

MBSJ2013: MBSJ Board of Directors' Forum - Full Record
Session 3. Academic Journal Systems for Preventing Scientific Misconduct

- Period: Dec. 4 (Wed) 10:00-11:35, 2013
 - Venue: Kobe Portpia Hotel, South Wing B1F, Topaz
 - Moderator: Yuji Kohara (Chair of Research Ethics Committee), Keiichi Nakayama (Vice-President), Akira Shinohara (Research Ethics Committee)
 - Lecturer: I-han Chou (Nature Editor), Noriko Osumi (President)
 - Panelist: Yoshiko Takahashi (Director), Yasunori Machida (Director)
- (Participant: approx. 40)

Note added on April 1, 2014 by Dr. I-han Chou:

This transcript is part of panel discussion and relates to general Nature policies only and not specific cases. It must be remembered that all retractions are considered on a case by case basis. Nature's retraction policies/procedures can be found at <http://www.nature.com/authors/policies/corrections.html>. In brief;, all co-authors must sign a retraction specifying the error and stating briefly how the conclusions are affected, and submit it to Nature's editorial office for consideration — if the grounds for a retraction seem questionable, the editors may seek advice from independent peer-reviewers as part of this process. Following these procedures, if the editorial office deems that a retraction is appropriate, the retraction notice will be published. In cases where not all of the authors agree on a retraction, Nature evaluates whether the evidence available supports the main conclusions of the paper. We may decide to retract in cases where the authors cannot provide evidence to support the main conclusions of the paper. In such cases, if some authors still disagree with the retraction, we note the dissenting authors in the retraction notice.

(Shinohara) It's almost time. Okay, we are going to start the third session on the 'Scientific Integrity and Research Ethics.'

Okay, before starting probably I would like to remind you about the kind of the rule on this forum's sessions. Please just look at this. This session is very specific because we have a Scientific Editor of the Nature Journal. That means we have to talk in English. However, fortunately, I-han understands some Japanese, so you can also talk in Japanese. Because some cases researchers probably cannot understand the words in this kind of ethics or something like that, so Japanese talk is also welcome then probably she can understand. Also, Osumi-sensei translates for her.

Also, as I mentioned yesterday, just please talk about more positive view on this topics. Of course, you can claim on negative side of the scientific research; however, we would like to have more constructive comments.

Also just as I mentioned yesterday, just pick up current problem in the scientific integrity and also research ethics. And based on such kind of problems, you may offer some kind of way to improve our scientific

integrity as well as research ethics in Japan or maybe in terms of international ways.

Also, just please avoid personal insults without any evidence. Of course, if you have the evidence you can just talk about such kind of topics; however, just please avoid.

Okay, as I mentioned, the more constructive comments are welcome. Also more importantly, this is very important reminder, all talks in this session will be recorded and expected to be public in the webpage of our Society, after some editing. Okay? That is very important.

We will now open our discussion to public, share such kind of information about all public in Japan. Okay, so we're going to toss the microphone to Osumi-sensei.

(Osumi) Before moving to the next speaker, I'd like to introduce Dr. I-han Chou as a good friend of mine, and fortunately come down to Kobe for this occasion.

I first met her at the Japanese Neuroscience meeting. At that time, she gave a talk about how-to-publish or how-to-present-yourself kind of thing. By that kind of context, I have invited her to Sendai as well. So, that is why she fortunately accepted my invitation to this session.

I-han is originally a neuroscientist and working in America for years, I don't know how many – but anyway in Harvard etcetera. But about 9 years ago, she became as a Senior Editor of the Nature office, but she is physically in Ichigaya in Nature Japan office.

Before Nature Editor, she was an editor in Nature Neuroscience. She has a long career in that direction. But also, she has a deep, deep interest in science and especially life science and neuroscience.

Okay. Today, she will talk about a kind of ethical issue as one of the editors in high journals. Thank you. I-han please.

(I-han Chou) Thank you very much, Dr. Osumi, and thank you all very much for coming here to the session today. My name is I-han Chou. I am a Senior Editor for Neuroscience. I am Head of the neuroscience editorial team at Nature.

My role at Nature is to select and peer review manuscripts. We publish approximately 5% to 8% of our submissions, so we are a very selective magazine and we like to think that we publish most high-impact, cutting edge research.

Today, I am going to talk about some general issues in ethics in publication and go over a few of our policies and procedures for how we deal with when situations concerning ethics in publication arise. I am not able to comment on any specific papers either currently or previously

under consideration, but hopefully I can give you a sense of our general policies.

As I said, I work for Nature Publishing Group and I work for the flagship journal Nature. We also have several sister journals, the research journals including Nature Cell Biology, Nature Neuroscience, Nature Genetics, Nature Medicine, so forth. We all share general editorial policies. Some of the details in how we manage things differ slightly, but in general, the principles are the same.

