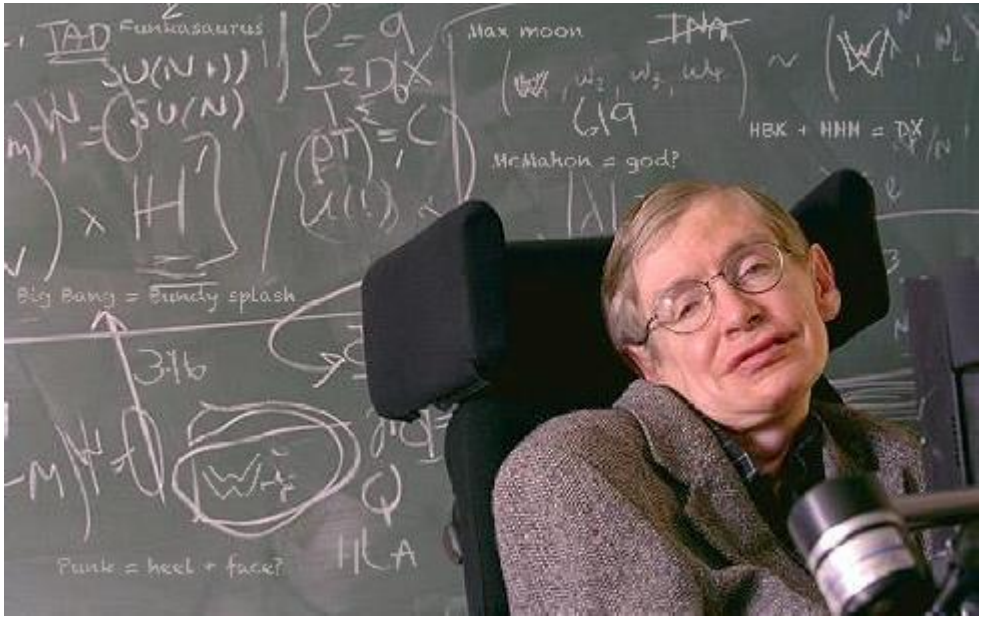
Science and Authority

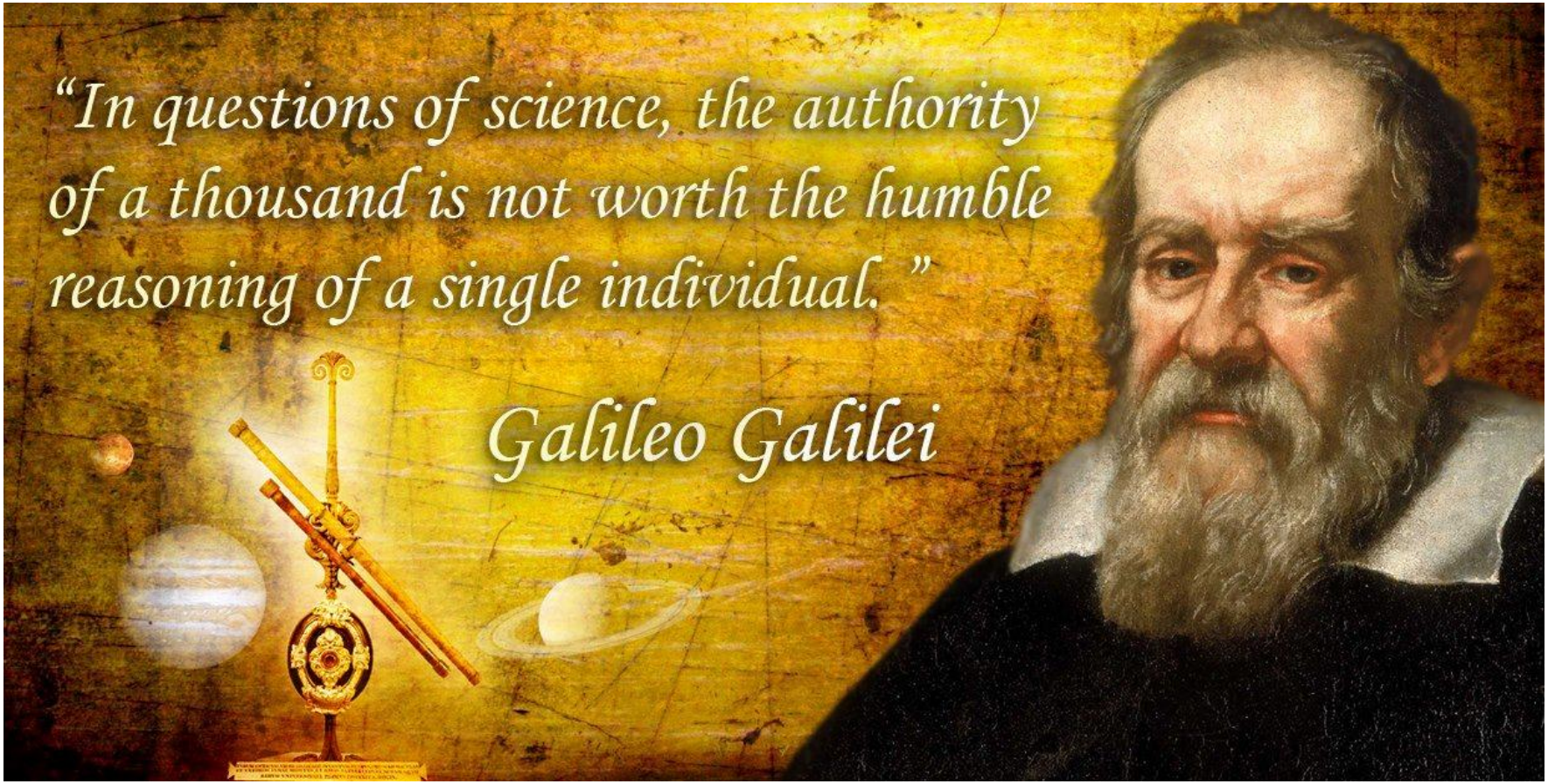
Are scientists ever told what to think?

