



# FOOLING OURSELVES

**HUMANS ARE REMARKABLY GOOD AT SELF-DECEPTION.  
BUT GROWING CONCERN ABOUT REPRODUCIBILITY IS DRIVING MANY  
RESEARCHERS TO SEEK WAYS TO FIGHT THEIR OWN WORST INSTINCTS.**

BY REGINA NUZZO

In 2013, five years after he co-authored a paper showing that Democratic candidates in the United States could get more votes by moving slightly to the right on economic policy<sup>1</sup>, Andrew Gelman, a statistician at Columbia University in New York City, was chagrined to learn of an error in the data analysis. In trying to replicate the work, an undergraduate student named Yang Yang Hu had discovered that Gelman had got the sign wrong on one of the variables.

Gelman immediately published a three-sentence correction, declaring that everything in the paper's crucial section should be considered wrong until proved otherwise.

ILLUSTRATION BY DALE EDWIN MURRAY

Reflecting today on how it happened, Gelman traces his error back to the natural fallibility of the human brain: “The results seemed perfectly reasonable,” he says. “Lots of times with these kinds of coding errors you get results that are just ridiculous. So you know something’s got to be wrong and you go back and search until you find the problem. If nothing seems wrong, it’s easier to miss it.”

This is the big problem in science that no one is talking about: even an honest person is a master of self-deception. Our brains evolved long ago on the African savannah, where jumping to plausible conclusions about the location of ripe fruit or the presence of a predator was a matter of survival. But a smart strategy for evading lions does not necessarily translate well to a modern laboratory, where tenure may be riding on the analysis of terabytes of multidimensional data. In today’s environment, our talent for jumping to conclusions makes it all too easy to find false patterns in randomness, to ignore alternative explanations for a result or to accept ‘reasonable’ outcomes without question — that is, to ceaselessly lead ourselves astray without realizing it.

Failure to understand our own biases has helped to create a crisis of confidence about the reproducibility of published results, says statistician John Ioannidis, co-director of the Meta-Research Innovation Center at Stanford University in Palo Alto, California. The issue goes well beyond cases of fraud. Earlier this year, a large project that attempted to replicate 100 psychology studies managed to reproduce only slightly more than one-third<sup>2</sup>. In 2012, researchers at biotechnology firm Amgen in Thousand Oaks, California, reported that they could replicate only 6 out of 53 landmark studies in oncology and haematology<sup>3</sup>. And in 2009, Ioannidis and his colleagues described how they had been able to fully reproduce only 2 out of 18 microarray-based gene-expression studies<sup>4</sup>.

Although it is impossible to document how often researchers fool themselves in data analysis, says Ioannidis, findings of irreproducibility beg for an explanation. The study of 100 psychology papers is a case in point: if one assumes that the vast majority of the original researchers were honest and diligent, then a large proportion of the problems can be explained only by unconscious biases. “This is a great time for research on research,” he says. “The massive growth of science allows for a massive number of results, and a massive number of errors and biases to study. So there’s good reason to hope we can find better ways to deal with these problems.”

“When crises like this issue of reproducibility come along, it’s a good opportunity to advance our scientific tools,” says Robert MacCoun, a social scientist at Stanford. That has happened before, when scientists in the mid-twentieth century realized that experimenters and subjects often unconsciously changed their behaviour to match expectations. From that insight, the double-blind standard was born.

“People forget that when we talk about the scientific method, we don’t mean a finished product,” says Saul Perlmutter, an astrophysicist at the University of California, Berkeley. “Science is an ongoing race between our inventing ways to fool ourselves, and our inventing ways to avoid fooling ourselves.” So researchers are trying a variety of creative ways to debias data analysis — strategies that involve collaborating with academic rivals, getting papers accepted before the study has even been started and working with strategically faked data.

## “SCIENCE IS AN ONGOING RACE BETWEEN OUR INVENTING WAYS TO FOOL OURSELVES, AND OUR INVENTING WAYS TO AVOID FOOLING OURSELVES.”

the non-profit Center for Open Science in Charlottesville, Virginia, which works to increase the transparency and reproducibility of scientific research. Today’s academic environment is more competitive than ever. There is an emphasis on piling up publications with statistically significant results — that is, with data relationships in which a commonly used measure of statistical certainty, the  $p$ -value, is 0.05 or less. “As a researcher, I’m not trying to produce misleading results,” says Nosek. “But I do have a stake in the outcome.” And that gives the mind excellent motivation to find what it is primed to find.

