holds more beer than a pint mug regardless of how much beer is poured into each. It is now known that it is the water vapor above 2 km that dominates the water vapor feedback, and this water vapor is, for the most part, not even reliably measured.[5] Current model predictions of climate sensitivity to doubling of CO_2 in excess of about a degree depend on positive feedback from water vapor above 2 km despite the fact that the physics appropriate to water vapor at these levels is absent from the models—an unsatisfactory situation to say the least.

Tomkin's suspicion that the role of CO_2 has been exaggerated is certainly not without foundation. The suggestions of Charnock and Shine and of Kandel that scientists outside the atmospheric sciences should know better about the "quite straightforward" basic physics of the climate system, given the unstated "stretching" in their remarks, might reasonably be regarded as disingenuous.

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CHARNOCK AND SHINE REPLY: Richard Lindzen observes that our calculations of the effects of the hypothetical removal of all atmospheric CO_2 were simplified. That was made clear in our original letter.

Lindzen will also be aware that results from one-dimensional (height) global mean radiative-convective models like ours are not inconsistent with the global means of results from three-dimensional (latitude, longitude, height) models. The results from the two-dimensional (latitude, height) model that he and Daniel Kirk-Davidoff have developed are particularly interesting because they differ markedly from both. We look forward to reading a fuller account of their work.

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KANDEL REPLIES: Richard Lindzen takes a sledgehammer to the back of an envelope in his attack on my "casual and . . . misleading" criticism of Jocelyn Tomkin's letter (December 1992, page 13). This exchange began (more or less) with Alison Campbell's (February 1992, page 123) unrealistic hypothesis of *complete* CO_2 removal and casual estimate of its impact on temperature. My letter (December 1993, page 66) attacked Tomkin for minimizing the role of CO_2 based on a gross underestimate of its radiative forcing, neglecting curve-of-growth saturation effects that should be an astrophysicist's bread and butter. Lindzen casually goes along with the remark in Tomkin's reply (December 1993, page 68) to my letter that my estimate "does not *appear* [my emphasis] to allow for . . . band overlap" between CO_2 and H_2O. But my estimate, which I qualified as "very rough," made use of ICRCCM case studies[1] based on line-by-line models that *explicitly include* this overlap. My interpretation of ICRCCM may be "stretching" things, but even with Lindzen's figures based on band models, cooling for zero CO_2 is at least 250% higher than Tomkin's result. For the real case of CO_2 reduced to 200 parts per million at glacial maximum, saturation effects were of course present, and the contribution to cooling fairly weak though not nil.

Lindzen notes rightly that the Clausius–Clapeyron relation does not tell us what atmospheric relative humidity to expect. I'll offer him a full liter (not a quart) mug of beer on his next visit to Paris. I should have written that if the relative summer–winter invariance of relative-humidity profiles is a fair guide to what can happen in climate change (and it may *not* be, according to recent work here at the CNRS Laboratoire de Meteorologie Dynamique[2]), the Clausius–Clapeyron relation suggests that positive water-vapor feedback is likely.[3] Results of simple climate models are very sensitive to explicit or implicit assumptions,[4] as are those of general circulation models.[5] But my second criticism of Tomkin was his casual ignorance ("It is unclear . . . ") of the fact that most models *do* consider cloud feedbacks, and his bald assumption that cloud feedback *must* be negative, as if one could carry out infrared astronomy under cloudy skies!

In his reply Tomkin almost makes a point, and Lindzen misses it as I did, regarding climate feedbacks. Of course, overall negative feedback must be dominant in a stable system. The trouble is that we in the climate research community have got into the bad habit of calling some reference model result a "no-feedback case." This is wrong: All of these cases include the *basic negative feedback* of radiation. The critical question is whether or not total negative feedback is brought fairly close to zero by positive feedbacks enhancing climate sensitivity. When the late Fritz Möller[6] inadvertently introduced too strong a positive water-vapor feedback in his calculations, he could only avoid runaway by artificially adding a negative feedback. Lindzen has argued[7] that the absence of significant observed warming over the past few decades means that positive feedbacks have been overestimated. Maybe so, although it may also be that *negative* radiative *forcing* by anthropogenic aerosols has been underestimated. Anyone can entertain suspicions that the role of CO_2 increase—or of solar variations—has been exaggerated. But Lindzen's argument gains no credibility from association with Tomkin's know-nothing rhetoric. Readers of PHYSICS TODAY deserve better.

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Another Probe of Protein Substates

We read with interest the February 1994 special issue of PHYSICS TODAY on physics and biology and we applaud your successful interfacing of the sciences. We were drawn especially to the article by Hans Frauenfelder and Peter G. Wolynes, "Biomolecules: Where the Physics of Complexity and Simplicity Meet" (page 58), which we found both enlightening and enjoyable. Their discussion of the experimental evidence that indicates the existence of conformational substates of proteins included Mössbauer spectroscopy, temperature-derivative spectroscopy and laser optical hole burning. However, the use of another spectroscopic technique that also early on suggested the existence of distributions of energy sites was apparently omitted.