(Should they be?)

“There is a fundamental difference between religion, which is based on authority, and science, which is based on observation and reason.”

Stephen Hawking




"In questions of science, the authority of a thousand is not worth the humble reasoning of a single individual."
Galileo Galilei

Kuhn on whether scientists think for themselves

"At least in the mature sciences, [the paradigm is] firmly embedded in the educational initiation that prepares and licenses the student for professional practice. Because that education is both rigorous and rigid, [the paradigm comes] to exert a deep hold on the scientific mind. … we shall want finally to describe that research as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education"

(Kuhn, *Structure*, pp. 4-5)

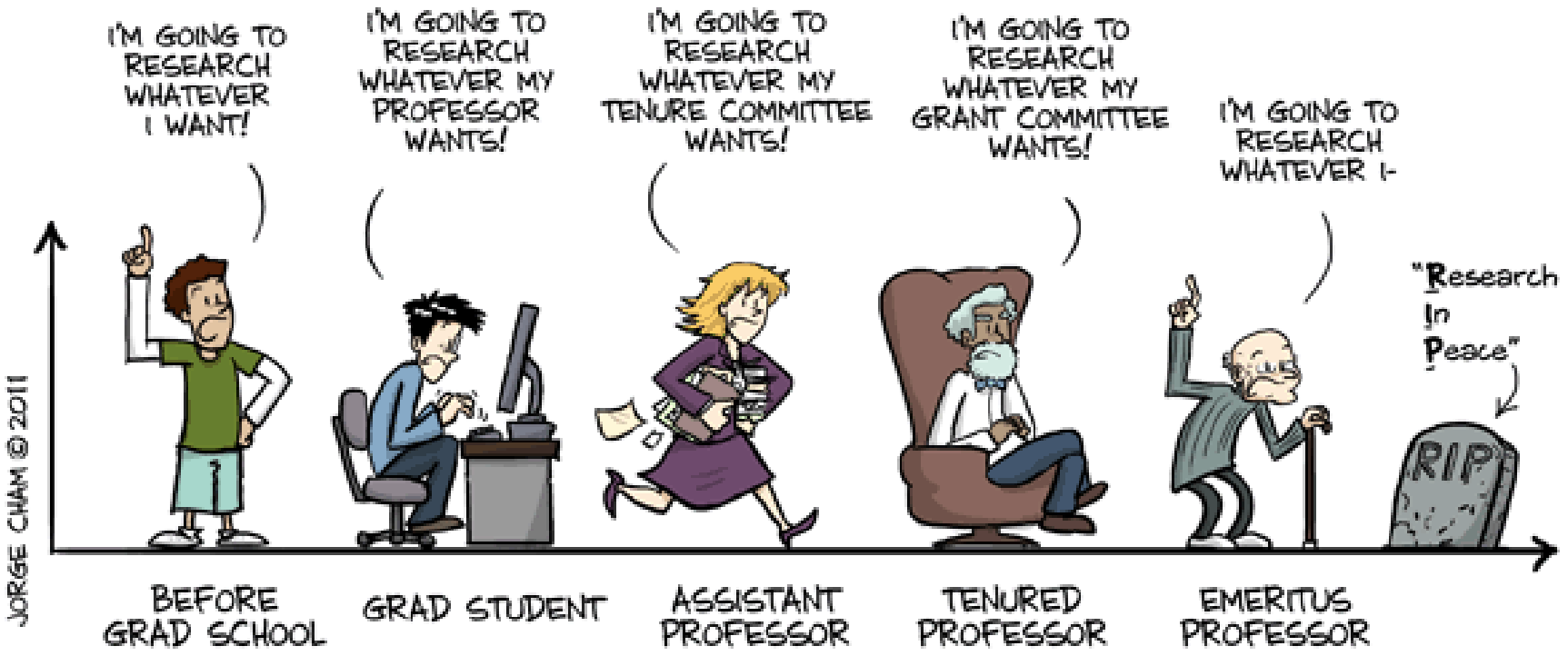
"Normal science, the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost. Normal science, for example, *often suppresses fundamental novelties* because they are necessarily subversive of its basic commitments."

(Kuhn, *Structure*, p. 5. Kuhn goes on to say that such novelties cannot be suppressed for very long, and they can lead to a scientific revolution or paradigm shift.)

Remember Kuhn's duck-rabbit metaphor?

Should Alice Grant be allowed to investigate the PCI?
Should 'rabbitism' be tolerated in the Faculty of Science?

