when mid-1990s data are used (this point is acknowledged in the text, but the value should be changed in Table 2.1). Although the CO2 growth rate anomaly is examined in considerably more detail, it would seem important for Dave Keeling to review the assessment presented in Sec 2.1.2. Keeling has been a central figure in making the measurements, and in providing primary interpretation. When examining the controversy over CO2 lifetime, including what can or cannot be inferred from decline of atmospheric $^{14}C$, the arguments of the discussion are difficult to follow; although the conclusion seems logical. It would be helpful if the arguments could be presented in a more straight-forward and less awkward way. [Note by M. MacCracken: The USG is

having intensive discussions about the carbon cycle presentation in the SPM, and this comment represents only one part of this exchange--and not all concur with it; the important point to make here is that we believe that the carbon budget table does not fully reflect the state of current understanding nor are the table and text in accord with points made in the forest chapters in the WG II report concerning carbon uptake, etc. We hope to be offering suggestions in this regard that will ensure that the SPM text (and thence the chapter 2 text) reflect what is known from both model results and observations.]

Roger Dahlman: There seems to be a discrepancy in the relative fraction of radiative forcing assigned to different greenhouse gases. For CO2, Chapter 2 states 64% (Sec 2.4.1) where the SPM says "about 70%" (SPM, ES 1).

Roger Dahlman: The aerosol discussion could be tightened up. The text seems to target somewhere between a literature review and an assessment of what is known, unknown and uncertain. For the non-aerosol expert it is difficult to track from the diffuse bits of literature information to an assessment or consensus and ultimately to a derivation of the forcing term.

EPA: Geographical Resolution of GHG Emissions: To maximize the utility of this report to policymakers, the authors should separate the human controlled or influenced sources for each GHG from the natural sources. For the human controlled or influenced sources, emissions should be reported at country and regional levels where such emission estimates are available. This level of reporting emissions is important for the purpose of providing an independent check on country submissions to National Action Plans under the FCCC, and to identify gaps in emission information where these exist. (For methane, a useful source for this purpose is Adler, M. J., ed., Jan. 1994: International Anthropogenic Methane Emissions: Estimates for 1990; USEPA Report to Congress.)

EPA: Currency of GHG Source and Sink Estimates: There does not appear to have been a sufficient amount of work done to assess the recent literature on methane emissions and sinks in particular. The sources and sinks estimates in this chapter should be closely coordinated with those being developed by authors of relevant chapters of IPCC W/G II for the SAR. For example, the authors of the W/G II agricultural mitigation chapter have developed estimates of agriculturally-related nitrous oxide emissions which are substantially at variance with the estimates in this chapter. It is recommended that, even for methane emissions where estimates agree within the relevant uncertainties, an effort be made between W/G I and W/G II authors to harmonize source and sink terms for each sector of each GHG to eliminate unnecessary differences between the two sets of estimates, thereby eliminating one source of confusion for the less-than-fully scientifically literate.

EPA: Identification of New Understanding and its Policy Relevancy: As this document is intended to provide policymakers with a synopsis of recent advances in scientific understanding regarding GHG contributions to radiative forcing, it should clearly summarize these advances, and indicate their policy relevancy. It falls quite short of this goal.

EPA: Treatment of Chemistry/Transport Models: The inclusion of model intercomparisons and modeling results for Chemistry/Transport Models (CTMs) is an important new effort made by the authors of this document. The further development of such models will provide challenges for many years to come. However, this draft is not careful enough in assessing the areas of application appropriate for 2D and 3D CTMs; it should compare modeling results against observed data as well as other modeling results, and should not overly raise expectations that anticipated development of CTMs over the next 5-10 years will necessarily remove the rather large uncertainties in resolving tropospheric chemistry issues.

Chapter 3: Observed Climate Variability and Change

No comments were received from the U. S. chapter reviewer.