The first and probably most important thing when talking about the role of a journal in dealing with misconduct and where we see ourselves is that we take very strongly that the principle of scientific publication is trust. We accept that the data that authors present in our papers is all correct and that it was obtained under appropriate circumstances and that it is an accurate reflection of the data that were collected in the lab.

So we as a journal are not actually the ones able to enforce or investigate situations under which misconduct is suspected. We do not have access to notebooks or computers and are therefore not well placed to investigate concerns with the rigor that may be required. When presented with evidence of irregularities, we investigate to the best of our ability. If we are unable to investigate fully with the information available to us, we alert the appropriate authorities to look into issues of concern.

Now, of course, things do happen and concerns are raised with papers, so I'm just going to go through quickly for you our procedures when this happens. When a complaint is brought against a paper, we look into all of them. And so, these may be brought by readers of the journal. These can be from authors on the paper, other people in the institution, and sometimes they are emailed to us as anonymous tips. Regardless of who they come from, we look into every allegation.

The majority of the complaints that we receive with respect to suspected fraud in a paper concerns images. And so, what we will do first is we have an in-house graphics team that will check the figures. So, when a complaint is brought against a paper, our own team will then check the images for suspected manipulation. So, they do a number of things including checking for duplications, for inappropriate adjustment of contrast, things like evidence of gels spliced together, and so forth.

Now, a lot of the times what this investigation will bring up is that there are irregularities in the images but that they are not necessarily fabrication. A lot of the times what happens is that there are irregularities but it might just be due to sloppy preparation or slightly inappropriate but not necessarily fraudulent manipulation.

So if we feel that there is something out of order in a figure, we will request the original data such as gels or supporting evidence from the author. We'll explain to them the nature of the complaint that's been

brought against them and we ask for a detailed explanation of how the figure was assembled.

At this point we try to make the consideration of whether there was actually intention to commit fraud or to mislead as opposed to just bad practice. What do I mean by bad practice? These are things like inappropriate manipulation of background in the gels, things that are not necessarily good practice, but not necessarily fraud.

If we do feel that there is a more serious concern potentially at this point – as I said, we do not actually have the resources nor do we feel ourselves to be the ones in the position to make actual investigations. So, we refer to the author's institution. If we feel there is a serious problem of a paper, we will contact the institution and the board that is sitting above the authors who are appropriate to investigate such claims. Even if we suspect that there is something wrong with the paper, we will wait for the results of a formal inquiry. We try to wait for the results of a formal inquiry in most cases, before going ahead with the correction. We do not have access to lab notebooks or to data and so are not always able to investigate fully. If, however, we are persuaded by the results of our own investigation, we will proceed with correction on that basis.

When the authors are in the US, this is usually relatively straightforward in that authors have an institution – the university or the research institute. We'll have a body that we can go to. At a higher level, there is the Office of Research Integrity that will undertake investigations. When it's outside of the US, it can sometimes be a little more difficult for us because it's not always clear where the lines of responsibility are and who we should be contacting.

Also, one thing I should say is that we do our best to get the results of these inquiries, but we hope that the institutions will share with us the results of their investigations. Sometimes these are confidential and so it is not always entirely straightforward. We do consider ourselves independent, so we don't just accept the findings of an inquiry and we would like to see the evidence and know how the inquiry was made so that we can make our judgment of whether the conclusion of the inquiry was appropriate or not.

If we feel that as a result of the inquiry – assuming there was no gross case of fraud – if the main conclusions of the paper are still valid, we will run a correction as opposed to retracting the paper. Retraction for us means that we no longer stand by the conclusions of the paper and we consider them invalid and therefore they need to be removed from the scientific record. If there is any element of doubt in the robustness of the conclusions, we will retract the paper.

That is what we do when we receive a complaint. We recognize that this is a problem and a challenge for the whole field and not just for journals but also for the scientific community. So going forward, we are taking some steps to try and improve the situation.

At Nature Publishing Group, we are trying to improve reporting standards. Now, when you submit a paper to the research journals or to Nature, you are required to submit a checklist that is aimed to increase the reproducibility of your experiments, so things like you have to list the antibodies, you have to provide more detail on statistics and so forth.

At Nature, also when you submit a paper and your paper is accepted for publication, we encourage authors but we do not require them to submit the raw data behind the figures. At the moment, we are asking for things like the numbers behind graphs, so we ask for Excel sheets that contains the data behind the figures. And we encourage people to put as much raw data into extended data as possible. I mean, obviously there are constraints on how much we can host or how much is feasible to show, to present to the reader, so we can't host hundreds and hundreds of gels for examples.

During the review process, we do spot-check papers for image manipulation. Again, this is very resource limited. There are other journals I know out there who routinely check all submissions. We don't have the resources to perform rigorous image checks, so we do random checks instead.

If there are any irregularities raised or even just minor comments raised by referees during the review process, we will also have the paper undergo our in-house check of the figures.

I'll just leave you then with a few other resources. We wrote an editorial about 3 years back that goes into greater detail of the procedures that I just described for you, which is how we deal with suspected irregularities or suspected misconduct in a paper. Here also are details for how you can download PDFs of our publication policies.