THE EVOLUTION OF INTELLECTUAL FREEDOM

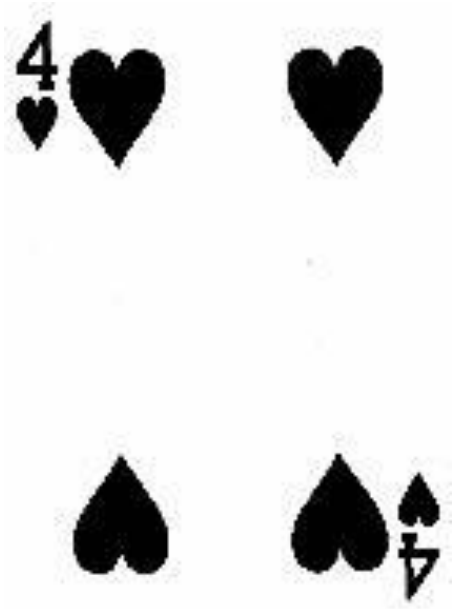


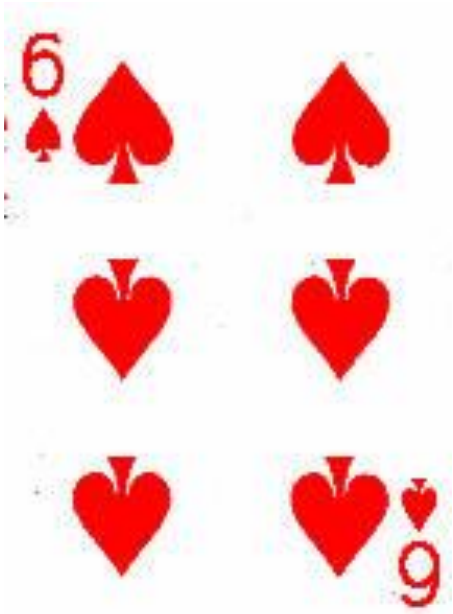
WWW.PHDCOMICS.COM

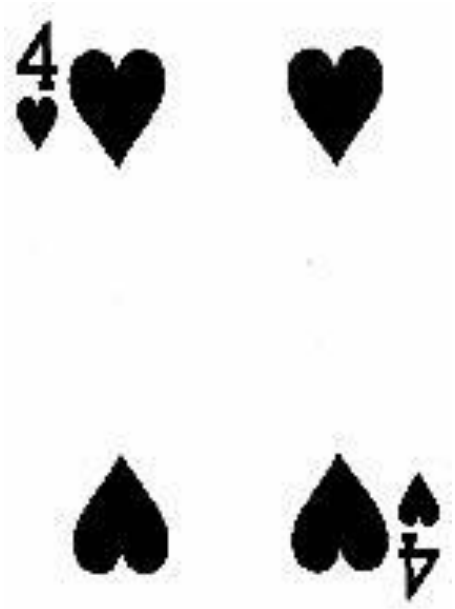
Bruner and Postman's Experiment

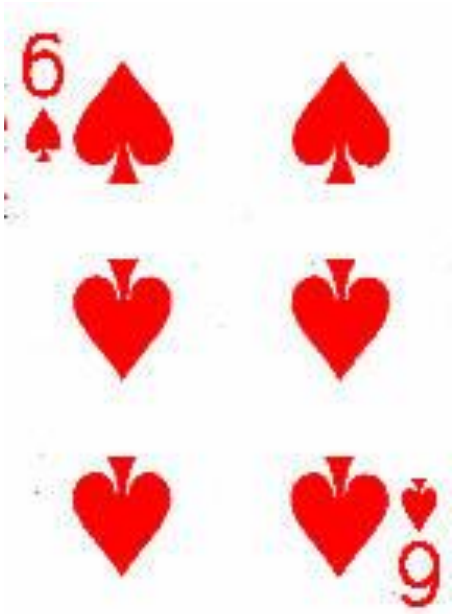
Kuhn illustrates how a paradigm exerts a strong influence on thought and perception by citing a psychological experiment.

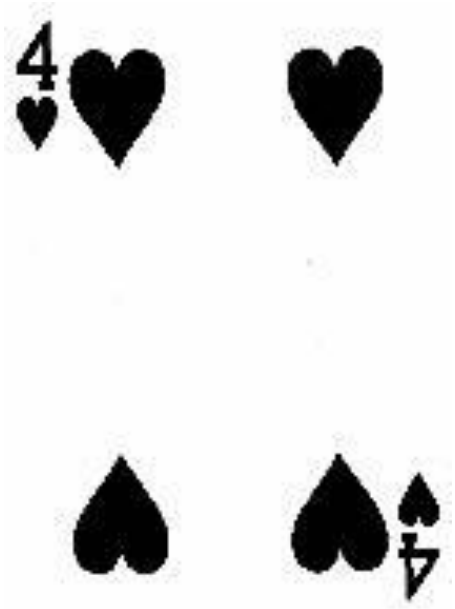
"B+P asked experimental subjects to identify on short and controlled exposure a series of playing cards. …

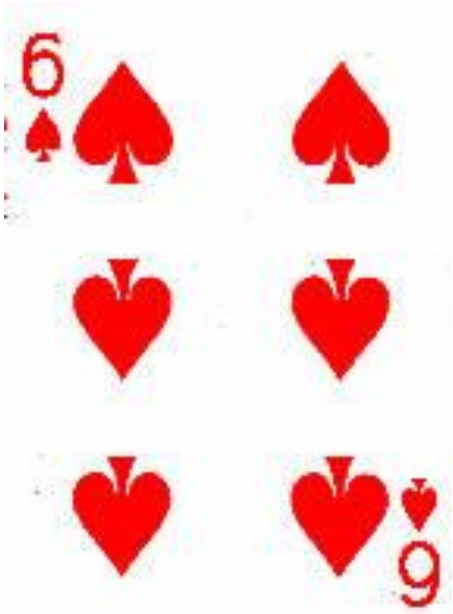

4
4


6
9


4
4


6
9


4
4


6
9

Bruner and Postman's Experiment

"… Many of the cards were normal, but some were made anomalous, e.g. a red six of spades and a black four of hearts. … After each exposure the subject was asked what he had seen …