Chapter 4: Climate Processes

Sankar-Rao Mopidevi: We (also Jay Fein) find to our satisfaction that the new version incorporates most important US comments and so is vastly improved. The new version of chapter 4, which we reviewed earlier, addressed the most important concern regarding upper tropospheric water vapor's effect on radiative forcing in a balanced way. This new version of chapter 4 clearly brings out the need for further observational and model research on this aspect which is not yet totally understood.

Chapter 5: Climate Models--Evaluation

Dave Bader: Chapter was originally, and is even more now, in quite good shape. No specific comments

Chapter 6: Projections of Future Climate

Ken Bergman: I have read over the 9 October 1995 draft version of Chapter 6 and have compared it with the earlier draft version of 24 April 1995. So far as I can determine, most of the critical comments and suggestions that were made for improving the earlier draft have been implemented. The revised version is improved, both stylistically and in terms of more precise statements of fact or inference. The several authors are to be praised for their efforts to improve the chapter. Although I might have said some things differently, I did not find any statement or conclusion whose essential correctness, in light of the available evidence, could be challenged. The aerosols sidebar (which I don't believe was in the earlier version) is good.

There are a few problems. One is the use of ranges of climatic response (in temperature, sea level rise, etc.) that include both the range of greenhouse gas forcing scenarios and the range of climate sensitivity. The current version is more careful in indicating when and how this has been done than was the previous version, but the possibility of some confusion of less-informed readers exists. Another problem is the use of unrefereed research results in the report. While the desire to include the latest model results is understandable, the danger of basing the IPCC assessment on results that later prove to be faulty is increased. Finally, the concluding section of the chapter (Section 6.7: Reducing Uncertainties, Future Model Capabilities, and Improved Climate Change Estimates) seems inappropriate for this chapter. Most of this section is about improving the representation of climate processes in models, so it most logically should be at the end of Chapter 4.

Chapter 7: Changes in Sea Level

James Titus: IPCC has made some of the changes we suggested, although many of them were also ignored. The change that we gave the greatest emphasis to--allowing for the possibility of a positive Antarctic contribution--was made. The most important change to the chapter is that the numbers themselves have changed, presumably because Tom Wigley is tinkering with his model; although Tom is not one of the lead authors, he appears to be providing the estimates not only of temperatures and thermal expansion, but of the small glacier contribution as well. What's more surprising, however, is that a completely new set of estimates by Oerlemans (who is a lead author) has been introduced, suggesting a rise of only 27 cm. IPCC will have to decide which estimates to use, a matter on which the U.S. might want to weigh in.

Recommended U.S. Comment: The alternative set of sea level rise estimates in section 7.5.3 should be deleted, perhaps with some of the models used in that section being folded into the revised projections if appropriate. For example, the Bintanjua and Oerlemans model of thermal expansion might be used in projecting the IPCC low scenario; but the best-guess scenario should continue to use the Wigley and Raper model.

The one other comment that we might want to consider, if appropriate, is the characterization of the Titus and Narayanan study. I say this not because I am the author of that study, but because questions from Bob Watson and Tony Socci suggest that there is some confusion about the appropriate comparison. The April draft correctly noted our projection of a best estimate of 45 cm; the current version changed that figure to 34 cm.

Recommended U.S. Comment. In table 7.7, change the best estimate for Titus and Narayanan from 34 cm back to the 45 cm that was listed in the April draft. For the range, change the "5 to 77" range to "19 to 110" with the footnote being changed to read as follows: "The high end of the range is estimated to have a 1 percent chance of being exceeded; the low end has a 90 percent chance of being exceeded. Projections include extrapolation of nongreenhouse contributors to sea level."

Chapter 8: Detection of Climate Change and Attribution of Causes

John Bell: The new draft (10/9/95) responds well to most of the comments of the United States on the earlier (4/18/95) draft of Ch. 8, although some specific suggestions were inexplicably ignored. It is generally well written and deals with some difficult concepts more lucidly than the previous version did. Quite a bit of new material has been added and previously included material deleted.

