

 [IP--scientism by Maureen O'Hara](#) by Louise S. [2014, Feb 06]

Here it is Louise. I think this gets to the issue of scientism in a very interesting way.

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 [Scientific Pride and Prejudice. Michael Suk- young](#)

 [Comment by Kiran Kumar Salagame](#) by Louise S. [2014, Feb 06]

That's a very interesting article on scientism. All of us in the IP group are perhaps aware of the issue of confirmation bias. But those who have turned science into scientism have to become aware of this. Who will bell the cat?

Kiran

 [Comment by John Zhang](#) by Louise S. [2014, Feb 06]

it is a good article.

like in the political sytems, those with the power cannot help abusing it, in science, those under great pressure or striving for fame or grants, cannot help confirming their 'great' theories or finding. knowing the confirmation bias does not work effectively. efforst must be spent by the goverment or by the scientific community organizing independent research to replicate. Checks and balances need to be in place in science just as in political system. science itself is not the problem, we won't say the political system itself is the problem, the way science is run is problematic.

best,

john
张学新

 [Comment by Paul TP Wong](#) by Louise S. [2014, Feb 06]

I agree with John's analysis. Another important factor is money. Those in the privileged position of power also have easy access to grant money and elite journals. There is no such thing as neutral and objective scientific community to provide funding to test an alternative perspective (e.g., based on IP) or to test an "influential theory". I was on NIMH Biological and Behavioural Science panel for 5 years, evaluating hundreds of grant applications. Those big names from big schools get easy approval, while those from small school and yet to achieve fame, have a difficult time getting a grant, regardless how creative and good their proposals are. I had to stand up and fight for these "unknowns" to convince the whole panel to fund their projects.

The current biased system of peer review for funding and publication is largely responsible for the confirmation bias. The so called evidence-based scientific findings can be more accurately called money-based scientific findings, because those who have no access to grant money cannot do research to do independent and objective test of "influential theories".

Paul

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 [Comment by Ilene Serlin](#) by Louise S. [2014, Feb 06]

This is a series of links from mainstream medical journals that paint a self critical view of the field of medicine.

Perhaps looking at that, we can better see the vision that we want to create, key ideas and links below in 2 parts.

1. The Lancet has this month published an important collection of articles on waste in medical research.

The collection has grown from an article by Iain Chalmers and Paul Glasziou in which they argued that 85% of expenditure on medical research (\$240 billion in 2010) is wasted. In a very powerful talk at last year's peer review congress John Ioannidis showed that almost none of thousands of research reports linking foods to conditions are

correct and how around only 1% of thousands of studies linking genes with diseases are reporting linkages that are real. His famous paper “Why most published research findings are false” continues to be the most cited paper of PLoS Medicine.

Ioannidis’ s conclusion as to why so much research is poor is similar to that of Altman’ s: “Most scientific studies are wrong, and they are wrong because scientists are interested in funding and careers rather than truth.” Researchers are publishing studies that are too small, conducted over too short a time, and too full of bias in order to get promoted and secure future funding. An editorial in the Lancet collection on waste in research quotes 2013 Nobel Laureate Peter Higgs describing how he was an embarrassment to his Edinburgh University department because he published so little. “Today,” he said, “I wouldn’ t get an academic job. It’ s as simple as that. I don’ t think I would be regarded as productive enough.” Producing lots of flawed research trumps a few studies that change our understanding of the world, as Higgs’ s paper did.

2. Jan 31, an article in the British Medical Journal (BMJ) discusses the state of medicine 20 years following

the most sighted article in medicine, an editorial by Doug Altman. “Sadly, the BMJ could publish this editorial almost unchanged again this week.” (quote here and below are from the article)

Twenty years ago this week the statistician Doug Altman published an editorial in the BMJ arguing that much medical research was of poor quality and misleading. In his editorial entitled, “The Scandal of Poor Medical Research,” Altman wrote that much research was “seriously flawed through the use of inappropriate designs, unrepresentative samples, small samples, incorrect methods of analysis, and faulty interpretation.” Twenty years later I fear that things are not better but worse.

Most editorials like most of everything, including people, disappear into obscurity very fast, but Altman’ s editorial is one that has lasted.

Why, asked Altman, is so much research poor? Because “researchers feel compelled for career reasons to carry out research that they are ill equipped to perform, and nobody stops them.” In other words, too much medical research was conducted by amateurs who were required to do some research in order to progress in their medical careers.

Ethics committees, who had to approve research, were ill equipped to detect scientific flaws, and the flaws were eventually detected by statisticians, like Altman, working as firefighters. Quality assurance should be built in at the beginning of research not the end, particularly as many journals lacked statistical skills and simply went ahead and published misleading research.

“The poor quality of much medical research is widely acknowledged,” wrote Altman, “yet disturbingly the leaders of the medical profession seem only minimally concerned about the problem and make no apparent efforts to find a solution.”

Altman’s conclusion was: “We need less research, better research, and research done for the right reasons. Abandoning using the number of publications as a measure of ability would be a start.”

Ilene Serlin PhD

 [Comment by John Zhang](#) by Louise S. [2014, Feb 08]

Thank you, Ilene! Even if 50% of the reserach money were wasted, it would already be very shocking. However, the leaders may say, what else can we do? do you have any better plans? many of us see the serious problems of the current academic system, more and more driven by capitals, but do we really have an altertive system? just as we see capitalism and crazy consumption lead to great damages to the enviroment, do we have a way out?

best,
john
张学新

 [Comment by k.k. hwang](#) by Louise S. [2014, Feb 09]

Dear Colleagues:

The issue of scientism is a very important reason for non-western countries to develop their Indigenous social science.