…the anomalous cards were almost always identified, without apparent hesitation or puzzlement, as normal. The black four of hearts, for example, might be identified as the four of either spades or hearts. …

… With a further increase of exposure to the anomalous cards, subjects did begin to hesitate and to display awareness of anomaly. … “That’s the six of spades, but there’s something wrong with it—the black has a red border”

Further exposure resulted in still more hesitation and confusion, until finally, and sometimes quite suddenly, most subjects would produce the correct identification without hesitation …

…Moreover, after doing this with two or three of the anomalous cards, they would have little further difficulty with the others. A few subjects, however, were never able to make the requisite adjustment of their categories … One of them exclaimed:

"I can't make the suit out, whatever it is. It didn't even look like a card that time. I don't know what color it is now or whether it's a spade or a heart. I'm not even sure now what a spade looks like. My God!" (p. 64)

Kuhn continues, "In the next section we shall occasionally see scientists behaving this way too."

- E.g. on pp. 83-84 Kuhn quotes Wolfgang Pauli, shortly before quantum mechanics emerged,

  “At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics”

Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself. That act reflects no on the paradigm but on the man. Inevitably he will be seen as "the carpenter who blames his tools".

(Kuhn, *Structure*, p. 79)

The geosynclinal theory is one of the great unifying principles of geology. In many ways its role in geology is similar to that of evolution that serves to integrate the many branches of biological sciences. The geosynclinal theory is of fundamental importance to sedimentation, petrology, geomorphology, ore deposits, structural geology, geophysics, and practically all the minor branches of geological science. Just as the doctrine of organic evolution is universally accepted among thinking biologists, so also the geosynclinal origin of the major mountain ranges is an established principle in geology.

Thomas Clark and Colin Stearn, *The Geological Evolution of North America: A Regional Approach to Historical Geology*, p.43 (Ronald Press, 1960)

Famous last words …

- (The geosynclinal theory of mountain formation was replaced by plate tectonics *just a few years later*!!!)

The Authority of Peer Review

- One very important scientific institution is the peer-reviewed scientific journal. This is a primary way for scientists to communicate with each other about their work.
- “Peer review” means that, before an article is published in the journal, it is read by two or three “peers” of the author(s), i.e. other, independent, scientists working in the same field. Each reviewer evaluates the article, and recommends either that it be published or not published.

The Authority of Peer Review

- Scientists are not paid to review articles, but do it as a ‘service to the profession’. It is supposed to be an objective, unbiased appraisal of the article.
- The purpose of peer review is to prevent the journals from getting clogged up with rubbish. Otherwise, the good science would get “drowned out” by the “noise” of bad science.

The Authority of Peer Review

Peer review performs other functions, however:

- The publication of an idea in a peer-reviewed journal gives it a stamp of authority, or seal of approval. Conversely, the failure of an idea to appear in a peer-reviewed journal renders it unworthy of consideration.
- The quality of a scientist is judged by the number of articles published in quality (peer-reviewed) journals.
- Decisions such as hiring, promotion, tenure and research funding are based largely on a scientist's publication record in peer-reviewed journals.

Criticisms

"We portray peer review to the public as a quasi-sacred process that helps to make science our most objective truth teller. But we know that the system of peer review is biased, unjust, unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish, and frequently wrong."

- Richard Horton, editor of the British medical journal *The Lancet*

“The interposition of editors and reviewers between authors and readers always raises the possibility that the intermediators may serve as gatekeepers. …

The peer review process may suppress dissent against "mainstream" theories. Reviewers tend to be especially critical of conclusions that contradict their own views, and lenient towards those that accord with them.

…ideas that harmonize with the established experts' are more likely to see print and to appear in premier journals than are iconoclastic or revolutionary ones ….”

(Wikipedia again. I know …)

Gaming the system

"Early in the game, I learned who most likely would be reviewing my paper for any given journal and I made certain I cited that reviewer's most recent papers. This ploy worked well for me as it has countless others. Publishing technical papers is a game and like any game, one must learn the rules..."

