Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Preface

Reviewer 5

Page vii, line 2: I would recommend to remove the words "and/or advancing". Scientists with the aim to advance the frontier of knowledge will probably have to read the relevant scientific publications in peer reviewed journals rather than a report that is also targeted at an audience including decision makers and the general public. In my view this is a too far stretch.

Whole report/executive summary

Reviewer 1

General comments:

1. This is a very comprehensive report and (as far as I can tell in the time available) is largely of high quality. Its goal of communicating accurate, substantiated carbon cycle science to audiences in the public-policy, private, and general-public sectors is commendable.
2. The report needs to place the carbon cycle in an earth system context. The primary motive for policy (and much of the scientific) interest in the carbon cycle is anthropogenic climate change resulting from current carbon cycle imbalances, and the possibility of further earth system feedbacks or vulnerabilities that could accelerate the coupled climate and carbon cycle changes. From this perspective, two issues need more emphasis in the report (especially the executive summary).
   (1) The agent of greenhouse-driven climate change is radiative forcing, not CO_2. There are many contributors to present radiative forcing, broadly (a) CO_2, (b) non-CO_2 greenhouse gases, and (c) non-gaseous forcings (direct and indirect aerosol effects, albedo, etc). CO_2 forcing is presently around 55 to 60% of the total gaseous forcing (a + b), while (c) is currently negative (although highly uncertain) and comparable in magnitude with (b). Future climate change depends on the future evolution of all of (a), (b) and (c), not just (a) – even though (a) is the largest single driver of the system. An appropriate place to make this point would be near page ES-3 line 18. Reference to the need to account for the full radiative forcing implications of carbon management options (not just the effect on CO_2) would also be helpful on page ES-10 after line 4-7.
   (2) More emphasis needs to be given in the executive summary to carbon cycle vulnerability, meaning possible acceleration of imbalances in the carbon cycle by climate change itself, thereby accelerating the imbalances. Vulnerability of terrestrial and ocean carbon pools is especially important (see Gruber et al 2004, cited on p. 2-13). This point could be made on P. ES-10 near line 10.
3. The executive summary overstates the certainty of the North American carbon budget relative to the global carbon budget. This is clearest at page ES-4 lines 19-27, where the global terrestrial sink is stated to be "quite uncertain", but the North American sink is given to 3 significant figures. No source for this number is given. Also, no year is given – a

crucial omission since the terrestrial sink is the most variable term in the C budget from year to year, both globally and regionally. This is a dangerous oversimplification in two ways. First, the extreme interannual variability of the terrestrial sink must be stressed at this point in the executive summary. Second, the actual order of uncertainty is opposite to what is implied: all continental and regional C sink estimates from atmospheric inversion estimates are more uncertain than global sink estimates, because of mass balance constraints. Bottom-up estimates (from inventories etc) are also subject to large uncertainties, though they are much harder to quantify and are often not estimated (see Raupach et al. 2005, Global Change Biology 11, 378 for discussion of errors and their estimation).

4. On the terrestrial C sink, there are contradictions between the executive summary (page ES-4 lines 19-27) and the text (page 2-8 lines 18-27). The latter gives a (properly) uncertain estimate ascribed to particular years.

5. There are multiple units in use for the same thing, in particular $MtCO_2$ per year, MtC per year, and GtC per year for fluxes. I would advocate dropping $MtCO_2$ per year and expressing all fluxes (both global and regional) in MtC per year throughout, to facilitate regional-global comparisons. (The factor of 1/1000 to go to GtC per year can be easily applied mentally by those who need to do so).

6. The executive summary is not overtly biased. However, I believe that it does leave open the possibility of misinterpretation, mainly through omission. There are three main examples:
(1) *Temporal variability of the terrestrial sink, and risk implications:* Some parts of the executive summary (page ES-4 lines 19-27, page ES-7 lines 1-7) imply that the terrestrial sink is steady and reliable, whereas it is actually highly dynamic and can fluctuate year-to-year by up to half the fossil-fuel source. This makes terrestrial biological sequestration strategies highly risky, both for reasons of short-term and long-term stability (see point 2(2) on C cycle vulnerability) and also poses difficulties for carbon accounting.
(2) *The carbon-GDP connection:* The report is correct (page ES-5 lines 22-34) in pointing out that the carbon intensity of GDP is falling. However, the critical point is that emissions are still rising. The intensity (emissions/GDP) is falling only because the denominator is rising faster than the numerator. The trend to decreasing carbon intensity of GDP is not a greenhouse solution.
(3) *Treatment of the C sink in North America:* A policymaker could conclude from this executive summary that full carbon accounting is very much in (say) the US interest because the total US emission (fossil + terrestrial) is around a third less than the fossil emission alone. This would not be a scientifically justifiable inference, because most of the earth system sinks for carbon (both terrestrial and oceanic) are unmanaged and are the unintended result of legacy actions and global C cycle imbalances. Therefore, they are not "owned" by any particular nation in the same way that a nation "owns" direct anthropogenic forcing of the C cycle (either through fossil fuel emissions or managed sequestration activities).

Minor Comments

P. ES-4 line 1-6: All fluxes and percentages like these are functions of time, so it is necessary to give time stamps to all numbers.

P. ES-4 line 5-18: The compartments for the budget figures quoted are not clear. For example, is the building-sector (mainly electricity) emission (line 15) also included in the electricity emission figure (line 8)? Emissions are given in awkward units (see comment 5) and as a mix of

absolute and percentage values. This could be fixed with a simple table of North American and global C sources and sinks, with the anthropogenic emissions classified by source (coal, oil, …) and end use (industry, home, transport …). The table should also include uncertainties, especially in the terrestrial sink.
P. ES-4 line 19-27: see points 3 (uncertainty and temporal variability), 4 (contradictions), 5 (units) and 6(2) (temporal variability again).
P. ES-5 line 6: Need to mention (here or elsewhere) the risks for terrestrial biological sequestration imposed by uncontrollable sink variability. See point 6(2).
P. ES-5 line 19: multiple (and changing) units.
P. ES-6 line 20-23: Is building power a service? If so, the carbon intensity of that GDP component could rise, not fall.
P. ES-7 line 2-7: Need consistent units and (more importantly) error estimates.
P. ES-7 line 27: What about the methane emissions from wetlands? This is an example of where CO_2 accounting rather than radiative-forcing accounting can be misleading.
P. ES-8 line 22: This statement could be stronger: predicted ocean acidification will pretty much wipe out coral reefs by 2100.
P. ES-9 line 3-10: reference to the multiple sources of terrestrial C vulnerability (nutrient limitation, fire, insect attack, increased respiration with warming, …) would be appropriate here. The important point is not to give the impression that interaction of the C cycle with other earth system processes always leads to benign outcomes.
P. ES-9 line 10: "the interwoven systems of North America" is a little regionalistic. The systems are just as interwoven over the whole globe.
P. ES-9 line 12: Too wordy. Why both "options" and "measures"? What is the intended distinction?
P. ES-9 line 21: Where does the hydrogen energy come from? Hydrogen is an energy carrier, not a source, so the mitigation question hinges on how the hydrogen is generated.
P. ES-10 line 6-7: Another example of why the issue is radiative forcing, not just CO_2.
P. ES-10 line 19: The demand for policy relevant information is now very high, as shown by this report.
P. ES-11 line 4-7: I like this definition of requirements for science to make a policy contribution (credible, salient, legitimate).
P. ES-11 line 19: What departures from existing practice? This point cannot be understood without some indication.

Reviewer 2

General Comments

I have quickly, but not carefully, read the full report. Overall I am extremely impressed by the document and the quality analysis and science that underpin it. In general the chapters are very well written, have useful figures and the information is clearly and comprehensively presented.

Of course it is no trivial task to condense all this information into an Executive Summary. The present version of the Executive Summary makes an excellent start, and is both informative and interesting. However, it needs more work to ensure that the information is provided accurately but also in a non-confusing manner.

Specific Comments

Introductory section.
This section is nicely written and gives a clear, understandable background.

1) However a small presentational problem is introduced which has echoes elsewhere in the document. The emphasis on North American terrestrial processes is justifiable given the subject matter of the report, however the context of the global carbon cycle requires more explicit mention of ocean uptake. The problem first appears on line 25 where the "piling up" of CO2 in the atmosphere is mentioned, without corresponding mention of the ocean. The impression is given here that we have fossil-fuel emissions and clearing of forests being "far larger" than the ability of "various terrestrial and marine reservoirs" to store carbon. This may even be slightly misleading: historically the atmospheric storage term has been significantly smaller than the cumulative emissions, and the oceans have played a major role here. Why not just say this clearly? By not mentioning the well-documented "piling up" of CO2 in the ocean it is also difficult to understand the concern about ocean acidification that is mentioned subsequently in the document.
2) It would be useful to mention not only the contribution of North America to global emissions in 2003 (27%), but also what the historical contribution has been since 1780. Given the long lifetime of CO2 in the atmosphere, information on past emissions is also of relevance to policy- and decision-makers.

"What is the Carbon Cycle and why should we care"
Good questions! Well-answered!
Here I did not find the analogy with the hydrological cycle so useful. There are as many differences as similarities (e.g. changes of chemical form; very different processes, residence times; etc). I think the analogy actually risks confusing the reader.

"modern, post-industrial societes": are all "modern" societies really "post-industrial"?

Use "pools" or "reservoirs" but preferably not both.

ES-3, line 4: "carbon building up in the atmosphere AND IN THE OCEAN".

ES-3, lines 6-9: again, I think that the analogy with changes in the hydrological cycle looks a little contrived.

ES-3, line 11: suggest "10's of years" for decades and "millions of years" instead of thousands of millennia.

ES-3, line 15: mention of methane is potentially confusing: is it necessary here?

ES-4, line 24: it should have earlier been made clear that releasing CO2 to the atmosphere leads to increased CO2 in the ocean and that this "acidifies" the ocean. Personally, my feeling is that "potentially dire consequences" is still a little too strong given our state of knowledge in this

area. Suggest: “may have serious consequences”. I also think the climate change concerns are being underplayed a little: for human beings this surely is the number one concern/risk and reason for caring about the carbon cycle and carbon management: even in the face of scientific uncertainty.

Missing from the final paragraph in this section, is the important point (made elsewhere in the document) that any assessment of the effectiveness of deliberate carbon management policies REQUIRES an understanding of the enormous fluxes and potential imbalances in the natural carbon cycle. How else would we know if human actions (wrt emissions or mitigation) are making a difference to atmospheric CO2 levels?

How do North American carbon sources and sinks relate to the global carbon cycle?

Here I got a little confused by the variety of different numbers and comparisons presented. We have emissions in Mt and as percentages; we have North America and then the USA and then global. Maybe this can be simplified a little. My feeling is that percentages are generally more useful in an Executive Summary than absolute numbers, although the absolute numbers must also be presented somewhere as a reference….

This section of the text is almost crying out for “pie charts”. Are these allowed?

In general there was a lack of consistency in numbers of significant figures between the absolute numbers (3-4 significant digits) and the percentages (generally 1-2 significant digit). This is potentially misleading in terms of uncertainties and some more thought on presentation is required here. (e.g. “approximately 30% … of emissions are offset by a smaller sink of 2170 Mt…)

I found the discussion of buildings interesting/important but also a little confusing. Is it necessary to discuss buildings with AND without electricity for example?

Page ES-4, line 20: risks implying that coastal oceans are a substantial net sink. Is this true?

Page ES-4, line 24. Here we are suddenly introduced to the “global terrestrial sink”. To me this is a quite complicated concept. If the term is used in the Executive Summary, it should be defined and explained what it is.

Page ES-4, line 28. Here again the lack of consideration of the ocean sink risks misleading as far as the relation of North America to the GLOBAL carbon cycle is concerned. There probably can be little question that North America makes (or has made) a “dominant” contribution to global carbon SOURCES. It may be harder to argue for the “dominance” of North America for carbon SINKS, at least when viewed historically. Here it may again be useful to contrast the situation presently (e.g. 2003) with the “cumulative” situation since 1780. Given that this comparison is the *raison d’etre* for this entire section, there may need to be more clarity with wording here.

What are the primary carbon sources and sinks…..

I found this section very informative. However again, there were perhaps a few too many different comparisons presented that risk confusing the reader. Also: issues of relative vs. absolute numbers and their precision, come up again.

Page ES-5, line 17: the first part of the section discusses "sources" so it was a little surprising to find the 2nd sentence stating that sources are 3x larger than sinks. This comparison should surely come AFTER the sinks have been discussed/explained.

Page ES-5, line 19: I had only just got used to dealing with emissions in Mt CO2 and now suddenly we have Mt C. While I prefer the latter, it is really only important to be consistent and use the same units throughout.

Page ES-5, line 24: "carbon intensity" is an important concept and should be defined/explained more clearly.

Page ES-5, line 31-32. The decoupling of emissions from economic growth is an extremely important point of course. Therefore it deserves more discussion and clarification: I assume this is both a "recent" phenomenon in the USA and is not the case everywhere in the world.

Page ES-6, line 15-28. again the discussion of buildings was interesting, but maybe a little too detailed for my taste. On the other hand, if the argument is that energy conservation in buildings is a really big issue: then that case seems to be very well made and may justify the detail given. (I was surprised that ONLY 67% of electricity was consumed in buildings.)

Page ES-7, line 2: again note the difference between exact and approximate numbers in the same sentence.

Page ES-7, line 6. How is the "coastal ocean" defined? It would likely make sense to consider the EEZ. Or is it the continental shelf? The coastal ocean source is of course largely a "natural" source of carbon that has not been greatly altered by mankind as far as we know.

Page ES-7, line 8. If it is true that forest regrowth basically takes up CO2 that was previously emitted due to deforestation, then this point should perhaps be mentioned explictly. Arguably, this makes this particular component of the terrestrial sink a "net sink", in relation to human interference in the carbon cycle, only the sense of "instantaneous" fluxes rather than cumulative emissions. Similarly, I wonder about the peat accumulation sink: should this be compared to fossil-fuel emissions? Effects of fire suppression may be qualitatively different.

I may be wrong on this and I may be risking confusing matters further. But perhaps this sort of conceptual discussion actually belongs in an Executive Summary about carbon sources and sinks written by scientists for policy-makers. Policy-makers may be confronted by such arguments by their international counterparts, after all. Presumably this issue has been discussed extensively by the authors, and I admit to not having examined the respective chapters to see whether this has, in fact, been presented/discussed in detail.

What are the direct, non-climatic effects of increasing atmospheric CO2…

Page ES-8, line 19. If the acidification impact is mentioned at all, then it requires slightly more explanation. For example it is the increasing levels of carbon in the ocean rather than in the atmosphere that causes acidification: with the present text, an uninformed reader might infer that changes to the pH of rainfall are responsible!! It is also unclear why we would worry about acidification in the oceans more than the effect on lakes and rivers….

What are the options….?

Reviewer 5

Page ES-10, line 6: if CH4 and N2O are mentioned it should be mentioned why increased emissions may be of concern.

Page ES-3, line 14: Somewhere before it should be mentioned that also methane and other carbon compounds also follow a cycle, and that they are also emitted by processes associated with fossil fuel use.

Page ES-7, line 17: All above mentioned sinks are also uncertain, I would recommend stating uncertainties with the numbers, or a range, or at least make a general statement on uncertainties.

Page ES-9, line 21: Hydrogen is not a primary source of energy. It should be stated how the hydrogen will be generated, given that nuclear, solar, wind, but also fossil fuels are all options.

Reviewer 6

Are uncertainties or incompleteness in the evidence explicitly recognized ?
NO:
It is in fact one of my major criticisms of the report that in the Executive Summary (Pages ES-4 to ES-8 line 12) as well as in Chapter 3 (except for Table 3-1 and page 3-7, line 19-22) no uncertainty ranges of the sources and sinks fluxes of carbon in North America are given. For example, the estimated uncertainty of fossil fuel CO_2 emissions is about 10% (with 95% confidence, see Table 3-1) but up to four significant digits of the cited numbers are given. This deficiency is even more obvious when it comes to the sinks which in most cases are uncertain to within 50-100%. This is very misleading as it gives the impression to the reader that the fluxes reported would be known to very high precision, but in fact the contrary is the case. The digits in the reported numbers need to be reduced to the significant ones (i.e. ≤ 2) and errors need to be reported, also in the Executive Summary.

There is a lot of repetition in the Executive Summary between the sections "How do North American carbon sources and sinks relate to the global carbon cycle" and the following section which is dealing only with North American carbon sources and sinks. The Executive Summary could be well shortened if the "relation to the global carbon cycle" would be imbedded in the latter section.

ES-3, line 14-17: "… and other carbon compounds in the earth's atmosphere, such as methane, are increasing." The context of this finding is not immediately clear from what is said before.

Reviewer 8

The Executive Summary, as well as many of the chapters, is in need of professional editing. It should be reviewed both for technical accuracy and for correct grammar. It should also be edited to reduce the redundancy with Chapter 1. Some examples illustrating the need for editing are listed below. The list is illustrative only and far from exhaustive.

Technical:

Page 4-5. Line 4 on page 4 gives the contribution of the United States to North American emissions due to fossil fuel combustion as 86%. Page 5 gives the contribution as 85%.

Page 8. The U.S. agricultural soils sink is given as 6 Mt C per year. This is out of date. The current estimate is 12 Mt C per year.

Page 9. The list of options is missing a number of potentially significant ones, e.g.,
Transportation: non-liquid biofuels and electric cars; in
Buildings: use of renewable energy;
Industry: carbon capture and sequestration.

Grammatical/Technical:

Page 7 states "The suppression of forest fires also increases net carbon storage in forest biomass." Suppression of forest fires presumably reduces emissions, but only increases carbon storage in relation to some projected losses.

Grammatical/Terms:

Page 3. Line 18. "forcing agent". Term unfamiliar to lay audience.
Page 5. Line 17 and repeated throughout Executive Summary. "fossil fuel source". Term unfamiliar to lay audience.
Page 6. Line 24. "…the number of households per unit population…" This turn of phrase is odd as the normal "unit" of population is one person.
Page 7. "dead organic carbon" This phrase strikes the average reader as odd.

Grammer:

Page 3. Line 15. "This facts…"
Page 6. Line 16-17. "The trend in the buildings sector over the last decade has been towards growth. Very poor construction.

Reviewer 13

1. The scope and intent of the ES is generally clear, appropriate and balanced. In places, however, the language assumes a medium to high degree of familiarity with the subject. For example, without explaining the term (ES-3, l. 18), the text states: "….is the largest single

forcing agent of climate change." Perhaps more effort could be put towards using less specialized language throughout the ES.

2. The readability of the document as a whole could be improved by a consistent use of carbon *or* CO_2, not both. For example, p. ES-7, l. 2 states, a natural sink of "592 Mt C" in contrast to p. ES-4, l. 21 "a smaller sink of 2170 Mt CO_2." Other examples occur in the various chapters as well, particularly when referring to the costs of carbon mitigation.

3. Additional editorial work should be done to insure consistent use of the terms "effects," "impacts," and other similar phrases. For example, in the Preface (p. v, footnote 1) in reference to carbon cycle changes and impacts the term "impacts" is defined to mean "effects of changes in the carbon cycle." In the ES, p. ES-8, l. 2 and l. 5, we see the phrases, "changes in the environment" and "effects of climate". It is unclear if the intent is climate "impacts" as defined in the Preface or whether a second meaning is implied.

4. P. ES-1-ES-2. The change in world carbon emissions attributable to North America noted between 2002 and 2003 (32% to 27%) seems large. Are these figures correct and based on the same underlying data sources?

5. P. ES-5 – ES-6. The statement, "This implies that emissions growth is essentially decoupled from economic growth," is contradicted later by "[c]hiefly as a result of economic growth, energy use by North American transportation is expected to increase by 46% from 2003 to 2025."

