

From: Sarah Raper <s.raper@uea.ac.uk>
 To: tar13@meto.gov.uk
 Subject: Chapter 13 review
 Date: Fri, 23 Jul 1999 19:52:44 +0100
 Cc: mnoguer@meto.gov.uk, pvanderlinden@meto.gov.uk

COMMENTS ON CH. 13 (SCENARIOS) FROM TOM WIGLEY
 (Page and line numbers are from the May 14 zero order draft.)

Dear contributors to Ch. 13,

Here are my comments on your chapter. I think you all know me well enough that you will not be offended by my occasional bluntness. The chapter needs a lot of work (not surprisingly), but it has at least touched most of the bases. It suffers from a lack of overview perspective, making the detail hard to wade through. I was disturbed by the lack of credit given to MAGICC/SCENGEN, since this software already addresses many of the key issues that arise in scenario development.

Apologies for not proof reading this. By the time I got to the end of typing it, I'd had enough.

Page 3 (lines 86-89) : Critically, this information doesn't give a full assessment of uncertainties.

3 (110-115) : Sentence too long.

3 (117) : State 'illuminate uncertainty' earlier, since this is a primary purpose of, e.g., MAGICC/SCENGEN.

3 (118) : 'indeterminate' is far too strong.

4 (124-125) : Not clear.

4 (155) : What is 'integrated assessment'? Define and/or explain earlier.

5 (170) : Clumsy grammar.

5 (171-172) : Silly! Scenarios per se do not have ANY uncertainty associated with them, by definition. They are, however, a very (if not the most) useful tool for assessing and quantifying uncertainties. For example, a primary purpose of MAGICC/SCENGEN is to quantify uncertainties. Major text revision is needed to clarify this point.

Part of the problem here is that the boundary between scenarios and predictions/projections is indistinct (as is the distinction between predictions and projections -- this too needs to be clarified). One could argue that 'scenarios' developed using MAGICC/SCENGEN are actually better predictions of some aspects of future climate change than any O/AGCM results. Certainly, 'scenarios' based on scaling are much more than just scenarios as defined here -- they are true predictions, based on some assumed scenario (this is the correct word here!) for future emissions.

Substantial work is required to the present text to clarify these issues -- they are the crux of the matter.

5 (178-179) : Note earlier that scenarios (a word I will continue to use even though it may be inappropriate in many cases) usually define CHANGES in climate. They are not, in these cases, 'scenarios', but 'scenarios of change'. Strict (i.e., absolute) scenarios are then constructed from them by adding the changes to a baseline climatology. This needs to be explained up front.

5 (187) : Delete '(and art)'. This is a derogatory term, likely to be misinterpreted/misrepresented.

6 (220) : Comma after 'scenarios'. The text contains many stylistic and grammatical errors (the most common being the failure to isolate parenthetical clauses). I will assume that someone with a better grasp of grammar will catch all these at some stage, so I will not comment further on them.

