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Contributor Well thank you, and thank you for inviting me. You didn't mention my other wonderful title which I told you, which is that I was Honorary Clinical Senior Lecturer without responsibility or stipend! (Laughter) Don't you think that's a fantastic appointment to hold? Anyway.

I should explain that I guess the reason I was invited is because, until a few months ago, I chaired an organisation called the Committee on Publication Ethics which started life as a sort of therapeutic or rather as a sort of group therapy for disaffected editors of medical journals who were very fed up with spending vast amounts of time dealing with cases that were problematic.

And for eight or nine years, it sort of bounced along with a couple of hundred members, until two years ago when a number of the larger academic publishers, Wiley-Blackwell, Elsevier, Taylor & Francis, Springer, signed up all of their journals to COPE and COPE now finds itself with a membership of well over 5,000 editors worldwide, covering – well – just about every possible discipline. But I'm going to concentrate on biomedicine because that's what I dealt with.

And just to give you an idea of the sort of things we dealt with, COPE would meet every three months. By COPE, I meant any editor who happened to want to come along, and cases would be presented and the editors between them would discuss how they advised the presenting editor how he or she might best deal with it. And this is a list of the cases in the first nine years that were formally discussed. There were lots of informal things as well. But it gives you an idea of the sort of issues that were troubling these editors.

And you might think falsification, fabrication, plagiarism, which tends to get shortened to FFP, was the biggest deal. But actually as you see, the greatest problem that editors felt they were facing were issues to do with either attempted or actual redundant or duplicate publication but also lots of problems with what might be called potentially unethical research or malpractice. So those are the sort of things that came across us.

Now, just to give you an idea, I wonder whether anybody in the room would like to take a guess at what these papers have in common? Okay, well, they're all lies. (Laughter) So it is perfectly possible to have published, in high impact journals, what looks like extremely important research and you might end up successfully bidding for a ten million dollar research grant on the basis of it and yet, the whole thing is based on fraud.

And this is Jon Sudbø, a Norwegian doctor and dentist who finally admitted to fraud after he published a paper in *The Lancet* in December 2006, which detailed what had happened to a large group of patients who were entered into a cancer registry in Norway. And it was to do with genetic analysis of a particular lesion on the tongue which might or might not become malignant.

And he claimed that he could determine, by his genetic studies on the cells of this lesion, whether that particular patient was destined to have malignant change or not. So terribly important. He also claimed to have demonstrated a way of preventing that. Unfortunately for him, his paper was read a few weeks later by the person at the Cancer Registry in Norway who actually controlled access to the register and she knew this man could not possibly have had access to the data of those patients, because if he had, it would have had to have come through her.

And bad luck for him being in a small country, she happened to be the sister of the Prime Minister. So things moved very fast. (Laughter) So, beware. If you ever have an administrator in your department called Mrs Brown or even Mrs Cameron, watch it! Interestingly, the Norwegians have restored him to dental practice. They reckon the north of Norway needs dentists more than the world needs honest researchers!

The investigation which was carried out by a team chaired by a Swedish epidemiologist found that of the 150 cases that he claimed to have reported, 69 of them were either duplicated or didn't exist. The ages of the patients that he described did not fit in with the raw data. He'd had no Research Ethics Committee approval, with no application, let alone approval for his research, no patient consent and they found that the data in The Lancet was invented and they ordered retraction of all of his papers.

And there are other rogues. [[Hendrik Sherden 0:05:02]] he was producing a paper every eight days at one point, which is pretty good isn't it? (Laughter) And you'd think somebody might have thought that was odd. You know about [[Lang 0:05:11]] in Korea who was perhaps a little unfortunate.

[[Eric Permon 0:05:13]] was even more unfortunate, an epidemiologist specialising in old age medicine who actually went to prison for research fraud. Well he's not so much for research fraud, but because he had entered fraudulent data on a research grant application to the NIH which is a federal crime and he was actually sentenced to 12 months and 1 day in prison. I don't know why it's 12 months and 1 day, there must be some reason.

