

From: "Mitchell, John FB" <jfbmitchell@meto.gov.uk>
 To: 'Mike Hulme' <m.hulme@uea.ac.uk>
 Subject: RE: GEC paper
 Date: Fri, 30 Apr 1999 17:23:15 +0100

see inserts

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> -----Original Message-----

> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]

> Sent: Friday, April 30, 1999 12:31 PM

> To: Mitchell, John FB

> Subject: RE: GEC paper

>

> John,

>

> Could you have a quick look at this paragraph (see below) from the GEC

> fast-track paper. I do not understand:

>

> a) why CO2-doubling forcing for CM2 is cited (see your original email at
 > the end of this message) as 3.26Wm⁻² when I thought it was 3.471Wm⁻² (I'm
 > sure I've seen 3.471Wm⁻² cited elsewhere for HadCM2).

[Mitchell, John FB] 3.471 in longwave, 3.26 when shortwave also
 taken into account. Unfortunately modellers do not always make clear how
 they have estimated their CO2 forcing.

> and

>

> b) why the forcing curves in the plot William Ingram sent show higher
 > forcing in CM2 than CM3 (by about 0.5Wm⁻²) when the CO2-doubling forcing
 > is

> *lower* in CM2 cf. CM3.

[Mitchell, John FB] HadCM2 is 1%/year increase in CO2 which is only
 approximately equivalent to IS92a. Hadcm 3 is "95a" - in fact "95a" I think
 differs only from in the conversion of the 92a emissions to concentrations,
 so strictly speaking is not an emissions scenario. As far as I know, Tom
 never did explain why his concentrations in 1995 were different from the
 ones Jonathan and I derived using his 1992 model- I think CH4 lifetimes and
 the CO2 sink were the main factors.

> [is this solely due again to the difference between IS92a and IS95a
 > concentrations?]

>

> and

>

> c) why the global-mean warmings in CM2 and CM3 are quite similar when CM3
 > has a higher sensitivity than CM2 (3.3 to 2.5K over the next century) and
 > CM3 also has a higher CO2-doubling forcing (3.74Wm⁻² to 3.26Wm⁻², or
 > 3.47Wm⁻² - see a)). Surely this should lead to faster warming in CM3 cf.
 > CM2?

[Mitchell, John FB] See above - HadCM2 uses 1%/year increase in
 CO2, which gives a greater forcing than HadCM3, even after the effect of
 explicit trace gases is added in.

(about 0.5Wm⁻² by 2100). The greater climate sensitivity does not
 make as big a difference as one would expect. The difference in CO2 forcing
 per doubling is not the issue- the net forcing is, and that has been
 calculated taking the difference in CO2 response into account

M aybe I have misinterpreted something here.

> Thanks,

>

> Mike

>

> _____

>

> Paragraph from GEC paper

>

> "In HadCM3, greenhouse gas concentrations were increased from their 1860
> values up to present (1990) as observed and then following the IPCC
> emissions scenario IS92a (Leggett et al., 1992) from 1990 to 2100. Only
> one simulation was carried out. The increase in radiative forcing during
> the twenty-first century is very similar to HadCM2, being only 0.5 Wm⁻²
> (about 10%) smaller by 2100 than in the HadCM2 experiment (Figure 2).
> Note
> that the ratio of the increases in CO2 concentration (HadCM2/HadCM3) is
> much greater than the ratio of the changes in radiative heating. There is
> a greater increase in heating in HadCM2, so a greater increase in CO2 is
> required to produce the same fractional increase in heating. Also,
> because
> the heating due to doubling CO2 in HadCM2 is less than in HadCM3 (3.26
> Wm⁻²
> compared to 3.74 Wm⁻²), a larger increase in CO2 is required to give the
> same change in heating. Note also that the increase in forcing varies as
> the logarithm of the change in CO2 concentration."
>
>

> At 14:54 09/04/99 +0100, you wrote:

> >Hi Mike.
> >
> >2xCO2
> >HadCM2 3.26 Wm⁻² including stratospheric adjustment and allowance for
> >solar absorption.
> >hadCM3 3.74 Wm⁻² as above.
> >
> >
> >Gordon C., C. Cooper, C. Senior, H. Banks, J. M. Gregory, T.C. Johns,
> >J.F.B.
> >Mitchell and R. Wood, 1999. Simulation of SST, sea ice extents and ocean
> >heat transports in a coupled model without flux adjustments. Climate
> >Dynamics (provisionally accepted)
> >
> >Note year is 1997
> >Gregory, J. M. and J.F.B Mitchell, 1997. The climate response to CO2 of
> >the
> >Hadley Centre coupled OAGCM with and without flux adjustment, J Geophys
> >Lett., 24, 1943 -1946.
> >
> >
> >I will try and look at then text now
> >John
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> >> -----Original Message-----
> >> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
> >> Sent: 09 April 1999 14:11
> >> To: Mitchell, John FB
> >> Subject: RE: GEC paper
> >>
> >> John,
> >>
> >> Here is a Word 6 version of the GEC paper. You need to give me two
> >> references (Gregory and Mitchell 1998 and Gordon et al 1999?) and check
> >> through the bits I have added. See especially what I have worded about
> >> CO2
> >> concentrations in Section 7 - quite what we cite for HadCM3 I'm not
> >> sure.
> >> It depends what the impacts people say about the sensitivity of their
> >> results to CO2 concentrations. I also have a question in the text in
> >> Section 5 for you.
> >>

