Observational Network Design

Venice, Italy, September 2009

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Based on my interpretation of a number of white papers prepared for the conference spanning a wide range of purposes. But primarly my own views directed at the generic issues.

By far the most difficult task is to define the goal(s). If more than one, must assign priorities.

Goals have to be quantitative (how well must x be known?) Only a few notable exceptions in the talks here provided an answer to the question: *How good does it have to be? Why? What is the present capability?* A few speakers discussed. Most did not.

Everything else is technical detail.

An observational system for regional mesoscale forecasting will be different than one for finding changing thermocline heat content changes, determining carbon uptake, or abyssal circulation shifts, or meridional enthalpy transports, or coastal circulation (see the various CWPs).

Technology constantly changes, as does scientific understanding. The limit of a design is probably about 10 years. But since the problems are open-ended, operators of these systems must be prepared to re-evaluate constantly. *Today's priorities may look little like that of 10-20 years from now, and sampling requirements may be completely different. An on-going, challenging scientific problem for which it is too easy to declare something "operational" and walk-away from it* (we have examples).

I will focus on climate—as being generic and challenging to our institutional structures. Some plausible postulates:

Climate is global and can only be understood on that basis. Time scales are completely open-ended---there is no climate problem that will be "solved" with a 10-year record, and it is truly bad science to claim otherwise. We have an inter-generational problem.

Observations will remain diverse---eclectic in type and sampling properties.

Knowledge embedded in theory must be combined with knowledge from observations.

The various state estimation systems around, with varying skill, demonstrate that we do know how to generate useful model/data combinations for a variety of purposes and with a variety of "costs". See the CWPs and other talks at this conference.

The existing systems are an eclectic mix of what individuals and groups have brought to the table. Unlikely to change. The ECCO list of data used suggests that no fundamental issue arises from observational technologies.

DATA TYPE	Source	Spatial Extent	Variable(s)	Duration	Number of Values
Altimetry: TOPEX/POSEIDON	PODAAC	Global equatorward of 66.5 degrees	height anomaly, temporal average	1993-2002 2003-2005	(4500/day) 3.0x10^{7}
Altimetry: Jason	PODAAC	Global equatorward of 66.5 degrees	height anomaly, temporal average	2002-2007	included above
Altimetry: Geosat-followon	US Navy, NOAA	Global, equatorward of 72 degrees	height anomaly	2001-2007	(4300/day) 2.6x10^{7}
Altimetry: ERS-1/2, ENVISAT	AVISO	Global, equatorward of 81.5 degrees	height anomaly	1992-2007	(3800/day) 2.2x10^{7}
Hydrographic climatology	Gouretski and Koltermann (2004)	global, 300m to seafloor	temperature, salinity	inhomogeneous average	(monthly) 1.7x10^{7}
Hydrographic climatology	World Ocean Atlas (2001), Conkright et al. (2002)	global to 300m	temperature, salinity	average seasonal cycle	included above
CTD synoptic section data	Various, including WOCE Hydro. Prog.	global, all seasons, to 3000m	temperature, salinity	1992-2005	(17000 profilx10s) 2.x10^{6}
XBTs	D. Behringer (NCEP)	global, but little So. Ocean	temperature	1992-2006	(470000 profiles) 1.2x10^{7}
ARGO Float profiles	IFREMER	global, above 2500m	temperature, salinity	1992-2007	(416000 profiles) 7x10^{6}
Sea Surface Temperature	Reynolds and Smith (1999)	global	temperature	1992-2007	(monthly) 8.0x10^{6}
Sea Surface Salinity	Etudes Climatiques de l'Oce'an Pacifique (ECOP)	tropical Pacific	salinity	1992-1999	(monthly) 5.5x10^{6}
TMI AMSRE	NASA/NOAA discoverearth.org	global	temperature	1998-2007	(daily) 2.1x10^{8}
Geoid (GRACE mission)	GRACE SM004-GRACE3 CLS/GFZ (H. M. Rio)	global	mean dynamic topography	NA	(1 deg) 5.8x10^{4}
Bottom Topography	Smith&Sandwell(1997)+ETOP05	Smith/Sandwell to 72.006, ETOP05 to 79.5	water depth	NA	(1 deg) 5.8x10^{4}
Toga-TAO, Pirata array	PMEL, NOAA	tropical Pacific	temperature, salinity	1992-2006	(daily) 2.2x10^{6}
SeaOS	Sea Mammal Research U. St. Andrews, Scotland	Southern ocean	temperature, salinity	2004-2007	(24590 profiles) 5.6x10^{5}
Rapid	BODC	Atlantic 26N	temperature, salinity	2004-2005	8.4x10^{5}
Florida Current transport	NOAA/AOML	Florida Straits	Transport	1992-2007	(1value/day) 5.8x10^{3}
FORCING:					
Windstress-scatterometer	PODAAC	global	stress	1992-2006	9x10^{8}

But what should we measure? How? How well? What geographical and depth distribution? For how long?

