SOUTH DELTA WATER AGENCY

4255 PACIFIC AVENUE, SUITE 2
STOCKTON, CALIFORNIA 95207
TELEPHONE (209) 956-0150
FAX (209) 956-0154
E-MAIL Jherrlaw@aol.com

Directors:
Jerry Robinson, Chairman
Robert K. Ferguson, Vice-Chairman
Natalino Bacchetti
Jack Alvarez
Mary Hildebrand

Counsel & Manager:
John Herrick

August 22, 2011

Via E-Mail cdibble@dfg.ca.gov

Mr. Chad Dibble
Department of Fish and Game
830 S St.
Sacramento, CA 95811

Re: Comments to Department of Fish and Game's Draft Conservation Strategy for Restoration

Dear Sir or Madam:

The South Delta Water Agency submits the following comments on the above referenced draft document. The location of the lands within our agency gives our agency a unique perspective on the many ongoing Bay-Delta efforts and processes. We hope these comments trigger serious reflection by DFG as it moves forward. Significant decisions on California's future, as well as the health and existence of many plant and animal species are pending. We encourage DFG to make its decisions based on an unbiased examination of the current situation.

Many other interests have a better understanding of fishery issues, and thier comments should also be carefully noted. The Draft includes a large amount of information, which cannot be realistically addressed in comments such as these. You will please excuse us if these comments do not follow the Draft's contents, as many issues overlap.

1. The Draft report contains a number of foundational problems. One is its failure to analyze the current problems from the perspective of "who caused what?" The Draft goes on at lengths to describe various problems facing a healthy ecosystem, while trying its hardest to avoid designating the causes of the adverse impacts to fisheries. One need only read the summaries relating to CVP/SWP export projects to see that DFG is being constrained from giving honest, straightforward opinions and conclusions. Although large ecosystems are complex and difficult to understand, that does not prevent us from identifying the main causes of fishery declines in the Bay-Delta system.

The Draft should note that as early as 1978, the State Water Resources Control Board found in its D-1485 that in order to mitigate the effects of the export projects on fisheries would require a virtual shutting down exports (and an additional 2 MAF of dedicated fishery water). Notwithstanding the volumes of additional data developed since then, there has been no indication that this SWRCB finding is anything but true. Hence, if mitigation of exports' effects on fish required a cessation of exports nearly 33 years ago, the fact that exports not only continued but increased should be a foundational part of any effort to protect the ecosystem. Thirty three additional years of exports must have caused an unmeasurable damage to the system. Describing detailed plans for new habitat, or proposing to raise subsided Delta lands would seem meaningless given the impacts of exports on fisheries. It is difficult to imagine how a host of peripheral actions would over come 50 years of damage by exports.

2. Similarly, the Draft makes no mention of the water availability issue, while repeating the current mantra of "co-equal goals." No yield analysis exists (to my knowledge) which shows anything other than the system (Sacramento-San Joaquin Rivers) produces much less than demand seeks. The attached chart shows that in drought times (1928-1934) the system produces approximately 8 MAF less than local demands (area of origin needs not including fishery needs); meaning there is no water available for exports. Depending on the various combinations of year types, the amount of water available for exports goes from zero to perhaps a few million acre feet. Thus, any examination (or acceptance) of water supply reliability (one of the co-equal goals) means that the "reliable supply" for exports is much less than the 6-7 MAF sought through the BDCP process. With this shortage of export supply as a starting point, DFG goals for restoring the ecosystem would have a completely different analysis as to what should be done. That is to say, if one assumes that the main cause of the destruction of the estuary (export projects) will be radically curtailed, the actions needed to restore the ecosystem will be different than if one assumes the continuation of that destruction.

3. The Draft attempts to state that DFG does not seek to return the Delta back to its condition of 100 years ago, but then goes on at length how it plans on re-establishing conditions similar to what it was 100 years ago. I believe this highlights the incorrect approach referenced above, in that the Draft makes no mention what the conditions in the Delta were when the fisheries were last healthy. First with regard to this, is the Draft's failure to mention what habitat acreage has been lost over the past 20-40 years. I believe there has been no significant loss of aquatic habitat in the Delta during this time frame, and if not, then proposals to create 10,000's of acres of new habitat would seem misguided and focused on the incorrect problem.

Second, if the species of concern had healthy populations 40 years ago, then examining the changes during those past 40 years should help focus on the causes which need to be addressed. No such analysis is included in the Draft. It is clear to all but the most obsessive proponents of exports that as exports increased, fisheries declined. This reaffirms the above comments. Any strategy to restore the Delta ecosystem which pretends exports are just one of many impacts to fisheries is doomed to failure. It should be noted that the numerous prior efforts that adopted this faulty analysis did indeed all fail; Delta Accord, CALFED, D-1641, CVPIA, Delta Vision, and now BDCP. Trying to export 6-7 MAF of water from a system that does not produce that amount of surplus water destroys the ecosystem.

4. Another foundational problem with the Draft is its reliance on the "climate change" trump card. A few years back a number of interests began touting the theory of "needing new flood overflow habitat" in the southern Delta. After the facts were finally made available, we showed that for the most part, the flows of the San Joaquin River, in relation to the elevation of the southern Delta lands and the slope of those lands, that there were simply very few opportunities to create the "Yolo Bypass-type" habitat being dreamed of. Now, rather than explain why fisheries were healthy when none such habitat existed for the last 80 years, the proponents have countered that "sea level rise will create the habitat in the future." It would seem risky at best to assume sea level rise/climate change will create the necessary habitat at the time or at the rate the DFG seems to think its necessary. Modeling the future has not been shown successful, and hoping the model results will help species nearing extinction would seem fanciful.

More importantly, DFG should explain the basis for its faith in these models. First, the sea level gauge information from NOAA indicates a certain sea level rise at the San Francisco which is greater than the measured sea level rise in the Bay, while the sea level measured in Alaska shows an decrease (see attached). Second, DFG should explain if the model results it cites have imbedded in them assumptions about atmospheric heat loss. The most recent data from NASA indicates the weather models all under predict such heat loss, and thus overstate predicted global temperature increases. If the models do indeed contain tese faulty asummptions, then they should be corrected and re-run. Of course as soon as one raise any issue regarding climate change predictions there is a cacophony indignation and often ridicule. However, if DFG's plan to save endangered, threatened and other species is counting on the creation of large amounts of new habitat resulting from sea level rise, it surely must be concerned about the reliability of the extent and timing of such.

5. History should teach us so we do not repeat our errors. For many years, DFG failed to require DWR to comply with CESA law. It was only in the last few years that a lawsuit uncovered this violation of law; the resolution of which is still pending. Rather than require DWR to comply with CESA, DFG cooperated in the numerous processes and initiatives to protect the ecosystem, all the while allowing the take on hundreds of thousands of endangered and threatened species.

We know that current operations of the projects do not even provide dry year protection for fish. In the 2009 SWRCB hearings on the DWR/USBR Urgency Petition to Amend Water

Rights Permits, the project sought to be released from responsibility for meeting X2. [Further X2 studies and investigations are part of the Draft rather than recommendations for enforcement.] In the testimony in support of the Petition, we learned that the projects wanted to preserve cold water behind the dams to meet later instream temperature obligations. If they tried to meet X2 at the time, they speculated they might not have enough cold water for the "more important" temperature flows later. As it turned out, the projects "weren't sure" they would have enough cold water anyway. One would assume this means the projects were simply piece-mealing their efforts to avoid permit conditions. In any event, the projects diverted 4000 cfs during the time X2 was to be 11,400 cfs leaving only 7,000 cfs of X2. Thus 2009 (at the time assumed to be the third year of drought) the projects had been operated to not have enough water for minimal fishery requirements and about 1/3 of the fish water was taken for exports. Obviously, the DFG Draft should be focusing on these sort of problems if it wants to restore the estuary.

6. As the Draft mentions, CALFED was to first examine the effectiveness of through Delta conveyance before any decision was made on isolated conveyance. However, the Draft cites CALFED's conclusion that the "through-Delta conveyance alternative has not achieved sufficient progress" in protecting the fisheries is, unfounded. There was a complete lack of effort to maximize through Delta actions to protect fish during CALFED. DFG can't therefore conclude that some sort of isolated facility is desirable. To the contrary, if the problems facing the ecosystem are a function of excessive exports, an isolated facility may be irrelevant.

DFG should avoid the numerous citations to CALFED given that efforts near complete failure. That process coincided with the near destruction of a number of fish species. Its only accomplishes appear to be some small upstream projects and a massive amount of study results.

7. The Draft's discussion of BDCP appears unwarranted. The Draft identifies sweeping goals and future actions which, in combination with "adaptive management" it hopes will restore the ecosystem. BDCP seeks fifty year permitting (and take) approval to allow exports of 6-7 MAF per year. It is hard to imagine how a set of goals and actions can be the basis for regulatory certainty when we do not know the how effective any of them will be or when they will be in place. We do know the extent of the exports impact on fisheries. "Adaptive management" through the CALFED years proved to be wholly inadequate to overcome the desire to export 6-7 MAF of water.

8. Another threshold question has not been recognized. There is currently a tension between protecting smelt and protecting salmon, with the former thought to be harmed with a spring head of Old River barrier ("HORB") and the latter needing it for protection. It is likely that some form of HORB will continued to be used with other measures taken to protect smelt from the effects of the export pumps. However resolving this issue requires a decision about "where" we want the fish to rear and the path through which we want them to migrate. Again, its likely fishery agencies will continue to try to keep fish away from the export pumps which

means keep them out of the southern Delta. If indeed that becomes or stays the preference, it makes no sense to be seeking habitat for fish in areas we do not want them and from which we will prevent them from entering. Of course, the goal will never be to remove all fish in the area, and new habitat can serve multiple needs. However, limited resources are available and should not be wasted. If we want to stop keep salmon smolts from going through the southern Delta, and we want to keep spawning, adult and juvenile smelt out also, then there is no reason to seek new southern Delta habitat for smelt and salmon.

DFG should make sure there are no other such foundational issues which would substantially alter how they effectuate the broad plans and goals contained in the Draft.

9. The description of current and historic salinities is not complete. First, it fails to note that the substantial salt now (as opposed to historically) entering the Delta from the San Joaquin River may itself be adverse to fish migration. Although there may be no data that SJ River salinity is unhealthful for fish, the salt gradient as one approaches the river may be confusing migrating salmon and other species. Salmon try to go from salty ocean to fresh streams, but going from the Bay into the cross channel flow into the San Joaquin River may cause the returning fish to turn back. This may also affect outmigration.

Second, the historic examples of high salinity entering the Delta are misleading. It was only in extreme droughts that ocean salinities reach far into the Delta, and then only for short periods. This intrusion of salt was very gradual allowing most animal species to move upstream to the fresher water. In all instances, the salt gradient was quickly established once the first freshets occurred. Third, the historic instances of these salt water intrusions occurred during times when upstream development was increasing and are thus not an indication of “natural” conditions. Periodically allowing salinity to intrude might not be “good” for the estuary. It is not a question of whether invasive species will be discouraged by salt intrusion, it is a question of whether native populations (at very low levels) will survive this un-natural salting up. Remember, the non-natives took hold after the Delta became saltier then was the case historically. It should be noted that in order to bring ocean salts up to Stockton (as some have proposed) it would require no outflow for 3-4 months. Such a proposal cannot be given serious consideration by DFG. Once again, some argue having less fresh water in the estuary is good for the estuary. A notion entirely at odds with history.

10. The Draft proposes river meander and increased siltation as a means of increasing habitat. However, this proposal brings with it numerous adverse impacts. Flood corridors are designed to carry specified amounts of water, thus determining which lands are protected and which are at risk (if flows are greater than the channel capacity). Altering the current channel geometry and size may interfere with flood protection needs. Encouraging siltation means a steady decrease in flood carrying capacity, and thus a greater flood risk. A better approach would be to maximize the use of upstream refuge lands already dedicated to habitat. The

traditional flood areas of the San Joaquin River coincided with these refuges, and thus they should yield the greatest habitat benefits as opposed to making new flood areas farther downstream.