Hans [[Werner Gottinger 0:05:38]] is a star. So far, so far, over 100 papers by Gottinger have been found to have been plagiarised from other people. He's an economist. And one of the interesting things about Gottinger who also appointed himself to a Chair in a university which he invented, but which nobody seemed to realise was invented, was actually, the address was his house. He called his house "The something or other University" which I think is wonderful. But somebody has actually plagiarised one of Gottinger's plagiarised papers. How about that?! (Laughter)

So, I don't think any of us would disagree with these concepts that honesty and integrity are essential if the public is to be protected, if science is to be validated and that all of those who are involved in the research and publication industry have to regard themselves as responsible. But it happens. It happens I guess because human beings are fallible and there's no reason to believe that researchers are any different from the rest of humanity. There will be amongst them some whose honesty and integrity is not up to the highest level.

But of course, journals exist for reasons and I speak as someone who edited and still edits an academic journal and I've edited academic journals for nearly 15 years. But of course, journals do other things other than enhance the database. They do that, they enhance seniority and income. They increase publisher's profits and of course, they might enhance pharmaceutical company profits. So there are all sorts of areas which might seek to tempt people.

How frequent is it? Well, it's difficult to know and there are no reliable figures as to how frequent is research misconduct. Probably the best analysis we have is one published this year in Plus 1 by [[Fornelli 0:07:32]] in which a large number of scientists working in the United States, who worked on federal programmes were asked, firstly, had they ever fabricated or falsified data. And they were told a definition of that and you will see that nearly 2% of those who answered the

questionnaire, anonymously of course, said “Yes they had” and a third of them admitted bending the rules.

Questionable research practices. You know, removing the odd outlier. Not publishing one particular statistic that didn’t seem to fit in with the rest or with what he or she would like to have shown.

It’s interesting that while 2% of people agreed they were crooks, 7 times that number thought that their colleagues were crooks, which I think is very intriguing. Not much trust amongst researchers it seems. Now, who knows, this is self-reported. No objective data to confirm whether this is correct or not but interestingly it does fit in with some other studies that have been done.

Here’s a very small one. It was conducted in the NHS in one particular region a few years ago and you will see that half of NHS consultants who were circulated – these were NHS consultants who also had academic appointments. Half of them reckoned they’d observed misconduct. Eleven of them admitted that they themselves had misbehaved in some way or another. That’s a worrying statistic.

It just shows that if you send questionnaires to people in Great Britain, they may just decide to take the piss out of the person sending them the questionnaire and so I don’t know whether it’s true that 35 of them are going to be dishonest in future, I just think they were fed up with being sent questionnaires. That was published in the Journal of Medical Ethics a few years ago.

Okay, so what sort of things? I told you that duplicated papers were at the top of our list. How common is duplication, how common is plagiarism? This was a recent analysis of 62,000 randomly chosen Medline citations and 0.04% of them didn’t have any shared authors, but by using plagiarism detection software, it was possible to show that there was a lot of highly similar material. So presumably, these had been plagiarised.

1.35% of this randomly selected group of citations did have shared authors but had very similar information. So presumably, that’s duplication. If you extrapolate that, one would say that there are 3,500 plagiarised and 117,500 duplicate papers floating round in the biomedical citations at the moment, which is a bit alarming. Of course, if you compare that with the tens of millions that there actually papers there are, maybe it isn’t as bad as all that. But it gives you some idea.

And that comes from an organisation at North Western University in the United States. They cleverly call themselves Déjà vu and they have developed software to find plagiarism, okay. Some of it of course is somewhat obvious. I was the editor of this bottom journal, [[?? 0:10:55]] Disease in Childhood and published this paper. Somebody else a month before, published that paper. Guess what? They were identical. Well, if you look carefully, you’ll see one bit that isn’t identical, namely an extra author. An author has been dropped out of the second paper. They obviously must have offended the plagiariser somewhat.

