From: on behalf of Andrew Dessler
To: Steve McIntyre
Subject: Re: thanks
Date: Wednesday, September 21, 2011 9:48:59 AM

> A first quick point - you should really learn R if you're doing statistical analysis. It has a fantastic number of good packages and top people contributing. At a quick look, the python regression that you're using is pretty simplistic. The payback for changing over will be almost instantaneous.

I looked at R about 2 years ago, installed it on my computer and even checked a book out of the library on it. It didn't stick, though.
Inertia is strong.

> Second, the calculation that you've sent me doesn't seem to show anything other than trivial properties of normal distributions in a longwinded way. I realize that you're commenting on Lindzen and on Spencer and I haven't gotten to parsing them yet. They may not agree that your calculation is on point, but that's a different issue. If you've characterized this aspect of the dispute correctly, it certainly doesn't seem like something that should be in dispute.

The dispute is not over the mechanics of the calculation, it's really on the parameters of the calculation (in particular, the relative size of the cloud vs. ocean forcing).

> I don't have as much time as I'd like right now. Normally I'd start a thread and ask for comments. There are some very sharp statisticians commenting at Climate Audit from time to time (Jean S, UC, Hu McCulloch, Roman Mureika, Nic Lewis) Do you mind if I throw your code online for comment? Sometimes academics are thin-skinned, but the commenters are probably much more knowledgeable on these matters than the peer reviewers (or promoters) of either article and on other occasions clarify points that are unclear to me.

Sure, throw the code online. But please make it clear that I'm not endorsing the calculation as physically meaningful ... I think the underlying model being simulated is wrong. I was just showing that, even if you accept the underlying model, the conclusions of SB11 and LC11 still do not hold if you use the correct ratio of ocean to cloud forcing.

> Regards, Steve Mc
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> -----Original Message-----
> From: [mailto: On Behalf Of Andrew Dessler
> Sent: September-17-11 9:38 PM
> To: Steve McIntyre
> Subject: Re: thanks
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> assuming that you're asking how the 0.4% number was determined, here's
> the python code. this is basically designed to replicate what Spencer
> did in his paper, and allow me to recalculate with more accurate
> parameters. Note, however, that I have concluded (as described in my
> paper), that the underlying physics of this model are incorrect. I do
> not think it conserves energy, and it does not have any ENSO physics
> in it, despite being a simulation of ENSO. Enjoy!
>

```
> import numpy as np
> import matplotlib.pyplot as plt
> from matplotlib.pyplot import *
> from scipy import stats
> import smooth
> import glob
>
> modelruns=1000.
> modlen=500
> calcslope=[]
>
> for kk in np.arange(modelruns):
>    dF=np.random.normal(size=(modlen+18))*20
>    dF=smooth.smoothListGaussian(dF,degree=9)
>    dR=np.random.normal(size=(modlen))*1
>    lmd=6
>    c=14*12. # heat capacity
>    t=[0.];dtdt=[]
>
>    for ii in range(modlen):
>        dtdt1=(dF[ii]+dR[ii]-lmd*t[-1])/c
>        t.append(t[-1]+dtdt1);dtdt.append(dtdt1)
>
>    t=np.array(t);dtdt=np.array(dtdt)
>    # t=(t[1:]+t[:-1])/2
>    t=t[:-1]
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>    calcslope.append(stats.linregress(t,dR-lmd*t)[0])
>
> print 'avg. bias = %4.2f, avg. std. = %4.2f' %
> ((-np.average(calcslope)/lmd-1)*100,(np.std(calcslope)/lmd)*100)
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> 2011/9/17 Steve McIntyre <
>> Maybe you'll volunteer to look into the Hockey Stick dispute :)
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>> Please don't assume that I was party to or understand or endorse what
>> Spencer did. I'm trying to understand both concurrently and am finding both
>> a bit hard to understand.
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>> dess=read.csv("http://www.climateaudit.info/data/dessler/dessler_2010.csv"
>> #collated from data sent Sep 6, 2011
>>       fm=lm(eradr~erats-1,dess)
>>       summary(fm)
>>        #erats   0.5400     0.3604   1.498    0.137
>>        #Residual standard error: 0.5018 on 119 degrees of freedom
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>> Are you saying that the D11 regression simply added a random series to the
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>>       K=nrow(dess)
>>       dess$random=rnorm(K,sd=13.7) #or some such sd
>>       fm2=lm(eradr~erats+random-1,dess)
>>       summary(fm2)
>>     #  Estimate Std. Error t value Pr(>|t|)
>>       #erats  0.539064   0.362050   1.489    0.139
```

```
>>      #random 0.000341   0.003239   0.105    0.916
>>      #Residual standard error: 0.5039 on 118 degrees of freedom
>>      #Multiple R-squared: 0.01861,    Adjusted R-squared: 0.001974
>>      #F-statistic: 1.119 on 2 and 118 DF,  p-value: 0.3301
>>
>>      dess$random=rnorm(K,sd=.5) #or some such sd
>>      fm3=lm(eradr~erats+random-1,dess)
>>      summary(fm3)
>>      #again similar values
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>> Is this right? If it isn't, can you send me your script (Python or
>> whatever) for this leg of the calculation as I am then stumped as to what's
>> going on.
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>> Sent: September-17-11 11:06 AM
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>> Subject: Re: thanks
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>> Roy did, which was generate random time series with a particular
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>>> Sent: September-09-11 1:03 PM
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>>>> I think that making data and code publicly available is a good way of
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From: Steve McIntyre
To: Andrew Dessler
Subject: FW: thanks
Date: Wednesday, September 21, 2011 9:21:57 AM

I'm parsing Spencer now. I see his page 8 and am working through that now.

-----Original Message-----
From: Steve McIntyre [mailto:
Sent: September-21-11 8:39 AM
To: 'Andrew Dessler'
Subject: RE: thanks

Andy,

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>> adessler@tamu.edu
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>> 979-862-1427
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Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Steve McIntyre
To: "Andrew Dessler"
Subject: RE: thanks
Date: Wednesday, September 21, 2011 7:39:12 AM

Andy,

A first quick point - you should really learn R if you're doing statistical analysis. It has a fantastic number of good packages and top people contributing. At a quick look, the python regression that you're using is pretty simplistic. The payback for changing over will be almost instantaneous.

Second, the calculation that you've sent me doesn't seem to show anything other than trivial properties of normal distributions in a longwinded way. I realize that you're commenting on Lindzen and on Spencer and I haven't gotten to parsing them yet. They may not agree that your calculation is on point, but that's a different issue. If you've characterized this aspect of the dispute correctly, it certainly doesn't seem like something that should be in dispute.

I don't have as much time as I'd like right now. Normally I'd start a thread and ask for comments. There are some very sharp statisticians commenting at Climate Audit from time to time (Jean S, UC, Hu McCulloch, Roman Mureika, Nic Lewis) Do you mind if I throw your code online for comment? Sometimes academics are thin-skinned, but the commenters are probably much more knowledgeable on these matters than the peer reviewers (or promoters) of either article and on other occasions clarify points that are unclear to me.

Regards, Steve Mc

-----Original Message-----
From: [mailto: On Behalf Of Andrew Dessler
Sent: September-17-11 9:38 PM
To: Steve McIntyre
Subject: Re: thanks

I'm still not sure I understand exactly what you're asking. However, assuming that you're asking how the 0.4% number was determined, here's the python code. this is basically designed to replicate what Spencer did in his paper, and allow me to recalculate with more accurate parameters. Note, however, that I have concluded (as described in my paper), that the underlying physics of this model are incorrect. I do not think it conserves energy, and it does not have any ENSO physics in it, despite being a simulation of ENSO. Enjoy!

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import numpy as np
import matplotlib.pyplot as plt
from matplotlib.pyplot import *
from scipy import stats
import smooth
import glob

modelruns=1000.
modlen=500
calcslope=[]

for kk in np.arange(modelruns):
    dF=np.random.normal(size=(modlen+18))*20
```

```
    dF=smooth.smoothListGaussian(dF,degree=9)
    dR=np.random.normal(size=(modlen))*1
    lmd=6
    c=14*12. # heat capacity
    t=[0.];dtdt=[]

    for ii in range(modlen):
        dtdt1=(dF[ii]+dR[ii]-lmd*t[-1])/c
        t.append(t[-1]+dtdt1);dtdt.append(dtdt1)

    t=np.array(t);dtdt=np.array(dtdt)
    # t=(t[1:]+t[:-1])/2
    t=t[:-1]

    calcslope.append(stats.linregress(t,dR-lmd*t)[0])

print 'avg. bias = %4.2f, avg. std. = %4.2f' %
((-np.average(calcslope)/lmd-1)*100,(np.std(calcslope)/lmd)*100)
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> Spencer did. I'm trying to understand both concurrently and am finding both
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> following R script:
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> dess=read.csv("http://www.climateaudit.info/data/dessler/dessler_2010.csv"
> #collated from data sent Sep 6, 2011
> fm=lm(eradr~erats-1,dess)
> summary(fm)
> #erats 0.5400 0.3604 1.498 0.137
> #Residual standard error: 0.5018 on 119 degrees of freedom
> #Multiple R-squared: 0.01852, Adjusted R-squared: 0.01027
> #F-statistic: 2.245 on 1 and 119 DF, p-value: 0.1367
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> Are you saying that the D11 regression simply added a random series to the
> regressand and re-did the regression - along the lines of the following:
> K=nrow(dess)
> dess$random=rnorm(K,sd=13.7) #or some such sd
> fm2=lm(eradr~erats+random-1,dess)
> summary(fm2)
> # Estimate Std. Error t value Pr(>|t|)
> #erats 0.539064 0.362050 1.489 0.139
> #random 0.000341 0.003239 0.105 0.916
> #Residual standard error: 0.5039 on 118 degrees of freedom
> #Multiple R-squared: 0.01861, Adjusted R-squared: 0.001974
> #F-statistic: 1.119 on 2 and 118 DF, p-value: 0.3301
>
> dess$random=rnorm(K,sd=.5) #or some such sd
> fm3=lm(eradr~erats+random-1,dess)
> summary(fm3)
> #again similar values
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> Is this right? If it isn't, can you send me your script (Python or
> whatever) for this leg of the calculation as I am then stumped as to what's
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> Sent: September-17-11 11:06 AM
> To: Steve McIntyre
> Subject: Re: thanks
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> Roy did, which was generate random time series with a particular
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> value, it's the value of 20 where the disagreement lies.
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>> From: [mailto: On Behalf

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>> Sent: September-09-11 1:03 PM
>> To: Steve McIntyre
>> Subject: Re: thanks
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From: on behalf of Andrew Dessler
To: Steve McIntyre
Subject: Re: thanks
Date: Saturday, September 17, 2011 8:37:33 PM

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To: "Andrew Dessler"
Subject: RE: thanks
Date: Saturday, September 17, 2011 10:41:55 AM

Maybe you'll volunteer to look into the Hockey Stick dispute :)

Please don't assume that I was party to or understand or endorse what Spencer did. I'm trying to understand both concurrently and am finding both a bit hard to understand.

I can quickly replicate the reported results of the D10 regression with following R script:

```
dess=read.csv("http://www.climateaudit.info/data/dessler/dessler_2010.csv"
#collated from data sent Sep 6, 2011
      fm=lm(eradr~erats-1,dess)
      summary(fm)
       #erats   0.5400     0.3604   1.498    0.137
       #Residual standard error: 0.5018 on 119 degrees of freedom
       #Multiple R-squared: 0.01852,    Adjusted R-squared: 0.01027
       #F-statistic: 2.245 on 1 and 119 DF,  p-value: 0.1367
```

Are you saying that the D11 regression simply added a random series to the regressand and re-did the regression - along the lines of the following:

```
      K=nrow(dess)
      dess$random=rnorm(K,sd=13.7) #or some such sd
      fm2=lm(eradr~erats+random-1,dess)
      summary(fm2)
    #  Estimate Std. Error t value Pr(>|t|)
      #erats  0.539064   0.362050   1.489    0.139
      #random 0.000341   0.003239   0.105    0.916
      #Residual standard error: 0.5039 on 118 degrees of freedom
      #Multiple R-squared: 0.01861,    Adjusted R-squared: 0.001974
      #F-statistic: 1.119 on 2 and 118 DF,  p-value: 0.3301

      dess$random=rnorm(K,sd=.5) #or some such sd
      fm3=lm(eradr~erats+random-1,dess)
      summary(fm3)
      #again similar values
```

Is this right? If it isn't, can you send me your script (Python or whatever) for this leg of the calculation as I am then stumped as to what's going on.

Regards, Steve Mc

-----Original Message-----
From: [mailto: On Behalf Of Andrew Dessler
Sent: September-17-11 11:06 AM
To: Steve McIntyre
Subject: Re: thanks

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> enterprise, I think that much of the subsequent history would have been
> avoided.
>
>
>
> Without editorializing further on my long backstory, if there's any
> opportunity for you and Roy to narrow down issues actually at stake, I think
> that both of you would benefit from it. If you're willing to try this, I am
> quite prepared to keep Roy's feet to the flames if you wish to try this.
>
>
>
> by keeping my criticisms away from complexity and on areas that almost
>
> anyone will be able to see. So I'm interested in knowing if I'm
>
> succeeding.
>
>
>
> I think that making data and code publicly available is a good way of doing
> this. I actually think that blogs are a good way of reconciling results.
>
>
>
> Regards, Steve Mc
>
>
>
>

>
>

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Steve McIntyre
To: "Andrew Dessler"
Subject: RE: thanks
Date: Friday, September 09, 2011 11:31:09 AM

> BTW, when you look at our exchange, do you see two scientists evenly arguing or can you see that Roy's arguments are fundamentally fraudulent?

I can't tell from the arguments. It seems to me that academics are far too quick to have hissy fits, but I don't think that one can extrapolate past that. I wrote to Roy suggesting that he withdraw his comment about "doing homework' and do what he can to see what points are agreed on as far too much debate is at cross-purposes.

> I'm sure you ran into this in the Hockey Stick debate: because the material is complicated, most people can't really understand the criticisms. All they see is an argument and they conclude it's a toss up, rather than digging into the arguments and determining that one side is actually right.

Yup. Though it's curious the degree to which professionals and scientists in other fields grasp the salient points and the degree to which people in the climate community have refused to grasp the nettle. Mostly they're too busy to be bothered and assume that Mann et al must be right. On the other hand, it's obviously attracted much interest among the technical public. The issues aren't as complicated as represented and can be understood.

The entire dispute should have been put to bed years ago and would have been, had a proposal that I made to Ammann at AGU 2005 (I bought him lunch)not been refused. This was pre-NAS panel, pre Barton. UCAR had put out a lurid press release saying that all our results were "unfounded". However, their emulation code almost exactly matched ours (in one day, I matched to 8 9s accuracy) and, as a result, their empirical results matched ours if the assumptions were specified. Any disputes were not over calculations, but over their interpretation. This was never clear in the public debate and, in my opinion, was wilfully obscured in favor of misleading assertions made at cross-purposes. (Something that seems to be on your mind in the present case.)

I suggested to Ammann that we try to make a joint paper summarizing points of agreement, points of disagreement and how matters could be resolved. A simple "businesslike" approach. That we declare a temporary armistice and allow 60 days to prepare such a joint paper, and, if we were unsuccessful, we could return to the status quo. Ammann refused, saying that such a statement would be bad for his career. I twice wrote him formally re-iterating the offer and he did not acknowledge either email.

After Ammann's refusal, I also asked Ammann to disclose that he had confirmed our calculations on verification r2 and other statistics (which I knew that

he had confirmed). Ammann refused even this. Verification r2 is a long and ugly story.

I realize that Ammann was young in his career and under pressure, but I still thought that his reasons were contemptible. And that the pressures on him were even more contemptible. I think that that it was, as events proved, an unwise decision as well even from the perspective of people most worried about warming. Had Ammann and Wahl co-operated in such an enterprise, I think that much of the subsequent history would have been avoided.

Without editorializing further on my long backstory, if there's any opportunity for you and Roy to narrow down issues actually at stake, I think that both of you would benefit from it. If you're willing to try this, I am quite prepared to keep Roy's feet to the flames if you wish to try this.

> by keeping my criticisms away from complexity and on areas that almost anyone will be able to see. So I'm interested in knowing if I'm succeeding.

I think that making data and code publicly available is a good way of doing this. I actually think that blogs are a good way of reconciling results.

Regards, Steve Mc

From: on behalf of Andrew Dessler
To: Steve McIntyre
Subject: thanks
Date: Friday, September 09, 2011 10:35:04 AM

I think that just having you the cc list limits Roy's mendacity.

BTW, when you look at our exchange, do you see two scientists evenly arguing or can you see that Roy's arguments are fundamentally fraudulent? I'm sure you ran into this in the Hockey Stick debate: because the material is complicated, most people can't really understand the criticisms. All they see is an argument and they conclude it's a toss up, rather than digging into the arguments and determining that one side is actually right. I'm trying to avoid that by keeping my criticisms away from complexity and on areas that almost anyone will be able to see. So I'm interested in knowing if I'm succeeding.

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Roy Spencer; Steve McIntyre
Subject: Re: ocean heat content
Date: Friday, September 09, 2011 10:05:18 AM

Sorry, Roy, more homework:

> 1) I stand by my CERES calculations (sd=0.53), which are indeed based upon
> 3-monthly averages. Monthly gives 0.73. Did you use *60N-60S* oceans
> radiative flux? That's the EXACT same region we computed the ocean
> temperatures for. Apples & apples. Do you know exactly what region your
> Levitus ocean heat content data is for?

Just to confirm: you do know that there are clouds over land and poleward of 60°?

Or is it your contention that only clouds over ocean and between 60°N-60°S can affect our climate?

Ultimately, though, this is small beer ... your number of 0.53 is pretty close to my number of 0.40. Nonetheless, your number is still wrong.

> 2) Andy, you really need to understand ocean heat fluxes better. We have
> analyzed the vertical structures of temperature variations in (1) Levitus,
> (2) the CMIP3 models, and (3) in our own ocean diffusion model. Variations
> in mixing BELOW the thermocline can cause WARMING below the thermocline.
> For example, in the Levitus data, temperature changes at 300 meters depth
> tend to be PRECEDED by warming BELOW that level, and then FOLLOWED by
> temperature changes ABOVE that level (even up to 100 meters).

OK, it looks like you're going with my option 3 (denying the first law of thermodynamics applies to you). Good idea! Just tell the editorial board of the WSJ that it's another job-killing regulation supported by Obama and they'll be onboard; and tell Lou Dobbs that it wasn't thought up by an American.

Seriously, if you don't have a term for heat flux down from your 100-m layer into the deep ocean, then your equation does not conserve energy. Period. This is a surprising stance, since you were big proponent of the first law up until, oh, about two days ago.

> You are making a major assumption when you assume ALL of the energy flux
> affecting the 0-700 m layer only comes through the TOP boundary (the ocean
> surface).

If it turns out that there is a big heat flux upward through the 700-m level, then it will mean that the OHC data simply cannot be used for this calculation at all. Your 100-m calculation will still not conserve energy and still be wrong, though.

Remember, however, that I have an independent calculation of this term, C(dTs/dt), based on surface T and an assumed heat capacity, which gives me about the same number as the OHC data. That suggests that my numbers are indeed correct.

Now it's on to the third term: lambda dTs. It's like Christmas morning.

I appreciate Roy's help with my homework.

> Andy, I am finished doing your homework for you. That's enough.
>
> -Roy
>
>> From: adessler@tamu.edu
>> Date: Thu, 8 Sep 2011 22:59:48 -0500
>> Subject: Re: ocean heat content
>> To:
>>
>> Hi guys-
>>
>> Two issues. First, I spent ~2 minutes reproducing Roy's dR number
>> (based on data downloaded from Steve's website) and I can reproduce
>> it, but only if I use the monthly data. If I do a 3-month average on
>> the CERES data before doing the standard deviation, I get a value
>> smaller than the one on Roy's website. So, my question to Roy is
>> whether he averaged the CERES data down to 3 months to match the
>> Levitus data? Or, if he's using 3-month Levitus data but 1-month
>> CERES data?
>>
>> BTW, here is my python code:
>> d1=np.loadtxt('spencer.txt',skiprows=1,delimiter=',') # edit file to
>> replace ',,' with ',0,'
>> crf=d1[:,4]-d1[:,7] # calculate CRF
>> crf2=[np.average(crf[ii*3:ii*3+3]) for ii in range(len(crf)/3)] #
>> average down to 3 months
>> 'standard deviation of 3-month avg.: %f' % np.std(crf2)
>> 'standard deviation of monthly.: %f' % np.std(crf)
>>
>> output:
>> Out[7]: 'standard deviation of 3-month avg.: 0.411202'
>> Out[8]: 'standard deviation of monthly.: 0.579952'
>>
>> As far as you adherence to 100 m goes, you can certainly do that, but
>> then you have to do one of the following:
>> 1) put into your energy balance equation an additional term for flow
>> of heat out of the 100-m layer into the deep ocean. Without that,
>> your equation does not conserve energy. How big is that term going to
>> be? It's the difference between your 2.3 W/m2 number and my 4.7 W/m2,
>> so adding that term to your 2.3 number produces a net flow into the
>> ocean that's the same as my 700-m number. Net result: no soup for
>> you.
>> 2) argue that heat flux out of the bottom of your 100-m layer is zero.
>> This makes it hard to explain temperature variations below 100 m,
>> though. No soup.
>> 3) declare that conservation of energy doesn't apply in the
>> post-normal climate change debate.
>>
>> Sadly, I suspect your best option is #3.
>>
>> Regarding your argument that the Argo data is noisy, I'll e-mail Josh
>> Willis at JPL to see if the experts agree with that.
>>
>> Looking forward to Roy's response about his dR values. If Roy does
>> not confirm my suspicion that Roy's using monthly CERES data, then
>> perhaps Steve M. can spend 1-2 minutes doing the same calculation ...
>> it will be trivial for him to confirm my or Roy's calculation.
>>

>> Best wishes.
>>
>>
>> On Thu, Sep 8, 2011 at 6:33 PM, Roy Spencer <
>> wrote:
>> > Andy:
>> >
>> > Not so quick on the draw there pardner...I stlll see 2 issues here
>> > before I
>> > change my estimates on my blog:
>> >
>> > 1) DEPTH SCALE OF AVERAGING: I think there are questions about what
>> > ocean
>> > layer depth should be used for this: my calculations are based upon the
>> > average depth to the middle of the global average thermocline (~100m),
>> > as
>> > seen (for example) in this movie we made of the temperature variations
>> > with
>> > depth. You have now chosen to use 700 m. Remember, we are discussing
>> > the
>> > degree to which *interannual* surface temperature variability can be
>> > affected by turbulent mixing versus unforced TOA radiative variations.
>> > If
>> > you would get the actual depth level temperature data and analyze them,
>> > you
>> > will see there is little interannual variability down there....but there
>> > is
>> > a WARMING TREND which, as you know, can be caused by CONSTANT radiative
>> > forcing over a long period of time, e.g. from CO2. So, it is not clear
>> > at
>> > all what depth should be used in these calculations....100 m, 700 m or
>> > somewhere in between. Also, the possible need to detrend the
>> > temperature
>> > time series first should be addressed.
>> >
>> > 2) TIME SCALE OF AVERAGING Regarding using monthly ocean temperature
>> > data,
>> > rather than 3-monthly, I can guarantee you there is a noise issue here:
>> > the
>> > standard deviation will be inflated at that time scale. If you don't
>> > believe me, try making the monthly global radiance anomalies match
>> > month-to-month changes in ocean heat content. 3-month is much less
>> > noisy.
>> > It could be even a little more time averaging is justified.
>> >
>> > No, my calculations at drroyspencer.com still stand until someone
>> > demonstrates (1) why 700 m (or any depth) should be used instead of 100
>> > m,
>> > and (2) how much time averaging should be used to reduce noise.
>> >
>> > Nevertheless, I encourage you to publish what you have, ASAP.
>> >
>> > -Roy
>> >
>> >> From: adessler@tamu.edu
>> >> Date: Thu, 8 Sep 2011 16:55:35 -0500
>> >> Subject: Re: ocean heat content
>> >> To:
>> >> CC:
>> >>

>> >> Wow, that was easy. Just have to know the right people to cc, I guess.
>> >>
>> >> The difference between the number using Roy's data (Levitus), about 5
>> >> W/m2, and mine (Argo), about 13 W/m2, is not due to data, but because
>> >> Levitus are 3-mo averages while Argo are monthly. I've verified that
>> >> by averaging down Argo data. I am not surprised that averaging the
>> >> data set temporally reduces its standard deviation. Both go down ~700
>> >> m, which is probably OK for this problem.
>> >>
>> >> Off the top of my head, I can't think of any reason why using the
>> >> highest temporal resolution data would be bad ... and I can think of
>> >> reasons why averaging the data would be misleading. I'll think more
>> >> about this while I verify Roy's other numbers.
>> >>
>> >> But rest assured, Roy, if you can convince me that using monthly data
>> >> is wrong, then I'll change the paper when I get the galleys.
>> >>
>> >> BTW, I hope to see a correction on your blog with the new number soon.
>> >>
>> >> So now, as suggested by Roy, I'm going to start looking at terms on
>> >> the RHS ... first up, deltaR. Steve has kindly provided me with Roy's
>> >> data, so I'm all set! Hopefully I'll get the same number as Roy, but
>> >> if I don't, then we can call in Steve to adjudicate.
>> >>
>> >> Thanks for all your help.
>> >>
>> >> On Thu, Sep 8, 2011 at 3:56 PM, Roy Spencer <
>> >> wrote:
>> >> > Andy (& Steve):
>> >> >
>> >> > OK, I think we have pretty close agreement on the number Andy is
>> >> > calculating.
>> >> >
>> >> > I understand better what you are doing *now*, Andy (but not what you
>> >> > did
>> >> > in
>> >> > the preprint). Your preprint was not clear...you mentioned 100 m,
>> >> > and
>> >> > you
>> >> > also used Lindzen's 186 number which scales to 104 m.
>> >> >
>> >> > We now also get about 5 Watts/m2 using the full 4-D Levitus
>> >> > temperature
>> >> > dataset.
>> >> >
>> >> >
>> >> > So, I now know what you did on the LEFT hand side of the equation.
>> >> > That's a
>> >> > start.
>> >> >
>> >> >
>> >> > So, are you going to change your paper then? Because the preprint
>> >> > has 9
>> >> > and
>> >> > 13 as two estimates for the LHS....not 5.
>> >> >
>> >> >
>> >> > -Roy
>> >> >
>> >> >> From: adessler@tamu.edu

>> >> >> Date: Thu, 8 Sep 2011 14:08:23 -0500
>> >> >> Subject: Fwd: ocean heat content
>> >> >> To:
>> >> >> CC:
>> >> >>
>> >> >> Steve-
>> >> >>
>> >> >> Since I've been helping you understand Dessler 2010, I'm wondering
>> >> >> if
>> >> >> you'd do me and Roy a favor. Please read our exchange below, from
>> >> >> the
>> >> >> bottom up.
>> >> >>
>> >> >> Can you do a calculation of the time series of the total heat
>> >> >> content
>> >> >> of the ocean? The Levitus heat capacity data is here:
>> >> >> http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/heat_global.html
>> >> >> All you have to do is calculate the time series of total heat
>> >> >> content
>> >> >> of the ocean (in J) and then do a month minus month difference (as a
>> >> >> crude derivative) and then take the standard deviation of the
>> >> >> 2000-2010 period.
>> >> >>
>> >> >> The number I get is about 4.7 W/m2. Roy gets about half that number,
>> >> >> using a different method. We'd love to find out who's right (I
>> >> >> certainly acknowledge I could have a bug in my code). And you'll
>> >> >> probably get a blog post out of it.
>> >> >>
>> >> >> I did get your last question and will respond ... probably about the
>> >> >> time you get back to me with an answer to this question. :)
>> >> >>
>> >> >> Thanks!
>> >> >>
>> >> >> PS: I don't know if you read python, but I've attached my code.
>> >> >> Perhaps you can post this on your blog and get readers to check my
>> >> >> math.
>> >> >>
>> >> >>
>> >> >> ---------- Forwarded message ----------
>> >> >> From: Andrew Dessler <adessler@tamu.edu>
>> >> >> Date: Thu, Sep 8, 2011 at 1:11 PM
>> >> >> Subject: Re: ocean heat content
>> >> >> To: Roy Spencer <
>> >> >>
>> >> >>
>> >> >> > Do you realize the 186 W-month/m2/K number in your paper
>> >> >> > corresponds
>> >> >> > to
>> >> >> > 104 m depth mixed layer? So, didn't you already assume 100 m in
>> >> >> > your
>> >> >> > argument there?
>> >> >>
>> >> >> No. You only assume 100 m when you use surface T to figure out the
>> >> >> temperature change. When you look at OHC, you do not need to assume
>> >> >> a
>> >> >> heat capacity/depth. That's built into the calculation.
>> >> >>
>> >> >> > Besides, the Levitus data show exactly what I was talking about,
>> >> >> > like
>> >> >> > the CNRM-CM3 model does, with a change in sign in the anomalies

>> >> >> > with
>> >> >> > depth.
>> >> >>
>> >> >> One of us has a bug in their code. As you integrate deeper the
>> >> >> answer
>> >> >> you get with your method must converge to the answer I get with mine
>> >> >> (assuming your using data, as I am). I'll email Steve McIntyre to
>> >> >> settle this. He can independently audit our results. I admit that
>> >> >> there might be a bug in my code, and I'd love to find that out if
>> >> >> so.
>> >> >>
>> >> >> > I now have results from MIROC-hires, S/N is about 1.8:1 to 1.9:1,
>> >> >> > *independent* of the depth you average over.
>> >> >>
>> >> >> Let's stick with data here.
>> >> >>
>> >> >> > Andy, *I'm* not the one trying to defend the CMIP models. I'm
>> >> >> > using
>> >> >> > YOUR
>> >> >> > arguments with the models YOU support, to show you are wrong.
>> >> >>
>> >> >> Stop it, Roy. Really.
>> >> >>
>> >> >> > -Roy
>> >> >> >
>> >> >> > _____
>> >> >> > From: adessler@tamu.edu
>> >> >> > Date: Thu, 8 Sep 2011 12:49:21 -0500
>> >> >> > Subject: Re: ocean heat content
>> >> >> > To:
>> >> >> >
>> >> >> > Are you trying to argue with the Levitus OHC data with data from a
>> >> >> > model?? Wow, even an "alarmist" like me wouldn't try that. You
>> >> >> > need
>> >> >> > to use
>> >> >> > data here. As you integrate to greater depth, the number from
>> >> >> > your
>> >> >> > method
>> >> >> > must converge with mine (about 5 W/m2 for seasonal OHC changes
>> >> >> > from
>> >> >> > Levitus). Thus, your claims make no sense. I suggest you proceed
>> >> >> > carefully, too.
>> >> >> >
>> >> >> > On Thu, Sep 8, 2011 at 11:39 AM, Roy Spencer
>> >> >> > <
>> >> >> > wrote:
>> >> >> >
>> >> >> > Andy:
>> >> >> >
>> >> >> > Thanks for the best wishes on our next paper.
>> >> >> >
>> >> >> > It's interesting that you mention the use of deeper layers,
>> >> >> > because
>> >> >> > I'm
>> >> >> > actually computing S/N from the CNRM-CM3 model now, using TOA
>> >> >> > fluxes,
>> >> >> > model
>> >> >> > SST and ocean layer temperatures, and the F&T diagnosed feedback
>> >> >> > net
>> >> >> > parameter.

>> >> >> >
>> >> >> > This is better than using standard deviations to get estimated
>> >> >> > magnitudes for S and N, since one can do an energy-conserving
>> >> >> > calculation.
>> >> >> > (BTW, based upon the AR4 report CNRM-CM3 arguably has the most
>> >> >> > realistic
>> >> >> > ENSO (certainly the strongest) variability in the NINO3 region.)
>> >> >> >
>> >> >> > Here's what I get for various assumed layer depths for the global
>> >> >> > oceans:
>> >> >> >
>> >> >> > 0-100 meters: S/N = 3.8
>> >> >> >
>> >> >> > 0-200 meters: S/N = 2.2
>> >> >> >
>> >> >> > 0-300 meters: S/N = 1.7
>> >> >> >
>> >> >> > You see, as you go deeper, even though it takes more W/m2 to
>> >> >> > change
>> >> >> > the
>> >> >> > temperature of the full layer (thus inflating the term on the left
>> >> >> > side of
>> >> >> > the equation), the temperature variability at deeper levels
>> >> >> > becomes
>> >> >> > not only
>> >> >> > smaller, it becomes negatively correlated with the temperature
>> >> >> > changes in
>> >> >> > the shallower layers.
>> >> >> >
>> >> >> > The net result is that S/N becomes SMALLER with assumed ML depth,
>> >> >> > NOT
>> >> >> > greater.
>> >> >> >
>> >> >> > I suggest you proceed carefully.
>> >> >> >
>> >> >> > -Roy
>> >> >> >
>> >> >> > _____
>> >> >> > From: adessler@tamu.edu
>> >> >> > Date: Thu, 8 Sep 2011 10:48:08 -0500
>> >> >> > Subject: Re: ocean heat content
>> >> >> > To:
>> >> >> >
>> >> >> > 100 m is a global average. In some regions, heat transport is
>> >> >> > much
>> >> >> > deeper. By doing cutting off at 100 m everywhere, you're
>> >> >> > underestimating
>> >> >> > the heat content changes of the ocean.
>> >> >> >
>> >> >> > The correct way to do it is to use the total heat content of the
>> >> >> > ocean
>> >> >> > (but 700 m will have to suffice if you use this data set), which
>> >> >> > avoids
>> >> >> > cutting off any heat content changes. That calculation agrees
>> >> >> > with
>> >> >> > the heat
>> >> >> > content calculation from the surface T data.
>> >> >> >
>> >> >> > The difference in data sets between our analyses is a complete red
>> >> >> > herring --- it's all in the assumptions, particularly how you've

>> >> >> > done
>> >> >> > the
>> >> >> > ocean heat capacity.
>> >> >> >
>> >> >> > And this does not even begin to address the issue of using
>> >> >> > seasonal
>> >> >> > vs.
>> >> >> > monthly. That's a whole other issue.
>> >> >> >
>> >> >> > But the good news is that you did answer my question and confirmed
>> >> >> > everything that I suspected about your rebuttal. Good luck
>> >> >> > getting
>> >> >> > this
>> >> >> > calculation through peer review. My recommendation is not to send
>> >> >> > it
>> >> >> > to
>> >> >> > Remote Sensing.
>> >> >> >
>> >> >> > On Thu, Sep 8, 2011 at 10:26 AM, Roy Spencer
>> >> >> > <
>> >> >> > wrote:
>> >> >> >
>> >> >> > Hi Andy:
>> >> >> >
>> >> >> > We used Levitus global gridpoint ocean temperatures at depth,
>> >> >> > which
>> >> >> > have
>> >> >> > 3-month temperature anomalies at 0, 10, 20, 30, 50, 75, 100
>> >> >> > meters,
>> >> >> > etc.
>> >> >> >
>> >> >> > Danny Braswell is the one who downloaded the dataset and computed
>> >> >> > area
>> >> >> > average (cosine-latitude weighted) anomalies for the global
>> >> >> > ice-free
>> >> >> > oceans
>> >> >> > (60N-60S).
>> >> >> >
>> >> >> > The global at-depth anomalies were then averaged from the surface
>> >> >> > to
>> >> >> > 100
>> >> >> > m depth, using a weighted average of the top 7 levels using the
>> >> >> > weights: 5,
>> >> >> > 10, 10, 15, 22.5, 25, and 12.5
>> >> >> >
>> >> >> > This then gives 3-month temperature anomalies for the 0-100 meter
>> >> >> > layer.
>> >> >> >
>> >> >> > Then do all of the 0-100m layer average temperature changes, from
>> >> >> > one
>> >> >> > 3-month period to the next,
>> >> >> >
>> >> >> > The standard deviation of those anomalies is 0.04332 deg. C
>> >> >> >
>> >> >> > Then convert this to Watts per sq meter...I assumed 91 days for
>> >> >> > delta-time.
>> >> >> >
>> >> >> > -Roy
>> >> >> >
>> >> >> >

>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> >
>> >> >> > > From: adessler@tamu.edu
>> >> >> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
>> >> >> > > Subject: ocean heat content
>> >> >> > > To:
>> >> >> > > spencer@nsstc.uah.edu
>> >> >> > >
>> >> >> > > Hi Roy.
>> >> >> > >
>> >> >> > > Enjoyed your blog post -- always good to get a scientific
>> >> >> > > challenge.
>> >> >> > >
>> >> >> > > I'm working through your numbers, and I'm stumped on the first
>> >> >> > > one:
>> >> >> > > 2.3 W/m2 for the standard deviation of ocean heat content. I
>> >> >> > > have
>> >> >> > > the
>> >> >> > > Levitus data and have done the season minus following season
>> >> >> > > subtraction to get the change over the season, and then took the
>> >> >> > > standard deviation (2000-2010) ... which gives me 1.3 10^22 J/(3
>> >> >> > > months). Converting this to W/m2 gives me about 4.7 W/m2 --
>> >> >> > > almost
>> >> >> > > exactly a factor of two higher than your number. I may have made
>> >> >> > > an
>> >> >> > > error, or perhaps you're doing something that I'm not. Do you
>> >> >> > > have
>> >> >> > > any idea what it is? Also, you mention that you assumed a 100-m
>> >> >> > > ocean, but I don't see where you have to assume an ocean depth
>> >> >> > > at
>> >> >> > > all
>> >> >> > > when you're using OHC. I'd appreciate your input here.
>> >> >> > >
>> >> >> > > Also, FYI, I'm happy to change the introductory paragraph of my
>> >> >> > > paper
>> >> >> > > when I get the galley proofs to better represent your views. My
>> >> >> > > apologies for any misunderstanding. Also, I'll be changing the
>> >> >> > > sentence "over the decades or centuries relevant for long-term
>> >> >> > > climate
>> >> >> > > change, on the other hand, clouds can indeed cause significant
>> >> >> > > warming" to make it clear that I'm talking about cloud feedbacks
>> >> >> > > doing
>> >> >> > > the action here, not cloud forcing. Please make sure you
>> >> >> > > correctly
>> >> >> > > represent my views in the future.
>> >> >> > >
>> >> >> > > Thanks!
>> >> >> > >
>> >> >> > >
>> >> >> > > --
>> >> >> > > Andrew Dessler
>> >> >> > > Professor of Atmospheric Sciences
>> >> >> > > Texas A&M University
>> >> >> > > adessler@tamu.edu

>> >> >> > > 979-862-1427
>> >> >> > > http://atmo.tamu.edu/profile/ADessler
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--
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From: Steve McIntyre
To: "Andrew Dessler"; "Roy Spencer"
Subject: RE: [Bulk] Re: ocean heat content
Date: Friday, September 09, 2011 9:16:41 AM

As a small point on the first calc, I got a std dev of 13.8 rather than 13.4 starting in 2000. Here is my replication in R. Note a nice feature of R - that it has a facility for directly downloading the *.nc data within the script and then handling the results. The structure of the code is pretty similar to the Py structure.