Physical oceanography and climate science have changed profoundly over the past 20 years: *We are still undergoing an extremely uncomfortable change from an academic science whose (mis)understandings and problems were of interest to a handful of specialists around the world, to an applied field in which headlines are made, governments can panic, etc. if, rightly or wrongly, particular results are announced with seeming authority.*

Casual and grossly incorrect uncertainty estimates hardly mattered 20 years ago. Today a proclamation that sea level is rising at 4mm/y rather than 2mm/y has gigantic societal implications.

The field must become much more quantitative and statements of certainty (or uncertainty) must be scrutinized at least as carefully as statements of nominal values.

Consider one simple, widely shared, problem:

Determine the uptake of heat by the ocean under global warming.

The direct greenhouse gas forcing from doubled CO_2 is believed to be about $1W/m^2$, and is thought to be the actual rate of the last 50 years. With feedbacks is perhaps $O(4W/m^2)$.

Most scientists would argue that a measurement accuracy of not less than $0.4W/m^2$ is *essential*. In 1980, the WCRP predecessor organization stated this as a goal. Little mention of numerical goals at this conference.

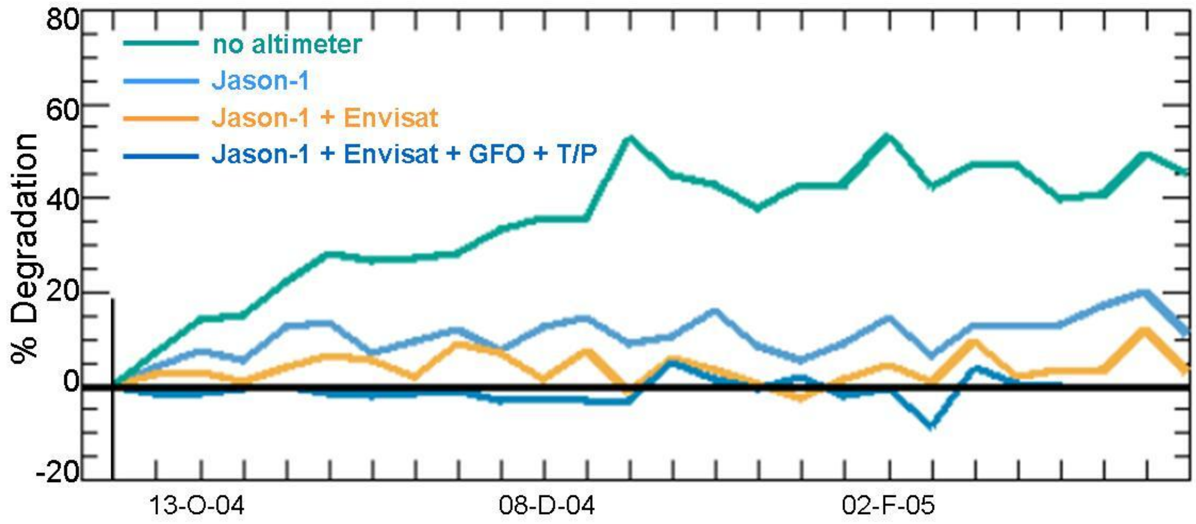
It is quite clear that *even the present coverage*, which is of course far better than it was 10 or 20 years ago, remains grossly inadequate to reach this accuracy.

A great deal is known about how to optimize for any given goal, and methods exist for multi-objective optimization. Nonetheless requires an ordering of priorities (the weighting).

Some of these methodologies are very expensive both in computer and human power. *But those expenses pale in comparison to the potential damages that can be incurred by climate change.*

Most common methodology is “cut and try”. (1) guess a design. (2) evaluate the result. (3) modify it and try again.