In addition, increasing flood flows areas through river setbacks also runs the risk of increasing seepage onto the lands protected by levees. Before adopting the Draft, DFG should consult further with engineering firms which deal with flood protection and levee work.

11. The Draft states that many Delta islands are protected by "...more than 1,100 miles of fragile levees ..." This is a significant overstatement. Although the conditions in the Delta vary somewhat, the levee system remains strong. The notion that the levees are "fragile" derives from the studies by Jeff Mount and the DWR DRMS study. This studies are not supported by the existing data and vastly overstate threats to levees because they lump current levee work and potential flooding with historic levee work and flooding. This significantly skews any analysis. For example, Mount and DRMS predict a number of islands flooding each year, none of which have happened. One need only read the US Army Corps of Engineers comments (attached) to the DRMS study to see the numerous and very real problems underlying the predictions.

12. DFG's conclusions about both the benefits derived from maintaining a "common pool" of fresh water in the Delta and the ability to do so are unfounded. It is likely that the pool can be maintained if sufficient fresh water is dedicated to fisheries (D-1485 noted that to protect the Suisun Bay it would take approximately 2 MAF of water) and current law requires it to be done (see Water Code Sections 12200 et.seq.).

13. The Draft notes that a change in conveyance may be needed to avoid reverse flows in the southern Delta. This misstates the problem. Export pumps do indeed reverse flows in some channels because the rate of export is almost always greater than San Joaquin River flows. However, decreasing exports to sustainable levels (in line with amount of surplus water available) should improve fishery conditions such that new conveyance would be unnecessary. Proposed increases in River flows would also help address the effects of export pumps on fish. DFG's incorrect analysis is based on the false assumption that there need to be high levels of exports. Maintaining high exports and moving the current diversion locations would result in less of the CVP salt in the San Joaquin River leaving the area. The result would be a "salting up" of the southern Delta where agriculture would not be sustainable (or possible) and radically different habitat for both terrestrial and aquatic species.

14. In-Delta diversions (other than the export pumps) are not believed to have any significant impact on fish. If one examines the export projects diversion of the entire flow of Old River and their "take" of every fish in that channel to the many small agricultural diversions throughout the Delta one finds only speculative impacts by the ag diversions. Thus addressing in-Delta ag intakes would appear to be a waste of effort and money.

15. DFG makes no note of the DO problems regularly experienced in Old River near Fabian Tract. This area is the largest null zone created by the export projects. In most years, the water in this channel reach never leaves the area. Its salinity and temperature increase and DO plummets. Fish kills are well documented. However, DFG makes no mention of how increased River flows and better operation of southern Delta barriers could create net flows and improve conditions for fish.

16. Although the Draft notes that methy mercury is an issue when describing increased wetlands habitat, it gives too little consideration to the problem. The Central Valley Regional Water Quality Control Board's TMDL clearly finds that the greatest producers of MeHG are seasonal wetlands; the type most sought after in the Draft. It is inconsistent with both public health and wildlife protection to emphasize substantial increases in that which increases MeHg. While other parties are struggling to figure out how they might comply with the restrictions in the TMDL, DFG is proposing creating new habitat which will produce many times the amount produced by all agriculture in the Delta. Qualifying this desire to create these new MeHG production sites by stating "further study is necessary" is to mis-characterize the problem.

17. The Draft also fails to note the consequences of significant increases in flood plain habitat and other new areas contemplated therein. There is no doubt that open water and riparian vegetation consume much more water than does irrigated agriculture. Thus, adding all the new habitat would result in millions of additional acre feet of consumption. This water would have to come from somewhere; meaning that any anticipated increased river flows would be significantly lessened and many other beneficial uses would have less. DFG needs to more fully explain its ideas of how and where the "new" water would derive. It would not seem appropriate to assume climate change will give us substantially more water than we now have.

18. The Draft suggests/predicts that as the Delta salts up, agriculture will shift to "salt-tolerant crops" or move to biofuel production. This indicates a lack of understanding. Although there are crops which can survive with higher salinities than now occur in the southern Delta, there aren't any crops which can be grown for profit if salinities are double or triple the current ones. Besides not being able to produce "biofuels in a salty environment, biofuel production is a shift of using water to produce food and fiber to producing energy. The proportional increase in water use vs. benefit means that we would be consuming even more of an already short supply. When the ecosystem is millions of acre feet short of water, DFG should not be advocating using water for fuel.

19. The Draft makes no mention of legal restraints on water use. This is important in that there exists a hierarchy of priorities controlling water use in California. Area of origin laws, public trust needs, license/permit priorities and other laws and principles mandate that we not adversely affect the Delta and upstream areas in order to provide water for development in drier

regions. Hence, a plan to significantly alter the Delta beneficial uses in order to protect exports uses is simply wrong.

20. Current Corps regulations mandate that all but native grasses be removed from project levees. If not, levee improvement, maintenance, and projects will not be authorized and emergency funding not available. If DFG seeks to expand floodways, set back levees, and river meanders it will result in either no such work be allowed under federal regulations, or many miles of new levees which will put people and property behind them at financial, regulatory, and insurance risk. Until the mutually incompatible levee rules are resolved, no substantial changes can be assumed possible.

21. Although the Draft acknowledges the problems associated with egeria and other invasive plants, it understates how these plants would affect proposals in the Draft. Unless and until a method by which ergeria can be stopped is developed, creating more habitat for this plant is illogical. The new tidal and meander habitats proposed are the very same ones in which ergeria currently chokes out most other plants and animals and adversely impacts species of concern.

22. The discussion on land subsidence does not appear to be correct. I suggest further discussion with those familiar with the levee network in the Delta (e.g. Dante Nomellini, Esq., KSN and MBK engineers). The reports noting the ability to create new peat would appear to be vastly overstated and should not be used to craft new programs.

23. The Draft notes other ongoing efforts in a number of places, including CVPIA. However, when noting the AFRP, there is no discussion about it complete inability to meeting its fish doubling goal. Before adopting a plan, DFG needs to go through a public analysis of why the federal mandate was not met and the reasons therefore. Trying to address those reasons seems to be the proper approach.

Computer problems prevent me from including the above referenced attachments. They will follow as soon as is possible. Please feel free to contact me if you have any questions.

Very truly yours,

JOHN HERRICK

Suisun Marsh. Full protection of Suisun Marsh now could be accomplished only by requiring up to 2 million acre-feet of freshwater outflow in dry and critical years in addition to that required to meet other standards. This requirement would result in a one-third reduction in combined firm exportable yield of State and federal projects. In theory, the existing Basin 5B Plan purports to provide full protection to the Marsh. However, during the 1976-77 drought when the basin plan was in effect, the Marsh received little if any protection because the system almost ran out of water and emergency regulations had to be imposed. This decision balances the limitations of available water supplies against the mitigation responsibility of the projects. This balance is based on the constitutional mandate "...that the water resources of the State be put to beneficial use to the fullest extent of which they are capable..." and that unreasonable use and unreasonable diversion be prevented (Article 10, Section 2, California Constitution).

The Bureau, the Department, Fish and Game, and U. S. Fish and Wildlife Service are working together to develop alternative water supplies for the Marsh. Such alternative supplies appear to represent a feasible and reasonable method for protection of the Marsh and mitigation of the adverse impacts of the projects. Under this decision the Department and Bureau are required, in cooperation with other agencies, to develop a plan for Suisun Marsh by July 1, 1979. The Suisun Marsh plan should ensure that the

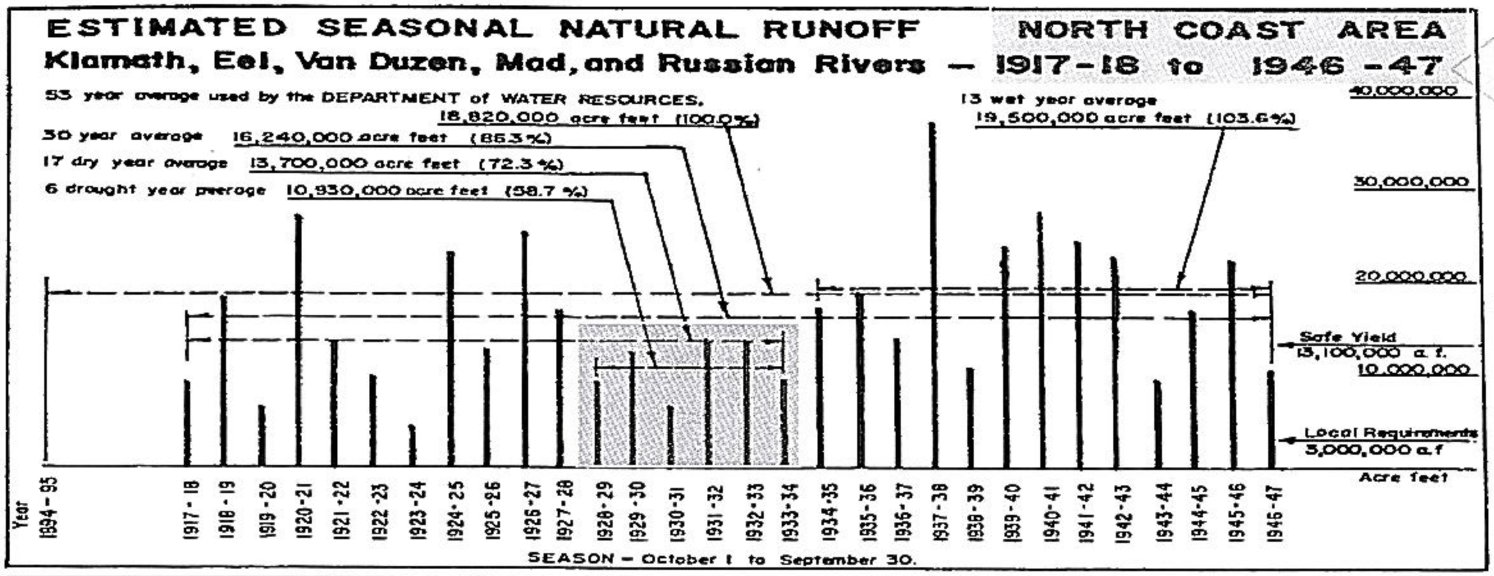
On October 12, 1948, Secretary of the Interior Krug, in a public speech at Oroville, stated: "Let me state, clearly and finally, the Interior Department is fully and completely committed to the policy that no water which is needed in the Sacramento Valley will be sent out of it." He added: "There is no intent on the part of the Bureau of Reclamation ever to divert from the Sacramento Valley a single acre-foot of water which might be used in the valley now or later." (Staff 9, p. 799 & SRDWA 19).