6. P. ES-9, l. 21 and l. 22. The text notes the use of "hydrogen energy." This is a mischaracterization of the technology. Hydrogen fuel (as correctly discussed in Chapter 7. Transportation) is not a source of energy but is an energy carrying medium generated from fossil, renewable or nuclear fuels. I suggest changing the phrases, "hydrogen energy" to "hydrogen fuel cells" or "hydrogen."

Reviewer 24

To ensure a balanced view of the US carbon cycle, the Executive summary needs to state historical number of integrated emissions as well as annual fluxes.

page ES-3, line 20-22. This text seems ambiguous. It is certain that the surface ocean becomes more acidic under elevated CO2, and this effect is not influenced by climate change.

p. ES-3, lines 25-31, it would be useful to highlight that both short-term and long-term solutions are helpful. The short-term solutions help to gain time and the long-term solutions to find real solutions.

Reviewer 27

I have examined the report through the 3d chapter carefully and quickly looked at the rest. I am especially charged to look at Chapter 2. My impression is that overall it is a well done and important publication. It is fair, impartial, and devoid of special pleading. It is likely nearly as complete and balanced as possible given the limited human and other resources available for its

completion. I think it deserves a lot of praise which I am trying to convey before expressing what I think are some serious deficiencies at various levels.

The most general issue I would like to raise is that I do not think that alone, the report provides a very good scientific and technical basis for what I think is a major objective and expressed on p 3: "management of the carbon cycle and concentrations of carbon dioxide in the atmosphere". These comments are primarily directed at the natural science aspects of the report. There is some thoughtful discussion in Chapter 5 that there has not been much focus yet in turning carbon science into information useful for decision makers.

What the report mostly provides for the natural science system is a general sense of variety of pieces of the system that need to be considered and what numbers are current best estimates of the contributions of these pieces to stores or fluxes. Synthesis is largely just adding up these numbers. However, the system has many time scales and is varying from year to year with many strong couplings to various aspects of weather, climate, and other aspects of the environment. Forest inventories done every decade or so may provide an important overall constraint but I do not think that somebody forced to make billion dollar economic decisions about future infrastructures connected to energy systems will be very happy having to work with that level of information. I think, at least in principle, we already have much more advanced information technology systems than any sense of is given of in this report. I will try to explain my point by analogy. Suppose a report was to be done about the Nations ability to manage hurricanes. I do not think that a report only giving historical descriptions of past hurricanes, even if including details of their locations, wind strengths, and damage done, would provide what was needed. Rather, I believe that somebody would want to know about how we are going to know about new hurricanes and where they might go. For that, a lot of observational systems, from aircraft to satellites, and various modeling systems for prediction of hurricanes would have to be reviewed. As another, perhaps closer example, much of the emphasis in the IPCC WGI report is about modeling projections of future climate and in doing so it highlights the advancement of the needed modeling tools and promotes international programs for improvement of these models through WCRP. Observational systems are addressed in IPCC primarily in the context of the elaborate statistical analyses used for "attribution" of climate change; I would prefer that they gave more emphasis on how we now get and will get the numbers we need in the face of serious decay of various global observational systems.

Our ability to better manage the carbon cycle will depend on both its understanding -down to the level of decision makers- and our ability to develop better technologies to quantify what it is doing in a lot more time space and process detail than the kind of numbers given in this report. What numbers do we need? What are their current accuracies, and what accuracy will we need? How can we synthesize these numbers within a modeling framework to describe the dynamic evolution of the carbon system? I have spent quite a few hours over the last two decades working toward such a system view under various rubrics, e.g. , "Mission to Planet Earth" . Any such view includes the application of a lot of measurement systems, satellite or otherwise. How well do these now work, what can they measure, and what else is still needed? It seems to me that the report is dominated by a "small exploratory science" view of the world; such is a great starting point but by itself cannot in the end provide information of the quality that a decision maker might find useful for making year by year decisions about carbon management. Financial

management decisions are typically made on the time scale of a few months and I would expect that appropriate flows of information on this time scale would eventually be needed.

In sum, if I had planned the report, I would have wanted much more to emphasize assessment of our measurement and modeling capabilities on continental to global scale as needed to address the issue of management of carbon. I find these aspects of the science and technology almost totally missing here.

Other aspects of the report in general.
a) I have already outlined my largest problem with the report – that it does not address what are some of the most important questions; however, it does largely address the questions raised in the preface so I can be largely affirmative about all the general evaluatory questions in the review instructions. While the report mentions uncertainties, I do not think it even provides a framework where they might usefully be quantified. Nor did I find any direct discussion of how NACP –supported research will reduce uncertainties. I am not even convinced the right questions are being asked. The information provided should be very helpful to policy makers in terms of improving their general scientific understanding but otherwise it is not clear to me how the information provided would be used quantitatively for policy.
b) ES-3: line 6 and also on p 1-2: "If vast quantities of water had been trapped …" This is scientific nonsense. If such as happened, they would have ended up in the oceans with a consequent rising of sea level, not in the atmosphere as clouds. This is not a good example.
c) ES-3 line 9 "…Mexico, …the country most dependent on road transport" What was this supposed to say? We are all happy to walk in the US?
d) I found the quantification of US to carbon budget in terms of sectors on ES-6 difficult to follow – it would have so nice to have it all in a good graphic-likewise for ES-7.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
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Chapter 1

Reviewer 3 (reviewed all of Part I and executive summary)

General Impressions:

The preparation of this report is a challenging undertaking given its intended audience (scientists, decision-makers in the public and private sectors, the international community, and the general public) and the wide range of technical issues that have to be addressed. In most respects, the sections I've examined appear to be reasonably well written with respect to use of the Queen's English and thus I have not focused on editorial concerns (e.g., obvious typos, etc., which are few) unless these significantly affected clarity or accuracy.

However, at this stage in its development, the report seems more targeted to a technically oriented audience than to non-scientist decision-makers or the general public. To reach the latter, I think there needs to be a much more careful elaboration of technical material in the text, as well as in table and figure legends.[1] Tables and figures need to "stand alone" even for a technical audience but merit considerable explanation when communicating with a less technically trained audience.

It also appears that the authors of specific chapters or sections have not had sufficient time to read other contributions, as evidenced by unnecessary repetition and some inconsistency in technical information conveyed from one section to the next. As one example, there appears to be considerable variation in the treatment of uncertainties in point estimates of carbon sources, fluxes, and sinks among the various sections of the report, and rather questionable handling of estimates and their uncertainties in most sections. Among the most glaring is the citation of the estimated carbon sink for North America (in Gt of C) to three significant figures, when the estimated error is on the order of ±50%. However, my concern also extends to some aspects of the better constrained estimates of fossil fuel emissions, which are sometimes given to four significant figures. I'll detail these concerns in my specific comments.

The title of Chapter 1, on which I was asked to focus my review, indicates that it is to provide information on the "purpose, scope, and structure" of the SOCCR. I do not think that it achieves this objective in its current state. I found little information on the scope of the report as a whole and nothing at all on its structure.

I kept looking for answers to questions that I thought should be addressed in a report of this type. What do we know and how well do we know it? "How well" in this context goes well beyond quantitative estimates of uncertainties in sources and sinks: How closely does current information meet the expected needs for carbon-climate modeling and analysis of carbon management options? Other questions include: What don't we know and why? What are the most important uncertainties and why? What will it take to reduce uncertainties to manageable levels and how long do we expect it might take? Where is R&D needed to provide the tools that could lead to improved understanding? What can't we know and what are the implications, e.g., where are uncertainties only poorly reducible (or perhaps irreducible) and how do we plan to deal with them?

I found very few answers to my questions in the current version of the report—and nothing at all in Chapter 5, despite its title. In fact, I get the impression that tackling these tough but critical questions is being deferred in pursuit of a yet to be established (and perhaps elusive) process (see fourth paragraph on page 5-9). Several other synthesis and assessment reports (namely SAP 5.1, 5.2, and 5.3), organized under the heading "Explore the uses and identify the limits of evolving knowledge to manage risks and

[1] The authors may want to consider the way some figure and table legends are handled in *American Scientist*; see, e.g., article by R. Seager, **94**: 335−341, July-August 2006.

opportunities related to climate variability and change," were identified in Chapter 5, but, based on the descriptions of these activities given on the CCSP web site, they will not address my questions either.

Based on the material I've looked at, I think the report could benefit from appointment of an overall editor(s) whose focus would be on consistency in presentation of technical information and facilitation of communication with the broad audience for which the report is apparently intended. Toward that end, I would also recommend that every effort be made to use a single unit of measure and carbon reference (e.g., either Gt or Pg of C) throughout the report. Mixing units of Pg, Gt, and Mt, and jumping between values referenced either to C or CO_2 equivalents is confusing to the reader.

Specific Comments:

Page 1-1, lines 18 and 19: Since all life on earth is carbon-based, why not say "pools of carbon on and near the earth's surface (mainly in plants and soils), in the atmosphere, and in water and sediments in the ocean"? That way you also include by inference freshwater systems and geological sediments mentioned in the next paragraph and included in Fig. 1-1.

Page 1-1, line 21: Should say "food, shelter, and energy." Biomass energy is already critical to the survival of much of humankind.

Page 1-1, lines 24−26, and Fig. 1-1 on page 1-9: The figure is too complex for a general audience without additional explanation. Incorporate into a text box to accomplish this? In addition, the sizes of the reservoirs/pools and exchanges do not match those in Fig. 2-1. Which year(s) does this set of values apply to?

Page 1-1, line 30: Add "**—**and back again" at the end of the sentence to match what is shown in the figure and reinforce the idea that these exchanges are typically a "two-way street"?

Page 1-2, line 1: I think "that transfer" should be "those transfers."

Page 1-2, lines 3, 4, and 5: The word "imbalance" is used four times in lines 4 and 5. How about replacing the words following "whether the budget" in line 3 with "is balanced, and if it is unbalanced can provide insights about why such a condition exists and how it might be managed." The words "in imbalance" in line 5 could simply be replaced by "unbalanced."

Page 1-2, line 6: Since tropical deforestation is a source of carbon to the atmosphere, would it not be more accurate to say that use of fossil fuels is "primarily" responsible?

Page 1-2, line 17: Would it be advisable to add the words "and continue to do so in tropical regions" at the end of the sentence?

Page 1-3, first paragraph, last sentence: This sentence begs the question: Why? A brief explanation should be added.

Page 1-3, line 18: Don't we care about the unbalanced state of the entire carbon cycle, of which the atmospheric component is only one aspect?

Page 1-3, lines 21 and 22: My favorite word again.

Page 1-3, lines 30 and 31: "Acidity" is simpler than "acidification" and seems to work just as well in this context; see earlier comment on Executive Summary.

Page 1-4, second paragraph: The estimates of the North American sink and its potential significance relative to the global sink do not match those given in the second paragraph on page ES-4 and the estimates of the global terrestrial sink in this paragraph don't match the estimate in Fig. 1-1.

Page 1-4, line 17: Insert the word "located" before the word "primarily"?

Page 1-4, third paragraph, last two sentences: Which processes and mechanisms are considered most significant? Give examples?

Page 1-4, last paragraph, last sentence, which continues at top of page 1-5: This sentence provides one important answer to one of questions I identified in my general comments. I think much more effort is needed to address such questions in this chapter and in the report as a whole.

Page 1-5, first full paragraph: One important question that was not comprehensively addressed in this section of the report was how well we think we need to understand the North American carbon budget to achieve our goals for carbon cycle modeling or carbon management. The issue of the spatial resolution needed to address key questions was not touched upon at all, for example, but is a critical one for some uses. Is this issue addressed somewhere else in this report?

Page 1-5, final section of Chapter 1 as a whole: The length of this is disproportionately long in relations to other sections of Chapter 1, and in view of the absence of substantive material on the scope and structure of the report, as promised by the title of the chapter. I think the section could be reduced significantly without loss of meaning.

Page 1-5, line 25, and page 1-6, line 1: For reasons given in my comments on text from page ES-11, lines 5, 12, and 19, I think the term "saliency" is another example of unnecessarily complex wording, and its usage by Cash et al. is in marked contrast to its dictionary definition. "Relevancy" makes more sense to me.

Page 1-5, last sentence: Credibility thus depends on effective, honest communication of uncertainties in data, parameters, and conclusions, e.g., in estimates of sources and sinks.

Page 1-6, fourth paragraph: The objectives given in this paragraph will not be accomplished unless much greater effort is made in this report to communicate more effectively with a more general audience.

Page 1-6, last paragraph: I expected that either Chapter 1 or the Executive Summary would have provided a roadmap to the report that would have pointed me to the Chapters and sections where the first and third of the three critical areas identified in this one-sentence paragraph were addressed, including information on the status of answers to the key questions they imply. Where are we on the road to providing substantive information to the address the areas identified in this paragraph? For example, how "mature" is our information with respect to understanding individual parts of the North American carbon cycle? How long do we think it will take to fill in critical data gaps? Is technology development a limiting factor? If so, for what components? How will we know when we have achieved the implied goals? Etc.

Reviewer 34

In general, the first part of the title (What is the carbon cycle) isadequately covered and can be understood by the general reader.

The second part of the title (why do we care?) is nearly non-existent. Taking into account that the entire rest of the report is still to follow, one might have expected an overview of the impacts and policy

dimensions related carbon imbalances, the opportunity costs of delayed interventions, the role of the public and private sectors, etc.

The section, Carbon cycle science in support of carbon management decisions,is relevant but unrelated to the chapter title. Here one would expect to find significant coverage of international initiatives related to the carbon cycle, taking into account the priorities, progress and work of the Igbp (to which Usa scientists have made significant contributions) and the Global observing systems - Gcos (Climate), Goos (Oceans), and Gtos (Terrestrial). It would seem appropriate for this section to place the "North American" initiative into the global context in which carbon science and policymaking is occuring.

Reviewer 26

This is a much needed chapter as the assessment will be speaking to very diverse group of stakeholders.

“Why the carbon budget of North America?”
In justifying the reasons why we should care about the carbon cycle I miss a clear statement on what I think are the most important reasons (they are embedded in various sentences but not clearly spelled out):

- The terrestrial sink (in NA or globally) is a service provided by terrestrial ecosystems worth billions of dollars if we had to pay for the equivalent amount through carbon sequestration or emission reductions. Consequently, we need to understand its dynamics and processes.
- Vulnerabilities of the carbon cycle into the future (eg, carbon-climate feedbacks) may change the strength of terrestrial sinks and put further pressure on carbon mitigation and emission reductions to achieve agreed stabilization targets. Thus, we wan to make sure we understand future trajectories of terrestrial sinks/sources and have them appropriately considered when designing CO2 stabilization pathways.

“Carbon cycle science in support of carbon management decisions”
I think the intent of this section is important but as it stands now, it largely reports on the “theory” of having an assessment like this one to be owned and recognized by key stakeholders. Instead, I would propose to tell the reader the different steps the managing team of this assessment have taken to ensure credibility, buy, etc. (eg, stakeholder consultation to ask what they need from the assessment).

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 2

Reviewer 3 (reviewed all of Part I and executive summary)

General Impressions:

The preparation of this report is a challenging undertaking given its intended audience (scientists, decision-makers in the public and private sectors, the international community, and the general public) and the wide range of technical issues that have to be addressed. In most respects, the sections I've examined appear to be reasonably well written with respect to use of the Queen's English and thus I have not focused on editorial concerns (e.g., obvious typos, etc., which are few) unless these significantly affected clarity or accuracy.

However, at this stage in its development, the report seems more targeted to a technically oriented audience than to non-scientist decision-makers or the general public. To reach the latter, I think there needs to be a much more careful elaboration of technical material in the text, as well as in table and figure legends.[2] Tables and figures need to "stand alone" even for a technical audience but merit considerable explanation when communicating with a less technically trained audience.

It also appears that the authors of specific chapters or sections have not had sufficient time to read other contributions, as evidenced by unnecessary repetition and some inconsistency in technical information conveyed from one section to the next. As one example, there appears to be considerable variation in the treatment of uncertainties in point estimates of carbon sources, fluxes, and sinks among the various sections of the report, and rather questionable handling of estimates and their uncertainties in most sections. Among the most glaring is the citation of the estimated carbon sink for North America (in Gt of C) to three significant figures, when the estimated error is on the order of ±50%. However, my concern also extends to some aspects of the better constrained estimates of fossil fuel emissions, which are sometimes given to four significant figures. I'll detail these concerns in my specific comments.

The title of Chapter 1, on which I was asked to focus my review, indicates that it is to provide information on the "purpose, scope, and structure" of the SOCCR. I do not think that it achieves this objective in its current state. I found little information on the scope of the report as a whole and nothing at all on its structure.

I kept looking for answers to questions that I thought should be addressed in a report of this type. What do we know and how well do we know it? "How well" in this context goes well beyond quantitative estimates of uncertainties in sources and sinks: How closely does current information meet the expected needs for carbon-climate modeling and analysis of carbon management options? Other questions include: What don't we know and why? What are the most important uncertainties and why? What will it take to reduce uncertainties to manageable levels and how long do we expect it might take? Where is R&D needed to provide the tools that could lead to improved understanding? What can't we know and what are the implications, e.g., where are uncertainties only poorly reducible (or perhaps irreducible) and how do we plan to deal with them?

I found very few answers to my questions in the current version of the report—and nothing at all in Chapter 5, despite its title. In fact, I get the impression that tackling these tough but critical questions is being deferred in pursuit of a yet to be established (and perhaps elusive) process (see fourth paragraph on page 5-9). Several other synthesis and assessment reports (namely SAP 5.1, 5.2, and 5.3), organized under the heading "Explore the uses and identify the limits of evolving knowledge to manage risks and

[2] The authors may want to consider the way some figure and table legends are handled in *American Scientist*; see, e.g., article by R. Seager, **94**: 335−341, July-August 2006.

opportunities related to climate variability and change," were identified in Chapter 5, but, based on the descriptions of these activities given on the CCSP web site, they will not address my questions either.

Based on the material I've looked at, I think the report could benefit from appointment of an overall editor(s) whose focus would be on consistency in presentation of technical information and facilitation of communication with the broad audience for which the report is apparently intended. Toward that end, I would also recommend that every effort be made to use a single unit of measure and carbon reference (e.g., either Gt or Pg of C) throughout the report. Mixing units of Pg, Gt, and Mt, and jumping between values referenced either to C or CO_2 equivalents is confusing to the reader.

Specific Comments:

Page 2-2, lines 12, 15, and 16: 48% of the total amount of carbon released to the atmosphere from fossil fuel burning (300 ± 30 Gt, from the first paragraph on page 2-4) and forest clearing (160 ± 160 Gt, also from the first paragraph on page 2-4), or 220 Gt C by my calculation, is said to still reside in the atmosphere, in agreement with the missing carbon estimate of 240 Gt given on line 15. However, these values do not agree the missing carbon estimate of 218 Gt obtained from data given in the first paragraph on page 2-7 (sum of 118 Gt taken up by the oceans plus 100 Gt stored on the land) or with either of the other estimates of the remainder of the human contribution to the atmosphere: 180 ± 5 Gt C given on line 34 of page 2-6 and 161 Gt C (given as Pg C) in Fig. 2-1.[3]

In addition, if the uncertainty in the inputs to the atmosphere from fossil-fuel use and forest clearing amount to 460 ± 160−190 Gt C, the uncertainty in the amount of the release remaining in the atmosphere cannot be 5% of the estimated release as stated in line 12 on this page.

Page 2-2, line 20, and Fig. 2-1 on page 2-20: The figure is too complex for a general audience without an extensive amount of additional explanation and the caption is obviously inadequate even for a technical audience. Incorporate into a text box to deal with this? In addition, the sizes of the reservoirs/pools and exchanges do not match those in Fig. 1-1. Which year(s) does this set of values represent? What is the reference for this figure?

Page 2-3, line 13: Because respiration and fires are combined in the flux back to the atmosphere in Fig. 2-1, I think this sentence could be revised as follows: replace text after "reproduction," in line 12 with "in combination with wildfires return a slightly smaller amount to the atmosphere, with the difference stored as plant biomass and soil organic carbon."