Intriguingly, the East African Medical Journal who didn’t actually reply to any letters or emails, published first because we were too slow. So the question then was were we going to have to withdraw, were we going to have to retract our paper and upset everybody at Great Ormond Street?

There are other very obvious plagiarisers. This is a doctor who was erased by the General Medical Council two years ago – no, less than two years ago. He was discovered to have submitted papers, not so much plagiarised, that’s probably the wrong word. He’d actually stolen data from other researchers who had presented their work at conferences but it hadn’t yet been published. He stole the data and managed to get the papers published, without the names of the genuine researchers on it.

His defence was that it was his colleague, Dr [[Kupp 0:12:05]] who was responsible for this and that he had no knowledge of this whatsoever. He maintained this all the way up to appearing in the witness box at the General Medical Council where finally a cross-examination broke him

down and he had to admit that Dr Kupp did not exist. Which I think takes the biscuit for sheer cheek actually to not only did he steal peoples' papers, but he invented a non-existent author and said he was responsible.

Okay, falsification, fabrication, plagiarism is serious, but my thesis really is that things that some people regard as of lesser importance, actually might have a greater impact because the frauds and the fabricators tend to get found out. They may have a career for a time, but they get found out eventually and their material can't be duplicated by others which doesn't help and eventually, they vanish from sight.

But other things don't vanish from sight and just to give you one idea of this, this was a meta-analysis of paper, of reviews that had been published – sorry, trials that had been published on the use of this particular drug in the treatment of gastro-oesophageal reflux, looking at the statistic of the number needed to treat as the major outcome measure. And you will see that there's a big difference, depending on whether one looks at all of the trials. If one looks at only the duplicated trials, if you look at only the unduplicated trials. And you will see that there's a difference.

Now, these figures are such that this is a very effective drug so the difference between 3.9 and 9.5 may not be a great deal, but one can imagine in other circumstances it might be extremely important. And in particular, the interesting thing is that duplicated trials tend to have more favourable outcomes. We'll come to that in a moment. But let me tell you a true case.

A group of scientists and academics had been asked by a national body which produces guidelines for the use of clinicians in that country, to look at a particular psychiatric disorder and determine whether there is any useful drug therapy that should be recommended to clinicians to use. Now, the general feeling out there in the bush was that there was no useful treatment for this condition and they performed a systematic review and they found that indeed, it did look. There were a large number of trials, they were mostly small, they were mostly poor and they didn't show any benefit of the drug.

However, there were two trials which were very large. Published by the same group and which, if one added them, if one added them to a meta-analysis with all of the other papers which showed no benefit, changed things. And they showed benefit.

The reviewers were very suspicious about these two papers for two reasons. One is it did not look, from the author's institution which was a private institution, and it's geographical site, that they could possibly have had the resources to undertake the trial they claimed they had undertaken. When asked, they said that they had had no external funding, they funded it themselves. One astute reviewer noticed in the Acknowledgements, the name of an individual which he recognised and he knew to work for a medical writing agency that was often employed by a particular pharmaceutical company.

So they were suspicious about these papers but with no proof. So what do they do? Do they perform the meta-analysis and tell the national body that "Yes, our meta-analysis shows that a particular drug is of value in this condition" or do they say "We're going to knock out these two papers because we don't believe them, though we've got no proof and therefore tell the national body that there is no useful drug in this condition." A decision which could make a huge difference to the Healthcare budget and to individual patients.

And I think that is the problem of research integrity and duplicate publication because these two papers, one thing that was quite clear was they were looking at the same data set, quite apart from anything else and it may even have been the data set didn't exist.

We all know about the desire to extenuate the positive. A systematic review of research in a particular pharmaceutical field showed that company sponsored research was less likely to be published than researched published through charities or Research Councils or by individuals.

Company sponsored studies are also more likely to have favourable outcomes but the company sponsored studies were not of poor quality. Indeed, often they were of higher quality.

