but the harder you try, the lower you fall. As with many personal issues, you have to reach a bottom point before recovery can begin. It is a long, painful journey just to admit you have a problem, but from that point you can begin to rebuild. The love for physics and research slowly comes back, and you are able to balance your career activities with the needs of your mind and body. While you were climbing back up, however, the rest of the world was going on without you.

As you recover, it may be necessary to take a backstage role for a time and leave to others the leadership and public roles that tend to carry more stress. Yet if you cannot be present at meetings and be visible through talks and seminars, then it appears you are not fulfilling the requirements of the modern professional researcher, who is more and more a manager and less and less a scientist. Colleagues may have pity on you and try to make room for you, perhaps temporarily, perhaps at a level that does not reflect your high academic value, but such adjustments are all the system currently allows. You have no real future and no real prospects.

I have witnessed some truly talented people run into this nightmare and, even after basically recovering, "disappear" scientifically. They have minimized their workload just to a survival level, but the system has no provision for a return to their full career. The burnout may leave them with side effects that hopefully will diminish with time—for example, the inability to stand in front of an audience, sit in a closed room for hours discussing a project, travel alone, or take on greater responsibilities.

I don't suggest that we pity those who are experiencing burnout or that we free them from all responsibility while they recover. Instead, I am asking that accommodation be made to allow a person with burnout to continue to have a respected position where they can fulfill academic duties according to their current capabilities. For example, a person who is so anxious as to be unable to speak in front of an audience could still be quite capable of doing the behind-the-scenes research and writing to prepare the lecture and then let someone else deliver it. The person could work remotely or take on additional tasks that do not involve the highest job stressors, tasks such as organizing seminars, correcting student exercises, and doing background research for presentations.

My hope is that the system, insensitive and competitive as it can be, may still afford dignity and respect to colleagues who fall into the black hole of burnout. One day, without even realizing it, any of us could take that fall. My experience is that with personal, professional, and practical support, recovery is just around the corner for people experiencing burnout.

An even more powerful way, perhaps the best way, to address burnout is prevention. Lightening the load by reducing the most stress-inducing commitments before reaching a breaking point does not indicate a lack of character, but rather a healthy intelligence and a desire to be efficient and productive for the long term.

People often associate physicists with the stereotype of mad genius. Many TV series make fun of the intelligence and eccentricities of physicists, but psychological fragility is a true nightmare. I would like to see the scientific community combine its intelligence and unite beyond competition to defeat burnout, for ourselves and for our colleagues. Then perhaps we can also find amusement in society's stereotypes of us.

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LETTERS

Climate change scenarios and risks

I enjoyed reading Heather Graven's article "The carbon cycle in a changing climate" (Physics Today, November 2016, page 48) and the other articles that month on modeling different aspects of climate change. After reading those and other articles on the subject, I have an uneasy feeling that people are missing the point when it comes to dealing with climate change.

The optimal strategy almost certainly depends far more on the very small probability of extreme changes in climate than on the most likely changes. People seem to focus only on modeling the most likely changes in climate, and maybe the one-standard-deviation error bars, which are likely to be almost irrelevant in choosing the best strategy. The general consensus of climate models, such as those discussed by Graven, is for average global temperatures to rise by a few degrees Celsius over the next several decades, which will have serious effects on agriculture, water resources, coastal flooding, species diversity, and human migration. People argue over justification of spending today to avoid the costs of those effects over the next several decades.

However, if one assumes that climate changes are 100% certain to resemble the shorter-term model consensus, then the best strategy for minimizing net costs might be to spend very little now and instead wait 20 or 30 years. By that time some future technology, perhaps involving intelligent robots, may be able to accomplish anything we want, virtually for free, to reverse whatever climate change has meanwhile occurred, even if much more drastic measures are needed then than would be needed now. Freeman Dyson makes a similar point about discounting future costs because of advancing technology.[1]

The problem with the wait-and-see approach is that we cannot be 100% certain that something like these consensus climate models is correct. In particular, we cannot exclude the possibility that a runaway greenhouse effect will become unstoppable in less than 20 or 30 years and eventually leave Earth uninhabitable. After all, it did happen on Venus, which started out similar to Earth but with about twice the solar forcing. If that is a real—even if unlikely—possibility, then the optimal strategy might be completely different: Spend a lot of money now to try to prevent the runaway greenhouse effect.