The one major difficulty I have with the new version is that, perhaps in reaction to the earlier criticisms, it stresses the uncertainties in the climate signal and noise estimates so much that the summary at the beginning of the chapter seems to be at odds with the tenor of the body of the chapter. The discussion of the uncertainties in climate noise, particularly those surrounding the temperature spectrum in Fig. 8.1, seem to imply that our present knowledge of the size of natural decadal- to century-scale changes is still rather uncertain. The discussion of the apparent importance of aerosol forcing for understanding the pattern of temperature change and the lack of a good climate model run including such forcing makes the present knowledge of the climate signal to be looked for equally uncertain. The summary statement "Taken together, these results point towards a human influence on climate," though carefully crafted, is therefore jarring. What is missing is the strength of scientific assurance with which this statement is made. One could say "Given our present knowledge, there is a 5% chance that the changes in climate are natural in origin," or one could say, "Given our present knowledge, there is a 49% chance that the changes are natural in origin." In both cases our best guess would be that the changes are anthropogenic in origin, but the guess is considerably weaker in the second case than the first. (I suspect we are somewhere in between these two.) What is missing from the summary statement is just how sure we are that our best guess is the right one. This needs to be somehow made explicit, or the summary can be legitimately criticized for leaving out important qualifications of the statement.

Specific remarks:

Page SPM.8.1, Line 9: "... are both purely external and externally-driven". Should be "... are both purely internal and externally-driven".

Page SPM.8.5, Line 3: "... independent of the spatial character ..." should be "... independent of the temporal character ...".

Page SPM.8.13, 3rd paragraph: The description of the significance of the Karl et al. results for the GCRI is not very convincing, since visual examination of the graph shows quite large swings in the index during the period before 1976. An 80-year long time series is rather short for investigating the significance of a 20-year excursion. The statistical methodology used here is open to the criticism that the statistical test was constructed after examining the data to find a hypothesis least likely to pass the test (that is, in the choice of the time interval over which the rise in the GCRI was considered). This example should be presented more cautiously.

Page SPM.8.14, 2nd to last paragraph: The result of Hasselmann et al. (1995) described here is disturbing and deserves more discussion. It seems to suggest that the addition of aerosols to the model improves the agreement of the climate model prediction with observed changes in the global mean temperature, but that the regional variations in temperature about the global mean agree better with the model without the addition of aerosol forcing. This seems to be in conflict with the results described earlier in this section.

Page SPM.8.16, Line 8: "Figure 8.12" referred to here does not exist.

Page SPM.8.17, 2nd to last line of Section 8.5.5: "Figure 8.2" should be "Figure 8.3".

Chapter 9: Terrestrial Ecosystems: Biotic Feedbacks to Climate

Diane Wickland: The revised Chapter 9 is much improved and very responsive to our major criticisms of the last draft. A key message is rather hard to extract. My best interpretation would be as follows: We know that ecosystems and climate are closely coupled and changes in one effect changes in the other. Quite a lot is known about how ecosystems respond to changes in single climatic factors, but much less is known about how they respond to changes in many factors, especially over the long-term. In a decades to centuries time-frame of ecosystem response, we should expect transient responses that are even more difficult to predict and, if change is fast, could have dramatic impacts. It is possible to predict responses using combinations of experimental and paleoecological observations and ecological and climate models, but there are substantial uncertainties (to the point of not even getting the direction of change right) associated with the predicted responses and their subsequent feedbacks to the climate system.

The revised Chapter 9 is very different from the last version. I think the organization is much improved, and the careful delineation of kinds of evidence (observations/models) is nicely done. The sections methane and nitrous oxide are welcome, and the roles of moisture and land use change are much better handled. This version reads more like a general science audience literature review and less like an assessment document. The lack of short "bullets" in the summary is an example, although I can see why this was done. The chapter does a good job capturing what we know and sizing the uncertainties, but it does not draw many conclusions. In this, it is very responsive to our criticisms, but may be somewhat less satisfying in an assessment. I believe it to be an honest statement of what is known and it does a better job of portraying both schools of thinking. Thus, I find it acceptable as is.

