Dear Garth:

Thanks for passing on Crichton's interesting paper. He raises a number of important issues that merit careful and critical examination. I've mulled it over for a few hours, and I thought I would pass on some random thoughts.

First, Crichton is a clever chap and a forceful writer. This implies that one should burrow through the upholstery of clever writing to identify the underlying rationale. To jump to my bottom line, I find that his argument is based on a naïve, unrealistic, and most likely irrelevant definition of "science," and a conception of the relationship between science and politics that is divorced from reality.

Crichton begins his argument by contrasting an ideal of international, dispassionate, altruistic science, with a depraved reality of science seduced by politics and publicity – a science that invents demons to feed the hungers of politics. He cites a number of examples of varying relevance and validity:

SETI. Here, he sets up a strawman in the famous equation, and declares that since none of the terms can be rigorously quantified, the equation is nonsense, and therefore the notion of extra-terrestrial life is nonsense. Is he saying that any probability that cannot be quantified must be taken as zero? Is he aware that the Hubble telescope has revealed about 150 planets in our general neighborhood of our galaxy among the billions and billons (to quote Sagan) of galaxies we can see? The probability that one planet somewhere developed a civilization that decided for some reason to send out a radio beacon at this instant in galactic time is certainly extremely and unquantifiably small. It is not zero. SETI may be an unwise investment of resources, but the conclusion that it is complete nonsense just does not flow from his argument.

Nuclear winter. Is it true that <u>none</u> of the variables can be determined? Given a description of the amount and nature of the aerosol to be injected into the atmosphere, my impression is that calculations of its radiative implications are not completely outlandish. Crichton's history of the nuclear winter flap is incomplete. After TTAPS, the NAS put together a review group, but botched it – they had too many TTAPS authors on the group, and they assigned an under-employed and inexpert staffer to it (rather than me). The first version of the report was lousy, in essence just echoing the TTAPS paper. I got Ramanathan to review it, and after almost a year's revision it came out with conclusions a lot less apocalyptic than the original. The problem <u>was</u> still somewhat overblown, but it is nevertheless a real issue of modest magnitude. After all, El Chichon and the Kuwait oil fires <u>did</u> produce measurable climatic effects.

In the course of all this, he waxes eloquent about "consensus science," which is in his view uniformly bad. I think he's in a terrible muddle here. First, any conclusion that is to any degree drawn from observational data must depend to some extent on a consensus on the data and its interpretation. The published values of the fundamental physical constants – and, for that matter, the distance to the sun -- represent a consensus drawn from numerous sources. Much of the practice of medicine is based on hopefully

thoughtful consensus on diagnoses and remedies drawn from clinical experience, usually in the absence of a deep understanding of the underlying biochemistry and physics. If he means something like, "Conclusions that are accepted as valid simply because a lot of people say they are," then one cannot quarrel with his distaste. Samuel Johnson made essentially that argument in favor of religion – since so many great men had believed it, we should also. The argument that consensus is right simply because it is consensus was nonsense then and is nonsense now.

But Crichton has a second line of argument embedded here: Consensus has been wrong on a number of occasions in the past. It only takes one investigator to be right. Therefore, consensus is wrong now. Granted, one contrary opinion that is right trumps a consensus that is wrong. But which opinion is right? I don't see how stating this truism helps his argument, or helps the rest of us in any practical way. There's a consensus that fluoride in drinking water reduces tooth decay, and a few dissenters who see it as a public health hazard. Should we give up putting fluorides in drinking water?

Crichton does, however, have a perfectly valid point that struggles to emerge from his polemic: The validity and utility of a piece of "science" depends on its intrinsic quality, not on the number of its adherents.

