The potential biomass resource is vast. Fast-growing trees and perennial grasses can be grown for energy purposes on dedicated farms in a manner inherently more environmentally friendly than growing annual food crops.[2] Even though the overall photosynthetic efficiency is low (for example, a "good" yield of 15 dry tonnes per hectare per year corresponds to an annual average photosynthetic efficiency of just 0.5%), the overall process of growing biomass, converting it to hydrogen and using the hydrogen to power FCVs is relatively energy-efficient. This fuel cycle will support nearly 7 times as many vehicle-kilometers of travel per hectare as does the current commercial process of making ethanol from grain for use in ICVs.[1] In fact, to run the entire expected worldwide fleet of one billion cars in 2020 on biomass-derived hydrogen would require only 60–70 million hectares of land. This is just twice the amount of cropland held out of production in the US today to keep food prices up and to control erosion—both of which objectives could be met by growing energy crops on these excess croplands instead.

If the availability of land eventually limits the extent of biofuels production, additional quantities of hydrogen could be produced electrolytically from renewable power sources such as wind or photovoltaic electricity. Land requirements for these renewable electrolytic sources would be tiny relative to the requirements for biomass-derived hydrogen.[3] Even though the cost of producing hydrogen electrolytically from renewable sources in the future would probably be roughly twice the cost of hydrogen derived from coal or biomass, the cost of electrolytic hydrogen per mile of driving would still be comparable to the cost of gasoline per mile for an ICV[4]—the fuel would be no less affordable, yet cause no emissions of local pollutants or CO_2.

As Harding suggests, we will always remain vulnerable to the "law of unintended consequences." Nevertheless there are fuel strategies for fuel cell vehicles that would make them clearly preferable to gasoline internal combustion vehicles with regard to primary energy requirements, global warming and local air pollution.

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Drell Defends Urging US Contribution to LHC

Jay Orear (January, page 73) challenged conclusions of the 1994 Subpanel on the Vision for the Future of High-Energy Physics of DOE's High-Energy Physics Advisory Panel—a subpanel that I headed. I am responding to several claims in his letter.

Orear first expressed puzzlement as to why our report recommended a US contribution to the CERN Large Hadron Collider even though an earlier panel, which I headed in 1990 for then-Secretary of Energy James Watkins, supported the Superconducting Super Collider in preference to the LHC. As was emphasized in the 1990 report, the SSC, with its 40-TeV collision energy, offered high confidence that we would be able fully to explore the mass region pertinent to discovering mechanisms responsible for the breaking of electroweak symmetry. In contrast, the LHC, while opening many important and unique new possibilities for discovery at energies up to one-third the energy of the SSC, will permit exploration of important parts, but not the entirety, of the critical region of interest for electroweak symmetry breaking. Regrettably the SSC is no longer a practical option for the US, its construction having been terminated in 1993.

Fortunately the LHC, which will permit a major advance (by a factor of 7) in collision energy above Fermilab's current frontier, received a go-ahead decision from the CERN council in December. Our 1994 report affirms in its analysis that the LHC "will offer a unique prospect for advancing to the highest energy frontiers" beyond the Fermilab Tevatron, and further that "the LHC will open new windows to discovery and present important opportunities to confront physics questions posed by current experiments and theories." It was on this basis that we recommended American participation in

building the LHC and doing research on this exciting new energy frontier.

In our study we did indeed address the challenge, the prospects and problems of upgrading the Fermilab Tevatron by doubling its energy and increasing the luminosity, as advocated by Orear. Suffice it to say that after analyzing that option seriously we came to the unanimous conclusion that US international collaboration with CERN on building and working at the LHC was the scientifically preferable option. Our conclusions and recommendations were also endorsed unanimously by the High-Energy Physics Advisory Panel, to which we reported our findings.

I should also add in response to Orear that my panel did not operate "under the rigid assumption that the next accelerator above the LHC energy *must* be an international enterprise." However, it is my personal view that international cooperation in scientific research, which has been prevalent in high-energy physics for decades, is appropriate when it comes to building billion-dollar accelerator facilities that will be unique research tools worldwide. It has long been my personal view that the SSC should have been initiated as a truly international design and construction project. It might still be alive today had that been the case!

Orear also alleges that not only is the report of my subpanel "being misused to promote the LHC over the physics that we *Americans* [emphasis added] would normally be doing at that time, but it is being used to promote linear colliders over hadron colliders." Orear is of course entitled to his personal view of what "we Americans would normally be doing at that time." It happens to be a view rejected by the subpanel on both practical and scientific grounds. However, I believe there are no substantive grounds to support his allegations of misuse of our report to promote future electron linear accelerators over hadron colliders. We expressed strong and clear support for a strong program in advanced accelerator R&D to create new technical possibilities for advancing the frontiers of high-energy physics. Prospects for continuing to probe for nature's elementary structures and forces at greater depths will depend on the inventiveness and creativity of accelerator physicists in developing practical new paths of progress, and of experimentalists and theorists in asking the right questions and advancing the sophisticated art of detectors. More power to them, and may the best ideas win! Past progress has made it abundantly clear through the years that we are not wise enough to predict *a priori* whether the electron or hadron frontier will lead to the next big breakthroughs. Both frontiers have proved to be of critical importance, their mutual progress has proved to be of great value, and their active proponents deserve our encouragement.

Finally, it was of utmost importance that our subpanel report successfully built a broad consensus among a large number of American physicists around a future vision that includes the LHC. If we cannot agree among ourselves as a community, we will have little ability to persuade our society and government, who must pay the bills to provide the necessary support.

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Antiferromagnetism's Parisian Premiere

I wish to point out an error made by Barbara Goss Levi in her news story about the 1994 Nobel physics laureates, Bertram N. Brockhouse and Clifford G. Shull (December, page 17). Writing about Shull's work at Oak Ridge National Laboratory with Ernest O. Wollan, Wallace Koehler and J. Samuel Smart, Levi states, "In the course of this work they came up with the first experimental demonstration of the existence of antiferromagnetism, which had been predicted by Louis Néel."

Néel, in the award lecture he gave on receiving *his* Nobel Prize, acknowledged that the first experimental demonstration of the existence of antiferromagnetism was done by Henri Bizette, Belling Tsai (who were graduate students in Paris) and me (a postdoctorate fellow).[1] I followed our 1938 paper with a full-length article.[2] Shull was well aware of this earlier work, and he and I discussed it at Oak Ridge in 1948.

The 1938 publication was presented to the French Academy of Science by Aimé Cotton, director of the laboratory at Bellevue, Paris. In the spring of 1938, I was sent to the University of Leiden, the Netherlands, to discuss our antiferromagnetism work with Hendrik A. Kramers and Hendrik Casimir. They were delighted.

All of this history of antiferromagnetism, with credit to Shull and Smart for their neutron diffraction

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