We showed the technique of fluorescence-line-narrowing spectroscopy, traditionally applied to small organic molecule systems, to be an effective tool in revealing the vibronic structure concealed in the inhomogeneously broadened absorption and emission lines of chromophores in intact protein matrices.[1] In FLN, using a combination of low temperatures (4 K) and narrowband excitation (less than 1 cm^{-1}), a subset of molecules whose transition energies are isoenergetic with the laser are excited. Given favorable conditions of weak "lattice" coupling, the emission spectrum is a quasi-line spectrum composed of zero phonon lines and phonon wings,[2] with linewidths for the former on the order of the laser linewidth.

FLN has afforded the opportunity to probe both ground- and excited-state vibrational structure and has been used to characterize fluorescent derivatives of electron-transfer and oxygen-transport proteins.[3]

Not only can FLN glean vibrational information, but it can also be used to monitor chromophore–protein interactions. If one follows an emission line as a function of excitation frequency, one obtains a distribution of emission intensity. This population distribution reflects the true distribution of zero phonon lines, and in a simple way, the width of the distribution is a measure of local disorder of the chromophore in the protein matrix.[4] We have consistently found distribution widths of 30–60 cm^{-1}—values straddling those for crystalline systems (less than 1 cm^{-1}) and for true amorphous glasses (greater than 200 cm^{-1}).

Within the framework of the Frauenfelder and Wolynes article, we attribute this distribution to conformational substates of the molecule. The population distribution has been used to probe structural changes of the protein, within the vicinity of the chromophore, as a function of various modifications, such as changes in solvent ionic strength and substrate binding.[5]

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Weaponeers Need Nukes; World Peace Doesn't

In his plea for the retention of the nuclear weapons competence of research establishments like Los Alamos and Livermore (May 1994, page 13) Joseph J. Devaney uses worn-out and discredited arguments. A similar plea could have come from the Chelyabinsk and Arzamas laboratories. It escaped Devaney that the threat to civilization that arose from the very work in these establishments necessitated a new approach to the problems of national and global security.

Nuclear weapons cannot be disinvented, but it is a non sequitur to say that they have to be kept and continually updated. It is a hallmark of a civilized society that it can control—by national laws or international treaties—the dangerous products of science and technology. The recent Chemical Weapons Convention—signed by 158 states—has knocked on the head the "disinvention" argument. Chemical weapons can be "reinvented" much more easily than nuclear ones, yet agreement has been reached on a complete ban and on steps to make the ban effective. There is no reason why the same could not be done with nuclear weapons.

Devaney quotes the old Roman dictum "*Si vis pacem, para bellum*" ("If you want peace, prepare for war"). But the whole course of history has shown that preparation for war results in war. Nowadays, even preparation for war has become too costly and is unsustainable. The nuclear arms race that raged for four decades (1949–89) could not have gone on much longer; it is likely that a nuclear holocaust would have resulted if an irrational hard-liner had come to power in the Soviet Union instead of Mikhail Gorbachev.

At no time during the whole period was either side satisfied with what it had in its arsenals; both sides had to keep on modernizing their weapons or developing new defense systems. To a large extent the momentum of the arms race was generated by the scientists in the research establishments.

Thus Lord Zuckerman, for many years scientific adviser to the British government, wrote: "In the nuclear world of today the military chiefs . . . as a rule merely serve as the channel through which the men in the laboratories transmit their views. For it is the man in the laboratory . . . who at the start proposes that for this or that reason it would be useful to improve an old or devise a new warhead. . . . It is he, the technician, not the commander in the field, who starts the process of formulating the so-called military need."[1]

The motivation of these scientists was explained by Herbert York, former director of the Livermore Laboratory: "The various individual promoters of the arms race are stimulated sometimes by patriotic zeal, sometimes by a desire to go along with the gang, sometimes by crass opportunism. . . . Some have been lured by the siren call of rapid advancement, personal recognition, and unlimited opportunity, and some have sought out and even made up problems to fit the solutions they have spent most of their lives discovering and developing."[2]

By a stroke of luck we have now a real prospect of putting an end to the obscene arms race. Let us make sure that this aberration of science is also brought to an end. The sooner these establishments are closed down or converted to peaceful work, the better the chance of our civilization's surviving in the nuclear age.

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Joseph J. Devaney's well-reasoned apologia for maintaining a high nuclear-deterrence capability is understandable in that anyone who makes a living through weaponry wants to remain employed. But he weakens his case by quoting the fatuous aphorism of the fourth-century military writer Vegetius: "If you want peace, prepare for war." And if you want