- 6 (225+) : A critical item missed here is inter-variable consistency. Later, consistency between climate and CO2 is mentioned; but there is no mention of consistency between, e.g., temperature and precipitation, etc. This is a major issue!
- 7 (257) : Instrumentally-based analogue scenarios were first introduced by Wigley et al. (Nature, 1979). Credit should be given. Also, the USDOE 'State of the Art (sic)' reports (1985) and the Bolin et al. SCOPE report (1986) both review this and other methods. This reviews should be cited.
- 7 (267-268) : What does 'extrapolating ...' mean?
- 7 (296) : Wigley et al. (1979) should also be cited here.
- 8 (306) : Nevertheless, they may do a better job of getting the inter-variable correlations 'right' than GCMs!
- 8 (315) : Delete 'questionable'. This word is entirely unnecessary here. More importantly, the authors need to be more careful in their choice of words, since there are many critics out there who will be looking for things that can be taken out of context, misinterpreted, or misrepresented.
- 8 (344-345) : Control run? So what? This is only relevant if the control is used in scenario development. This raises the issue of 'Definition 1' versus 'Definition 2' for defining climate change (a terminology introduced by Santer et al., 1994, JGR). (Later, this difference is attributed to Cubasch et al., but it was first clearly enunciated by Santer et al.) The difference is whether or not one subtracts the control from the perturbed result. More needs to be said about this. It is often assumed that subtracting the control will remove any spurious drift in the perturbation experiment. This, of course, is clearly wishful thinking, both a priori, and as shown by Raper and Cubasch (1996). Basically, there is no way to reliably remove drift in a perturbation experiment; which makes it all the more important to have drift-free models. Flux adjustments do not necessarily remove drift -- just look at some of the ECHAM control-run results. There are some very important issues here, central to the use of O/AGCMs in scenario generation. They need better coverage. More is said later, but this is still inadequate.
- 9 (357) : Yes, they can be different, but so what? The issue is whether the differences are statistically significant. To my knowledge, no one has addressed this issue properly.
- 9 (358) : I'm sure (at least I hope) you don't mean 'observed'. The issue is the difference between the equilibrium PATTERNS of change and the MODELLED (NOT 'observed') transient patterns of change.
- 9 (to 361) : You've missed the most important point! The advantage of an equilibrium result over an O/AGCM result is that the former is pure signal.
- 9 (to 376) : The Definition 1 versus Definition 2 issue is relevant here.
- 9 (379) : Please don't propagate garbage. The issue here is natural internally generated variability. There is no need for such variability to be chaotic, so you should eschew use of this word.
- 9 (to 387) : I presume here that you are talking about O/AGCMs. You should not use just 'GCM' -- you must be specific. Also, you've missed some vital points: the natural internal variability problem (i.e., output is signal plus noise -- noted elsewhere, but must be stated here); and the model-specific nature of the climate sensitivity.
- 10(399) : Please give credit to the first work on this (Santer et al., 1990). I should point out that this was actually my idea.
- 10(404-406) : Totally unclear.
- 10(420-421) : Poor wording. Should be '.. to which changes are added'.
- 10(423) : Delete 'appropriate'.
- 10(429) : Insert 'based' after 'period'.
- 10(431) : 'weather generators' comes as a non sequitur here. In any event, you haven't said what they are!
- 10(435-437) : So what? The issue is what period one is measuring the impacts from. In most cases it will be some nominal 'present-day',

- so the baseline climatology must refer to the same period. Whether or not the period has some sulphate effect in it is utterly irrelevant.
- 10(437-438) : What garbage. See above.
- 11(448-450) : More garbage -- think about it! The reason 1990 is not so useful as a reference 'period' is because the impacts variable is probably not adequately definable over a single year. You have really messed up this issue.
- 11(460) : Yet more garbage! Given what I have tried to explain above, it is ludicrous to consider daily data as part of the baseline climatology. The impacts variable may require daily data from a baseline period in order to define ITS reference level (but probably not), but this is NOT the same thing. Either all this is very badly worded, or you don't know what your doing.
- 11(468) : No!! Think about it!
- 11(470) : No!! This is NOT the reason.
- 11(473) : No!! Not 'observed' (which is past or present), but FUTURE data.
- 11(482-483) : Duplication.
- 12(to 492) : This is a very confused paragraph.
- 12(497-499) : Wrong. For upper air, their is a major paper by Santer et al. (JGR, 1999), which also touches on some surface issues. There are also a number of papers by Trenberth that are relevant.
- 12(507) : Again, introduction of an undefined term/concept (downscaling).
- 12(510) : At last, mention of changes. Sadly, it is inappropriate here, since this is NOT the reason.
- 12(514) : Why should this Figure be here?
- 12(518) : Wrong. As a scenario, this could be justified. You are confusing scenario (as you have defined it, which I have already criticized) with prediction/projection.
- 12(521) : See above.
- 12(525-527) : This is the Def. 1 vs Def. 2 issue. However, you have the history and motivation wrong.
- 12(527-531) : Wrong. This issue has nothing to do with cold start vs warm start; it is to get over the drift problem (which it fails to do).
- 12(537) : Not 'especially'; mor appropriate may be 'but only'.
- 13(543) : 'were'; grammar!
- 13(543-545) : Not clear.
- 13(552-553) : Not clear.
- 13(579-581) : So what? Given your definition of scenario, this doesn't matter.
- 14(594) : Why use 'perceived'?
- 14(604) : This issue was first raised by Kim et al. (1987?). It was first addressed in a credible manner by Wigley et al. (1990).
- 14(606) : 'appending' is a ridiculous word to use. Try 'adding'.
- 14(608) : 'often' to 'usually'.
- 14(613) : 'appended' to 'added'.
- 14(616) : 'appended' to 'added'.
- 14(617) : 'appended' to 'added'.
- 14(627,628) : Please cite the key initial papers by Kim et al. and Wigley et al.
- 15(635,636) : Clumsy sentence.
- 15(638) : Isn't the word 'physical' usually used? The process does not just involve dynamics.
- 15(642-648) : Mention of 1-way vs 2-way nesting needed here.
- 15(657-659) : You have failed to mention the most important reason for using LAMs, orography/topography.
- 16(667) : Please cite the key initial papers by Kim et al. and Wigley et al.
- 16(673) : 'predict and' to 'predictand'.
- 16(679-683) : Once again, you fail to mention the main advantage; viz. that statistical downscaling involve real-world data and so ensures that inter-variable relationships are realistic. Of course, these relationships may change; but LAMs don't even get the correct relationships for the present.
- 16(703)-17(716): These are VERY important results. They need far