So the question is, if these studies are of good quality and yet they're less likely to be published and they're more likely to have favourable outcomes, the obvious question is "Well, where are the negative studies?" Well we all know where the negative studies are. The negative studies are hidden away somewhere because it would be inconvenient to publish them. And it isn't just researchers who might be responsible for this. We all know that positive trials are more likely to be published or trials with apparently positive results I should say, are more likely to be published than those with apparently negative results and that's due to the frailty of editors.

Similarly, they're more likely to be published quickly and they're more likely to be submitted, which is the responsibility of authors and sponsors. So, there are problems everywhere and here's a small analysis published a few years ago in the BMJ. The authors developed a scoring system to try and demonstrate the favourability of outcome, so far as the sponsor was concerned and gave it a score. And the higher the score, the more favourable was the apparent outcome and you'll see that if the trial was funded by a 'For Profit' organisation, it tended to have a higher outcome than if it was not.

Competing interests of course get in the way all over the place and this is a standard paper that's often quoted. It may now be six years out of date but there isn't a great reason to believe that things will have changed very much, although I should say that this is United States based and not UK based. So with one in three lead authors having financial interest in their research, and a quarter having a quarter of all researchers having received pharmaceutical funding and half having received what are politely called research related gifts. So we all know about competing interests.

But of course, and a more recent study just published this year in cancer, looked at consecutive studies in cancer in eight of the high impact journals and looked at whether a conflict of interest was declared or was not declared. 30% of authors did declare some conflict of interest, of which just over half was an industrial conflict of interest. Of course industry funded studies are more likely to focus on treatment so you might say there is more likely to be a conflict of interest. Interestingly, in the randomised trials, if there was a conflict of interest, they were more likely to report positive survival outcome.

So, competing interests get in the way. We'll come back to that. Now this sort of misbehaviour COPE has found can be detected by various people. Junior colleagues are often the people who first alert us, although there are other whistleblowers including professional whistleblowers. Reviewers of course, particularly in terms of duplication, because they've often read a similar paper somewhere else, read for the same reason, regulatory bodies very rarely because they tend to be reactive, not proactive.

Editors - those few editors who work for very well resourced journals, so will use plagiarism software, look at images to see if they've been manipulated by Photoshop and may pick up fraud. Statisticians of course are probably the best at picking it up. Sponsors and publishers not very often but sometimes. And there seem to be a lot of reasons why researchers don't detect it, given that first or early slide I showed where researchers seemed to think there was quite a lot of it about. And the reasons are there and of course it's not surprising that researchers might feel fearful, that they might feel bullied, that they also might not trust themselves.

Something we come across at the General Medical Council very frequently is that patients, women, who claim that they have been sexually assaulted by their doctor often don't make their complaint until it's happened two or three times and when they're asked why, often by a rather belligerent defending counsel, they always say "Well, I came out of the surgery and I didn't believe it had really happened, I couldn't believe that had happened and it was only when it happened again that I realised it must have happened." And I think there's a bit of that around.

Lack of support from the institution might come into it as well. What about universities? Well, many universities are very good at this. They unquestionably issue guidelines on research conduct. Many of them monitor their researchers and audit their researchers, but not all of them and when a mandatory supervisory body was recommended by Universities UK two years ago now, it was actually turned down. Because universities, as you would expect said “We’re independent bodies, we’re not going to be told what to do by some self-appointed quangocrats.

And the United Kingdom Research Integrity Office which has recently published procedures for both the Code of Practice and how to look into alleged misconduct, though it has published a recent report, it is of course only advisory and it may be or it may not be that individual institutions will pay very much attention to it.

But just to give you some examples. The Minister of Culture, Health and Tourism in Serbia investigated allegations, partly made by COPE against a very senior researcher of serial plagiarism, starting with the theft of students’ PhD thesis and ending with a whole series of papers.

