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**May 17, 2005**

***Is the "Hockey Stick" Debate Relevant to Policy?***

Posted to [Author: Pielke Jr., R.](#) | [Climate Change](#)

On several occasions I have alluded to the fact that I think that the debate over the so-called "hockey stick" temperature reconstruction is a distraction from the development and promulgation of climate policy. And the debate goes on (and on). We have written frequently about the core dynamic of the climate debate in which political opponents pick a scientific sandbox to fight in, with little connection to policy, and fight things out in a public manner under a pretense that the debate has significance beyond science. Over time this skeptic vs. hawk debate has taken place over a supposed CO<sub>2</sub> sink in North American, surface vs. satellite temperature trends, hurricanes and climate change and also the "hockey stick" (among other areas). Within science (including my own area of expertise) subject matter experts engage in vocal and at times nasty debates over knowledge. Such dust-ups are characteristic of the academic enterprise. But I'd assert that the battles over climate science go far beyond typical academic wrangling, are really proxy wars over something else. But what is that something else?

It might be political power, i.e., who gets to have a public voice on issues of climate change? Science is widely viewed as authoritative and legitimate, so everyone wants the imprimatur of science on their side. From this perspective the battle over climate science is a battle for standing, with little connection to the substantive connections of the scientific debates and practical decision making. In other words, the battle is over who gets to decide what action we take on climate change and not what actions we should take. This battle for political supremacy is most visible on the rival WWW sites that have sprung up to join the battle over the hockey stick (see [RealClimate](#) and [ClimateAudit](#).)

But there is also evidence to suggest that some people believe that the hockey stick is relevant to decision making. For example, in its most recent assessment the IPCC clearly considered the "hockey stick" to be relevant to policy, as it included it prominently in its 2001 "[Summary for Policy Makers](#)." The mission of the IPCC is to be "policy relevant" so presumably it is safe to say that any science that it presents (and particularly in its summary for policymakers) it considers it to be relevant for policy. But because the IPCC does not explicitly discuss policy, there is no way to glean from its reports why it thinks that the hockey stick is policy relevant. Further, the IPCC clearly does not need the hockey stick to assert its scientific authority and legitimacy, so there must be a very real presumption of policy relevance.

[There is also the possibility that the battle over the hockey stick has more to do with personalities and egos than politics or policy. However, while I am sure that personalities and egos play an important role in the evolution of this and the other proxy wars on climate, I do not think that they overshadow the politics and policy issues above.]

So here is my question to Prometheus readers: Is the debate over the "hockey stick" of any policy relevance whatsoever, other than as a battleground for political standing? That is to say, is the future resolution of the "hockey stick" debate at all relevant to understanding (a) our available scope of options on climate change, or (b) how we might evaluate those options? The views of those actually engaged in the "hockey stick" debate are solicited as well – why are you involved in this debate?

Posted on May 17, 2005 10:41 AM

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## Comments

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It's not relevant to policy. The hockey stick is an iconic construct intended to be knocked down for rhetorical gain.

D

Posted by: [Dano](#) at May 17, 2005 05:16 PM

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I've been a professional scientist for over thirty years and as far as I can tell climate change/global warming is policy masquerading as science. The policy is anti-development and redistribution of resources from the wealthier to the poorer nations. The "hockey stick" is a tool to push the policy. The models are another tool to the same end. It doesn't matter that they have no predictive power and don't agree with each other at any level of detail except to "forecast" catastrophic warming. The clear sign that this is policy and not science is the unwillingness to countenance any cause other than CO2 as a possible source for changing climate, e.g., the sun.

This is just a continuation of the (now discredited) doomsayers from the 1960's and 1970's that began with the Club of Rome, Paul Ehrlich, and the rest. The sad part about it is that

is impossible to be really scientific and do independent experiments that would put a stake through the heart of the whole nonsense as was done with cold fusion.

As for your questions, the hockey stick, even if true, does not establish the source of the warming. It can only claim to be a measure of temperature over the past thousand years. Without knowing the cause, there is no justifiable course of action.

