

experiments were selected and what the results mean for the broader agenda of reproducibility in research — is already hotly contested.

Perhaps the most influential aspect of the exercise, called the Reproducibility Project: Cancer Biology, has nothing to do with those arguments. It lies beneath the surface, in the peer reviews of the project teams' replication plans, which were published before the studies began. These reviews can be read as part of the editorial decision letters linked to each replication plan, or 'registered report' (see go.nature.com/2jte08a).

What, one might ask, could be less interesting? What insights can emerge from technical feedback on plans to repeat what other groups of scientists have done? Plenty. The decision letters reveal the practice of science at its best: probing whether an experiment truly tests a particular idea; identifying shortcomings in the original set-ups; and proposing further, sounder tests. To those committed to improving science, reading these insights — the fruit of voluntary donations of time and effort — will prove a moving experience. Journal clubs would do well to read the original papers, and then compare their own analyses with those of the scholars who have reviewed the replication reports.

Again and again, the peer reviewers and the replicators clash. The reviewers are eager to produce the best experiment to test a publication's conclusions; they want to correct deficiencies in the design of the original high-impact studies. The replicators do, on several occasions, agree to add an extra measurement, particularly of positive and negative controls that had originally been neglected. Often, however, they resist calls for more definitive studies. Testing "the experiment's underlying hypothesis," they insist, "is not an aim of the project."

This is a frustrating, if understandable, response. It is easier to compare the results of highly similar experiments than to assess a conclusion. Thus, the replication efforts are not especially interested in, say, the big question of whether public gene-expression data can point to surprising uses for new drugs. They focus instead on narrower points — such as whether a specific finding that an ulcer drug stalls the growth of lung cancer in mice holds up (it did, more or less; see I. Kandela *et al.*

eLife 6, e17044; 2017). Even so, the results are not definitive. One set of dissimilar results can't establish that the original result was a false positive; nor do similar results show that the original conclusion is correct.

Yet a project that sticks to direct replication and eschews broader 'truth' can still raise bigger issues. After all, grand conclusions are built on myriad laboratory experiments. How well (or not) such experiments work outside the original lab is too often communicated through gossip rather than open discourse. For just US\$2 million in private funding — less than a typical 5-year grant from the US National Institutes of Health to a single investigator — this replication project shines a very public light on the sticking points of experiments.

One aspect that merits increased focus is how markedly the results of control experiments varied between the original work and replications. In one case, mice in the control group of an original study survived for nine weeks after being engrafted with tumours, whereas those in the replication survived for just one. In another, a bioluminescent signal used to track tumour burden differed markedly between the original and replication studies. Such situations occur frequently in biology, but are often overlooked because researchers focus only on results in their own laboratories. There is great value in highlighting variability in 'established' experimental systems.

Such results deserve more attention, both technically and philosophically. Researchers routinely optimize experimental conditions to see the signals that they are looking for. But how far can this proceed until confirmation bias makes results uninterpretable? How does one do pilot studies without overly encouraging favourable results?

More than 50 years ago, the philosopher Thomas Kuhn defined 'normal science' as the kind of work that faithfully supports or chisels away at current hypotheses. It is easy to dismiss this as workmanlike and uninteresting. But only by doing such normal science — and doing it well — can we recognize when revolutions are necessary. ■

Pricing the planet

The social cost of carbon must be based on science, not politics.

Take a step back. Earth is an orb reeling through the emptiness of space, warmed from the inside by molten rock and from the outside by the Sun. Thanks to the wonders of physics, a magnetic shield protects us from the most damaging cosmic radiation, and our exceedingly thin atmosphere forms a protective blanket that keeps the surface temperature at more or less acceptable levels for a wild variety of life. Humans have no control over the former, but it's now crystal clear that maintenance of the latter is on us.

One of the many innovations that enabled humanity's rise to global dominance is the development of money, which is a measure of relative value. Humans put a price on almost everything, from tomatoes and houses to entertainment and information. Scientists and economists have even sought to put a value on services rendered by the global climate. More specifically, the 'social cost of carbon' (SCC) represents the hidden costs from climate impacts such as extreme weather, declining crop yields and rising sea levels. At present, the US government's central estimate of this is US\$36 per tonne of carbon dioxide.

It might seem arcane, but the administration of US President Barack Obama has used this metric to estimate the costs and the benefits of a wide variety of government decisions that affect greenhouse-gas emissions, including climate regulations targeted at the fossil-fuel industry. Despite obvious challenges in settling on a single number, this figure is a way of accounting for the far-reaching impacts of decisions. The social

cost of carbon is also a target for the incoming administration of president-elect Donald Trump, and his operatives have hardly hidden their animosity towards the idea. In its notorious, and eventually disavowed, memorandum to the US Department of Energy, the Trump transition team sought the names of employees who attended meetings related to the social cost of carbon. And in a memo last November, issued before former industry lobbyist Thomas Pyle was selected to lead Trump's transition team for the same department, Pyle said that Trump could seek to limit the use of the social cost of carbon in federal rule-making.

That could be difficult given that US courts have both ordered and upheld the metric's use in the past, but Pyle also said that the Trump administration will push for a review of the underlying science. "If the SCC were subjected to the latest science, it would certainly be much lower than what the Obama administration has been using," Pyle wrote.

This is simply not true. Some have indeed argued that it is too high, but numerous studies have concluded that the price tag for damages is too low — perhaps much too low. Indeed, a 2014 meta-analysis suggested a floor of \$125 (J. C. J. M. van den Bergh and W. J. W. Botzen *Nature Clim. Change* 4, 253–258; 2014).

Last week, the US National Academies of Science, Engineering, and Medicine weighed in with a lengthy report. It did not name a specific price, but it did outline a series of technical recommendations intended to tap the latest research to bolster the underlying science and to increase transparency. The panel also recommended establishing a process for updating the carbon price every five years.

The recommendations are timely. Most important of all, given the disregard for science that Trump and his political appointees have shown so far, is the call for transparency. There is, of course, plenty of room for debate. But any useful assessment of the carbon price must take into account the full range of scientific and economic research, and not bend to the political proclivities of those in charge. ■