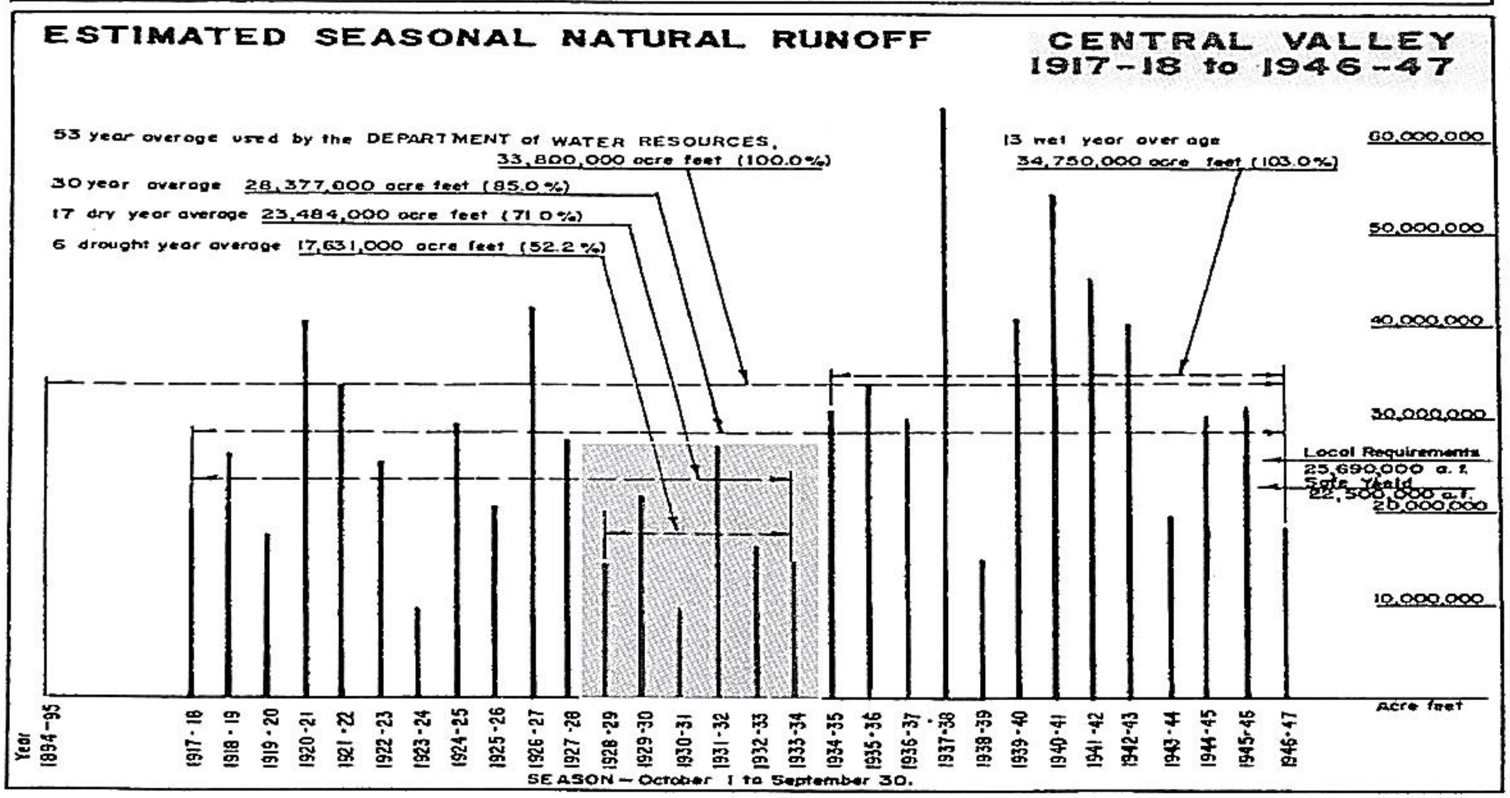
On November 15, 1949, Regional Director Richard L. Boke reaffirmed these main policy statements and summarized them in a letter to Congressman Clair Engle, stating, "We believe the foregoing is a summary of the main policy statements by Government officials on the subject of importation of Sacramento Valley water to the San Joaquin Valley." (Staff 9, p. 799 & SRDWA 19).

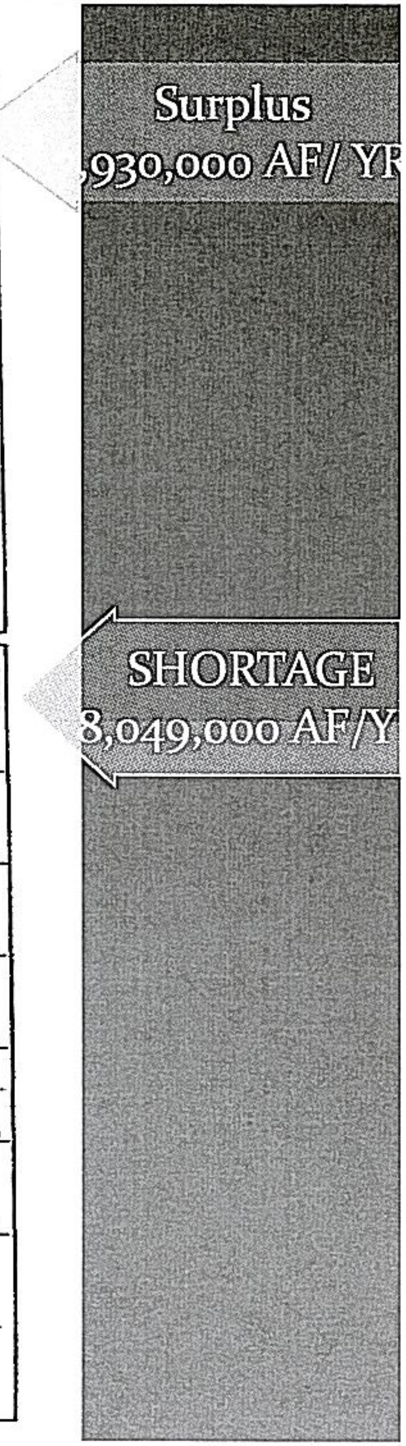
PROMISE NOT KEPT

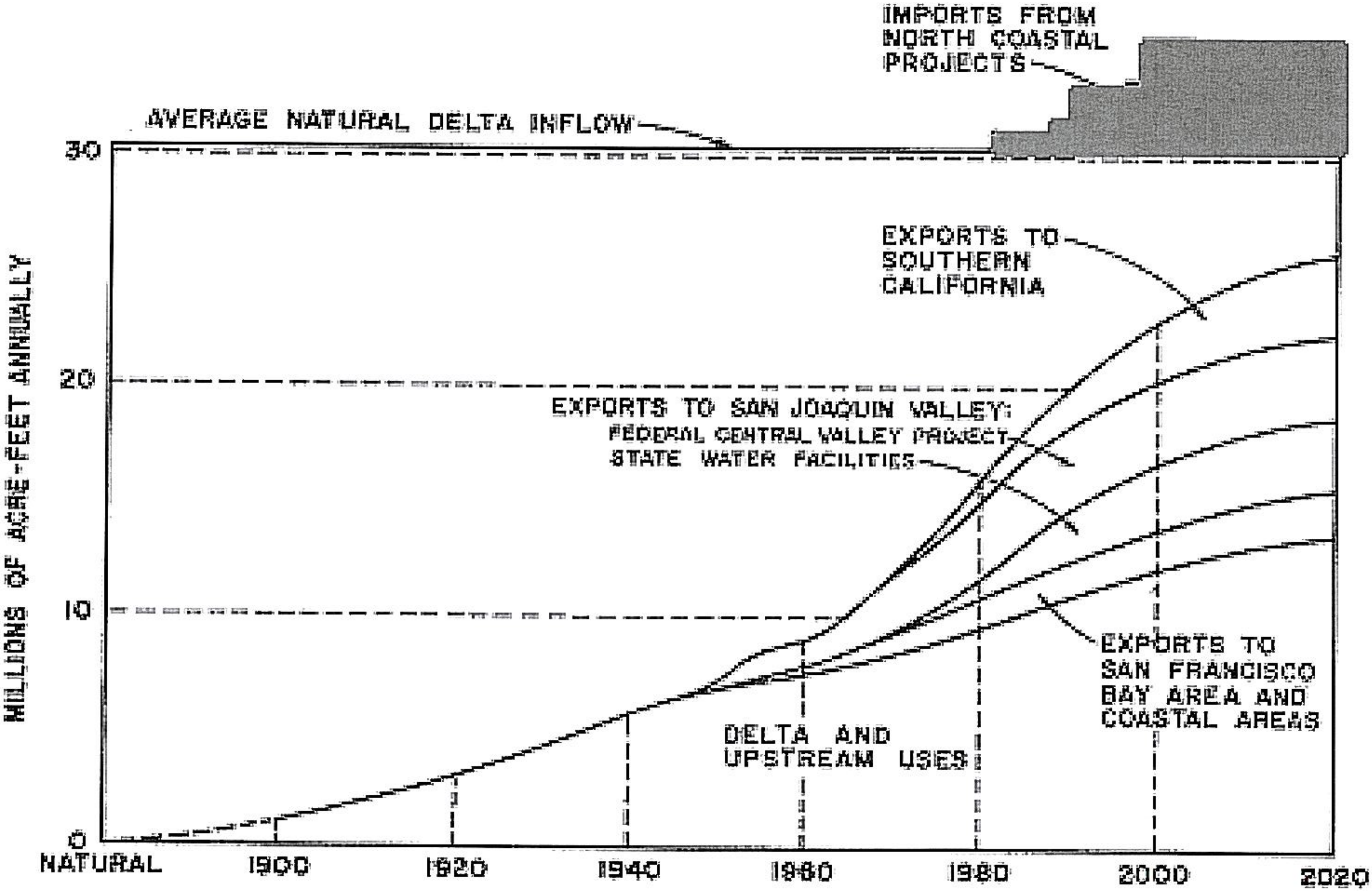
D 990 at pages 70 and 71

WEBER FOUNDATION STUDIES









USE OF DELTA WATER SUPPLIES

EXTRACTS OF USACE MAY 23, 2007 COMMENTS

The assumption that the 23 large watershed's 100-year flows can be added together to produce the 100-year Delta flow is invalid.

The assumption that failures in a levee system will not significantly reduce stage elevations along channel is questionable.

Annual mean number for seismic levee failures is 3.41 . . . . 341 failures per 100 years which is 341 more than observed in the past 100+ years . . . . Surely, these numbers cannot be credible results.

The average of 7.35 flood failures per year is three times the (undocumented) 2.60 number and nearly 6 times the observed flood failure rate from 1950 to 2006. Thus, as with the seismic failure number above, this flood number simply appears way outside the bounds of credibility.

Return periods of 2.7 or 5 years for many levees just seem incorrect and incompatible with decades of recent data.

Overall, the seismic fragilities simply appear unrealistic - with far too many breaks to be credible.

Figure 6-40 implies that for a M 7.5 event this type of levee has a 10% chance of displacing 10 ft. at all PGAs > 0.10. This seems Really Extreme.

Conclusion that 40% of historical failures (2.6) are from through seepage results in over 1.0 per year is different than historical rate and needs to be explained.

At first glance, the calculated annual number of failures is, to be polite, "extraordinary" albeit not as extreme as the seismic results above.

The estimated 30 or more island breaches in the next 25 years due to flood events seem too high/pessimistic.

The BAU assumption that levee crest elevations will not be raised in response to increased tidal and flood elevations is not realistic.
1 ft easy, 3 ft maybe doable for 100 years of effort.

TIDES & CURRENTS

Search

Home | Products | Programs | Partnerships | Education | Help

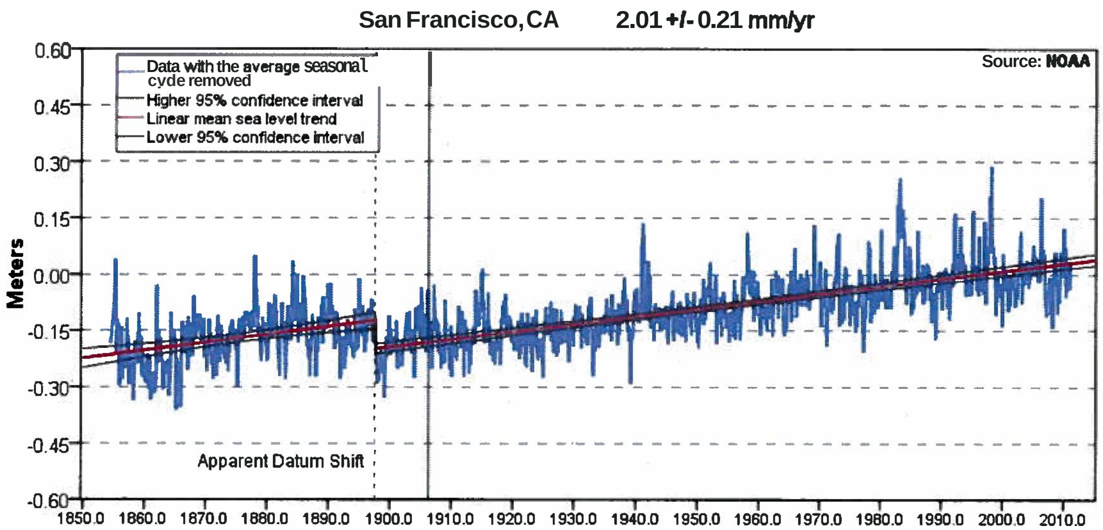
sea level trends

Alabama
Alaska
California
Connecticut
Delaware
Florida
Georgia
Hawaii
Louisiana
Maine
Maryland
Massachusetts
New Jersev
New York
North Carolina
Oregon
Pennsvlvania
Rhode Island
South Carolina
Texas
Virginia
Washinqton
Washinqton DC
Island Stations

Global Stations

main page

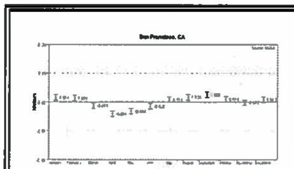
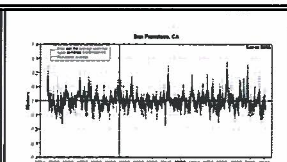
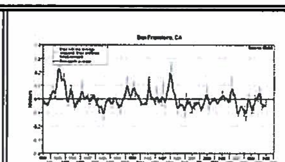
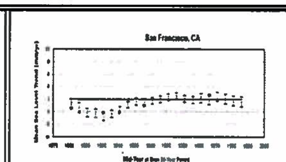
Mean Sea Level Trend
9414290 San Francisco, California



The mean sea level trend is 2.01 millimeterslyear with a 95% confidence interval of +/- 0.21 mm/yr based on monthly mean sea level data from 1897 to 2006 which is equivalent to a change of 0.66 feet in 100 years.