(a scientist writing on an email discussion)

Gaming the system

"This reminds me of a paper that was being reviewed by the boss of the lab I was in. He passed it around to see what people thought. I told him that I thought it was pretty poor. He said, 'Yeah I know. They cite us really well so I am going to accept it anyway.'"

(another scientist responding to that one)

Polanyi and Dissent in Science

Let's look at Michael Polanyi, "The Potential Theory of Adsorption: Authority in science has its uses and its dangers"

(*Science*, vol. 141, 13 September 1963)

- Polanyi published a paper in 1914, proposing a mechanism for a chemical process known as ‘adsorption’. The theory was called the “potential theory of adsorption”. It did fairly well at predicting the data.

- Unfortunately for Polanyi, at about the same time important discoveries were made concerning the forces that hold atoms together – electric forces. Polanyi's theory of adsorption made no sense *relative to* this new understanding of chemical bonds.
- Consequently, Polanyi's theory was rejected by the best physicists at the time.

"All this evidence seemed to bear out the picture of short-range electrical forces, or valences, originating at discrete points of the atomic lattice forming the wall—a picture which would render my theory of adsorption untenable."

- In 1930, Fritz London proposed a new theory of cohesive forces, based on quantum mechanics (developed in 1920-25).
- Relative to London's new (apparently correct) theory of chemical bonds, Polanyi's theory of adsorption made sense. The two theories fitted together very well.
- However, Polanyi *still* couldn't get anyone to believe his theory!

"It seems that by this time the opinion that my theory was false had hardened to a point where the reasons for which it had been rejected were forgotten. Hence my refutation of these objections had no effect."

- Polanyi retired from science in 1948, to become a philosopher of science. After that, his theory of adsorption slowly gained acceptance among scientists.

Polanyi's Conclusions

"Could this miscarriage of the scientific method have been avoided? I do not think so. **There must be at all times a predominantly accepted scientific view of the nature of things**, in the light of which research is jointly conducted by members of the community of scientists. A strong presumption that any evidence which contradicts this view is invalid must prevail. Such evidence has to be disregarded, even if it cannot be accounted for, in the hope that it will eventually turn out to be false or irrelevant."

The orthodoxy must be enforced

“The dangers of suppressing or disregarding evidence that runs counter to orthodox views about the nature of things are, of course, notorious, and they have often proved disastrous. Science guards against these dangers, up to a point, by allowing some measure of dissent from its orthodoxy. But scientific opinion has to consider and decide, at its own ultimate risk, how far it can allow such tolerance to go, if it is not to admit for publication so much nonsense that scientific journals are rendered worthless thereby.”

"I … insist on *acknowledgment* of the fact that **the scientific method is, and must be, disciplined by an orthodoxy which can permit only a limited degree of dissent**, and that such dissent is fraught with grave risks to the dissenter. I demand a clear *recognition* of this situation for the sake of our intellectual honesty as scientists, and I charge that this situation is not recognized today but is, on the contrary, obscured by current declarations about science."

(my emphasis)

e.g. Bertrand Russell

“The triumphs of science are due to the substitution of observation and inference for authority. Every attempt to revive authority in intellectual matters is a retrograde step. And it is part of the scientific attitude that the pronouncements of science do not claim to be certain, but only the most probable on the basis of present evidence.”

[This story] “makes me ponder the perils of a particular dangerous mode of scientific explanation. The physicists of the period from 1912 to 1930 considered it as **established beyond reasonable doubt** that only electrical forces could account for intramolecular attraction. Arguments for the insufficiency of this explanation were rejected as unscientific, because **no other principles of molecular interaction appeared conceivable**.”

[Recall the ‘Sherlock Holmes rule’ in IBE.]

“This reminds me of the impatience with which most biologists set aside today all the difficulties of the current selectionist theory of evolution, because no other explanation that can be accepted as scientific appears conceivable. This kind of argument, based on the absence of any alternative that is accepted as scientific, may often be valid, but it seems to me the most dangerous application of scientific authority.”

A Contrary View

"How Academia and Publishing are Destroying Scientific Innovation: A Conversation with Sydney Brenner", *King's Review*, Feb. 24, 2014.