Page 2-4, line 3: Per Fig. 1 in the overview to Part II of the report and the text in Chapter 1, the industrial revolution *began* in the 18th century and *expanded* in the 19th century, accelerating the releases from fossil fuels.

Page 2-4, lines 6 and 7: How can references published in 1984 and 1999 give estimates of atmospheric releases through the year 2004?

Page 2-4, line 11: How can we say we know the concentration of atmospheric CO_2 in 1850 to three significant figures? What is the reference for this value and what is its estimated uncertainty?

Page 2-4, line 13: I think you need to either drop the third significant figure in the estimate given or add a second significant figure to the error term.

[3] Oceanic share of missing carbon appears to be 110 Gt C per Fig. 2-1 (as opposed to 118 Gt C given in text on page 2-2) but the terrestrial component is not decipherable without a more descriptive figure legend.

Page 2-4, line 20: I recommend deleting the parenthetical expression because the term described is never used elsewhere in text, tables, or figures.

Page 2-4, paragraphs 2, 3, and 4 (continuing on page 2-5) and Fig. 2-3 on page 2-21: Since this same material is covered in more depth in Chapter 3 and the figure is repeated as Fig. 3-2, why not delete Fig. 2-3, keep only the most important parts of the text, and reference Chapter 3 for the details?

Page 2-6, line 2: How can ^{14}C be considered a passive tracer?

Page 2-6, line 33: The error in the estimated release cannot be ± 100 Gt C if the errors on the two components (±30 Gt C and ±160 Gt C, respectively) given in the first paragraph on page 2-4 are correct.

Page 2-6, line 34, and page 2-6, lines 1 and 2: See earlier comments on page 2-2, lines 12, 15, and 16.

Page 2-11, last paragraph: Although I liked the tone of this paragraph, I found myself asking whether it was needed here since it deals with matters covered in more detail in Chapter 4. Another option might be to move it to the Executive Summary.

Page 2-19, Table [2-]1: Why are there no error estimates for the values given in the table?

Page 2-21, Fig. 2-2: The figure caption or a label on the lower panel should indicate that the data in the lower panel represent annual averages.

Page 2-23, Fig. 2-4: The figure caption needs more explanation to be more comprehensible to some members of a general audience. I think it should say explicitly that negative values indicate regions that are CO_2 sinks (as in Table 3-1 and Figure 15-3). In addition, the figure legends do not indicate the units of measure (Gt C per year?).

Page 2-24, Fig. 2-5: The patterns shown in panel (a) of the figure suggest that North America, rather than representing a net sink for CO_2, was neutral on average with respect to exchanges with the atmosphere during the full period from 1988 to about 2003. The data in the figure are compatible with the idea expressed on page 2-7 in the text that North America represented a net sink during the 1990s, but the pattern after 1995 indicates that North America was, on balance, a very strong source of CO_2. In order to avoid confusion by the reader—and potential criticism from some corners about the interpretation of these data—would it not be advisable to discuss the implications of the patterns represented by the data *in toto*. (perhaps in a text box accompanying the figure), rather than focusing solely on the 1990s (as in the text on page 2-7)? Another option might be to provide a brief summary of the causes and implications of the variations in this chapter with a reference to more detailed discussion in another chapter in the report.

Also, the figure caption probably should indicate that data for ocean basins are represented by "heavy" dashed lines and it should say explicitly that negative values indicate when the oceans/continents are CO_2 sinks (as in Table 3-1 and Figure 15-3).

Reviewer 4

Page 2-2, Line 28: "Future increases in carbon uptake in this portion of the carbon cycle could …."

Page, 2-3, Line 10: Why specify such a large range (100-200) for the land exchange? This is particularly noticeable in relation to the rather specific amount noted for the oceans (92 +/- 5). Though less certain, isn't there a more specific, citeable amount for annual gross exchange?

Page 2-3, Line 20: the transfer to the oceans is by rivers? Perhaps write, "….to the oceans by rivers and released from …."

Page 2-4, line 22 and 23, page 2.5, line 1: Perhaps the use of "rich" could be misunderstood? It may be better to use "industrialized" countries? That may be the point here – that among the industrial countries there are varying amounts of efficiency in generating wealth.

Page 2-5, line 22: perhaps, "…..spatial scaling presents formidable challenges due to heterogeneity of the landscape" this provides a bit more explanation to the reader as to the limitation of the eddy flux method in this context.

Page 2-6, line 14: The inverse method relies on both the space and time patterns of CO_2 concentrations rather than just space (though for the calculation of the long-term means, it is primarily using space patterns). Perhaps use "spatiotemporal pattern" in this sentence?

Page 2-6, lines 15,16: It may be important to mention that the flask observing network has stations that go back to roughly 1980 (calibrated) and that many of the 100 mentioned were added in the last decade. This has meant that the calculation of long-term means and inverse estimated flux time series use far less than the 100 currently available stations.

Page 2-6, line 21/22: the sources of uncertainty for the inverse method could be listed a bit better perhaps. "Limitations in the accuracy of atmospheric inversions come from the limited density of concentration measurements, especially in the tropics, the uncertainty of observations, transport uncertainty, mismatches between the resolution of observations versus simulations, and varying a priori assumptions in the inverse process."

Page 2-7, line 2: This leaves 100 Gt? My subtraction suggests 160 Gt C.

Reviewer 27

I have examined the report through the 3d chapter carefully and quickly looked at the rest. I am especially charged to look at Chapter 2. My impression is that overall it is a well done and important publication. It is fair, impartial, and devoid of special pleading. It is likely nearly as complete and balanced as possible given the limited human and other resources available for its completion. I think it deserves a lot of praise which I am trying to convey before expressing what I think are some serious deficiencies at various levels.

A. The most general issue I would like to raise is that I do not think that alone, the report provides a very good scientific and technical basis for what I think is a major objective and expressed on p 3: "management of the carbon cycle and concentrations of carbon dioxide in the atmosphere". These comments are primarily directed at the natural science aspects of the report. There is some thoughtful discussion in Chapter 5 that there has not been much focus yet in turning carbon science into information useful for decision makers.

What the report mostly provides for the natural science system is a general sense of variety of pieces of the system that need to be considered and what numbers are current best estimates of

the contributions of these pieces to stores or fluxes. Synthesis is largely just adding up these numbers. However, the system has many time scales and is varying from year to year with many strong couplings to various aspects of weather, climate, and other aspects of the environment. Forest inventories done every decade or so may provide an important overall constraint but I do not think that somebody forced to make billion dollar economic decisions about future infrastructures connected to energy systems will be very happy having to work with that level of information. I think, at least in principle, we already have much more advanced information technology systems than any sense of is given of in this report. I will try to explain my point by analogy. Suppose a report was to be done about the Nations ability to manage hurricanes. I do not think that a report only giving historical descriptions of past hurricanes, even if including details of their locations, wind strengths, and damage done, would provide what was needed. Rather, I believe that somebody would want to know about how we are going to know about new hurricanes and where they might go. For that, a lot of observational systems, from aircraft to satellites, and various modeling systems for prediction of hurricanes would have to be reviewed. As another, perhaps closer example, much of the emphasis in the IPCC WGI report is about modeling projections of future climate and in doing so it highlights the advancement of the needed modeling tools and promotes international programs for improvement of these models through WCRP. Observational systems are addressed in IPCC primarily in the context of the elaborate statistical analyses used for "attribution" of climate change; I would prefer that they gave more emphasis on how we now get and will get the numbers we need in the face of serious decay of various global observational systems.

Our ability to better manage the carbon cycle will depend on both its understanding -down to the level of decision makers- and our ability to develop better technologies to quantify what it is doing in a lot more time space and process detail than the kind of numbers given in this report. What numbers do we need? What are their current accuracies, and what accuracy will we need? How can we synthesize these numbers within a modeling framework to describe the dynamic evolution of the carbon system? I have spent quite a few hours over the last two decades working toward such a system view under various rubrics, e.g. , "Mission to Planet Earth" . Any such view includes the application of a lot of measurement systems, satellite or otherwise. How well do these now work, what can they measure, and what else is still needed? It seems to me that the report is dominated by a "small exploratory science" view of the world; such is a great starting point but by itself cannot in the end provide information of the quality that a decision maker might find useful for making year by year decisions about carbon management. Financial management decisions are typically made on the time scale of a few months and I would expect that appropriate flows of information on this time scale would eventually be needed.

In sum, if I had planned the report, I would have wanted much more to emphasize assessment of our measurement and modeling capabilities on continental to global scale as needed to address the issue of management of carbon. I find these aspects of the science and technology almost totally missing here.

D. Chapter 2. Overall, gives an impression of hurried writing and not careful editing. For example, the 4th bullet which should be presenting some major conclusion says: Global carbon emissions have increased for the last 30 years. In comparison North American carbon dioxide

emissions have increased at an average rate of approximately 1% per year for the last 30 years" What am I supposed to learn from that? What's the significance of the 30 years? What's the significance that global increased while US increased 1%?

a) concept on p 2-2 line1 14 that nature has been providing humans a subsidy seems to be underlain by a thought that we shouldn't be expecting nature to do this. I could equally well defend a viewpoint that nature is "cheating" us. By extracting fossil fuels we are speeding up what is in the long run a closed cycle – why can't nature speed up its end to keep the atmosphere at the same level of carbon dioxide? is to me no less an interesting framing than way does nature take up as much as it does. I would drop the "subsidy" terminology as not helpful for understanding. Fig. 2-1 when referred to at the beginning appears intended to dump on the reader a lot of global budget numbers that may not otherwise be addressed in the text. It would better it not be mentioned until discussed. Likewise, Fig 2-2 comes in on the first page without discussion. It might be more appropriately introduced as part of the section "Anthropogenic Perturbations".

b) p 2-3 line 1: "Future changes could…alter" Does that mean that the current lowering of pH by 0.1 is not enough to alter anything?
c) p 2-3, l 24" "The lower ice-age concentrations.." is better phrased as "The lower concentrations in the atmosphere during the previous ice age…"
d) line 33 6000 pm 3000 Gt. Is this something I should be seeing on Fig. 2.1?
e) page 2-4 line 3. change the "or" to "i.e."
f) p 2-4 line 7-10: How can the rate of fossil-fuel comsumption = 6 be 400 times the current global primary production? This is a good example where numerical statements supported by some reference to a graphics would be a lot clearer as to intent.
g) p 2-5 line 17-19 "Changes …five different approaches." These are the key technologies currently available to provide information – I would have expected the report to be much more responsive to the need for assessment of what these currently can and cannot do and how they may advance in the future. Fortunately, the report is overall well referenced so a scientist can make his own assessment from the relevant literature. However, the other intended audience may not have that capability. line 29: "increasingly, the need to treat land as a residual is receding…" is a casual assessment statement that I have a hard time believing having just read the IPCC AR4 treatment of carbon cycle and seeing that they judged that still the most accurate way to get land sink was as such a residual.
h) p 2-8 line 28-32: Mentions one reference with one sign and a second reference giving an opposite sign – not a good place to stop for a reader who is trying to get something out of this.
i) p 2-19 Table 1: what is the final "sink" row? In particular, where does the 0.03 come from?

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July 2006

Chapter 3

Reviewer 3 (reviewed exec summary and all of Part I)

Page 3-1, lines 25 and 27: See previous comments on text from page ES-4, second and fourth sentences, with respect to handling of estimates and their uncertainties.

Page 3-1, lines 30−37, and page 3-2, lines 1−3: The bullets on these lines of text largely repeat material given in the KEY FINDINGS in Chapter 2. Thus, the authors of the two chapters should coordinate their presentations to avoid unnecessary duplication.

Page 3-2, line 26: See previous comments on text from page ES-4, second paragraph, second and fourth sentences, with respect to handling of estimates and their uncertainties.

Page 3-3, first paragraph in subsection on Carbon Sinks and Table 3-1 on page 3-17: I liked the way the data and uncertainties were presented in Table 3-1. However, I think the table is too complex for a general audience without an extensive amount of additional explanation. Incorporate into a text box to deal with this or expand the discussion in the first paragraph of the subsection? Which year(s) does this set of values represent?

Page 3-3, lines 19, 21, 22, and 24−28: See comment on page 3-1, lines 25 and 27. Also note that Chapter 15 indicates that the estimate of river export to the oceans (given as −35 Gt C in Table 3-1) is essentially unknown.

Page 3-3, line 27: The word "are" should be replaced by "may be;" the uncertainties are huge, per Chapter 15.

Page 3-3, line 32: The land sink is given as 1.1 Gt C per year (or 1100 Mt C per year) on page 2-7 vs the figure of 1500 Mt C per year given here. What is the reason for the difference and which value is preferred?

Page 3-4, line 3: Table 3-1 is referenced as the source of information given on lines 1−3 but it contains no data on land area or global carbon sinks.

Page 3-4, lines 19−25: Excellent! This is the sort of information I was asking for in previous comments, such as: What types of activities are most critical to achieving the goals of carbon cycle research? However, it would also be useful to estimate long we think it will take to obtain such estimates and to identify critical obstacles (e.g., technology development needed to provide meaningful data)?

Page 3-4, line 24: If I have read Table 3-1 correctly there are five, rather than four, missing pieces to the puzzle represented by Canada's carbon budget.

Page 3-5, lines 25−30, and Fig. 3-2 on page 3-20: The second sentence in the figure caption reads as follows: Note that carbon emissions per unit GDP decelerate as a country gains wealth. I could not discern such a pattern because most countries of the world are not identified in the figure and the patterns

for countries in the region identified as Western Europe are different from those for the U.S, Japan, and Canada (the patterns for which are similar to that for China).

Page 3-6, lines 9 and 10: Because of the general readership intended for the report, I suggest saying explicitly why C emissions from coal, oil, and natural gas are different, e.g., because of increased H:C ratio as you move from coal to gas.

Page 3-6, lines 31−34, and page 3-7, lines 1−7 (caption for Fig. 3-3), and Fig. 3-3 on pages 3-21 and 3-22: The organization of the figure caption is confusing and the sets of information shown in the three panels of the figure are different enough to deserve being separated into three separate figures.

Page 3-7, last five sentences in first paragraph of subsection on North American Carbon Sink: I think that most of this material should have been used on page 3-3 to introduce the subsection on Carbon Sinks. It seems out of place here, well after the critical discussion of Table 3-1 to which it is related.

Page 3-8, lines 10 and 11: The material highlighted in bold belongs in the introduction to the subsection on Carbon Sinks on page 3-3, where Table 3-1 is called out.

Page 3-8, lines 19, 20, 21, 30, 31, and 33: What are the errors in the cited estimates? Are the number of significant figures given justified, given the errors? The value of 23 Mt C yr^{-1} for urban and suburban trees given in line 19 and referenced to Chapter 14 contrasts with the range of 13.7−25.9 Mt C yr^{-1} given in Chapter 14. How was the point estimate derived from this range?

Page 3-8, last sentence in subsection on Forests: One published study of one site, however well performed, probably doesn't constitute confirmation. How about inserting the words "are producing data that seem to" just before the word "confirm" in line 27.

Page 3-9, line 1: The text refers to "The two studies of Mexican forests." Do I correctly interpret this to mean that these are the *only two* studies of Mexican forests that deal with the subject at hand?

Page 3-9, last sentence in subsection on Woody Encroachment: Excellent! Any thoughts to on how to tackle this challenge successfully?

Page 3-10, line 3: The current wording is awkward. How about deleting "1.5 to −6 Mt C yr^{-1}" and inserting the words "either a small source of 1.5 Mt C yr^{-1} or a sink of 6 Mt C yr^{-1}" after the word "and"?

Page 3-10, first sentence in subsection on Wetlands: Wording seems awkward. How about deleting replacing the last part of the sentence on line 8 and inserting with "because plant productivity has exceeded decomposition" before the word "thousands" on page 7?

Page 3-10, last sentence in second paragraph of subsection on Wetlands: Good, but, given the technical challenges associated with doing this, should recommendations on how to do this also be given?

Page 3-10, third paragraph of subsection on Wetlands: The potential importance of CH_4 with respect to its properties as a GHG begs the question why distinctions haven't been made about differences in fluxes of the various types of carbon compounds. Are there other situations where fluxes of CH_4 or CO need to be considered specifically. Perhaps a brief discussion somewhere in the report (at the beginning of Chapter 3?) could suffice to answer this question, e.g., to estimate in rough terms what uncertainties are introduced into source/sink estimates by considering carbon fluxes without regard to the chemical species present. I suspect that these would be quite small in all but a few (but potentially very important cases, e.g., marine sediments, permafrost soils, and wetlands).

Finally, shouldn't the reference in line 25 be to Chapter 13 rather than Chapter 9?

Page 3-10, line 30: Probably need to define alluvium and colluvium for a more general audience.

Page 3-11, lines 1, 5, 8, 29, and 30−33: Once again: What are the errors in the cited estimates? Are the number of significant figures given justified, given the errors?

Page 3-11, subsection on Coastal Waters: Within the coastal waters of North America (see Fig. 15-3) are significant deposits of methane hydrates, which at least some analyses identify as a potentially significant carbon source to the atmosphere under some climate-change scenarios, one which could augment global warming from CO_2. Given results from paleoclimate studies that indicate that such a release led to dramatic warming during the Tertiary period, doesn't the uncertainty in the future carbon flux associated with this potential source deserve to be mentioned somewhere in the report, e.g., in Chapters 3, 12, and/or 15?

Page 3-17, Table 3-1: How can the totals for the U.S, Canada, Mexico, and North America all have the same estimated uncertainty, given the wide variation in inputs (including more missing data for Canada and Mexico than for the U.S.)? The estimated uncertainty of 10% for emissions from coastal waters of North America is in seeming conflict with the material in Chapter 15, which suggests that the errors are huge and exceed ±100% (see page 15-1).

Page 3-18, Table 3-2: What are the estimated uncertainties in the tabulated values?

Page 3-23, paragraph 2, third sentence: Figure 3-2 does not provide the information on emissions and change in cropland area discussed in this sentence.

Page 3-24, lines 31 and 33: Again: What are the errors in the cited estimates? Are the number of significant figures given justified, given the errors?

Reviewer 5

Page 3-2, line 22: The other chapters don't seem to have such an extended introduction. This should be homogenized. The main points of this introductory summary are also is mentioned in the section "key findings".

Page 3-3, line 15: Zero emission growth doesn't mean zero emissions. Since the target is essentially a decrease in emissions, the reduction to 0% growth shouldn't be overemphasized.

Page 3-3, line 25: "i.e." should be replaced by "e.g."

Page 3-4, line 4: The introductory summary shouldn't refer to the appendix. First there should be more detailed information contained in the chapter itself that can refer to the appendix.

Page 3-5, line 20: the sentence "Thus, countries with a slope close to the line have higher carbon intensities than countries far from the line." should be moved to line 17, before the sentence starting "Note that the United States is no outlier in this respect.".

Page 3-7, lines10-12: I would recommend first mentioning the focus of this chapter, then referring for historical development to the appendix 3A. Otherwise the reader may be inclined to read first the appendix.

Page 3-7, line 12 "we rely exclusively on inventory methods", and page 3-7, line 26 "We do not include estimates obtained in this way because they are still highly uncertain at continental scales": I don't think it is a wise decision to not at all include results from inverse modelling of atmospheric observations, for several reasons:

1) Atmospheric inversions provide independent evidence, even if current uncertainty estimates seem larger than inventory based approaches.
2) Estimates from inventory methods need upscaling from the plot scale to the region/continent; atmospheric inversions provide a constraint at these scales that are inaccessible to other methods.
3) Interannual variability in biosphere-atmosphere exchange cannot be measured with inventories that are repeated every 5 to 10 years; the atmosphere provides information on this variability, which can give insight in biosphere-climate interactions (c.f. Roedenbeck et al., Atmos. Chem. Phys., 3, 1919–1964, 2003).
4) Comparing uncertainty estimates of a single inventory based assessment with the overall uncertainty of multiple inversion results (several transport models, coarse and fine temporal and spatial resolution) might be misleading; a comparison of many inventory based assessments with a single inversion result would be required for a more balanced assessment of uncertainties.