> > Figure 10 is not made yet - I thought I would produce this inter-model
> > comparison plot for the Amazon given the interesting results we were
> > getting there.
> >
> > I will wait for your comments before sending it to Martin and the other
> > impacts people, but I must do this by the 19th April at latest.
> >
> > I think I understand where the various CO2 numbers come from now.
> >
> > Regards,
> >
> > Mike
> >
> >
> > At 11:59 09/04/99 +0100, you wrote:
> > >Dear Mike,
> > > I think we have traced where the different CO2 values have come from
> > >

	HadCM2		HadCM3	
	assumed	'correct'	assumed	'correct'
> > > 2020s	441	470	457	434
> > > 2050s	565	590	574	528
> > > 2080s	731	770	712	638

> > >
> > > The left hand HadCM2 value we think comes from SA90 - Peter Cox will
> > >check. The second Hadcm2 value is notional- I don't think the
> > > inconsistency
> > > between the the columns matters that much, since there is no "correct"
> > > HadCM2 value.
> > > The Hadcm3 values do matter. The right hand side value is
> > > what was used in the model, and what Willaim took from the TOM Wigley
> > > as
> > > being the SAR IS95a values. I do not know where these are publicaly
> > > available, and I have asked Dave Griggs that if we use new scenarios
> > > (eg
> > > SRES) in the TAR, they are publicly available and well documented. The
> > > left
> > > hand column appears to be from the 1992 IPCC supplement.(The annex by
> > > Mitchell and Gregory). This used the then current UEA enrgy
> > > balance/carbon
> > > cycle model to convert CO2 emissions to concentrations. I presume the
> > > discrepancy comes from changes to the carbon cycle model and anything
> > > else
> > > affecting the conversion from emissions to concentrations.
> > > Unfortunately,
> > > as
> > > far as I can tell, the SAR never refers to these or explains why the
> > > concentrations are different.
> > > This could easily happen again. The situation with the new
> > > SRES scenarios to me seems rather chaotic, anad again they are
> > > emissions
> > > scenarios, not concentration scenarios. The initial GCM runs will use
> > > CO2
> > > concentrations from one particular model. The TAR may report (probably
> > > will
> > > report) different values since they will use a different model. The
> > > best
> > > thing is to talk to the people who set up the GCM run to find out
> > > exactly
> > > what was used in the model
> > >
> > > With best wishes
> > > John
> > >
> > >
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> > > -----Original Message-----

> > > From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]

> > > Sent: 08 April 1999 17:35

> > > To: N.W.Arnell; Sari Kovats; Matt Livermore; parryml@aol.com;

> > Andrew

> > > White; jfbmitchell@meto.gov.uk; gjjenkins@meto.gov.uk;

> > > r.nicholls@mdx.ac.uk

> > > Subject: HadCM3 CO2 concentrations

> > > Importance: High

> > >

> > > Dear Fast-trackers,

> > >

> > > In putting the scenario paper together for the GEC issue, John

> Mitchell

> > > and

> > > I have come up with slightly different CO2 concentrations for HadCM2

> > and

> > > HadCM3 to what we had earlier assumed. These CO2 concentrations

> will

> > > really have to appear in the scenario paper to be consistent with

> the

> > GCM

> > > experiments. Given the differences from the values (I think) you

> have

> > all

> > > used in the impacts work, what significance does this have for your

> > work?

> > >

> > >

> > >

	HadCM2		HadCM3	
	assumed	'correct'	assumed	'correct'
> > > 2020s	441	470	457	434
> > > 2050s	565	590	574	528
> > > 2080s	731	770	712	638

> > >

> > >

> > > The difference is that the assumed HadCM2 concentrations are

> 20-30ppmv

> > too

> > > low while the assumed HadCM3 concentrations are 20-70ppmv too high.

> > >

> > > The assumed HadCM2 concentrations came from Cox and Friend (they had

> > > already run Hybrid with these concentrations before the FT work got

> > under

> > > way, so we adopted their values). I cannot yet trace where the

> assumed

> > > HadCM3 concentrations came from, but the 'correct' values are what

> both

> > > John Mitchell and the IPCC (1996 report) have calculated for the

> IS92a

> > > scenario.

> > >

> > > Your suggestions on how best to handle this inconsistency would be

> > > appreciated. How big a difference do these differences make to your

> > > impacts?

> > >

> > > Thanks,

> > >

> > > Mike

> > >

> > >

> > >

> >

> *****

> > > ***

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> > > Norwich NR4 7TJ
> > >
> > >
> *****
> > > ***
> > > Annual mean temperature in Central England during 1999
> > > is about +1.5 deg C above the 1961-90 average
> > > *****
> > > The global-mean surface air temperature anomaly for 1998
> > > was +0.58 deg C above the 1961-90 average, the warmest year yet
> > > recorded
> > >
> > >
> *****
> > > ***
> > > << File: gec.fasttrack.doc >>
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