Nobel Prize in Physiology or Medicine, 2002

- Beginning in the 1950s, Brenner worked in the 'Laboratory of Molecular Biology' (LMB) at Cambridge University. (The group included James Watson, Francis Crick and Fred Sanger).

"This young group of scientists, considered renegades at the time, made a series of successive revolutionary discoveries that ultimately led to the creation of a new field called molecular biology."

"… in the 1950s, the hypotheses generated at the LMB were dismissed as inconceivable nonsense"

"[Crick] gave the lecture and biochemists stood up in the audience and said this is completely ridiculous, because if there were twenty enzymes, we biochemists would have already discovered them."

Scientific training is a handicap!

"Biology got its main success by the importation of physicists that came into the field not knowing any biology ...

I think ignorance in science is very important. If you're like me and you know too much you can't try new things. I always work in fields of which I'm totally ignorant."

Science and Bureaucracy

Brenner argues that the breakthroughs of the LMB would be impossible in today's climate, where the bureaucratic systems that fund science wouldn't take a chance on such an unorthodox approach.

"I think it would have been difficult to keep going without the strong support we had from the Medical Research Council. I think they took a big gamble when they founded that little unit in the Cavendish."

Peer Review is hindering science

"And of course all the academics say we've got to have peer review. But I don't believe in peer review because I think it's very distorted and as I've said, it's simply a regression to the mean.

I think peer review is hindering science. In fact, I think it has become a completely corrupt system."

- Peer review is opposed to novel ideas and creativity.
- Brenner says science needs to allow researchers some freedom to oppose the orthodoxy, without losing their funding and access to journals.

The Case of “Climategate”

- In November 2009 thousands of emails were hacked (or perhaps leaked) from the Climate Research Unit at the University of East Anglia.
- Many emails, especially from CRU director Prof. Phil Jones, seemed to show academic misconduct.
- “The email correspondence also apparently showed that Prof Jones lobbied for the sacking of a journal editor who published papers that questioned the scientific consensus on global warming.” (*The Daily Telegraph*, Nov. 24 2009)

“Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal.”

Michael Mann, writing about the journal *Climate Research*, that had published articles sceptical of AGW.

Tim Ball, AGW sceptic:

- “What you’ve got here is confirmation of the small group of scientists … who were publishing together and also peer-reviewing each other’s literature.”

- “About twenty years ago, I started saying ‘Well why are they pushing the peer review?’… And now of course we realise it’s because they had control of their own process. That’s clearly exposed in these emails.”

The Case of ID: Sternberg and Meyer

- On 4 August 2004, an article by Stephen C. Meyer (Director of Discovery Institute's Center for Science and Culture) titled "The origin of biological information and the higher taxonomic categories", appeared in the peer-reviewed journal, *Proceedings of the Biological Society of Washington*, edited by Richard Sternberg.
- The paper cited numerous scientists saying that Darwinian (selectionist) mechanisms are unable to account for the appearance of the novel complex structures that emerged repeatedly during the evolution of life. Meyer then suggested, right at the end, that the hypothesis of an intelligent designer might account for these data.

“For this reason, recent scientific interest in the **design hypothesis** is unlikely to abate as biologists continue to wrestle with the problem of the origination of biological form and the higher taxa.”

(The last sentence of Meyer’s paper.)

Why did the editor publish it?

"Sternberg [the journal editor] harbored his own doubts about Darwinian theory. He also acknowledged that this journal had not published such papers in the past and that he wanted to stir the scientific pot.

"I am not convinced by intelligent design but they have brought a lot of difficult questions to the fore," Sternberg said. "Science only moves forward on controversy."

(*The Washington Post*, August 19, 2005.)

The reaction

"When the article appeared, the reaction was near instantaneous and furious. …

… A senior Smithsonian scientist wrote in an e-mail:

"We are evolutionary biologists and I am sorry to see us made into the laughing stock of the world, even if this kind of rubbish sells well in backwoods USA."

- Meyer's paper was withdrawn by the journal's publisher. Sternberg alleges that he was then "targeted for retaliation and harassment" at the Smithsonian Museum, where he was an (unpaid) research associate.