Now he moves on to set a standard of intrinsic quality. "…(I)t is so important for the future of science that the line between what science can say with certainty, and what it cannot, be drawn clearly-and defended." Once we abandon strict adherence to what science tells us, all is lost, he avers. This sounds great, but in the end seems hopelessly simplistic and naïve to me. It sounds like something a medically trained chap who has assimilated the jargons of various sciences well enough to write fiction about them, but never really worked in real science, would say – someone like Crichton, in fact. Now, mathematics can tell us some things with absolute certainty: There are infinitely many prime numbers; some equations have no integer solutions; etc. But other branches of science just invent successive generations of fables that accommodate the current consensus on the facts of the real world, are reasonably consistent with each other, and make reasonably decent predictions. Neither Newton's, Maxwell's, nor the Navier-Stokes equations account for quantum physics – nor do they invalidate it. They're all tools, "models" if you will, of reality that are useful in dealing with various aspects of the real world. Relativity overlays Newtonian physics. Both are "true" and useful within their domains of relevance. In that sense, science is an evolving social and intellectual construct, constrained by nature but not unambiguously instructed by her.

"Eureka" insights by individual geniuses constitute but a tiny fraction of the work of science. Today's science is more and more a community endeavor. As evidenced by the pages of *Science* and *Nature*, science increasingly deals with exceedingly complex objects through iterative processes of disaggregation and synthesis. After the "Eureka" discovery of DNA's structure, biology has been one long slog. A workable understanding of the ENSO has emerged only after decades of work by the atmospheric and oceanic tribes. In campaigns such as these, immutable truths that "science can say

with certainty" are few and far between. But I would argue that the partial, perhaps ephemeral, insights that science can provide are still useful.

Thus, Crichton's criterion of clearly defined certainty and uncertainty is unrealistic, unattainable in any practical sense, and indeed utter nonsense when one thinks about it seriously. The real problem is not how to achieve certainty, but rather how to deal rationally with inescapable uncertainty.

Finally, Crichton zeroes in on the climate issue. He trots out some old chestnuts. For example, weather forecasts are no good, so climate projections can't be any good either. In fact, weather forecasts are indeed pretty good for a few days (in my latitude, anyway), and they decay after a week or so for quite well known fundamental reasons elucidated by Lorenz. Climate is a different animal. How do we know? Well, the earth itself handles weather and climate quite differently. Knowing the weather, i.e. short-term variability, of this December tells us nothing whatever about the weather of next December. However, knowledge of a handful of Decembers tells us a lot about the climate – the general character – of Decembers. Climate is strongly determined by boundary conditions, and independent of the chaotic behavior of transitory circulation systems – so much so that formulations such as the famous Paltridgean entropy hypothesis that contain no weather at all do a pretty good job of depicting global climate. Moreover, we have encouraging indications that climate can be predicted: What we know about the atmospheres and physical parameters of Mars and Venus squares very well with their observed climates. Moreover, the earth system – the best climate model of all – solves the problem of predicting the transition of climate from December to July every year.

Crichton then speaks of the "now-familiar pattern" of glossed-over uncertainties, unseemly rush for overarching policy, and searches for grants to support patrons' desired policy. [And he criticizes <u>science</u> for inventing "demons"?] Who the hell is rushing to policy? Who wants "demons"? Certainly not the "patrons" of science. Most science is funded by governments, and the last thing they want is to disturb the status quo. Are scientists driven then by a lust for grants? Well, one hotbed of climate work has been GFDL. They're solely government-funded, and Joe Smagorinsky told me that they didn't get a nickel on the basis of climate research – their budget was justified solely in terms of weather forecasting. Joe could just get away with diverting money into what interested him!

If you want to look into a very real connection between science and moneyed patrons, check out some of the "skeptics". Read *The Heat is On* by Ross Gelbspan. If anybody is luxuriously dining out on the climate issue, it's Fred Singer. Were it not for his conversion to climate-skeptic guru in his dotage, he would be shopping for out-of-date groceries at Safeway rather than basking in fuel-industry contracts. Nobody outside of Charlottesville would ever have heard of Pat Michaels (despite the fact that he's a clever chap) if he had not transformed himself into a professional climate change skeptic fueled by several hundreds of thousands of dollars of fuel industry money. Call it *ad hominem* if your will, but <u>these</u> guys are the *hominem* lusting after patrons' big bucks.

