windows of greater than 60 days with submissions from more than 1000 proposers annually, I have found that the data adhere to a modified hyperbolic function, as plotted in the figure. The model is simple, which means it omits factors such as delays introduced by the universities' sponsored research offices. Nevertheless, the procrastination behavior is predicted quite well, and without any fitting parameters.

As is shown in the figure, the ULP curve provides a simple means of predicting the impact of proposal pressure and of estimating the number of proposals expected as a function of remaining time to deadline. Practical concerns for a receiving institution include how to handle the number of proposals received on the deadline date and whether that load will overtax or crash the existing computer infrastructure.

Bear in mind, though, that hyperbolic functions diverge to infinity at the asymptote. To procrastinating submitters, the most critical issue is that by waiting until the deadline or close to it, they eliminate the time needed for identifying and correcting errors that could make their proposal ineligible for consideration.

I appreciate helpful discussions with Andy Lovinger at NSF and the encouragement of Cornelius König of Saarland University. Any opinions expressed in this material are mine and do not necessarily reflect the views of NSF.

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Pictures of climate change

Spencer Weart's article on climate impacts (PHYSICS TODAY, September 2015, page 46) describes the sociology of how opinion has evolved on anthropogenic change, but it says little about the opinion's scientific content. It is remarkable that the scientific giant in this field, Svante Arrhenius (1859–1927), without knowledge of the Planck function—much less the quantum mechanics of molecular opacity or computer codes—made predictions of climate sensitivity that are within a factor of two or three of modern estimates. Was that a lucky guess, or is the phenomenon so robust that even the crudest estimates are almost as good as the most sophisticated?

Weart describes, but does not explain, how the consensus about the effects of climate change has shifted from equanimity to fear and trembling that a great disaster will ensue. Is climate change a phenomenon to be observed, like the weather? Is it of direct concern mostly to farmers? Or is it a problem to be solved, and if so, how urgently? The shift is a sociological phenomenon that calls for explanation, but not by physicists.

The physical principles have long been known, and François Massonnet's Commentary in the same issue (page 8) explains that even our present understanding and computational capabilities are not sufficient to predict regional effects such as droughts and floods. The fact that multiphysics codes—which combine multiple models to simulate complex phenomena—could not predict the failure of National Ignition Facility targets should make us skeptical of their power to predict any complex phenomenon, and climate is more complicated than a laser target.

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Having formerly worked for the National Weather Service for 40 years, including assignments at the National Severe Storms Forecast Center and various field forecast offices, I was struck by the images in Spencer Weart's article "Climate change impacts: The growth of understanding." I thought it was interesting that the editors chose to illustrate the article with several weather-disaster photos.

The cover photo shows flooding of small fields lined with palms and other tropical fauna. Other photos show drought and floodwaters extending halfway up storefront shops.

The inference, I suppose, is that climate change caused those weather disasters, despite the author's stating he was unable "to present a convincing case, based on logic and observations, of why anyone should believe the consensus statements" about climate change impacts.

Those photographs perhaps make it more pleasing visually to leaf through a publication, but their inclusion only perpetuates the myth that individual storms are the result of climate change. For example, the vast majority of the flooding shown in the Hurricane Sandy photo was due to the storm surge that typically accompanies hurricanes. The track of Hurricane Sandy was an outlier in the data set. The unusual flooding can be explained entirely by storm dynamics over the ocean. A sea-level rise of several inches due to ice melt would not by itself cause 20- to 25-foot storm surges.

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▶ **Weart replies:** Jonathan Katz worries about the validity of computer studies of projected impacts of climate change. And John Curran notes that illustrations to my article show particular events, which computer studies indeed have difficulty attributing individually to climate change. I apologize if any reader jumped to the conclusion that a specific attribution was intended. I wanted only to illustrate the subject of the article—namely, impacts in general. Still, part of the sea-level rise of the past century is reliably attributed to global warming, and the rise did extend the area of Sandy's inundation. And a peer-reviewed study has reported that global warming did contribute to the Texas drought that was illustrated.

For reasons of length I had to leave out the interesting story of attribution studies of particular impacts; for a sketch and references see http://www.aip.org/history/climate/impacts.htm. Researchers have labored for decades to test computer models against observations, and the matches have been good although imperfect. Anyway, it is not computers but simply the thermal expansion of water and the visible decay of ice sheets that support expectations of further sea-level rise if greenhouse gas emissions continue. Other serious impacts have already been observed in weather statistics, including global intensification of heat waves and of extreme precipitation events.

Finally, Curran misunderstands a

phrase he took out of context. I wrote that "in this short article" I could not present a convincing scientific case for impact projections. A convincing case can certainly be made, but only, alas, to those who will undertake a thorough study of the technical literature. Therefore officials and the public have little choice but to heed the consensus of committees of expert scientists—unless (like some people) they dismiss the entire scientific process.

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Correcting the history of the CMB idea

In their response to my letter (PHYSICS TODAY, August 2015, page 10) regarding the prediction by Ralph Alpher and Robert Herman of the cosmic microwave background (CMB), what John Carlstrom, Tom Crawford, and Lloyd Knox report as the correct history unfortunately continues to perpetuate myths.

The authors include George Gamow in their attribution of early predictions of the CMB in the late 1940s. It does not disparage Gamow to point out that he had no role in the prediction and interpretation of the CMB at 5 K. The misattribution is so common that citing all of its occurrences would be virtually impossible.

Gamow did not embrace the work by Alpher and Herman; for several years he rejected the validity of their CMB concept.[1,2] In addition, the question of Gamow's involvement can easily be answered by further documentation. During the summer of 1948, when Alpher and Herman were working on the CMB idea and preparing a manuscript, Gamow was busy giving lectures in Ohio and at the Los Alamos laboratory in New Mexico.[3]

I must also take issue with the statement by Carlstrom and coauthors that Robert Dicke's research group at Princeton University "immediately understood the significance" of the measurement by Arno Penzias and Robert Wilson. Actually, the Princeton group posited several different cosmological scenarios in 1965; none of them referred to Alpher and Herman's work, which provided the theoretical framework for the CMB some 17 years earlier than is generally accepted today.

Before giving his Nobel Prize acceptance address in 1978, Penzias met with Alpher because he wanted a correct, firsthand account of Alpher's work on the CMB.

Alpher and Herman did immediately understand the cosmological significance of the work by Penzias and Wilson: It confirmed their prediction. That caused a great deal of angst during the rest of their professional lives, as repeated publications regarding the CMB ignored their pioneering publications. They wondered why so many radio astronomers told them the radiation could not be measured back in the 1940s and 1950s despite attempts over many years.[1] Penzias remarked to Alpher that the measurement could have been made "back then" with a bolometer. Today we know that before 1965 several CMB measurements were made but not interpreted as significant.[4]

In later publications, Gamow discussed one attempt to make the CMB calculation on his own—with predictable inexactitude.[1] He traveled often to present talks based on Alpher's 1948 dissertation titled "On the origin and relative abundance of the elements." In revising his presentations, Gamow frequently requested updates, slides, and preprints from Alpher.[3] The revisions were published in major journals through the early 1950s. An analysis of the 20 years of that written correspondence is forthcoming.

It is unfortunate that so much dogma has permeated the literature for the past 50 years. Hopefully, new generations of physicists will become aware of this problem in scholarship in cosmology and astrophysics and will not continue to perpetuate such myths.

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