- greater emphasis.
- 17(720) : In Australia? Or anywhere for that matter.
- 17(723-724) : See, e.g., Wigley (1999 - Pew report- and material cited therein).
- 17(725) : 'muiltple'?
- 17(730-732) : Not clear.
- 17(739-740) : This sentence sounds stupid. Rephrase.
- 17(744) : You cannot say 'most areas' and then cite only agriculture cases.
- 17(748) : The first clear exposition of this is in the oft-cited paper by Wigley (Nature, 1985). See also later paper in Climate Monitor.
- 17(755-756) : I disagree. Both methods have strengths and weaknesses.
- 18(770) : At last! A definition of 'weather generators'.
- 18(778-779) : Unclear.
- 18(798) : What means 'more definitive'?
- 18(803) : "Wilk's" to "Wilks".
- 18(805) : Hence, the work is irrelevant in the present context. Delete irrelevant text.
- 19(to 821) : Most of the agriculture studies dealing with the effects of variability changes are flawed since they fail to separate the low-frequency effect of induced changes in winter soil moisture levels from the specific effect of within-growing-season variability changes.
- 19(826-839) : Since this should refer back to lines 823,824, this whole section amounts to a giant non sequitur.
- 20(880) : One could be much stronger than this. The use of high spatial resolution information is more than just 'warranted', it is absolutely essential. However, there is another approach that you have failed to mention at all. This is 'upscaling' of the impacts model. There is some relevant work on this in papers by Jarvis and McNaughton (and vice versa). Another related approach is the direct modelling of spatial patterns of agricultural yield (as in work by Wigley and Tu Qipu, which relates yield patterns to climate patterns). Presumably one could apply a similar approach to direct modelling of river flow. These approaches complement the rather boring direct approach of downcsaling, and they may well circumvent some of its problems.
- 20(898) : Under this comes: model errors; sensitivity uncertainties; aerosol forcing uncertainties; lag uncertainties, regionalization versus global-mean uncertainties.
- 21(905) : lesser or greater than what??
- 21(916) : 'adequacy' is not the right word; hoe about 'appropriateness'?
- 21(928) : I disagree. Re-analysis data for precipitation are simply not good enough, and precipitation is the key variable in most impact areas. Also, in the regions where scenario data are most needed, real observational data are available. Re-analyses largely provide useful new data in regions where data are not needed. The authors seem not to have thought this through.
- 21(to 931) : There are two papers by Wigley (conference proceedings, edited by Hanisch) which address the issue of the relative magnitudes of different sources of uncertainty in global-mean projections (emissions, aerosol forcing, carbon cycle, other trace gases, climate sensitivity). These papers are singularly relevant to this section.
- 21(939) : Actually, the range for total emissions is from 7.9 to 29.0GtC/yr. For fossil CO2 emissions, the range is 6.5 to 28.8GtC/yr.
- 21(943) : Not just 'time-dependent evolution', but anything that has a specific time attached to it.
- 22(948) : The reference to Alcamo et al. here seems either perverse or ignorant. Recall that the topic is CLIMATE scenarios. In this context, MAGICC/SCENGEN is FAR better suited to exploring the consequences (right down the line) of emissions 'uncertainties'.
- 22(959-960) : MAGICC/SCENGEN already does this at the global-mean