Posted by: [Paul](#) at May 17, 2005 06:38 PM

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Paul- Thanks for these comments. I think that it is of critical importance to differentiate between the role that science plays in bringing a problem to the attention of policy makers from the role of science in policy and political processes in response to that problem. In the case of climate change it is very straightforward that the incessant emission of greenhouse gases should provide reason for concern. The scientific community has done a heroic job in identifying why we ought to place climate on our decision making agenda. Where my interests lie are what occurs after that, and the role of science in helping or compromising our ability to clearly define and respond to concerns about human influence on climate.

Posted by: [Roger Pielke, Jr.](#) at May 17, 2005 10:03 PM

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"In the case of climate change it is very straightforward that the incessant emission of greenhouse gases should provide reason for concern." Maybe, maybe not. There is good evidence that the CO2 level in the atmosphere was as much as 10x higher at earlier times in the earth's existence. Life did fine and we're here. If the scientific case can't be made of a direct and very strong connection between CO2 levels and genuine catastrophe then there is nothing to be concerned about and certainly nothing to be done.

As time goes by, the case has gotten weaker and weaker. For example Hansen has kept lowering his estimate of GW to the point where the temperature in 2100 might be as warm as it was during the Medieval Maximum. Roman times were even warmer. Is that something to worry about?

The whole issue has diverted attention from real environmental problems that can be addressed in practical ways: pollution; energy conservation; habitat destruction. I'm sure you can think of others. These are genuine problems that don't rely on doubtful models and uncertain science, they exist. Policy makers should not make policy based on "fake but accurate" science.

Posted by: [Paul](#) at May 18, 2005 10:00 AM

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Dano- Thanks for your comment. But here is the problem that needs to be addressed -- if the Hockey Stick is a rhetorical construct (which I tend to think is correct) then it must have also been a rhetorical construct when used by the IPCC in its TAR. But the IPCC must have thought that it was policy relevant. So this leaves two options (a) the IPCC was misusing the HS to promote some agenda (i.e., as rhetorical construct not really policy relevant, but politically useful), or (b) the IPCC was simply incorrect in its judgment of the "policy relevance" of the HS. Because no one openly addressed these policy or political issues at the time of the release of the IPCC's TAR, we are now saddled with an (apparently) unending debate over the HS, which lots of folks surely enjoy as great sport, but which contributes little to actual policy development.

Posted by: [Roger Pielke, Jr.](#) at May 18, 2005 10:13 AM

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Paul- Thanks -- now I am going to act like a professor and point you to one of my papers, where you'll probably find something to agree with and something to disagree with:

Sarewitz, D., R. A. Pielke, Jr., 2000: Breaking the Global-Warming Gridlock. The Atlantic Monthly, 286(1), 55-64.

[http://sciencepolicy.colorado.edu/admin/publication\\_files/resource-69-2000.18.pdf](http://sciencepolicy.colorado.edu/admin/publication_files/resource-69-2000.18.pdf)

Posted by: [Roger Pielke, Jr.](#) at May 18, 2005 10:16 AM

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Roger:

The term Hockey Stick (HS) is the rhetorical construct.

The data collected and analyzed that result in data points used by IPCC are empirical evidence that contribute to policy debate. The data points themselves that inform policy decisions are not rhetorical.

Those data can be called whatever. Bob. Lacrosse stick. Or not. It doesn't matter. Terms are irrelevant in the context you created here, Roger.

Subsequent empirical findings such as, say, by Moberg et al. have a different shape but similar conclusion, so the narrow confines of one particular rhetorical shape are immaterial to policy [decision-makers presumably cannot extract signal from noise and rely upon climatologists for that].

Semantically and sociologically, of course, rhetoric contributes to ways of knowing and influences decision-making. The creators of the HS icon know that well and have used it to muddle the debate, sowing public doubt and delaying decision-making. Lawmakers in a term-limit environment respond to influence and access.

Prometheus' attempts to assert that scientists remain above the fray are laudible for adherents of Platonic and Cartesian science. Alternatives are problematic and lead to acceptance of rhetorical dialogue from the likes of Paul Linsay above, so I appreciate your attempts to bookend the debate, sir.

Best,

D

Posted by: [Dano](#) at May 18, 2005 12:04 PM

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Dano- Actually I was referring to the data. Clearly the HS as a symbol has great political significance. But my initial query was focused on whether the HS (or, if you'd prefer, any of the other proxy reconstructions) have any meaning for policy, i.e., to understanding (a) our available scope of options on climate change, or (b) how we might evaluate those options. Thanks!