Another reason for concern about cognitive bias is the advent of staggeringly large multivariate data sets, often harbouring only a faint signal in a sea of random noise. Statistical methods have barely caught up with such data, and our brain’s methods are even worse, says Keith Baggerly, a statistician at the University of Texas MD Anderson Cancer Center in Houston. As he told a conference on challenges in bioinformatics last September in Research Triangle Park, North Carolina, “Our intuition when we start looking at 50, or hundreds of, variables sucks.”

Andrew King, a management specialist at Dartmouth College in Hanover, New Hampshire, says that the widespread use of point-and-click data-analysis software has made it easy for researchers to sift through massive data sets without fully understanding the methods, and to find small  $p$ -values that may not actually mean anything. “I believe we are in the steroids era of social science,” he says. “I’ve been guilty of using some of these performance-enhancing practices myself. My sense is that most researchers have fallen at least once.”

Just as in competitive sport, says Hal Pashler, a psychologist at the University of California, San Diego, this can set up a vicious circle of chasing increasingly better results. When a few studies in behavioural neuroscience started reporting improbably strong correlations of 0.85, Pashler says, researchers who had more moderate (and plausible) results started to worry: “Gee, I just got a 0.4, so maybe I’m not really doing this very well.”

### HYPOTHESIS MYOPIA

One trap that awaits during the early stages of research is what might be called hypothesis myopia: investigators fixate on collecting evidence to support just one hypothesis; neglect to look for evidence against it; and fail to consider other explanations. “People tend to ask questions that give ‘yes’ answers if their favoured hypothesis is true,” says Jonathan Baron, a psychologist at the University of Pennsylvania in Philadelphia.

For example, says Baron, studies have tried to show how disgust influences moral condemnation, “by putting the subject in a messy room, or a room with ‘fart spray’ in the air”. The participants are then asked to judge how to respond to moral transgressions; if those who have been exposed to clutter or smells favour harsher punishments, researchers declare their ‘disgust hypothesis’ to be supported<sup>5</sup>. But they have not considered competing explanations, he says, and so they ignore the possibility that participants are lashing out owing to anger at their foul treatment, not simply disgust. By focusing on one hypothesis, researchers might be missing the real story entirely.

Courtrooms face a similar problem. In 1999, a woman in Britain called Sally Clark was found guilty of murdering two of her sons, who had died suddenly as babies. A factor in her conviction was the presentation of statistical evidence that the chances of two children in the same family dying of sudden infant death syndrome (SIDS) were only 1 in 73 million — a figure widely interpreted as fairly damning. Yet considering just one hypothesis leaves out an important part of the story. “The jury needs to weigh up two competing explanations for the babies’ deaths: SIDS or murder,” wrote statistician Peter Green on behalf of the Royal Statistical Society in 2002 (see [go.nature.com/ochsja](http://go.nature.com/ochsja)). “The

## THE PROBLEM

Although the human brain and its cognitive biases have been the same for as long as we have been doing science, some important things have changed, says psychologist Brian Nosek, executive director of

fact that two deaths by SIDS is quite unlikely is, taken alone, of little value. Two deaths by murder may well be even more unlikely. What matters is the relative likelihood of the deaths under each explanation, not just how unlikely they are under one explanation.” Mathematician Ray Hill of the University of Salford, UK, later estimated<sup>6</sup> that a double SIDS death would occur in roughly 1 out of 297,000 families, whereas two children would be murdered by a parent in roughly 1 out of 2.7 million families — a likelihood ratio of 9 to 1 against murder. In 2003, Clark’s conviction was overturned on the basis of new evidence. The Attorney General for England and Wales went on to release two other women who had been convicted of murdering their children on similar statistical grounds.

## THE TEXAS SHARPSHOOTER

A cognitive trap that awaits during data analysis is illustrated by the fable of the Texas sharpshooter: an inept marksman who fires a random pattern of bullets at the side of a barn, draws a target around the biggest clump of bullet holes, and points proudly at his success.

His bullseye is obviously laughable — but the fallacy is not so obvious to gamblers who believe in a ‘hot hand’ when they have a streak of wins, or to people who see supernatural significance when a lottery draw comes up as all odd numbers.

Nor is it always obvious to researchers. “You just get some encouragement from the data and then think, well, this is the path to go down,” says Pashler. “You don’t realize you had 27 different options and you picked the one that gave you the most agreeable or interesting results, and now you’re engaged in something that’s not at all an unbiased representation of the data.”

Psychologist Uri Simonsohn at the University of Pennsylvania, gives an explicit nod to this naivety in his definition of ‘*p*-hacking’: “Exploiting — perhaps unconsciously — researcher degrees of freedom until  $p < 0.05$ .” In 2012, a study of more than 2,000 US psychologists<sup>7</sup> suggested how common *p*-hacking is. Half had selectively reported only studies that ‘worked’, 58% had peeked at the results and then decided whether to collect more data, 43% had decided to throw out data only after checking its impact on the *p*-value and 35% had reported unexpected findings as having been predicted from the start, a practice that psychologist Norbert Kerr of Michigan State University in East Lansing has called HARKing, or hypothesizing after results are known. Not only did the researchers admit to these *p*-hacking practices, but they defended them.