The plot shows the monthly mean sea level without the regular seasonal fluctuations due to coastal ocean temperatures, salinities, winds, atmospheric pressures, and ocean currents. The long-term linear trend is also shown, including its 95% confidence interval. The plotted values are relative to the most recent Mean Sea Level datum established bv CO-OPS. The calculated trends for all stations are available as a table in millimeters/vear or a table in feet/century (0.3 meters = 1 foot).

If present, solid vertical lines indicate times of any major earthquakes in the vicinity of the station and dashed vertical lines bracket any periods of questionable data.

 Average seasonal cycle	 Interannual variation for all data	 Interannual variation since 1980	 Variation of 50-year mean sea level trends

Frequently Asked Questions:

What is Sea Level?
Why does Sea Level chanae over time?
What does Sea Level have to do with Climate?

Back to Sea Levels Online

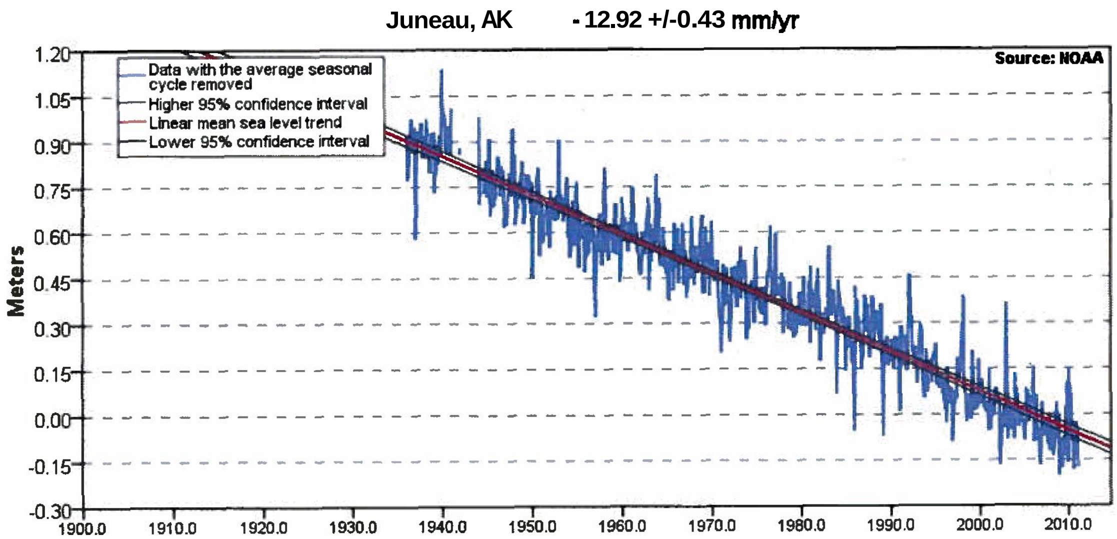
sea level trends

Alabama
Alaska
California
Connecticut
Delaware
Florida
Georaia
Hawaii
Louisiana
Maine
Maryland
Massachusetts
New Jersey
New York
North Carolina
Oregon
Pennsylvania
Rhode Island
South Carolina
Texas
Virginia
Washinqton
Washington DC
Island Stations

Global Stations

main page

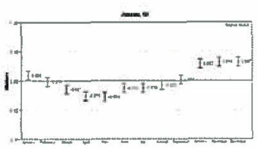
Mean Sea Level Trend
9452210 Juneau, Alaska



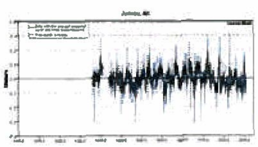
The mean sea level trend is -12.92 millimeterslyear with a 95% confidence interval of +/- 0.43 mm/yr based on monthly mean sea level data from 1936 to 2006 which is equivalent to a change of -4.24 feet in 100 years.

The plot shows the monthly mean sea level without the regular seasonal fluctuations due to coastal ocean temperatures, salinities, winds, atmospheric pressures, and ocean currents. The long-term linear trend is also shown, including its 95% confidence interval. The plotted values are relative to the most recent Mean Sea Level datum established by CO-OPS. The calculated trends for all stations are available as a table in millimeters/vear or a table in feet/century (0.3 meters = 1 foot).

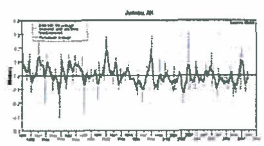
If present, solid vertical lines indicate times of any major earthquakes in the vicinity of the station and dashed vertical lines bracket any periods of questionable data.



Average seasonal cycle



Interannual variation for all data



Interannual variation since 1980

Frequently Asked Questions:

What is Sea Level?
Why does Sea Level chanae over time?
What does Sea Level have to do with Climate?

Back to Sea Levels Online

Search

Home | Products | Programs | Partnerships | Education | Help

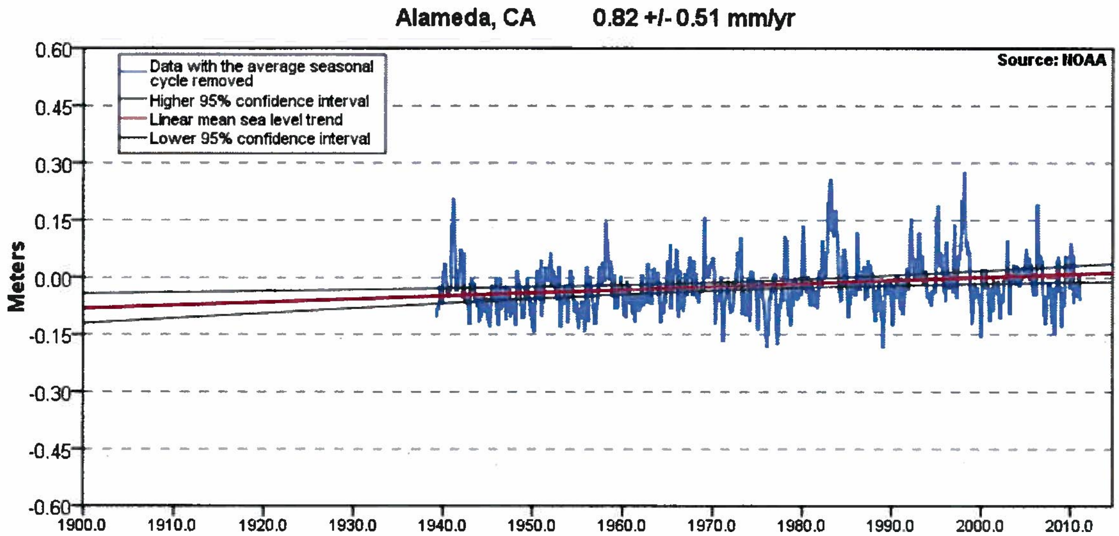
sea level trends

Alabama
Alaska
California
Connecticut
Delaware
Florida
Georgia
Hawaii
Louisiana
Maine
Maryland
Massachusetts
New Jersey
New York
North Carolina
Oregon
Pennsylvania
Rhode Island
South Carolina
Texas
Virginia
Washington
Washington DC
Island Stations

Global Stations

main page

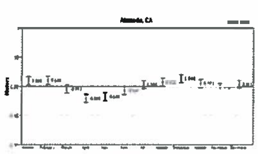
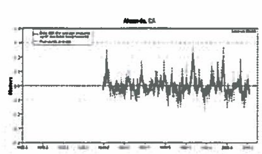
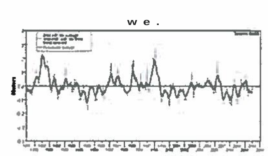
Mean Sea Level Trend
9414750 Alameda, California



The mean sea level trend is 0.82 millimeters/year with a 95% confidence interval of +/- 0.51 mm/yr based on monthly mean sea level data from 1939 to 2006 which is equivalent to a change of 0.27 feet in 100 years.

The plot shows the monthly mean sea level without the regular seasonal fluctuations due to coastal ocean temperatures, salinities, winds, atmospheric pressures, and ocean currents. The long-term linear trend is also shown, including its 95% confidence interval. The plotted values are relative to the most recent Mean Sea Level datum established by CO-OPS. The calculated trends for all stations are available as a table in millimeters/year or a table in feet/century (0.3 meters = 1 foot).

If present, solid vertical lines indicate times of any major earthquakes in the vicinity of the station and dashed vertical lines bracket any periods of questionable data.

 Average seasonal cycle	 Interannual variation for all data	 Interannual variation since 1980

Frequently Asked Questions:

What is Sea Level?
Why does Sea Level chanae over time?
What does Sea Level have to do with Climate?

Back to Sea Levels Online

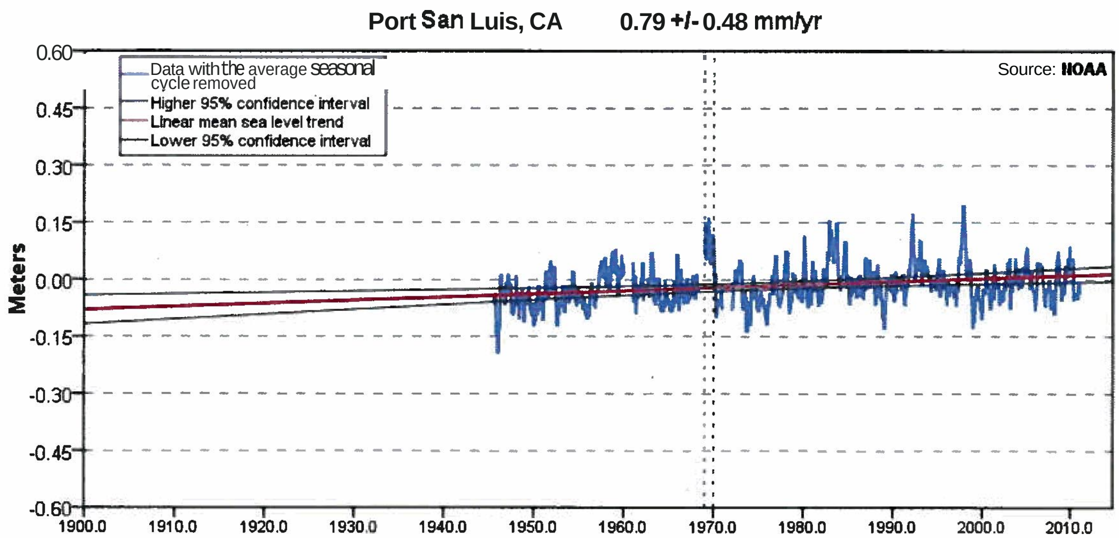
home | products | programs | partnerships | education | help sea level trends

Alabama
Alaska
California
Connecticut
Delaware
Florida
Georgia
Hawaii
Louisiana
Maine
Maryland
Massachusetts
New Jersey
New York
North Carolina
Oregon
Pennsylvania
Rhode Island
South Carolina
Texas
Virginia
Washington
Washington DC
Island Stations

Global Stations

main page

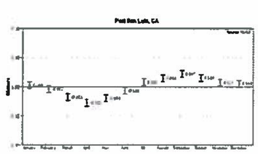
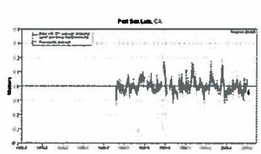
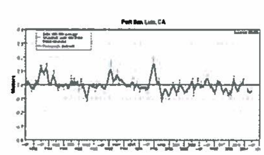
Mean Sea Level Trend
9412110 Port San Luis, California



The mean sea level trend is 0.79 millimeters/year with a 95% confidence interval of +/- 0.48 mm/yr based on monthly mean sea level data from 1945 to 2006 which is equivalent to a change of 0.26 feet in 100 years.