Posted by: [Roger Pielke, Jr.](#) at May 18, 2005 01:50 PM

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That the hockey stick is useful politically in no way minimizes its scientific importance. When the question of "detection of climate change" is being considered, the subject is

actually detecting climate change against the background of natural variability. One clear message of the flat shaft of the hockey stick is that natural variability has been relatively small for a long time before the mid-19th century or so.

Risbey et al. [Clim. Res. 16:61,2000] and Risbey and Kandlikar [Bull. Amer. Met. Soc. 83:1317, 2002] report many and diverse types of evidence which various experts find variously persuasive in answering the question of whether climate change has been detected. One of the four main evidences of detection reported by experts to Risbey and Kandlikar was the 20th-century global mean temperature trend. I stress that this means the contrast in recent global mean trend with the relatively flat trend portrayed throughout the hockey stick shaft period.

Loehle [Ecological Modelling 171:433, 2004; and Energy & Environment 15:1, 2004] discusses Risbey et al's work in this regard, and points out that multiproxy approaches like Mann et al's inherently tend to smear variability out of the record, even though the long term trend may be correct.

The question of whether climate change has or has not been detected, I think has clear policy relevance. Your (a) and (b) categories seem to relegate determination of need for policy change to someone other than "policymakers". I strenuously disagree, if that is the case. If the science is clear that a need for change is warranted, then scientists should be able to communicate this to the policymakers.

The hockey stick scandal has important implications in this regard. That IPCC used the Mann et al. graph in a promotional way is as clear a fact as can be. The policymakers should hear scientists' explanation for why the hockey stick was so embraced. It will help the policymakers understand what IPCC reports represent. One notion that I think policymakers will not find reassuring is that IPCC relies so much on initial peer review by journals like Science and Nature.

Refs:

\* Loehle, Craig 2004a: Climate change: detection and attribution of trends from long-term geologic data", Ecological Modelling 171:433, 2004

\* Loehle, Craig 2004b: Using Historical Climate Data to Evaluate Climate Trends: Issues of Statistical Inference. Energy & Environment 15(1):1-10, 2004

\* Risbey, James, Milind Kandlikar, and David Karoly 2000: A Protocol to Articulate and Quantify Uncertainties in Climate Change Detection and Attribution. Climate Research 16 (1), 61-78. pdf -- <http://www.maths.monash.edu.au/~ris/publications/daproto.pdf>

\* Risbey, James, and Milind Kandlikar 2002: Expert Assessment of Uncertainties in Detection and Attribution of Climate Change. Bull. Amer. Met. Soc., 83 (9), 1317-1326. pdf -- <http://www.maths.monash.edu.au/~ris/publications/daexpert.pdf>

Posted by: [Steve Schulin](#) at May 18, 2005 08:06 PM

The 'hockey stick' is undoubtedly important but it is only part of the debate and a distracting one at that.

The hockey stick supports a conclusion that the effect of man on global temperature is large. This seems to go further than your comment above that it is cause for concern. This comes from a conclusion that previous natural variability was small and, thus, current variability can all be attributed to man. This directly leads to conclusions about (b) how we evaluate options on climate change. (I do not see that anything can affect (a) the options available to us they always remain the same although our consideration of them varies).

Beyond that, the 'hockey stick' has a much more limited role to play - if man-made contributions to the atmosphere are solely accountable for the recent increase (the blade) then that leads to conclusions about the strength of responses we might generate if we reverse or reduce these and, thus, the efficacy of attempts to do so. But it does not illuminate the cost of doing so or the benefits from doing so - just the global temperature that might result.

The discussion about the costs and benefits is primarily an economic discussion. At best the hockey stick can inform one part of the evaluation - the effect of any policies to restrict CO2 output on global temperatures.

And now the distracting part of the hockey stick. The evaluation of costs and benefits is one aspect of the politics of climate change that seems to be poorly studied (but not poorly pronounced on). The discussion on climate change to date is primarily conducted by people who specialise in the physical sciences but not the social sciences.