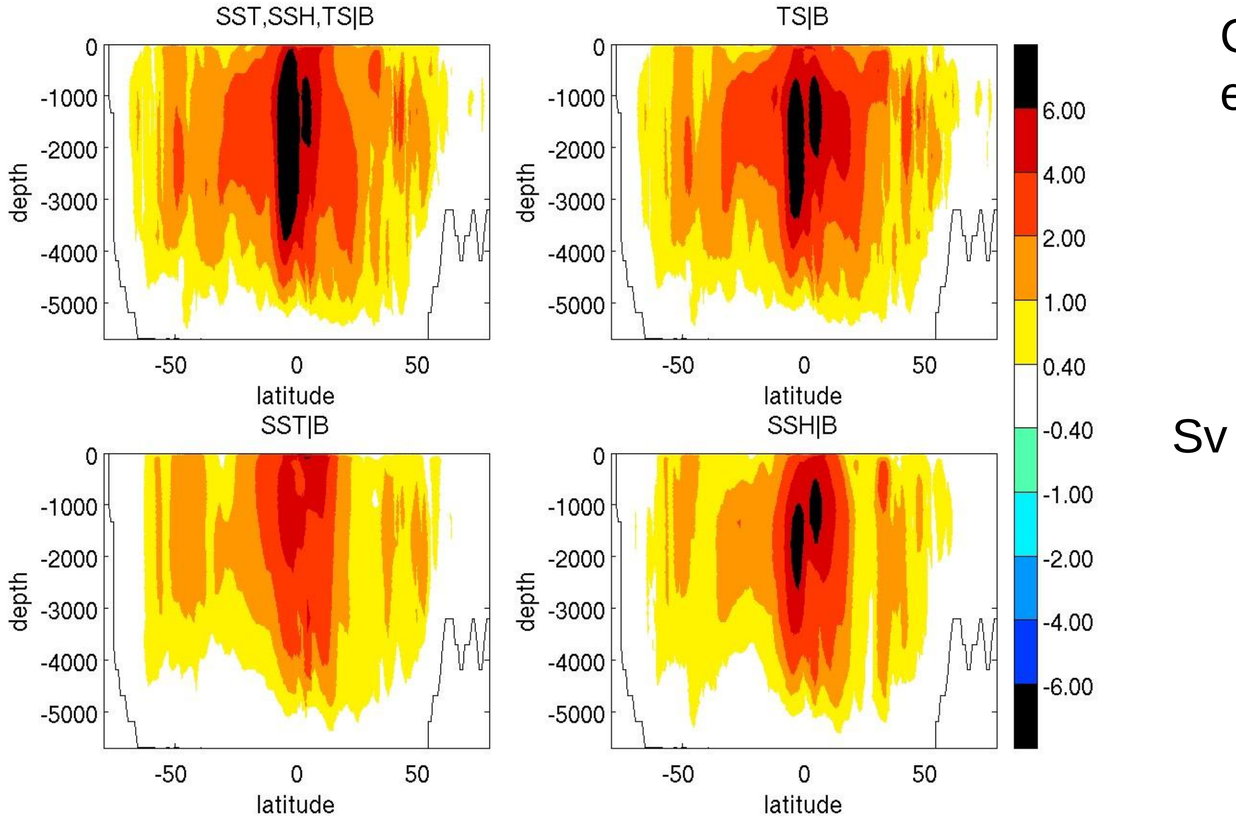
Advantage: Relatively straightforward, relying on existing state estimation schemes. Disadvantage: Result could be far from optimal.



Impact of altimeter numbers on forecast skills, numbering 0-4.

from P. Oke et al. white paper

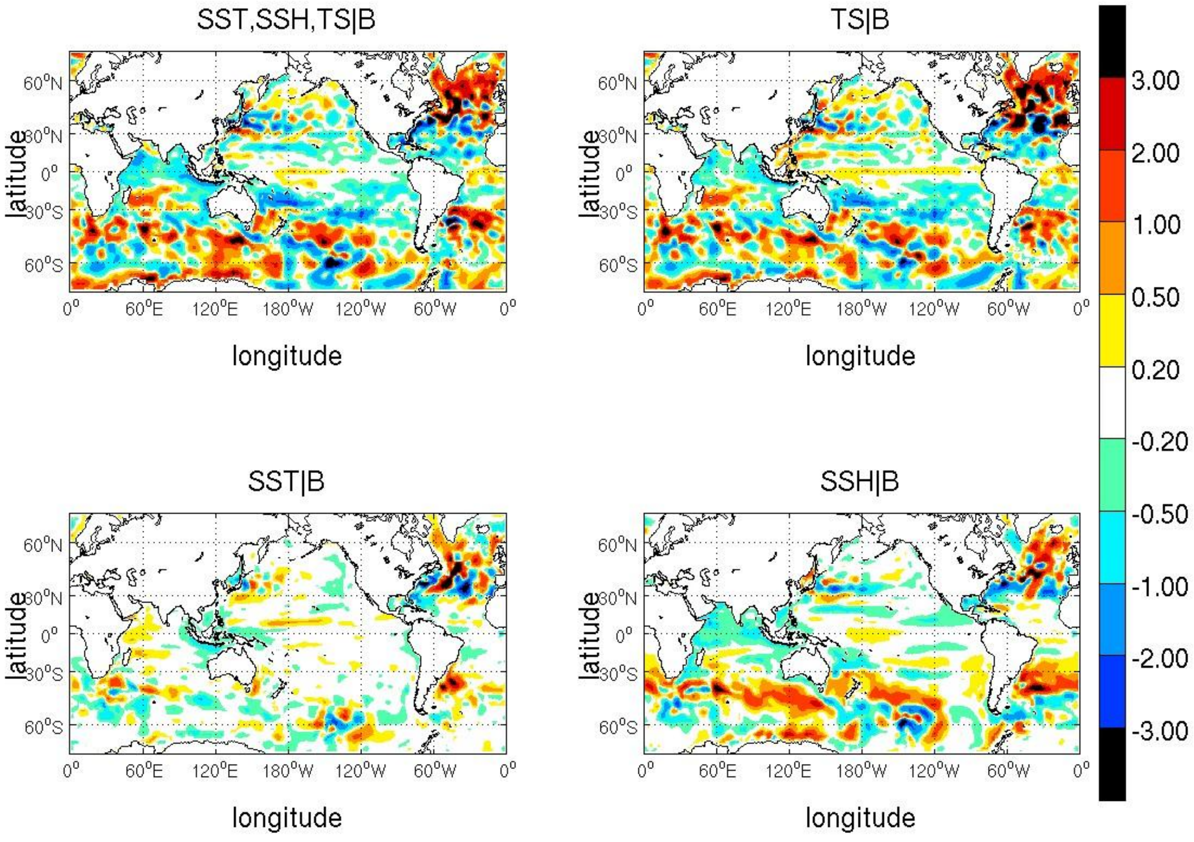
G. Forget 2009, from Heimbach et al. white paper.