```
#OHC Calc

download.file("ftp://ftp.nodc.noaa.gov/pub/data.nodc/woa/DATA_ANALYSIS/3M_HE
AT_CONTENT/NETCDF/seasonal_ocean_heat_content.nc","temp.nc",mode="wb")
      ohc.nc=open.ncdf("temp.nc")
      names(ohc$var)
        #[1] "lat_bnds" "lon_bnds" "ohc"      "volume"   "crs"
      names(ohc.nc$dim) #     [1] "lon"  "lat"  "time" "nv"

      ohc = get.var.ncdf(ohc.nc,"crs")
        dim(ohc) #[1] 360 180 224
      x= sapply(ohc.nc$dim, function(A) A$vals)
      x[[3]]=1955+x[[3]]
      for(i in 1:3) dimnames(ohc)[[i]]= x[[i]]
      ohc[ abs(ohc)> 200]=NA
        range(ohc,na.rm=T) #[1] -177.781  154.303
      ohc=ohc/1000

# calculate total heat content
      gohc=rep(NA,224)
      for (i in 1:224)  gohc[i]=sum(ohc[,,i],na.rm=T) # in units of 10^21
      dG= ts( c(NA,diff(gohc)),start=c(1955,1),freq=4)
      sd(window(dG,start=2000.1))
        #[1] 13.81529
        # This is about 13.4 (units of 10^21 J), which is about 4.7 W/m2
```

-----Original Message-----
From: [mailto: On Behalf Of Andrew Dessler
Sent: September-08-11 5:56 PM
To: Roy Spencer
Cc:
Subject: [Bulk] Re: ocean heat content

Wow, that was easy. Just have to know the right people to cc, I guess.

The difference between the number using Roy's data (Levitus), about 5 W/m2, and mine (Argo), about 13 W/m2, is not due to data, but because Levitus are 3-mo averages while Argo are monthly. I've verified that by averaging down Argo data. I am not surprised that averaging the data set temporally reduces its standard deviation. Both go down ~700 m, which is probably OK for this problem.

Off the top of my head, I can't think of any reason why using the highest temporal resolution data would be bad ... and I can think of reasons why averaging the data would be misleading. I'll think more

about this while I verify Roy's other numbers.

But rest assured, Roy, if you can convince me that using monthly data is wrong, then I'll change the paper when I get the galleys.

BTW, I hope to see a correction on your blog with the new number soon.

So now, as suggested by Roy, I'm going to start looking at terms on the RHS ... first up, deltaR. Steve has kindly provided me with Roy's data, so I'm all set! Hopefully I'll get the same number as Roy, but if I don't, then we can call in Steve to adjudicate.

Thanks for all your help.

On Thu, Sep 8, 2011 at 3:56 PM, Roy Spencer < wrote:
> Andy (& Steve):
>
> OK, I think we have pretty close agreement on the number Andy is
> calculating.
>
> I understand better what you are doing *now*, Andy (but not what you did
in
> the preprint). Your preprint was not clear...you mentioned 100 m, and you
> also used Lindzen's 186 number which scales to 104 m.
>
> We now also get about 5 Watts/m2 using the full 4-D Levitus temperature
> dataset.
>
>
> So, I now know what you did on the LEFT hand side of the equation. That's
a
> start.
>
>
> So, are you going to change your paper then? Because the preprint has 9
and
> 13 as two estimates for the LHS....not 5.
>
>
> -Roy
>
>> From: adessler@tamu.edu
>> Date: Thu, 8 Sep 2011 14:08:23 -0500
>> Subject: Fwd: ocean heat content
>> To:
>> CC:
>>
>> Steve-
>>
>> Since I've been helping you understand Dessler 2010, I'm wondering if
>> you'd do me and Roy a favor. Please read our exchange below, from the
>> bottom up.
>>
>> Can you do a calculation of the time series of the total heat content
>> of the ocean? The Levitus heat capacity data is here:
>> http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/heat_global.html
>> All you have to do is calculate the time series of total heat content
>> of the ocean (in J) and then do a month minus month difference (as a
>> crude derivative) and then take the standard deviation of the
>> 2000-2010 period.

>>
>> The number I get is about 4.7 W/m2. Roy gets about half that number,
>> using a different method. We'd love to find out who's right (I
>> certainly acknowledge I could have a bug in my code). And you'll
>> probably get a blog post out of it.
>>
>> I did get your last question and will respond ... probably about the
>> time you get back to me with an answer to this question. :)
>>
>> Thanks!
>>
>> PS: I don't know if you read python, but I've attached my code.
>> Perhaps you can post this on your blog and get readers to check my
>> math.
>>
>>
>> ---------- Forwarded message ----------
>> From: Andrew Dessler <adessler@tamu.edu>
>> Date: Thu, Sep 8, 2011 at 1:11 PM
>> Subject: Re: ocean heat content
>> To: Roy Spencer <
>>
>>
>> > Do you realize the 186 W-month/m2/K number in your paper corresponds to
>> > 104 m depth mixed layer? So, didn't you already assume 100 m in your
>> > argument there?
>>
>> No. You only assume 100 m when you use surface T to figure out the
>> temperature change. When you look at OHC, you do not need to assume a
>> heat capacity/depth. That's built into the calculation.
>>
>> > Besides, the Levitus data show exactly what I was talking about, like
>> > the CNRM-CM3 model does, with a change in sign in the anomalies with
depth.
>>
>> One of us has a bug in their code. As you integrate deeper the answer
>> you get with your method must converge to the answer I get with mine
>> (assuming your using data, as I am). I'll email Steve McIntyre to
>> settle this. He can independently audit our results. I admit that
>> there might be a bug in my code, and I'd love to find that out if so.
>>
>> > I now have results from MIROC-hires, S/N is about 1.8:1 to 1.9:1,
>> > *independent* of the depth you average over.
>>
>> Let's stick with data here.
>>
>> > Andy, *I'm* not the one trying to defend the CMIP models. I'm using
YOUR
>> > arguments with the models YOU support, to show you are wrong.
>>
>> Stop it, Roy. Really.
>>
>> > -Roy
>> >
>> > _____
>> > From: adessler@tamu.edu
>> > Date: Thu, 8 Sep 2011 12:49:21 -0500
>> > Subject: Re: ocean heat content
>> > To:
>> >

>> > Are you trying to argue with the Levitus OHC data with data from a
>> > model?? Wow, even an "alarmist" like me wouldn't try that. You need
to use
>> > data here. As you integrate to greater depth, the number from your
method
>> > must converge with mine (about 5 W/m2 for seasonal OHC changes from
>> > Levitus). Thus, your claims make no sense. I suggest you proceed
>> > carefully, too.
>> >
>> > On Thu, Sep 8, 2011 at 11:39 AM, Roy Spencer <
>> > wrote:
>> >
>> > Andy:
>> >
>> > Thanks for the best wishes on our next paper.
>> >
>> > It's interesting that you mention the use of deeper layers, because I'm
>> > actually computing S/N from the CNRM-CM3 model now, using TOA fluxes,
model
>> > SST and ocean layer temperatures, and the F&T diagnosed feedback net
>> > parameter.
>> >
>> > This is better than using standard deviations to get estimated
>> > magnitudes for S and N, since one can do an energy-conserving
calculation.
>> > (BTW, based upon the AR4 report CNRM-CM3 arguably has the most
realistic
>> > ENSO (certainly the strongest) variability in the NINO3 region.)
>> >
>> > Here's what I get for various assumed layer depths for the global
>> > oceans:
>> >
>> > 0-100 meters: S/N = 3.8
>> >
>> > 0-200 meters: S/N = 2.2
>> >
>> > 0-300 meters: S/N = 1.7
>> >
>> > You see, as you go deeper, even though it takes more W/m2 to change the
>> > temperature of the full layer (thus inflating the term on the left side
of
>> > the equation), the temperature variability at deeper levels becomes not
only
>> > smaller, it becomes negatively correlated with the temperature changes
in
>> > the shallower layers.
>> >
>> > The net result is that S/N becomes SMALLER with assumed ML depth, NOT
>> > greater.
>> >
>> > I suggest you proceed carefully.
>> >
>> > -Roy
>> >
>> > _____
>> > From: adessler@tamu.edu
>> > Date: Thu, 8 Sep 2011 10:48:08 -0500
>> > Subject: Re: ocean heat content
>> > To:
>> >

>> > 100 m is a global average. In some regions, heat transport is much
>> > deeper. By doing cutting off at 100 m everywhere, you're
underestimating
>> > the heat content changes of the ocean.
>> >
>> > The correct way to do it is to use the total heat content of the ocean
>> > (but 700 m will have to suffice if you use this data set), which avoids
>> > cutting off any heat content changes. That calculation agrees with the
heat
>> > content calculation from the surface T data.
>> >
>> > The difference in data sets between our analyses is a complete red
>> > herring --- it's all in the assumptions, particularly how you've done
the
>> > ocean heat capacity.
>> >
>> > And this does not even begin to address the issue of using seasonal vs.
>> > monthly. That's a whole other issue.
>> >
>> > But the good news is that you did answer my question and confirmed
>> > everything that I suspected about your rebuttal. Good luck getting
this
>> > calculation through peer review. My recommendation is not to send it
to
>> > Remote Sensing.
>> >
>> > On Thu, Sep 8, 2011 at 10:26 AM, Roy Spencer <
>> > wrote:
>> >
>> > Hi Andy:
>> >
>> > We used Levitus global gridpoint ocean temperatures at depth, which
have
>> > 3-month temperature anomalies at 0, 10, 20, 30, 50, 75, 100 meters,
etc.
>> >
>> > Danny Braswell is the one who downloaded the dataset and computed area
>> > average (cosine-latitude weighted) anomalies for the global ice-free
oceans
>> > (60N-60S).
>> >
>> > The global at-depth anomalies were then averaged from the surface to
100
>> > m depth, using a weighted average of the top 7 levels using the
weights: 5,
>> > 10, 10, 15, 22.5, 25, and 12.5
>> >
>> > This then gives 3-month temperature anomalies for the 0-100 meter
layer.
>> >
>> > Then do all of the 0-100m layer average temperature changes, from one
>> > 3-month period to the next,
>> >
>> > The standard deviation of those anomalies is 0.04332 deg. C
>> >
>> > Then convert this to Watts per sq meter...I assumed 91 days for
>> > delta-time.
>> >
>> > -Roy
>> >

>> >
>> >
>> >
>> >
>> >
>> >
>> >
>> >
>> >
>> > > From: adessler@tamu.edu
>> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
>> > > Subject: ocean heat content
>> > > To:
>> > > spencer@nsstc.uah.edu
>> > >
>> > > Hi Roy.
>> > >
>> > > Enjoyed your blog post -- always good to get a scientific challenge.
>> > >
>> > > I'm working through your numbers, and I'm stumped on the first one:
>> > > 2.3 W/m2 for the standard deviation of ocean heat content. I have the
>> > > Levitus data and have done the season minus following season
>> > > subtraction to get the change over the season, and then took the
>> > > standard deviation (2000-2010) ... which gives me 1.3 10^22 J/(3
>> > > months). Converting this to W/m2 gives me about 4.7 W/m2 -- almost
>> > > exactly a factor of two higher than your number. I may have made an
>> > > error, or perhaps you're doing something that I'm not. Do you have
>> > > any idea what it is? Also, you mention that you assumed a 100-m
>> > > ocean, but I don't see where you have to assume an ocean depth at all
>> > > when you're using OHC. I'd appreciate your input here.
>> > >
>> > > Also, FYI, I'm happy to change the introductory paragraph of my paper
>> > > when I get the galley proofs to better represent your views. My
>> > > apologies for any misunderstanding. Also, I'll be changing the
>> > > sentence "over the decades or centuries relevant for long-term
climate
>> > > change, on the other hand, clouds can indeed cause significant
>> > > warming" to make it clear that I'm talking about cloud feedbacks
doing
>> > > the action here, not cloud forcing. Please make sure you correctly
>> > > represent my views in the future.
>> > >
>> > > Thanks!
>> > >
>> > >
>> > > --
>> > > Andrew Dessler
>> > > Professor of Atmospheric Sciences
>> > > Texas A&M University
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--
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979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Roy Spencer
To: adessler@tamu.edu
Subject: RE: ocean heat content
Date: Friday, September 09, 2011 8:08:12 AM

Andy:

1) I stand by my CERES calculations (sd=0.53), which are indeed based upon 3-monthly averages. Monthly gives 0.73. Did you use *60N-60S* oceans radiative flux? That's the EXACT same region we computed the ocean temperatures for. Apples & apples. Do you know exactly what region your Levitus ocean heat content data is for?

2) Andy, you really need to understand ocean heat fluxes better. We have analyzed the vertical structures of temperature variations in (1) Levitus, (2) the CMIP3 models, and (3) in our own ocean diffusion model. Variations in mixing BELOW the thermocline can cause WARMING below the thermocline. For example, in the Levitus data, temperature changes at 300 meters depth tend to be PRECEDED by warming BELOW that level, and then FOLLOWED by temperature changes ABOVE that level (even up to 100 meters).

You are making a major assumption when you assume ALL of the energy flux affecting the 0-700 m layer only comes through the TOP boundary (the ocean surface).

Andy, I am finished doing your homework for you. That's enough.

-Roy

> From: adessler@tamu.edu
> Date: Thu, 8 Sep 2011 22:59:48 -0500
> Subject: Re: ocean heat content
> To:
>
> Hi guys-
>
> Two issues. First, I spent ~2 minutes reproducing Roy's dR number
> (based on data downloaded from Steve's website) and I can reproduce
> it, but only if I use the monthly data. If I do a 3-month average on
> the CERES data before doing the standard deviation, I get a value
> smaller than the one on Roy's website. So, my question to Roy is
> whether he averaged the CERES data down to 3 months to match the
> Levitus data? Or, if he's using 3-month Levitus data but 1-month
> CERES data?
>
> BTW, here is my python code:
> d1=np.loadtxt('spencer.txt',skiprows=1,delimiter=',') # edit file to
> replace ',,' with ',0,'
> crf=d1[:,4]-d1[:,7] # calculate CRF
> crf2=[np.average(crf[ii*3:ii*3+3]) for ii in range(len(crf)/3)] #
> average down to 3 months
> 'standard deviation of 3-month avg.: %f' % np.std(crf2)
> 'standard deviation of monthly.: %f' % np.std(crf)
>
> output:
> Out[7]: 'standard deviation of 3-month avg.: 0.411202'
> Out[8]: 'standard deviation of monthly.: 0.579952'
>
> As far as you adherence to 100 m goes, you can certainly do that, but
> then you have to do one of the following:
> 1) put into your energy balance equation an additional term for flow
> of heat out of the 100-m layer into the deep ocean. Without that,
> your equation does not conserve energy. How big is that term going to

> be? It's the difference between your 2.3 W/m2 number and my 4.7 W/m2,
> so adding that term to your 2.3 number produces a net flow into the
> ocean that's the same as my 700-m number. Net result: no soup for
> you.
> 2) argue that heat flux out of the bottom of your 100-m layer is zero.
> This makes it hard to explain temperature variations below 100 m,
> though. No soup.
> 3) declare that conservation of energy doesn't apply in the
> post-normal climate change debate.
>
> Sadly, I suspect your best option is #3.
>
> Regarding your argument that the Argo data is noisy, I'll e-mail Josh
> Willis at JPL to see if the experts agree with that.
>
> Looking forward to Roy's response about his dR values. If Roy does
> not confirm my suspicion that Roy's using monthly CERES data, then
> perhaps Steve M. can spend 1-2 minutes doing the same calculation ...
> it will be trivial for him to confirm my or Roy's calculation.
>
> Best wishes.
>
>
> On Thu, Sep 8, 2011 at 6:33 PM, Roy Spencer < wrote:
> > Andy:
> >
> > Not so quick on the draw there pardner...I stlll see 2 issues here before I
> > change my estimates on my blog:
> >
> > 1) DEPTH SCALE OF AVERAGING: I think there are questions about what ocean
> > layer depth should be used for this: my calculations are based upon the
> > average depth to the middle of the global average thermocline (~100m), as
> > seen (for example) in this movie we made of the temperature variations with
> > depth. You have now chosen to use 700 m. Remember, we are discussing the
> > degree to which *interannual* surface temperature variability can be
> > affected by turbulent mixing versus unforced TOA radiative variations. If
> > you would get the actual depth level temperature data and analyze them, you
> > will see there is little interannual variability down there....but there is
> > a WARMING TREND which, as you know, can be caused by CONSTANT radiative
> > forcing over a long period of time, e.g. from CO2. So, it is not clear at
> > all what depth should be used in these calculations....100 m, 700 m or
> > somewhere in between. Also, the possible need to detrend the temperature
> > time series first should be addressed.
> >
> > 2) TIME SCALE OF AVERAGING Regarding using monthly ocean temperature data,
> > rather than 3-monthly, I can guarantee you there is a noise issue here: the
> > standard deviation will be inflated at that time scale. If you don't
> > believe me, try making the monthly global radiance anomalies match
> > month-to-month changes in ocean heat content. 3-month is much less noisy.
> > It could be even a little more time averaging is justified.
> >
> > No, my calculations at drroyspencer.com still stand until someone
> > demonstrates (1) why 700 m (or any depth) should be used instead of 100 m,
> > and (2) how much time averaging should be used to reduce noise.
> >
> > Nevertheless, I encourage you to publish what you have, ASAP.
> >
> > -Roy
> >
> >> From: adessler@tamu.edu

> >> Date: Thu, 8 Sep 2011 16:55:35 -0500
> >> Subject: Re: ocean heat content
> >> To:
> >> CC:
> >>
> >> Wow, that was easy. Just have to know the right people to cc, I guess.
> >>
> >> The difference between the number using Roy's data (Levitus), about 5
> >> W/m2, and mine (Argo), about 13 W/m2, is not due to data, but because
> >> Levitus are 3-mo averages while Argo are monthly. I've verified that
> >> by averaging down Argo data. I am not surprised that averaging the
> >> data set temporally reduces its standard deviation. Both go down ~700
> >> m, which is probably OK for this problem.
> >>
> >> Off the top of my head, I can't think of any reason why using the
> >> highest temporal resolution data would be bad ... and I can think of
> >> reasons why averaging the data would be misleading. I'll think more
> >> about this while I verify Roy's other numbers.
> >>
> >> But rest assured, Roy, if you can convince me that using monthly data
> >> is wrong, then I'll change the paper when I get the galleys.
> >>
> >> BTW, I hope to see a correction on your blog with the new number soon.
> >>
> >> So now, as suggested by Roy, I'm going to start looking at terms on
> >> the RHS ... first up, deltaR. Steve has kindly provided me with Roy's
> >> data, so I'm all set! Hopefully I'll get the same number as Roy, but
> >> if I don't, then we can call in Steve to adjudicate.
> >>
> >> Thanks for all your help.
> >>
> >> On Thu, Sep 8, 2011 at 3:56 PM, Roy Spencer <
> >> wrote:
> >> > Andy (& Steve):
> >> >
> >> > OK, I think we have pretty close agreement on the number Andy is
> >> > calculating.
> >> >
> >> > I understand better what you are doing *now*, Andy (but not what you did
> >> > in
> >> > the preprint). Your preprint was not clear...you mentioned 100 m, and
> >> > you
> >> > also used Lindzen's 186 number which scales to 104 m.
> >> >
> >> > We now also get about 5 Watts/m2 using the full 4-D Levitus temperature
> >> > dataset.
> >> >
> >> >
> >> > So, I now know what you did on the LEFT hand side of the equation.
> >> > That's a
> >> > start.
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> >> >
> >> > So, are you going to change your paper then? Because the preprint has 9
> >> > and
> >> > 13 as two estimates for the LHS....not 5.
> >> >
> >> >
> >> > -Roy
> >> >

> >> >> From: adessler@tamu.edu
> >> >> Date: Thu, 8 Sep 2011 14:08:23 -0500
> >> >> Subject: Fwd: ocean heat content
> >> >> To:
> >> >> CC:
> >> >>
> >> >> Steve-
> >> >>
> >> >> Since I've been helping you understand Dessler 2010, I'm wondering if
> >> >> you'd do me and Roy a favor. Please read our exchange below, from the
> >> >> bottom up.
> >> >>
> >> >> Can you do a calculation of the time series of the total heat content
> >> >> of the ocean? The Levitus heat capacity data is here:
> >> >> http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/heat_global.html
> >> >> All you have to do is calculate the time series of total heat content
> >> >> of the ocean (in J) and then do a month minus month difference (as a
> >> >> crude derivative) and then take the standard deviation of the
> >> >> 2000-2010 period.
> >> >>
> >> >> The number I get is about 4.7 W/m2. Roy gets about half that number,
> >> >> using a different method. We'd love to find out who's right (I
> >> >> certainly acknowledge I could have a bug in my code). And you'll
> >> >> probably get a blog post out of it.
> >> >>
> >> >> I did get your last question and will respond ... probably about the
> >> >> time you get back to me with an answer to this question. :)
> >> >>
> >> >> Thanks!
> >> >>
> >> >> PS: I don't know if you read python, but I've attached my code.
> >> >> Perhaps you can post this on your blog and get readers to check my
> >> >> math.
> >> >>
> >> >>
> >> >> ---------- Forwarded message ----------
> >> >> From: Andrew Dessler <adessler@tamu.edu>
> >> >> Date: Thu, Sep 8, 2011 at 1:11 PM
> >> >> Subject: Re: ocean heat content
> >> >> To: Roy Spencer <
> >> >>
> >> >>
> >> >> > Do you realize the 186 W-month/m2/K number in your paper corresponds
> >> >> > to
> >> >> > 104 m depth mixed layer? So, didn't you already assume 100 m in your
> >> >> > argument there?
> >> >>
> >> >> No. You only assume 100 m when you use surface T to figure out the
> >> >> temperature change. When you look at OHC, you do not need to assume a
> >> >> heat capacity/depth. That's built into the calculation.
> >> >>
> >> >> > Besides, the Levitus data show exactly what I was talking about, like
> >> >> > the CNRM-CM3 model does, with a change in sign in the anomalies with
> >> >> > depth.
> >> >>
> >> >> One of us has a bug in their code. As you integrate deeper the answer
> >> >> you get with your method must converge to the answer I get with mine
> >> >> (assuming your using data, as I am). I'll email Steve McIntyre to
> >> >> settle this. He can independently audit our results. I admit that
> >> >> there might be a bug in my code, and I'd love to find that out if so.

> >> >>
> >> >> > I now have results from MIROC-hires, S/N is about 1.8:1 to 1.9:1,
> >> >> > *independent* of the depth you average over.
> >> >>
> >> >> Let's stick with data here.
> >> >>
> >> >> > Andy, *I'm* not the one trying to defend the CMIP models. I'm using
> >> >> > YOUR
> >> >> > arguments with the models YOU support, to show you are wrong.
> >> >>
> >> >> Stop it, Roy. Really.
> >> >>
> >> >> > -Roy
> >> >> >
> >> >> > _____
> >> >> > From: adessler@tamu.edu
> >> >> > Date: Thu, 8 Sep 2011 12:49:21 -0500
> >> >> > Subject: Re: ocean heat content
> >> >> > To:
> >> >> >
> >> >> > Are you trying to argue with the Levitus OHC data with data from a
> >> >> > model?? Wow, even an "alarmist" like me wouldn't try that. You need
> >> >> > to use
> >> >> > data here. As you integrate to greater depth, the number from your
> >> >> > method
> >> >> > must converge with mine (about 5 W/m2 for seasonal OHC changes from
> >> >> > Levitus). Thus, your claims make no sense. I suggest you proceed
> >> >> > carefully, too.
> >> >> >
> >> >> > On Thu, Sep 8, 2011 at 11:39 AM, Roy Spencer
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> >> >> > wrote:
> >> >> >
> >> >> > Andy:
> >> >> >
> >> >> > Thanks for the best wishes on our next paper.
> >> >> >
> >> >> > It's interesting that you mention the use of deeper layers, because
> >> >> > I'm
> >> >> > actually computing S/N from the CNRM-CM3 model now, using TOA fluxes,
> >> >> > model
> >> >> > SST and ocean layer temperatures, and the F&T diagnosed feedback net
> >> >> > parameter.
> >> >> >
> >> >> > This is better than using standard deviations to get estimated
> >> >> > magnitudes for S and N, since one can do an energy-conserving
> >> >> > calculation.
> >> >> > (BTW, based upon the AR4 report CNRM-CM3 arguably has the most
> >> >> > realistic
> >> >> > ENSO (certainly the strongest) variability in the NINO3 region.)
> >> >> >
> >> >> > Here's what I get for various assumed layer depths for the global
> >> >> > oceans:
> >> >> >
> >> >> > 0-100 meters: S/N = 3.8
> >> >> >
> >> >> > 0-200 meters: S/N = 2.2
> >> >> >
> >> >> > 0-300 meters: S/N = 1.7
> >> >> >

> >> >> > You see, as you go deeper, even though it takes more W/m2 to change
> >> >> > the
> >> >> > temperature of the full layer (thus inflating the term on the left
> >> >> > side of
> >> >> > the equation), the temperature variability at deeper levels becomes
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> >> >> > smaller, it becomes negatively correlated with the temperature
> >> >> > changes in
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> >> >> > 100 m is a global average. In some regions, heat transport is much
> >> >> > deeper. By doing cutting off at 100 m everywhere, you're
> >> >> > underestimating
> >> >> > the heat content changes of the ocean.
> >> >> >
> >> >> > The correct way to do it is to use the total heat content of the
> >> >> > ocean
> >> >> > (but 700 m will have to suffice if you use this data set), which
> >> >> > avoids
> >> >> > cutting off any heat content changes. That calculation agrees with
> >> >> > the heat
> >> >> > content calculation from the surface T data.
> >> >> >
> >> >> > The difference in data sets between our analyses is a complete red
> >> >> > herring --- it's all in the assumptions, particularly how you've done
> >> >> > the
> >> >> > ocean heat capacity.
> >> >> >
> >> >> > And this does not even begin to address the issue of using seasonal
> >> >> > vs.
> >> >> > monthly. That's a whole other issue.
> >> >> >
> >> >> > But the good news is that you did answer my question and confirmed
> >> >> > everything that I suspected about your rebuttal. Good luck getting
> >> >> > this
> >> >> > calculation through peer review. My recommendation is not to send it
> >> >> > to
> >> >> > Remote Sensing.
> >> >> >
> >> >> > On Thu, Sep 8, 2011 at 10:26 AM, Roy Spencer
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> >> >> > wrote:
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> >> >> > Hi Andy:
> >> >> >
> >> >> > We used Levitus global gridpoint ocean temperatures at depth, which
> >> >> > have

> >> >> > 3-month temperature anomalies at 0, 10, 20, 30, 50, 75, 100 meters,
> >> >> > etc.
> >> >> >
> >> >> > Danny Braswell is the one who downloaded the dataset and computed
> >> >> > area
> >> >> > average (cosine-latitude weighted) anomalies for the global ice-free
> >> >> > oceans
> >> >> > (60N-60S).
> >> >> >
> >> >> > The global at-depth anomalies were then averaged from the surface to
> >> >> > 100
> >> >> > m depth, using a weighted average of the top 7 levels using the
> >> >> > weights: 5,
> >> >> > 10, 10, 15, 22.5, 25, and 12.5
> >> >> >
> >> >> > This then gives 3-month temperature anomalies for the 0-100 meter
> >> >> > layer.
> >> >> >
> >> >> > Then do all of the 0-100m layer average temperature changes, from one
> >> >> > 3-month period to the next,
> >> >> >
> >> >> > The standard deviation of those anomalies is 0.04332 deg. C
> >> >> >
> >> >> > Then convert this to Watts per sq meter...I assumed 91 days for
> >> >> > delta-time.
> >> >> >
> >> >> > -Roy
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> >> >> >
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> >> >> >
> >> >> >
> >> >> >
> >> >> >
> >> >> >
> >> >> >
> >> >> >
> >> >> > > From: adessler@tamu.edu
> >> >> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
> >> >> > > Subject: ocean heat content
> >> >> > > To:
> >> >> > > spencer@nsstc.uah.edu
> >> >> > >
> >> >> > > Hi Roy.
> >> >> > >
> >> >> > > Enjoyed your blog post -- always good to get a scientific
> >> >> > > challenge.
> >> >> > >
> >> >> > > I'm working through your numbers, and I'm stumped on the first one:
> >> >> > > 2.3 W/m2 for the standard deviation of ocean heat content. I have
> >> >> > > the
> >> >> > > Levitus data and have done the season minus following season
> >> >> > > subtraction to get the change over the season, and then took the
> >> >> > > standard deviation (2000-2010) ... which gives me 1.3 10^22 J/(3
> >> >> > > months). Converting this to W/m2 gives me about 4.7 W/m2 -- almost
> >> >> > > exactly a factor of two higher than your number. I may have made an
> >> >> > > error, or perhaps you're doing something that I'm not. Do you have
> >> >> > > any idea what it is? Also, you mention that you assumed a 100-m
> >> >> > > ocean, but I don't see where you have to assume an ocean depth at
> >> >> > > all

> >> >> > > when you're using OHC. I'd appreciate your input here.
> >> >> > >
> >> >> > > Also, FYI, I'm happy to change the introductory paragraph of my
> >> >> > > paper
> >> >> > > when I get the galley proofs to better represent your views. My
> >> >> > > apologies for any misunderstanding. Also, I'll be changing the
> >> >> > > sentence "over the decades or centuries relevant for long-term
> >> >> > > climate
> >> >> > > change, on the other hand, clouds can indeed cause significant
> >> >> > > warming" to make it clear that I'm talking about cloud feedbacks
> >> >> > > doing
> >> >> > > the action here, not cloud forcing. Please make sure you correctly
> >> >> > > represent my views in the future.
> >> >> > >
> >> >> > > Thanks!
> >> >> > >
> >> >> > >
> >> >> > > --
> >> >> > > Andrew Dessler
> >> >> > > Professor of Atmospheric Sciences
> >> >> > > Texas A&M University
> >> >> > > adessler@tamu.edu
> >> >> > > 979-862-1427
> >> >> > > http://atmo.tamu.edu/profile/ADessler
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From: on behalf of Andrew Dessler
To: Roy Spencer; Steve McIntyre
Subject: Re: ocean heat content
Date: Thursday, September 08, 2011 10:59:48 PM

Hi guys-

Two issues. First, I spent ~2 minutes reproducing Roy's dR number (based on data downloaded from Steve's website) and I can reproduce it, but only if I use the monthly data. If I do a 3-month average on the CERES data before doing the standard deviation, I get a value smaller than the one on Roy's website. So, my question to Roy is whether he averaged the CERES data down to 3 months to match the Levitus data? Or, if he's using 3-month Levitus data but 1-month CERES data?

BTW, here is my python code:

```
d1=np.loadtxt('spencer.txt',skiprows=1,delimiter=',') # edit file to
replace ',,' with ',0,'
crf=d1[:,4]-d1[:,7] # calculate CRF
crf2=[np.average(crf[ii*3:ii*3+3]) for ii in range(len(crf)/3)] #
average down to 3 months
'standard deviation of 3-month avg.: %f' % np.std(crf2)
'standard deviation of monthly.: %f' % np.std(crf)
```

output:

```
Out[7]: 'standard deviation of 3-month avg.: 0.411202'
Out[8]: 'standard deviation of monthly.: 0.579952'
```

As far as you adherence to 100 m goes, you can certainly do that, but then you have to do one of the following:
1) put into your energy balance equation an additional term for flow of heat out of the 100-m layer into the deep ocean. Without that, your equation does not conserve energy. How big is that term going to be? It's the difference between your 2.3 W/m2 number and my 4.7 W/m2, so adding that term to your 2.3 number produces a net flow into the ocean that's the same as my 700-m number. Net result: no soup for you.
2) argue that heat flux out of the bottom of your 100-m layer is zero. This makes it hard to explain temperature variations below 100 m, though. No soup.
3) declare that conservation of energy doesn't apply in the post-normal climate change debate.

Sadly, I suspect your best option is #3.

Regarding your argument that the Argo data is noisy, I'll e-mail Josh Willis at JPL to see if the experts agree with that.

Looking forward to Roy's response about his dR values. If Roy does not confirm my suspicion that Roy's using monthly CERES data, then perhaps Steve M. can spend 1-2 minutes doing the same calculation ... it will be trivial for him to confirm my or Roy's calculation.

Best wishes.

On Thu, Sep 8, 2011 at 6:33 PM, Roy Spencer < wrote:

> Andy:
>
> Not so quick on the draw there pardner...I stlll see 2 issues here before I
> change my estimates on my blog:
>
> 1) DEPTH SCALE OF AVERAGING: I think there are questions about what ocean
> layer depth should be used for this: my calculations are based upon the
> average depth to the middle of the global average thermocline (~100m), as
> seen (for example) in this movie we made of the temperature variations with
> depth. You have now chosen to use 700 m. Remember, we are discussing the
> degree to which *interannual* surface temperature variability can be
> affected by turbulent mixing versus unforced TOA radiative variations. If
> you would get the actual depth level temperature data and analyze them, you
> will see there is little interannual variability down there....but there is
> a WARMING TREND which, as you know, can be caused by CONSTANT radiative
> forcing over a long period of time, e.g. from CO2. So, it is not clear at
> all what depth should be used in these calculations....100 m, 700 m or
> somewhere in between. Also, the possible need to detrend the temperature
> time series first should be addressed.
>
> 2) TIME SCALE OF AVERAGING Regarding using monthly ocean temperature data,
> rather than 3-monthly, I can guarantee you there is a noise issue here: the
> standard deviation will be inflated at that time scale. If you don't
> believe me, try making the monthly global radiance anomalies match
> month-to-month changes in ocean heat content. 3-month is much less noisy.
> It could be even a little more time averaging is justified.
>
> No, my calculations at drroyspencer.com still stand until someone
> demonstrates (1) why 700 m (or any depth) should be used instead of 100 m,
> and (2) how much time averaging should be used to reduce noise.
>
> Nevertheless, I encourage you to publish what you have, ASAP.
>
> -Roy
>
>> From: adessler@tamu.edu
>> Date: Thu, 8 Sep 2011 16:55:35 -0500
>> Subject: Re: ocean heat content
>> To:
>> CC:
>>
>> Wow, that was easy. Just have to know the right people to cc, I guess.
>>
>> The difference between the number using Roy's data (Levitus), about 5
>> W/m2, and mine (Argo), about 13 W/m2, is not due to data, but because
>> Levitus are 3-mo averages while Argo are monthly. I've verified that
>> by averaging down Argo data. I am not surprised that averaging the
>> data set temporally reduces its standard deviation. Both go down ~700
>> m, which is probably OK for this problem.
>>
>> Off the top of my head, I can't think of any reason why using the
>> highest temporal resolution data would be bad ... and I can think of
>> reasons why averaging the data would be misleading. I'll think more
>> about this while I verify Roy's other numbers.
>>
>> But rest assured, Roy, if you can convince me that using monthly data
>> is wrong, then I'll change the paper when I get the galleys.
>>
>> BTW, I hope to see a correction on your blog with the new number soon.
>>

>> So now, as suggested by Roy, I'm going to start looking at terms on
>> the RHS ... first up, deltaR. Steve has kindly provided me with Roy's
>> data, so I'm all set! Hopefully I'll get the same number as Roy, but
>> if I don't, then we can call in Steve to adjudicate.
>>
>> Thanks for all your help.
>>
>> On Thu, Sep 8, 2011 at 3:56 PM, Roy Spencer <
>> wrote:
>> > Andy (& Steve):
>> >
>> > OK, I think we have pretty close agreement on the number Andy is
>> > calculating.
>> >
>> > I understand better what you are doing *now*, Andy (but not what you did
>> > in
>> > the preprint). Your preprint was not clear...you mentioned 100 m, and
>> > you
>> > also used Lindzen's 186 number which scales to 104 m.
>> >
>> > We now also get about 5 Watts/m2 using the full 4-D Levitus temperature
>> > dataset.
>> >
>> >
>> > So, I now know what you did on the LEFT hand side of the equation.
>> > That's a
>> > start.
>> >
>> >
>> > So, are you going to change your paper then? Because the preprint has 9
>> > and
>> > 13 as two estimates for the LHS....not 5.
>> >
>> >
>> > -Roy
>> >
>> >> From: adessler@tamu.edu
>> >> Date: Thu, 8 Sep 2011 14:08:23 -0500
>> >> Subject: Fwd: ocean heat content
>> >> To:
>> >> CC:
>> >>
>> >> Steve-
>> >>
>> >> Since I've been helping you understand Dessler 2010, I'm wondering if
>> >> you'd do me and Roy a favor. Please read our exchange below, from the
>> >> bottom up.
>> >>
>> >> Can you do a calculation of the time series of the total heat content
>> >> of the ocean? The Levitus heat capacity data is here:
>> >> http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/heat_global.html
>> >> All you have to do is calculate the time series of total heat content
>> >> of the ocean (in J) and then do a month minus month difference (as a
>> >> crude derivative) and then take the standard deviation of the
>> >> 2000-2010 period.
>> >>
>> >> The number I get is about 4.7 W/m2. Roy gets about half that number,
>> >> using a different method. We'd love to find out who's right (I
>> >> certainly acknowledge I could have a bug in my code). And you'll
>> >> probably get a blog post out of it.

>> >>
>> >> I did get your last question and will respond ... probably about the
>> >> time you get back to me with an answer to this question. :)
>> >>
>> >> Thanks!
>> >>
>> >> PS: I don't know if you read python, but I've attached my code.
>> >> Perhaps you can post this on your blog and get readers to check my
>> >> math.
>> >>
>> >>
>> >> ---------- Forwarded message ----------
>> >> From: Andrew Dessler <adessler@tamu.edu>
>> >> Date: Thu, Sep 8, 2011 at 1:11 PM
>> >> Subject: Re: ocean heat content
>> >> To: Roy Spencer <
>> >>
>> >>
>> >> > Do you realize the 186 W-month/m2/K number in your paper corresponds
>> >> > to
>> >> > 104 m depth mixed layer? So, didn't you already assume 100 m in your
>> >> > argument there?
>> >>
>> >> No. You only assume 100 m when you use surface T to figure out the
>> >> temperature change. When you look at OHC, you do not need to assume a
>> >> heat capacity/depth. That's built into the calculation.
>> >>
>> >> > Besides, the Levitus data show exactly what I was talking about, like
>> >> > the CNRM-CM3 model does, with a change in sign in the anomalies with
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From:	Roy Spencer
To:	adessler@tamu.edu
Cc:	
Subject:	RE: ocean heat content
Date:	Thursday, September 08, 2011 6:33:23 PM

Andy:

Not so quick on the draw there pardner...I stlll see 2 issues here before I change my estimates on my blog:

1) DEPTH SCALE OF AVERAGING: I think there are questions about what ocean layer depth should be used for this: my calculations are based upon the average depth to the middle of the global average thermocline (~**100m**), as seen (for example) in this movie we made of the temperature variations with depth. You have now chosen to use **700 m**. Remember, we are discussing the degree to which *interannual* surface temperature variability can be affected by turbulent mixing versus unforced TOA radiative variations. If you would get the actual depth level temperature data and analyze them, you will see there is little interannual variability down there....but there is a WARMING TREND which, as you know, can be caused by CONSTANT radiative forcing over a long period of time, e.g. from CO2. So, it is not clear at all what depth should be used in these calculations....100 m, 700 m or somewhere in between. Also, the possible need to detrend the temperature time series first should be addressed.

2) TIME SCALE OF AVERAGING Regarding using monthly ocean temperature data, rather than 3-monthly, I can guarantee you there is a noise issue here: the standard deviation will be inflated at that time scale. If you don't believe me, try making the monthly global radiance anomalies match month-to-month changes in ocean heat content. 3-month is much less noisy. It could be even a little more time averaging is justified.

No, my calculations at drroyspencer.com still stand until someone demonstrates (1) why 700 m (or any depth) should be used instead of 100 m, and (2) how much time averaging should be used to reduce noise.

Nevertheless, I encourage you to publish what you have, ASAP.

-Roy

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> Date: Thu, 8 Sep 2011 16:55:35 -0500
> Subject: Re: ocean heat content
> To:
> CC:
>
> Wow, that was easy. Just have to know the right people to cc, I guess.
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> >> I did get your last question and will respond ... probably about the
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> >> > This then gives 3-month temperature anomalies for the 0-100 meter layer.
> >> >
> >> > Then do all of the 0-100m layer average temperature changes, from one
> >> > 3-month period to the next,
> >> >
> >> > The standard deviation of those anomalies is 0.04332 deg. C
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> >> > Then convert this to Watts per sq meter...I assumed 91 days for
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> >> >
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> >> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
> >> > > Subject: ocean heat content
> >> > > To:
> >> > > spencer@nsstc.uah.edu
> >> > >
> >> > > Hi Roy.
> >> > >
> >> > > Enjoyed your blog post -- always good to get a scientific challenge.
> >> > >
> >> > > I'm working through your numbers, and I'm stumped on the first one:
> >> > > 2.3 W/m2 for the standard deviation of ocean heat content. I have the
> >> > > Levitus data and have done the season minus following season
> >> > > subtraction to get the change over the season, and then took the
> >> > > standard deviation (2000-2010) ... which gives me 1.3 10^22 J/(3
> >> > > months). Converting this to W/m2 gives me about 4.7 W/m2 -- almost
> >> > > exactly a factor of two higher than your number. I may have made an
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Cc:
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Date: Thursday, September 08, 2011 4:55:35 PM

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>> > The standard deviation of those anomalies is 0.04332 deg. C
>> >
>> > Then convert this to Watts per sq meter...I assumed 91 days for
>> > delta-time.
>> >
>> > -Roy
>> >
>> >
>> >
>> >
>> >

>> >
>> >
>> >
>> >
>> >
>> > > From: adessler@tamu.edu
>> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
>> > > Subject: ocean heat content
>> > > To:
>> > > spencer@nsstc.uah.edu
>> > >
>> > > Hi Roy.
>> > >
>> > > Enjoyed your blog post -- always good to get a scientific challenge.
>> > >
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--
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979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Roy Spencer
To: adessler@tamu.edu;
Subject: RE: ocean heat content
Date: Thursday, September 08, 2011 3:56:40 PM

Andy (& Steve):

OK, I think we have pretty close agreement on the number Andy is calculating.

I understand better what you are doing *now*, Andy (but not what you did in the preprint). Your preprint was not clear...you mentioned 100 m, and you also used Lindzen's 186 number which scales to 104 m.

We now also get about 5 Watts/m2 using the full 4-D Levitus temperature dataset.

So, I now know what you did on the LEFT hand side of the equation. That's a start.

So, are you going to change your paper then? Because the preprint has 9 and 13 as two estimates for the LHS....not 5.

-Roy

> From: adessler@tamu.edu
> Date: Thu, 8 Sep 2011 14:08:23 -0500
> Subject: Fwd: ocean heat content
> To:
> CC:
>
> Steve-
>
> Since I've been helping you understand Dessler 2010, I'm wondering if
> you'd do me and Roy a favor. Please read our exchange below, from the
> bottom up.
>
> Can you do a calculation of the time series of the total heat content
> of the ocean? The Levitus heat capacity data is here:
> http://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/heat_global.html
> All you have to do is calculate the time series of total heat content
> of the ocean (in J) and then do a month minus month difference (as a
> crude derivative) and then take the standard deviation of the
> 2000-2010 period.
>
> The number I get is about 4.7 W/m2. Roy gets about half that number,
> using a different method. We'd love to find out who's right (I
> certainly acknowledge I could have a bug in my code). And you'll
> probably get a blog post out of it.
>
> I did get your last question and will respond ... probably about the
> time you get back to me with an answer to this question. :)
>
> Thanks!
>
> PS: I don't know if you read python, but I've attached my code.
> Perhaps you can post this on your blog and get readers to check my
> math.
>
>

> ---------- Forwarded message ----------
> From: Andrew Dessler <adessler@tamu.edu>
> Date: Thu, Sep 8, 2011 at 1:11 PM
> Subject: Re: ocean heat content
> To: Roy Spencer <
>
>
> > Do you realize the 186 W-month/m2/K number in your paper corresponds to 104 m depth mixed layer? So, didn't you already assume 100 m in your argument there?
>
> No. You only assume 100 m when you use surface T to figure out the
> temperature change. When you look at OHC, you do not need to assume a
> heat capacity/depth. That's built into the calculation.
>
> > Besides, the Levitus data show exactly what I was talking about, like the CNRM-CM3 model does, with a change in sign in the anomalies with depth.
>
> One of us has a bug in their code. As you integrate deeper the answer
> you get with your method must converge to the answer I get with mine
> (assuming your using data, as I am). I'll email Steve McIntyre to
> settle this. He can independently audit our results. I admit that
> there might be a bug in my code, and I'd love to find that out if so.
>
> > I now have results from MIROC-hires, S/N is about 1.8:1 to 1.9:1, *independent* of the depth you average over.
>
> Let's stick with data here.
>
> > Andy, *I'm* not the one trying to defend the CMIP models. I'm using YOUR arguments with the models YOU support, to show you are wrong.
>
> Stop it, Roy. Really.
>
> > -Roy
> >
> > _____
> > From: adessler@tamu.edu
> > Date: Thu, 8 Sep 2011 12:49:21 -0500
> > Subject: Re: ocean heat content
> > To:
> >
> > Are you trying to argue with the Levitus OHC data with data from a model?? Wow, even an "alarmist" like me wouldn't try that. You need to use data here. As you integrate to greater depth, the number from your method must converge with mine (about 5 W/m2 for seasonal OHC changes from Levitus). Thus, your claims make no sense. I suggest you proceed carefully, too.
> >
> > On Thu, Sep 8, 2011 at 11:39 AM, Roy Spencer < wrote:
> >
> > Andy:
> >
> > Thanks for the best wishes on our next paper.
> >
> > It's interesting that you mention the use of deeper layers, because I'm actually computing S/N from the CNRM-CM3 model now, using TOA fluxes, model SST and ocean layer temperatures, and the F&T diagnosed feedback net parameter.
> >
> > This is better than using standard deviations to get estimated magnitudes for S and N, since one can do an energy-conserving calculation. (BTW, based upon the AR4 report CNRM-CM3 arguably has the most realistic ENSO (certainly the strongest) variability in the NINO3 region.)
> >

> > Here's what I get for various assumed layer depths for the global oceans:
> >
> > 0-100 meters: S/N = 3.8
> >
> > 0-200 meters: S/N = 2.2
> >
> > 0-300 meters: S/N = 1.7
> >
> > You see, as you go deeper, even though it takes more W/m2 to change the temperature of the full layer (thus inflating the term on the left side of the equation), the temperature variability at deeper levels becomes not only smaller, it becomes negatively correlated with the temperature changes in the shallower layers.
> >
> > The net result is that S/N becomes SMALLER with assumed ML depth, NOT greater.
> >
> > I suggest you proceed carefully.
> >
> > -Roy
> >
> > _____
> > From: adessler@tamu.edu
> > Date: Thu, 8 Sep 2011 10:48:08 -0500
> > Subject: Re: ocean heat content
> > To:
> >
> > 100 m is a global average. In some regions, heat transport is much deeper. By doing cutting off at 100 m everywhere, you're underestimating the heat content changes of the ocean.
> >
> > The correct way to do it is to use the total heat content of the ocean (but 700 m will have to suffice if you use this data set), which avoids cutting off any heat content changes. That calculation agrees with the heat content calculation from the surface T data.
> >
> > The difference in data sets between our analyses is a complete red herring --- it's all in the assumptions, particularly how you've done the ocean heat capacity.
> >
> > And this does not even begin to address the issue of using seasonal vs. monthly. That's a whole other issue.
> >
> > But the good news is that you did answer my question and confirmed everything that I suspected about your rebuttal. Good luck getting this calculation through peer review. My recommendation is not to send it to Remote Sensing.
> >
> > On Thu, Sep 8, 2011 at 10:26 AM, Roy Spencer < wrote:
> >
> > Hi Andy:
> >
> > We used Levitus global gridpoint ocean temperatures at depth, which have 3-month temperature anomalies at 0, 10, 20, 30, 50, 75, 100 meters, etc.
> >