One example of this is the contre temps between Castles/Henderson and the IPCC. Castles and Henderson suggested that the IPCC was straying outside its area of expertise in generating economic forecasts that were used as inputs to their climate models - and had made some errors in doing so. They have consequently called for greater input from statistical and economic agencies in formulating and evaluating policies on climate change.

To sum up, a scientific conclusion on the effect of man on global temperatures should not lead to any policy conclusions absent a thorough evaluation of the costs and benefits of doing so. (But the scientific conclusion will obviously inform that evaluation.) To do that economists or similar social scientists need to be involved in policy formulation to a much greater extent that seems to have been the case so far.

Posted by: [John S](#) at May 18, 2005 10:35 PM

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Thanks both Steve and John for your comments, and Steve for your pointers to the literature. Very useful stuff. I would suggest however that this discussion has yet to go beyond the political to considerations of practical action. (And to be clear, by politics I mean bargaining, negotiation and compromise. See also my earlier posts on honest broker vs. issue advocate.) The approach to climate change under Article 2 of the Climate Convention makes "detection and attribution" central to political debate over climate. I.e., Article 2 results in detection and attribution as being necessary conditions for action with action defined narrowly as Kyoto and its follow ons. If the HS is viewed as scientific proof of detection and/or attribution then it will obviously become a target around which climate hawks and skeptics will gather and engage in battle. I would characterize this more as being politically relevant to adoption of a particular policy, rather than being more broadly policy relevant (to policy makers) in terms of (a) our available scope of options on climate change, or (b) how we might evaluate those options? As the world approaches the end of the Kyoto time frame. I'd argue that my questions (a) or (b) become much more important than Kyoto per se. For more on these dynamics see this paper:

Pielke, Jr., R. A., 2005 (in press). Misdefining Climate Change: Consequences for Science

and Action, Environmental Science and Policy.

[http://sciencepolicy.colorado.edu/admin/publication\\_files/resourse-479-2004.10.pdf](http://sciencepolicy.colorado.edu/admin/publication_files/resourse-479-2004.10.pdf)

Posted by: [Roger Pielke, Jr.](#) at May 19, 2005 06:43 AM

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Roger:

Ah: does the visual have intrinsic value as a metric, thus having value for decision-making? Got it. The next step being how do we make data compelling for decision-makers.

I say data alone is insufficient to compel those without expertise. Non-contextualized data is like asking whether a particular phone number tells you something about the person answering the phone. Even bell curves need labels on the axes, but there is usually accompanying explanatory text to contextualize the graphic.

I see you were trying to bookend the debate to frame your (before now unannounced) paper. Certainly, as John states above (and folks like Lindzen allude to when stating we can't forecast in the future) the socioeconomic component is understudied. Hence the need for adaptive management and scenario analysis. Whether humans can utilize this approach is, as you infer from your paper above, rather problematic.

We simply don't manage ourselves well enough to plan for this situation. "Herding cats" comes to mind.

Best,

D

Posted by: [Dano](#) at May 19, 2005 10:43 AM

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There is a rather long response to Castles and Henderson which is rather well known.

<http://www.ingentaconnect.com/content/mscp/ene/2003/00000014/F0020002/art00005>

The abstract rather captures the tone:

"The response is:

- The IPCC SRES reviews existing literature, most of which is MER based, including that from the World Bank, IEA and USDoE.
- Scenarios of GDP growth are typically expressed as MER (the preferred measure for GDP growth, as opposed to PPP which is a preferred measure for assessing differences in economic welfare).
- IPCC scenarios did include PPP-based scenarios, which Mr. Castles and Mr. Henderson have conveniently ignored.
- Contrary to what Mr. Castles and Mr. Henderson claim, IPCC scenarios are consistent with historical data, including that from 1990 to 2000, and with the most recent near term (up to 2020) projections of other agencies.
- Long-term emissions are based on multiple, interdependent driving forces, and not just economic growth. Mr. Castles and Mr. Henderson need to look beyond GDP.

- The IPCC scenarios provided information for only four world regions, and not for specific countries. Mr. Castles' and Mr. Henderson's critique is not of IPCC scenarios but of ongoing unpublished work in progress that is not part of SRES.

We therefore show that Mr. Castles and Mr. Henderson have focused on constructing a "problem" that does not exist. SRES scenarios are sound and the IPCC has responded seriously and conscientiously."