Further it should be mentioned that current developments in the CO_2 measurement network (e.g. tall observing towers, remote sensing of atmospheric CO_2 columns from space) as well as in inverse modelling (increased spatial and temporal resolution, coupling of atmospheric transport with better a priori information in form of flux models) will provide a significantly higher data density for future assessments.

Page 3-17, line 3-8: A relative uncertainty of numbers that can be either positive or negative does not make sense. For example, agricultural soils in Canada and Mexico would have a 95% confidence range from 0 to 0 Mt (i.e. zero uncertainty), which is obviously wrong.

Reviewer 6

Are the scope and intend of the synthesis clearly described ?
YES

Are all aspects of this charge fully addressed ?
YES

Do the authors go beyond this charge or their expertise ?
NO

Are the conclusions and recommendations adequately supported by evidence, analysis and argument ?
YES

Are uncertainties or incompleteness in the evidence explicitly recognized ?
NO:
It is in fact one of my major criticisms of the report that in the Executive Summary (Pages ES-4 to ES-8 line 12) as well as in Chapter 3 (except for Table 3-1 and page 3-7, line 19-22) no uncertainty ranges of the sources and sinks fluxes of carbon in North America are given. For example, the estimated uncertainty of fossil fuel CO_2 emissions is about 10% (with 95% confidence, see Table 3-1) but up to four significant digits of the cited numbers are given. This deficiency is even more obvious when it comes to the sinks which in most cases are uncertain to within 50-100%. This is very misleading as it gives the impression to the reader that the fluxes reported would be known to very high precision, but in fact the contrary is the case. The digits in the reported numbers need to be reduced to the significant ones (i.e. ≤ 2) and errors need to be reported, also in the Executive Summary.

Are the data and analyses handled competently? Are statistical methods applied appropriately?
In general YES, except for two points:
(1) To calculate the mean increase rate of fossil fuel CO_2 emissions, the authors chose the time period of 1974 – 2003 (30 years). This period includes 12-15 years of constant or even decreasing emissions while the last 20 years, starting about 1983 until today show a much larger increase rate than 1% per year (Figure 3-1). Later, e.g. in Figure 3-2 when discussing the relation between GDP and fossil fuel CO_2 emissions the time window from 1980-2003 is used. I think it would be more appropriate to chose the same time periods for the analysis of the emissions increase rate throughout the report.
(2) On page 3-2 line 27 and on page 3-4 line 1 the authors refer to the global land area and the North American share of 16.5 % of this area. I think a relation of the North American carbon sink to the total global land area is not really appropriate here as total land area includes Antarctica and Greenland (ca. 10%), as well as deserts (ca. 6%). A comparison with land areas with similar ecosystems may be appropriate but I would suggest skipping this relation completely.

Are the report's exposition and organization effective ?
NO
There is a lot of repetition in the Executive Summary between the sections "How do North American carbon sources and sinks relate to the global carbon cycle" and the following section which is dealing only with North American carbon sources and sinks. The Executive Summary could be well shortened if the "relation to the global carbon cycle" would be imbedded in the latter section.

In Chapter 3 there are even more repetitions of this kind as there is a section on "Key Findings" which is nice but this is followed by an "Introductory Summary" which e.g. for the fossil fuels has approximately the same length as the main section on "North American fossil fuel emissions". Again I would combine the "Introductory Summary" with the main sections which would avoid these many repetitions. In fact, most of the message of the section is summarized in Table 3-1 so that Chapter 3 could be shortened considerably without loosing the major messages.

Figure 3-1 has a somewhat odd scaling, would be easier to read if a metric system for the tics was used.

Figure 3-3 should have larger labels and in (a) the green dots do not copy well in b&w. The sectors in the caption in (c) should be named the same as in the legend.

It would be very helpful and much more instructive if SI units were used for the fluxes throughout the text, i.e. instead of Mt C yr^{-1} it should read 10^{12} gC. My favourite would be 10^{15} gC = 1 Pg C everywhere which would also solve the problem with the large numbers with insignificant digits as those numbers will become small then.

Is the report fair and appropriately balanced ?
YES.

Is the report's tone impartial and devoid of special pleading ?
YES.

Are any of the report's findings based on value judgements or the collective opinions of the authors ?
NO.

Does the Executive Summary concisely and accurately describe the key findings and recommendations ?
NO. Could be considerably shortened (see above).

What other significant improvements might be made in the report ?

ES-3, line 14-17: "… and other carbon compounds in the earth's atmosphere, such as methane, are increasing." The context of this finding is not immediately clear from what is said before.

3-1, line 30: Should read North American "fossil fuel" carbon dioxide emissions …

3-2, line 31: EIA needs to be explained
3-2, line 33: …, "with approximately … global total" should be deleted as it was mentioned in the sentence before.
3-2, line 36: It should read: Total U.S. emissions "are expected" to continue growing …

3-3, line 28: However, "much of the CO_2…" "much" could be something between 40% and 95%, is there an approximate number to be given, such as more than 50% or so ?

3-5, line 20: should read …to the "solid" line …

3-7, line 31: (1700 MtC yr^{-1}) here I would also put a minus sign as this number should be compared with the -753 Mt C yr^{-1}. The signs of the numbers of sources and sinks should be VERY consistent throughout the text !! I am not sure if this is the case yet.

3-8, line 7: should read …and North America "as a whole" are listed …

3-8, line 8-10: mixing up “millions” and “billions” could immediately be avoided if numbers were always given in Pg C or Pg C yr^{-1}.
3-8, line 10-11: I do not understand why reference is given here to Table 3-1.
3-8, line 27-28: To “confirm” estimates of inventories and to “converge towards better agreement” (see 3-25, line 18) are of significantly different quality … A more quantitative statement should be made here.

3-9, line 4: That these 10 years old numbers are used in Table 3-1 should be explicitly mentioned.

3-10, line 13: The unit Gt C should be avoided here, better use Pg C (or 1000 Mt C).
3-10, line 19-20: Are the CH_4 fluxes included at all in the carbon fluxes reported here (i.e. cattle breeding and rice cultivation as anthropogenic sources). This should be made clear.
3-10, line 30-32: What kind of reservoirs ?

3-18: What are the uncertainties of the carbon stock numbers given here ?

3-23, line 19: I do not see any cropland change plotted in Figure 3-2.

3-25, line 14: … consistent within several tens of g C m^{-2} yr^{-1} for … Here it would be better to report relative rather than absolute deviations.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 4

Reviewer 3 (reviewed exec summary and all of Part I)

Page 4-1, title of chapter: The title is somewhat vague and lacks pizzazz. As an alternative, how about "Options and Measures for Rebalancing the Carbon Cycle and Reducing Atmospheric CO_2"?

Page 4-2, first paragraph of subsection on Source Reduction Options: These conclusions are very important; however, they are not supported by evidence and references. This is a recurring theme in the presentation of material throughout this chapter.

Page 4-3, subsection on Efficiency Improvement: The main focus seems to be on improved efficiency in end use rather on generation and transmission/transport. Shouldn't options for increased efficiency in electric power generation (i.e., in addition to cogeneration) or transmission or in vehicles (e.g., hybrids, fuel cells) be mentioned?

Page 4-4, third paragraph: No mention is made of biodiesel which also can be used directly.

Page 4-5, last paragraph: Should the current research on development of methane hydrates from marine sediments and permafrost soils as a potentially significant energy source also be mentioned, even though this is a longer term option?

Page 4-6, line 5: The difference between afforestation and reforestation should be explained for the general reader.

Page 4-6, fifth paragraph, first sentence: This is hyperbole. Many but certainly not thousands have been identified.

Page 4-6, line 29: The comma after "telecommuting" should be moved and placed after "demand."

Page 4-7, last two paragraphs and figure caption, and Fig. 4-1 on page 4-20: A reference to Chapter 8 as the source of the data presented and of more detailed discussion on the topic should be given both in the text and the figure caption. The figure caption also needs to indicate these cost estimates are for options to reduce emissions and/or enhance sequestration of carbon. The options presented in Table 4-1 seem to be too general to be appreciated without additional information on the characteristics of each. Unless there is some effort to indicate the potential significance of each option by presenting the carbon reduction potential on a common basis, preferably in Mt C per year, the comparisons will not be particularly meaningful. Also, what does "marginal cost" mean with respect to the last three options in the table?

Page 4-7, last paragraph, seventh sentence: This statement deserves additional explanation and perhaps an example to illustrate what you mean.

Page 4-8, second paragraph, last sentence: Please identify the chapters in which these complications are discussed.

Page 4-8, third paragraph: I recommend that you again reference Chapter 8 as the source of this information.

Page 4-8, fourth paragraph, line 21: Ancillary costs (e.g., from environmental degradation or risks to human health from some sequestration options) should also be mentioned.

Page 4-8, fourth paragraph as a whole: What appears to be needed is an integrated analysis that covers all types of emissions and all costs, including those produced by negative environmental consequences. Focusing only on benefits from CO_2 reduction could overlook critical unforeseen consequences (e.g., from effects of some proposed sequestration options and development of alternative energy sources with lower carbon intensity). One example of the latter: Extraction techniques could destabilize deposits of methane hydrates in marine sediments and increase the potential for catastrophic releases in conjunction with expected future warming. A total systems approach is needed.

Page 4-8, last paragraph: The use of the verb "will" in each sentence in this paragraph has not been justified by the material presented thus far. Although I might agree with the current wording, the justification will not be apparent to all readers. Thus, I recommend making this the second paragraph of the Overview subsection and leading off with the paragraph at the top of page 4-9.

Page 4-9, line 17: What does "environmentally effective" mean?

Pages 4-9 and 4-10, third and fourth paragraphs of subsection on General Considerations: The authors need to provide evidence with references to support their conclusions.

Page 4-10, line 5: Awkward wording. How about "The framework for choosing a policy instrument needs to include consideration of institutional…"

Page 4-10, third paragraph, last sentence: Would the sentence read better if "lower costs for" were inserted before "societal benefits" and "offset" was substituted for "exceed" in line 20?

Page 4-11, lines 1 and 2: The words "macroeconomic" and "distortionary" need to be defined.

Page 4-12, line 29: Either "help" or "are needed" should be deleted.

Pages 4-13 and 4-14, Conclusions section: This section as a whole is marked by presentation of conclusions that are not supported by the information presented in the chapter or in several cases not discussed at all prior to this section. Although I have provided specific comments below, I think that this entire subsection could be deleted, given that much of the material was included in the KEY FINDINGS section at the start of the chapter. Of course, the key findings would still need to be supported with evidence and references.

Page 4-13, lines 11 and 12: I would argue that sequestration of 20% of current emissions is not small when measured against other control options discussed in this chapter. In addition, the reversibility of uptake by agricultural soils and forests was discussed previously (on page 4-12) in the context of "a forest fire or tilling the soil," implying a single event or location. This does not convince me that a coordinated continent-wide program "can be reversed easily."

Page 4-13, lines 22−29: These subjects were not covered in this chapter.

Page 4-14, lines 18−23, and bullet on page 4-2, lines 5−10: This material was not covered in this chapter.

Reviewer 7

page 4-3, lines 6-23 – This section should highlight the overwhelming potential of improvements in energy efficiency to reduce greenhouse gas emissions. The United States uses nearly twice as much energy per person as Japan, the United Kingdom, and other countries that enjoy a high material standard of living (IEA 2005). The United States could significantly improve the efficiency of its energy use and reduce greenhouse gas emissions by up to half using existing technology without major sacrifices to the material standard of living.

page 4-4, lines 19-23 – This section should highlight the overwhelming potential of renewable energy sources to reduce greenhouse gas emissions. In 2003, the world rate of energy use totaled 14 TW or 14 trillion watts.
Nevertheless, available solar and wind power resources could potentially provide energy to the world at a rate of 70 TW (UNDP 2000).

page 4-11, line 7 – The section should note that twenty states and the District of Columbia have enacted policies that set a target for the fraction of electricity that utilities generate from renewable sources from 5% to 30%
(REN21 2005).

page 4-11, lines 23-25 – The chapter would benefit from citing the potential positive impact of an increase in U.S. Corporate Average Fuel Efficiency (CAFE) motor vehicle standards. Raising CAFE from the current level of 22.2 miles per gallon for light trucks and 27.5 miles per gallon for passenger cars to 39 miles per gallon, a level still lower than current standards in the European Union and Japan, could reduce oil consumption and carbon emissions by 37% (National Commission on Energy Policy 2004).

references
International Energy Agency (IEA). 2005. *Key World Energy Statistics 2005.* IEA, Paris, France.
National Commission on Energy Policy. 2004. *Ending the Energy Stalemate: A bipartisan Strategy to Meet America's*
Energy Challenges. National Commission on Energy Policy, Washington, DC.
REN21 Renewable Energy Policy Network. 2005. *Renewables 2005 Global Status Report*. Washington, DC:
Worldwatch Institute.
United Nations Development Programme (UNDP). 2000. *World Energy Assessment*. UNDP, New York, NY.

Reviewer 8

In general Chapter 4 performs a credible job of reviewing technological and policy options for addressing carbon dioxide emissions. Given that the Chapter is charged with presenting an exceeding complex and large range of information in very few pages, the author is to be commended on having, by and large, successfully carried out this task. The three main areas in need of attention are:

1. Readability.
2. Definition of scope of cap-and trade system and its relation to reductions achieved through regulations
3. Accuracy or completeness of a number of statements.

Readability

The Chapter is, no doubt as a consequence of the attempt to cover a great deal of complex material in very few pages, written in a very terse manner. A good editor could, and should be used to, render the text smoother and more easily readable.

Cap-and-trade and Relation to Regulatory Approach

The most serious problem with the chapter is the disconnect between the primacy given to an emissions trading program and evidence presented which suggests serious limitations of such a program. This problem is compounded by the omission, throughout the chapter, of any definition of the scope of the cap program and of the emissions trading program. The chapter seems to imply that a cap-and-trade program would be confined to large point sources but never states this, and it is never made clear whether only capped sources could trade or whether the emission trading system is envisioned as including both capped sources and emission reductions achieved through other regulatory approaches. The chapter should specify which sources are envisioned as being covered by a cap and whether the trading system is confined to capped sources or not.

Two limitations on a cap-and-trade program discussed in the chapter seem to raise questions about the primacy of its role suggested by the chapter. These are:

A. Need to use regulatory approach for some sources. The chapter acknowledges that many sources of CO_2 – both where energy efficiency is key to reductions and where industries or individuals do not respond well to price signals-- will need to be addressed through regulations (i.e. energy efficiency standards), which would "complement" the cap-and-trade program. Energy efficiency is a major avenue for emission reductions from buildings, transportation, and appliances, "sources" which, together, are responsible for a very large fraction of CO_2 emissions. These are also sectors in which response to price signals are dampened due to a multiplicity of factors. Thus if these are not part of the cap-and-trade program, the ground for primacy of a cap-and-trade program are unclear. This is particularly true if these emission reductions (i.e., those resulting from efficiency regulations) would not be part of the emission trading system. As pointed out above, the chapter fails to clarify whether they would or would not be. As part of clarification of this question, the chapter should mention the difficult issue that would need to be resolved for such emission reductions to trade into a cap-and-trade system, e.g., establishment of baselines (to achieve "additionality" and avoid compromising the cap); avoiding double-counting; and establishing equivalencies between (fungibility of) very difference types of reductions.

B. Inability to incorporate ancillary benefits or costs. The chapter correctly points out that many options to address GHG emissions have ancillary benefits which are not taken into account by a cap-and-trade approach and that there are potential conflicts between emission reduction goals and other societal goals. These are serious issues that do not seem to be reflected in the chapter's evaluation of cap-and-trade approaches. The inability of a cap-and-trade program to incorporate multiple values is a major drawback in land use where the land use with the highest carbon benefits may conflict with other societal priorities, e.g., land for food production. The single issue focus of a cap-and-trade approach (or any other approach designed solely to reduce GHG emissions) is also likely to be a major drawback for many countries and in other sectors. For example, a cap – assuming it functions as envisioned to elicit least-cost reductions -- would very likely fail to support biofuel production at societally desirable level because they are a relatively costly reduction option that has energy security and enhanced rural income benefits. This

suggests that regulatory approaches that can take multiple societal goals into account (e.g., a biofuels mandate) may be more useful and more likely to secure support.

Finally, the chapter points out that choosing the least-cost combination of options would be a daunting task and that it is unlikely that policy-makers can do so. It then goes on to state that policy-makers can adopt permit trading and allow the emitter to choose the lowest cost options. This assumes that the emitters (i.e., the private market) will be better able to find and choose the least-cost emission reduction path. However, the chapter fails to provide any support for this position. One option would be to define the circumstances under which the private market will be better able to select least-cost options than the government. Furthermore, if least-cost options occur through energy efficiency regulations – and there is good reason to suppose that energy-efficiency improvements in sectors such as transportation, buildings and appliances may indeed be a major source of low-cost reductions -- it is unclear whether such reductions would be available for use by capped entities (see A above). If they are to be available, the chapter should acknowledge circumstances under which companies may not select such options, e.g. preference for options over which they have more control, about which they are better informed, or which provide ancillary benefits (e.g., learning by doing, PR, etc.).

In short, the chapter should clarify the envisioned extent of a cap-and-trade program (e.g., large point sources) and whether reductions achieved through other types of regulation are envisioned to participate in the trading scheme. Its evaluation of cap-and-trade should also reflect the seriousness of the limitations described in the chapter.

Accuracy or completeness of a number of statements

Statements that should be modified to improve accuracy or comprehensiveness:

1. Page 4-2. List of options to reduce energy-related emission. The chapter covers both energy and non-energy based emissions. Therefore there should also be a list of the non-energy related options covered in the chapter.
2. Page 4-4. Lines 17-18. Other factors in the CO_2 reductions achieved should be listed, e.g., the inputs used to produce the biomass (fertilizer, irrigation water), whether the land is existing cropland or converted from forests or grasslands, and the management practices used (no-till, conventional till).
3. Page 4-5. Lines 13. While perhaps technically correct, the statement that integrating CO_2 capture and storage into our energy system is mainly a long-term option may mislead readers into thinking that one can not start deployment of CCS today. CCS can currently be undertaken in "niche" situations, and its more widespread deployment is feasible both in the near and medium-term.
4. Page 4-5. Lines 30-32. It should be pointed out both that the opportunities to reduce ruminant emissions in the United States are limited (due to the fact that animal feed is in most cases already optimized) and that little is known about the costs of achieving such reduction.
5. Page 4-6. Line 14. The rate of sequestration following conversion to forestland depends on a good many factors other than soil type, including both environmental factors (such as climate, topography, type of trees planted) and management practices (including thinning, fertilization, pest control, etc.).

6. Page 4-6. Lines 22-24. Policy makers also need to know the magnitude of reductions likely to occur in response to pursuing reductions of a given type or at a given price.
7. Page 4-6. Line 28. Insert ",in addition to the factors previously cited," prior to "...on other measures as well, such as telecommuting,…"
8. Page 4-7. First sentence. Provide some substantiation of this claim or delete.
9. Page 4-7 Text box and Table 4-1. In the Text box an excellent job is done of explaining supply curves and informing the reader of their pitfalls. Similar cautions should be provided for the costs presented in Table 4.1 as these cost estimates involve as least as many problematic assumptions as the supply curves.
10. Page 4-8. Line 11-12. If examples are provided in other chapters, the numbers of such chapters should be specified.
11. Page 4-10. Line 5. Insert "technical" into the list, i.e., the choice of policy instrument also needs to consider technical constraints.
12. Page 4-10. Line. 14. Explain the term compensating variation or delete.
13. Page 4-10. Footnote #15. While this may be true of some regulatory approaches, I doubt that it has been proven, in general, for all regulatory approaches, e.g., for those than require a certain efficiency level. I doubt there has been enough experience with trading programs in general to support this.
14. Page 4-11. Line 13. There is contradictory evidence about the impact of taxes on vehicle fuels, at least at any level likely to be imposed. Although there may be some demand response to price spikes, transportation demands appears to be relatively inelastic.
15. Page 4-11. Lines 18-19. While the diversity in sources of CO_2 may mean that emissions trading could yield significant cost-savings, this same diversity poses serious problems for such a system (see discussion above) and this should be acknowledged.
16. Page 4-11. Line 28. Change the title to "Terrestrial Sequestration Policies"
17. Page 4-12. Lines 7-12. Both the establishment of baselines and leakage also poses a major challenge for such polices. These should be added.