The plot shows the monthly mean sea level without the regular seasonal fluctuations due to coastal ocean temperatures, salinities, winds, atmospheric pressures, and ocean currents. The long-term linear trend is also shown, including its 95% confidence interval. The plotted values are relative to the most recent Mean Sea Level datum established by CO-OPS. The calculated trends for all stations are available as a table in millimeters/year or a table in feet/century (0.3 meters = 1 foot).

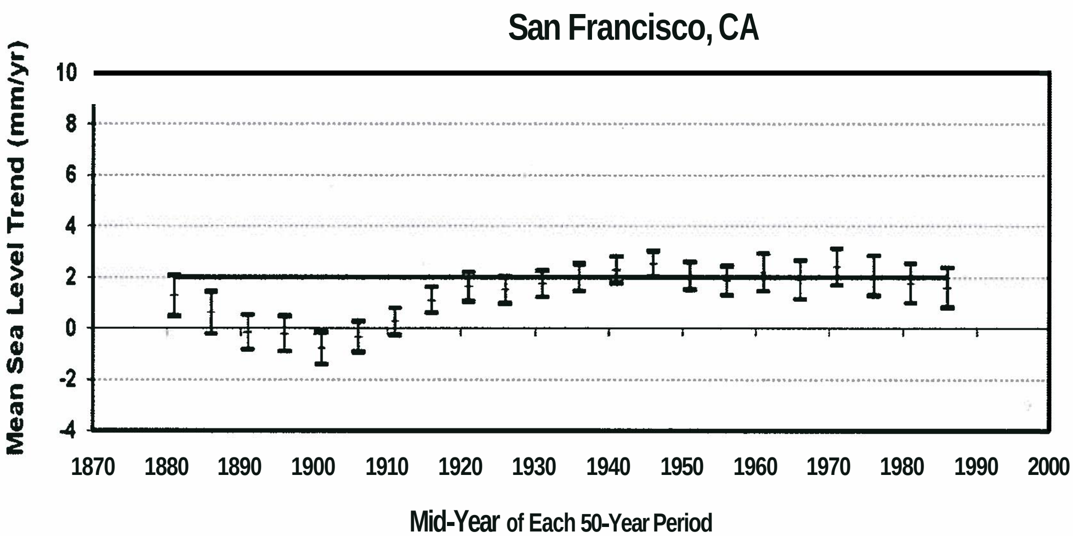
If present, solid vertical lines indicate times of any major earthquakes in the vicinity of the station and dashed vertical lines bracket any periods of questionable data:

 Average seasonal cycle	 Interannual variation for all data	 Interannual variation since 1980

Frequently Asked Questions:

What is Sea Level?
Why does Sea Level change over time?
What does Sea Level have to do with Climate?

Variation of 50-Year Mean Sea Level Trends
9414290 San Francisco, California



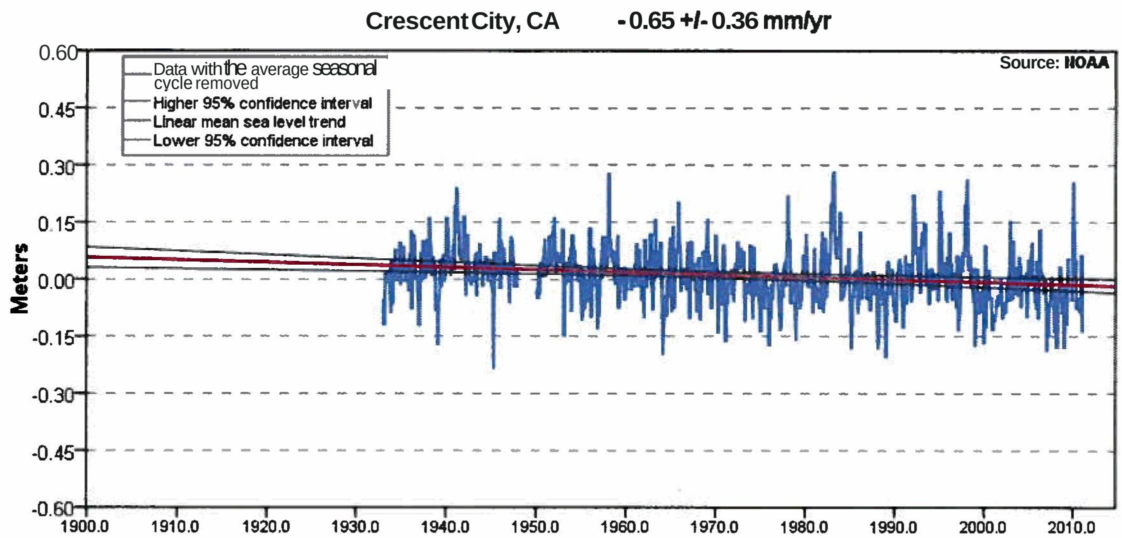
Linear mean sea level trends were calculated in overlapping 50-year increments for stations with sufficient historical data. The variability of each 50-year trend, with 95% confidence interval, is plotted against the mid-year of each 50-year period. The solid horizontal line represents the linear mean sea level trend using the entire period of record.

Variation of 50-Year Mean Sea Level Trends

1612340 Honolulu, Hawaii	1617760 Hilo, Hawaii	8418150 Portland, Maine	8443970 Boston, Massachusetts	8518750 The Battery, New York
8534720 Atlantic City, New Jersey	8545240 Philadelphia, Pennsylvania	8557380 Lewes, Delaware	8574680 Baltimore, Maryland	8594900 Washington, DC
8638610 Sewells Point, Virginia	8665530 Charleston, South Carolina	8720030 Fernandina Beach, Florida	8724580 Key West, Florida	8727520 Cedar Key, Florida
8729840 Pensacola, Florida	8771450 Galveston Pier 21, Texas	9410170 San Diego, California	9410230 La Jolla, California	9410660 Los Angeles, California
9414290 San Francisco, California	9439040 Astoria, Oregon	9447130 Seattle, Washington	9450460 Ketchikan, Alaska	9451600 Sitka, Alaska

Back to Sea Levels Online

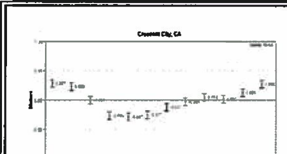
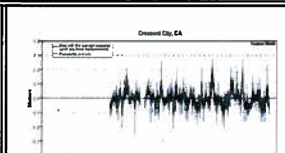
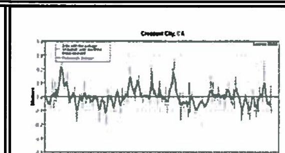
Mean Sea Level Trend
9419750 Crescent City, California



The mean sea level trend is -0.65 millimeterslyear with a 95% confidence interval of +/- 0.36 mm/yr based on monthly mean sea level data from 1933 to 2006 which is equivalent to a change of -0.21 feet in 100 years.

The plot shows the monthly mean sea level without the regular seasonal fluctuations due to coastal ocean temperatures, salinities, winds, atmospheric pressures, and ocean currents. The long-term linear trend is also shown, including its 95% confidence interval. The plotted values are relative to the most recent Mean Sea Level datum established bv CO-OPS. The calculated trends for all stations are available as a table in millimeters/year or a table in feet/century (0.3 meters = 1 foot).

If present, solid vertical lines indicate times of any major earthquakes in the vicinity of the station and dashed vertical lines bracket any periods of questionable data:

 Average seasonal cycle	 Interannual variation for all data	 Interannual variation since 1980

Frequently Asked Questions:

What is Sea Level?
Whv does Sea Level chanae over time?
What does Sea Level have to do with Climate?

Back to Sea Levels Online

Exhibit 6-20. Changes in relative sea level along U.S. coasts, 1958-2008
Millimeters per year

Location name	Latitude	Longitude	Mean relative sea level change
Nawiliwili, Hawaii	21.96	-159.36	1.3361
Honolulu, Hawaii	21.31	-157.87	1.2621
Kahului, Hawaii	20.90	-156.47	1.8835
Hilo, Hawaii	19.73	-155.06	2.6532
Johnston Atoll	16.74	-169.53	0.5723
Sand Island, Midway Is.	28.21	-177.36	1.395
Guam, Marianas Is.	13.44	144.65	2.6003
American Samoa	-14.28	-170.69	2.2878
Kwajalein, Marshall Is.	8.74	167.74	2.087
Wake Island	19.29	166.62	2.0405
Bermuda	32.37	-64.70	1.5085
Eastport, Maine	44.90	-66.99	0.9659
Bar Harbor, Maine	44.39	-68.21	1.5721
Portland, Maine	43.66	-70.25	1.0163
Boston, Massachusetts	42.36		2.2047
Woods Hole, Massachusetts	41.52	-70.67	2.3719
Nantucket Island, Massachusetts	41.29	-70.10	2.9368
Newport, Rhode Island	41.51	-71.33	2.4958
Providence, Rhode Island	41.81	-71.40	1.9739
New London, Connecticut	41.36	-72.09	2.4164
Bridgeport, Connecticut	41.17	-73.18	2.3146
Montauk, New York		-71.96	2.7699
Kings Point, New York	40.81	-73.77	2.0713
The Battery, New York	40.70	-74.02	2.7292
Sandy Hook, New Jersey	40.47	-74.01	3.58
Atlantic City, New Jersey	39.36	-74.42	4.3522
Philadelphia, Pennsylvania	39.93	-75.14	3.5187
Lewes, Delaware	38.78	-75.12	3.2052
Baltimore, Maryland	39.27	-76.58	2.8429
Annapolis, Maryland	38.98	-76.48	2.991
Solomons Island, Maryland	38.32	-76.45	3.7482
Washington, DC	38.87	-77.02	2.9554
Kiptopeke, Virginia	37.17	-75.99	3.4554
Gloucester Point, Virginia	37.25	-76.50	3.958
Sewells Point, Virginia	36.95	-76.33	4.6204
Beaufort, North Carolina	34.72	-76.67	2.832
Wilmington, North Carolina	34.23	-77.95	2.198
Charleston, South Carolina	32.78	-79.93	2.9447
Fort Pulaski, Georgia	32.03	-80.90	3.2904
Fernandina Beach, Florida	30.67	-81.47	2.4199
Mayport, Florida	30.40	-81.43	2.5459
Key West, Florida	24.55	-81.81	2.517
Naples, Florida	26.13	-81.81	2.027
Fort Myers, Florida	26.65	-81.87	2.229
St. Petersburg, Florida	27.76	-82.63	2.6246
Cedar Key, Florida	29.14	-83.03	1.7058
Pensacola, Florida	30.40	-87.21	2.0069

Grand Isle, Louisiana	29.26	-89.96	9.2911
Galveston Pier 21, Texas	29.31	-94.79	6.5704
Galveston Pleasure Pier, Texas	29.29	-94.79	6.9176
Freeport, Texas	28.95	-95.31	8.4887
Rockport, Texas	28.02	-97.05	6.1652
Port Isabel, Texas	26.06	-97.22	4.407
San Diego, California	32.71	-117.17	1.7841
La Jolla, California	32.87	-117.26	1.9505
Los Angeles, California	33.72	-118.27	0.9553
Santa Monica, California	34.01	-118.50	0.8125
Port San Luis, California	35.18	-120.76	0.2763
San Francisco, California	37.81	-122.47	1.579
Alameda, California	37.77	-122.30	0.5961
Crescent City, California	41.75	-124.18	-0.8903
South Beach, Oregon	44.63	-124.04	2.3148
Astoria, Oregon	46.21	-123.77	-0.3775
Neah Bay, Washington	48.37	-124.62	-2.3007
Seattle, Washington	47.61	-122.34	1.7332
Friday Harbor, Washington	48.55	-123.01	0.9287
Ketchikan, Alaska	55.33	-131.63	-0.4991
Sitka, Alaska	57.05	-135.34	-2.0497
Juneau, Alaska	58.30	-134.41	-13.5467
Yakutat, Alaska	59.55	-139.74	-8.3745
Cordova, Alaska	60.56	-145.75	4.9718
Seward, Alaska	60.12	-149.43	6.9319
Seldovia, Alaska	59.44	-151.72	-9.5183
Adak Island, Alaska	51.86	-176.63	-2.8201
Unalaska, Alaska	53.88	-166.54	-5.3827
Magueyes Island, Puerto Rico	17.97	-67.05	1.3208

Remote Sens. **2011**, *3*, 1603-1613; doi:10.3390/rs3081603

OPEN ACCESS

Remote Sensing

ISSN 2072-4292

www.mdpi.com/journal/remotesensing

Article

On the Misdiagnosis of Surface Temperature Feedbacks from Variations in Earth's Radiant Energy Balance

Roy W. Spencer * and William D. Braswell

ESSC-UAH, University of Alabama in Huntsville, Cramer Hall, Huntsville, AL 35899, USA;
E-Mail: danny.braswell@nsstc.uah.edu

* Author to whom correspondence should be addressed; E-Mail: roy.spencer@nsstc.uah.edu;
Tel.: +1-256-961-7960; Fax: +1-256-961-7751.