- level. Furthermore, at least three O/AGCMs have fully embedded sulphur cycles already.
- 22(968) : 'specifications' is the wrong word. These things are NOT 'specified'.
- 22(970) : 'determine' to 'have'
- 22(972) : See also Wigley's Pew report (1999).
- 22(974-976) : Not straightforward? This really is utter garbage. In MAGICC/SCENGEN, this is extremely easy and straightforward.
- 22(985) : Ah ha! The 1-way/2-way nesting issue surfaces at last!
- 22(989-990) : See above.
- 23(999) : Actually, this issue was first raised in Santer et al. (1990). It has also been addressed in papers by Wigley and Palutikof (probably before anyone else).
- 23(1010-1011): The wording here is not quite right.
- 23(1022) : First done in Santer et al. (1990).
- 23(1030) : If one assumes stable patterns, which has been shown to be okay for the CO2 component of change, then the SNR problem can be minimized by using changes over a long time interval.
- 23(1033) : This average response method was alluded to in Santer et al. (1990). It was first implemented in ESCAPE and later in MAGICC/SCENGEN. A good illustration of the method, including some relevant discussion of it, is given in the Wigley Pew report (1999). One of the critical aspects of this method (which is not even mentioned here!) is that the results must be normalized by the global-mean temperature before averaging.
- 24(1040) : Is this the ACACIA program run out of NCAR? This program was established some years ago, and it would be extremely confusing if there were two programs with the same acronym.
- 24(1047) : Not 'a few', but many -- CMIP1.
- 24(1060) : 'rations' to 'ratios'.
- 24(1060-1062): Not clear.
- 24(1073) : What means 'non-standard forcing'? In my view, something like IS92a forcing would be 'standard', whereas 1% compound CO2 is 'non-standard' (i.e., unrealistic and artificial).
- 24(1076-1078): Really? Why? I think this statement is wrong. There are a number of ways to determine SNR values from a single O/AGCM run. (Note the continuing confusing use of 'GCM', instead of O/AGCM.)
- 24(1085) : I don't think 'uncertainties' is quite the right word here. Input emissions scenarios, which are scenarios in the strict sense of the word, do not directly address uncertainty issues (although they can, with some trepidation and a not-inconsiderable amount of ingenuity, be used to define uncertainties). By the way, as far as I can see, the only scenario development method/software that does address the input and uncertainty issues is MAGICC/SCENGEN.
- 25(1090) : Again, these are not the most appropriate references. Key references are Santer et al. (1990), and papers on ESCAPE and MAGICC/SCENGEN.
- 25(1093) : What means 'annotation' here?
- 25(1102) : Actually, it was my idea.
- 25(1105,1106): No! The key assumption is actually linear superposition. This is the way that SO4 effects are handled. There are a number of papers that show that this assumption works well for temperature, and a paper by Ramaswamy and Chen in GRL that shows that it works also for precipitation. The tricky thing for this variable would be to prove statistically that it doesn't work. Given the SNR, it would be very difficult to reject the null hypothesis that $P(A)+P(B)=P(A+B)$, where A,B are the forcings and P(.) is the response pattern.
- 25(1108) : Plus numerous other papers.
- 25(1112,1113): This is very galling. The method may have been used in IMAGE, but they got it from ESCAPE, which goes back to Santer et al. (1990). MAGICC/SCENGEN pushes the idea as far as is possible. Schlesinger's COSMIC does things quite similarly to MAGICC/SCENGEN. (Schlesinger was a co-author of the Santer et al. paper.)

25(1115) : Not clear.

25(1122) : All you can say here is 'may not hold', not 'probably does not hold'. Indeed, there are reasons to expect it to hold quite well.

25(1123) : Could begin new paragraph with 'Uncertainties'.

25(1123,1124): I think this statement is categorically wrong. MAGICC/SCENGEN incorporates SO4 influences, as does COSMIC. There is no evidence at all that the uncertainties are thereby amplified. Indeed, there is evidence to the contrary (e.g. Penner et al., 1997). Idle and unsupported speculations like this do nobody any good.

25(1124,1125): I suspect your argument here would have to hinge on the possible spatial effects of a THC slowdown or shutdown. If so, say so. But, if this is the case, you must also note that the latest non-flux-corrected O/AGCMs do not show these major THC changes, and scaling approaches may well work out very well for these situations, even in stabilization cases. Please avoid jumping to unsubstantiated conclusions.

25(1125) : I refereed this paper, and I judged it to be an appalling display of ignorance. It should not be cited.

26(1134) : Why is this Figure here?

26(1138) : Ah ha! At last the normalization issue. This must come much earlier.