On a more substantive side, I staffed a NAS study that dealt with a couple of the skeptics then active, Reggie Newell and Sherwood Idso (the former very reputable; the latter more than a little nutty), and I can assure you that their arguments were studied and assessed very conscientiously. I've also been in at least two meetings at which Dick Lindzen has been very thoroughly dissected by a roomful of very competent people. Skepticism and argument are the life blood of science. Science is a rough contact sport, and nobody should engage in it if he can't put up with the bruises. One hears a lot of whining from the professional "skeptics," but my impression is that in fact they often receive a wider and fairer hearing than their arguments actually merit.

"Group think" is too prevalent, money doesn't mix well with the search for truth, and politics doesn't mesh well with the inherently uncertain nature of scientific knowledge. But I just can't buy Crichton's simplistic model of avaricious scientific whores turning tricks for big shots with big bucks.

Finally, Crichton moves on to another familiar target – "computer models." It's unclear to me whether he is worried about models in general, or just models that are implemented on a computer. If his concern is the former, then he has a hard life ahead of him, for virtually every statement one can make about the world is based on some sort of "model". We buy life insurance based on actuarial models. We set interest rates based on economic models. Test pilots take off in aircraft designed using quite elaborate models. If his point is that one should never use models for understanding phenomena, estimating future behavior, or making decisions, then he's taking a rather extreme position and would probably be happier living in some alternative world.

Crichton is probably intending to say something less unreasonable. His point may be that large, complex models embodying a multitude of diverse and interacting components and processes, none of which can be unequivocally quantified, can exhibit bizarre behavior when they stray outside the domain within which they have been calibrated, and thus have limitations as tools for making serious decisions. Now this is a perfectly reasonable, non-polemical point. It's well known, for example, that econometric models become less and less reliable as they become more and more complex. Complex models implemented on a supercomputer become gigantic "black boxes", producing results in a manner as opaque as the Delphic Oracle. So if someone came to you with some sort of complex piece of machinery with a few hundred thousand interlinked parts, admitted that he really didn't know the precise dimensions or characteristics of them all, nor all the possible means by which they might interact or change over time, and then proposed to bet his life on a computer model prediction of its detailed state a hundred years hence – you would conclude that he's crazy. This seems to be the "straw man" that Crichton is depicting, and of course he has no trouble at all in blowing it down.

But, the current generation of climate models does not entirely fit that model, and the main concerns about climate change aren't based on these models anyway. All the modelers I know go to considerable lengths to calibrate the components they incorporate, and to test the models on independent data sets. Thus, the main models handle the

change of seasons and past climates (e.g. glacial, post-glacial) reasonably well. There may be some cheating here in the form of specifying rather than calculating ice extent and ocean temperature, and jiggery-pokery like "flux correction," but the models do not go crazy over quite a range of boundary conditions and forcings. This, of course, does not guarantee that they will maintain their sanity when forced outside their range of calibration, but it seems to me unlikely that they will go looney under modest changes in key parameters.

In any event, as I have blathered ad nauseum, concern about climate change neither originates in nor depends crucially on the projections of complex climate models. Climate is very largely determined by atmospheric composition (cf. Mars and Venus); Earth's atmospheric composition is changing; ergo climate should be expected to change; climate is important; ergo climate change is important. As very concisely laid out by Houghton, increasing concentration of greenhouse gases raises the effective altitude of IR emission to space, and in an atmosphere with a lapse rate, reduces its magnitude. Other things being equal, warming to a new equilibrium must ensue. Other things are of course very seldom equal, and therein lies all the argument. But worry about climate change is not something that emerged from fancy models cleverly crafted to produce disaster scenarios: It stems from elementary physics.