Since our optimal strategy depends very much on unlikely, but still possible extreme scenarios, it seems to me we should be devoting a significant portion of our research budget to modeling how those scenarios might occur, rather than modeling only what is likely to occur.

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Explaining a few discoveries

The editorial "Discoveries and explanations" by Charles Day in the March 2017 issue of PHYSICS TODAY (page 8) discusses the work of my late colleague Vera Rubin. She showed that the rotation curves of stars in the outskirts of spiral galaxies were flat rather than Keplerian, which implied the presence of large amounts of dark matter. The history of that discovery deserves some elaboration, not to diminish Rubin's influential work but to highlight its precursors.

Rubin's first paper reaching that conclusion,[1] with coauthors W. Kent Ford Jr and Norbert Thonnard, was published in 1978. Over the preceding decade, several researchers had already found that the rotation curves of neutral hydrogen gas in spiral galaxies were flat, and they concluded that those galaxies contained at least as much dark mass in their outskirts as the mass in visible stars and gas.[2] In 1974 two independent groups, one in the US[3] and one in Estonia,[4] used those results, along with fragmentary evidence from various other sources, to argue that galaxies were surrounded by extended halos of dark matter containing up to 30 times the mass in visible stars. Reference 3 is particularly notable because it estimated that relative to the critical cosmological density, the density Ω of dark and luminous matter was ~0.2, remarkably close to the best current estimate[5] of Ω = 0.308 ± 0.012.

Many of the earlier papers are cited in Rubin and coauthors' 1978 paper, which states explicitly that "[Morton] Roberts and his collaborators deserve credit for first calling attention to flat rotation curves."

Like Saul after his conversion on the road to Damascus, Rubin accepted a revolutionary idea after it was fully formulated, and she became one of its most effective advocates.

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In his March 2017 editorial, Charles Day implies sexism and possibly slights the Royal Swedish Academy of Sciences by asserting that a poorly qualified Nobel evaluation panel is a principal reason for Lise Meitner's not being awarded a Nobel Prize for her and her nephew Otto Frisch's explanation of the fission of uranium. However, it would have strengthened his case if he had provided a background or reference of earlier events that may have influenced the academy.

James Chadwick, in Germany with Hans Geiger at the beginning of World War I, was able to continue some of his studies using improvisations and materials provided by German scientists. He found that the energy spectrum of electrons emitted in beta decay was continuous with a defined maximum energy. His initial results were obtained while he was in a German prison camp during World War I. Depending on the radioactive source, some weak lines were superimposed on a continuous spectrum. Chadwick used two methods to measure the electron energies: the deflection of the beta rays in a magnetic field and a method that exploited the known ionization-energy thresholds of the electrons.

The explanation offered by Meitner was that the beta electrons lost energy by several mechanisms, including collisional interactions with the substrate of the beta-ray source.

However, the heating measured from the beta-decay electrons corresponded to the mean energy of Chadwick's distribution and not to the maximum electron energy. Meitner and her colleagues, possibly influenced by the defined energy peaks found in alpha decay, held fast for approximately 15 years to their explanation that the beta-ray energies were quantized. Charles Ellis and William Wooster set up a sensitive experiment to measure the total energy of the beta electrons. They inserted their radioactive sample into a thermal calorimeter that had been calibrated by collecting electrons of known energies.

Ruth Lewin Sime's biography[1] of Meitner briefly mentions the 1924 paper[2] by K. George Emeléus (my thesis adviser) reporting that there was just over one electron per "radium E" (bismuth-210) decay. If only one electron were emitted, the decay process could not possibly have satisfied conservation of energy and momentum. Then Meitner's student Nikolaus Riehl repeated Emeléus's experiment, with about the same result. Still, Meitner did not regard those results as proof of the energy spread of the primary electrons. She did become less confident of her long-held conviction of a discrete quantized electron-energy spectrum.

Later experiments confirmed the continuous energy spectrum, and eventually Meitner and others accepted the results. The long delay until Wolfgang Pauli suggested the antineutrino was at least partly because of Meitner's unusual and uncharacteristic refusal to accept the continuous electron-energy spectrum.

Meitner may well have deserved the Nobel Prize. However, the Royal Swedish Academy of Sciences may have been influenced by this long controversy.

Possibly more important was a much earlier, well-documented precedent for a correct explanation of nuclear fission by another competent female scientist, Ida