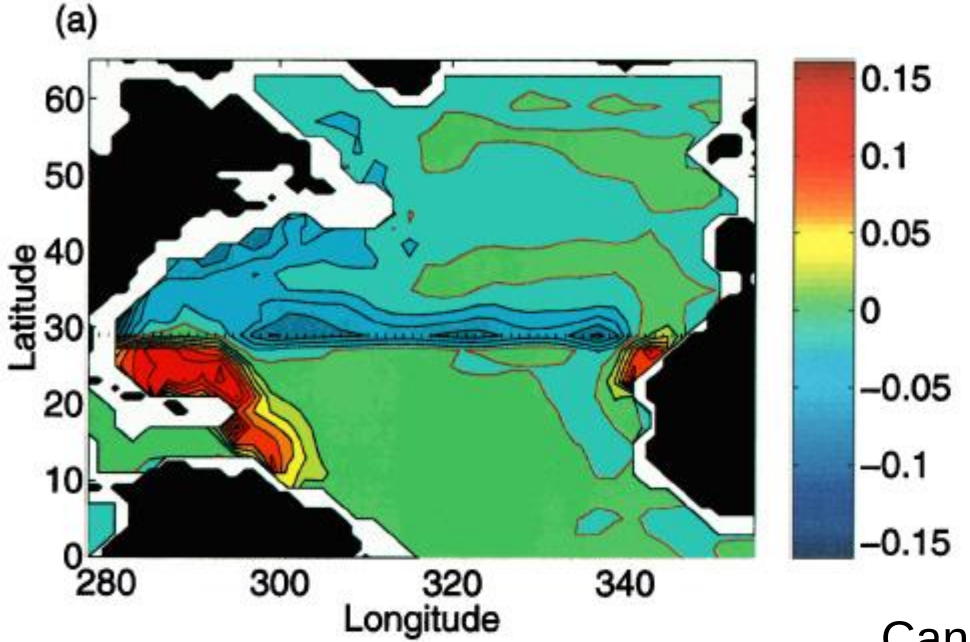
Variability of the meridional overturning as various data sets are added to a baseline of hydrographic climatology, NCEP/NCAR reanalysis, and the ECCO GCM. Evaluates what exists, or an arbitrary choice of what could exist.

Time-mean heat content change as different data sets are added to the baseline.

G. Forget, 2009 from Heimbach et al. CWP.

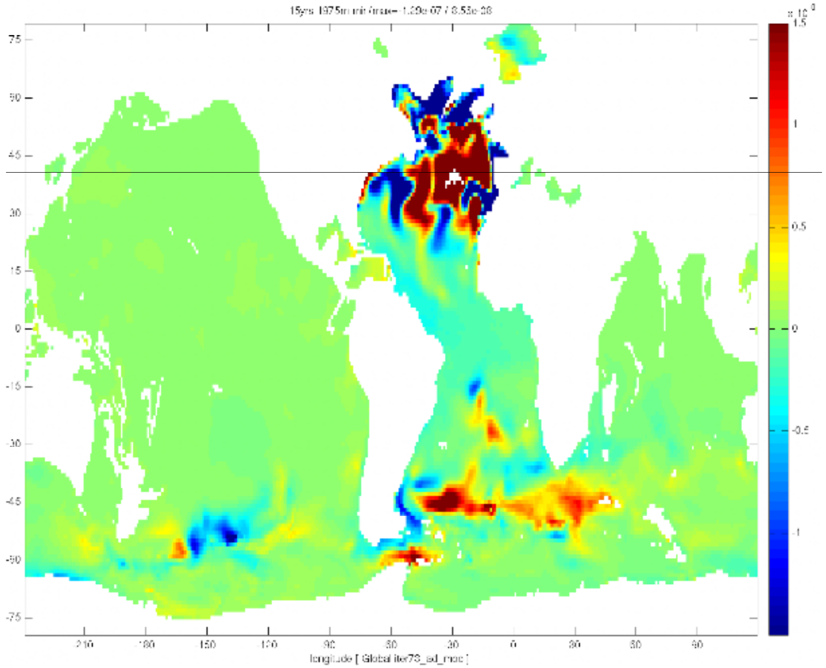


Beyond cut-and-try we can examine sensitivities to various data types e.g., by using adjoint models. Here sensitivity of annual mean enthalpy transport across 25N to January temperatures.