This May, a journalist described how he had teamed up with a German documentary filmmaker and demonstrated that creative *p*-hacking, carried out over one “beer-fueled” weekend, could be used to ‘prove’ that eating chocolate leads to weight loss, reduced cholesterol levels and improved well-being (see [go.nature.com/blkpke](http://go.nature.com/blkpke)). They gathered 18 different measurements — including weight, blood protein levels and sleep quality — on 15 people, a handful of whom had eaten some extra chocolate for a few weeks. With that many comparisons, the odds were better than 50–50 that at least one of them would look statistically significant just by chance. As it turns out, three of them did — and the team cherry-picked only those to report.

## ASYMMETRIC ATTENTION

The data-checking phase holds another trap: asymmetric attention to detail. Sometimes known as disconfirmation bias, this happens when we give expected results a relatively free pass, but we rigorously check non-intuitive results. “When the data don’t seem to match previous estimates, you think, ‘Oh, boy! Did I make a mistake?’” MacCoun

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says. “We don’t realize that probably we would have needed corrections in the other situation as well.”

The evidence suggests that scientists are more prone to this than one would think. A 2004 study<sup>8</sup> observed the discussions of researchers from 3 leading molecular-biology laboratories as they worked through 165 different lab experiments. In 88% of cases in which results did not align with expectations, the scientists blamed the inconsistencies on how the experiments were conducted, rather than on their own theories. Consistent results, by contrast, were given little to no scrutiny.

In 2011, an analysis of over 250 psychology papers found<sup>9</sup> that more than 1 in 10 of the *p*-values was incorrect — and that when the errors were big enough to change the statistical significance of the result, more than 90% of the mistakes were in favour of the researchers’ expectations, making a non-significant finding significant.

## JUST-SO STORYTELLING

As data-analysis results are being compiled and interpreted, researchers often fall prey to just-so storytelling — a fallacy named after the Rudyard Kipling tales that give whimsical explanations for things such as how the leopard got its spots. The problem is that post-hoc stories can be concocted to justify anything and everything — and so end up truly explaining nothing. Baggerly says that he has seen such stories in genetics studies, when an analysis implicates a huge number of genes in a particular trait or outcome. “It’s akin to a Rorschach test,” he said at the bioinformatics conference. Researchers will find a story, he says, “whether it’s there or not. The problem is that occasionally it ain’t real.”

Another temptation is to rationalize why results should have come up a certain way but did not — what might be called JARKing, or justifying after results are known. Matthew Hankins, a statistician at King’s College London, has collected more than 500 creative phrases that researchers use to convince readers that their non-significant results are worthy of attention (see [go.nature.com/pwctoq](http://go.nature.com/pwctoq)). These include “flirting with conventional levels of significance ( $p > 0.1$ )”, “on the very fringes of significance ( $p = 0.099$ )” and “not absolutely significant but very probably so ( $p > 0.05$ )”.

# THE SOLUTIONS

In every one of these traps, cognitive biases are hitting the accelerator of science: the process of spotting potentially important scientific relationships. Countering those biases comes down to strengthening the ‘brake’: the ability to slow down, be sceptical of findings and eliminate false positives and dead ends.

One solution that is piquing interest revives an old tradition: explicitly considering competing hypotheses, and if possible working to develop experiments that can distinguish between them. This approach, called strong inference<sup>10</sup>, attacks hypothesis myopia head on. Furthermore, when scientists make themselves explicitly list alternative explanations for their observations, they can reduce their tendency to tell just-so stories.

In 2013, researchers reported<sup>11</sup> using strong-inference techniques in a study of what attracts female túngara frogs (*Engystomops pustulosus*) during mating calls. The existing data could be explained equally well by two competing theories — one in which females have a preset neural template for mating calls, and another in which they flexibly combine auditory cues and visual signals such as the appearance of the males’ vocal sacs. So the researchers developed an experiment for which the

two theories had opposing predictions. The results showed that females can use multi-sensory cues to judge attractiveness.

## TRANSPARENCY

Another solution that has been gaining traction is open science. Under this philosophy, researchers share their methods, data, computer code and results in central repositories, such as the Center for Open Science's Open Science Framework, where they can choose to make various parts of the project subject to outside scrutiny. Normally, explains Nosek, "I have enormous flexibility in how I analyse my data and what I choose to report. This creates a conflict of interest. The only way to avoid this is for me to tie my hands in advance. Precommitment to my analysis and reporting plan mitigates the influence of these cognitive biases."