Page 4-12. Line 27. While induced technological change may justify earlier targets, either support the statement that it justifies more stringent targets or delete.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 5

Reviewer 3

General Impressions [addressed to whole report]:

The preparation of this report is a challenging undertaking given its intended audience (scientists, decision-makers in the public and private sectors, the international community, and the general public) and the wide range of technical issues that have to be addressed. In most respects, the sections I've examined appear to be reasonably well written with respect to use of the Queen's English and thus I have not focused on editorial concerns (e.g., obvious typos, etc., which are few) unless these significantly affected clarity or accuracy.

However, at this stage in its development, the report seems more targeted to a technically oriented audience than to non-scientist decision-makers or the general public. To reach the latter, I think there needs to be a much more careful elaboration of technical material in the text, as well as in table and figure legends.[4] Tables and figures need to "stand alone" even for a technical audience but merit considerable explanation when communicating with a less technically trained audience.

It also appears that the authors of specific chapters or sections have not had sufficient time to read other contributions, as evidenced by unnecessary repetition and some inconsistency in technical information conveyed from one section to the next. As one example, there appears to be considerable variation in the treatment of uncertainties in point estimates of carbon sources, fluxes, and sinks among the various sections of the report, and rather questionable handling of estimates and their uncertainties in most sections. Among the most glaring is the citation of the estimated carbon sink for North America (in Gt of C) to three significant figures, when the estimated error is on the order of ±50%. However, my concern also extends to some aspects of the better constrained estimates of fossil fuel emissions, which are sometimes given to four significant figures. I'll detail these concerns in my specific comments.

The title of Chapter 1, on which I was asked to focus my review, indicates that it is to provide information on the "purpose, scope, and structure" of the SOCCR. I do not think that it achieves this objective in its current state. I found little information on the scope of the report as a whole and nothing at all on its structure.

I kept looking for answers to questions that I thought should be addressed in a report of this type. What do we know and how well do we know it? "How well" in this context goes well beyond quantitative estimates of uncertainties in sources and sinks: How closely does current information meet the expected needs for carbon-climate modeling and analysis of carbon management options? Other questions include: What don't we know and why? What are the most important uncertainties and why? What will it take to reduce uncertainties to manageable levels and how long do we expect it might take? Where is R&D needed to provide the tools that could lead to improved understanding? What can't we know and what are the implications, e.g., where are uncertainties only poorly reducible (or perhaps irreducible) and how do we plan to deal with them?

I found very few answers to my questions in the current version of the report—and nothing at all in Chapter 5, despite its title. In fact, I get the impression that tackling these tough but critical questions is being deferred in pursuit of a yet to be established (and perhaps elusive) process (see fourth paragraph on page 5-9). Several other synthesis and assessment reports (namely SAP 5.1, 5.2, and 5.3), organized under

[4] The authors may want to consider the way some figure and table legends are handled in *American Scientist*; see, e.g., article by R. Seager, **94**: 335−341, July-August 2006.

the heading "Explore the uses and identify the limits of evolving knowledge to manage risks and opportunities related to climate variability and change," were identified in Chapter 5, but, based on the descriptions of these activities given on the CCSP web site, they will not address my questions either.

Based on the material I've looked at, I think the report could benefit from appointment of an overall editor(s) whose focus would be on consistency in presentation of technical information and facilitation of communication with the broad audience for which the report is apparently intended. Toward that end, I would also recommend that every effort be made to use a single unit of measure and carbon reference (e.g., either Gt or Pg of C) throughout the report. Mixing units of Pg, Gt, and Mt, and jumping between values referenced either to C or CO_2 equivalents is confusing to the reader.

Pages 5-1 through 5-15, Chapter 5: Although I don't have much to quibble about with respect to this subject or how it is presented in this chapter, I do question why an entire chapter is devoted to the subject of improved application of scientific information to decision support when so little is devoted to an assessment of where we are currently (see General Comments). Our "process" will never be perfect, but we have to tackle the difficult questions now in order to make continued, effective progress.

Page 5-3, lines 1, 3, and 6: Replacing the words "normative," "actors in these sectors," and "entities" with simpler synonyms would help to improve communication with the more general audience for which the report is reportedly intended.

Page 5-4, first paragraph: Why is there no mention of NOAA's role, i.e., the Climate Program Office and its activities?

Page 5-4, last paragraph, last sentence: I recommend "translating" the quote so that it is more likely to be understood by a member of the general public.

Page 5-5, lines 16−18, and page 5-10, line 3: I would recommend purging the word "salient" from your lexicon, and simply replace it with "relevant" or "particularly relevant." It is another example of unnecessarily complex wording, and is a very poor synonym for the parenthetical expression on lines 16 and 17, despite the assertions of Cash et al. In fact, I think deleting both "salient" and "legitimate" and eliminating the associated parentheses is not only simpler but more effective in getting your message across.

Page 5-5, line 32: Would inserting the word "expanded" before the word "participation" communicate your message more effectively?

Page 5-8, line 6: I think the acronym NGO needs to be added to the Text Box (see line 9 on page 5-15) since the definition of NGO is not given elsewhere in the text.

Reviewer 8

The entire chapter should be edited to improve readability. The title and a number of sections in the chapter require too many readings in order to understand them and grasp their points. The title should be shortened and simplified. The ideas are pretty diffuse and conceptual, and it is difficult to relate some of the sections to the main point of the chapter.

The Chapter does provide a useful summary of the general barriers to linking carbon cycle science with solutions and offers possible approaches to overcome some of those barriers. However, the manner in which this chapter is written has the result that it is likely that it will only be understood, or considered relevant, by the science audience. Other audiences or

stakeholders are unlikely to get much out of this chapter as presently written, which makes it unlike the rest of the report.

Although the chapter as written is not directly useful for applying science to management, it does shed some light on areas that most scientists don't think much about. With that in mind, the recommendations, as loose as they are, are appropriate insofar as they are directed at scientific organizations which may be able to implement or modify programs to enhance the utility of their science for carbon management.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 5

Reviewer 3

General Impressions [addressed to whole report]:

The preparation of this report is a challenging undertaking given its intended audience (scientists, decision-makers in the public and private sectors, the international community, and the general public) and the wide range of technical issues that have to be addressed. In most respects, the sections I've examined appear to be reasonably well written with respect to use of the Queen's English and thus I have not focused on editorial concerns (e.g., obvious typos, etc., which are few) unless these significantly affected clarity or accuracy.

However, at this stage in its development, the report seems more targeted to a technically oriented audience than to non-scientist decision-makers or the general public. To reach the latter, I think there needs to be a much more careful elaboration of technical material in the text, as well as in table and figure legends.[5] Tables and figures need to "stand alone" even for a technical audience but merit considerable explanation when communicating with a less technically trained audience.

It also appears that the authors of specific chapters or sections have not had sufficient time to read other contributions, as evidenced by unnecessary repetition and some inconsistency in technical information conveyed from one section to the next. As one example, there appears to be considerable variation in the treatment of uncertainties in point estimates of carbon sources, fluxes, and sinks among the various sections of the report, and rather questionable handling of estimates and their uncertainties in most sections. Among the most glaring is the citation of the estimated carbon sink for North America (in Gt of C) to three significant figures, when the estimated error is on the order of ±50%. However, my concern also extends to some aspects of the better constrained estimates of fossil fuel emissions, which are sometimes given to four significant figures. I'll detail these concerns in my specific comments.

The title of Chapter 1, on which I was asked to focus my review, indicates that it is to provide information on the "purpose, scope, and structure" of the SOCCR. I do not think that it achieves this objective in its current state. I found little information on the scope of the report as a whole and nothing at all on its structure.

I kept looking for answers to questions that I thought should be addressed in a report of this type. What do we know and how well do we know it? "How well" in this context goes well beyond quantitative estimates of uncertainties in sources and sinks: How closely does current information meet the expected needs for carbon-climate modeling and analysis of carbon management options? Other questions include: What don't we know and why? What are the most important uncertainties and why? What will it take to reduce uncertainties to manageable levels and how long do we expect it might take? Where is R&D needed to provide the tools that could lead to improved understanding? What can't we know and what are the implications, e.g., where are uncertainties only poorly reducible (or perhaps irreducible) and how do we plan to deal with them?

I found very few answers to my questions in the current version of the report—and nothing at all in Chapter 5, despite its title. In fact, I get the impression that tackling these tough but critical questions is being deferred in pursuit of a yet to be established (and perhaps elusive) process (see fourth paragraph on page 5-9). Several other synthesis and assessment reports (namely SAP 5.1, 5.2, and 5.3), organized under

[5] The authors may want to consider the way some figure and table legends are handled in *American Scientist*; see, e.g., article by R. Seager, **94**: 335−341, July-August 2006.

the heading "Explore the uses and identify the limits of evolving knowledge to manage risks and opportunities related to climate variability and change," were identified in Chapter 5, but, based on the descriptions of these activities given on the CCSP web site, they will not address my questions either.

Based on the material I've looked at, I think the report could benefit from appointment of an overall editor(s) whose focus would be on consistency in presentation of technical information and facilitation of communication with the broad audience for which the report is apparently intended. Toward that end, I would also recommend that every effort be made to use a single unit of measure and carbon reference (e.g., either Gt or Pg of C) throughout the report. Mixing units of Pg, Gt, and Mt, and jumping between values referenced either to C or CO_2 equivalents is confusing to the reader.

Pages 5-1 through 5-15, Chapter 5: Although I don't have much to quibble about with respect to this subject or how it is presented in this chapter, I do question why an entire chapter is devoted to the subject of improved application of scientific information to decision support when so little is devoted to an assessment of where we are currently (see General Comments). Our "process" will never be perfect, but we have to tackle the difficult questions now in order to make continued, effective progress.

Page 5-3, lines 1, 3, and 6: Replacing the words "normative," "actors in these sectors," and "entities" with simpler synonyms would help to improve communication with the more general audience for which the report is reportedly intended.

Page 5-4, first paragraph: Why is there no mention of NOAA's role, i.e., the Climate Program Office and its activities?

Page 5-4, last paragraph, last sentence: I recommend "translating" the quote so that it is more likely to be understood by a member of the general public.

Page 5-5, lines 16−18, and page 5-10, line 3: I would recommend purging the word "salient" from your lexicon, and simply replace it with "relevant" or "particularly relevant." It is another example of unnecessarily complex wording, and is a very poor synonym for the parenthetical expression on lines 16 and 17, despite the assertions of Cash et al. In fact, I think deleting both "salient" and "legitimate" and eliminating the associated parentheses is not only simpler but more effective in getting your message across.

Page 5-5, line 32: Would inserting the word "expanded" before the word "participation" communicate your message more effectively?

Page 5-8, line 6: I think the acronym NGO needs to be added to the Text Box (see line 9 on page 5-15) since the definition of NGO is not given elsewhere in the text.

Reviewer 8

The entire chapter should be edited to improve readability. The title and a number of sections in the chapter require too many readings in order to understand them and grasp their points. The title should be shortened and simplified. The ideas are pretty diffuse and conceptual, and it is difficult to relate some of the sections to the main point of the chapter.

The Chapter does provide a useful summary of the general barriers to linking carbon cycle science with solutions and offers possible approaches to overcome some of those barriers. However, the manner in which this chapter is written has the result that it is likely that it will only be understood, or considered relevant, by the science audience. Other audiences or

stakeholders are unlikely to get much out of this chapter as presently written, which makes it unlike the rest of the report.

Although the chapter as written is not directly useful for applying science to management, it does shed some light on areas that most scientists don't think much about. With that in mind, the recommendations, as loose as they are, are appropriate insofar as they are directed at scientific organizations which may be able to implement or modify programs to enhance the utility of their science for carbon management.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 7

Reviewer 11

- Are the scope and intent of the synthesis and assessment product clearly described in the report? Are all aspects of this charge fully addressed? Do the authors go beyond this charge or their expertise?
    - Some of what I expected is here. Unfortunately three elements are missing: 1) better discussion of the driving forces behind rising emissions 2) more on the nature of the controversy over how emissions can be reined in and 3) more on the fundamental weaknesses of the data, both in Mexico and Canada, as well as the US. More effort should be undertaken to make the data and descriptions compatible, as these are likely to be quoted widely without caveats.
- Are the conclusions and recommendations adequately supported by evidence, analysis, and argument?
    - The key parts on projections and mitigation options/potentials are only presented weakly – what is driving per capita travel and freight, what is driving fuel use/travel or fuel use/freight, and what changes would mitigate these.
    - For Mexico, I recognize that data and even analyses are not good, but it would be very useful to review some work (even in English, but Spanish language work is more thoroughly) to give a few on projections and mitigation options. This is because Mexico is not totally motorized, so mitigation means more avoidance rather than changes to patterns that have take hold in the US and Canada.
- Are uncertainties or incompleteness in the evidence explicitly recognized?
    - No, this is a major weakness. The data from each country are fraught with uncertainties that at times are extremely misleading. This is not the fault of the author, but he should point these problems out explicitly, as others will quote these data as if they are whole. They are not. This is recognized in the last bullet of the "Key Findings" .
- Are the data and analyses handled competently? Are statistical methods applied appropriately?
    - As above, series problems in the data from each country make the overall presentations of each country incompatible with each other And there is little analysis applied to Mexico, the country portending the largest growth, and therefore the largest deviation from trends.
    - In general the huge differences in population and GDP of these countries make comparisons of absolute totals rather hopeless. It would be much more enlightening if comparisons were also offered on a per capita bases, and per unit of GDP correctly calculated using purchasing power parity of a similar base year for each country.
- Are the report's exposition and organization effective? Is the title appropriate?

  - Yes and Yes
- Is the report fair and appropriately balanced?
  - Fair and balanced but lacking a few important elements. For example, "Options for Management" skips over the heart of why there is a controversy over how much GHG could be managed. The author himself has probably written more balanced analysis of this controversy than anyone else, something worth summarizing here.
- Is the report's tone impartial and devoid of special pleading?
  - yes
- Are any of the report's findings based on value judgments or the collective opinions of the authors? If so, is this acknowledged, and are scientifically defensible reasons given for reaching those judgments?
  - No, if anything the report is so devoid of "sides" it almost comes off as empty of real content.
- Does the executive summary concisely and accurately describe the key findings and recommendations? Is it consistent with the other sections of the report?
- What other significant improvements, if any, might be made in the report?

Details by page and line
P7.1 In the fourth bullet, I would say 'biomass is a promising medium- and long-term option". There is little evidence that significant amounts of biomass can reduce GHG emissions in the US or Canada in the near term.

P7.5 Acknowledge that Mexico simply has no acceptable data on vkm by type or mode, and only has data for limited modes for pass-km or tonne-km, period. However, it is possible to note that Mexican land travel p-km is dominated by urban and inter-city buses, with rail playing a minor amount.
P7.8 line 1. Surely the author meant EJ, as 4.3 PJ is a truly tiny amount, well less than .001% of likely emissions in 2025.

Table 7-1. State explicitly how emissions from electricity production for power used by transport are counted.

Table 7.2 First, the US and Canada are probably the only countries in the world that (correctly) report natural gas use for pipelines as transport. Please check if this is the case for Mexico – it looks to me as if only natural gas vehicles are counted.

Second, until the late 1990s at least, Stats Canada reported all bunker fuel used by Canadian owned ships, whether in international or domestic (coastal, river, lake) transport, as "domestic". Kindly check whether this rather huge error (about a factor of five) has been eliminated.

Tab. 7-3. Canada and the US are able to allocate electric traction into rail and a small amount into road transport. Why not Mexico? It might be valuable to break each kind of transport into fuels, i.e., Road, rail, waterborne, and pipeline.

Worse, when one examines the table casually, one notes the ratios of emission to fuel for any

given row differ noticeable between Canada and the US. This seems to be a figment of the different definitions of "carbon emissions" and should be fixed.

Bunkers are only listed for the US, and then only their CO2 emissions. These should be explored further to 1) obtain similar figures for the other countries (or as I suggested elsewhere, ascertain whether Canada (and for that matter) count bunkering of aircraft or ships correctly) and 2) make both international aircraft fueling and international shipping part of bunkers.

The comments on Table 7.3 are troubling. If the Canadian data include different GHG than the US data, why not calculate the CO2 emissions from Canada in a comparable way --- this involves roughly 15 calculations, as was apparently done for the Mexican emissions. Again, this kind of problem leads to someone else copying the table without the caveats…I strongly object to ignoring the carbon emissions of electricity.

Figure 3a. As late as the late 1990s, Canada omitted "own account" trucking tonne-km. Please check whether these are now counted, since their counting backwards would have entailed some serious work. Please note that the US does not tabulate road freight as other countries do, rather by vehicle ("class 1"), and that some kinds of road freight, namely intrastate, are not counted but only estimated by Eno foundation and others. Kindly also check what tonne-km are counted in the Mexican data.

Figure 7-4a. Surely we could portray the US travel by mode for 2003 or at least 2002 for a report set to appear in 2006! The same should be true for Canada. Rather than relying on an old source (NATS)….A troubling aspect of these data is that they imply there is nearly as much passenger travel in light trucks as in cars. It should be noted that this estimate must be counting the use of commercial light trucks to/from work, which is acceptable as long as it is clearly marked. Otherwise, light trucks/SUVs in the US account for something like 40% of total travel.

Figures 7-5 a and b are mislabeled. The first is Mexico, the 2nd is the US. Figures 7-5x and 7.6x should be made as compatible as possible. Thus "international" in Figure 7-6b—where is that in Figure 7-5b? Why did we switch to EPA source, whose basic assumptions about energy use in transport may not agree with those used in earlier figures? Note these should also be shown per capita and per unit of GDP in US dollars converted at purchasing power parity.

Reviewer 12

Transportation is an extremely complex topic, and the authors are to be commended for covering so much information in so little space. By necessity, the treatment of various issues has had to be compressed. But, by and large, I think that the chapter does a good job of presenting both the factual information and the complexity of the issues. I do have one significant concern and several smaller ones.

Significant concern: The omission of well-to-tank emissions

At page 7-3 lines 9-14, the draft states: "In this chapter, upstream, or well-to-tank, carbon emissions are not included with transportation end-use, nor are end-of-life emissions produced in the disposal or recycling of materials used in transportation vehicles or infrastructure. These two categories of emissions typically comprise 20-30% of total life cycle emissions for transport

vehicles [citation omitted]. In the future, it is likely that upstream carbon emissions will be of greater importance in determining the total emissions due to transportation activities."

The final sentence of this quotation is certainly correct, but it understates the potential importance of upstream carbon emissions in determining transport-related carbon emissions. I believe that the report cannot claim to have provided an appropriate understanding of the likely evolution of transport-related emissions without incorporating a discussion of the "well-to-tank" emissions of various fuel types.

When I first read the paragraph quoted above, I thought that the "well-to-tank" portion of transport-related carbon emissions might be discussed in Chapter 6, "Energy Extraction and Conversion." However, when I looked at Chapter 6, this proved incorrect. I have yet to be able to find such a discussion anywhere in the report.