Received: 24 May 2011; in revised form: 13 July 2011 / Accepted: 15 July 2011 / Published: 25 July 2011

Abstract: The sensitivity of the climate system to an imposed radiative imbalance remains the largest source of uncertainty in projections of future anthropogenic climate change. Here we present further evidence that this uncertainty from an observational perspective is largely due to the masking of the radiative feedback signal by internal radiative forcing, probably due to natural cloud variations. That these internal radiative forcings exist and likely corrupt feedback diagnosis is demonstrated with lag regression analysis of satellite and coupled climate model data, interpreted with a simple forcing-feedback model. While the satellite-based metrics for the period 2000–2010 depart substantially in the direction of lower climate sensitivity from those similarly computed from coupled climate models, we find that, with traditional methods, it is not possible to accurately quantify this discrepancy in terms of the feedbacks which determine climate sensitivity. It is concluded that atmospheric feedback diagnosis of the climate system remains an unsolved problem, due primarily to the inability to distinguish between radiative forcing and radiative feedback in satellite radiative budget observations.

Keywords: climate; sensitivity; temperature; feedback; clouds; warming; CERES; models

1. Introduction and Background

The magnitude of the surface temperature response of the climate system to an imposed radiative energy imbalance remains just as uncertain today as it was decades ago [1]. Over 20 coupled ocean-atmosphere climate models tracked by the Intergovernmental Panel on Climate Change (IPCC) produce a wide range of warming estimates in response to the infrared radiative forcing theoretically expected from anthropogenic greenhouse gas emissions [2].

From a modeling standpoint, this lack of progress is evidence of the complexity of the myriad atmospheric processes that combine to determine the sign and magnitude of feedbacks. It is also due to our inability to quantify feedbacks in the real climate system, a contentious issue with a wide range of published feedback diagnoses [1] and disagreements over the ability of existing methods to diagnose feedback [3,4].

Spencer and Braswell ([5] hereafter SB10) discussed what they believed to be the primary difficulty in diagnosing feedback from variations in the Earth's radiative energy balance between absorbed shortwave (SW) solar radiation and thermally emitted longwave (LW) infrared (IR) radiation. SB10 attributed the difficulty to the contamination of the feedback signature by unknown levels of time-varying, internally generated radiative forcing; for example, 'unforced' natural variations in cloud cover.

In simple terms, radiative changes *resulting from* temperature change (feedback) cannot be easily disentangled from those *causing* a temperature change (forcing).

Much can be learned about the interaction between radiative forcing and feedback through a simple time dependent forcing-feedback model of temperature variations away from a state of energy equilibrium,

$$C_p\, d\Delta T/dt = S(t) + N(t) - \lambda\Delta T \tag{1}$$

Equation (1) states that time-varying sources of non-radiative forcing S and radiative forcing N cause a climate system with bulk heat capacity C_p to undergo a temperature change with time away from its equilibrium state ($d\Delta T/dt$), but with a net radiative feedback 'restoring force' ($-\lambda\Delta T$) acting to stabilize the system. For the interannual temperature climate variability we will address here, the heat capacity C_p in Equation (1) is assumed to represent the oceanic mixed layer. (Note that if C_p is put inside the time differential term, the equation then becomes one for changes in the heat content of the system with time. While it is possible that feedback can be more accurately diagnosed by analyzing changes in the heat content of the ocean over time [6], our intent here is to examine the problems inherent in diagnosing feedback based upon surface temperature changes.)

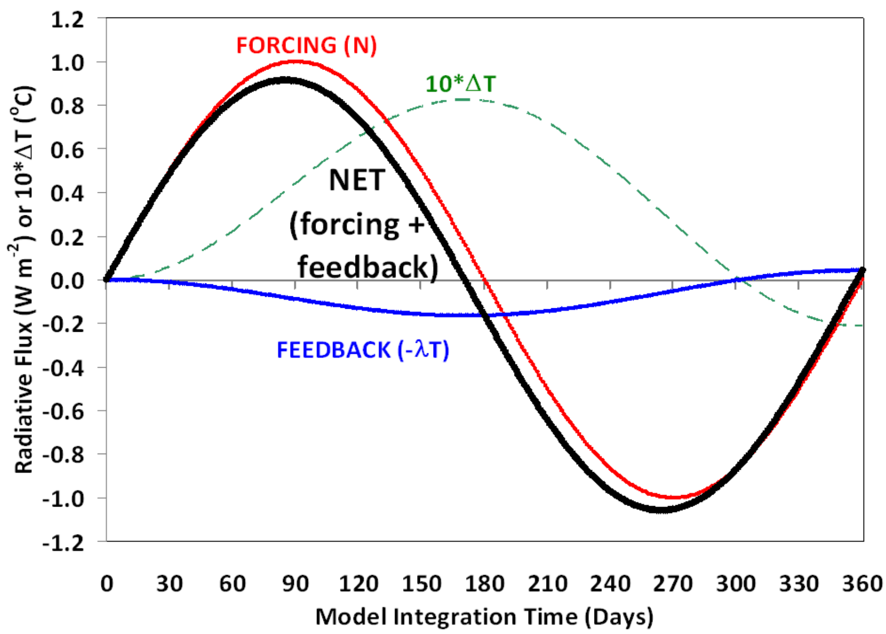
Radiative forcings (N) of temperature change could arise, for example, from natural fluctuations in cloud cover which are not the direct or indirect result of a temperature change (that is, not due to feedback) [7]. Examples of non-radiative forcing (S) would be fluctuations in the heat exchange between the mixed layer and deep ocean, or between the mixed layer and the overlying atmosphere. Importantly, satellite radiative budget instruments measure the combined influence of radiative forcing (N) and radiative feedback ($-\lambda\Delta T$) in unknown proportions.

Although not usually considered a feedback *per se*, the most fundamental component of the net feedback parameter λ is the direct dependence of the rate of IR emission on temperature, estimated to be about 3.3 W m^{-2} K^{-1} in the global average [8]. This 'Planck' or 'Stefan-Boltzmann' response stabilizes the climate system against runaway temperature changes, and represents a baseline from which feedbacks are traditionally referenced. Positive feedbacks in the climate system reduce the net feedback parameter below 3.3, while negative feedbacks increase it above 3.3. Here we will deal with the net feedback parameter exclusively, as it includes the combined influence of all climate feedbacks, as well as the Planck effect.

The larger the net feedback parameter λ, the smaller the temperature response to an imposed energy imbalance N will be; the smaller λ is, the greater the temperature response will be. A negative value for λ would indicate a climate system whose temperature is unstable to radiative forcing. The coupled ocean-atmosphere climate models tracked by the IPCC have diagnosed long-term net feedback parameters ranging from $\lambda = 0.89$ for the most sensitive model, MIROC-Hires, to $\lambda = 1.89$ for the least sensitive model, FGOALS [8]. Since this range is below the Planck response of 3.3 W m^{-2} K^{-1}, all of the IPCC models therefore exhibit net positive feedbacks. Also, since all climate models have net feedback parameters greater than zero, none of the climate models are inherently unstable to perturbations.

It is worth reiterating that satellite radiative budget instruments measure the combined effect of the radiative terms on the RHS of Equation (1), that is, the radiative forcing term N and the feedback term ($-\lambda\Delta T$). That the presence of N can have a profound impact on feedback diagnosis is easily demonstrated with a simple time dependent model based upon Equation (1). If we assume C_p consistent with a 25 m deep oceanic mixed layer, a net feedback parameter $\lambda = 3$, and a sinusoidal forcing with period of one year, the temperature response shown in Figure 1 will result.

Figure 1. Simple forcing-feedback model demonstration that satellite radiative budget instrument measurements of Net radiative flux (forcing + feedback) are very different from what is needed to diagnose the net feedback parameter (feedback only).



In response to radiative forcing, the model ocean warms, which in turn causes a net radiative feedback response. Significant to our goal of diagnosing feedback, the net feedback response to a temperature change is always smaller than the radiative forcing which caused it, owing to the heat capacity of the system, until radiative equilibrium is once again restored. At that point the radiative feedback equals the radiative forcing.

Unfortunately, in the real climate system radiative forcings are continually changing, which means the feedback response will in general be smaller than the radiative forcing. The presence of this radiative forcing tends to confound the accurate determination of feedback. If the only source of radiative variability was feedback, then regression of the time series ($-\lambda\Delta T$) against the temperature time series (ΔT) in Figure 1 would yield an accurate feedback diagnosis with the regression slope $\lambda = 3$ W m^{-2} K^{-1}. But the presence of time varying radiative forcing in Figure 1 has a very different signature than that of feedback, yet it is the sum of the two which the satellite measures.

As shown by SB10, the presence of any time-varying radiative forcing decorrelates the co-variations between radiative flux and temperature. Low correlations lead to regression-diagnosed feedback parameters biased toward zero, which corresponds to a borderline unstable climate system. We believe that the low correlations associated with previous feedback diagnoses with satellite data are themselves *prima facie* evidence of the presence of radiative forcing in the data.

In the real climate system, it is likely there is almost always a time-varying radiative forcing present, as various internally-generated changes in clouds and water vapor oscillate between positive and negative values faster than the resulting temperature changes can restore the system to radiative equilibrium. This means that feedback diagnosis will, in general, be contaminated by an unknown amount of time-varying internal radiative forcing N. If those forcings were known, they could have been subtracted from the measured radiative flux variations before diagnosing feedback, e.g., as has been done for the feedback response of the coupled climate models to transient carbon dioxide forcing [8].

Central to the difficulty of feedback diagnosis is the very different time-dependent relationships which exist between forcing and temperature, *versus* between feedback and temperature. While there is a substantial *time lag* between forcing and the temperature response due to the heat capacity of the ocean, the radiative feedback response to temperature is *nearly simultaneous* with the temperature change. This near-simultaneity is due to a combination of the instantaneous temperature effect on the LW portion of λ (the Planck response of 3.3 W m^{-2} K^{-1}), and the relatively rapid convective coupling of the surface to the atmosphere, which causes surface temperature-dependent changes in water vapor, clouds, and the vertical profile of temperature.