Chapter 10: Marine Biotic Responses and Feedbacks to Climate Change

Paul Falkowski: While improved, the revised draft falls short of what is needed in the IPCC report. My comments are as follows:

1. The authors failed to include any substantive information related to higher levels of the marine food chain. If they insist on leaving this section out, they should so acknowledge in the introduction to the chapter.
2. The chapter is a collection of what some biological oceanographers think are important linkages between climate change and ocean biota. Overall, the chapter is not coherent, nor does it provide any blueprint for research strategies. The chapter seems to be written with other oceanographers in mind - and no quantitative framework is given to provide answers to such questions as how the oceans will respond if the atmospheric content of CO2 doubles, how long will such a response take, and what will be the consequences of that response.
3. The issues of new production, DMS, etc., seem to be written in the context of steady state cycles. Is DMS more important now than at the beginning of the industrial revolution? Is it more important in the Southern Hemisphere than the Northern Hemisphere? Is new production changing in the ocean? If so, why? Does it affect the net CO2 exchange with the atmosphere? If not, why is it important to study in the context of the IPCC? How are nutrients external to the ocean changing? Is this only happening in the coastal ocean? Has desertification or volcanic activity affected the flux of iron to HNLC regions? (The authors might include a reference to Genine et al Nature, 1995, Oct. 12 issue.) The coastal ocean sections are confusing. Is the coastal ocean exporting or importing carbon? If denitrification is increased, how can the coastal ocean be exporting carbon? How has or will thermal structure be related to the ocean carbon cycle? We have heard for over 20 years that the ocean carbon cycle has uncertainties of 100% - is that the best we can do after spending untold millions on JGOFS. If so, how much will it cost to reduce the uncertainties to 50%?, 10%? Why should we care about changes in the Redfield ratios unless (a) they continuously change in the same direction, or (b) the biomass in the ocean becomes a significant store for carbon (i.e. there is an net increase in biomass each and every year that is significant?) Is radiative transfer in the ocean related to ocean color? Is that in turn, related to phytoplankton distribution and mixed layer depth? Is that important in the heat budgets?
4. The chapter advertises that it will discuss feedbacks, but there is no real discussion of feedbacks that is important to atmospheric radiative forcing. There is no real diagram showing any feedbacks. The chapter should describe relevant feedbacks, provide some hypotheses to test, and indicate how to test them.
5. What do we need to do to improve models? Do we only need bigger, faster, better computers? Do we need better observations? If so, of what type? Do we need to make some new models, or can old ones do? How can we tell if models are good or bad? How can we relate ocean models to atmospheric models?

Over all, the chapter falls far short of convincing anyone outside of the oceanographic sciences that biological processes in the ocean are important in the non-steady-state carbon, nitrogen and/or sulfur cycle, that such processes are important in global climate change studies, or that oceanographers have made any progress over the past 20 years or have a coherent vision as to where they are going in the next 20. Perhaps it is a statement of the science at this point in our history. I hope and believe that is not the case.

Chapter 11: Advancing Our Understanding

V. Krishnamurthy: The new version of Chapter 11 contains a reorganization (or reordering) of the sections and the entire chapter has numerous rewritten parts that have made the chapter much better, even though the earlier version itself was well written. Most of the previous concerns we expressed in the general comments have been addressed. The notable exception is the lack of discussion on terrestrial ecosystems and marine ecosystems and the essential assessment products from chapters 9 and 10. The chapter now starts with a summary that includes a list of climate research priorities which has been completely rewritten in a better way to include all the issues. The new list has addressed our major concerns by including the need for research to focus on the hydrological cycle, precipitation, natural variability and climate record from proxy indicators among other issues. This priority list is also given in the last section (11.11). The Introduction has been expanded and provides a better discussion. The modifications suggested in our previous specific comments have been implemented.