Ironically, the discussion in the chapter is not consistent with the first of the two sentences quoted above. At several places throughout the chapter, the authors acknowledge the importance of looking at "full fuel cycle" emissions. For example, on page 7-6, lines 10-12, the draft states: "Carbon emissions by transport are determined by the levels of passenger and freight activity, the shares of transport modes, the energy intensity of passenger and freight movements, *and the carbon intensity of transportation fuels.*" (emphasis added) In fact, a given change in *any one* of these four factors, *ceteris paribus*, produces the same change in carbon emissions from transport. Changes in the carbon intensity of transport fuels can magnify or offset changes in the energy intensity of passenger and freight movements. Understanding the conditions under which magnification and/or offset occurs is vital.

There is no lack of information on "well-to-tank" emissions of transport fuels. In our WBCSD report, we took special pains to show both "well-to-tank" and "tank-to-wheels" carbon emissions for a wide range of transport vehicle types and fuel types. Our spreadsheet model was designed to permit us (and any other user, since we made it available on the web) to analyze these two components either separately or together. In the WBCSD report itself, we included charts showing the relative importance of each component. We also presented the results of analyses showing the relative impact on transport-related carbon emissions of various changes in vehicle technologies and fuels. The analyses we presented were for the entire world, but the model is set up to permit similar analyses for individual regions, including North America.

I strongly urge that Chapter 7 be modified to incorporate "well-to-tank" emissions and to discuss in detail the tradeoffs between "well-to-tank" and "tank-to-wheels" emissions implied by a number of potential transport-related actions. Otherwise, the chapter will produce a distorted and incomplete view of transport-related carbon emissions.

Lesser concern: Inappropriate comparison of fuel economy "drivers" for freight versus passenger modes

In the section titled "Trends and Drivers" (pp. 7-5 to 7-7), the impression is created that the absence of fuel economy standards applied to freight trucks is responsible for the fact that emissions from freight have grown faster than emissions from passenger transport. Specifically,

on line 34 of page 7-5 and lines 1-6 of page 7-6, growth in freight and passenger transport energy use for the US and Canada are compared. The assertion is made that "Fuel economy standards in both countries were effective in restraining the growth of passenger car and light-truck energy use." The statement is then made that freight energy use increased faster than passenger car energy use. From this, the reader may draw the implication that had fuel economy standards been applied to trucks, the rate of increase in freight energy use and emissions might have been considerably lower. I know of no information to support such an impression. In the absence of fuel economy standards, fuel consumption per mile by medium and large trucks has declined significantly. And the energy efficiency of air transport has also improved significantly without standards.

The factor driving the improvements in both freight and air transport is the value of reducing fuel consumption. Fuel costs are such a large percentage of the total operating cost of both modes that fuel economy is a very important feature. In contrast, fuel costs for light-duty vehicles are a relatively small share of total vehicle operating costs.

Lesser concern: Comparison of "top down" and "bottom up" calculations of cost to reduce fuel consumption

The draft notes the sharp differences in the estimated cost of reducing fuel consumption in light-duty vehicles generated by "top down" and "bottom up" estimates. In my own experience, this difference results from contrasting assumptions employed in the two types of studies. The "top down" estimates are based on projections of historical trends of how consumers have responded to changes in vehicle and fuel prices. The "bottom up" estimates assume that the vast majority of the technological potential to reduce fuel consumption is actually devoted to doing so. However, as the annual EPA study of fuel consumption performance clearly shows, only a small fraction of the technological potential to reducing fuel consumption actually has been devoted to doing so. The vast majority has been used to improve acceleration and permit larger vehicles. Only when fuel prices "spiked" in the late 1970s did the actual improvement exceed the technological potential. This was possible because of the sharp shift in vehicle mix purchases – a shift that reduced the average weight of new passenger cars by approximately 1000 pounds with little or no change in technology. (Front-wheel drive cars came later.) The report needs to discuss this issue in a somewhat more balanced manner.

Lesser concern: Inadequate attention given to air transport

The draft gives only slight attention to the growing importance of air transport as a source of transport-related GHG emissions. (In the case of air transport, emissions in addition to carbon dioxide are significant.) Our analysis showed that, even though the fuel consumption per passenger carried in air transport is improving relatively rapidly, the growth in air transport demand is so great that air transport will become an increasingly-significant source of transport-related GHG emissions in the future. (Its present significance is understated by the authors' decision to exclude aviation bunkers from their fuel use totals.) Emissions from air transport are certain to become a growing source of political and social concern in the decades ahead. The issue should at least receive a mention.

Reviewer 13

1. Chapter 7. Transportation presents a balanced and fair synthesis of the state of knowledge and its conclusions are supported by published evidence and analysis. At the same time, as is highlighted by the author, there is a need for improved data and comprehensive and systematic assessments of mitigation potentials by each country.

2. The chapter describes hybrid vehicle, plug-in hybrid vehicle and fuel cell vehicle technology all as "highly" promising (p. 7-11, l. 6). In the Key Findings (p. 7-1, l. 26 and Executive Summary (ES-9, l. 21), hydrogen fuel cell technology is noted as an option for reducing transportation carbon emissions, but hybrid technology (grid and non-grid connected) is not noted. Hybrid technology should be highlighted in these places or the justification for highlighting hydrogen fuel cell technology above hybrid technology should be provided.

3. The Options for Management section (pp. 7-7 – 7-10) presents a balanced review of studies of the costs of CO_2 mitigation from the transportation sector. Given the likelihood of high and possibly volatile oil prices, some additional discussion on how high oil prices may affect these cost estimates would be useful.

4. P. 7-9, l. 24. Table reference 7-4 probably correctly refers to Table 7-5.

5. P. 7-9, l. 31. The numbered list is missing point (4).

6. P. 7-1, l. 25. The term "carbon fuels" should be replaced with "carbon-based fuels."

7. P. 7-3, l. 32. The text notes that "[t]his pattern of energy use has persisted for more than half a century" and refers readers to Figure 7-1. Figure 7-1 shows the regional breakdown of transportation energy use since 1990. Has there been a shift in the Figure numbering?

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 8

Reviewer 14

Are the scope and intent of the synthesis and assessment product clearly described in the report? Yes.

Are all aspects of this charge fully addressed? No. Report should provide quantitative estimates of carbon sequestration in products and wastes (i.e., on page 8-10). EPA (2005) is an appropriate source.
Do the authors go beyond this charge or their expertise? No.

Are the conclusions and recommendations adequately supported by evidence, analysis, and argument?
Not in all cases.

- **Report should provide much more documentation to support the author's estimates of potential emission reductions and costs shown in Table 8-3.**

- **Author's estimate that pulp and paper emissions could be reduced 40% for less than $25 per ton CO2 is unrealistically low.**

- **Report should make clear that several of the studies cited as support for estimates of potential emission reductions were focused on "technical potential" without regard to economic and structural limitations on emission control options. Estimates of potential reductions based on "technical potential" may be too high.**

Are uncertainties or incompleteness in the evidence explicitly recognized? No – see previous comment.

Are the data and analyses handled competently? Are statistical methods applied appropriately? In some instances, no.

- The report does not consistently distinguish biomass carbon from fossil carbon. For example, biomass and fossil carbon are combined in the Figures 8.2, 8-A1, 8-A2, and 8-A3.

- It appears calculation errors were made in producing cost numbers in lines 7-13 on page 8-12. Cost per increment CO2 should be less than (not greater than) equivalent cost per increment C.

- Page 8-12 includes the statement that "as the cost of carbon increases, one can always obtain greater reductions, but the return on these expenditures becomes marginal or insignificant." The basis for this statement should be explained.

- The economics of industrial emission reductions is a complex subject that cannot be explored in depth in this report. Perhaps the author should eliminate the sector-level analysis (Table 8-3) and instead provide a broader overview of the relevant literature including general factors affecting the feasibility and cost of reductions. The concept of "capital investment cycles" should be mentioned as an important factor that should be considered when assessing emission reduction options.

Are the report's exposition and organization effective? Is the title appropriate? Yes
Is the report fair and appropriately balanced? Yes
Is the report's tone impartial and devoid of special pleading? Yes

Are any of the report's findings based on value judgments or the collective opinions of the authors? No.

Does the executive summary concisely and accurately describe the key findings and recommendations? Yes
Is it consistent with the other sections of the report? Yes
What other significant improvements, if any, might be made in the report?

- Page 8-6 includes the statement that "These plants could be considered carbon neutral … etc." This statement should be rewritten to make it clear that the concept of carbon neutrality applies to biomass fuels and not necessarily to a facility that uses biomass fuel. For example, the statement might be rewritten as follows: "Biomass fuels are considered carbon neutral because return of the biomass carbon to the atmosphere completes a cycle that began with carbon uptake from the atmosphere by vegetation"
- The footnote on page 8-6 should also be revised to indicate that carbon neutrality applies to biomass fuel and not necessarily to an industry that uses biomass fuel.
- On page 8-9, accuracy of third sentence could be improved by inserting the word "often" as follows: "For example, recycling materials *often* reduces demands in processing…."
- On page 8-11, accuracy of second complete sentence could be improved by inserting the word "sometimes" as follows: "Their combustion greatly alleviates the net contribution to GHG emissions and *sometimes* provides power or steam… etc."
- Footnote on page 8-11 may be incorrect. IPCC 3rd Assessment Report (WG1, Sec. 6.12.3) says "….the climate forcing caused by CO2 produced from the oxidation of CH4 is not included in…. GWP estimates."

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 9

Reviewer 15

Are the scope and intent of the synthesis and assessment product clearly described in the report? Are all aspects of this charge fully addressed? Do the authors go beyond this charge or their expertise? YES/YES/YES
• Are the conclusions and recommendations adequately supported by evidence, analysis, and argument? YES
• Are uncertainties or incompleteness in the evidence explicitly recognized? YES
• Are the data and analyses handled competently? Are statistical methods applied appropriately? YES/ YES
• Are the report's exposition and organization effective? Is the title appropriate? YES/ YES
• Is the report fair and appropriately balanced? YES
• Is the report's tone impartial and devoid of special pleading? YES
• Are any of the report's findings based on value judgments or the collective opinions of the authors? If so, is this acknowledged, and are scientifically defensible reasons given for reaching those judgments? NO
• Does the executive summary concisely and accurately describe the key findings and recommendations? Is it consistent with the other sections of the report? YES/YES
• What other significant improvements, if any, might be made in the report? SEE COMMENTS BELOW

Page	Line(s)	Comment
9-1	28	"…secondary to reducing building costs." It's not clear what this means. If it is an assertion that systematic pursuit of cost reduction (e.g., over the previous building built or the previous month's expenses paid) is a primary motive/activity in the buildings sector, I'd like to see some evidence or reference to previous work.
9-2	17	Footnote 5 requires a better reference.
	19	"…large area available for siting…" the amount of floor space doesn't convert very directly to roof area for solar, e.g., on multi-story buildings
9-3	1-2	We don't have enough information to understand what these "fluxes" are.
9-5	10	Define is this a large number? "Mt";
	13-15	The California total includes a considerable amount of energy used for agricultural pumping. The following sentence would seem to reinforce that, since 94% of total US water use seems to be going somewhere besides homes and businesses. This needs to be clarified in the text.

	19-22	A very awkward sentence.
9-6	1-2	Should provide some #s to support this.
	6-7	If "…wealthier people live in larger households…" means that family size increases with income, I'd like to see a reference to those data. There is a modest correlation in the US Census data between household size and income, but it is very modest and the causal direction likely goes the other way (more people = more money). If the statement is supposed to mean that income is correlated with dwelling size, that may be supportable, but the relationship is not linear and is complex. In either case, reference to data is needed.
9-7	15	"…will likely include one or more…" I think the author intends to say "more than one." If not, the sentence doesn't make much sense. If the list is exhaustive, then, by definition, any effective approach will include at least one of its elements.
	18	Definition of "ESCO" needed.
9-9	17	Not clear what "including energy demand and supply" means.
	26	What is a "roadmap" that will need to be updated?
		Overall Assessment: A useful review that could benefit from some editorial work and tightened references.

		Conclusion: Unfortunately, the paper tiptoes around the questions of (1) the sources of the market failures that are apparent in this sector, (2) what can be done to address those failures, and (3) what the mostly likely avenues to success might be. I suspect that it is the nature of the authors' charge—including a quest for a scientifically objective tone and the current political climate surrounding climate change issues and Federal science policy in general—that limits the paper in this way, not a weakness in their knowledge.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 10

Reviewer 16

Overall comments:

I think that the authors contributing to this chapter have done an excellent job summarizing (in a logical, easy to read manner) how the bidirectional exchange of CO_2 and CH_4 in/out of these landscapes is being affected by climate and changing land management.

While the title is succinct, I am left to ponder whether "Arid Lands" really belongs in it? I scanned the paper a few more times after the initial read of it, and I would estimate 99% of what is written are in relation to agriculture, pastures, and grasslands. There is minimal discussion of woody encroachment, and very little attention paid directly to arid lands in the context that there is something distinctly different about C cycling on Arid Lands to warrant its separation in the title. Would it be better to add "Pastureland" (or grazing lands) into the title instead? I encourage the authors to think a bit more as to whether a better title needs to be constructed.

I wrote out the overall outline of Chapter 10, and noted that the major headings are: (1) Inventory, (2) Drivers and Trends, (3) Options for Management, and (4) Research and Development Needs. This seemed appropriate, although within (3) above, I thought that the subheadings "Economics and Policy Assessment", and "Other Policy Considerations" might be deserving of their own separate major heading (relating to policy). However, I am guessing you are trying to adhere to a standardized outline given for constructing these report chapters so it's probably OK to leave as is. In present form, there really isn't a disruption to the flow of the chapter, so it's probably a minor point.

I particularly liked the last section (4) that highlights the urgent need for a more organized & expanded network of field monitoring sites. Halleluiah! Currently, it seems as if it's real easy to establish eddy covariance flux towers to measure short timescale fluxes (it's the attractive and very fundable thing to do if you are filling a data void for an obscure ecosystem), but why doesn't it seem to be just as easy to get an organized monitoring array of field study sites established for measuring soil C stocks (when this is something that actually tells us the integral of many years of flux measurements)? Hopefully NACP starts to change that.

Detailed comments: Most of my suggestions are in relation to clarity of presentation, and adding some additional information for completeness of the story presented.

INVENTORY

Table 10-1 caption. I think this could be worded better considering the first line starts off "Carbon pools for undisturbed native systems were derived…" and the table is showing C pools for ag/grazing lands. While I know you have a story to tell as to how you derived these values, it might be best to not start it off the way you currently have it. Maybe something like: "Current

soil C stocks are secondary quantities derived from an initial starting point of undisturbed native ecosystem C content, which were quantified using the intersection…These undisturbed ecosystem stock values were then multiplied…

P.10-3, L26-7. …manipulating species composition and growing conditions. Are you implying irrigation? It might be nice to put in parentheses the examples you are thinking of. If the manipulation of growing conditions is only in relation to irrigation, then I would just state that.

P.10-3, L29-31. I'm not entirely sure that what you state here is indeed accurate, particularly the restricted growing season length argument for croplands, and how this can reduce carbon uptake relative to that in other ecosystems. First, the C uptake of many temperate and boreal forests is occurring at nearly the same time as it is on croplands (let's say roughly April – September) in the central U.S., for example and annual productivity is currently much higher than of many natural ecosystems (see Article "Gross primary production and ecosystem respiration of irrigated maize and irrigated soybean during a growing season" by Suyker et al., 2005, Agric. For. Meteorol., 131: 180-190). Suyker et al. (2005) cite GPP value for maize (1744 g C/m2) that are larger than temperature deciduous forests (1122-1507 g C/m2) and most temperate and boreal coniferous forests (992-1570 g C/m2).

My understanding is that another contributing factor for the depleted C stocks in agricultural soils was that low cropland productivity from the mid 1800s – 1930s was replacing higher NPP ecosystems (prairies/grasslands) which had a higher proportion of their assimilated C allocated belowground (e.g., 70-80% for prairies vs. 15% for row crops); thus, this fact coupled with the burning of crop residues and tillage have led to this observed decline in soil C levels. However, now that crop productivity has increased 6-fold, thereby increasing the amount of residue available to go back, and conservation tillage is now used on a large fraction of land, these lands are now realizing their potential to become C sinks (e.g., Buyanovsky and Wagner, 1998). It would be nice to see this minor point covered somewhere in this section, and in the discussion at P.10-4, L8-10.

P.10-4, L4. What are the increased decomposition rates attributed to? Increased N inputs, and lower C:N of residue?

Figure 10-1. It appears that the data presented in Table 10-2 duplicates verbatim the pictorial presented in Figure 10-1. Thus, could the figure be deleted? If I were to have my pick as to which data presentation method to chose, it would be the table because the reader can easily extract quantities without having to guess/interpolate values from a chart. While the Figure is a nice visual display, I am not sure it is adding anything in addition to the table considering the duplication in information.

P.10-5, L.27-28. Excellent point.

DRIVERS AND TRENDS

In the first paragraph, you might want to consider mentioning the debate of how much sequestration might be expected, e.g., how much of the gap between pre-settlement levels of soil C and current values can be made up by the trends in current practices. This will at least put

things in perspective that it's not expected that we are going to be able to recover 100% of what was once lost, and that it isn't going to happen in the next decade no matter how much land management practices change to deliberately sequester C.

I am going to suggest that this section be expanded upon a bit; it's very short and it is a bit limited to a discussion of soil C stocks and how they might be affected by warmer temperatures (citing the debate in the literature currently about how decomposition and respiration might be influenced by climate change). It seems there needs to be at least a paragraph or two to balance these arguments, discussing how the uptake of C and inputs to the soils on these lands might be affected by (1) changing temperature regimes also (2) other factors – e.g., more than the brief mention of how climate could perturb productivity (P.10-7, L.13-14) at the end of the section.

Some potential discussion points that come to mind: (1) impact that warmer temperatures might have on extending the growing season length in northern locations (e.g., northern Corn Belt, southern Canada, allowing earlier planting) and how this would likely help to increase plant productivity and C inputs; (2) However, warmer temperatures may actually decrease yields and productivity in southern regions that aren't already temperature limited as the longest season hybrids might actually mature more quickly (e.g., progress through their complete phenological phases), and thereby decrease the amount of APAR and the time the plant has to accumulate biomass (you might want to refer to the Lobell and Asner, 2003 paper in *Science* on trends in yields influenced by management and climate); (3) Continued genetic improvements to crops and an increase in nitrogen use efficiency will allow for yields and residue to gradually edge upward, although we might be well-entrenched in the law of diminishing returns as it is getting more and more difficult to increase yields each year. Some additional search of the literature is probably necessary here.

The authors might have better ideas on how to fill this section out to present both sides of the story, particularly how temperature perturbations can lead to a very complex net result because increased CO2 efflux might be balanced by more C inputs. You will also have to integrate more discussion here with what is already stated in more general terms on P.10-7, in lines 25-27.

OPTIONS FOR MANAGEMENT

Figure 10-2. You might want to add to the caption that the default soil C stocks refer to a value of 1.0 – the dotted line – in Figure 10-2. While this might sound a bit ridiculous, I wouldn't take any chances on assuming that everyone is going to know that the dotted line is referring to the conventionally tilled, medium-input cultivated land and/or moderately grazed…

You also have a typo for the "temperate wet" in the legend for Figure 10-2.

P10-8, L.19. Possibly add as a concluding statement, "But, these obviously come at a cost to the overall net C budget, particularly fertilizer usage and irrigation, because they require fossil fuels in their production and implementation." (or something to this effect).

P10-9, L.18. I would think that keeping these storage tanks "cool" would require some sort of energy demand during warm weather, potentially defeating the purpose in some capacity? Other

ideas on how to keep them cool without using additional energy? Is this offset worth mentioning here?