> > Danny Braswell is the one who downloaded the dataset and computed area average (cosine-latitude weighted) anomalies for the global ice-free oceans (60N-60S).
> >
> > The global at-depth anomalies were then averaged from the surface to 100 m depth, using a weighted average of the top 7 levels using the weights: 5, 10, 10, 15, 22.5, 25, and 12.5
> >
> > This then gives 3-month temperature anomalies for the 0-100 meter layer.
> >
> > Then do all of the 0-100m layer average temperature changes, from one 3-month period to the next,
> >

> > The standard deviation of those anomalies is 0.04332 deg. C
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> > > Date: Wed, 7 Sep 2011 21:44:39 -0500
> > > Subject: ocean heat content
> > > To: spencer@nsstc.uah.edu
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From: Andrew Revkin
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Wednesday, September 07, 2011 1:49:14 PM

---

indeed~~~
i try to foster constructive discourse but....

On Wed, Sep 7, 2011 at 1:10 PM, Andrew Dessler <adessler@tamu.edu> wrote:
> nice blog post on my paper. but, man, some of your commenters are
> nuts! but I guess I already knew that.
>
> On Tue, Sep 6, 2011 at 8:30 AM, Andrew Revkin < wrote:
> > great. will get to this soon.
> > your apt comment below says reams.
> >
> > On Tue, Sep 6, 2011 at 9:28 AM, Andrew Dessler <adessler@tamu.edu> wrote:
> >>
> >> Hi Andy. I wanted to send you a press release and a copy of a new
> >> paper that was released this morning (EDT) by the AGU. This
> >> paper rebuts the suggestions by Lindzen and Spencer that climate
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> >> Let me know if you have any questions.
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> >
> >
> > --
> > | Please excuse typos; I had a (blessedly minor) stroke that's affected my
> > typing a bit. |

>
> ANDREW C. REVKIN
> Dot Earth blogger, The New York Times
> http://www.nytimes.com/dotearth
> Senior Fellow, Pace Acad. for Applied Env. Studies
> Cell: 914-441-5556
> Twitter: @revkin Skype: Andrew.Revkin
>

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Andrew Dessler
Professor of Atmospheric Sciences
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adessler@tamu.edu
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Twitter: @revkin Skype: Andrew.Revkin

From: on behalf of Andrew Dessler
To: Andrew Revkin
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Wednesday, September 07, 2011 12:10:21 PM

---

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From: Leo Hickman
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 3:31:43 PM

---

yes, a blog or an op-ed. i have mentioned it to my editors but it;s
late here now. i;ll let you know what they say in the morning, or they
may contact you direct.

leo

On 6 September 2011 18:46, Andrew Dessler <adessler@tamu.edu> wrote:
> by comment article, do you mean an oped? if so, I'd be happy to do
> that, as long as your deadline fits with my schedule. and thanks for
> the tweet.
>
> On Tue, Sep 6, 2011 at 11:15 AM, Leo Hickman <leo.hickman@guardian.co.uk> wrote:
>> Hi, thanks for this Andrew. I have tweeted the link to your video (I
>> wish more scientists did this when they publish a paper!), but not
>> sure Guardian will follow up story after the resignation story we did
>> last Friday that mentioned your forthcoming paper. I will check with
>> my editors though. They might want a comment article from you, though
>> - would you be open to that idea??
>>
>> Best, Leo
>>
>> On 6 September 2011 16:40, Andrew Dessler <adessler@tamu.edu> wrote:
>>> Hi Leo. I wanted to send you a press release and a copy of a new
>>> paper that was released this morning (EDT) by the AGU. This
>>> paper rebuts the suggestions by Lindzen and Spencer that climate
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Registered in England Number 908396

From:
To: adessler@tamu.edu
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 3:30:32 PM

Many thankshe paper and your comments.

Peter Gwynne
North America correspondent
Physics World
http://www.physicsworld.com
508-833-6789

In a message dated 9/6/2011 2:32:40 PM Eastern Daylight Time, adessler@tamu.edu writes:

> Hello. Here is a press release and a copy of the new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)
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> Andrew Dessler
> Professor of Atmospheric Sciences
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> adessler@tamu.edu
> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Steve McIntyre
Subject: Re: [Bulk] Re: Dessler et al 2010
Date: Tuesday, September 06, 2011 3:28:53 PM

On Tue, Sep 6, 2011 at 12:50 PM, Steve McIntyre < wrote:
> Thanks for this. I did a post at Climate Audit on the topic. It's not a core
> area of study for me and, if there are any points that I've mis-stated, I'd
> be happy to correct them.

I skimmed it. I think there are some conceptual errors, but I don't have time to enumerate them now. Maybe I can write you a longer e-mail this weekend. There's also quite a bit of snark -- but I guess you know your audience.

> I would also appreciate the data on models that you used for your analysis
> (as it entered into the statistical analysis).

are you asking for the raw model data, or for the dRcloud and dTs from the models?

> You used a different R_clear-sky than Spencer and Braswell. What was the
> provenance of your values?

S&B don't use clear-sky fluxes. They use all-sky. As described in D10, my clear-sky fluxes come from the reanalysis.

> Thanks, Steve Mc
>
>
>
> -----Original Message-----
> From: [mailto: On Behalf
> Of Andrew Dessler
> Sent: September-05-11 10:46 PM
> To: Steve McIntyre
> Subject: [Bulk] Re: Dessler et al 2010
>
> Attached are the data for Fig. 1. Each data set is a time series running
> from 3/2000-2/2010. You can make Fig. 2 by plotting erats from Fig. 1c vs.
> eradr from Fig. 1b. I assume you don't want the model data from Fig. 2b.
> Let me know if you do.
>
> FYI, the disagreement between Spencer and Lindzen and me is not in the
> numbers. I have reproduced much of LC11 and almost all of SB11 and I know
> that L&C have reproduced my numbers (see Figure 9 of LC11). So we actually
> agree on the numbers. The disagreement is in what the numbers mean. I think
> their interpretation is absurd, and I have a paper coming out in GRL this
> week that will describe why. We'll see if anyone is convinced.
>
> As far as the treatment Spencer has gotten, I'm not sure if you're just
> giving advice or asking for my opinion. If the latter, then I'd be happy to
> talk with you about this. Feel free to call me at your convenience at
> 979-862-1427. Because of schedule issues, I may not be able to return any
> calls until later in the week.
>
> Regards,
> Andy

>
> On Mon, Sep 5, 2011 at 1:49 PM, Steve McIntyre < wrote:
>> Dear Dr Dessler,
>>
>> I think that you should find the recent Remote Sensing controversy
>> quite troubling. I understand that you disagree with Spencer's
>> analyses, but the responses by Trenberth and others are totally
>> disproportionate and are the sort of conduct that should trouble
>> people that are worried about doubled CO2. It is as though no lessons
>> were learned from Climategate and, instead of climate scientists
>> avoiding the worst excesses of Climategate conduct, have been emboldened
> to behave even more poorly.
>>
>>
>>
>> As a result of the controversy, I've got some data from Spencer and am
>> trying to reconcile it to Dessler 2010. Can you send me the summary
>> data used to plot Figures 1 and 2?
>>
>>
>>
>> Thanks, Steve McIntyre
>>
>>
>>
>>
>
>
>
> --
> Andrew Dessler
> Professor of Atmospheric Sciences
> Texas A&M University
> adessler@tamu.edu
> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler
>
>

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Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Stephanie Pappas
To: Andrew Dessler
Subject: RE: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 3:16:09 PM

Sounds great, talk to you then.

_____ __
From: [On Behalf Of Andrew Dessler
[adessler@tamu.edu]
Sent: Tuesday, September 06, 2011 2:22 PM
To: Stephanie Pappas
Subject: Re: new paper rebutting Spencer/Lindzen

10 am CDT works for me. call me at 979-862-1427. talk to you tomorrow.

On Tue, Sep 6, 2011 at 1:54 PM, Stephanie Pappas
< wrote:
> Hi Andrew,
>
> Thanks so much for sending this along. So, I'd like to cover the research but use it as a jumping-off point to explain the cloud/climate change "debate" in general - why clouds are a favorite topic for skeptics and what the science really says. I think you're completely right about the way that skeptical papers cycle through the media, and I'd like to have a sort of definitive, feature-style article on what's really up with clouds rather than reporting your single paper and making it look even more like an equal back-and-forth, if that makes sense.
>
> Anyway, my rambling aside, are you free on Wednesday to talk about your new research and the context of these cloud studies in general? I'm free anytime after about 9:30 Central, so just let me know a good time to call.
>
> Best,
> Stephanie
>
> Stephanie Pappas
> Senior Writer, LiveScience.com
>
> (713) 657-0868
>
> _____ __
> From: [On Behalf Of Andrew Dessler
[adessler@tamu.edu]
> Sent: Tuesday, September 06, 2011 8:29 AM
> To: Stephanie Pappas
> Subject: Fwd: new paper rebutting Spencer/Lindzen
>
> Hi Stephanie. I wanted to send you a press release and a copy of a new
> paper that was released this morning (EDT) by the AGU. This
> paper rebuts the suggestions by Lindzen and Spencer that climate
> change is caused by clouds. Attached you can find the paper and the
> press release. I've put up a youtube video (http://goo.gl/zFJmt) that
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> To me, the real story here is that, every month, dozens if not
> hundreds of papers are published that are in agreement with the
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> skeptical papers get published, and these are then trumpeted by
> sympathetic media outlets as if they'd discovered the wheel. It

> therefore appears to the general public that there's a debate.
>
> Let me know if you have any questions.
>
> Thanks!
>
>
>
>
>
> --
> Andrew Dessler
> Professor of Atmospheric Sciences
> Texas A&M University
> adessler@tamu.edu
> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler
>

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Berger, Eric
To: Andrew Dessler
Subject: RE: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 3:13:04 PM

Thanks Andy. I hope to blog about this later in the week. I do like the quote in the news release about you hoping this ends the debate (!)

-----Original Message-----
From: [mailto: On Behalf Of Andrew Dessler
Sent: Tuesday, September 06, 2011 8:27 AM
To: Berger, Eric
Subject: Fwd: new paper rebutting Spencer/Lindzen

Hi Eric. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

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Let me know if you have any questions.

Thanks!

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Stephanie Pappas
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 2:22:04 PM

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< wrote:
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>
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> Anyway, my rambling aside, are you free on Wednesday to talk about your new research and the context of these cloud studies in general? I'm free anytime after about 9:30 Central, so just let me know a good time to call.
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> Best,
> Stephanie
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> Stephanie Pappas
> Senior Writer, LiveScience.com
>
> (713) 657-0868
>
> _____ __
> From: [On Behalf Of Andrew Dessler [adessler@tamu.edu]
> Sent: Tuesday, September 06, 2011 8:29 AM
> To: Stephanie Pappas
> Subject: Fwd: new paper rebutting Spencer/Lindzen
>
> Hi Stephanie. I wanted to send you a press release and a copy of a new
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> Let me know if you have any questions.
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> Thanks!
>
>
>

>
>
> --
> Andrew Dessler
> Professor of Atmospheric Sciences
> Texas A&M University
> adessler@tamu.edu
> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler
>

--
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979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Stephanie Pappas
To: Andrew Dessler
Subject: RE: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 1:58:12 PM

Hi Andrew,

Thanks so much for sending this along. So, I'd like to cover the research but use it as a jumping-off point to explain the cloud/climate change "debate" in general - why clouds are a favorite topic for skeptics and what the science really says. I think you're completely right about the way that skeptical papers cycle through the media, and I'd like to have a sort of definitive, feature-style article on what's really up with clouds rather than reporting your single paper and making it look even more like an equal back-and-forth, if that makes sense.

Anyway, my rambling aside, are you free on Wednesday to talk about your new research and the context of these cloud studies in general? I'm free anytime after about 9:30 Central, so just let me know a good time to call.

Best,
Stephanie

Stephanie Pappas
Senior Writer, LiveScience.com

(713) 657-0868

_____ __
From: [On Behalf Of Andrew Dessler
[adessler@tamu.edu]
Sent: Tuesday, September 06, 2011 8:29 AM
To: Stephanie Pappas
Subject: Fwd: new paper rebutting Spencer/Lindzen

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Let me know if you have any questions.

Thanks!

--
Andrew Dessler

Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Jesse Smith
To: Andrew Dessler
Subject: Re: Fwd: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 1:57:30 PM

Andy,

Thanks for the email, and the copy of your recent paper. It is frustrating that the public perception of scientific knowledge has not enough to do with the science itself, and too much to do with other, extraneous factors that influence how a scientifically-uneducated audience decides whomto believe about issues that they themselves do not understand. We know the dilemma well here, as you can imagine.

Cheers,

Jesse

> **From:** Andrew Dessler <adessler@tamu.edu>
> **To:** Richard Kerr < Jesse Smith < >
> **Date:** 9/6/2011 9:32 AM
> **Subject:** Fwd: new paper rebutting Spencer/Lindzen
>
> Hi guys. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)
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> Let me know if you have any questions.
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> Thanks!
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> --
> Andrew Dessler
> Professor of Atmospheric Sciences
> Texas A&M University
> adessler@tamu.edu
> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To:
Subject: Fwd: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 1:32:19 PM
Attachments: dessler paper on climate change.docx
LC11SB11responseV3b.pdf

Hello. Here is a press release and a copy of the new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

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Let me know if you have any questions.

Thanks!

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Andrew Dessler
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adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

-30-

Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

Brohan, P., J. Kennedy, I. Harris, S. Tett, and P. Jones (2006), Uncertainty estimates in regional and global observed temperature changes: A new dataset from 1850, J. Geophys. Res., 111, D12106, doi: 10.1029/2005JD006548.

Dee, D. P., et al. (2011), The ERA-Interim reanalysis: Configuration and performance of the data assimilation system, Q. J. R. Meteor. Soc., 137, doi: 10.1002/qj.828, 553-597.

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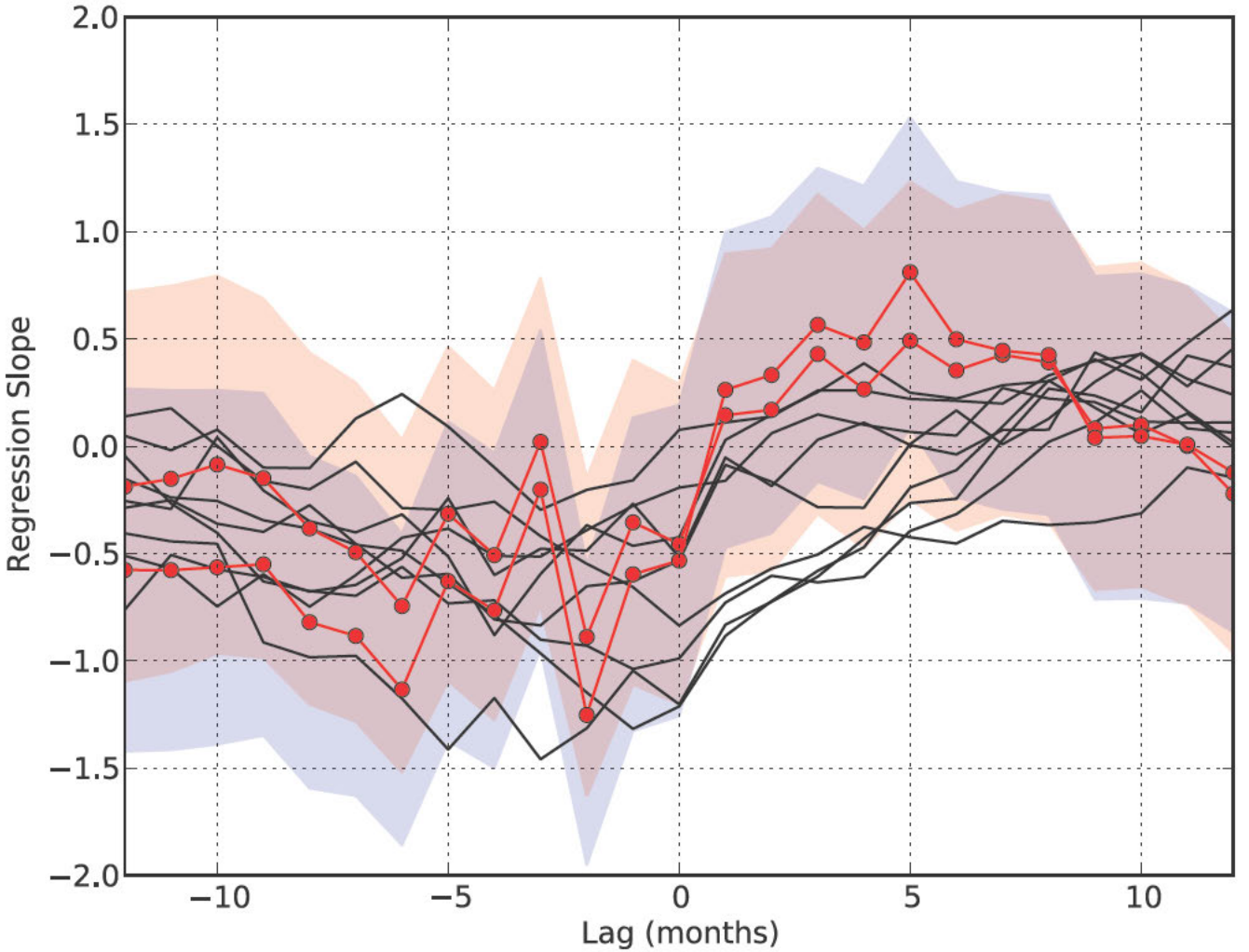
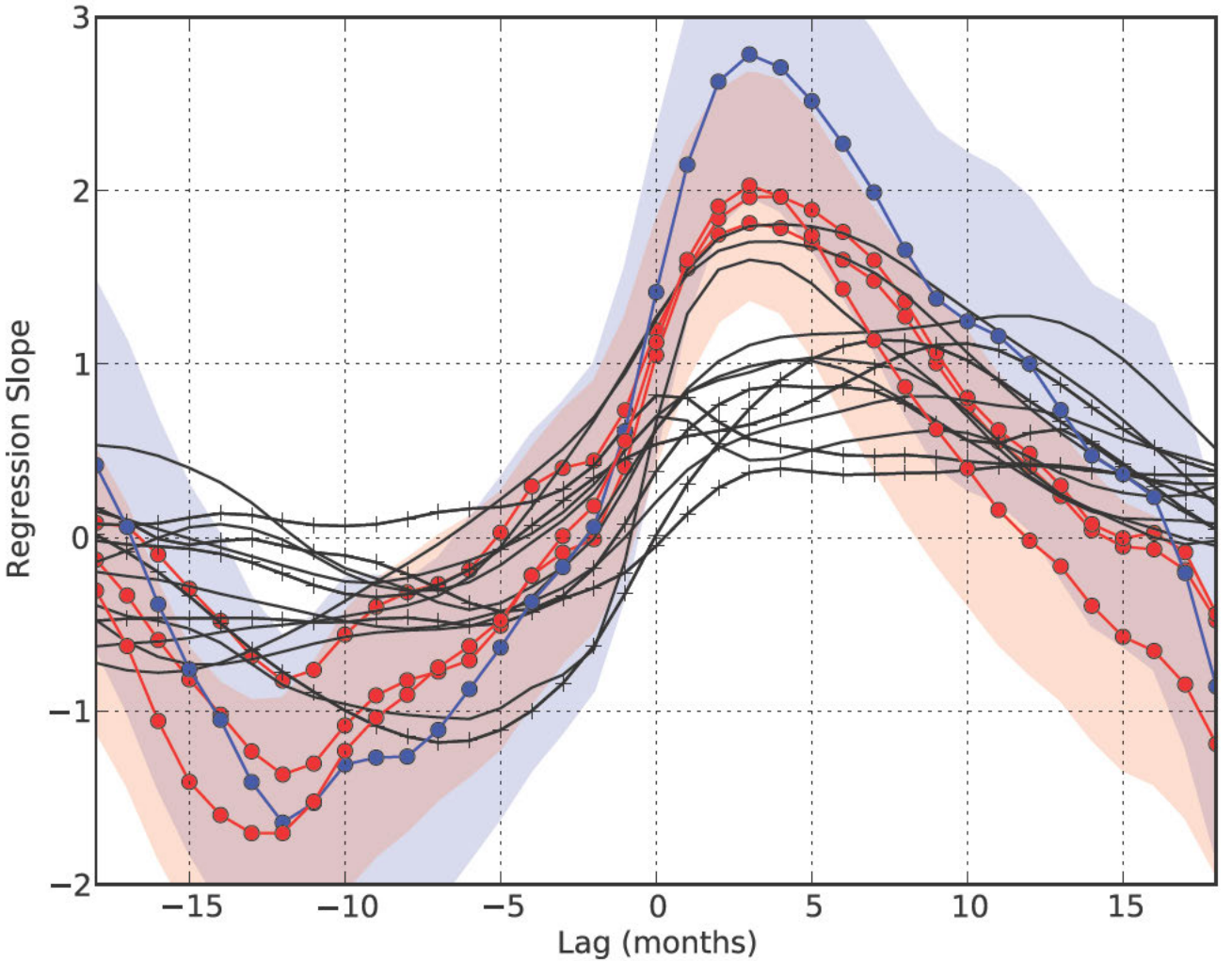
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.


2.0
1.5
1.0
0.5
0.0
−0.5
−1.0
−1.5
−2.0
Regression Slope
−10
−5
0
5
10
Lag (months)


Regression Slope
Lag (months)
3
2
1
0
−1
−2
−15
−10
−5
0
5
10
15

From: Steve McIntyre
To: "Andrew Dessler"
Subject: RE: [Bulk] Re: Dessler et al 2010
Date: Tuesday, September 06, 2011 12:50:47 PM

Thanks for this. I did a post at Climate Audit on the topic. It's not a core area of study for me and, if there are any points that I've mis-stated, I'd be happy to correct them.

I would also appreciate the data on models that you used for your analysis (as it entered into the statistical analysis).

You used a different R_clear-sky than Spencer and Braswell. What was the provenance of your values?

Thanks, Steve Mc

-----Original Message-----
From: [mailto: On Behalf Of Andrew Dessler
Sent: September-05-11 10:46 PM
To: Steve McIntyre
Subject: [Bulk] Re: Dessler et al 2010

Attached are the data for Fig. 1. Each data set is a time series running from 3/2000-2/2010. You can make Fig. 2 by plotting erats from Fig. 1c vs. eradr from Fig. 1b. I assume you don't want the model data from Fig. 2b. Let me know if you do.

FYI, the disagreement between Spencer and Lindzen and me is not in the numbers. I have reproduced much of LC11 and almost all of SB11 and I know that L&C have reproduced my numbers (see Figure 9 of LC11). So we actually agree on the numbers. The disagreement is in what the numbers mean. I think their interpretation is absurd, and I have a paper coming out in GRL this week that will describe why. We'll see if anyone is convinced.

As far as the treatment Spencer has gotten, I'm not sure if you're just giving advice or asking for my opinion. If the latter, then I'd be happy to talk with you about this. Feel free to call me at your convenience at 979-862-1427. Because of schedule issues, I may not be able to return any calls until later in the week.

Regards,
Andy

On Mon, Sep 5, 2011 at 1:49 PM, Steve McIntyre < wrote:
> Dear Dr Dessler,
>
> I think that you should find the recent Remote Sensing controversy
> quite troubling. I understand that you disagree with Spencer's
> analyses, but the responses by Trenberth and others are totally
> disproportionate and are the sort of conduct that should trouble
> people that are worried about doubled CO2. It is as though no lessons
> were learned from Climategate and, instead of climate scientists
> avoiding the worst excesses of Climategate conduct, have been emboldened
to behave even more poorly.

>
>
>
> As a result of the controversy, I've got some data from Spencer and am
> trying to reconcile it to Dessler 2010. Can you send me the summary
> data used to plot Figures 1 and 2?
>
>
>
> Thanks, Steve McIntyre
>
>
>
>

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Leo Hickman