The government report found him guilty of plagiarism and requested the university to take action and basically the University of Zagreb told the Minister that they weren’t going to take any action, firstly because this was a very honourable and senior researcher. Secondly, because he was about to retire anyway and thirdly because it was none of the Minister’s business what went on in the university because it’s an independent body. And of course, being Serbia, politics came into it.

Some of you may have read in The Times Educational Supplement about the very interesting paper from the University of Newcastle, submitted and published in the Stem Cell Developmental Journal, which was found to contain elements of plagiarism. Now to be fair, the elements were not in the data, the plagiarised material was actually in the introduction and in the discussion of the paper, not actually the data themselves.

The journal retracted the paper. The university said it should never have been retracted, this was a junior author who’s now moved on, who didn’t realise that it was a bad idea to cut and paste for early drafts and not keep a record of the fact that you’ve cut and pasted it, so that a year later, or whatever it was, the other authors think it was original and not stolen. And in fact the University of Newcastle says that it’s advising the authors to submit the paper elsewhere which is an interesting idea.

And as we speak, a medical school Dean is being investigated and this is public knowledge it’s not private knowledge by me, it’s been reported, allegedly because he declined to take action on behalf of a whistleblower who was reporting research misconduct because of the large sum of money that the sponsor concerned was putting into the university. And that may or may not be true. So academic responses, sometimes extremely good, sometimes not so good.

And what about sponsors? Can we trust them? That, it would seem that we might because the GMC in the last 10 years or so has actually sanctioned about 20 doctors or a little more than that because of research misconduct. And nearly all of the allegations have not come from their colleagues or managers in institutions, they’ve actually come from the pharmaceutical industry who have a sort of group of private investigators who look into what’s going on. Because it’s obviously not in their interests that there should be crooks around unless they’re themselves I suppose.

And this normally are doctors often in general practice who are taking part in a trial and who fabricate the data in order to get the payment without the irritation of actually having to see the patients and collect data from them. So that looks as if sponsors are on the lookout, but not always.

Can we trust publishers? Here’s a journal, which is not a journal. This is a ghost journal and it went through a number of issues, published by Elsevier, a member of COPE and therefore signed up to the highest levels of ethics, paid for by a pharmaceutical company. Nothing original in it, it only published summarised papers that were favourable to [[?? 0:26:16]] product. Now, there’s nothing wrong with all of that so far. It’s the last sentence that is the problem.

No disclosure anywhere on the cover or the inside of this journal or on the articles that told people this was a sponsored journal. Now everybody knows that supplements to journals are often sponsored. This was basically an advertorial and it went through several issues until it was discovered by chance. So we can't.

Now here's something much more serious. This is all material that was – that came into the public domain because of an application under the Freedom of Information Act in the United States of America. And these were trials of this drug, originally designed of course for the treatment of arthritic disorders and therefore often given to elderly people. And there was some interest in the possibility that it might have some affect in dementia. And if it did, that was obviously very interesting and a number of papers were published.

It turned out to be a negative trial but at least it was published. A number of papers were published. When the Freedom of Information Act obtained all the data that was in the hands of the sponsors about this study, a very interesting graph came to light that did not appear in the published paper. The top line represents patients treated with the drug, the bottom line represents patients treated with placebo in the blinded controlled trials and we have time to death.

Now not surprisingly, quite a lot of people died because these are very elderly people and they're very sick people usually. But an interesting phenomenon, you don't need to be a statistician to say that the drug Rofecoxib looks a lot more dangerous than placebo. Over the period of time there were 56 deaths in the Rofecoxib group and 29 deaths in the placebo group. This did not appear in the final paper, it just did not appear. This only came to light under the Freedom of Information Act.

And of course, as we all know now, this drug was subsequently withdrawn because of its adverse cardiovascular affects which was presumably responsible for the excess deaths. So the excess deaths were hidden. And you'd think editors should detect this sort of thing, they don't, firstly because the, you know, the whole of scientific publishing really works on trust. You can't run a journal assuming that people who send you papers are dishonest, that's nonsense. The vast majority of them are entirely honest and you have to work on that assumption.