P.10, L24-27. This sounds like a very important point to be made, but I am not sure if I completely understand the reasoning why this would be the case? Is it worthwhile to elaborate a bit more on this point? You are basically saying that the management improvements that can be made in a farm operation that is already ongoing (and is trying to maximize profitability) can more effectively lead to cheaper sequestration costs than a piece of land that is specifically managed deliberately to sequester C? Does the same hold true for a farm that still has crop/livestock as the major income source, but has 10-20 ha enrolled in CRP? Where would this type of model fall in cost to sequester? This was just a very intriguing statement and might be more deserving of follow-up (even if it's just a few more details).

P.11, L1-8. You lost me here…My interpretation is that the "price required as an incentive for the mitigation activity" is how much would be required to pay all landowners to ensure their participation, or get some percentage of landowners to participate? Are there some other details such as how many participants and how much land would be devoted based on the subsidy offered for participation? Is doesn't appear to be a linear relationship. The bullet point you also make in the "Key Findings" in relation to this idea also doesn't stand alone as well as the other points made. I would encourage you to either reword or add more information so it is clearer.

REFERENCES

P10-13, L.15. Spelling, Ottawa.

P10-13, L.31. Typo – I don't think you want "Cynthia" in there.

Text boxes all look OK.

Reviewer 17

General Comments:

This chapter is an adequate review of the potential C sequestration in agricultural lands, grasslands, shrublands, and arid lands for the most part, but I believe it misses a critical issue related to the close ties of C and N in soils. Nearly all N in soils is tied up in organic matter, and it is not possible to add C to soils without adding N – unless one throws the C:N ratio way out of whack, potentially causing N deficiencies, lowered primary production, and therefore lower ecosystem C sequestration. Maybe N is not such a big issue in agricultural soils in that it is added routinely, but for grasslands, shrublands and even arid lands it is highly relevant and needs to be duly considered in this document.

Secondly, the role of fire is completely missing in the discussion of grasslands, shrublands and arid lands. Fire is a major issue in these ecosystems, it has an obvious immediate and also a long-term effect on C sequestration, and it needs to be included. A specific point in this regard that appears in the Executive Summary (page ES-7, pages 16-22) and as a key finding (page 10-1,

lines 27-28) is the woody encroachment of grasslands – in the Great Basin, at least, this is widely viewed as a negative development and current management practices are aimed at reversing it, potentially taking away this uncertain C sink. I do not mean to argue against this management objective, but do argue that it needs to be taken into account before this C can be "counted".

Specific Comments:

Key Finding number 3 (page 10-1, lines 27-28 and also page 10-4, lines 3-5: The woody encroachment of Pinyon-Jumiper to grazing lands in the Great Basin is seen as a decidedly negative thing by nearly everyone, and efforts are now underway to convert this back to grazing land with prescribed fire. This should be taken into account when the authors begin to tally the benefits of C sequestration in this ecosystem.

p. 10-4, lines 8-21: Since the range of soil C:N ratios for these systems is generally known, it would be an easy thing to calculate how much N it would take to achieve these levels of C sequestration in soils and to further assess whether that much N is available from atmospheric deposition, fertilizer, and other sources. You cannot store C in soils without storing N as well.

p. 10-8, lines 21-33: What about fossil fuel offsets from growing crops for ethanol production? Should that kind of analyses not be included here?

p. 10-11, lines 26-29: See comment about N needed for soil C sequestration above.

p. 10-12, lines 31-34: See the comment about PJ encroachment on grasslands in the Great Basin above. This needs to be taken into account. Management policies now aim at reducing PJ and going back to grazing lands.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 11

Reviewer 14

Are the scope and intent of the synthesis and assessment product clearly described in the report? Yes

Are all aspects of this charge fully addressed? No.

- The report deals extensively with two topics: (a) forest carbon stocks and fluxes at three scales: continental, national and forest stands, and (b) ecological factors affecting forest carbon stocks and fluxes at the stand scale.

- Social and economic factors affecting forest carbon sequestration at all scales are mentioned occasionally (e.g., lines 1-6 on page 11-10) but are not considered in sufficient detail. This is surprising given that the first sentence of the section titled "Effects of Climate and Atmospheric Chemistry" acknowledges that "the combined effects of climate and atmospheric chemistry changes on carbon sequestration are likely to be significantly smaller than the effects of land management and land use change" (p. 11-7). Highly relevant topics that are not adequately addressed include government policies, markets for forest products, technological innovation in forest management and wood processing, and trends in investment in the forest sector.

- Landscape and regional scales of analysis are all but ignored. This is unfortunate because social, economic, and ecological factors affecting carbon sequestration have important effects at these scales.

- The report recognizes carbon sequestration in wood products in several places, but virtually ignores other potential contributions of active forest management to reducing greenhouse gases including (a) production of renewable biomass energy, and (b) production of renewable materials that have lower life-cycle emissions of greenhouse gases than non-renewable alternatives.

Do the authors go beyond this charge or their expertise? No. However, the authors fail to address adequately several important aspects of their charge.

Are the conclusions and recommendations adequately supported by evidence, analysis, and argument? **In some cases, no.**

- **On page 11-4, it is stated that "Large-scale estimates of ecosystem carbon fluxes can only be explained by a more detailed examination of the dynamics of individual forest stands that have unique combinations of disturbance history, management intensity, vegetation, and site characteristics." This statement has important implications for research priorities, but is unsupported by evidence, analysis, or argument. The statement is probably false if "large-scale" is taken to include regional and national scales. To my knowledge, no one has**

demonstrated the feasibility of scaling-up observations of stand dynamics to explain estimates of carbon fluxes at regional and national scales. Methodologies that integrate information from several scales are more likely to be successful.

- **Page 11-8 includes the statement that "… a long-term increase in impacts of disturbance is likely in the future, with associated losses of forest carbon stocks." This statement is unsupported by evidence, analysis, or argument.**

- **Page 11-12 includes the statement that "Effective carbon management options to increase the retention time of sequestered carbon require a thorough understanding of current carbon stock sizes and flux rates in boreal, temperate, and tropical forest ecosystems in North America." Implicit in this statement is the assertion that "increasing the retention time of sequestered carbon" is a promising strategy that merits special consideration. This assertion is unsupported by evidence, analysis, or argument.**

- **Page 11-13 includes the statement that "With the exception of land use change (afforestation and deforestation), there is very little information about how forest management affects various carbon pools…" This is obviously incorrect. There are many useful papers and books on effects of forest management on various carbon pools.**

- **Page 11-13 includes the statement that "Few decision-support tools are available…." Taken literally, this statement is obviously incorrect. No doubt the authors were intending to refer to some particular kind of decision for which tools are lacking. The section on decision support tools is weak and needs to be reworked.**

Are uncertainties or incompleteness in the evidence explicitly recognized? Yes

Are the data and analyses handled competently? Are statistical methods applied appropriately? Not applicable.

Are the report's exposition and organization effective? Is the title appropriate?

- Title is appropriate.

- Exposition and organization need to be revised to address problem discussed above that report focuses too much on stand-level ecology and not enough on social, economic, and ecological factors affecting carbon sequestration at landscape and regional scales.

Is the report fair and appropriately balanced? Is the report's tone impartial and devoid of special pleading? For the most part, but not entirely.

- The report's balance and fairness are compromised by its excessive focus on stand-level ecology and by its insufficient attention to potential contributions of active forest management to reducing greenhouse gases including (a) production of renewable biomass energy, and (b) production of renewable materials that have lower life-cycle emissions of greenhouse gases than non-renewable alternatives.
- The report's lack of balance is exacerbated by Appendices 11A and 11B. The appendices give special attention to selected research approaches, results, and carbon accounting concepts but do not add substantial value to the overall report.
  - The authors should consider condensing the material in the appendices and integrating it into the main text. For example, information on ecosystem carbon fluxes from eddy covariance and ground-based measurements (Appendix 11A) could

be summarized in a single table and integrated into the main text section on "Carbon Stocks and Fluxes."

- Page 11-23 includes the statement that "Mature forests can be substantial sinks for atmospheric carbon." This statement is presented without appropriate context and seems designed to promote forest preservation. A balanced presentation would also mention the potential for mature forests to be substantial sources of atmospheric carbon (e.g., high potential for wildfire in mature forests experiencing cohort senescence).

- The fourth bullet on page 11-26 offers the opinion that "replacement of fossil fuel by biomass fuel can be counted as an emissions offset, if residual or manufacturing "waste" would otherwise be lost via decomposition or other processes." This brief commentary on a single aspect of carbon accounting is clearly out of context and inappropriate.

Are any of the report's findings based on value judgments or the collective opinions of the authors? See comments above regarding (a) need to broaden the scope of the report, and (b) concerns about Appendices.

Does the executive summary concisely and accurately describe the key findings and recommendations? Is it consistent with the other sections of the report? Yes
What other significant improvements, if any, might be made in the report? None.

Reviewer 18

Thanks for the opportunity to review Chapter 11: North American Forest of the CCSP2.2 Report.

The authors have done a very good job in synthezing many diverse sources of data into a very coherent report on the influence of North American forests on the global carbon cycle. They have expressed their estimates of forest carbon pools, fluxes and balance in the context of actual tons of carbon and relative to the annual North American emissions from fossil fuel.
The tables and charts the authors have provided will serve as a baseline for future assessments to be compared against. The uncertainities associated with their stated estimates are given.

The major factors that drives changes in carbon sequestration, fluxes and pools were identified and discussed for the three countries that comprise North America. I think a table showing the relative ranking of these drivers over time, past (pre 1970), present (1970 to present) and future (next three decades) for each of the three countries would be useful. It would help to emphasize where there is or is not consistency over time and between countries in the factors that have made major influences on each countries carbon cycle. For example: for the USA:

USA Past	**USA-Present**	**USA-Future**
Land use (For. To Ag.)	Land use (Ag –For-Sub)	Land Use (For/Ag-Sub)
Disturbance (Fire, Pest, etc)	For. Mgmt/Use Intensity	For. Mgmt/Use Intensity
For Mgmt/Use Intensity	Disturbance(Pest, wind, fire)	CO2 –global change

CO2-global change	CO2-global change	Disturbance(Pest, weather, fire, etc)

Canada and Mexico rankings would be very different. I do not know if the above rankings are correct, just an example. The authors are in the best position to do the rankings and give uncertainties. These rankings will help us to focus on the main factors for each country that we need to influence through policy or science.

For the important role that forests play in the North American Carbon balance, I think the introduction section should be beefed up. State the extent of forests relative to the total N.A. land area, its contribution to offsetting annual fossil fuel emissions of NA and the globe. Give the three fundamental ways that forests influence the carbon cycle-balance (1) CO2 sequestration, storage and emissions (2) a substitute fuel for fossil fuel (3) product substitution for high energy (fossil fuel) cost products. The authors should identify the major federal research or incentive programs that are in-place to advance our understanding of or to enhance the role of North American forests for mitigating carbon cycle imbalances. A table showing the programs, what each is expected to contribute (estimates of stand pools, changes in fluxes for the various major forest types, or mitigating/improving the role of forest for storing, offsetting fossil fuel emissions, etc) and which forest-types are being addressed and which are not would be useful. I think we may see we are heavy on the Science (understanding) side and light on the enhancing program side but I can't tell from this chapter. The research status and needs are one of the mandates of this report.

From the authors results, it is clear that changes in land use, the extent that forest are used to offset fossil fuel and the management intensity of N.A. forest are the main factors that can be influenced through future research and policy changes to greatly enhance the role that NA forests play in solving the C imbalance issue. When only a small percent of the energy stored in NA forest trees (?? 1%) is being converted to offset fossil fuel, only 1% of the trees in managed forest are being harvested and most of the forest land are in the "Others" category (which is historically under-managed and under-utilized) this should send a strong signal that only a minor fraction of the potential for NA forests to affect the NA carbon balance is being realized. A statement of the potential role versus the estimated actual would be useful.

Gaps should include:

- How do we get significantly more stored forest energy converted into an energy form that offsets fossil fuel use? We are using very little of the "renewable energy mine" we have and the potential to add to or regrow this energy source is large.
- What are the most efficient ways (policies, programs) to get more acres into forests or to enhance the C sequestration rates of existing forest lands in each country?
- How can we have better inventories of C pools by forest types and improve our ability to detect significant changes that are taking place in these pools?
- What Forest-types do we need better flux data for? Can we obtain this data smarter than we have in the past? For example, do we need continuous monitoring eddy-covariance sites for only a few forest types or do we know the key stand development stages that influence stand carbon fluxes and the key parameters we need to measure so we could take only periodic data and scale (model) carbon flux estimates over time and forest stand

development? This would permit getting flux data estimates for more forest types and conditions.

Thanks again for the opportunity to review this important chapter.

Reviewer 19

General comments

While authors did a very thorough job at collecting relevant literature, the numerous reports and studies cited in the chapter are based on inconsistent definitional frameworks, categories, purposes, and approaches. Upon reading the entire chapter, my conclusion was that the data and knowledge currently available do not allow to make comprehensive statements on forest carbon dynamics and our ability to manipulate them, with any confidence, at the north American scale. The sections on data and knowledge gaps reinforced this conclusion. The wording of 'key findings' should better reflect this uncertainty.

Clarify at the outset the purpose of compiling on the one hand, the findings of scientific studies on the complex factors driving carbon fluxes in and out of forests, their annual variability, long-term effect, and relative importance in different landscapes and social settings, and on the other hand national-scale estimates of C stocks and C stock changes (in the chapter's first sections), which often do not incorporate this scientific knowledge. Perhaps the chapter should point out with greater clarity the missing links between the two information types. Indeed, the complexity of the issues warrants a finer analysis. A valuable goal for the chapter, rather than listing options, gaps and needs in a semi-quantitative fashion, would be to attempt to identify and prioritize the key questions we should address to determine the potential for manipulating forest C dynamics to reduce atmospheric loading of CO2. The challenge is to move from site-specific studies and qualitative statements to large-scale, quantitative assessments.

One also wonders if an ecosystem-based approach (tropical, temperate, boreal forests, with various levels of management intensity) should not be considered. Given the diversity of forests and forest management practices across the continent, the current, country-based assessment is too general.

Specific comments

P 11-2, line 22: forest products (FP) are not a carbon sink, since, as opposed to vegetation, they do not remove carbon from the atmosphere. Use clearer terminology, such as "C uptake" (by trees) and "C storage" (in forest ecosystems and FP). Authors should clarify the meaning of the data used to represent C stored in forest products (FP) in this chapter, since different estimation methodologies drastically affect these estimates (see also comment below on p 11-3).

P 11-3 line 29 and following :
For Canada, use Environment Canada's 2006 submission to the UNFCCC, as opposed to the 2005 version. The 2006 report contains vastly improved estimates. This reviewer can provide the updated data. Notably, Canada's managed forests were variously a source or a sink in the 1990-2004 period; in this context, the use of a single, undated and unexplained figure in Table 11-3 of the SOCCR is misleading. Annex 3 (section A3.5.7) of Environment Canada's 2006 GHG Inventory Report indicates that in 2004 off-site emissions from decaying FP are estimated

between 91 and 135 Mt CO2, depending on approaches. Again, using a single figure without further explanation lacks transparency.

P 11-4 line 26: remove "and wood products", since by and large the factors listed affected forest C dynamics, but not those of wood products.

P 11-6 lines 11-17: update with Environment Canada's 2006 GHG Inventory Report. While forest and other wooded lands occupy 402 Mha, forests alone cover 310 Mha, and managed forests 255 Mha or 83% of all forests. On page 11-12, line 19, change also the 47% figure to 17%. Managed forests include private and public forests potentially subject to harvesting, and forests actively protected from fires.

p. 11-6 lines 21-25: there is a need to reconcile the statement about the relative importance of harvesting and natural causes in tree death, with the statement on p 11-8 lines 10-11. The two sentences are somewhat inconsistent. On what kind of evidence relies the statement of lines 21-25 if evidence of the impact of disturbances is missing?

The section 'Options for management' needs further work. Remove p. 11-9, lines 26-31: this rather assertive statement reads like a conclusion, and is not warranted by the few examples provided. More examples are needed similar to the two Canadian examples of how forest management strategies could affect forest carbon dynamics in northern landscapes. On p. 11-10, lines 7-11 fail to mention the combination of strategies that could potentially yield such a significant increase in C sequestration by US forests. I doubt that the economic assessement of p11-11, lines 3 to 11 applies to the entire north American continent, or any of its component countries: provide context to these figures. What are the conclusions of this section, given the caveats of p 11-13?

Table 11-1: are there 'polar' forests?

Table 11-3: Use figures in Environment Canada (2006) Canada's Greenhouse Gas Inventory 1990-2004. The use of a single figure is misleading, since during the 1990-2004 period the annual GHG budgets of Canada's managed forests vary between a sink of 186 Mt CO2e and a source of 177 Mt CO2e.

Reviewer 7

General comment – Although this chapter provides useful aggregate statistics on forest carbon by biome and country, users of the chapter would benefit greatly from an analysis of spatial estimates of forest carbon. Such an analysis would involve matching estimates based on forest inventories divided by political unit and general forest type (Birdsey and Lewis 2003) with spatial estimates from remote sensing (Running et al. 2004).
Research at individual sites has combined analysis of forest inventories and remote sensing (for example, Van Tuyl et al. 2005, Turner et al. 2006). Therefore, I suggest adding a section "Spatial estimates of Forest Carbon" that would review the scientific literature and take a step towards producing a map of forest carbon across North America.

page 11-1, line 19 – Instead of “highly uncertain,” add the numerical error range to the estimate of 350 Mt C y^{-1}. From
page 11-3 line 20, the error is ± 350 Mt C y^{-1}.
page 11-1, lines 33-37 – Identify the areas of development of better estimates of potential estimates of forest carbon under different scenarios of climate change. For example, Bachelet et al. (2003) have continued to improve the skill of the dynamic global vegetation model MC1 to simulate potential CO_2 fertilization.
page 11-3, line 29 – The uncertainty of the estimates of forest carbon suggest that the text should use an appropriate precision of two significant figures. Therefore, change 109 Mt C y^{-1} to 110 Mt C y^{-1} and round other numbers throughout the report to two significant figures.
page 11-7, line 24 – Data from humid evergreen tropical forest in Costa Rica show one impact of climate change on forest growth, namely, reduction of annual growth due to increased respiration at night (Clark et al. 2003).

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Reviewer 17

General Comment:
This document seems a bit thin and out of date in places as per the situation in the US. See specific comments below.

Specific Comments:

Page 11-3, lines 1-15: The units in this section are completely inconsistent with one another. In Canada, they are given on a per ha basis, whereas for the US and Mexico they are given as totals. How can we compare these?

Page 11-4, lines 1-7: This section need to include the critical issue related to the close ties of C and N in soils. Nearly all N in soils is tied up in organic matter, and it is not possible to add C to soils without adding N – unless one throws the C:N ratio way out of whack, potentially causing N deficiencies, lowered primary production, and therefore lower ecosystem C sequestration.

Page 11-7, lines 1-24: I am surprise that the recent studies on the effects of elevated CO2 at the Duke site by Finzi et al are not included here. The Duke study is the longest-term study in a forest ecosystem

Page 11-8, lines 15-17: Surely there are later figures for fires in the US than a 1998 reference. The US section here seems a lot less comprehensive than that for Canada. More homework should probably be done.

Reviewer 34

I have also quickly reviewed chapter 11, Agricultural lands, grasslands, shrublands, and arid lands. It is factual and covers the main science topics although also rather inward looking (i.e. not taking into account what is happening elsewhere) I find this less of a problem for this chapter, although the document title implies it will address "implications for global carbon cycle".

In the Economics and policy assessment section, you may wish to consider discussion of options such as payments for environmental services, costs to the agriculture sector in adapting to carbon imbalances, initiatives and opportunities in the large multi-national food corporations to deal with carbon-related issues.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 12

Reviewer 20

General Comments

The manuscript follows the scope and intent of the overall synthesis. The conclusions and recommendations are adequately supported by evidence provided. However, the section on uncertainties and gaps in knowledge needs amplification (see suggestions below). The methods are applied appropriately. In general the exposition and organization of the report are effective, except for the use of headings (see comment below). The report is fair and appropriately balanced. The tone of the report is impartial. The executive summary is concise and accurately reflects the key findings and recommendations.