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 12:46:17 PM

---

by comment article, do you mean an oped? if so, I'd be happy to do that, as long as your deadline fits with my schedule. and thanks for the tweet.

On Tue, Sep 6, 2011 at 11:15 AM, Leo Hickman <leo.hickman@guardian.co.uk> wrote:
> Hi, thanks for this Andrew. I have tweeted the link to your video (I
> wish more scientists did this when they publish a paper!), but not
> sure Guardian will follow up story after the resignation story we did
> last Friday that mentioned your forthcoming paper. I will check with
> my editors though. They might want a comment article from you, though
> - would you be open to that idea??
>
> Best, Leo
>
> On 6 September 2011 16:40, Andrew Dessler <adessler@tamu.edu> wrote:
>> Hi Leo. I wanted to send you a press release and a copy of a new
>> paper that was released this morning (EDT) by the AGU. This
>> paper rebuts the suggestions by Lindzen and Spencer that climate
>> change is caused by clouds. Attached you can find the paper and the
>> press release. I've put up a youtube video (http://goo.gl/zFJmt) that
>> explains the work. (my paper coincidentally follows the resignation
>> on Friday of the editor of the journal that published Spencer's paper)
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>> hundreds of papers are published that are in agreement with the
>> mainstream theory of climate science. But, every year, one or two
>> skeptical papers get published, and these are then trumpeted by
>> sympathetic media outlets as if they'd discovered the wheel. It
>> therefore appears to the general public that there's a debate.
>>
>> Let me know if you have any questions.
>>
>> Thanks!
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>>
>>
>> --
>> Andrew Dessler
>> Professor of Atmospheric Sciences
>> Texas A&M University
>> adessler@tamu.edu
>> 979-862-1427
>> http://atmo.tamu.edu/profile/ADessler
>>
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--
Andrew Dessler
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adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Leo Hickman
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 12:11:18 PM

GuardianEco has just retweeted my tweet which means it will now reach about 50k followers...

http://twitter.com/#!/guardianeco/status/111120803457810432

On 6 September 2011 17:15, Leo Hickman <leo.hickman@guardian.co.uk> wrote:
> Hi, thanks for this Andrew. I have tweeted the link to your video (I
> wish more scientists did this when they publish a paper!), but not
> sure Guardian will follow up story after the resignation story we did
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>> skeptical papers get published, and these are then trumpeted by
>> sympathetic media outlets as if they'd discovered the wheel. It
>> therefore appears to the general public that there's a debate.
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>> Let me know if you have any questions.
>>
>> Thanks!
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>> --
>> Andrew Dessler
>> Professor of Atmospheric Sciences
>> Texas A&M University
>> adessler@tamu.edu
>> 979-862-1427
>> http://atmo.tamu.edu/profile/ADessler
>>
>

From: Mark Zelinka
To: Andrew Dessler
Subject: Re: paper on feedbacks
Date: Tuesday, September 06, 2011 11:30:42 AM

No problem. Good luck facing the onslaught! I just saw that the Editor in Chief at Remote Sensing resigned over the Spencer and Braswell paper.

Mark

On 9/5/11 12:35 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:

>thanks. I'm swamped for a few days, but hopefully I can get to this
>next weekend. my spencer/lindzen paper is being released online
>tomorrow and I'm prepping for the press onslaught (hopefully, anyway).
>
>On Fri, Sep 2, 2011 at 2:04 PM, Mark Zelinka <mdz113@uw.edu> wrote:
>> Hi Andy,
>>
>> A netcdf file containing the cloud masking of the temperature feedback,
>>LW
>> WV feedback, GHG forcing, and the sum of all three can be accessed here:
>> http://www.atmos.washington.edu/~mzelinka/research/adjustments.nc
>>
>> I also attach two figures. One is the timeseries of our global mean LW
>> and SW adjustments. The small offset visible in the SW plot is due to
>>our
>> use of two slightly different time stamps. The second one is the
>> regression of our adjustments on the global mean surface temperature
>> anomaly from HadCRUT3v. They look pretty similar but have some
>> differences, especially over the sea ice edge and over the continents.
>>
>> Let me know what you think.
>> Mark
>>
>> On 8/23/11 8:38 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>>
>>>Thanks for the Colman paper.
>>>
>>>I'd be interested in tracking down the difference between our LW
>>>adjustment term. Could you send me a netCDF file that contains the
>>>individual terms of the adjustment (water vapor, temperature, etc.)?
>>>I'll compare to my individual terms, and hopefully we can track this
>>>down.
>>>
>>>On Fri, Aug 19, 2011 at 4:59 PM, Mark Zelinka <mdz113@uw.edu> wrote:
>>>> Got it. I didn't think it would make such a difference but I can see
>>>>that
>>>> it does. When I tried to recreate your Figure 3 using EOF-derived
>>>>slopes,
>>>> it looks like nonsense, with gigantic slopes at most latitudes.
>>>>Whenever
>>>> there is a lot of scatter, least squares will give you a small slope
>>>>with
>>>> tons of uncertainty whereas EOF might give you a huge slope as it
>>>>aligns
>>>> the best-bit to minimize perpendicular distances.
>>>>

>>>> It is somewhat disturbing however that the slope you get from a
>>>> least-squares fit does not fall within the errorbars of the
>>>>EOF-derived
>>>> slope: T feedback ranges from -3.6 to -4.5 in Table 1, but least
>>>>squares
>>>> gives you -3.14.
>>>>
>>>> I don't think you should necessarily get the same T and WV feedbacks
>>>>in
>>>> observations as in the A1B scenario. Colman and Power (Attached) show
>>>> different feedback magnitudes in the BMRC model depending on whether
>>>>it
>>>>is
>>>> interannual variability or transient warming.
>>>>
>>>> Maybe report both estimates of the slopes and let the reader decide?
>>>>
>>>> Mark
>>>>
>>>>
>>>>
>>>> On 8/19/11 1:45 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>>>>
>>>>>I know exactly what is going on and it is discussed a bit in the paper
>>>>>starting on line 68. I calculate global average feedbacks for water
>>>>>vapor and temperature as the 1st EOF of the data (per Hartmann, this
>>>>>is equivalent to minimizing the square of the orthogonal distance
>>>>>between the data and the line). I do this because, in a normal linear
>>>>>least squares fit, noise in the x-axis data (temperature) ³dilutes²
>>>>>the slope and gives you a smaller value than I think you should get.
>>>>>
>>>>>However, for the plots of the spatial pattern and the zonal average, I
>>>>>did just do a linear least squares fit between the local flux and the
>>>>>global average T<I didn't think anyone would notice (damn you!). Thus,
>>>>>if you do a global average of the local feedback in Fig. 2 or 3, then
>>>>>you get the slope that you would have gotten had I done a normal
>>>>>least-squares fit to the global data (FYI, the slope I get for the
>>>>>data for water and temperature using a least squares fit is +1.18
>>>>>W/m2/K and -3.14 W/m2/K).
>>>>>
>>>>>Doing the fits for water and temperature the way I do gets you the
>>>>>same answer as the method Soden has used (differencing a decade at the
>>>>>beginning and end of the 21st century). Also, it gives me a better
>>>>>fit, by eye.
>>>>>
>>>>>I hope that is clear. If not, let me know.
>>>>>
>>>>>Let me know if you have any suggestions on the best way to handle
>>>>>this. BTW, I'm attaching a plot of dR(temperature) vs. dTs showing
>>>>>both the ordinary least squares (red) and the 1st EOF (black) fits.
>>>>>
>>>>>On Fri, Aug 19, 2011 at 3:09 PM, Mark Zelinka <mdz113@uw.edu> wrote:
>>>>>>> Hi Andy,
>>>>>>>
>>>>>>> Thanks for sending this along. I look forward to reading it.
>>>>>>>
>>>>>>> I recently computed my own LW and SW adjustments using ERA Interim
>>>>>>>data.
>>>>>>> I would have just used yours to convert CRF anomalies into feedback,
>>>>>>>but

>>>>>> decided to re-calculate cloud forcing using kernel-derived clear-sky
>>>>>> fluxes rather than using the CERES clear sky fluxes (which have gaps
>>>>>>and
>>>>>> are subject to the well known clear-sky sampling biases). Our SW
>>>>>> adjustments are pretty much identical and our LW adjustments are
>>>>>>similar,
>>>>>> but not identical.
>>>>>>
>>>>>> I can reproduce quite closely the black solid lines in your Figure
>>>>>>3,
>>>>>>but
>>>>>> for some reason when I compare with your Table 1, my WV and
>>>>>>temperature
>>>>>> feedbacks are much smaller in magnitude (1.1 and -3.2 respectively).
>>>>>> Globally averaging (by eye) your figure 3 (which you can do since
>>>>>>the
>>>>>>sine
>>>>>> of latitude is plotted), I would not expect your observed WV
>>>>>>feedback
>>>>>>to
>>>>>> be 2.1. At the same time, a WV feedback of ~1 seems way too small.
>>>>>> Comments?
>>>>>>
>>>>>> Mark
>>>>>>
>>>>>>
>>>>>>
>>>>>> On 8/19/11 11:54 AM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>>>>>>
>>>>>>>Hi Mark. Attached is a paper I just finished on feedbacks, esp.
>>>>>>>clouds. it's based on my GRC poster. I'd be very interested in
>>>>>>>your
>>>>>>>comments. as a bonus, I only have one Zelinka reference in there
>>>>>>>now
>>>>>>>... but I'm happy to add more if you tell me where I should put
>>>>>>>them!
>>>>>>>
>>>>>>>
>>>>>>>--
>>>>>>>Andrew Dessler
>>>>>>>Professor of Atmospheric Sciences
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From: Leo Hickman
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 11:16:07 AM

Hi, thanks for this Andrew. I have tweeted the link to your video (I wish more scientists did this when they publish a paper!), but not sure Guardian will follow up story after the resignation story we did last Friday that mentioned your forthcoming paper. I will check with my editors though. They might want a comment article from you, though - would you be open to that idea??

Best, Leo

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> paper rebuts the suggestions by Lindzen and Spencer that climate
> change is caused by clouds. Attached you can find the paper and the
> press release. I've put up a youtube video (http://goo.gl/zFJmt) that
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> Let me know if you have any questions.
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>

From: on behalf of Andrew Dessler
To: leo.hickman@guardian.co.uk
Subject: Fwd: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 10:40:26 AM
Attachments: dessler paper on climate change.docx
LC11SB11responseV3b.pdf

Hi Leo. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.

Let me know if you have any questions.

Thanks!

--
Andrew Dessler
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adessler@tamu.edu
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http://atmo.tamu.edu/profile/ADessler

COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

-30-

Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

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In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

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As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

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I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

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Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

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SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

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maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.

FIGURE 1

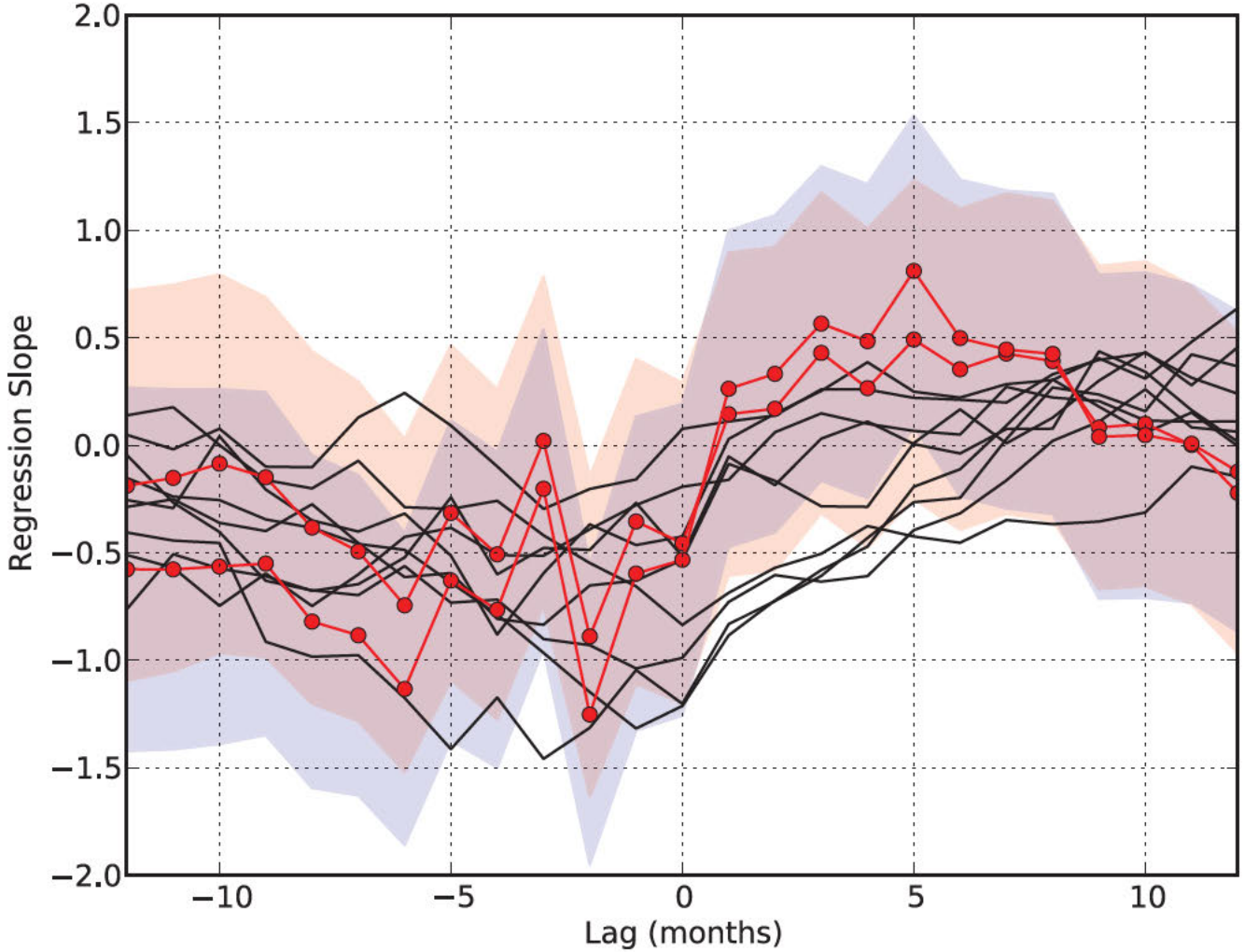
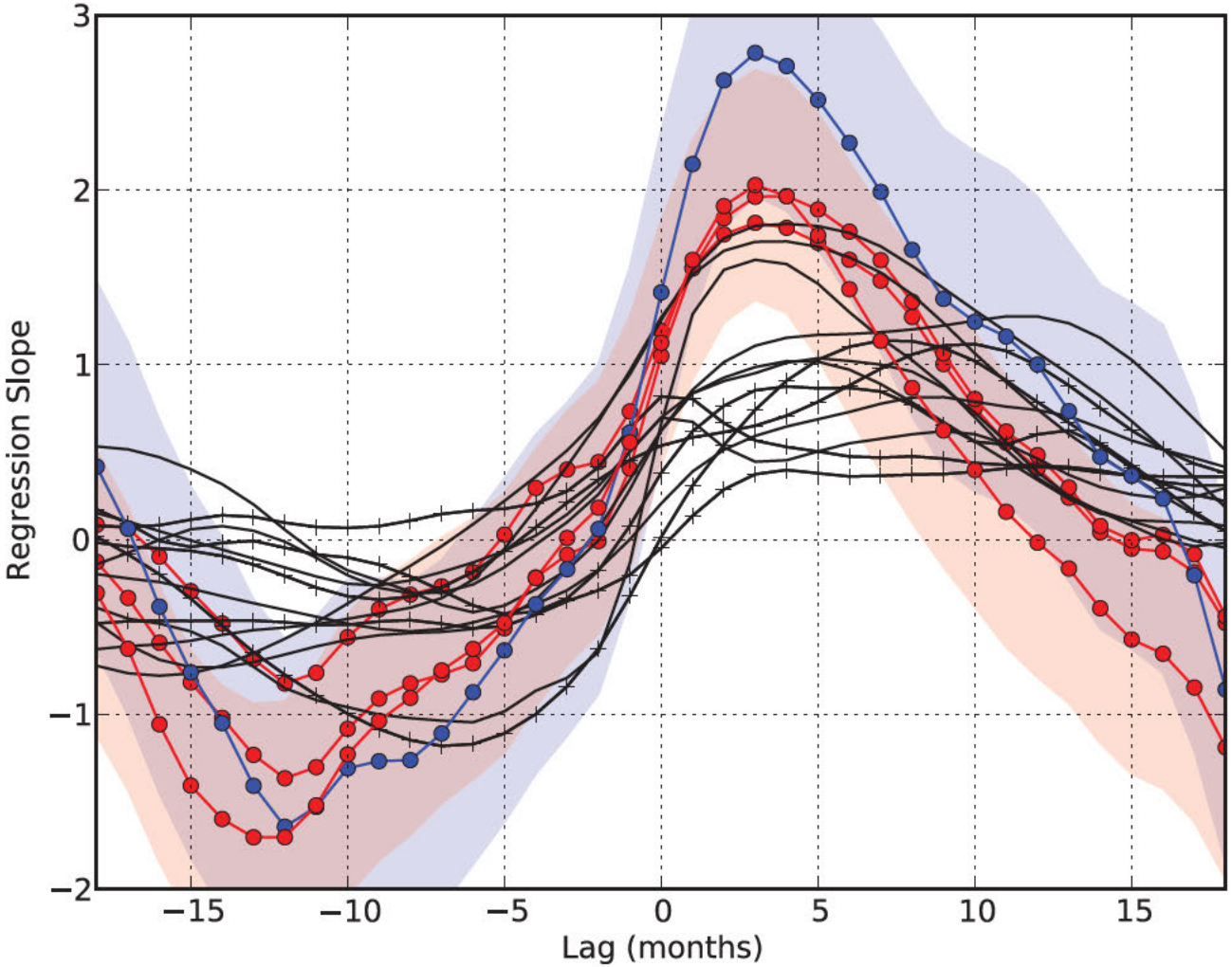


Figure 2



From:	on behalf of Andrew Dessler
To:	Lauren Morello
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 10:38:55 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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Hi Laruen. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.

Let me know if you have any questions.

Thanks!

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Andrew Dessler
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http://atmo.tamu.edu/profile/ADessler

COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

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Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

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Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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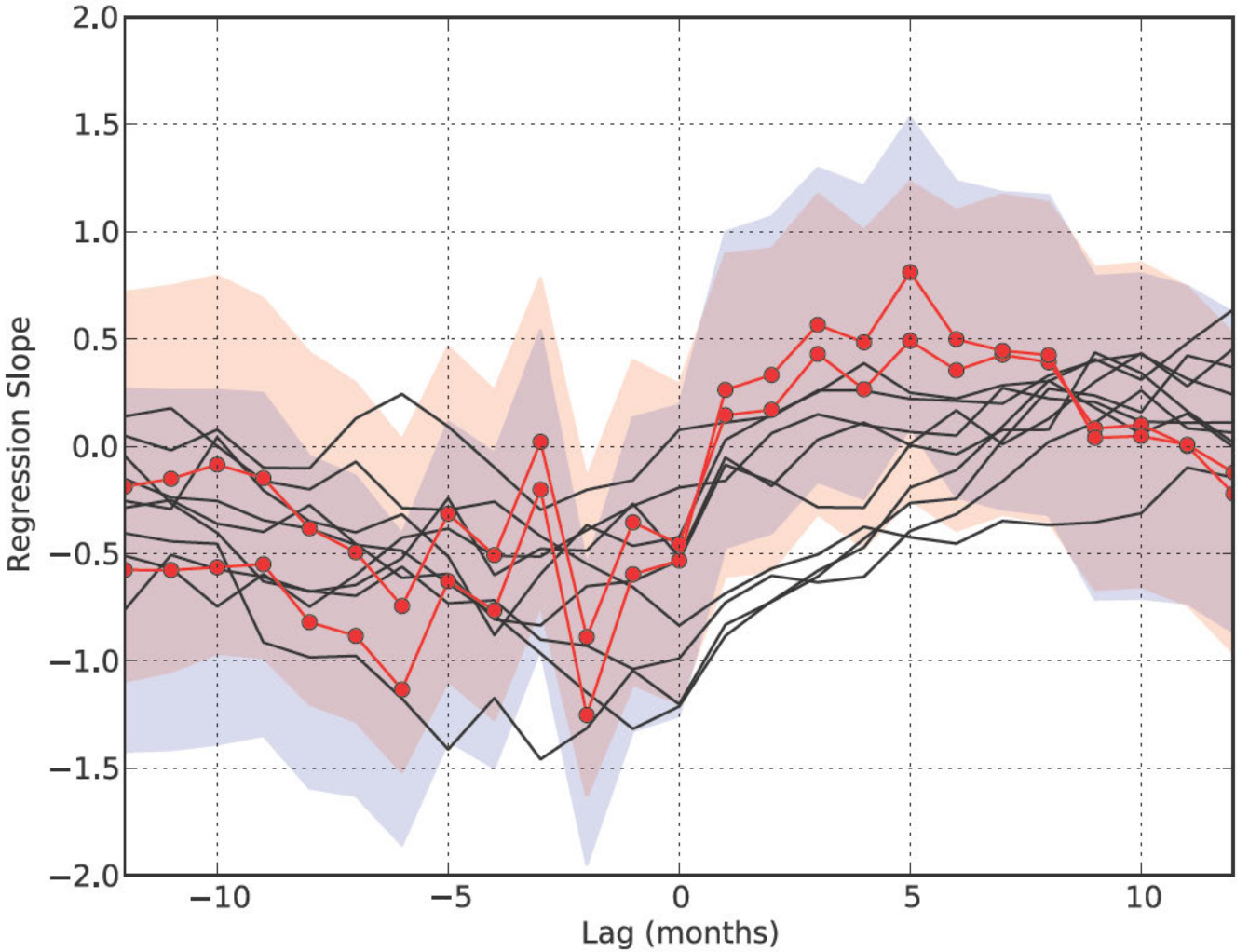
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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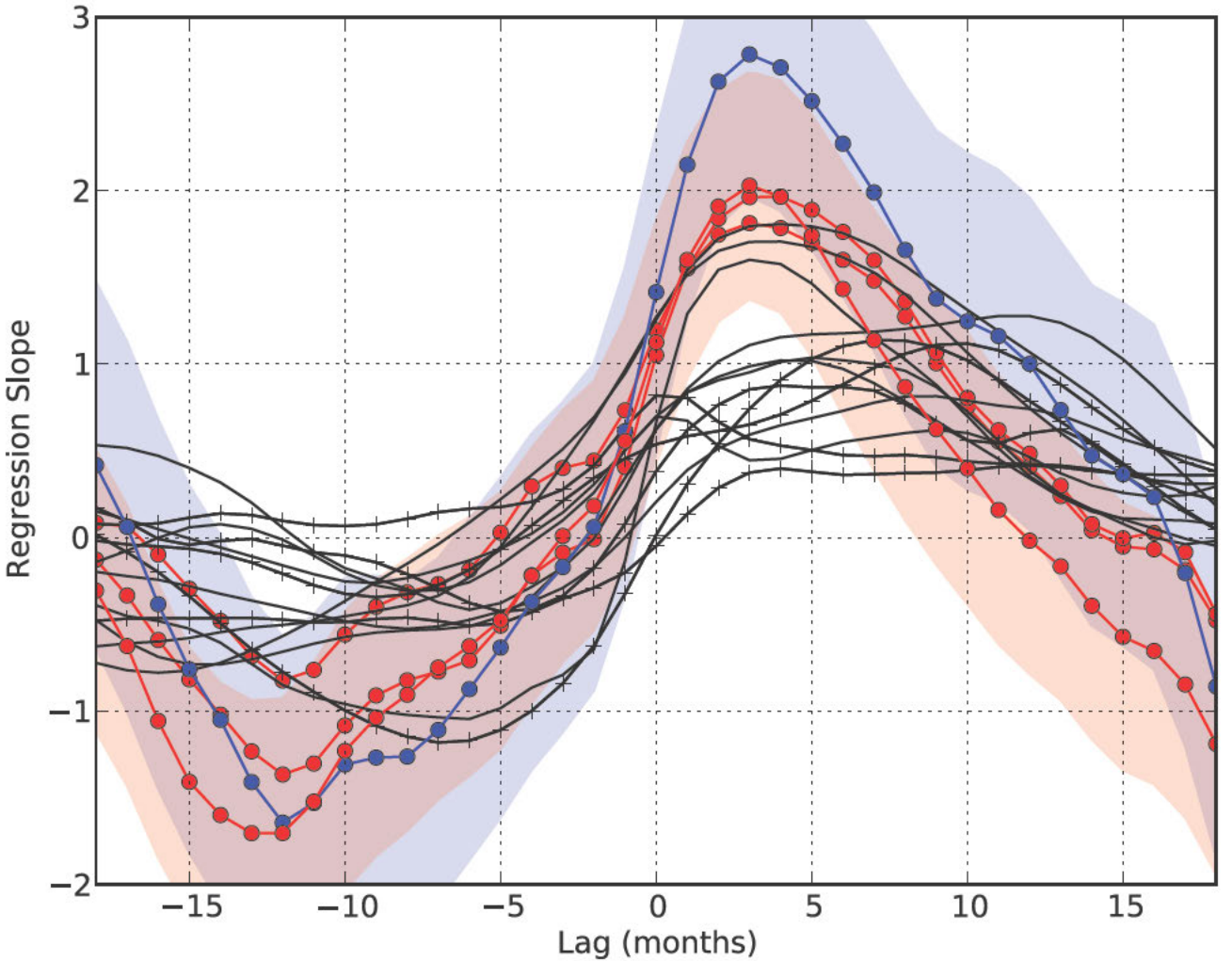
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From:	on behalf of Andrew Dessler
To:	Alex Dessler
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 9:47:24 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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Press release attached. You can get a copy of the paper here:
http://goo.gl/q4E7f

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979-862-1427
http://atmo.tamu.edu/profile/ADessler

COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

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Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

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Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

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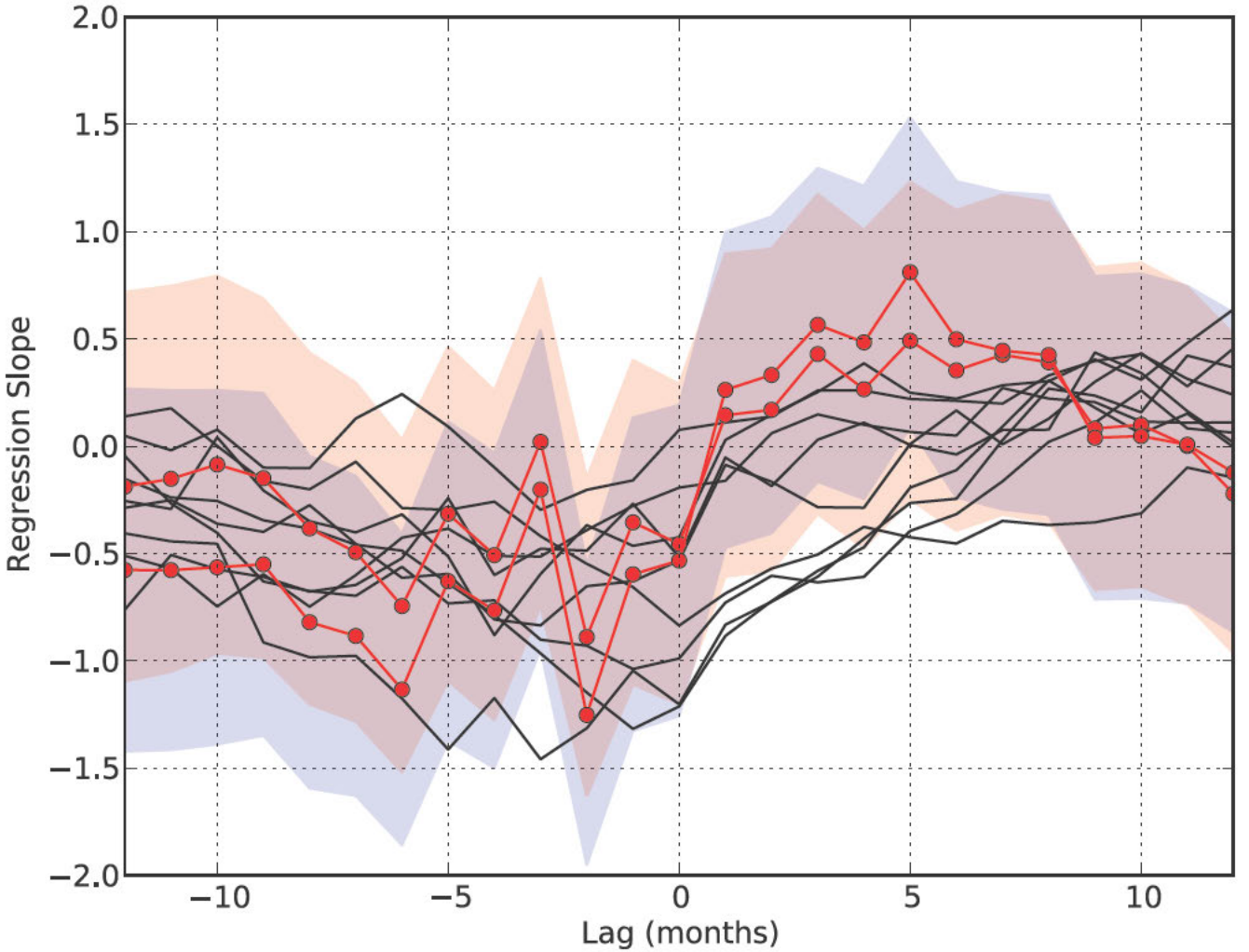
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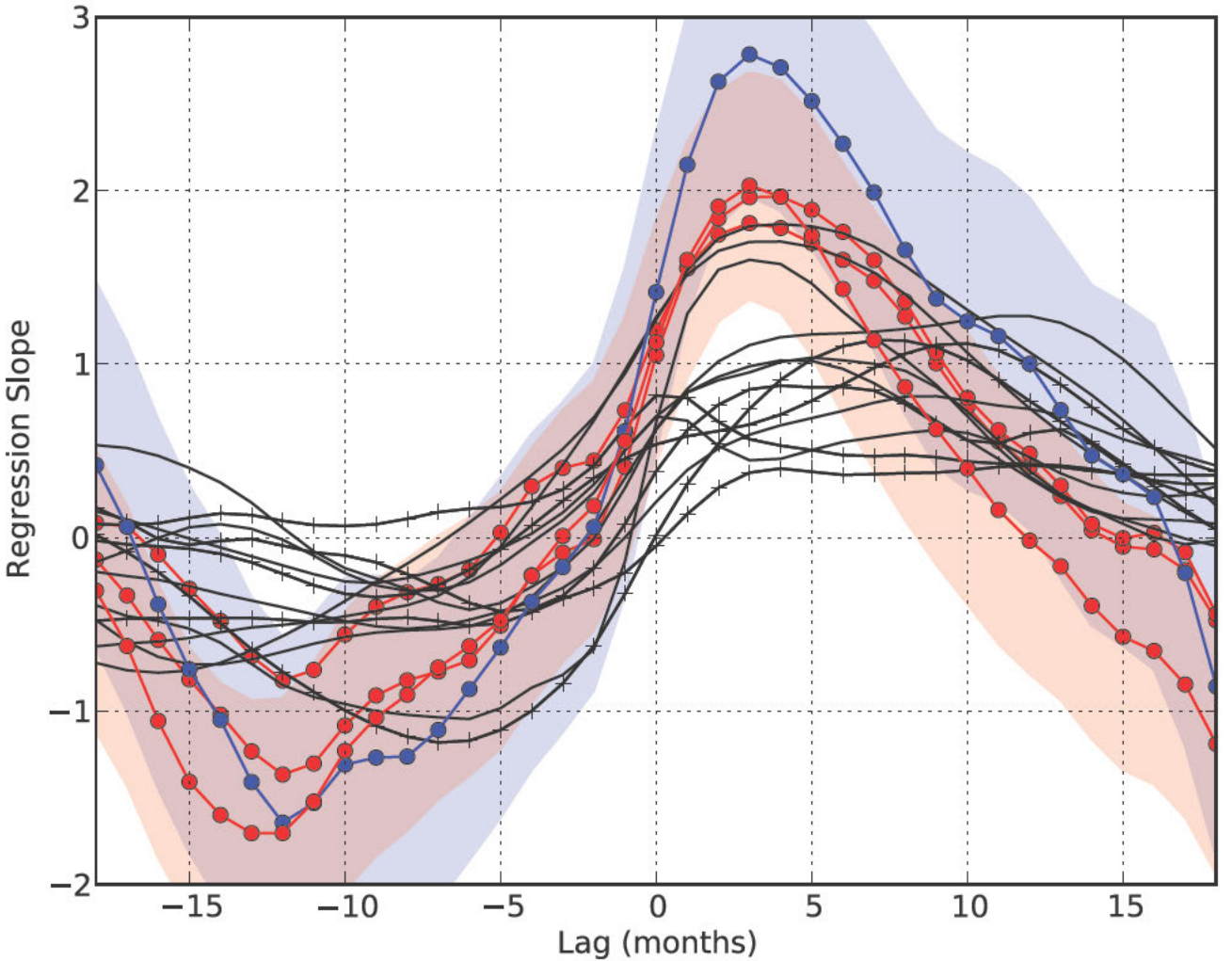
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From: Alexandra Witze
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 9:13:04 AM

Thanks Andrew - I'll take a look!

Best
Alex

On Sep 6, 2011, at 7:31 AM, Andrew Dessler wrote:

> Hi Alexandra. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)
>
> To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.
>
> Let me know if you have any questions.
>
> Thanks!
>
> --
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> Texas A&M University
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Science News, published since 1922, is the magazine of the Society for Science and the Public. Our weekly iPad publication, Science News Prime, is now available in the iTunes App store.

From: Andrew Revkin
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:53:20 AM

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around 9,000 total so far (significant error bars) in 2011 via google scholar: http://scholar.google.com/scholar?hl=en&q=co2+climate+greenhouse+change&btnG=Search&as_sdt=0%2C33&as_ylo=2011&as_vis=0

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ANDREW C. REVKIN
Dot Earth blogger, The New York Times
http://www.nytimes.com/dotearth
Senior Fellow, Pace Acad. for Applied Env. Studies
Cell: 914-441-5556
Twitter: @revkin Skype: Andrew.Revkin

From:	Richard Kerr
To:	Jesse Smith; adessler@tamu.edu
Subject:	Re: Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:51:40 AM

Thanks, Andy. The problem with asymmetric coverage of climate science is intractable, but your effort is commendable. Too bad Wagner didn't give you a headsup so you could have ridden that wave of coverage. All to the good, however.

Dick

Richard A. Kerr
Senior Writer, Science
phone 202 326-6587
fax 202 371-9227

1200 New York Avenue, N.W.
Washington, DC 20005

>>> Andrew Dessler <adessler@tamu.edu> 09/06/11 9:32 AM >>>
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adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From:
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:48:43 AM

Thanks-- on the road, but I'll read tonight

Sent from my iPhone; please excuse the inevitable typos

On Sep 6, 2011, at 9:32 AM, Andrew Dessler <adessler@tamu.edu> wrote:

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> <dessler paper on climate change.docx>
> <LC11SB11responseV3b.pdf>

From: on behalf of Andrew Dessler
To: Gillis, Justin
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:45:24 AM

To be honest, I have not been following editor-gate very closely. I think the episode is something of a distraction ... the real story is in the weakness of the skeptical science. At least that's my (admittedly biased) view. Let me know if you decide to report on this and need more info.

On Tue, Sep 6, 2011 at 8:39 AM, Gillis, Justin < wrote:
> Thanks for this, Andrew. As you know, we ignored the original Spencer paper, but I'll think about whether we ought to do something now. I'm confused about the situation with the editor of that journal, which Peter Gleick called our attention to last week. Why did the guy approve and publish the paper in the first place, if he now feels it was so bad as to be worth resigning over?
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>
> _____ __
> From: [On Behalf Of Andrew Dessler [adessler@tamu.edu]
> Sent: Tuesday, September 06, 2011 9:33 AM
> To: Gillis, Justin
> Subject: Fwd: new paper rebutting Spencer/Lindzen
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From: Gillis, Justin
To: Andrew Dessler
Subject: RE: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:41:39 AM

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From: [On Behalf Of Andrew Dessler
[adessler@tamu.edu]
Sent: Tuesday, September 06, 2011 9:33 AM
To: Gillis, Justin
Subject: Fwd: new paper rebutting Spencer/Lindzen

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http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Andrew Revkin
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:39:06 AM

Because I found that clouds did not trap enough energy to have a significant effect on the climate over the period studied, then whatever was driving the cloud variations (cosmic rays, meteorological variability, chemtrails) therefore could not have had an important impact on the climate.

On Tue, Sep 6, 2011 at 8:34 AM, Andrew Revkin < wrote:
> can you add a line for dot earth on whether/how this relates to the cosmic
> /cloud / climate arguments that have propagated post CERN?
>
> On Tue, Sep 6, 2011 at 9:28 AM, Andrew Dessler <adessler@tamu.edu> wrote:
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From: Andrew Revkin
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:34:10 AM

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From:	on behalf of Andrew Dessler
To:	Gillis, Justin
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:33:59 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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From: on behalf of Andrew Dessler
To: Richard Kerr; Jesse Smith
Subject: Fwd: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:33:19 AM