Marotzke et al., 1999

Can readily use to decide what to measure where (and when).



Sensitivities to 25N enthalpy transports to deep temperatures 15 years previously (P. Heimbach, et al. CWP).

What we see anywhere, happening today, is a consequence in part of changes that took place remotely long ago.   Any observational network design must reflect that uncomfortable fact. Small regional arrays, or chokepoint measurements, *can document that a change is occurring, but only a global, long-duration system can answer the question of why?, lead to understanding and conceivably, to predictive capability.*

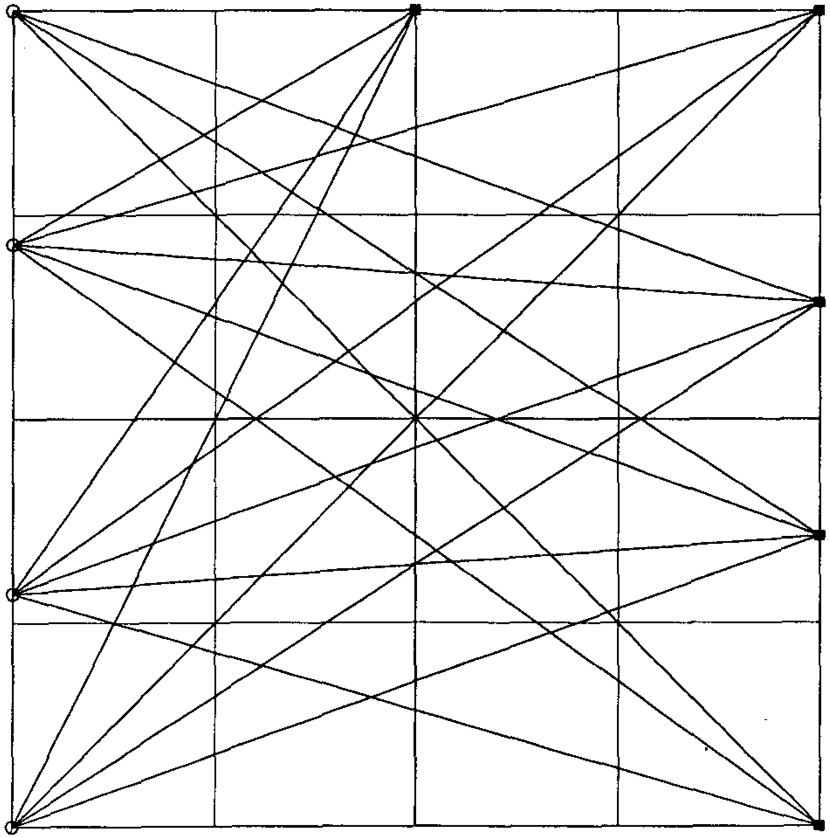


FIG. 1. The
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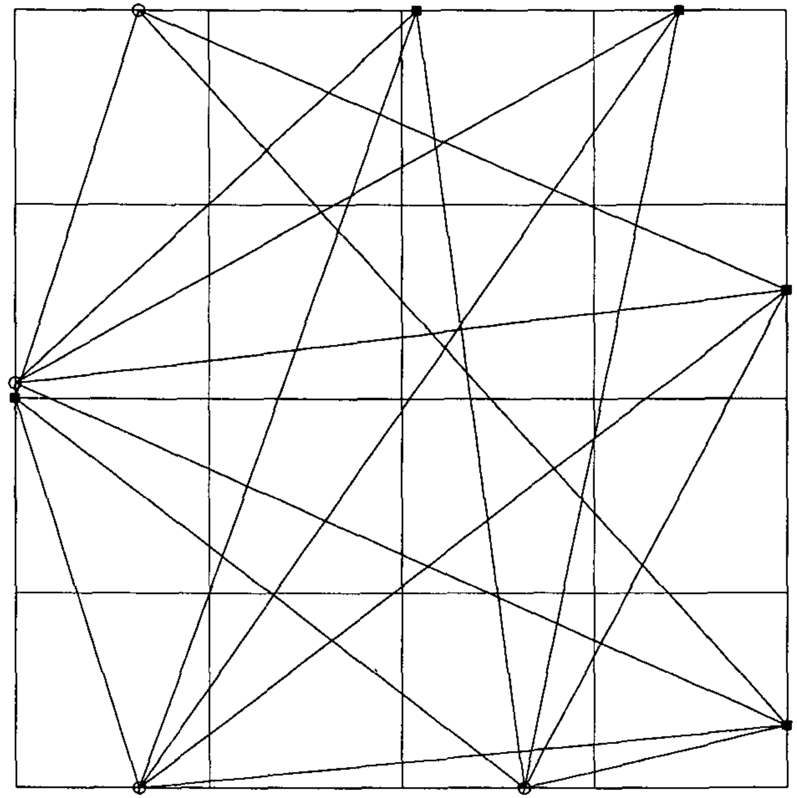


FIG. 3b. The array configuration associated with the final state of the simulated annealing program output displayed in Fig. 3a. All

Formal, true optimization methods exist. Here is a guessed design of an actual tomographic array (left). Optimized (simulated annealing) design on the right. N. Barth, JPO, 1990. Goal was mapping accuracy (arbitrary choice). We know how to do this, if we want, in far more complicated situations.