An even more radical extension of this idea is the introduction of registered reports: publications in which scientists present their research plans for peer review before they even do the experiment. If the plan is approved, the researchers get an 'in-principle' guarantee of publication, no matter how strong or weak the results turn out to be. This should reduce the unconscious temptation to warp the data analysis, says Pashler. At the same time, he adds, it should keep peer reviewers from discounting a study's results or complaining after results are known. "People are evaluating methods without knowing whether they're going to find the results congenial or not," he says. "It should create a much higher level of honesty among referees." More than 20 journals are offering or plan to offer some format of registered reports.

## TEAM OF RIVALS

When it comes to replications and controversial topics, a good debiasing approach is to bypass the typical academic back-and-forth and instead invite your academic rivals to work with you. An adversarial collaboration has many advantages over a conventional one, says Daniel Kahneman, a psychologist at Princeton University in New Jersey. "You need to assume you're not going to change anyone's mind completely," he says. "But you can turn that into an interesting argument and intelligent conversation that people can listen to and evaluate." With competing hypotheses and theories in play, he says, the rivals will quickly spot flaws such as hypothesis myopia, asymmetric attention or just-so storytelling, and cancel them out with similar slants favouring the other side.

Psychologist Eric-Jan Wagenmakers of the University of Amsterdam has engaged in this sort of proponent-sceptic collaboration, when he teamed up with another group in an attempt<sup>12</sup> to replicate its research suggesting that horizontal eye movements help people to retrieve events from their memory. It is often difficult to get researchers whose original work is under scrutiny to agree to this kind of adversarial collaboration, he says. The invitation is "about as attractive as putting one's head on a guillotine — there is everything to lose and not much to gain". But the group that he worked with was eager to get to the truth, he says. In the end, the results were not replicated. The sceptics remained sceptical, and the proponents were not convinced by a single failure to replicate. Yet this was no stalemate. "Although our adversarial collaboration has not resolved the debate," the researchers wrote, "it has generated new testable ideas and has brought the two parties slightly closer." Wagenmakers suggests several ways in which this type of collaboration could be encouraged, including a prize for best adversarial collaboration, or special sections for such collaborations in top journals.

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## BLIND DATA ANALYSIS

One debiasing procedure has a solid history in physics but is little known in other fields: blind data analysis (see page 187). The idea is that researchers who do not know how close they are to desired results will be less likely to find what they are unconsciously looking for<sup>13</sup>.

One way to do this is to write a program that creates alternative data sets by, for example, adding random noise or a hidden offset, moving participants to different experimental groups or hiding demographic categories. Researchers handle the fake data set as usual — cleaning the data, handling outliers, running

analyses — while the computer faithfully applies all of their actions to the real data. They might even write up the results. But at no point do the researchers know whether their results are scientific treasures or detritus. Only at the end do they lift the blind and see their true results — after which, any further fiddling with the analysis would be obvious cheating.

Perlmutter used this method for his team's work on the Supernova Cosmology Project in the mid-2000s. He knew that the potential for the researchers to fool themselves was huge. They were using new techniques to replicate estimates of two crucial quantities in cosmology — the relative abundances of matter and of dark energy — which together reveal whether the Universe will expand forever or eventually collapse into a Big Crunch. So their data were shifted by an amount known only to the computer, leaving them with no idea what their findings implied until everyone agreed on the analyses and the blind could be safely lifted. After the big reveal, not only were the researchers pleased to confirm earlier findings of an expanding Universe<sup>14</sup>, Perlmutter says, but they could be more confident in their conclusions. "It's a lot more work in some sense, but I think it leaves you feeling much safer as you do your analysis," he says. He calls blind data analysis "intellectual hygiene, like washing your hands".

Data blinding particularly appeals to young researchers, Perlmutter says — not least because of the sense of suspense it gives. He tells the story of a recent graduate student who had spent two years under a data blind as she analysed pairs of supernova explosions. After a long group meeting, Perlmutter says, the student presented all her analyses and said that she was ready to unblind if everyone agreed.

"It was 6 o'clock in the evening and time for dinner," says Perlmutter. And everyone in the audience said, "If the result comes out wrong, it's going to be a very disappointing evening, and she's going to have to think really hard about what she's going to do with her PhD thesis. Maybe we should wait until morning."

"And we all looked at each other, and we said, 'Nah! Let's unblind now!' So we unblinded, and the results looked great, and we all cheered and applauded." ■ [SEE COMMENT P.187 & P.189](#)

*Regina Nuzzo is a freelance writer in Washington DC.*

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