Attachments: dessler paper on climate change.docx
LC11SB11responseV3b.pdf

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From:	on behalf of Andrew Dessler
To:	Michael Lemonick
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:32:11 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

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Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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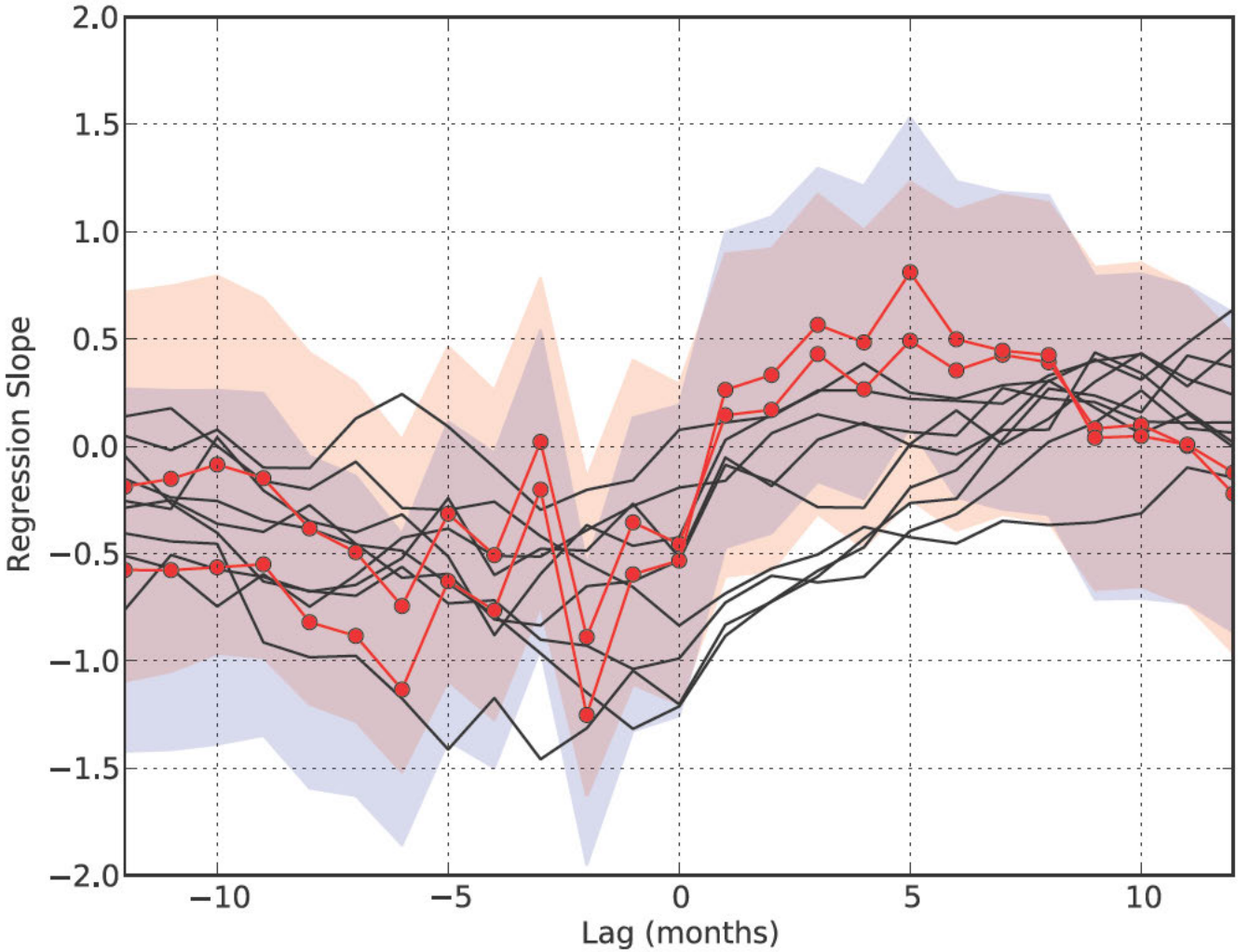
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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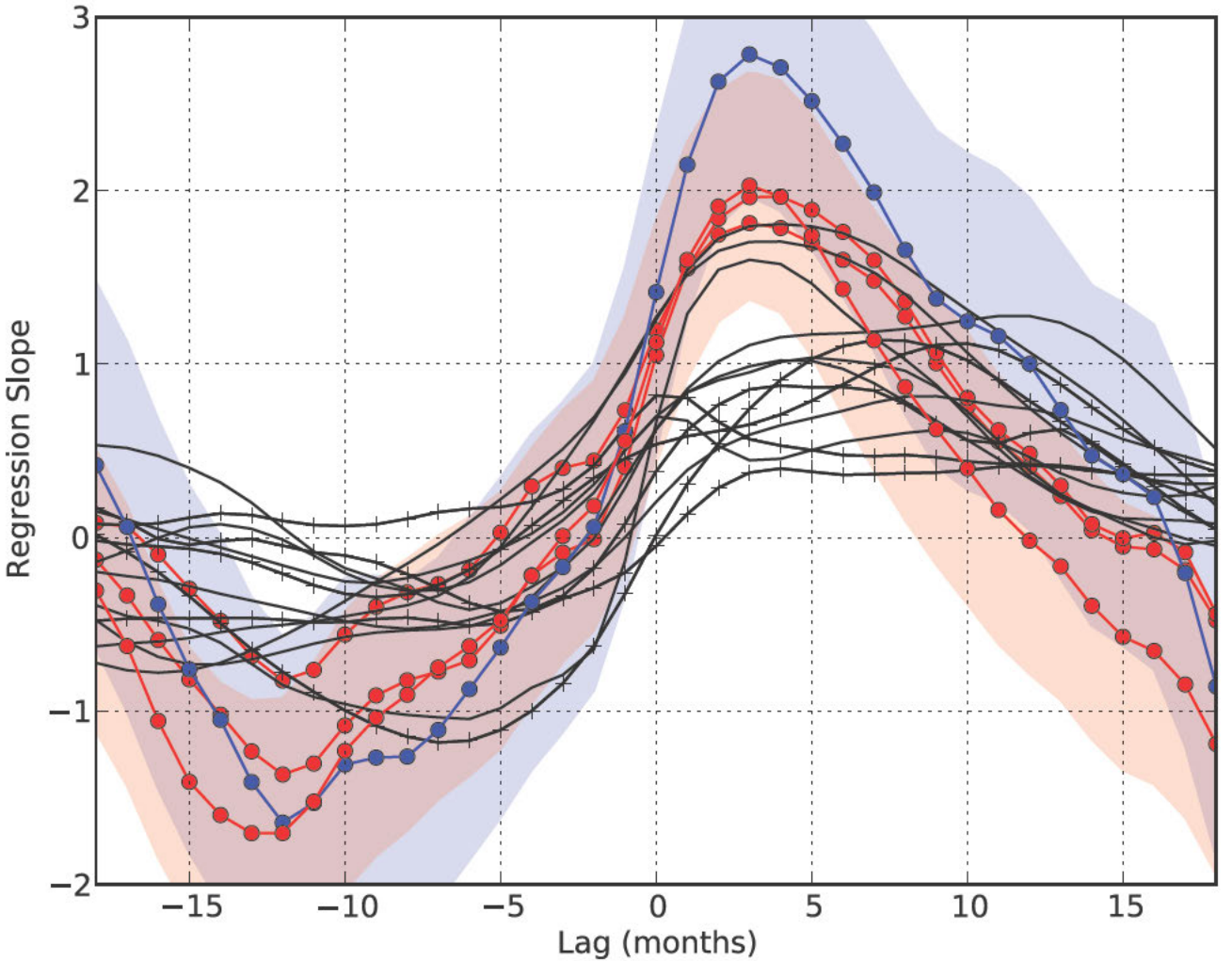
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From: on behalf of Andrew Dessler
To: Alexandra Witze
Subject: Fwd: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:31:22 AM
Attachments: dessler paper on climate change.docx
LC11SB11responseV3b.pdf

Hi Alexandra. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.

Let me know if you have any questions.

Thanks!

--
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COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

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Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

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Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

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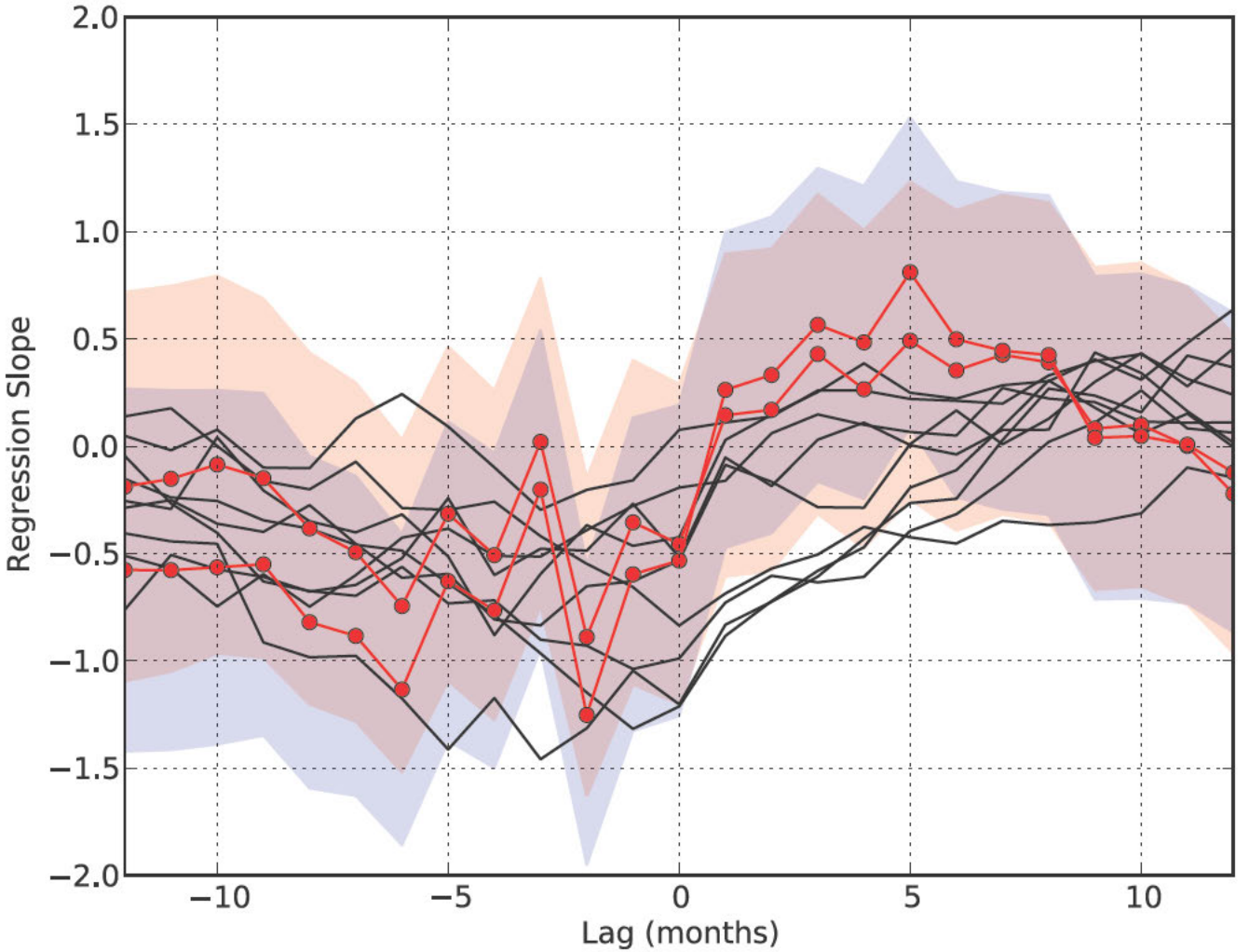
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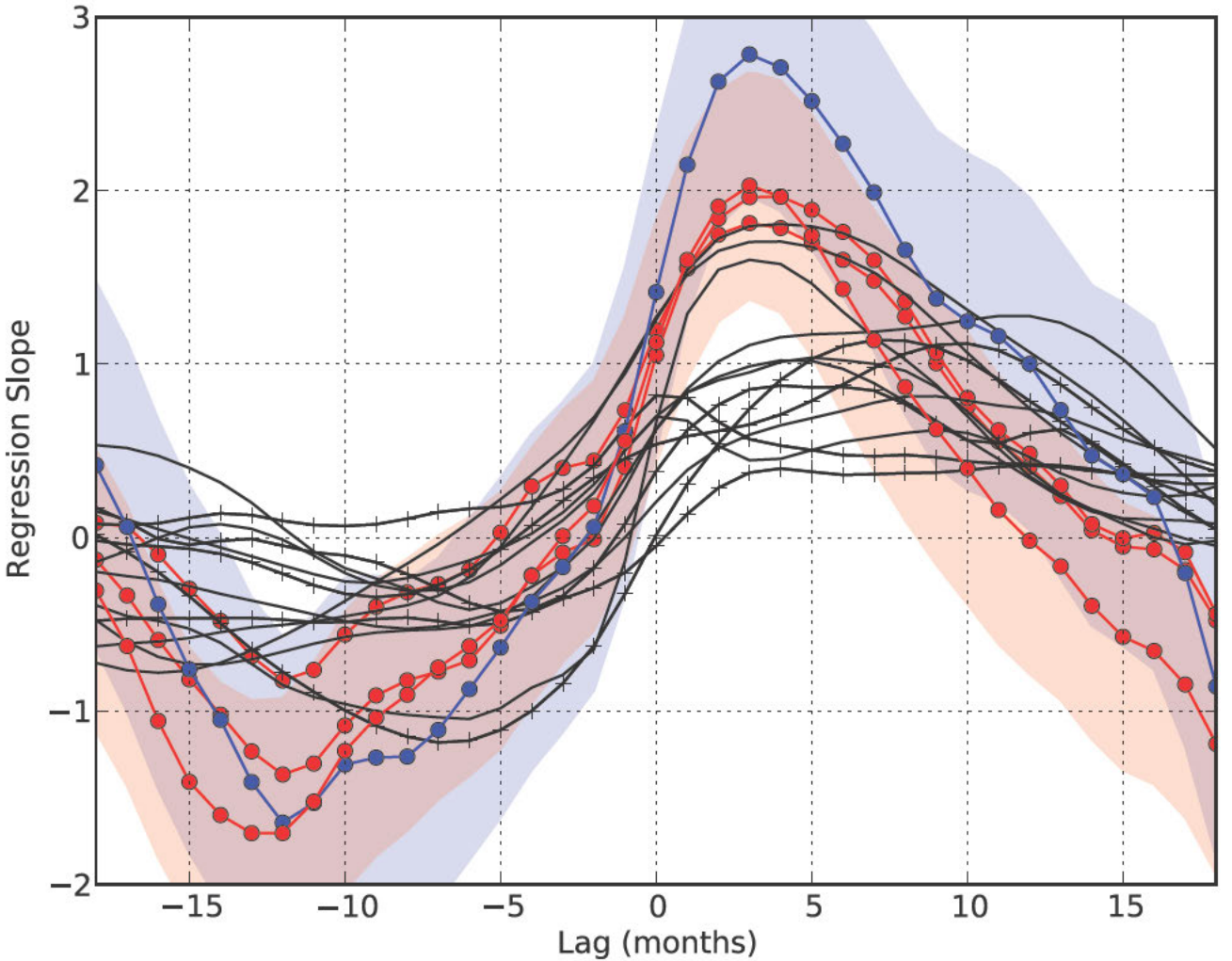
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From: Andrew Revkin
To: Andrew Dessler
Subject: Re: new paper rebutting Spencer/Lindzen
Date: Tuesday, September 06, 2011 8:30:18 AM

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great. will get to this soon.

your apt comment below says reams.

On Tue, Sep 6, 2011 at 9:28 AM, Andrew Dessler <adessler@tamu.edu> wrote:

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> Andrew Dessler
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| Please excuse typos; I had a (blessedly minor) stroke that's affected my typing a bit. |

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From:	on behalf of Andrew Dessler
To:	Stephanie Pappas
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:29:55 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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Hi Stephanie. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.

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Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

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Cloud variations and the Earth's energy budget

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Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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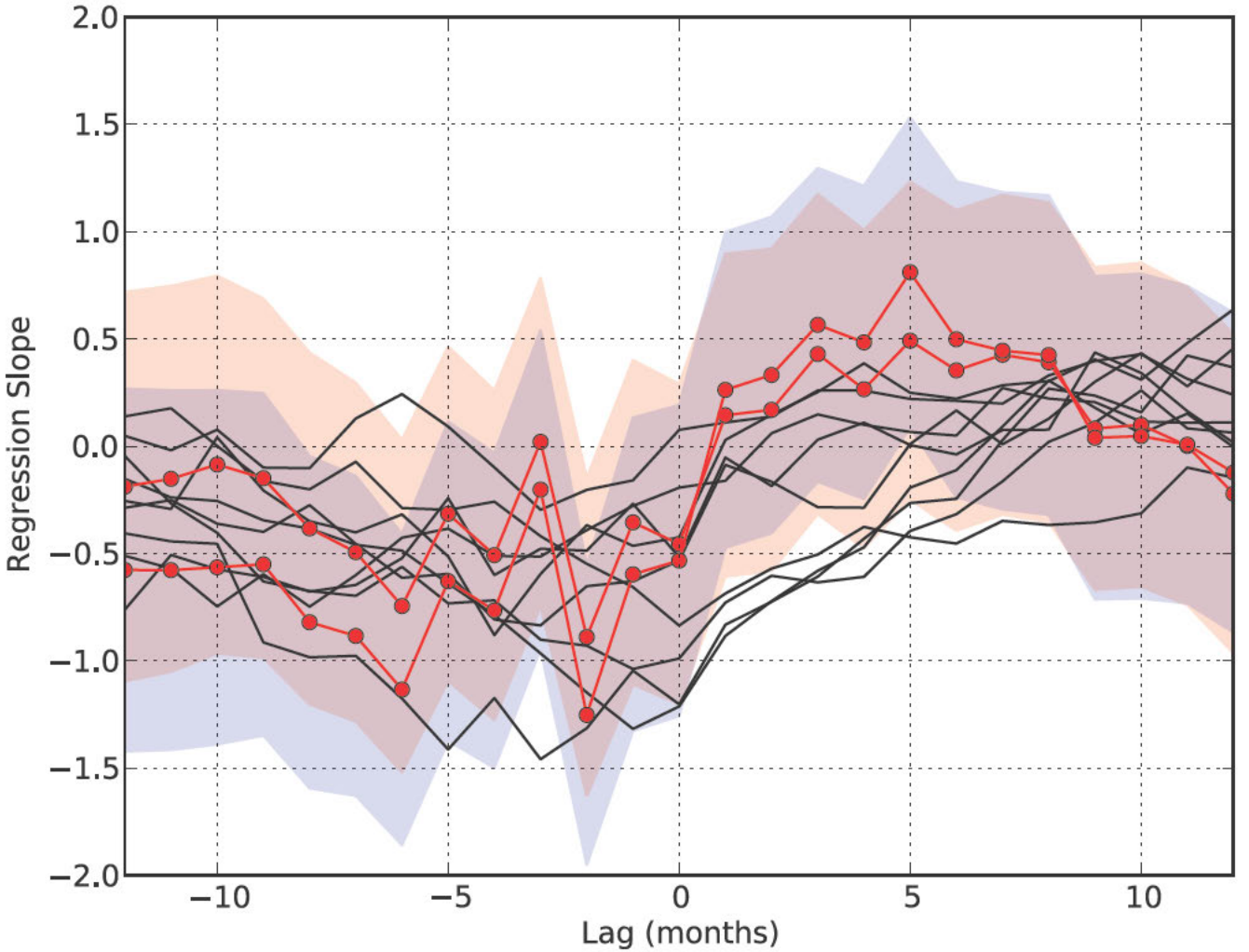
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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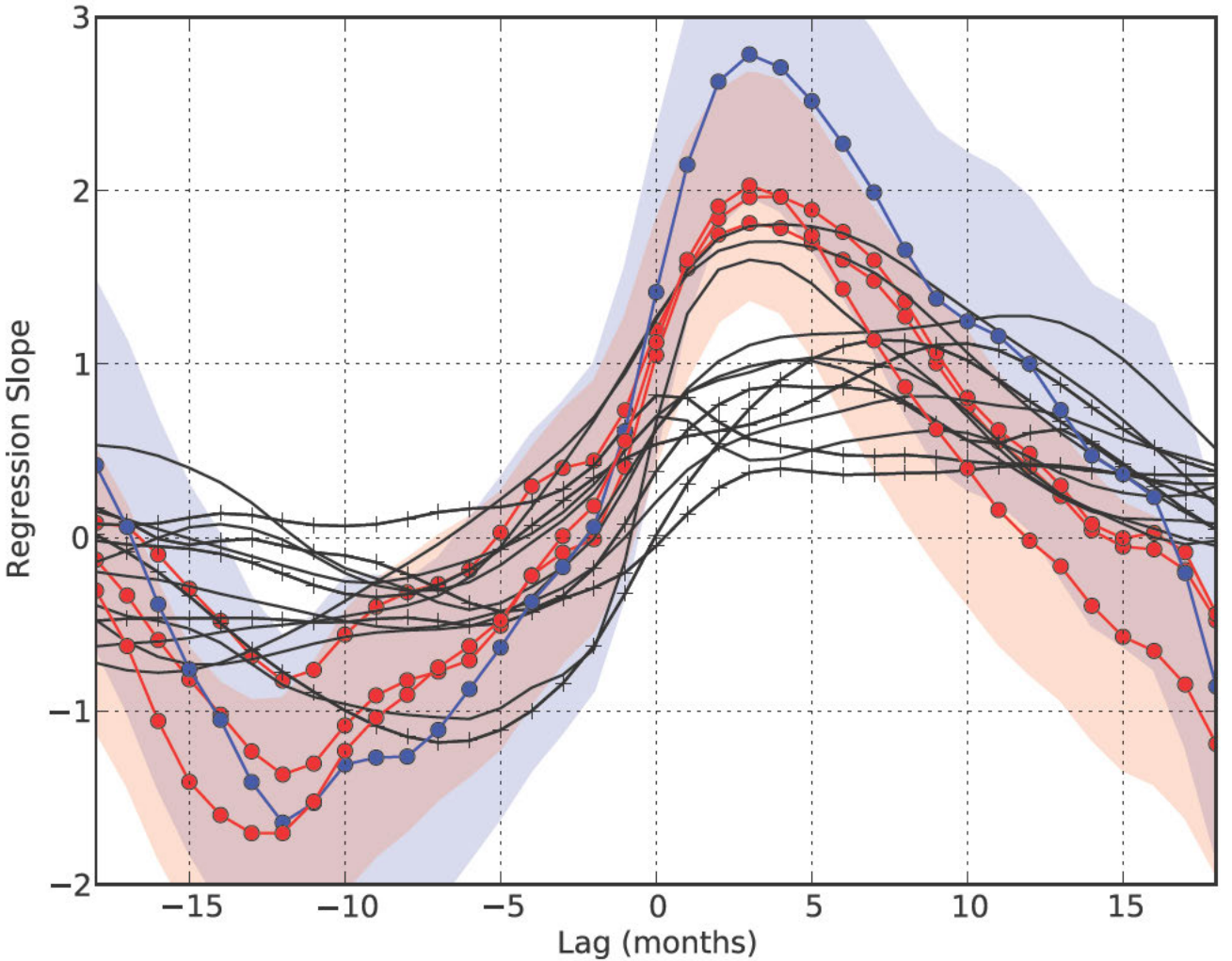
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A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

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SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

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There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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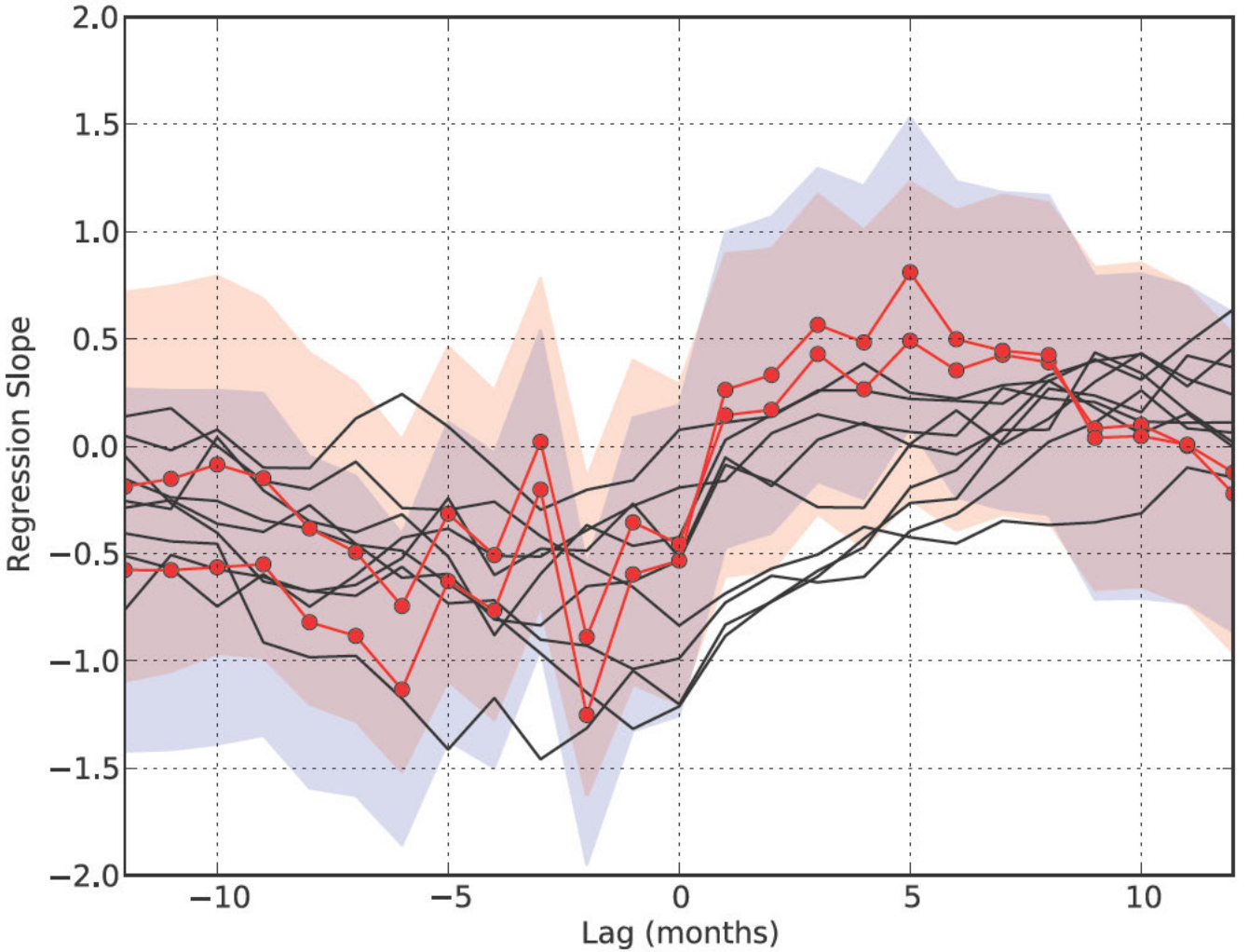
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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1.5
1.0
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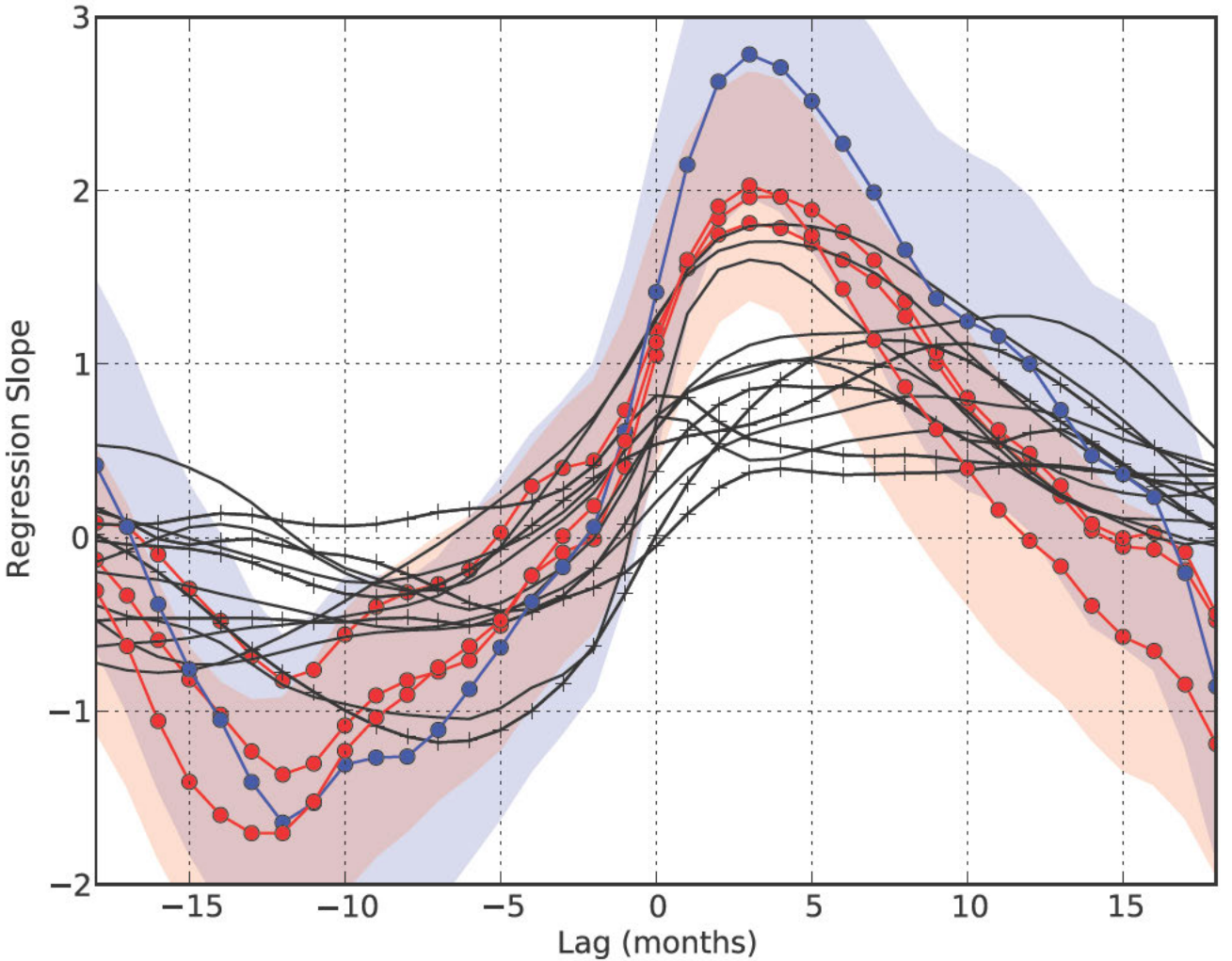
Regression Slope
−10
−5
0
5
10
Lag (months)


Regression Slope
Lag (months)
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2
1
0
−1
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−15
−10
−5
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10
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From:	on behalf of Andrew Dessler
To:	Eric Berger
Subject:	Fwd: new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:27:01 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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Hi Eric. I wanted to send you a press release and a copy of a new paper that was released this morning (EDT) by the AGU. This paper rebuts the suggestions by Lindzen and Spencer that climate change is caused by clouds. Attached you can find the paper and the press release. I've put up a youtube video (http://goo.gl/zFJmt) that explains the work. (my paper coincidentally follows the resignation on Friday of the editor of the journal that published Spencer's paper)

To me, the real story here is that, every month, dozens if not hundreds of papers are published that are in agreement with the mainstream theory of climate science. But, every year, one or two skeptical papers get published, and these are then trumpeted by sympathetic media outlets as if they'd discovered the wheel. It therefore appears to the general public that there's a debate.

Let me know if you have any questions.

Thanks!

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Andrew Dessler
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COLLEGE STATION, Sept. 6, 2011 – Clouds only amplify climate change, says a Texas A&M University professor in a study that rebuts recent claims that clouds are actually the root cause of climate change.

Andrew Dessler, a Texas A&M atmospheric sciences professor considered one of the nation's experts on climate variations, says years of data support the mainstream and long-held view that clouds are primarily acting as a so-called feedback that amplifies warming from human activity. His work is published today in the American Geophysical Union's peer-reviewed journal *Geophysical Research Letters.*

Dessler studied El Nino and La Nina cycles over the past 10 years and calculated the Earth's "energy budget" over this time. El Nino and La Nina are times of the year when waters in the central Pacific Ocean tend to get warmer or colder, and these changes have a huge impact on much of the world's weather systems for months.

Dessler found that clouds played a very small role in initiating these climate variations — in agreement with mainstream climate science and in direct opposition to these previous claims.

"The bottom line is that clouds have not replaced humans as the cause of the recent warming the Earth is experiencing," Dessler explains.

Texas is currently in one of the worst droughts in the state's history, and most scientists believe it is a direct result of La Nina conditions that have lingered in the Pacific Ocean for many months.

Dessler added that, "Over a century, however, clouds can indeed play an important role amplifying climate change."

"I hope my analysis puts an end to this claim that clouds are causing climate change," he adds.

For more information about Dessler's research, go to

ohttp://geotest.tamu.edu/userfiles/216/GRL2011v5.m4a

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Contact: Andy Dessler at (979) 862-1427 or adessler@tamu.edu or Keith Randall, News & Information Services, at (979) 845-4644 or keith-randall@tamu.edu

Cloud variations and the Earth's energy budget

A.E. Dessler
Dept. of Atmospheric Sciences
Texas A&M University
College Station, TX

Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

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As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

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This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

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SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

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Conclusions

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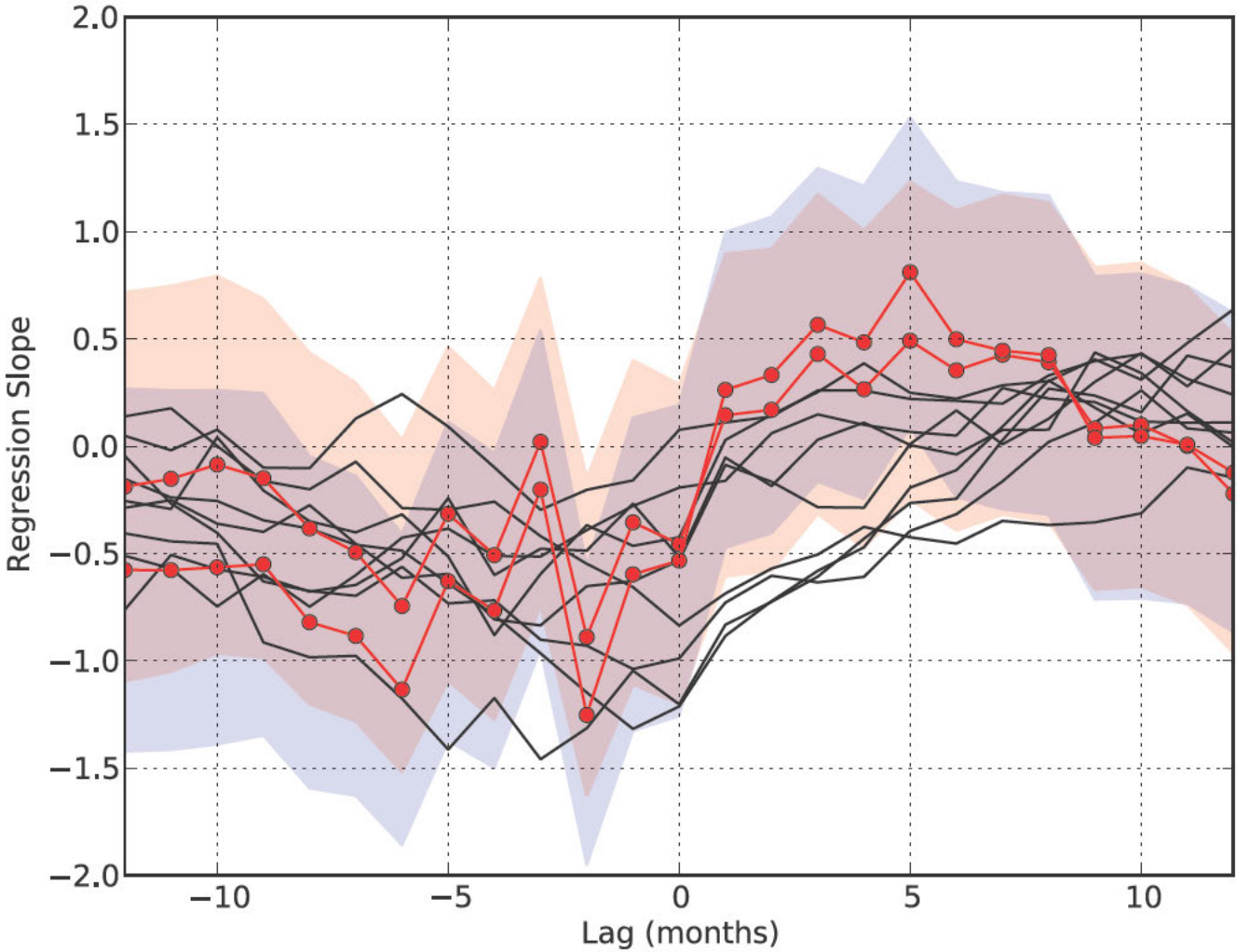
Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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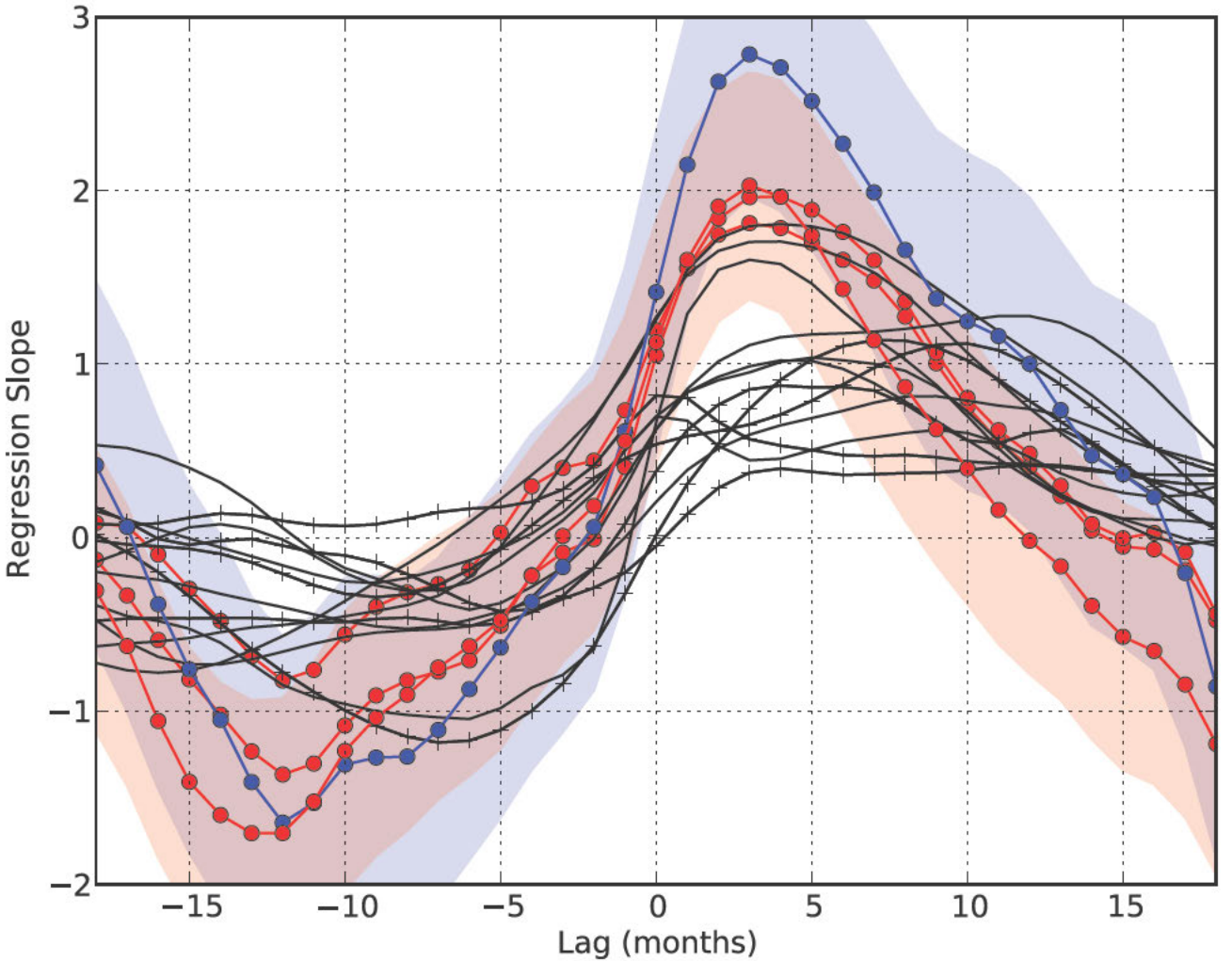
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From:	on behalf of Andrew Dessler
To:	Oliver Morton
Subject:	new paper rebutting Spencer/Lindzen
Date:	Tuesday, September 06, 2011 8:26:29 AM
Attachments:	dessler paper on climate change.docx LC11SB11responseV3b.pdf

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Cloud variations and the Earth's energy budget

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Abstract: The question of whether clouds are the cause of surface temperature changes, rather than acting as a feedback in response to those temperature changes, is explored using data obtained between 2000 and 2010. An energy budget calculation shows that the energy trapped by clouds accounts for little of the observed climate variations. And observations of the lagged response of top-of-atmosphere (TOA) energy fluxes to surface temperature variations are not evidence that clouds are causing climate change.

Introduction

The usual way to think about clouds in the climate system is that they are a feedback — as the climate warms, clouds change in response and either amplify (positive cloud feedback) or ameliorate (negative cloud feedback) the initial change [e.g., Stephens, 2005]. In recent papers, Lindzen and Choi [2011, hereafter LC11] and Spencer and Braswell [2011, hereafter SB11] have argued that reality is reversed: clouds are the cause of, and not a feedback on, changes in surface temperature. If this claim is correct, then significant revisions to climate science may be required.

Energy budget calculation

LC11 (their Eq. 8) and SB11 (their Eq. 1) both write the Earth's energy budget as:

$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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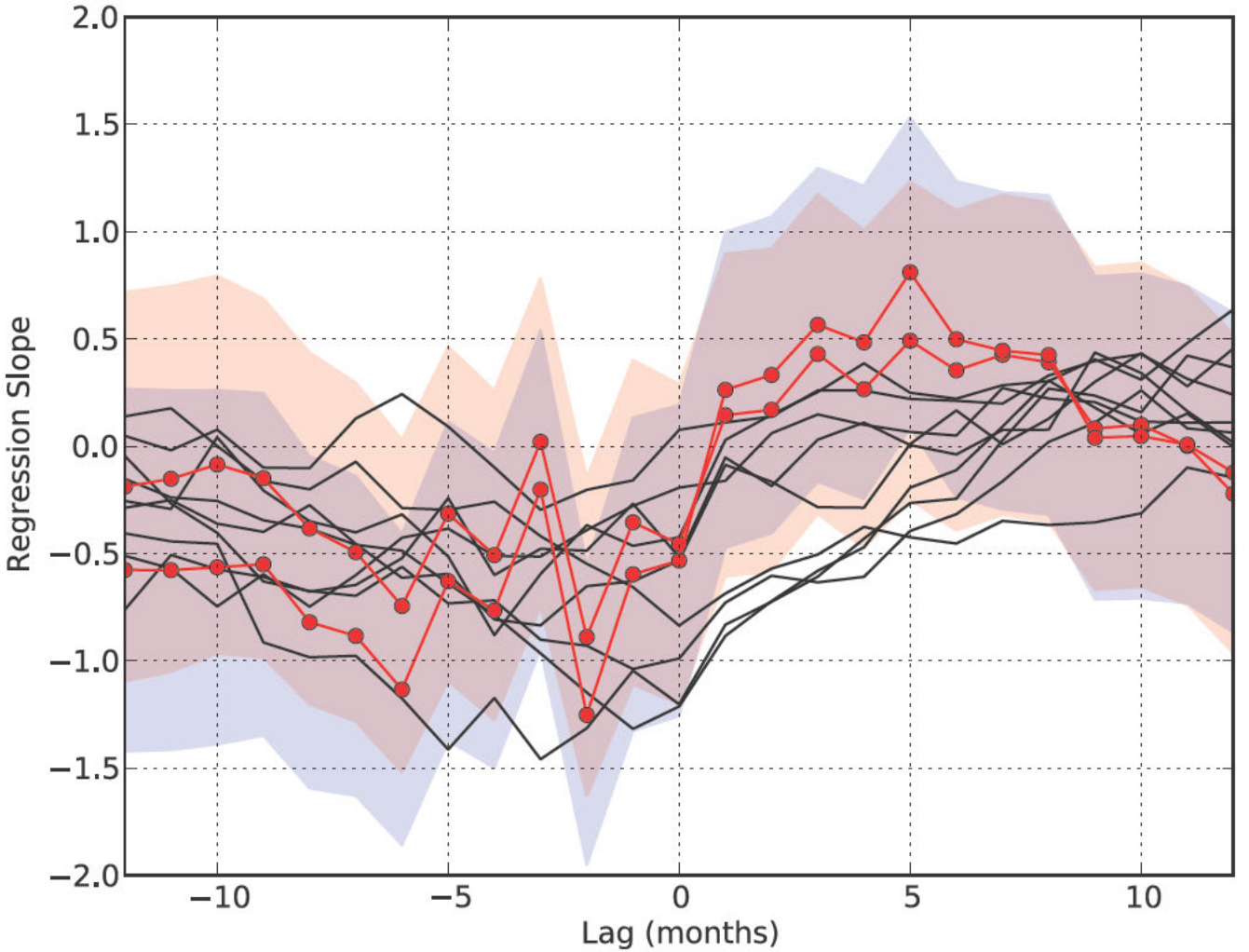
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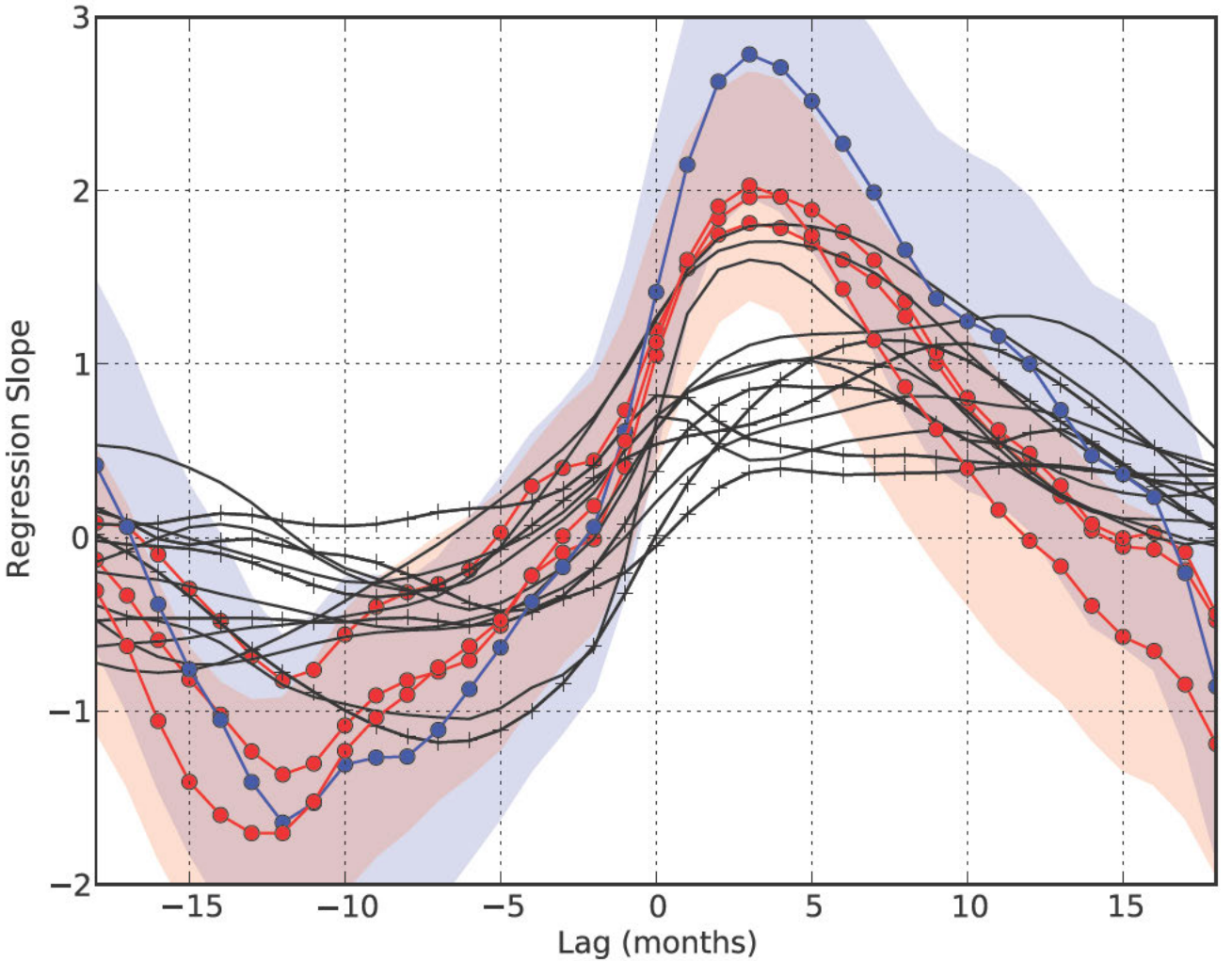
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$$C\frac{dT_s}{dt} = \Delta R_{cloud} + \Delta F_{ocean} - \lambda \Delta T_s \qquad (1)$$

C is the heat capacity of the ocean's mixed layer, ΔT_s is the surface temperature, and ΔF_{ocean} is the heating of the climate system by the ocean. The term $-\lambda\Delta T_s$ represents the enhanced emission of energy to space as the planet warms. λ is the climate sensitivity and it contains the Planck response as well as the climate feedbacks. ΔR_{cloud} is the change in TOA flux due to clouds. Note that ΔR_{cloud} is not a feedback in this formulation — it is a forcing and is independent of surface temperature (the cloud feedback is in the $-\lambda\Delta T_s$ term). All quantities are global monthly average anomalies (anomalies are calculated by subtracting the mean annual cycle.). Other terms, such as the change in radiative forcing by greenhouse gases, are small over the period examined, so they are ignored.

The formulation of Eq. 1 is potentially problematic because the climate system is defined to include the ocean, yet one of the heating terms is flow of energy to/from the ocean (ΔF_{ocean}). This leads to the contradictory situation where heating of their climate system by the ocean ($\Delta F_{ocean} > 0$) causes an increase of energy in the ocean ($C(dT_s/dt) > 0$), apparently violating energy conservation. While it may be possible to define the terms so that Eq. 1 conserves energy, LC11 and SB11 do not provide enough information to show that they have actually done so. However, to comprehensively evaluate the arguments of LC11 and SB11, I simply note this potential problem and assume in the rest of the paper that Eq. 1 is correct.

In their analyses, LC11 and SB11 test Eq. 1 by creating synthetic data for ΔF_{ocean} and ΔR_{cloud}, and this requires an assumption about the relative magnitudes of these terms. LC11 choose the ratios of the standard deviations of the time series $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 2$ while SB11 choose, for their most realistic case, $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) \approx 0.5$ (the time series are anomalies, so their means are zero by definition; thus, the standard deviation is a measure of the magnitude of the terms).

However, it is possible to use data to estimate the magnitude of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud})$. I will focus on the period from March 2000 to February 2010, during which good data exist and the primary climate variations were caused by ENSO. This is the same period evaluated by SB11, and LC11's analysis also included this period.

To evaluate the magnitude of the first term, $C(dT_s/dt)$, I assume a heat capacity C of 168 W-month/m^2/K, the same value used by LC11 (as discussed below, SB11's heat capacity is too small). The time derivative is estimated by subtracting each month's global average ocean surface temperature from the previous month's value. Temperatures used in this calculation come from NASA's Modern Era Retrospective-analysis for Research and Application (MERRA) [Rienecker et al., 2011]. The standard deviation of the monthly anomaly time series, $\sigma(C(dT_s/dt))$, is 9 W/m^2.

This can be confirmed by looking at the Argo ocean heat content data covering 2003-2008. Using data reported in Douglass and Knox [2009], the month-to-month change in monthly interannual heat content anomalies can be calculated (σ = 1.2×10^{22} J/month). Assuming the ocean covers 70% of the planet, this corresponds to 13 W/m^2, in agreement with the previous estimate.

In Dessler [2010] (hereafter D10), the energy trapped by clouds each month over this period was computed (LC11 calculated similar values). If all of this energy is assumed to be a climate forcing — i.e., unrelated to surface temperature changes — then I can use these values for ΔR_{cloud}. This yields $\sigma(\Delta R_{cloud}) = 0.5$ W/m^2. Calculations for potential water vapor forcing are of a similar magnitude.

To calculate $\lambda \Delta T_s$, I assume that λ is between 1 and 6 W/m^2/K. Global and monthly averaged ΔT_s are from the MERRA reanalysis. I calculate that $\sigma(\lambda \Delta T_s) < 0.4$ W/m^2.

ΔF_{ocean} can be calculated as a residual using Eq. 1 and the terms calculated above. The result is that $\Delta F_{ocean} \approx C(dT_s/dt)$, and that $\sigma(\Delta F_{ocean}) \approx \sigma(C(dT_s/dt))$. Despite potential problems in Eq. 1, the conclusion here is robust: energy trapped by clouds can explain only a few percent of the surface temperature changes. This is consistent with previous work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport [e.g., Trenberth et al., 2002;

Trenberth et al., 2010] and it means that clouds were not causing significant climate change over this period.

A related point made by both LC11 and SB11 is that regressions of TOA flux or its components vs. ΔT_s will not yield an accurate estimate of the climate sensitivity λ or the cloud feedback. This conclusion, however, relies on their particular values for $\sigma(\Delta F_{ocean})$ and $\sigma(\Delta R_{cloud})$. Using a more realistic value of $\sigma(\Delta F_{ocean})/\sigma(\Delta R_{cloud}) = 20$, regression of TOA flux vs. ΔT_s yields a slope that is within 0.4% of λ, a result confirmed in Fig. 2b of Spencer and Braswell [2008]. This also applies to the individual components of the TOA flux, meaning that regression of ΔR_{cloud} vs. ΔT_s yields an accurate estimate of the magnitude of the cloud feedback, thereby confirming the results of D10.

As a side note, SB11 estimated their heat capacity by regressing ΔR_{cloud} vs. dT_s/dt and assuming that C is the slope. This is only correct, however, if $\Delta F_{ocean} = 0$. For the realistic case where $\sigma(\Delta F_{ocean}) >> \sigma(\Delta R_{cloud})$, the slope is much less than C, which explains why SB11's heat capacity is too small.

Comparison with models: LC11

LC11 base their conclusion that clouds are a forcing rather a feedback on a plot like the one in Fig. 1 (see their Fig. 9). The figure shows the slope of the correlation between ΔR_{cloud} and ΔT_s as a function of lag for the observations in D10.

The observations show that larger negative slopes exist when the cloud time series leads the surface temperature, with mostly positive slopes when the temperatures leads the cloud time series. Based on this correlation, LC11 conclude that clouds must be initiating the climate variations.

I've also plotted the results from nine models from the Atmospheric Model Intercomparison Project (AMIP) (CNRM CM3, INMCM 3.0, IPSL CM4, MIROC 3.2 MEDRES, MIROC 3.2 HIRES, MPI ECHAM 5, MRI CGCM 2.3.2a, NCAR CCSM, UKMO HADGEM1). While some disagreements between the observations and models exist, the models clearly simulate the key aspect of the data identified by LC11: larger negative slopes when ΔR_{cloud} leads ΔT_s.

This is an important result because the sea surface temperatures (SST) are specified in AMIP models. This means the interaction in these models is one-way: clouds respond to SST changes, but SST does not respond to cloud changes. In other words, realistic ΔR_{cloud} variations are generated in these models by specifying ΔT_s variations. The suggests that the observed lead-lag relation is a result of variations in atmospheric circulation driven by ΔT_s variations and is not evidence that clouds are initiating climate variations. This conclusion also agrees with the energy budget presented earlier that concluded that clouds are not trapping enough energy to explain the ΔT_s variations.

Calculations using fully coupled models yield similar lead-lag relations as the AMIP models. This means that closing the loop to allow clouds to affect SST does not change these conclusions.

Comparison with models: SB11

SB11's analysis is built on a plot like LC11's, but using TOA net flux instead of ΔR_{cloud}. Figure 2 shows my reconstruction of SB11's Fig. 3. Each line shows, for a single data set, the slope of the relation between TOA net flux and ΔT_s as a function of lag between them. The colored lines are observations: the blue line shows the data used by SB11 (CERES fluxes and HadCRUT3 temperature [Brohan et al., 2006]); the red lines use the same flux data, but different surface temperature data sets (MERRA, ERA-Interim, GISTEMP [Hansen et al., 2010]). The shaded regions show the 2σ uncertainties of the observations using GISTEMP and HadCRUT3. As done by SB11, all data have been 1-2-1 filtered.

The black lines are from pre-industrial control runs of 13 fully coupled climate models (CCCMA CGCM 3.1, CNRM CM3, GFDL CM 2.0, GFDL CM 2.1, GISS ER, FGOALS 1.0G, INMCM 3.0, IPSL CM4, MIROC 3.2 HIRES, MIROC 3.2 MEDRES, MPI ECHAM5, MRI CGCM 2.3.2A, NCAR CCSM 3.0) from the CMIP3 database [Meehl et al., 2007] (SB11 used de-trended 20th century runs; differences with my calculations appear minor). The models with the crosses '+' are 5 of the 6 models analyzed by SB11.