(1) What do you want to optimize for? (2) What is the resource mix? (3) What information can come from a model rather than the specific measurement?

Where are we headed?

With some exceptions, the age of exploration in the *physical* side of the problem is over (what is the nature of the variability in this region?).

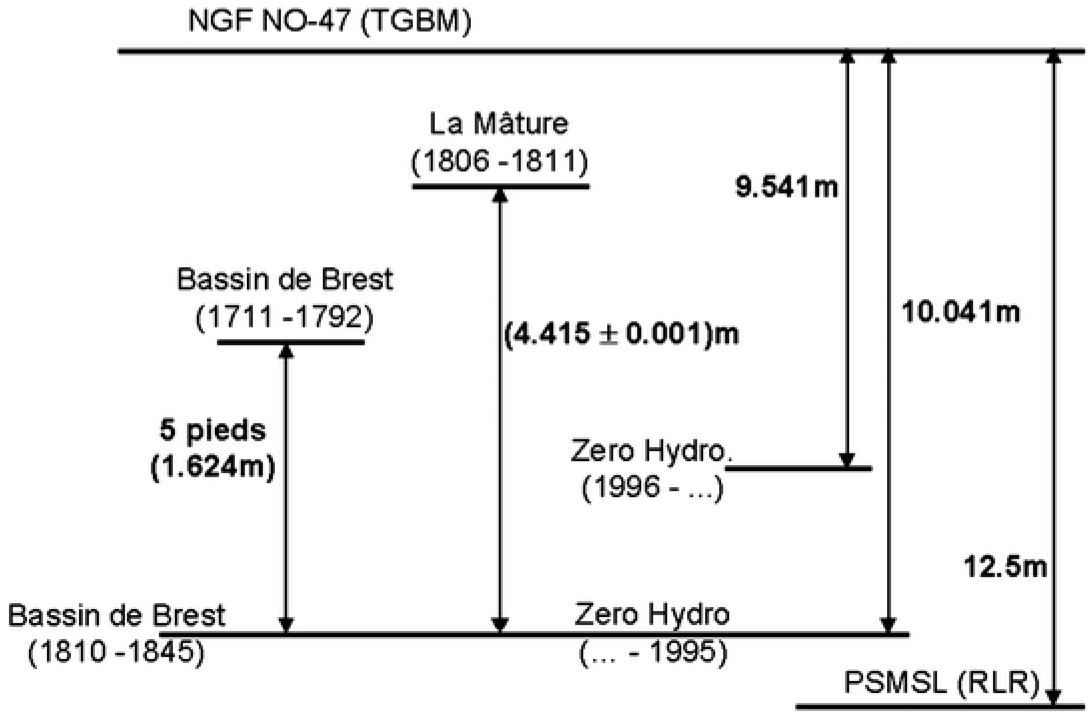
The present frontier is exploration in the time domain (what is the nature of the variability on time scales of e.g., 25 years and longer?) New physics enters as time scales expand, and the existing instrumental record is extremely short compared to any plausible estimate of climate time scales and memory.

For climate problems the major challenges are (1) definition of the needs, (2) maintenance of the system by scientists and engineers over decades to come. These are both very important, but difficult problems requiring a lot of hard work, poorly rewarded by society.

Consider the duration/maintenance problem.

We have essentially no long term measurements of the climate system in which fundamental issues of calibration and technology change are not present.

Estimated shifting datum of the Brest tide gauge.



Wöppelmann et al., 2006. A tour-de-force---but also a nighmare. Note change of units across the French revolution (and the foot (pied) and inch were not uniquely defined).