Comments from Derek Winstanley

A major issue is whether or not we can state with confidence that we are able to attribute all or part of any observed climate change to human activities. Even after the chapters have been revised, there still appear to be major inconsistencies within the IPCC documents as to whether scientists can, with confidence, attribute climate change to human influence. The following extracts illustrate my concerns, starting with a conclusion in the WG1 Policymakers's Summary, followed by extracts from the WG1 Chapters:

WG1, SPM, Page 3, lines 7-8. "Taken together, these results point towards a detectable human influence on global climate."

WG1, SPM, Page 27, lines 5-6. "The model results exhibit natural variability on a wide range of time- and space-scales which is broadly comparable to that observed."

WG1, Chapter 8, Page 8.7, lines 20-25. "For these reasons and many others, scientists have been unable to use paleoclimate data in order to reconstruct a satisfactory, spatially-comprehensive picture of climate variability over even the past 1,000 years." "Without a better paleocliamtic data base for at least the past millenium, it will be difficult to rule out natural variability as an explanation for recent observed changes, or to validate coupled model noise estimates on century time scales (Barnett et al., 1995)."

WG1, Chapter 3, Page 3.22, lines 28-30. "However, at this point, it is not yet possible to say whether, on a hemispheric scale, temperatures declined from the 11th-12th to the 16th-17th century. Nor, therefore, is it possible to conclude that global temperatures in the Medieval Warm Period were comparable to the warm decades of the late 20th century."

WG1, Chapter 6, Page 6.22, lines 36-38. "Even using a 100-year sampling window (Figure 6.29b) they found that the model's internally generated fluctuations of ENSO amplitude on a multi-century time-scale can be comparable in magnitude to the long-term change due to increased CO2. These studies point to the complications that arise due to inherent long-time-scale variability, and the difficulties that variability presents for analyzing changes of the climate system on almost all time scales due to increased CO2, as well as for a detection of a CO2-induced climate change signal (see Chapter 8)."

WG1, Chapter 8, Page 8.8, lines 41-43. "One preliminary attempt to compare decadal- to century-time scale natural variability estimates from coupled models and paleoclimatic data suggests that there is disagreemnet between the two, both in terms of patterns and amplitude of variability (Barnett et al., 1995; see Figure 8,2)."

WG1, PMS, Page 41. Glossary - Climate Change (IPCC usage). "Climate change as referred to in the observational record of climate occurs because of internal changes within the climate system or in the interaction between its components, or because of changes in external forcing either for natural reasons or because of human activities. It is generaly not possible clearly to make attribution between these causes."

WG1, Chapter 8, Page 8.15, lines 19-22. "The four studies also show that these combined signals are detectable relative to current model estimates of natural internal climate variability. The results mean that the combined CO2 + sulphate aerosol signals can be distinguished from model noise estimates with a high-level of statistical confidence. The latter conclusion is strongly dependent on the realism of internal noise estimates, both in terms of pattern and amplitude."

WG1, Chapter 8, Page 8.18, lines 34-35. "While some of the pattern-based studies discussed here have claimed detection of a significant climate change, no study to date has positively attributed all or part of that change to anthropogenic causes."

WG1, Chapter 8, Page 8.18, lines 43-44. "Any claims of positive detection and attribution of significant climate change are likely to remain controversial until uncertainties in the total natural variability of climate system are reduced."

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Derek also suggests the following, with which I disagree, but it does raise a point about how to present all of this and the need to make a strong case, So Derek's suggested addition:

I suggest adding the following sentences to line 8, page 3 of the WG1 SPM and also including them in the overall IPCC SPM and the Synthesis report: "However, this is a controversial conclusion as no study to date has positively attributed all or part of the change to anthropogenic causes. A major obstacle to human attribution is the large uncertainty on the natural variability of the climate system.