Posted by: [Eli Rabett](#) at May 22, 2005 08:53 PM

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Eli,

I am aware of that response. The essence of the comment remains. That paper you refer to and subsequent statements from the IPCC suggest that the IPCC don't really know what they are talking about with respect to PPPs and MERs.

A potted version of the counter can be found in the Economist [http://www.economist.com/printedition/PrinterFriendly.cfm?Story\\_ID=2189568](http://www.economist.com/printedition/PrinterFriendly.cfm?Story_ID=2189568)

I have also seen a paper by some people from Statistics Norway that suggests that the IPCC made an error in using MER but that, because of an offsetting assumption the net result is that the emissions scenarios are little affected ([http://stephenschneider.stanford.edu/Publications/PDF\\_Papers/HoltmarkAlfsen.sub170204.pdf](http://stephenschneider.stanford.edu/Publications/PDF_Papers/HoltmarkAlfsen.sub170204.pdf).) I can't comment on this paper in more depth but the relevant point is still that the IPCC doesn't understand the difference between PPP exchange rates and MERs. That is, they are not experts in statistics and economics and that if they are 'correct', it is by chance not design.

Posted by: [John S](#) at May 23, 2005 01:59 AM

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John and Eli- FYI, we discussed MER vs. PPP and the Norway study a while back here:

[http://sciencepolicy.colorado.edu/prometheus/archives/risk\\_uncertainty/000088reducing\\_uncertainty.html](http://sciencepolicy.colorado.edu/prometheus/archives/risk_uncertainty/000088reducing_uncertainty.html)

Posted by: [Roger Pielke, Jr.](#) at May 23, 2005 06:14 AM

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The useful thing about the IPCC response is that it contains claims which directly contradict the claims of Castle and Henderson, thus they should be testable. Neither the Economist, nor Prof. Pielke deal with these. For example:

"IPCC scenarios did include PPP-based scenarios, which Mr. Castles and Mr. Henderson have conveniently ignored."

If we look at the detailed part of the IPCC response (no cost at <http://crga.atmos.uiuc.edu/publications/ipcc-sres-revisited.pdf>

pp 5 near the top

"Both measurements of GDP are provided in the underlying report and in particular in Appendix VII for all MESSAGE scenarios (see also in SRES, 2000: Figure 3-1; Section 3.3.1, Box 3-1, Figure 3-12, and Table 4-19). The writing team decided not to report both measures of GDP throughout the SRES report in order not to generate confusion about



growth rates and other scenario indicators that include GDP especially in comparison with the literature that almost exclusively reports GDP in MER, and which the SRES writing team had the mandate to review."

Holttsmark and Alfsen have an interesting comment on PPP vs MER, basically that the latter overstates economic growth, but that at the same time (and for the same reasons) it would overstate the growth of emissions.

[http://stephenschneider.stanford.edu/Publications/PDF\\_Papers/HolttsmarkAlfsen.sub170204.pdf](http://stephenschneider.stanford.edu/Publications/PDF_Papers/HolttsmarkAlfsen.sub170204.pdf)

Posted by: [Eli Rabett](#) at May 24, 2005 08:41 PM

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The metaphoric term "hockey stick" has multiple meanings, at least two:

(a) climate reconstructions of the past 1000 years or so in general;

(b) the reconstructions by Mann et al. (1998 in "Nature" and 1999 in "Geophysical Research Letters").

The confusion between the two subjects might be inevitable during the preparation and immediately after the publication of the IPCC WG1 2001 report, because then Mann et al.'s seemed to be the only well-documented reconstruction of global mean temperature.

But, since then results of many more studies has appeared, and debates on the two things should be distinguished now.

It seems that some people intentionally use the term "hockey stick" to perpetuate the confusion and to deflect public attention from the reconstructions other than those of Mann et al.

I do not think that Dr. Pielke had such intention, and from the context I can guess that he meant (a). But, as far as I know, the most well-known debate was about validity of (b).

Thus I think that the unqualified use of the term "hockey stick debate" has an unintended effect to prolong the confusion.

