public safety. Such changes have yet to be defined, but they almost always increase regulatory complexity.

Acceptance. Fusion has rightly been described as the fundamental energy source in the universe, though the general public has given it little attention. It has also been characterized as inherently safe, which is true for the plasma. However, the public is largely unaware of the high levels of radioactivity and the safety risks of superconducting magnets. When the huge costs, large quantities of radioactivity, and safety concerns become more broadly known, acceptance is sure to suffer dramatically.

One can only guess at why ITER continues to be built. Did the researchers ignore the engineering warnings associated with "sufficient"? Perhaps they chose to circle the wagons and hide the realities of their chosen concept. Where were the government officials who were supposedly responsible for overseeing fusion research? The media must not have been paying attention either. When the truth regarding current tokamak fusion research is recognized, embarrassment and repercussions may well be widespread.

Nevertheless there is hope of satisfying the "necessary" and "sufficient" conditions for fusion power.[5] In light of what has been learned from tokamaks, other plasma-physics research, engineering studies, and the application of the EPRI criteria, moving to a much cleaner fusion reaction would seem appropriate. Of particular interest is the proton and boron-11 reaction, which involves significantly more challenging physics but produces no neutrons directly. The absence of neutrons would largely eliminate the risks due to radioactivity and thereby dramatically enhance economics, regulatory simplicity, and public acceptance. Thankfully, a few privately funded projects in the US and elsewhere are pursuing p–^{11}B and other concepts. Although more difficult from a physics standpoint, those concepts do not appear impossible, and such systems might stand a chance of being sufficient.

The ITER-tokamak approach fails against the EPRI criteria. However, concepts based on different fusion fuels might succeed. An objective engineering review is urgently needed to verify the insufficiencies of ITER-like tokamaks. A dramatic reorganization of fusion research and a better-focused research program could result in power plants that will be sufficient.

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Editors' note: We invited Steven Cowley, former CEO of the UK Atomic Energy Authority, to comment on points raised by Robert Hirsch.

▶ **Cowley replies:** Undoubtedly, tokamaks have yielded by far the best plasma confinement of all fusion experiments. Indeed, the Tokamak Fusion Test Reactor at the Princeton Plasma Physics Laboratory in New Jersey and the Joint European Torus (JET) at the Culham Centre for Fusion Energy in the UK have achieved stable fusion conditions and significant fusion power—up to 16 MW in JET—from the deuterium–tritium reaction. Furthermore, detailed modeling from models validated against experimental data predicts that the international tokamak experiment ITER will attain a fusion "burn," a state in which external heating is negligible and self-heating by the fusion-generated alpha particles is sufficient or almost sufficient to sustain the discharge.

A burn would be the long-awaited scientific demonstration that energy production from fusion is possible. Only ITER offers the chance of reaching that hugely important milestone in the next two decades. However, as Robert Hirsch indicates, ITER will not prove the economic viability of fusion power. Such a determination is nontrivial, and without further R&D it is necessarily uncertain.

Hirsch is wrong that tokamak reactor studies have ended in most parts of the world. For example, at the time of writing, demonstration tokamak reactor designs are being developed in the European Union (EU),[1] South Korea,[2] and China,[3] and less directed reactor studies are being pursued by all other ITER partners. Those studies address the well-known and serious technical issues raised by Hirsch. The authors made no attempt to downplay their significance. To appreciate the depth of the analysis, one has to read the extensive literature. I can only summarize briefly the current understanding of each of Hirsch's issues.

In fission and in fusion, cost is determined by much more than the mass of the core. Detailed estimates of the cost of electricity from the 2006 EU fusion reactor designs put the range[4] between 0.03 and 0.09 €/kWh. ITER's cost overruns, which are expected to be significantly less than Hirsch's estimate, reflect a project that requires extensive R&D at every stage. They do not reflect the intrinsic industrial cost of components. Nonetheless, it is important to understand the ITER costs much better. Recent research, such as on the suppression of plasma turbulence, and expected improvements in technology, such as for superconducting magnets,[5] suggest that innovation will drive down the cost and scale of tokamak reactors. Although I would not take any cost estimates too seriously, they indicate that tokamaks may enter the market in the right cost range. It is simply too early to be conclusive about cost.

Hirsch is correct in identifying the quenching of superconducting magnets as being an issue for nuclear regulators. In fact, it is an issue with the French nuclear regulator for ITER. Technical studies of ITER show that a rapid quench of the superconducting magnets, caused by impact or otherwise, would not breach the containment of the vacuum vessel, let alone the main containment of the cryostat. Thus such an accident, although costly, would not endanger the surrounding population.

The radioactivity of DT fusion reactors is a well-known issue.[4] Material scientists have developed low-activation steels that reduce key impurities—nickel, for example—so that the radio isotopes produced by neutron bombardment are short-lived. With such materials, the activated material made in a fusion power plant will be low-level waste after 100 years.

Tokamak reactors also face challenges not mentioned by Hirsch: tritium breeding and storage, for example.

Success is not assured, but it is far too early to say that tokamaks fail against the Electric Power Research Institute criteria. Stimulating innovation on a broader range of ideas is also desirable. But we have an opportunity with ITER to create a burning plasma with an output of approximately 500 MW of fusion power. That opportunity should not be missed.

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Hall-effect metamaterials and "anti-Hall bars"

In his letter in the July 2017 issue of Physics Today (page 13), Ramesh Mani points to the connection between part of one unit cell of our three-dimensional chainmail-like Hall-effect metamaterial[1] (see Physics Today, February 2017, page 21) and his earlier work on planar "anti-Hall bars."[2] We were not aware of his work and thank Mani for pointing it out to us. However, the conclusions he derives in his comment are misleading.

He argues that the change in Hall-voltage sign "should be attributed to a change in effective geometry rather than to a change in sign of the Hall coefficient." That viewpoint completely ignores the idea of metamaterials and composites, as described by homogenization theory.[3,4] Indeed, as emphasized by Mani, the Hall coefficient of the host material does *not* change when one introduces voids into it. However, the geometry or structure inside the metamaterial unit cell determines the *effective* Hall coefficient of the metamaterial crystal.

What does the metamaterial community generally mean by effective material parameters? Suppose, in the sense of a black box, an experimentalist cannot look into the unit cell of an artificial crystal but can perform experiments on the crystal. He or she may change the strength and direction of the applied static magnetic field, the amplitude and direction of the injected electrical current, the pickup of the Hall voltage, and the size of the sample, measured by the number of unit cells in any one direction.

For our 3D metamaterial, the experimentalist would conclude that all observations are perfectly consistent with a sign reversal of the Hall coefficient—that is, the effective Hall coefficient—with respect to that of the bulk host material. In sharp contrast, that statement is not true for a single planar anti-Hall bar.

Wiring up many individual Hall elements into a 3D, electrically isotropic metamaterial crystal has been the main aim of our work.[1] It is demanding: In the resulting 3D chainmail-like geometry,[5] which has been inspired by the work of