The technology of the fancy climate models derives from weather forecasting models, where increasing resolution and sophistication has by and large produced improved results. Their application to climate has been motivated first by Norm Phillips' observation that even medium-period weather forecasts required a model that got the climate roughly right, and second by their utility as research tools. When Keeling's data came along, people like Roger Revelle became concerned, and modelers began to press their toys into service to illuminate in a speculative fashion the more detailed consequences of atmospheric changes. Thus grew what Roger later characterized as the "cottage industry" of climate worrywarting.

Crichton correctly points out the increasingly close and not particularly happy intertwining of policy and various flavors of science. It's not entirely clear to me whether he sees unsound science driving policy into unwise actions, or unsound politics driving science into unsound projections. Who's the whore; who's the john? My perspective is that while the initial spark came from the science side, the subsequent pressure has very much come from the political side. The increasing prominence of climate models has primarily been driven by the demand of the policy/political community for ever more detailed assessment of probable/possible consequences. I doubt that anybody would be projecting carbon dioxide emissions and Kansas mean rainfall fifty years ahead were it not for the unrelenting demand of politicians for such speculations. Chuck Leith once remarked to me that he had never heard a question from a politician about any aspect of climate that did not boil down to a demand for an hour by hour weather forecast.

The problem with the climate issue is that any attempt to project decades hence inescapably requires models of the physical climate system, the biosphere, and the entire human system – population, social structure, economics, technology, politics, and so on.

This combined system must surely exhibit chaotic behavior, and be fundamentally unpredictable in its detailed evolution, even if one assumes that the vagaries of its human elements could somehow be parameterized. But does that mean that model-based scenario development is useless? I think not. While predicting the future is largely a waste of time, exploring the future is always worthwhile. I recall a study carried out by Dan Slotnick's group at the University of Illinois some decades ago. The Mexican government asked them to assess planning for a massive proposed housing program designed to give every Mexican a decent house. The Slotnick group developed a model of the entire construction industry – building one house required x concrete, y timber, x concrete required z energy, etc. They found, to everybody's complete surprise, that the primary constraint was transportation – the Mexican road and rail system just couldn't move all the material around in a timely fashion.

So I'm a big fan of modeling, and scenario-building, and speculative exploration of the future. I think that's the way you discover hazards you might not have expected, open up possibilities you might have overlooked, identify bottlenecks, get some notions of where more capable technologies would be especially helpful, and so on. But this sort of analysis is not prediction, and I think that the people who carry it out should emphasize more strongly than they do that they are not forecasters.

So I don't see anything especially sinister about the shotgun marriage between science and policy. The two don't get along very well, mainly because they make unreasonable demands on each other, and don't communicate very well their needs and responses. (This sounds uneasily familiar to us married folks!) Politicians tend to demand a level of certainty and specificity that science just cannot provide, and then use science's inadequate responses as excuses to ignore the really useful information that science can provide. Scientists expect politicians to understand and rationally act upon a complex, probabilistic, and highly nuanced mass of information, ignoring the fact that politicians have to communicate with their constituencies in highly simplistic terms.

It's a troubled marriage, but I don't think that divorce is the solution. Would the world be better off if Rowland and Molina had stayed in their laboratory instead of crying alarm about CFCs and ozone? Should that London doctor who noted a pattern in typhoid deaths have left the famous pump handle in place so that he could gather better data? It seems to me that scientists are inescapably members of society and have a responsibility to employ their capabilities to aid society. As a community, we humans have to plan ahead, we have to project possible futures, and we have to base these on the knowledge and tools we have in hand – however imperfect they may be. Our "model" of the future may be that nothing will ever change, or that all trends will continue forever, or that everything will behave like some computer program – but if we think of the future at all, we are using a "model." We just need to explore possible futures rationally using appropriate tools, illuminating possible pathways through the forest but not deluding ourselves that we can foretell which we will necessarily follow. Like the rest of us, neither politicians nor scientists are as good as they would probably like to be, or as helpful to each other. But I think it's important that both try to do their jobs with a sense of responsibility, and in the meantime to just get along with each other as best they can.

Well, I've run out of ideas and energy now – but it's been fun!

Cheers,

John