There's also the fact that, particularly if you're an editor of a general journal, you may not know much about the subject concerned. Initial paper triage can be cursory which means you're then very dependent on reviewers. Not many editors, or rather the other way round, there are many editors with a lack of statistical expertise. Editors themselves may of course have a conflicting interest, publish an exciting paper about pharmaceutical research, and you might sell 10,000 reprints.

Using the Journal of Medicine on the Massachusetts Medical Society which owns it, makes several million dollars a year out of reprints. I'm not suggesting that's because they've published anything dishonest but there is always a conflict about whether to publish something or publish nothing if your publisher thinks there's a good reason to publish it.

There's that and some of us, paediatricians in particular, feel that the hunger for high impact papers has sometimes provoked enthusiastic editors into publishing papers that should never have been published and which have had, for example, in the case of MMR and the alleged link to autism, a pretty dreadful public health impact. And of course there's the fact that most journals are small, they're not well resourced. Most editors work for nothing and do these things in their spare time and how can they possibly afford to do those things that I mentioned, image screening or use plagiarism detection software?

So, there are all sorts of reasons. But editors, I can tell you, do watch out for certain things. I've already mentioned that authors, such as the author for example, the group of authors who allegedly had reported on over a million patients over four East European countries. The referee estimated it would have cost about three million euros to have undertaken the trial and there was absolutely no way in which they could possibly have had that under their belt. So that's one.

Editors are always worried about that and of course sometimes they are true, of course. And if you work on the assumption that if it's too good to be true, it must be a lie, that's dangerous. But all it means is that that's a paper that you should regard as a risky one.

For example, a paper that was submitted which was to test out the value of a device for testing hearing in newborn babies, and which claimed to have followed up 96% of the babies to the age of 18 months. And these were babies living in an inner borough of London. Quite impossible to have followed up 96% in 18 months. Nobody believed it was remotely possible. Epidemiologists thought you'd be lucky to get 70% of them.

Find things that are hard to believe, well, there's things that do turn out to be counter-intuitive sometimes. But if they are hard to believe, think twice about whether you believe them. That's always a worrying one. One of the famous historical papers in fraud in the UK which was a paper published by the British Journal of Obstetrics about alleged transplantation of ectopic pregnancies at an early stage from the tubes into the uterus, allowing for eventual delivery of a healthy child. And which turned out to be absolutely fake, absolutely false, got into the Journal because the editorial team were having a champagne lunch.

That's because they're obstetricians of course and gynaecologists, not paediatricians – having a champagne lunch to discuss papers and one of the associated editors who happened to be the fraudster, told the others what he had done. They were very excited and said “Oh, we must have that paper” and in fact one of the editors said “Yes, we must have the paper and I'll write a commentary to it.” So of course, it got in by the back door, always a mistake.

Editors are very suspicious of – it's fine for authors to put reasonable pressure on you. Why are you so slow? It's not a good idea when all their friends and relations phone you up at weekly intervals to want to know what's going on. Obviously, if a reviewer reports concern, you're worried. So all of these things are sort of risk factors that editors watch out for. Others, others, who have some resources at their command, will look at things like that and it is quite interesting.

The Journal of Cell Biology editors reckon that 5% of the images that they screen have been tampered with and 1% of the images that they screen have been tampered with in such a way that it changes the data significantly. The others have just been sort of brightened up a bit to look nice. So some editors look out for that and of course some editors will pay for the cross-check team to install their plagiarism detection software.

Science has suggested that editors can do some other things. I've already mentioned risk stratification of papers. They also demand trial registration as a way of making sure that the negative papers don't get quietly stuffed at the bottom of the drawer and never published. They ask for the clarification of the contributions of authors to try and do away with guest and ghost authors, though I have to say, it doesn't.