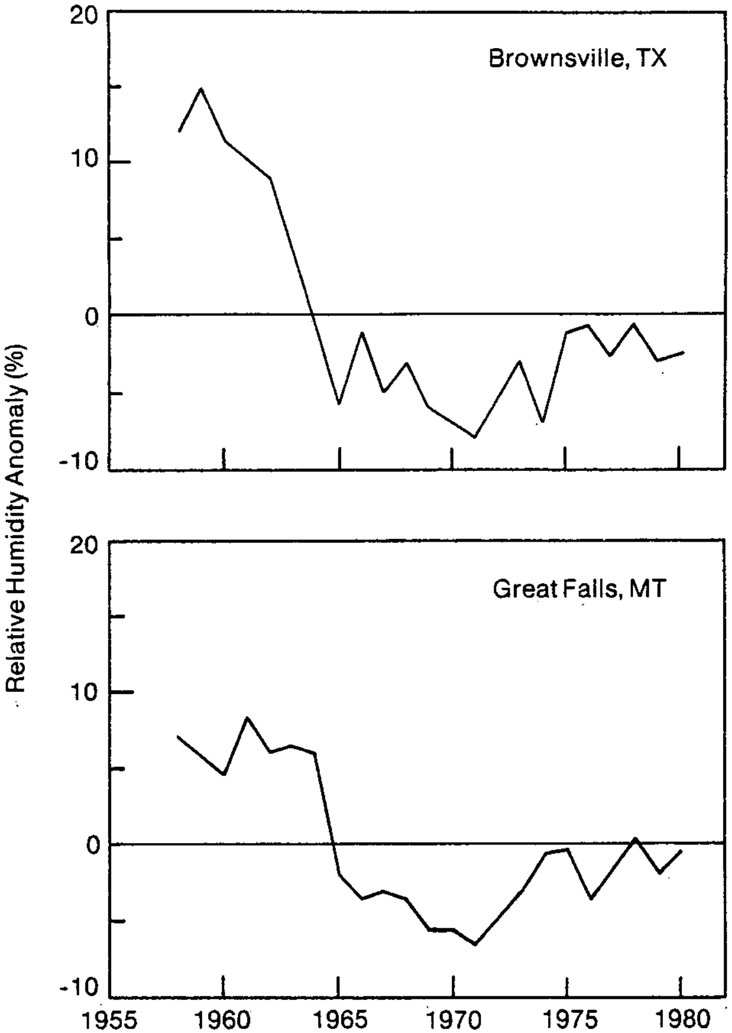


FIG. 5. Variation in annual average RH at the 500-mb level from 1957 to 1980, expressed as a deviation from the long-term mean at Brownsville, Texas, and Great Falls, Montana. The high RH values at the beginning of the record result from the practice of reporting observations with RH less than 20% as missing. (Adapted from Angell et al. 1984.)

Elliott and Gaffen, BAMS, 1992

From the radiosonde network

"RH [relative humidity] high values at the beginning…result from the practice of reporting values with RHO less than 20% as missing."