26(1144-1147): This is simply wrong. It is true that Ramaswamy and Chen dreamed up a case with big hemispheric-scale responses but little global-mean response, but this was totally unrealistic. In all cases that I have looked at, using the method employed by MAGICC/SCENGEN and COSMIC, this is simply NOT a problem.

26(1147,1148): Again, this is just WRONG!

26(1150+) : Again, this is my idea, and it was first implemented in MAGICC/SCENGEN. Please give credit where due.

26(1156-1159): Isn't this ALWAYS the case. In other words, the scaling method is almost universally applicable and useful.

26(1159-1162): I do not think this has been proven.

26(1164,1165): There are other methods, too.

26(1172) : Oh come on! Scaling handles MANY types of uncertainty (perhaps all), not just 'one type'.

27(1181) : 'documented' to 'quantified'?

27(to 1185) : etc., etc.

27(1193) : MAGICC/SCENGEN allows the user to consider this issue by providing data on global precipitation pattern correlations. Indeed, this software was the first to consider this issue (in spite of the Whetton and Pittock paper cited on line 1199).

27(1198-1201): Very clumsy text.

27(1203-1204): This is an issue we considered years ago in developing ESCAPE and MAGICC/SCENGEN. The trouble with judging a model on its regional performance is one of statistical significance. It is much easier to get a good regional result by chance than to get results that are good globally.

27(1208-1211): Very clumsy text.

27(to 1214) : You have failed to mention a key issue. Is model skill in simulating present-day climate a reliable indicator of its skill in predicting future climate change? There is no evidence to support this idea, although it does sound a priori reasonable. You must at least raise the issue.

28(1227) : Cite Morgan and Keith (1995) here.

28(1231) : This is a critical point. It needs more emphasis.

28(1235+) : What about inter-variable consistency? This needs to be discussed.

28(1236) : 'the manifold' to 'possible'.

28(1239) : Insert 'give' after 'chapters'.

28(1252) : Not clear.

28(1255) : So what? It is almost certainly irrelevant unless the CO2 changes are bigger than anything anticipated, or unless there are nonlinear effects associated with THC changes (which looks increasingly unlikely).

28(1257) : 'mimics'? You must be joking! How about 'approximates'?

28(1262) : 'equal' (grammar).

- 28(1262,1263): How can smart people like this make such an elementary mistake!
- 29(1280,1281): This does not seem to be an appropriate reference.
- 29(1282) : 'albino' to 'albedo'.
- 29(1294) : This sea level consistency issue was first addressed by Wigley and Raper (Warrick et al. sea level book). It is, of course, avoided in MAGICC/SCENGEN.
- 29(1295) : 'dependable' to 'dependent'.
- 29(1295-1301): A giant red herring! Maybe some ignorant people produced inconsistent scenarios like this years ago, but the issue was also resolved years ago. All you need to say is that comprehensive software suites avoid these naive problems. Concentrate on the strengths of existing methods/software; don't reraise issues that were solved long ago.
- 29(1305-1308): Another misleading red herring, that fails to reflect the current state of the science. Global-mean responses to aerosol forcing CAN be used to drive regional patterns. This is just what is done in MAGICC/SCENGEN and COSMIC.
- 29(1310,1311): Not clear.
- 29(1314) : Delete 'scenario'.
- 29(1318) : 'to daily' to 'in daily'.
- 30(1329,1330): 'stimulated new techniques' Oh yeah? The MAGICC/SCENGEN method has not changed in 7 years, and it still represents the state of the science.
- 30(1332,1333): True, but you have not explained them very well. Could you not have a summary Table that lists the strengths and weaknesses of the various methods, including the direct use of O/AGCM output. This would have helped you a lot in planning and structuring this chapter. It can still help in revising it; and be useful to readers.
- 30(1336-1339): Not clear.
- 30(1342) : You have mentioned this before, but you have failed to tell us what it is or given any example. A mention alone is valueless.
- 30(1344) : What means 'semi-formal'. I thought it was a dress protocol.
- 30(general) : A crucial need for scenarios (and for simple models) is to expand the range of cases covered by O/AGCMs.

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* Dr. Sarah Raper *
* Climatic Research Unit *
* University of East Anglia *
* Norwich *
* NR4 7TJ *
* *
* Tel. + 44 1603 592089 *
* Fax. + 44 1603 507784 *
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