There are three notable points to be made. First, SB11 analyzed 14 models, but they plotted only six models and the particular observational data set that provided

maximum support for their hypothesis. Plotting all of the models and all of the data provide a much different conclusion. Second, some of the models (not plotted by SB11) agree with the observations, which means that the observations are not fundamentally inconsistent with mainstream climate models containing positive net feedbacks. Third, the models that do a good job simulating the observations (GFDL CM 2.1, MPI ECHAM5, and MRI CGCM 2.3.2A) are among those that have been identified as realistically reproducing ENSO [Lin, 2007]. And since most of the climate variations over this period were due to ENSO, this suggests that the ability to reproduce ENSO is what's being tested here, not anything directly related to equilibrium climate sensitivity.

ENSO coupling in the model

This leads us to a fundamental problem in their analysis of Eq. 1: LC11 and SB11 model ΔF_{ocean} as random time series, but this is incorrect. ΔF_{ocean} is actually a function of ΔT_s, with the coupling occurring via the ENSO dynamics: ΔT_s controls the atmospheric circulation, which drives ocean circulation, which determines ΔF_{ocean}, which controls ΔT_s.

Putting everything together, the evolution of ΔT_s during ENSO is due primarily to heat transport by the ocean. As the AMIP models show, these changes in ΔT_s also change clouds, but the impact of these cloud changes on ΔT_s is small. Thus, the lead-lag relation between TOA flux and ΔT_s tells us nothing about the physics driving ΔT_s.

Conclusions

These calculations show that clouds did not cause significant climate change over the last decade (over the decades or centuries relevant for long-term climate change, on the other hand, clouds can indeed cause significant warming). Rather, the evolution of the surface and atmosphere during ENSO variations are dominated by oceanic heat transport. This means in turn that regressions of TOA fluxes vs. ΔT_s can be used to accurately estimate climate sensitivity or the magnitude of climate feedbacks. In addition, observations presented by LC11 and SB11 are not in fundamental disagreement with mainstream climate models, nor do they provide evidence that clouds are causing climate change. Suggestions that significant revisions to mainstream climate science are required are therefore not supported.

Acknowledgments: This work was supported by NSF grant AGS-1012665 to Texas A&M University. I thank A. Evan, J. Fasullo, D. Murphy, K. Trenberth, M. Zelinka, and A.J. Dessler for useful comments.

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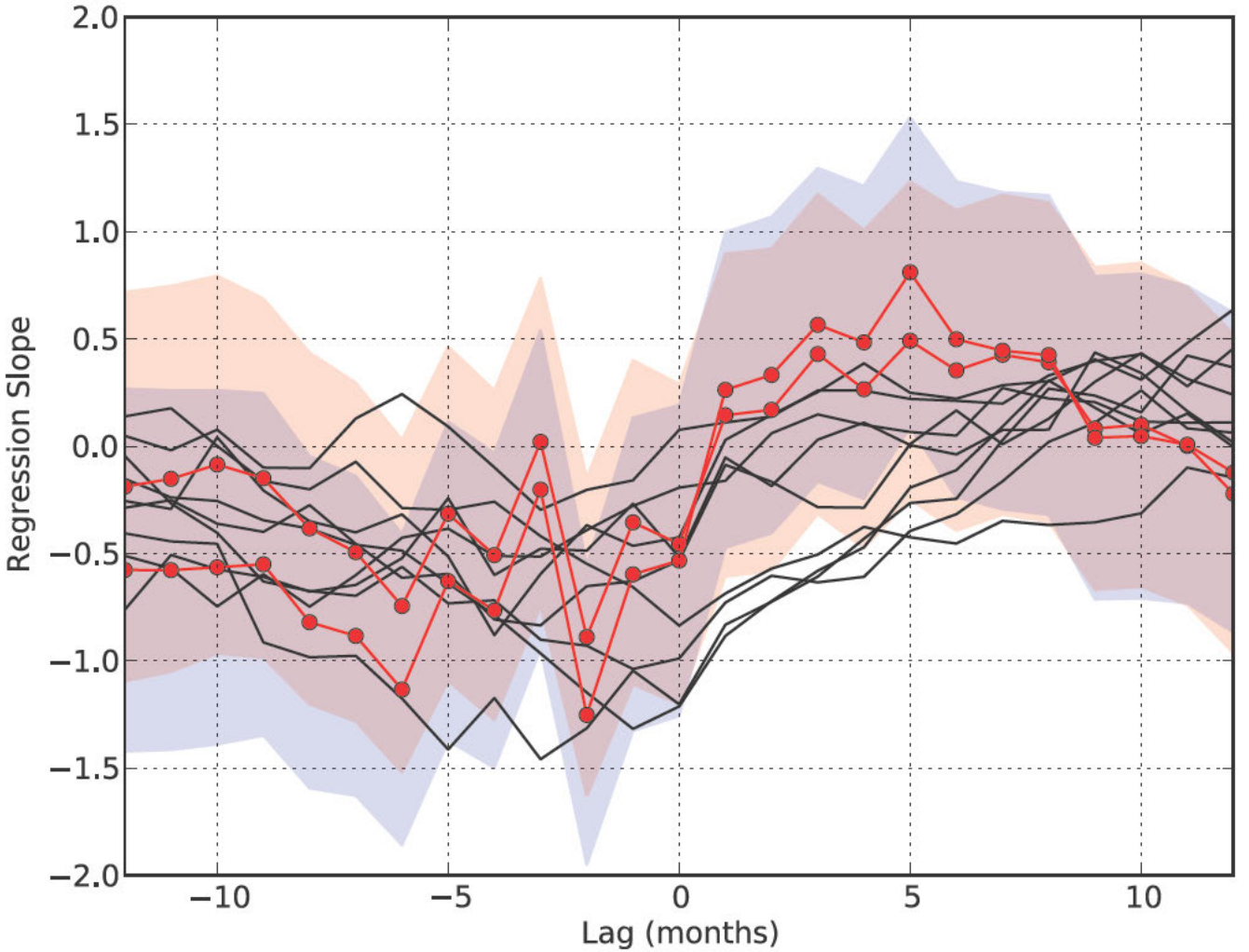
Figure 1. The slope of the regression ($W/m^2/K$) of energy trapped by clouds ΔR_{cloud} vs. surface temperature ΔT_s, as a function of the lag between the time series in months. Negative values of lag indicate that ΔR_{cloud} leads ΔT_s. The red lines are based on the observations in D10, using CERES flux data [Wielicki et al., 1996] and either ERA-Interim [Dee et al., 2011] or MERRA reanalyses [Rienecker et al., 2011]. The red and blue shading indicates the 2σ uncertainty of the lines (purple shading is where the red and blue shading overlaps). The thin black lines are AMIP climate model runs.

Figure 2. Slope of the relation between TOA net flux and ΔT_s, in $W/m^2/K$ as a function of lag between the data sets (negative lags mean that the flux time series leads ΔT_s). The colored lines are from observations (covering 3/2000-2/2010 using the same TOA flux data, but different time series for ΔT_s); the shading represents the 2σ uncertainty of two of the data sets. The black lines are from 13 fully coupled pre-industrial control runs; lines with the crosses '+' are models used by SB11. Following SB11, all data are 1-2-1 filtered. See the text for more details about the plot.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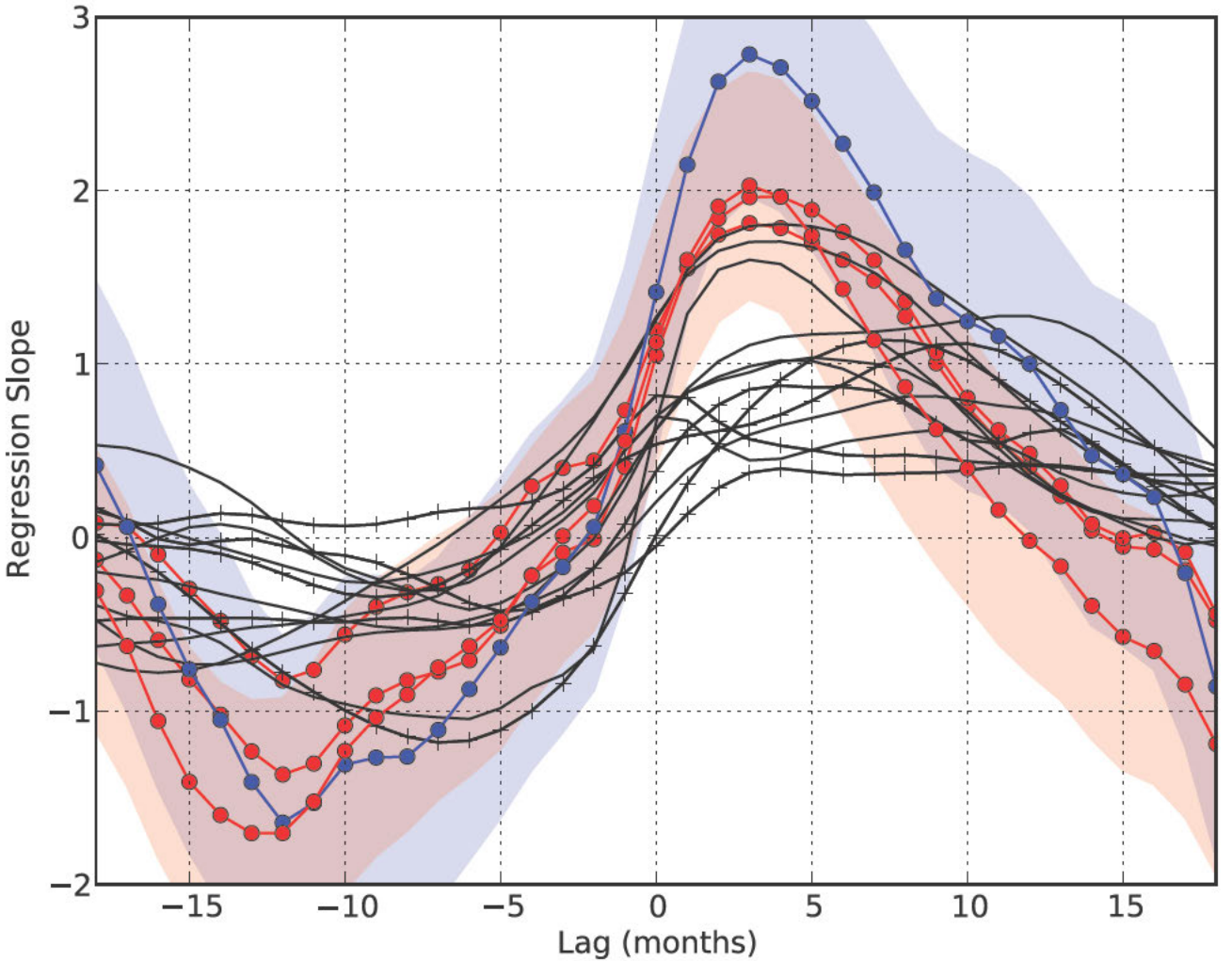
Lag (months)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Lag (months)
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−5
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5
10
15

From: on behalf of Andrew Dessler
To: Steve McIntyre
Subject: Re: Dessler et al 2010
Date: Monday, September 05, 2011 9:45:51 PM
Attachments: fig1b.txt
fig1a.txt
fig1c.txt

Attached are the data for Fig. 1. Each data set is a time series running from 3/2000-2/2010. You can make Fig. 2 by plotting erats from Fig. 1c vs. eradr from Fig. 1b. I assume you don't want the model data from Fig. 2b. Let me know if you do.

FYI, the disagreement between Spencer and Lindzen and me is not in the numbers. I have reproduced much of LC11 and almost all of SB11 and I know that L&C have reproduced my numbers (see Figure 9 of LC11). So we actually agree on the numbers. The disagreement is in what the numbers mean. I think their interpretation is absurd, and I have a paper coming out in GRL this week that will describe why. We'll see if anyone is convinced.

As far as the treatment Spencer has gotten, I'm not sure if you're just giving advice or asking for my opinion. If the latter, then I'd be happy to talk with you about this. Feel free to call me at your convenience at 979-862-1427. Because of schedule issues, I may not be able to return any calls until later in the week.

Regards,
Andy

On Mon, Sep 5, 2011 at 1:49 PM, Steve McIntyre < wrote:
> Dear Dr Dessler,
>
> I think that you should find the recent Remote Sensing controversy quite
> troubling. I understand that you disagree with Spencer's analyses, but the
> responses by Trenberth and others are totally disproportionate and are the
> sort of conduct that should trouble people that are worried about doubled
> CO2. It is as though no lessons were learned from Climategate and, instead
> of climate scientists avoiding the worst excesses of Climategate conduct,
> have been emboldened to behave even more poorly.
>
>
>
> As a result of the controversy, I've got some data from Spencer and am
> trying to reconcile it to Dessler 2010. Can you send me the summary data
> used to plot Figures 1 and 2?
>
>
>
> Thanks, Steve McIntyre
>
>
>
>

--
Andrew Dessler

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Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

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0.022226799	0.0163638
0.086382799	0.0161888
0.18713281	0.082219802

From: on behalf of Andrew Dessler
To: Mark Zelinka
Subject: Re: paper on feedbacks
Date: Monday, September 05, 2011 2:35:31 PM

---

thanks. I'm swamped for a few days, but hopefully I can get to this
next weekend. my spencer/lindzen paper is being released online
tomorrow and I'm prepping for the press onslaught (hopefully, anyway).

On Fri, Sep 2, 2011 at 2:04 PM, Mark Zelinka <mdz113@uw.edu> wrote:
> Hi Andy,
>
> A netcdf file containing the cloud masking of the temperature feedback, LW
> WV feedback, GHG forcing, and the sum of all three can be accessed here:
> http://www.atmos.washington.edu/~mzelinka/research/adjustments.nc
>
> I also attach two figures. One is the timeseries of our global mean LW
> and SW adjustments. The small offset visible in the SW plot is due to our
> use of two slightly different time stamps. The second one is the
> regression of our adjustments on the global mean surface temperature
> anomaly from HadCRUT3v. They look pretty similar but have some
> differences, especially over the sea ice edge and over the continents.
>
> Let me know what you think.
> Mark
>
> On 8/23/11 8:38 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>
>>Thanks for the Colman paper.
>>
>>I'd be interested in tracking down the difference between our LW
>>adjustment term. Could you send me a netCDF file that contains the
>>individual terms of the adjustment (water vapor, temperature, etc.)?
>>I'll compare to my individual terms, and hopefully we can track this
>>down.
>>
>>On Fri, Aug 19, 2011 at 4:59 PM, Mark Zelinka <mdz113@uw.edu> wrote:
>>> Got it. I didn't think it would make such a difference but I can see
>>>that
>>> it does. When I tried to recreate your Figure 3 using EOF-derived
>>>slopes,
>>> it looks like nonsense, with gigantic slopes at most latitudes.
>>>Whenever
>>> there is a lot of scatter, least squares will give you a small slope
>>>with
>>> tons of uncertainty whereas EOF might give you a huge slope as it aligns
>>> the best-bit to minimize perpendicular distances.
>>>
>>> It is somewhat disturbing however that the slope you get from a
>>> least-squares fit does not fall within the errorbars of the EOF-derived
>>> slope: T feedback ranges from -3.6 to -4.5 in Table 1, but least squares
>>> gives you -3.14.
>>>
>>> I don't think you should necessarily get the same T and WV feedbacks in
>>> observations as in the A1B scenario. Colman and Power (Attached) show
>>> different feedback magnitudes in the BMRC model depending on whether it
>>>is
>>> interannual variability or transient warming.

>>>
>>> Maybe report both estimates of the slopes and let the reader decide?
>>>
>>> Mark
>>>
>>>
>>>
>>> On 8/19/11 1:45 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>>>
>>>>I know exactly what is going on and it is discussed a bit in the paper
>>>>starting on line 68. I calculate global average feedbacks for water
>>>>vapor and temperature as the 1st EOF of the data (per Hartmann, this
>>>>is equivalent to minimizing the square of the orthogonal distance
>>>>between the data and the line). I do this because, in a normal linear
>>>>least squares fit, noise in the x-axis data (temperature) ³dilutes²
>>>>the slope and gives you a smaller value than I think you should get.
>>>>
>>>>However, for the plots of the spatial pattern and the zonal average, I
>>>>did just do a linear least squares fit between the local flux and the
>>>>global average T<I didn't think anyone would notice (damn you!). Thus,
>>>>if you do a global average of the local feedback in Fig. 2 or 3, then
>>>>you get the slope that you would have gotten had I done a normal
>>>>least-squares fit to the global data (FYI, the slope I get for the
>>>>data for water and temperature using a least squares fit is +1.18
>>>>W/m2/K and -3.14 W/m2/K).
>>>>
>>>>Doing the fits for water and temperature the way I do gets you the
>>>>same answer as the method Soden has used (differencing a decade at the
>>>>beginning and end of the 21st century). Also, it gives me a better
>>>>fit, by eye.
>>>>
>>>>I hope that is clear. If not, let me know.
>>>>
>>>>Let me know if you have any suggestions on the best way to handle
>>>>this. BTW, I'm attaching a plot of dR(temperature) vs. dTs showing
>>>>both the ordinary least squares (red) and the 1st EOF (black) fits.
>>>>
>>>>On Fri, Aug 19, 2011 at 3:09 PM, Mark Zelinka <mdz113@uw.edu> wrote:
>>>>> Hi Andy,
>>>>>
>>>>> Thanks for sending this along. I look forward to reading it.
>>>>>
>>>>> I recently computed my own LW and SW adjustments using ERA Interim
>>>>>data.
>>>>> I would have just used yours to convert CRF anomalies into feedback,
>>>>>but
>>>>> decided to re-calculate cloud forcing using kernel-derived clear-sky
>>>>> fluxes rather than using the CERES clear sky fluxes (which have gaps
>>>>>and
>>>>> are subject to the well known clear-sky sampling biases). Our SW
>>>>> adjustments are pretty much identical and our LW adjustments are
>>>>>similar,
>>>>> but not identical.
>>>>>
>>>>> I can reproduce quite closely the black solid lines in your Figure 3,
>>>>>but
>>>>> for some reason when I compare with your Table 1, my WV and
>>>>>temperature
>>>>> feedbacks are much smaller in magnitude (1.1 and -3.2 respectively).
>>>>> Globally averaging (by eye) your figure 3 (which you can do since the

>>>>>sine
>>>>> of latitude is plotted), I would not expect your observed WV feedback
>>>>>to
>>>>> be 2.1. At the same time, a WV feedback of ~1 seems way too small.
>>>>> Comments?
>>>>>
>>>>> Mark
>>>>>
>>>>>
>>>>>
>>>>> On 8/19/11 11:54 AM, "Andrew Dessler" <adessler@tamu.edu> wrote:
>>>>>
>>>>>>Hi Mark. Attached is a paper I just finished on feedbacks, esp.
>>>>>>clouds. it's based on my GRC poster. I'd be very interested in your
>>>>>>comments. as a bonus, I only have one Zelinka reference in there now
>>>>>>... but I'm happy to add more if you tell me where I should put them!
>>>>>>
>>>>>>
>>>>>>--
>>>>>>Andrew Dessler
>>>>>>Professor of Atmospheric Sciences
>>>>>>Texas A&M University
>>>>>>adessler@tamu.edu
>>>>>>979-862-1427
>>>>>>http://atmo.tamu.edu/profile/ADessler
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Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu

979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: Steve McIntyre
To: Andrew Dessler
Subject: Dessler et al 2010
Date: Monday, September 05, 2011 1:50:10 PM

Dear Dr Dessler,
I think that you should find the recent Remote Sensing controversy quite troubling. I understand that you disagree with Spencer's analyses, but the responses by Trenberth and others are totally disproportionate and are the sort of conduct that should trouble people that are worried about doubled CO2. It is as though no lessons were learned from Climategate and, instead of climate scientists avoiding the worst excesses of Climategate conduct, have been emboldened to behave even more poorly.

As a result of the controversy, I've got some data from Spencer and am trying to reconcile it to Dessler 2010. Can you send me the summary data used to plot Figures 1 and 2?

Thanks, Steve McIntyre

From: sarefo@googlemail.com on behalf of Peter Hartmann
To: Andrew Dessler
Subject: Re: Andrew Dessler"s article is being freeped
Date: Sunday, September 04, 2011 8:17:24 AM

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i will, but it's unlikely that anything else will come across my radar.

don't know if you know of the Planet Climate project; i've been mostly collecting links by climate contrarian since late 2009, and their rebuttals.

this page on the spencer/braswell paper could be interesting to you:

http://planet-climate.org/wiki/index.php?title=On_the_Misdiagnosis_of_Surface_Temperature_Feedbacks_from_Variations_in_Earth%27s_Radiant_Energy_Balance

username
password:

all the best!

p.

On Sun, Sep 4, 2011 at 5:32 AM, Andrew Dessler <adessler@tamu.edu> wrote:

> Thanks for sending this. Pls forward anything else you think I may be interested in. Regards,
>
> On Sat, Sep 3, 2011 at 4:34 AM, Peter Hartmann < wrote:
>
> > Dear Mr. Borden,
> >
> > i want to inform you that there has been a call by a group of people unconvinced of climate science to influence the debate on Andrew Dessler's article from August 30, see below.
> >
> > The "Climategate Country Club" group, owned by Mark E. Gillar, has about 700 members. The letter below has also been forwarded to other groups, so the potential base for this could be much higher. Please take this into account when evaluating the balance of incoming letters and comments.
> >
> > http://www.climategatecountryclub.com/
> >
> > here's a list of their members, for easy reference.
> >
> > http://pastebin.com/eYmDVWFU
> >
> > best regards
> >
> > Peter Hartmann
> >
> > ---------- Forwarded message ----------
> > From: **Climategate Country Club** <mail@climategatecountryclub.ning.com>
> > Date: Sat, Sep 3, 2011 at 3:11 AM
> > Subject: Global Warming Skeptic Fire Power Needed - Please Help.
> >
> > **Climategate Country Club**
> >
> > **A message to all members of Climategate Country Club**
> >
> > Dear Climategate Country Club Members,
> >
> > As many of you might know, Texas (my home state) is in the middle of a drought. Naturally
> >
> > the warmist are using this as an excuse to tout anthropogenic global warming.
> >
> > Dr. Dessler of Texas A&M University recently wrote this guest column for our local paper.

By ANDREW DESSLER

Special to The Eagle

Texas Gov. Rick Perry stirred up controversy on the campaign trail recently when he dismissed the problem of climate change and accused scientists of basically making up the problem.

As a born-and-bred Texan, it's especially disturbing to hear this now, when our state is getting absolutely hammered by heat and drought. I've got to wonder how any resident of Texas -- and particularly the governor who not so long ago was asking us to pray for rain -- can be so cavalier about climate change.

As a climate scientist at Texas A&M University, I can also tell you from the data that the current heat wave and drought in Texas is so bad that calling it "extreme weather" does not do it justice. July was the single hottest month in the observational record, and the 12 months that ended in July were drier than any corresponding period in the record.

I know that climate change does not cause any specific weather event. But I also know that humans have warmed the climate over the past century, and that this warming has almost certainly made the heat wave and drought more extreme than it would otherwise have been.

I am not alone in these views. There are dozens of atmospheric scientists at Texas institutions such as Rice, the University of Texas and Texas A&M, and none of them dispute the mainstream scientific view of climate change.

This is not surprising, since there are only a handful of atmospheric scientists in the entire world who dispute the essential facts -- and their ranks are not increasing, as Gov. Perry claimed.

And I can assure Gov. Perry that scientists are not just another special interest looking to line their own pockets. I left a job as an investment banker on Wall Street in 1988 to go to graduate school in chemistry. I certainly didn't make that choice to get rich, and I didn't do it to exert influence in the international arena either.

I went into science because I wanted to devote my life to the search for scientific knowledge and to make the world a better place. That's the same noble goal that motivates most scientists. The ultimate dream is to make a discovery so profound and revolutionary that it catapults one into the pantheon of the greatest scientific minds of history: Newton, Einstein, Maxwell, Planck, etc.

This is just one of the many reasons it is inconceivable for an entire scientific community to conspire en masse to mislead the public.

In fact, if climate scientists truly wanted to maximize funding, we would be claiming that we had no idea why the climate is changing -- a position that would certainly attract bipartisan support for increased research.

The economic costs of the Texas heat wave and drought are enormous. The cost to Texas alone will be many billion dollars (hundreds of dollars for every resident), and these costs will ripple through the economy so that everyone eventually will pay for it. Gov. Perry needs to face squarely the choice confronting us: either we pay to reduce emissions of greenhouse gases, or we pay for the impacts of a changing climate. There is no free lunch.