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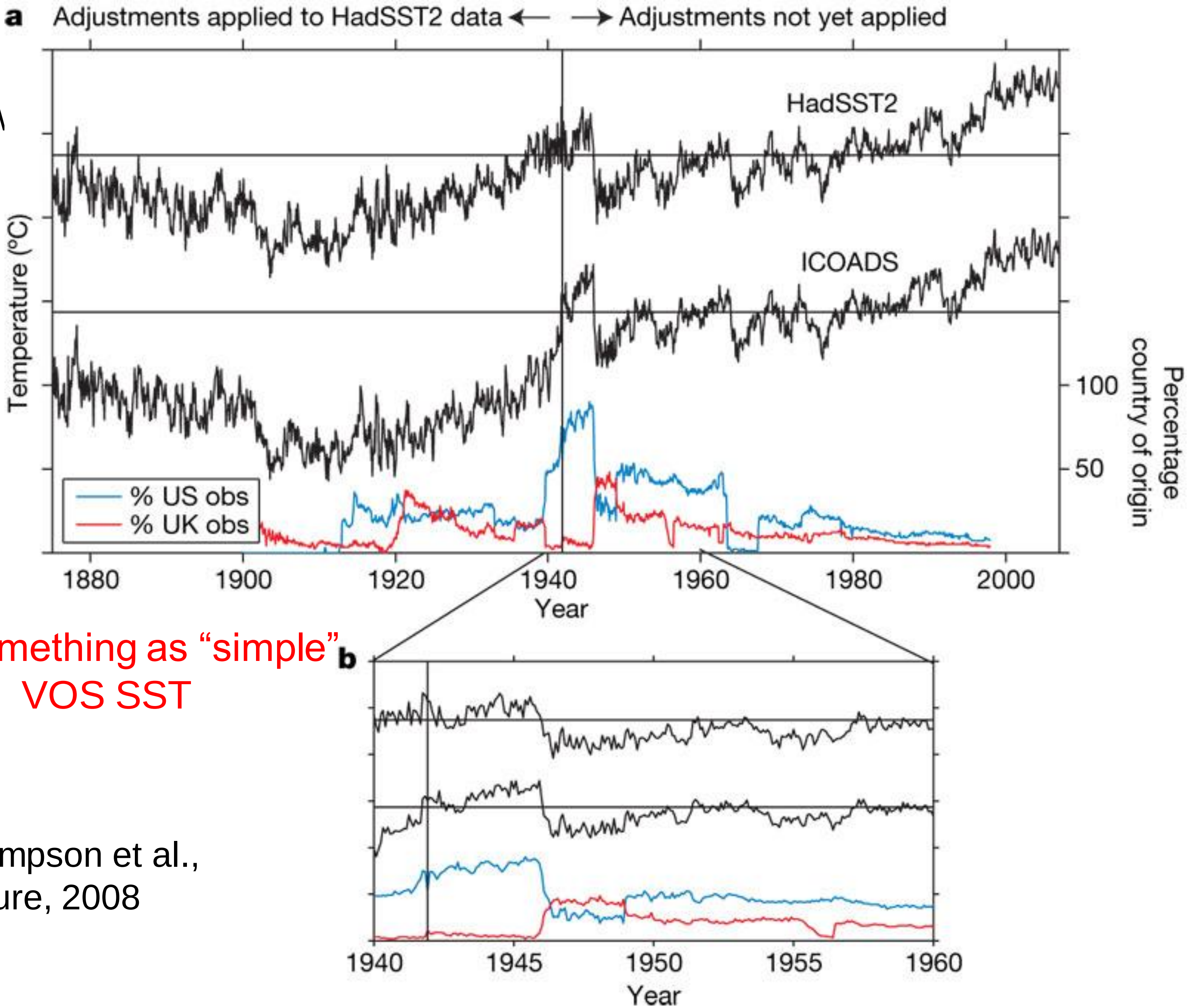
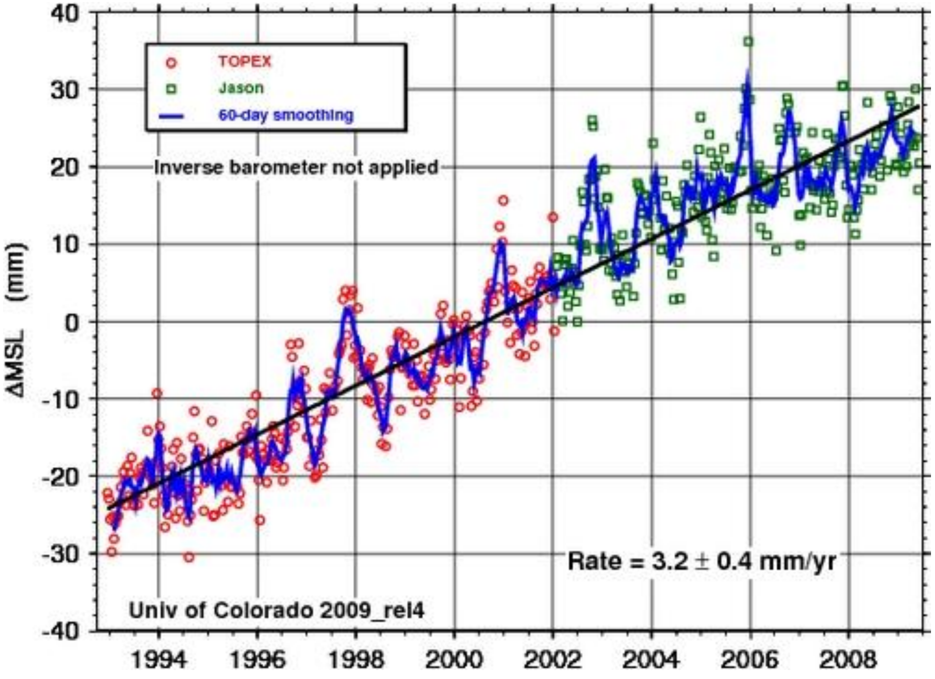


Something as “simple” as VOS SST

Thompson et al., Nature, 2008

Estimated near global sea level change from altimetry



Because of the societal interest, the need to be carefully quantitative is *much more necessary than it was when this was a purely academic subject.*

There are potential drifts in measurements (and corrections) for atmospheric water vapor, wave height, navigation (GPS) systems, seasonal coverage (sea ice)…. (Error estimate is purely formal.) A major difference in societal implications if the rate is 2mm/y versus 6 mm/y. If the system has drifts of 2 mm/y, is it sensible to tell the public we know the rates to a few tenths of millimeters/year?

The technology inevitably changes.
Scientific understanding changes. (Models change, and the data required to test them changes too.)

Calibration, decisions about substituting new technologies, or abandoning particular measurements, or starting new ones, *requires the highest levels of scientific and engineering insights* if the measurements are ultimately to be useful (often decades hence). Cost-effectiveness decisions can be very troublesome. How is this oversight and evolving strategy to be sustained and by whom? Who is in charge?

Some international authoritative body with a long time-horizon is required.

Some final remarks concerning mainly the generic problems of long-term measurements of almost any kind of problem.

Main issues concern “what”, “why”, and “how well”, and how to sustain observing systems through the evolving technology and understanding.

The technical knowledge of how to use most types of data, and how to optimize observational systems already exists. Costs may appear high, but they pale in comparison to the potential societal costs. We lack the human infrastructure for coping with this problem.

The construction of *quantitatively* useful observation systems to be sustained usefully for long periods is the major frontier for the science of climate change. It involves the most sophisticated of scientific and engineering understanding. The “payback” will accrue primarily to future generations and not to us.

A truly *inter-generational* problem for which we lack any helpful infrastructure. Perhaps a useful topic of discussion.

Thank you.