

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 range⁴ 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 suppres-

sion 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.

References

1. G. Federici et al., *Fusion Eng. Des.* **109–111**, 1464 (2016).
2. K. Kim et al., *Nucl. Fusion* **55**, 053027 (2015).
3. B. N. Wan et al., *IEEE Trans. Plasma Sci.* **42**, 495 (2014).
4. D. Maisonnier et al., *Fusion Eng. Des.* **81**, 1123 (2006).
5. B. N. Sorbom et al., *Fusion Eng. Des.* **100**, 378 (2015).

Steven Cowley
(steven.cowley@ccc.ox.ac.uk)
Oxford University
Oxford, UK

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 metamaterial¹ (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

Marc Briane and Graeme Milton,³ all connections are made from the same semiconductor material as the Hall elements. Thus local Hall voltages appear wherever a current is flowing, and, strictly speaking, one cannot even make a distinction between the connections and the local Hall elements inside the metamaterial.

Our work should not be misunderstood to mean that we simply somehow wanted to change the sign of the Hall voltage. That would be trivial indeed: One could simply interchange the two wires picking up the Hall voltage.

References

1. C. Kern, M. Kadic, M. Wegener, *Phys. Rev. Lett.* **118**, 016601 (2017).
2. R. G. Mani, K. von Klitzing, *Appl. Phys. Lett.* **64**, 1262 (1994).
3. M. Briane, G. W. Milton, *Arch. Ration. Mech. Anal.* **193**, 715 (2009).
4. G. W. Milton, *The Theory of Composites*, Cambridge U. Press (2002).
5. M. Kadic et al., *Phys. Rev. X* **5**, 021030 (2015).

Martin Wegener

(martin.wegener@kit.edu)

Muamer Kadic

Christian Kern

Karlsruhe Institute of Technology
Karlsruhe, Germany

Peer review as collaboration

Raymond Goldstein provides a valuable commentary on the peer-review process (PHYSICS TODAY, December 2016, page 10), in which he finds that “with few exceptions, there is no mechanism for the referees and the editor to discuss the paper and arrive at a consensus recommendation *before* reviews are sent to the authors. Instead, the initial recommendation is based on the editor’s implicit averaging of the reports.” Goldstein then discusses the journal *eLife*, in which the “review process is an online discussion between the referees and the handling editor of a paper so that they arrive at a single consensus report . . . that is sent to the authors.”

I agree entirely with Goldstein’s assessment that scientists who work with the “deeply flawed process . . . deserve better” than the averaging he describes.

At the same time, as founding editor, now emeritus, of *Physical Review Applied*, I offer some insight into the process that I was not aware of before taking on that position.

As an author, I, like most of my colleagues, have often been frustrated by conflicting referee reports such as the example Goldstein recounts. The canonical outcome of the review process is one review that, to varying degrees, pans the paper and a second that does the opposite. As an editor, I have found—to my

surprise—precisely the same thing: I expected I would be able to either choose better referees or direct the referees in a way that would avoid quite so many diametrically opposed reviews. Alas, that has not been the case. It seems that we scientists are an opinionated and often contrary bunch.

That said, I have also learned that conscientious full-time editors with whom I have worked expend much time and energy working to reconcile differences between reviewers. Indeed, the

TESTED AND PROVEN WORLDWIDE

- Liquid Helium Plants**
Our laboratory scale Liquid Helium Plants ensure you never pay for liquid helium again
- Helium Recovery System**
Designed for your space and needs
- Cryocoolers**
The most extensive line of GM & PT Cryocoolers on the planet
- 1K Cryostats**
Fully integrated, closed-cycle refrigeration for applications below 2K
- Liquid Nitrogen Plants**
Your own LN2 supply: as easy as having power and air
- Helium Reliquefiers**
Convert your open-cycle device to a close-cycle helium recycling system

World leaders in cryorefrigeration for more than 50 years

CRYOMECH
315.455.2555 cryomech.com