While SB10 provided evidence that such radiatively-induced temperature changes do exist, and in general lead to an underestimate of the net feedback parameter, this view has been challenged ([9] hereafter D10) with estimated cloud feedback from satellite observed variations in Earth's radiative energy balance during 2000–2010. D10 used the usual regression approach. Further, D10 assumed that the temperature changes during 2000–2010 were not radiatively forced by the atmosphere, but non-radiatively forced through changes in ocean circulation associated with the El Niño/Southern Oscillation (ENSO) [10] phenomenon. If D10 is correct that radiative forcing can be neglected ($N(t) \approx 0$), then satellite observed radiative variations would be dominated by feedback rather than forcing, and one should be able to diagnose feedback through regression of radiative variations against temperature variations.

Here we will provide evidence that those temperature changes instead had a strong component of radiative forcing, with radiative accumulation preceding, and radiative loss following temperature maxima. While SB10 used phase space analysis to demonstrate the presence of radiative forcing, here we will use lag regression analysis. By examining regression coefficients between temperature and radiative flux at a variety of leads and lags, rather than at just zero time lag, we can identify behaviors of the climate system that otherwise cannot be discerned.

First we will demonstrate what these lag relationships look like in the satellite observations and in the coupled climate models. Then, we will explore with a simple forcing-feedback model of the climate system what the relationships mean in terms of forcing and feedback.

2. Time-Lagged Signatures in Observational Data and Coupled Climate Models

2.1. Observational Data

The CERES (Clouds and the Earth's Radiant Energy System) [11] radiative budget instruments on NASA's Terra satellite have provided globally distributed estimates of reflected solar shortwave (SW) and thermally emitted infrared longwave (LW) radiative fluxes on a daily basis since March 2000. Variations in SW are caused mostly by changes in cloud cover, particularly low clouds, while variations in LW are mainly caused by temperature, water vapor, and high clouds.

We will use the same SSF Edition 2.5 monthly gridpoint radiative flux dataset used by D10, updated through June 2010, from which D10 claimed evidence for positive cloud feedback. The SSF dataset also includes a calculation of the 'Net' flux, which additionally accounts for the effect of small variations in the solar constant during 2000–2010,

$$\text{Net} = S/4 - (\text{LW} + \text{SW}) \tag{2}$$

where S is the top-of-atmosphere (TOA) incident solar radiation. By convention, the LW and SW fluxes are positive upward (away from Earth), while the Net flux is positive downward (toward Earth). In the context of our analysis of anomalies (departures from the average annual cycle), note the only difference between (−Net) and (LW + SW) is the small interannual variation in the incident solar flux; otherwise, the two are equivalent, and are sometimes treated interchangeably.

We computed monthly global area averages from the monthly gridpoint Net radiative fluxes in the 10+ year SSF Edition 2.5 dataset. From the resulting time series of monthly averages we then computed monthly anomalies, where each month's anomaly is the departure from the ten-year (or eleven-year) average for that calendar month. This allows us to examine year-to-year variations in the climate system.

Global monthly anomalies in surface temperature were similarly computed from the HadCRUT3 surface temperature dataset [12] between March 2000 and June 2010. In addition to globally averaged anomalies, we also computed area average anomalies over the ice-free oceans, between 60°N and 60°S, for all variables.

2.2. Coupled Climate Model Data

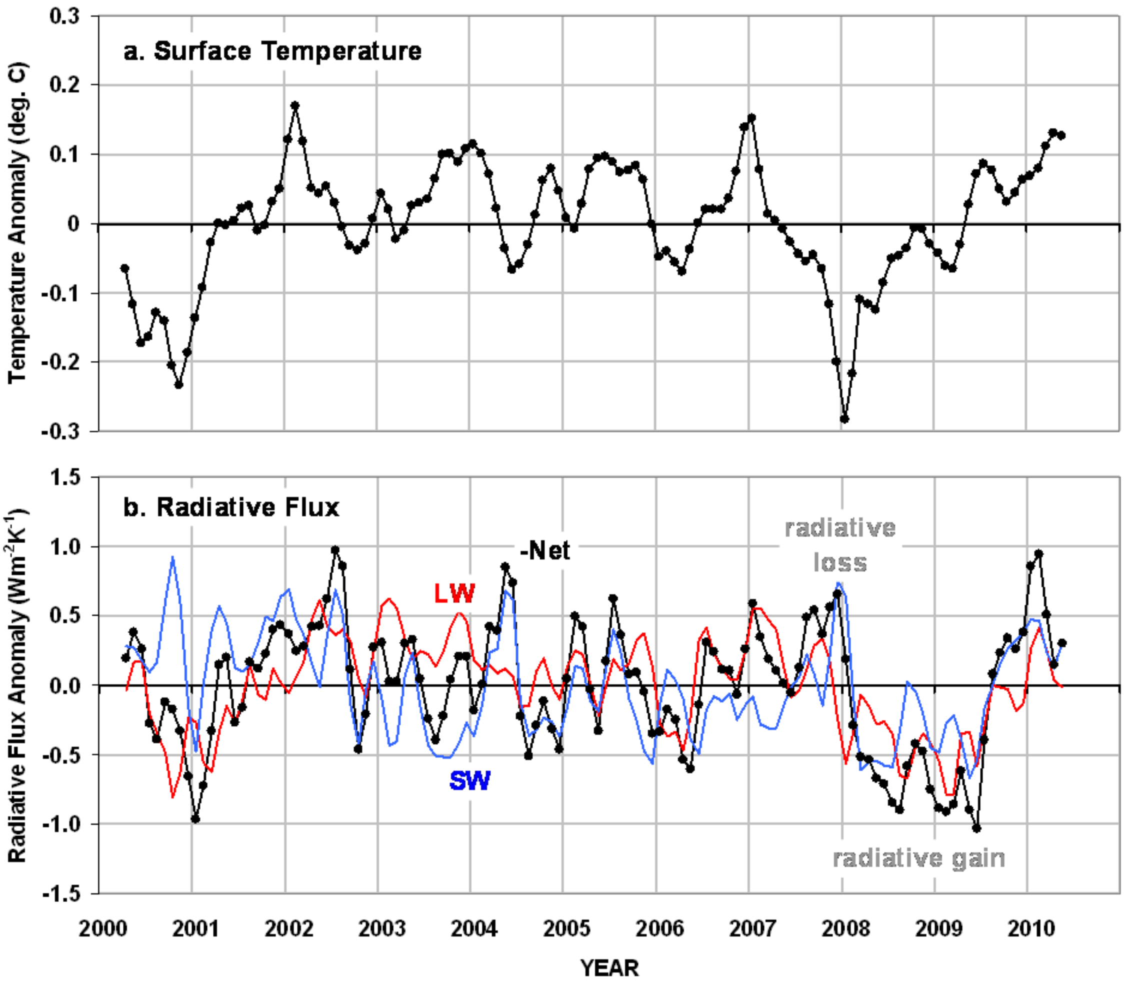
Global monthly anomalies in LW and SW fluxes, as well as in surface temperature, were also computed from the 20th Century runs of the World Climate Research Programme's (WCRP's) Coupled Model Intercomparison Project phase 3 (CMIP3) multi-model dataset archived at PCMDI, for the years 1900 through 1999. Because of the significant trends in the 20th Century simulations, the 100-year trend was removed from each anomaly time series in order to better isolate the interannual variability that will be compared to the relatively short (10 year) period of satellite data. While we computed results for 14 of the models archived, here will only present results for the three most sensitive models (MIROC3.2-hires; IPSL-CM4; MIROC3.2-medres), and the 3 least sensitive models

(FGOALS; NCAR PCM1; GISS-ER), where their sensitivity to transient carbon dioxide forcing was estimated by [8].

2.3. Observations versus *Coupled Climate Models*

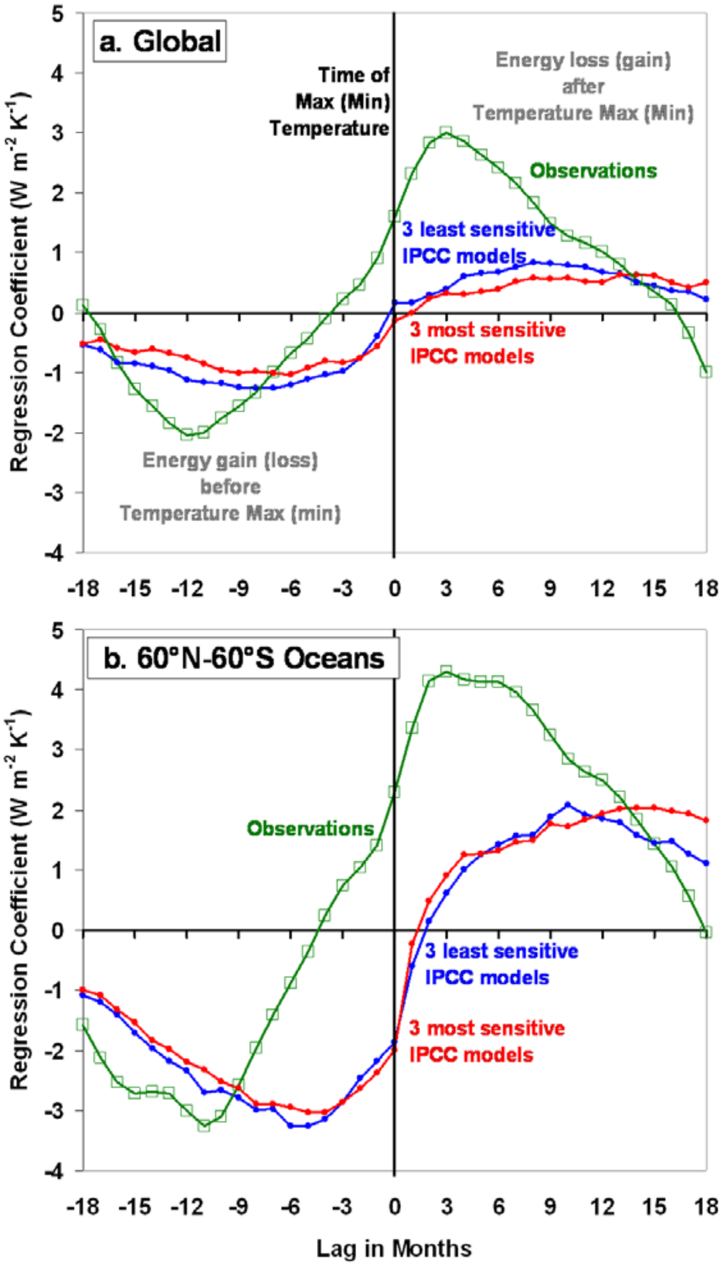
The time series of observed monthly global HadCRUT3 surface temperature anomalies from March 2000 through June 2010 is shown in Figure 2(a), while the LW, SW, and Net radiative fluxes from CERES are shown in Figure 2(b). Note that the negative of the Net flux is plotted so that its sign convention matches the individual LW and SW flux components, which is positive upward (away from Earth).

Figure 2. Times series of monthly global average anomalies in **(a)** surface temperatures from HadCRUT3, and **(b)** radiative fluxes from Terra CERES SSF Edition 2.5, for the period March 2000 through June 2010. All time series have a 1-2-1 smoother applied to reduce sampling noise.



Lagged regressions were performed between the surface temperature and the Net radiative flux time series shown in Figure 2, with the resulting regression coefficients shown in Figure 3. Computations for global anomalies (Figure 3(a)) and anomalies based upon only data over the global ice-free oceans (Figure 3(b)) are shown separately.

Figure 3. Lead and lag regression coefficients between monthly surface temperature anomalies and Net radiative flux anomalies in observations *versus* coupled climate models for: **(a)** global averages, and **(b)** global ocean averages, 60°N to 60°S.



One of the most obvious conclusions from Figure 3 is that the satellite observations and climate models display markedly different time-dependent behaviors in their temperature *versus* radiation variations, especially over the oceans (Figure 3(b)) which are of great interest in climate change studies due to their inherently long time scales of variability. Note that the differences in Figure 3 exist not just at zero time lag, which is where feedback estimates from these regression coefficients have previously been made, but for several months when radiative flux leads and lags temperature.

Also, note the change in sign of the radiative imbalances in Figure 3 depending upon whether radiation leads or lags temperature. As we will see, this behavior gives us clues about the relative roles of forcing *versus* feedback in the data.