Detailed Comments

The title is incomplete and should mention carbon stocks as well as cycles.

The authors emphasize that Cryosols contain 61% of the SOC in all soils of North America (p. 12-1, line 18; p. 12-2, line 12; p. 12-6, line 21). I find this hard to believe. The value is inconsistent with other data including those of Mr. Tarnocai, the lead author. In the Tarnocai (1998) publication, 39% of the soil C mass in Canadian soils occurs in Cryosols. Using the value of 417 Gt for the North American soil C mass (Ch. 3 of the CCSP report), the percentage of soil C mass attributed to Cryosols would be 51%. Perhaps, the authors should provide a table summarizing average SOC and soil C mass for each eco-region or soil order of North America. In any case, more information should be provided to justify the 61% value.

The headings are confusing and make the chapter somewhat disjointed. I suggest that the primary heading on p. 12-3, lines 17-18 be "CARBON STOCKS" so as to be consistent with those that follow, e.g., CARBON FLUXES (p. 12-6), etc. The heading "BELOW-GROUND CARBON STOCKS" on p. 12-6 is confusing, in that all of the C stocks reported in the manuscript are belowground, and should be eliminated.

The authors may wish to include a diagram showing the three-component conceptual model of Cryosols, in which the transition zone is recognized as a layer intermediate between the seasonally thawed active layer above and the stable permafrost below (Bockheim and Hinkel, 2005, *Arctic*, vol. 58, pp. 406-417). The transient zone episodically thaws over decadal to centennial periods and is important relative to the vulnerability of SOC in permafrost as a source of CO_2 to the atmosphere.

The authors state that little is known about C fluxes in permafrost-affected soils and have not reviewed any of the literature pertaining to SOC fractions and their vulnerability to loss during climate warming. A number of papers report chemical, physical, and radionuclide fractions of SOC and could be drawn upon to make judgments regarding vulnerability of SOC decomposition and CO_2 evolution.

The section on data gaps and uncertainties is incomplete. The authors could mention the lack of information on SOC below 100 cm, the possible influence of arctic warming on cryoturbation, and other data gaps.

The figures and tables generally are acceptable. However, as mentioned previously there is need of a table giving mean SOC and soil C mass for eco-regions or soil orders of North America. Table 12-5 should provide standard deviations to accompany mean values. The drawings below figures 12-3 and 12-4 are rather crude and could be done more professionally.

Reviewer 21

Here are my comments on the CCSP report. Largely it is very solid. I have only modest comments. The one issue that I disagree with the authors on is that they downplay the importance of roots and really consider that aboveground litter lands on the soil surface and that DOC leaches down. In some boreal systems more than 75% of C fixed in the ecosystem goes directly into the root systems. In peat soils of the Arctic, most of the "soil" is just dead roots. I think the authors should revise their consideration of how SOM forms to give more credence to the importance of roots and "direct injection" of organic matter. I do not need to remain anonymous on this.

Page 11-3: DOM usually means "dissolved organic matter", rather than "dead organic matter."

The main problem with the chapter is how they discuss sources of soil organic matter. It is consistently presented as a process where litter lands on the soil surface and then must be transported into the soil. In fact, in high latitude systems the root:shoot ratio is much greater than 1. That means that most of the C entering the decomposition system is injected into the soil via root growth. Roots may well provide the bulk of C that becomes soil organic matter.

The other issue is the role of wetlands in producing CH4. Currently high latitude wetlands are a substantial CH4 source. As wetlands warm and dry, they will lose C and supply CO2 to the atmosphere. However, they will also decrease the amount of CH4 that they produce. This is suggested but not developed.

Page:
12-4 1st paragraph:
No, No, No- Most C is not deposited on the soil surface. Most is injected into the soil in the form of roots. DOC movement is likely a relatively modest source of DOC into soils in comparison. Residence time of root C is short? I don't think so! Most of the C that turns into SOM may well start as roots.
12-4, line 22-23: Even in peat soils, roots are the main part.
12-5: Line 16: never say "no decomposition occurs"
12-7: Lines 3-5: Again, you down play roots.
12-10: Line 6-11: But as they drain, the CH4 production mentioned in the previous paragraph will be reduced. Thus, the C balance will shift, but the overall climate impact may not be as clear since CH4 is a much stronger greenhouse gas than CO2.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 13

Reviewer 22

The authors of Chapter 13 – Wetlands should be commended for the effort they put into compiling and analyzing data they obtained from disparate sources. The result of these efforts is a comprehensive document that provides the reader with a better understanding of the important role wetlands can and do play as sources and sinks of greenhouse gases.

There are important limitations in the data available to estimate the type and amount of wetland coverage in the North American landscape. Some of these limitations result from inadequate effort applied to conducting wetland inventories on a sufficiently large scale and at appropriate return frequencies. Other limitations relate to the incomplete level of standardization among the definitions of some wetland types. For instance, bogs are easily identified and therefore peat inventories of these ecosystems are better quantified. Other wetland types can be classified differently by different observers, like swamps or forested wetlands, which could be classified as "freshwater mineral soil (FWMS)" wetlands or just "forest". These discrepancies are compounded when remotely sensed data are used to classify a landscape and ground truthing effort is insufficient so that the size of the wetland carbon pool is uncertain. The authors are aware of these limitations and included cautionary language where appropriate.

Much of the interest in studying wetlands revolves around their potential as a sink for atmospheric carbon. Some studies are geared to understanding carbon sinks as their main objective, but many studies that concentrate on ecology, hydrology or biogeochemistry have the sink function of wetlands as an important "background" concern. The authors have done a good job reviewing the sink/source potential of many types of wetlands and, within the limitations mentioned above, present useful estimates on a national and continental basis and compare them to global estimates.

The authors are probably not aware of a recently published paper by Euliss *et al.* (2006). This paper concerns a specific type of wetland, the prairie pothole wetlands, which are widely distributed on the Great Plains of north central US and southwestern Canada. These wetlands are an important component of the predominantly agricultural landscape of the plains, and many of them are directly or indirectly impacted by agricultural land management practices. These wetlands provide important ecological services and in particular they are critical habitat for migratory waterfowl. The Euliss *et al.* (2006) paper suggests that restoration of prairie pothole wetlands – conversion from current cropped status to grassland – could result in 378 Tg of carbon sequestration over a 10-year period. In Chapter 13 sequestration is reported in Mt per year, so for comparison restoration of prairie pothole wetlands might sequester 38 Mt per year, which is comparable to the total sequestration of current FWMS wetlands for all of North America (34 Mt per year – Table 13A-2). Prairie pothole wetlands would be classified as FWMS according to the descriptions provided by Euliss *et al.* (2006). It might be worth the effort to determine how similar the inventory and flux estimates are between the Euliss *et al.*

(2006) paper and the current chapter. If a significant proportion of the prairie pothole wetlands were not captured in the current tabulations, then this demonstrates the veracity of the > 100% error estimates for C-sequestration.

It is important that the authors addressed emissions of both CO_2 and CH_4 when considering carbon balances and when estimating the probable magnitude of net sequestration. Future compilations should be able to address the emissions of N_2O, particularly if agriculturally impacted wetlands are included. This greenhouse gas has been hardly studied outside of the agricultural setting (see Chapter 10) where it is known to be very important. Chapter 10 does not consider wetlands in the agricultural landscape, so these should be considered in future compilations under wetlands with cross-referencing to the future agricultural chapter.

Overall the wetlands chapter represents fairly the current state of knowledge of carbon cycling in North American wetlands allowing for an understanding of where they fit in the total carbon budget and greenhouse gas picture

Reference:

Euliss, N.A. Jr., R.A. Gleason, A. Olness, R.L. McDougal, H.R. Murkin, R.D. Robarts, R.A. Bourbonniere and B.G. Warner (2006). North American prairie wetlands are important nonforested land-based carbon storage sites. *Science of the Total Environment*, **361**: 179 –188.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
August 2006

Chapter 14

Reviewer 35

The key findings at the beginning of this chapter work well to frame the article. Pataki et al. do a good job keeping to the form of this outline, and the piece is for the most part clearly written. Nonetheless areas for improvement are also apparent from the overview of findings. While improvements can be made, the report is useful, appropriately balanced, and informative within the explicitly recognized limits of existing data and analyses.

In their first finding, Pataki et al. note that human settlements occupy 5% of the North American land area. They note in finding two that while there is currently insufficient information to determine the complete carbon balance of human settlements, fossil fuel emissions are likely to dominate carbon fluxes from settlements. The third finding is very specifically focused on the impact of trees. Findings 4-6 are very general and deal with the (obvious) link of urban development to fossil fuel emissions. For example it is stated: "while historical trends are not available, density and development patterns are the likely drivers of outcomes especially in residential and transportation sectors." The last two findings deal with a specific local government initiative and the statement that research is needed to improve comprehensive carbon inventories for settled areas. The major contribution of the review is the description of the limitations of current knowledge and the discussion of how to improve on the limited information now available.

Overall, the chapter's findings are at a quite high level of generalization, and where there is specificity there is little linkage to the general. Even though this is a single chapter in a book, more detail could be very helpful in several areas. First, it would be useful to survey the literature to include a few existing simple statistics that are currently used as rule of thumb measures of impact of human settlement to frame what is known and what is not known, if available. Second, if detailed information is to be provided about some impacts, it would be useful to explain why such detail is important and why it is lacking for other possibly more important phenomenon. Third, there are statements about likely impacts and likely research advances that could be better substantiated. Most importantly, it would be useful to discuss further how to overcome the problems of measuring changes in the carbon cycle due to human settlements or at least to discuss in more detail the source of the identification and measurement problems.

To expand on the first suggestion, it would be helpful for the authors to discuss in greater detail the existing ways of measuring the human impact on the carbon cycle. By now we are well aware of the general problems that arise when researchers attempt to gauge human impact on the environment. The authors do mention that population growth, household size, and urban form affect CO2 emissions, but it would helpful to discuss how best, given the current state of the art, to measure the impact of land use decisions. This is important because while income and

population growth are to a large extent givens, land use is a policy variable. If this is not now possible, specifically what knowledge do we need and how can we proceed to gather this information. For example, it would be interesting to know if researchers can measure the amount of CO2 released when one acre of forest is paved over and then turned into parking lot (using a "typical" parking lot specification) or when land is converted from agriculture to a parking lot (or the effect of house size on CO2 emissions or the cutting down trees and building homes, or the conversion of agricultural land to communities, building golf courses, regularly tilling agricultural land etc). Or what are the best estimates of impacts of examples such as the above human settlement decisions. If researchers knew these answers, estimates of these impacts could be made across the US and they could be put them into various growth models. These pieces of information would have direct policy implications for decision makers. The authors state that metabolic and footprint analyses of specific settlements are seldom done and are needed—what are the barriers to this knowledge? What do we need to know, how must we expand current research to improve on the current state of knowledge of the impact of the form and size of human settlements on the carbon cycle.

In any case, a key piece of the knowledge of future impacts will be the population build out and the form in which this takes. The authors refer to a one-time "snap-shot" of the extent of human settlements for North America as well as to the one-time measure of the urbanized area of the US. Of course we will need to know as best we can how this has changed over time and is likely to change going forward. Some discussion of how to gain this knowledge would be useful. Is it possible to have access to a moving picture which then could be modeled going forward? Social scientists are conducting work that attempts to model the places where future growth will occur in the US, which would in turn help in predicting human impacts to the carbon cycle. It would be useful to discuss further the limitations and potential of this work.

In the "Options for Management" section Pataki et al. begin by talking about a specific local government initiative. There are also state level initiatives some of which are associated with federal requirements. Some reference to these initiatives would be of interest.

Overall, the piece does a good job of pointing out potential human impacts on the carbon cycle and highlights the possibilities of quantifying various human inputs in the future, but it does not really elaborate on ways to move forward. Considering that it is a single chapter, it does mention many human impacts that do affect the carbon cycle, but it would helpful if it more systematically dealt with what is known, what is not known about several human impacts, and, most importantly, how to move forward.
The chapter does point to local governments that are voluntarily taking action to reduce carbon cycle impacts, but without the proper data to quantify the impact of taking specific steps, local government initiatives cannot be evaluated. We need to know which separate actions have the greatest impact. That way federal, state and local government can evaluate appropriate policies going forward.

Expert Peer Review Comments for CCSP 2.2, *The First State of the Carbon Cycle Report (SOCCR): The North American Carbon Budget and Implications for the Global Carbon Cycle*
July 2006

Chapter 15

Reviewer 24

This chapter presents an impressive synthesis of ocean pCO2 observations around the coast of the US, and quantifies the observed fluxes on an annual basis. In addition to the new pCO2 observations, the chapter nicely synthesizes existing information on US coastal ocean fluxes. This is scientifically very interesting, and it also very useful to develop a comprehensive picture of the US carbon budget.

Comments:

throughout the chapter, the information is not always clear which processes are active from the natural carbon cycle, and which processes have been influenced by either human activities or increasing atmospheric CO2. For example, p.15-2 lines 20-21 says that the biological pump removes atmospheric CO2. This suggests that the biological pump removed ***anthropogenic*** CO2, which I think is not the case here. Similarly page 15-5 lines 2-4 is ambiguous. I suggest that the introduction is revised to explain that many natural processes drives fluxes in and out of the ocean, that in addition the ocean responds to increasing atmospheric CO2 and to changes in the input of nutrients from land. The same problem re-appears in the section "trends and drivers", where it is not entirely clear is processes are natural or response to anthropogenic changes.

the information on processes is sparse and incomplete. In particular, it would be useful to know for each processes which direction are the CO2 fluxes expected to go if the process is enhanced in the future. Pars of this information could be integrated in the "global coastal ocean carbon fluxes" and part in the "trends and drivers" sections.

page 15-1, first bullet, what time period is this for?

page 15-1, lines 16-31, please clarify as much as possible which information relates to the natural carbon cycle and which information is due to anthropogenic influence.

page 15-5. I would have liked to have more information if the new data presented agree with the climatology of Takahashi et al (2002) in the regions where there is an overlap.

page 15-8 lines 7-10. Please revise. The flow of the information is difficult to follow.

page 15-8 line 18-19. It is not clear to me why the seasons summer+fall and winter+spring is used here. I would have thought that spring+summer and winter+fall would make more sense both physically and biologically.

page 15-8 line 24, if only summer months are available, I do not think these observations should be represented in the figure.

I would have liked to have some information on the Arctic ocean, especially because of the projected decrease in ice cover.

page 15-10, line 16. The net effect of El Nino is well known and not uncertain at all (see many publications by Feely and also your own Figure 15-5).

page 15-10, line 16-19, this statement is not supported by material in this chapter. I suggest to strengthen the section on processes to support this statement.

page 15-10, ocean Fe fertilization. I am not aware of any scientific studies that support the efficiency of Fe fertilization. All studies show that this is highly inefficient because as soon as Fe fertilization is stopped, the CO2 goes back to the atmosphere. This chapter need to say clearly that Fe fertilization is not a prospect for reducing atmospheric CO2, and not be ambivalent like it is here.

Figure 15-3 need some information on summer/winter distribution of the data (perhaps use different color).

Figure 15-3 bottom panel, need to use same scale and units as 15-1. The color scale does not allow the reader to see if the regions are sources and sinks and makes the reading of the text very difficult.

Figure 15-4, please provide units in axis label.

Figure 15-5, please provide a smooth (filtered) curve in the bottom panel if possible.

Reviewer 25

Overall, I believe that this is a clearly written, succinct and high quality summary of our state-of-knowledge of carbon cycling in coastal, lake and estuarine systems. My main significant concern is the potential under statement of potential uncertainties. This and a few minor grammatical and editorial suggestions organized by line number follow.

p.1, l-15. delete "global". Since you are making a distinction between global and coastal oceans (the title of this chapter), declaring that the global ocean takes up 1.3 - 2.3 Gt/y of anthropogenic CO_2 presupposes that little uptake can be attributed to the coastal ocean. Also, "anthropogenic" needs to be added to this statement.

p. 1, l-18. Not clear if sediments are included in estimating storage.

p. 1, l, 20-23. It seems to me that there is more uncertainty in this assessment than indicated here - see later comment.

p.1, l-29. Again, I think that there is more uncertainty as to whether North America's coastal ocean is a source of CO_2.

p. 2, l, 15 - 16. It is correct of the authors to point out that most previous studies have been limited to assessing air-sea exchange. However, the authors should also point out that adjacent to continents, significant inputs can be derived laterally from terrestrially pools. These would include freshwater inputs, groundwater inputs and coastal waters exchanged with coastal zone systems (e.g. salt marshes). One of the authors (WJC) has shown that on the Georgia shelf, exchange with the marshes supplies sufficient carbon to uncouple coastal air-sea exchange from coastal - open ocean exchange. That is, shelf waters on the Georgia shelf are both a source of CO_2 to the atmosphere AND the open ocean. Thus, in this setting, the use of air-sea exchange to assess net anthropogenic invasion is not valid and in fact is of the wrong sign. If the authors wish to dismiss these recent findings, they should provide a reason. If not, this exchange should be included which will significantly increase the uncertainty of the net exchange for North American coastal systems.

p. 3, l-1. replace "global" with "deep" since (as the authors point out in the next line) the coastal ocean is not included.

p. 3, l-6. There is also a more recent wind speed - gas exchange relationship reported by McGillis. This should be mentioned and the uncertainty in invasion reported.

p.8, l-14. The authors briefly mention high PCO_2 associated with terrestrial inputs. However, the important offshore flux here is reflected in the total CO_2 (not PCO_2) in the shelf waters all at salinities above 30.

p. 9, l-6. Again the authors focus on air-sea exchange exclusively without noting potential uncertainties associated with the boundary.

p. 9, l-25. The authors note that the air-sea flux is approximately 1% of the deep ocean (note the authors should replace "global" with "deep"), but again do not note potential lateral fluxes. Also, earlier the authors cite Ducklow and McCallister (2004). I do not believe the results of the D&M analysis but if the authors are going to cite them in one location, they should be consistent. Since D&M come up with a value that is inconsistent with the 1% coastal flux presented here, they should at least acknowledge the uncertainty.

p. 10, l-15. Again in this section, the authors completely equate ocean uptake of anthropogenic CO_2 with air-sea exchange which has been shown to not be true at ocean margins. What is true is that the importance of non-air-sea CO_2 inputs is uncertain, but preliminary extrapolations indicate that it can not be objectively ignored with the present data set.

p. 11, l-1. The importance of WJC's observations on the Georgia coast seems to be ignored in the much of the ending discussion and in suggested R&D needs. The most important point is that the margin inputs of CO_2 to the ocean are not necessarily reflected completely in the PCO_2 but one needs to also measure the total CO_2 and residence times of the coastal waters.

Reviewer 32

I have a major question on the review: The title is "Coastal Oceans, Lakes and Rivers." The discussion of the oceans is very good, as would be expected by that author group. But they say

nothing about Lakes and Rivers, other than a brief allusions to input to coastal zone. The Wetlands does a nice job of ..just wetlands, mostly northern.

The statement in Houghton "Rivers, lakes, dams, and other inland waters are mentioned in Chapter 15 as being a source of carbon, but they are claimed elsewhere to be a sink (Chapter 3). The sign of the net carbon flux attributable to erosion, transport, deposition, accumulation and decomposition is uncertain (e.g., Stallard, 1998; Lal, 23 2001; Smith /et al/., 2005)." pretty much sums up the treatment - pretty marginal.

At a minimum, I would suggest that the title of Ch 15 be changed to "Coastal Oceans," and delete the Rivers and Lakes bit.

As I remember, this was a gap pretty much identified at the kickoff meeting, a few years ago.