Economists have looked at this problem repeatedly over the past two decades, and virtually every mainstream economist has concluded that the costs of reducing emissions are less than the costs of unchecked climate change. The only disagreement is on the optimal level of emissions reductions.

I suppose it should not be surprising when politicians such as Gov. Perry choose to shoot the messenger rather than face this hard choice. He may view this as a legitimate policy on climate change, but it's not one that the facts support.

* *Andrew Dessler is a professor of atmospheric sciences at Texas A&M University. Distributed by McClatchy-Tribune News Service.*

http://www.theeagle.com/columnists/Paying-the-price-for-climate-change

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Opinions Editor

979-731-4621

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Sincerely,

Mark Gillar

CCC Founder

Visit Climategate Country Club at: http://climategatecountryclub.ning.com/?xg_source=msg_mes_network

--
Andrew Dessler
Professor of Atmospheric Sciences
Texas A&M University
adessler@tamu.edu
979-862-1427
http://atmo.tamu.edu/profile/ADessler

From: on behalf of Andrew Dessler
To: Peter Hartmann
Subject: Re: Andrew Dessler"s article is being freeped
Date: Saturday, September 03, 2011 10:32:44 PM

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Regards,

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--
Andrew Dessler
Professor of Atmospheric Sciences

Texas A&M University
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From:
To: ADessler@tamu.edu
Cc:

Dan_Barron@epw.senate.gov
Subject: Correctly Labeling the Branch Carbonian Cult
Date: Saturday, September 03, 2011 11:27:50 AM

Dear Dr. Andrew Dessler: Although I have no idea of the source of Governor Rick Perry's serious doubts about the efficacy of anthropogenic global warming (AGW), I am suspicious of those who slip-slide without explanation or justification into the misleading language of so-called "climate change" -- and then proceed to berate and even to question the sanity of those who are (Forsooth!) "against climate change."

Because your writing about this subject reflects ever more closely the pseudo-religious -- belief-based rather than science-based -- postulations of the AGW Movement, you might like to consider observations I have made in an August 23, 2009 AmericanThinker.com essay, entitled "**The Branch Carbonian Cult."**

http://truespeak.org/contents/view/branch_carbonian_cult

and also .... http://truespeak.org/contents/view/obamasgreatleahpbackward

As you study this article's bill of particulars -- 30 items long -- you will be able to determine whether you are already a *de facto* captive of the BC Cult and a proactive proselytizer of its belief system -- or are still trying to determine whether these "**whacko warmists**," as we alleged "**dimwit deniers**" call them, are as truly devoted to objective science as they so fervently and so combatively claim to be.

Although you owe me no response whatever, I look forward to knowing of any questions or constructive comments you might have about these vital matters of shared concern -- and do remain, Sincerely yours,

JIM GUIRARD -- TrueSpeak.org 703-768-0957

A DC-area attorney, writer and national security strategist, Jim Guirard was longtime Chief of Staff to former US Senators Allen Ellender and Russell Long of Louisiana. His TrueSpeak.org Web site is devoted to truth in language and truth in history in public discourse.

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The Branch Carbonian Cult

Print **Email**

Author: Jim Guirard

Source: AmericanThinker.com -- August 23, 2009

The Anthropogenic Global Warming (AGW) Movement has taken on the worrisome attributes of a *pseudo-religious cult,* which operates far more on the basis of an apocalyptic "belief" system than on objective climate science.

Since this worldwide Movement and its strident policies of *Less Energy at Higher Prices* (in order to achieve reductions in everyone's "carbon footprint") are at the heart of America's enormous energy shortfall, it poses a national security threat of major proportions.

And in this context, the AGW Crusade should be understood in a "Know Thy Enemy" frame of reference -- perhaps not in terms of a fully *conscious* or *intentional* enemy of the American people at a time of war and economic crisis *but as a deadly threat to our economic stability and national security, nonetheless*.

Kingdom of the Cults

Here, therefore, in far more detail than any routine allegation of "cultism" conveys, are no fewer than ten of this AGW ideology's very specific characteristics, many of whose roots and lock-step influences can be found in Walter Martin's and Ravi Zacharias' definitive, award-winning 2003 book, "Kingdom of the Cults:"

1. Leadership by a self-glorifying, manipulative New Age Prophet -- in this case, former Vice-President Al Gore, though he is rapidly being supplanted by President Barack Obama.

2. Assertion of an apocalyptic threat to all mankind.

3. An absolutist definition of both the threat and the proposed solution(s).

4. Promise of a salvation from this pending apocalypse.

5. Devotion to an inspired text which (*arguendo*) embodies all the answers -- in this case, Prophet Gore's pseudo-scientific book "Earth in the Balance" and his more recent "An Inconvenient Truth" documentary.

6. A specific list of "truths" (see the Ten Commandments listed below) which must be embraced and proselytized by all Cult members..

7. An absolute intolerance of any deviation from any of these truths by any Cult member.

8. A strident intolerance of any outside criticism of the Cult's definition of the problem or of its proposed solutions.

9. A "Heaven-on-Earth" vision of the results of the mission's success and/or a "Hell-on-Earth" result if the cultic mission should fail.

10. An inordinate fear (and an outright rejection of the possibility) of being proven wrong in either the apocalyptic vision or the proposed salvation.

Prophet Gore's (and now Prophet Obama's) Ten Commandments

With this half of the AGW Cult's self-definition now clearly established, here is the other half -- its Ten Commandments of "Thou-Shalt" and "Thou-Shalt-Not" absolutes -- designed for keeping its devoted cultists in lockstep support and its intimidated detractors in retreat:

o Thou shalt have but one Mother Earth (Gaia) Goddess before you

o Thou shalt not worship false Prophets -- especially sun cycles, ocean cycles, volcanic influences and "Objective Science" in general

o Thou shalt never doubt catastrophic depletion of the so-called "Ozone Layer"

o Thou shalt not doubt man-made "Greenhouse Gasses" as the primary cause of GW

o Thou shalt condemn such doubters as "Extremists" and "Criminals Against Humanity"

o Thou shalt minimize, ignore and deny any and all environmental good news

o Thou shalt avoid benefit-cost evaluations of AGW solutions and never admit error or falsehood about anything

o Thou shalt continue opposing all Nuclear and new Hydro power, despite their non-AGW attributes

o Thou shalt promote "zero-carbon-footprint" policies of Less Energy at Higher Prices, except for heavily subsidized ethanol

o Thou shalt engage forever in "Eeekology" and "Eeekonomics" (scare-tactics ecology and economics) and never, ever vote Republican

Finally, since this AGW juggernaut seems to have brainwashed a majority of Americans, most of the media and academia, a majority of the Congress and even many churches into a mind-set of support for its pseudo-religious scam, a recent Wall Street Journal's recent conclusion that this represents a "Mass Neurosis" of a cultic nature seems alarmingly accurate.

Truths to be Ignored or Denied

On the more climatically correct side, all that is needed to begin the collapse of this house-of-cards scam is yet another list of certifiable facts and truths -- one which will disprove much of the Cult's mission, tactics and alleged "solutions" -- namely,

(a) the fact that while Arctic ice may (or may not, of late) be receding, Antarctic ice has been *increasing* for about 40 years

(b) the fact that global temperatures have been on a slightly *decreasing* trend since 1998,

(c) the fact that Mars (which features no man-made factor at all) is experiencing "global warming," as well,

(d) the fact that Antarctic "ice shelves" which occasionally break off, float away and melt at sea, do not raise ocean levels *at all*,

(e) the fact that several of the "hottest years" on record were in the 1930s and 1940s, when CO2 levels were much lower than today's,

(f) the fact that ever more scientists assert convincingly that atmospheric CO2 is a lagging *consequence*, rather than a triggering *cause*, of alleged global warming,

(g) the fact that all earlier glacial and inter-glacial periods were clearly caused not by man but by solar, ocean and volcanic cycles and "natural" fluctuations,

(h) the fact that di-hydrogen oxide (H2O) molecules -- water vapor -- and methane molecules are *20-30 times* more heat-retentive than CO2 molecules are,

(i) the fact that termites worldwide expel about as much "greenhouse gasses" into the atmosphere as does all the burning of fossil fuels by human beings,

(j) the fact that even if all Kyoto-type limits on CO2 were obeyed by all nations, the estimated net impact by 2050 would be less than half a degree F -- with a ruinous cost-to-benefit ratio of *hundreds to one,* when the standard requirement is no more than one to one.

<u>Conclusion</u>: Since every such Prophet-led, scare-mongering, pseudo-religious conspiracy needs a properly descriptive name, and since this one's primary concerns over alleged depletion of the so-called "ozone layer" over Antarctica have shifted to a panic over CO2, instead, a fitting name for this cultic gaggle might be the "<u>Branch Carbonian Cult</u>" --

o "Branch" because it is a radical offshoot from the main body of science-based

environmentalism;

o "Carbonian" because of its professed fear of carbon dioxide as a primary cause of AGW; and

o "Cult" because of its self-evident structure and practices -- which are in full accord with most elements of the typical religious cult, Branch Davidian or Jim Jonesian or otherwise.

JIM GUIRARD -- TrueSpeak.org 703-768-0957

A DC-area attorney, writer and national security strategist, Jim Guirard was longtime Chief of Staff to former US Senators Allen Ellender and Russell Long of Louisiana. His TrueSpeak.org Web site is devoted to truth in language and truth in history in public discourse.

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PS: See also ... http://truespeak.org/contents/view/obamasgreatleahpbackward

... http://joannenova.com.au/page/2/

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From: on behalf of Peter Hartmann
To: letterseditor@theeagle.com;
Subject: Andrew Dessler"s article is being freeped
Date: Saturday, September 03, 2011 4:34:35 AM

Dear Mr. Borden,

i want to inform you that there has been a call by a group of people unconvinced of climate science to influence the debate on Andrew Dessler's article from August 30, see below.

The "Climategate Country Club" group, owned by Mark E. Gillar, has about 700 members. The letter below has also been forwarded to other groups, so the potential base for this could be much higher. Please take this into account when evaluating the balance of incoming letters and comments.

http://www.climategatecountryclub.com/

here's a list of their members, for easy reference.

http://pastebin.com/eYmDVWFU

best regards

Peter Hartmann

---------- Forwarded message ----------
From: **Climategate Country Club** <mail@climategatecountryclub.ning.com>
Date: Sat, Sep 3, 2011 at 3:11 AM
Subject: Global Warming Skeptic Fire Power Needed - Please Help.

Climategate Country Club

A message to all members of Climategate Country Club

Dear Climategate Country Club Members,

As many of you might know, Texas (my home state) is in the middle of a drought. Naturally

the warmist are using this as an excuse to tout anthropogenic global warming.

Dr. Dessler of Texas A&M University recently wrote this guest column for our local

paper.

By ANDREW DESSLER

Special to The Eagle

Texas Gov. Rick Perry stirred up controversy on the campaign trail recently when he dismissed the problem of climate change and accused scientists of basically making up the problem.

As a born-and-bred Texan, it's especially disturbing to hear this now, when our state is getting absolutely hammered by heat and drought. I've got to wonder how any resident of Texas -- and particularly the governor who not so long ago was asking us to pray for rain -- can be so cavalier about climate change.

As a climate scientist at Texas A&M University, I can also tell you from the data that the current heat wave and drought in Texas is so bad that calling it "extreme weather" does not do it justice. July was the single hottest month in the observational record, and the 12 months that ended in July were drier than any corresponding period in the record.

I know that climate change does not cause any specific weather event. But I also know that humans have warmed the climate over the past century, and that this warming has almost certainly made the heat wave and drought more extreme than it would otherwise have been.

I am not alone in these views. There are dozens of atmospheric scientists at Texas institutions such as Rice, the University of Texas and Texas A&M, and none of them dispute the mainstream scientific view of climate change.

This is not surprising, since there are only a handful of atmospheric scientists in the entire world who dispute the essential facts -- and their ranks are not increasing, as Gov. Perry claimed.

And I can assure Gov. Perry that scientists are not just another special interest looking to line their own pockets. I left a job as an investment banker on Wall Street in 1988 to go to graduate school in chemistry. I certainly didn't make that choice to get rich, and I didn't do it to exert influence in the international arena either.

I went into science because I wanted to devote my life to the search for scientific knowledge and to make the world a better place. That's the same noble goal that motivates most scientists. The ultimate dream is to make a discovery so profound and revolutionary that it catapults one into the pantheon of the greatest scientific minds of history: Newton, Einstein, Maxwell, Planck, etc.

This is just one of the many reasons it is inconceivable for an entire scientific community to conspire en masse to mislead the public.

In fact, if climate scientists truly wanted to maximize funding, we would be claiming that we had no idea why the climate is changing -- a position that would certainly attract bipartisan support for increased research.

The economic costs of the Texas heat wave and drought are enormous. The cost to Texas alone will be many billion dollars (hundreds of dollars for every resident), and these costs will ripple through the economy so that everyone eventually will pay for it. Gov. Perry needs to face squarely the choice confronting us: either we pay to reduce emissions of greenhouse gases, or we pay for the impacts of a changing climate. There is no free lunch.

Economists have looked at this problem repeatedly over the past two decades, and virtually every mainstream economist has concluded that the costs of reducing emissions are less than the costs of unchecked climate change. The only disagreement is on the optimal level of emissions reductions.

I suppose it should not be surprising when politicians such as Gov. Perry choose to shoot the messenger rather than face this hard choice. He may view this as a legitimate policy on climate change, but it's not one that the facts support.

** Andrew Dessler is a professor of atmospheric sciences at Texas A&M University. Distributed by McClatchy-Tribune News Service.*

http://www.theeagle.com/columnists/Paying-the-price-for-climate-change

Please join me in responding to this nonsense. Responding to the posting board will only place your response

on the website. Please also submit your response to letterseditor@theeagle.com so that it will appear

in the local paper. The rules for submitting posted below.

Should you feel that you need more words to respond, you may submit a guest column to:

Robert Borden
Opinions Editor

979-731-4621

The rules for submitting a a letter to the editor are:

1. No more than one letter per writer will be printed each 30 days.

2. Letters should be no more than 300 words and are subject to editing for lenth, clarity, libel and good taste.

publication is descretionary.

3. All letters must contain the writer's name, city of residence and a daytime phone number for verification.

4. Anonymous lettters will not be published.

5. Form letters and letters associated with organized efforts involving candidates or causes will not be published.

Thank you so much for your help. I look forward to seeing your letters and guest columns appear in our local paper.

Sincerely,

Mark Gillar

CCC Founder

Visit Climategate Country Club at: http://climategatecountryclub.ning.com/?xg_source=msg_mes_network

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From: Eric Pooley
To: Andrew Dessler
Cc: Sharyn Stein; Ramon Alvarez
Subject: Re: Dessler Op Ed media hits as of Aug. 31
Date: Thursday, September 01, 2011 6:04:54 AM

Nice to hear. I noticed that on Statesman.com yesterday, all 14 commenters agreed with you, thanked you, etc -- none of the usual flames from skeptics or deniers. I don't think I've ever seen that before.

On Aug 31, 2011, at 10:40 PM, "Andrew Dessler" <adessler@tamu.edu> wrote:

> FYI ...
>
> Interestingly, I've gotten dozens of e-mails from people saying
> thanking me for writing this. that's not the usual reaction to my
> op-eds. something in it really hits a nerve with people.
>
>
> ---------- Forwarded message ----------
> From: Karen Riedel <kriedel@tamu.edu>
> Date: Wed, Aug 31, 2011 at 3:31 PM
> Subject: Fwd: Dessler Op Ed media hits as of Aug. 31
> To: Andrew Dessler <adessler@tamu.edu>
>
>
> Not to add fuel to the fire . . .
>
>
> Begin forwarded message:
>
> From: "Randall, Keith M" <keith-randall@tamu.edu>
> Date: August 31, 2011 3:29:06 PM CDT
> To: 'Karen Riedel' <kriedel@tamu.edu>
> Subject: FW: Dessler Op Ed media hits as of Aug. 31
>
> Updated list of media hits on the Dessler column….kr
>
> Keith Randall
> Associate Director
> The Division of Marketing & Communications
> Texas A&M University
> keith-randall@tamu.edu
>
> 1372 TAMU | College Station, TX 77843-1372
> Tel. 979.845.4644 | Cell.979.820.3160 | Fax 979.845.9909
>
> Follow us on Twitter at http://www.twitter.com/tamutalk

>
> It's Time For Texas A&M
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>
>
>
>
> --
> Andrew Dessler
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> 979-862-1427
> http://atmo.tamu.edu/profile/ADessler
> <Dessler-OpEd-Aug2011.docx>