3. Simple Model Simulations of Observed Behavior

The effect of radiative (N) *versus* non-radiative (S) forcing on the lagged regression coefficients can be demonstrated by a simple model based upon Equation (1). This helps to explain the difference between the satellite-measured *versus* climate model signatures in Figure 3. We again ran the simple forcing-feedback model with an assumed net feedback parameter of $\lambda = 3$ W m^{-2} K^{-1}; and an ocean mixed layer depth of 25 m, a choice which requires some discussion.

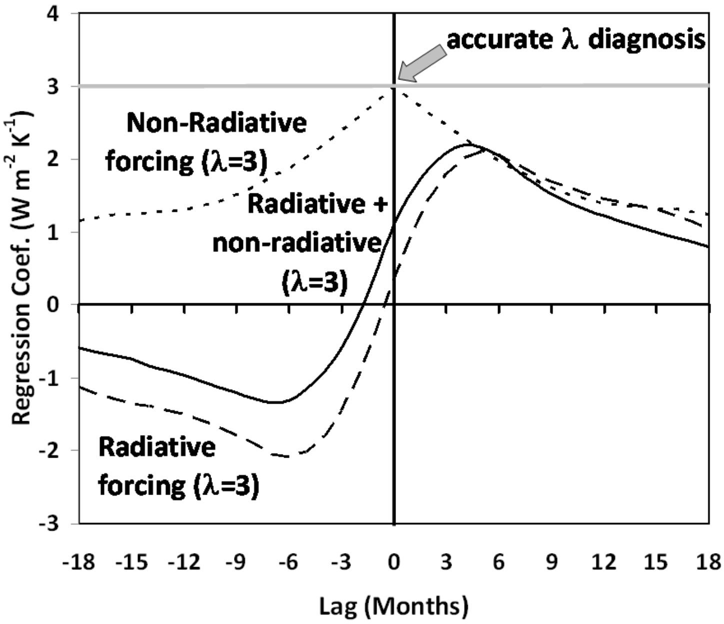
We found that the assumed mixed layer depth of 25 m is consistent with the average behavior of both the IPCC AR4 coupled climate models and the satellite observations on interannual time scales. Using Equation (1), we estimated C_p from both the coupled climate models and the satellite data by regressing 5-month trends ($d\Delta T/dt$) in the global average surface temperature anomalies against the 5-month average radiative imbalances, to get $1/C_p$ as the regression coefficient. The resulting C_p values from 14 IPCC AR4 models ranged from 11 m to 50 m, with a 14-model average of 27 m, while a similar regression on the 10+ years of satellite data revealed an equivalent mixing depth of 26 m, which supports our use of 25 m. (Note that, since about 30% of Earth is land having comparatively negligible heat capacity, the equivalent mixing depth of 25 m implies an average ocean mixing depth of about (25/0.7=) 35 m for the interannual time scales addressed here. Also, if most of the interannual temperature variability originates in the tropics, our diagnosed mixed layer depth will be biased toward tropical values, which are typically much shallower than at high latitudes.)

For the radiative forcing $N(t)$ we used a time series of normally-distributed monthly random numbers with box filter smoothing of 9 months to approximate the time scales of variations seen in the climate models and observations in Figure 3. A separate time series of random numbers without low pass filtering was used for the non-radiative forcing $S(t)$. This mimics what we believe to be intraseasonal oscillations in the heat flux between the ocean and atmosphere seen in the data [5,13]. The model time step was one month, and the model simulations were carried out for 500 years of simulated time.

The lag regression results from the simple model are shown in Figure 4 for (1) pure radiative forcing N, (2) pure non-radiative forcing S, and (3) a 70/30% mixture of both. Note that only in the case of pure non-radiative forcing (dotted line), at zero time lag, can accurate diagnosis of the feedback parameter can be made. As discussed above, this is because there is no radiative forcing present to contaminate the radiative feedback signal. Again, this is the only type of forcing D10 assumed was causing the surface temperature variability during 2000–2010, an assumption which allowed neglect of the radiative forcing issues raised here and by SB10.

If the temperature variations are radiatively forced, the lag regression relationships are very different (dashed line in Figure 4). In this case, radiative gain precedes, and radiative loss follows a temperature maximum, as would be expected based upon conservation of energy considerations. Significantly, the pure radiative forcing curve is most similar to the behavior seen in the coupled climate model output shown in Figure 3, indicating the dominating presence of internal radiative forcing in those models.

Figure 4. Lag regression coefficients between temperature and radiative flux from the simple forcing-feedback model run for three forcing cases: pure non-radiative forcing (dotted line); pure radiative forcing (dashed line); and a 70% radiative/30% non-radiative forcing mixture. A feedback parameter of 3 W m^{-2} K^{-1} and ocean mixing depth of 25 m were specified for all three simulations, which each ran for 500 years of simulated time.



Finally, a mixture of 70% radiative and 30% non-radiative forcing (solid line in Figure 3) produces lag regression coefficients that vary in a manner similar to the satellite data in Figure 3. This suggests that, while the temperature variations during 2000–2010 had a strong radiative forcing component, they were also influenced by more non-radiative forcing than is exhibited by the coupled climate models. In contrast, D10 assumed that non-radiative forcing dominated the climate variability measured by the satellite during 2000–2010.

Thus, we must conclude that time-varying radiative forcing exists in the satellite observations, as evidenced by the radiative gain/loss couplet patterns seen in Figures 3 and 4. Diagnosis of feedback cannot easily be made in such situations, because the radiative forcing decorrelates the co-variations between temperature and radiative flux. For example, no matter what feedback is specified when the simple model is only radiatively forced, the regression coefficient at zero time lag for a sufficiently long model simulation is always near-zero. We believe this effect has led to low biases in previously diagnosed feedback parameters from satellite data.

Determination of whether regression coefficients at various non-zero time lags might provide a more accurate estimate of feedback has been recently explored by [14], but is beyond the scope of this paper. Our preliminary work on this issue suggests no simple answer to the question. We conclude that the fundamental obstacle to feedback diagnosis remains the same, no matter what time lag is addressed: without knowledge of time-varying radiative forcing components in the satellite radiative flux measurements, feedback cannot be accurately diagnosed from the co-variations between radiative flux and temperature.

4. Discussion and Conclusions

We have shown clear evidence from the CERES instrument that global temperature variations during 2000–2010 were largely radiatively forced. Lag regression analysis supports the interpretation that net radiative gain (loss) precedes, and radiative loss (gain) follows temperature maxima (minima). This behavior is also seen in the IPCC AR4 climate models.

A simple forcing-feedback model shows that this is the behavior expected from radiatively forced temperature changes, and it is consistent with energy conservation considerations. In such cases it is difficult to estimate a feedback parameter through current regression techniques.

In contrast, predominately non-radiatively forced temperature changes would allow a relatively accurate diagnosis of the feedback parameter at zero time lag using regression since most radiative variability would be due to feedback. Unfortunately, this appears not to be the situation in either the satellite observations or the coupled climate models.

Yet, as seen in Figure 2, we are still faced with a rather large discrepancy in the time-lagged regression coefficients between the radiative signatures displayed by the real climate system in satellite data *versus* the climate models. While this discrepancy is nominally in the direction of lower climate sensitivity of the real climate system, there are a variety of parameters other than feedback affecting the lag regression statistics which make accurate feedback diagnosis difficult. These include the amount of non-radiative *versus* radiative forcing, how periodic the temperature and radiative balance variations are, the depth of the mixed layer, *etc*., all of which preclude any quantitative estimate of how large the feedback difference is. More recent work which attempts to minimize non-feedback influences [14] might well provide more accurate feedback estimates than previous studies.

Finally, since much of the temperature variability during 2000–2010 was due to ENSO [9], we conclude that ENSO-related temperature variations are partly radiatively forced. We hypothesize that changes in the coupled ocean-atmosphere circulation during the El Niño and La Niña phases of ENSO cause differing changes in cloud cover, which then modulate the radiative balance of the climate system. As seen in Figure 3(b) for the ocean-only data, the signature of radiative forcing is stronger over the oceans than in the global average, suggesting a primarily oceanic origin.

What this might (or might not) imply regarding the ultimate causes of the El Niño and La Niña phenomena is not relevant to our central point, though: that the presence of time varying radiative forcing in satellite radiative flux measurements corrupts the diagnosis of radiative feedback.

Acknowledgments

We acknowledge the modeling groups, the Program for Climate Model Diagnosis and Intercomparison (PCMDI) and the WCRP's Working Group on Coupled Modeling (WGCM) for their roles in making available the WCRP CMIP3 multi-model dataset. Support of this dataset is provided by the Office of Science, US Department of Energy. This research was sponsored by DOE contract DE-SC0005330 and NOAA contract NA09NES4400017.

References

1. Knutti, R.; Hegerl, G.C. The equilibrium sensitivity of the Earth's temperature to radiation changes. *Nature Geosci.* **2008**, *1*, 735-743.
2. Randall, D.A.; Wood, R.A.; Bony, S.; Colman, R.; Fichefet, T.; Fyfe, J.; Kattsov, V.; Pitman, A.; Shukla, J.; Srinivasan, J.; Stouffer, R.J.; Sumi, A.; Taylor, K.E. Climate models and their evaluation. In *IPCC, Climate Change 2007: The Physical Science Basis*; Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K.B., Tignor, M., Miller, H.L., Eds.; Cambridge University Press: Cambridge, UK and New York, NY, USA, 2007.
3. Aires, F.; Rossow, W.B. Inferring instantaneous, multivariate and nonlinear sensitivities for analysis of feedbacks in a dynamical system: Lorenz model case study. *Quart. J. Roy. Meteor. Soc*. **2003**, *129*, 239-275.
4. Stephens, G.L. Cloud feedbacks in the climate system: A critical review. *J. Clim.* **2005**, *18*, 237-273.
5. Spencer, R.W.; Braswell, W.D. On the diagnosis of radiative feedback in the presence of unknown radiative forcing. *J. Geophys. Res.* **2010**, *115*, D16109.
6. Jacob, D.J.; Avissar, R.; Bond, G.C.; Gaffin, S.; Kiehl, J.T.; Lean, J.L.; Lohmann, U.; Mann, M.E.; Pielke, R.A., Sr.; Ramanathan, V.; Russell, L.M. *Radiative Forcing of Climate Change: Expanding the Concept and Addressing Uncertainties*; The National Academies Press: Washington, DC, USA, 2005; Volume 208, p. 207. Available online: http://www.nap.edu/openbook.php?isbn=0309095069 (accessed on 9 July 2011).
7. Forster, P.M.; Gregory, J.M. The climate sensitivity and its components diagnosed from Earth radiation budget data. *J. Clim.* **2006**, *19*, 39-52.
8. Forster, P.M.; Taylor, K.E. Climate forcings and climate sensitivities diagnosed from coupled climate model integrations. *J. Clim.* **2006**, *19*, 6181-6194.
9. Dessler, A.E. A determination of the cloud feedback from climate variations over the past decade. *Science* **2010**, *330*, 1523-1527.
10. Rasmusson, E.M.; Carpenter, T.H. Variations in tropical sea surface temperature and surface wind fields associated with the Southern Oscillation. *Mon. Wea. Rev*. **1982**, *110*, 354-384.
11. Wielicki, B.A.; Barkstrom, B.R; Harrison, E.F.; Lee, R.B., III.; Smith, G.L.; Cooper, J.E. Clouds and the Earth's Radiant Energy System (CERES): An earth observing system experiment. *Bull. Amer. Meteor. Soc*. **1996**, *77*, 853-868.
12. Brohan, P.; Kennedy, J.J.; Harris, I.; Tett, S.F.B.; Jones, P.D. Uncertainty estimates in regional and global observed temperature changes: A new dataset from 1850. *J. Geophys. Res.* **2006**, *111*, D12106.
13. Spencer, R.W.; Braswell, W.D.; Christy, J.R.; Hnilo, J. Cloud and radiation budget changes associated with tropical intraseasonal oscillations. *Geophys. Res. Lett*. **2007**, *34*, L15707.
14. Lindzen, R.S.; Choi, Y.-S. On the observational determination of climate sensitivity and its implications. *Asia-Pacific J. Atmos. Sci*. **2011**, in press.