[Disclosure:

As one of translators of Spencer Weart's "Discovery of Global Warming" which includes the graph of Mann et al. (1999) as one of the few graphs in the book with the word "hockey stick curve" in the caption, I myself might have contributed to propagate the confusion in Japan. Though the book was published in 2003 (and in Japanese in 2005), it contains a perspective as viewed in 2001, and it was difficult for me to add an appropriate note as the translator. That was one of the reasons I began watching both Prometheus and RealClimate.]

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I think that the answer to the Dr. Pielke's main question is dependent on another issue about how to frame climate policy which he often emphasizes (typically, in the article here titled "What is climate change?" on 22 December 2004).

As long as we base on the qualified definition of "Climate Change" adopted by UNFCCC, and as long as not all stakeholders are confident about projection of future climate based on theories and numerical models, we cannot avoid the path of detection and

attribution. Then, the question what the climate was like in the past millenium and how well we know it comes central, even though it still seems to be one of weakest parts of climate science. As long as UNFCCC is as it is, it seems inevitable for some people who has opinions about climate policy to say something about climate reconstruction.

Even without such external forcing, scientists will continue scientific debates about validity of one study or another, but they will be detached from policy discussion.

But, on the other hand, it is difficult imagine how to frame climate policy instead of the way that UNFCCC currently adopts.

One direction is, as Dr. Pielke wrote in "Adaptation and Climate" on 2 March 2005, focus on adaptation or resilience.

No matter what brings them, climate change and variation will certainly occur some time, and it is better for the humans to be less vulnerable to them.

Another thing is, as he discusses recently in "Cart or Horse?" on 19 May 2005, the issue of "mitigation of excessive greenhouse warming" may be subordinate to energy policy.

I agree with the general direction of the suggestion, though I am not sure about particular things.

(I understand that the discussion there is mainly oriented to the national policy of the United States.

As a foreigner I do not want to participate in the discussion itself but to international implications of it.)

I do not mean that climate change due to greenhouse gases is irrelevant. But I mean that it can be considered less grave than year-to-year variation of climate on one hand, and than the issue of limits of energy resources (as a physical scientist I would like to call it "low-entropy resources" but it may be too pedantic.) on the other hand.

I do not think that any "climate policy" concerning anthropogenic greenhouse effect is viable without simultaneously considering so-called energy policy.

But, as I guess, international agreement about energy policy is much more difficult than what has been done about climate policy, even though the result of the latter until now may be tiny.

I think that we need to restrict growth of energy needs, and that it implies de-construction of the concepts of "economic growth" and "development". (We must distinguish "good developments" and "bad developments" from a new viewpoint, I mean.)

Also, if we include nuclear power as an option, the issue of proliferation of nuclear weapons will enter together, and, in some sense, all the military issues will also be connected.

I am afraid that to frame the issue as primarily energy policy (internationally, I mean) will be equivalent to postpone any effective decision practically forever.

On the other hand, framing climate policy stand-alone seems futile. It seems that we have a real dilemma.

Kooiti Masuda

In Yokohama (sometimes in Fujisawa), Japan

Posted by: [Kooiti Masuda](#) at May 24, 2005 09:12 PM

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"John and Eli- FYI, we discussed MER vs. PPP and the Norway study a while back here: ..."

MER vs PPP is a lot like the "hockey stick"...a bunch of argument that is basically irrelevant.

The IPCC and virtually the entire economics profession are so far wrong in their projections of economic growth in the 21st century, that the differences between MER and PPP analyses pale in comparison.

The IPCC TAR has a median projected world GDP per capita in 2100 of approximately \$45,000, and a maximum of \$129,000 (in 1990 dollars). The actual value will be approximately \$8,000,000 (in year 1990 dollars):

[http://markbahner.typepad.com/random\\_thoughts/2004/09/second\\_thoughts.html](http://markbahner.typepad.com/random_thoughts/2004/09/second_thoughts.html)

[http://markbahner.typepad.com/random\\_thoughts/2004/10/3rd\\_thoughts\\_on.html](http://markbahner.typepad.com/random_thoughts/2004/10/3rd_thoughts_on.html)

<http://www.longbets.org/194>

Since the IPCC's projected GDP per capita in 2100 is off by approximately a factor of 200, it's not worth spending much time arguing PPP versus MER.

Posted by: [Mark Bahner](#) at June 5, 2005 08: 11 PM

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