A promise to make primary data available if anyone wants it. But there are problems with that because the primary data may be somebody's property and it sometimes is not clear whose property it is. And on one occasion at the BMJ when we asked to see some primary data because we were suspicious, four large cardboard boxes of totally unsorted A4 paper arrived at the office and we realised we'd have to appoint a statistician, perhaps with a three month contract, simply in order to make sense of it. And that makes it impossible.

So, those are the sort of things that science is suggesting and JAMA: The Journal of the American Medical Association suggests some other things, including independent statistical analysis of all sponsored papers, which is now required by JAMA before they will even look at a submission. They are also proposing sanctions on miscreants. Something COPE has always objected to because without proper due process, one really shouldn't impose a sanction on anybody. But JAMA as you see has suggested some other ideas.

Publishers, well there's a Code of Conduct for scientific publishers which I've seen, which is currently in press. The fact that many large publishers have joined COPE shows that they're

worried about research and publication misconduct but I've already mentioned that reprints can make millions and that at least one company has published journals that are really advertorials. And trial registration is turning out, so far, to be rather disappointing.

This is a trial that's just been published in JAMA. Sorry, not a trial, a survey that's just been published in JAMA, looking at trials published in major journals during 2008. And they found that over half of them had not been properly registered on clinicaltrials.gov but of those that had been properly registered, a significant proportion, a third of them, actually had a quite different primary outcome in the published work from what the primary outcome was said to be in the protocol published on clinicaltrials.gov. So that somewhere in the process, things had changed.

And of 23 of the 46 that they were able to evaluate, it's interesting. They were able to evaluate them from the point of view of what the registration said was going to be the outcome and what the published paper actually found, they found that interestingly, the vast majority of outcomes had been changed to reflect favourable results, rather than anything else. Very disappointing.

And a similar study in the Public Library of Science Medicine, also published this year. This looked at a larger number of trials that had been registered and completed. Sorry, that's wrong, it was 677 trials registered in 2005 and completed. Only 311 were traceable so more than half of them seem not to have been published even though they'd been completed. And of those that have been published, only two thirds reported their stated primary outcomes. They might have reported other primary or changed primary outcomes, but only 60% reported stated primary outcomes.

And that's led the FDA in the US to require – I'm sorry about the spelling, that should say clinicaltrials.gov, not clinicltrials.giv. My typing finger obviously like the letter 'i' – now require that the registration website be updated with outcomes within two years of the trial being initially registered. So let's hope that helps.

And there are bodies that publish guidelines. There are bodies that publish Codes of Conduct. There are some examples. The World Association of Medical Editors, The International Committee of Medical Journal Editors, often called the Vancouver Group. The Committee on Publication Ethics, which I mentioned to you which is a collection of 5,000 editors. I see MJE as a dining club of editors of extremely important wealthy journals, self-perpetuating oligarchy, but nonetheless still produce very useful and helpful information.

The Council of Science Editors is a sort of COPE equivalent in the United States of America, though much smaller surprisingly. And WAME: World Association of Medical Editors is basically a sort of editor's Facebook. Or, if you want to be – if you're a rebel, you go to the Scientific Misconduct blog, run by Aubrey Blumsohn whom we see on the right, who is an academic and a whistleblower who believes that he suffered the fate that whistleblowers tend to suffer when he whistleblew, i.e. he was sacked.

And he publishes a misconduct blog where he collects, goodness knows where from, all sorts of crookery from all over the world. Some of which is probably true, much of which may not be. Like, I know Aubrey very well and like many whistleblowers, he's a very prickly customer.

And it's not just medicine. Between the wars, a professor of botany whose belief was that the Ice Age had once come down as far as Scotland, used to spend his summers picking plants in the Swiss Alps. He would then take them over to the Isle of Rum, replant them and then he'd take his research fellows there the following year and they would discover them and that would show that the Ice Age came to the Isle of Rum. And Carl Sommer who's a local author has written a splendid book called "A Rum Affair" about it. Actually, people have been writing about it for even longer than Carl Sommer. That was 250 years ago.

Thank you.

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