As you may remember, I circulate a series of article attacking Dr. Ovid J. L. Tzeng
Who published a 4-page article in Nature(1979) and claimed that Western model of

Visual lateralization effect can be applied in reading Chinese characters.

This article brought him big fame. He became prime minister of education in Taiwan

utilized his power to buy fMRI machines for several universities, and sought collaborators to publish papers in international journals by mass production.

But, one of his students conducted 11 experiments and found that his research published in Nature(1979) could not be replicated. She published her findings in both local and international journals, but Dr. Tzeng made no response to her critique.

Some of his colleagues also conducted researches on the same issue and got the same conclusion. But Dr. Tzeng ignored them.

I published more than ten articles in local newspapers to address on this issue,

Dr. Tzeng is still playing dumb to my critique.

Now he is still the President of United System of Four Universities in Taiwan.

Can you image the story? Is it very popular in non-Western countries? What we can do for that?

Best regards,

K. K. Hwang

 [Comment by Kiran Kumar Salagame](#) by Louise S. [2014, Feb 09]

Hwang, and all,

It is really difficult to change the rules of the ballgame called science, because it has resulted in so much of technological innovations helping the humanity in so many ways. So we cannot easily make a dent, even if so much of wasteful research is going on in every field. The latest bogey raised is so called impact factor. It is adding to the scientism, rather than science.

As the saying goes, "knowledge is power" and for people like Dr. Tzeng

and others research knowledge is a matter of securing power, not arriving at truth.

The only thing we as a group can do, I think, is to highlight the relative significance of scientific knowledge in the larger context of life as understood in the indigenous psychologies, which speak of a higher purpose in life such as transcendence.

Apart from that we cannot, I think, do anything directly with the present trend of emphasizing on science and scientific research, good or bad.

Kiran

 [Comment by Paul Wong](#) by Louise S. [2014, Feb 09]

 Dear KK:

In the West, even very famous professors have been forced to retract their publications that cannot be replicated and sometime forced to resign.

A better strategy may be to bring Dr. Tzeng's case to professional societies of which he is a member and demands them to take action of severe ethical violation. You need to have the support of other researchers who cannot replicate his findings.

Paul

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 [Comment by Louise Sundararajan](#) by Louise S. [2014, Feb 09]

 K. K. Hwang and All,

Paul has a good point. Dr. Tzeng received an award from APS. You may want to send APS the failure to replicate findings, asking them to retract their award.

For those who can read Chinese, please find attached Professor Hwang's critiques of Tzeng's self-colonization.

Enjoy,

Louise

 [Comment by Ruth A McConnell](#) by Louise S. [2014, Feb 09]

A very valid point, Paul! Which confirms why CBT is the most evidence-based therapeutic modality in the northern hemisphere!

Kind regards

Ruth

Ruth A McConnell

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 [Comment by John Zhang](#) by Louise S. [2014, Feb 09]

It would be great if it works out this way! There are also similar psychologists like Tzeng in mainland China, using 1 or 2 Science paper to get to the top but are not real a real researcher at all.

best,

john

张学新

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 [Comment by Paul TP Wong](#) by Louise S. [2014, Feb 09]

Dear John:

It will work if KKHwang brings his case to the international professional societies of which Tzeng is a member and the journal editors in the US which have published his famous but unreplicable papers. Right now, editors are very sensitive of making such mistakes and they are willing to retract such papers. Another way is to solicit support from famous international psychologists in Tzeng's field of specialty.

Paul

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 [Comment by Paul TP Wong to John](#) by Louise S. [2014, Feb 09]

Dear John:

I suggest that you may want to consult James Coyne regarding "open review". Here is a link to his blog. I hope that it will be helpful to our IP group, which has so many disadvantages with the antiquated peer review system.

<http://blogs.plos.org/mindthebrain/2013/10/22/join-pubmeds-revolution-in-post-publication-peer-review/>

Here is another example for kkhwang to consider. James Coyne's just post the following on Facebook to openly challenge Prof. Morrison:

This week I am going to be offering a wager to Professor Tony Morrison: I will contribute US\$500 to a bona fide UK or Irish charity if he and his co-authors can demonstrate that an effect size of 6.9 best characterizes the outcome of their clinical trial in Lancet. If he cannot produce such information or if he refuses the wager, I will seek a formal retraction. I am frustrated with the failure of Professor Morrison and his co-authors to respond to reasonable criticism of their work, which has so many important implications for what persons with severe mental illness believe are their effective treatment options.

I carefully have examined the Lancet paper and it appears that at the end of the intervention, there were no differences between the intervention and an inappropriate control group. Maybe I am missing something and will have to pay US\$500, but regardless, this will will be an interesting wager.

Please nominate your suggestions for a bona fide UK or Irish charity. I will leave to Professor Morrison to which charity he will want to contribute. I suppose I could offer to contribute more than US\$500 because of a confidence that I will not have to pay out. But I do not want to frighten off Professor Morrison and his colleagues from the wager.

In the interest of better science appearing in prestigious journals like Lancet.

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