Finally, I would also refer you to the Committee on Publication Ethics, which is an independent body that creates guidelines and standards for publication ethics.

Thank you.

(Osumi) Okay. So, today I am going to talk about the first retracted papers in our official journal 'Genes to Cells.' This is just the kind of example and a kind of material to discuss about those kinds of journal-misconducting things. Let me introduce about 'Genes to Cells.' This is a peer-reviewed scientific journal, and publishes original research on the molecular mechanisms of the biological events and with beautiful cover pictures that I love so much. It was established in 1996, and the founder editor is Dr. Tomizawa, and now Dr. Yanagida is in charge as the 'Editor-in-Chief.'

Actually I will tell you a little bit more in detail later, but I would like to mention about our society's action about those misconduct events. Probably, most of you or the audience here know that ex-Professor Dr.

Shigeaki Kato resigned at the end of March in 2012, and the media reported those kinds of news. It's a kind of big, big sensation.

Our Society is so much serious about those kinds of things because Dr. Kato is one of the big guys in this Society and actually was the director and also some important person in the working group for the how to deal with the misconduct.

Just last year we held a forum for talking about the misconduct. It was just one occasion during the annual meeting, and it was in parallel with poster sessions. This year we hold six-session series of the forum to talk about the misconduct in many aspects. So, today is more focusing on the papers.

This year in June we put on the website the President's Report and Statement of the MBSJ Board of Directors concerning Scientific Misconduct; we are seriously dealing with those kinds of things. We also asked questionnaires on the web. We, fortunately, collect more than 1000 responses which was analyzed and appeared in our website in the end of August this year.

So, Dr. Kato, as the corresponding author, requested the retraction of the four papers to the 'Genes to Cells' editorial office.

Also in parallel we sent — actually, this is the first time — we also sent a 'letter of request' for the University of Tokyo because they had a kind of committee to investigate what's going on but there are no reports; so we just wait, wait, wait, and we don't know exactly what's going on there or what is the background behind the misconduct.

In the middle of October we had a letter from the University of Tokyo which was also uploaded in our homepage, but it's not actual action at all, so it's a little bit disappointing. But of course they have their own kind of logistics. Finally, at the end of November the 'Genes to Cells' editorial office announced the retraction of the four papers that were submitted from Dr. Kato.

These are the four papers, and appeared like this as a retraction on the webpage. This is one of them as retraction. Here, the reason or the explanation of the retraction is and the retracted paper is now like this.

The reason of the retraction is explained like this. The retraction of this article has been retracted by agreement between the authors, the journal 'Editor-in-Chief' Mitsuhiro Yanagida and Wiley Publishing Asia Pty Ltd. The reason of the retraction is due to multiple usage of the tissue images: for example in this case, the Figure 1(B), 2, 3(C), 4, and 6; and the absence of the gel image for the lane 5 of GFP in Figure (B). I will show you.

Okay, so this is the picture of Figure 1(B) and these are the images of the *Drosophila* imaginal discs. Now this, I don't know so much about in detail but this is 1(B) and this is the Figure 2. Can you figure out what kind of

misconduct in picture? Actually, it's a little bit difficult for the first glance. And for me as well, so I watched, watched, watched over times.

This is the Figure 1(B) and this is the Figure 2, and still probably most of you don't know what is misconduct picture exactly. Then, I realized that these two panels — this is from 1(B) and this is from 2 — and if we widen the right figure, then it's become like this. This is just by example and I did how to do that by myself for my first time actually.

Of course, the contrast is a little bit changed. So, please see this kind of curves like this. It's quite the same. The gel thing is here. Okay, probably it's a little bit difficult to see from this monitor, but if you download the PDF or if you see your own monitor, it's more easy to understand which lane is misconducted actually probably. So, this thing is actually wrong. No image there.

Probably, another one is more easy to understand, so I will show you. In this case this totally blind absent lane. Actually, there are no images; just a kind of white image of the square is just put there. Again, it's a little bit difficult to see at this kind of image quality but it's easy to understand when you download the paper in PDF.

Okay. I checked the submission date and acceptance date of these four papers. You understand that these three are relatively a kind of very high speed accepted papers, and the third one a little bit takes longer time which means probably there some kind of major revision has been done. In case of the three, probably just a kind of minor revision. That's my guess. I don't know what's going on there.

Actually, our journal is also advertising the quick reviewing process, so probably those kinds of things a little bit some background on why those kinds of things have happened.

Probably, the background of the misconduct contains too much competition and pressure for not only PIs but also the students or postdocs as well. We all need to rush for the publication of our papers as soon as possible, as high-impact journal as possible. And, because of some kind of IT things, we have probably less and less communication within our laboratory. Also, some laboratory is so much isolated from other laboratory but by that way they don't know what is the standard, what is the consensus of other laboratories and just follows how the PI says or how group leaders say.

Also, I am wondering for those people what is the purpose doing science? Probably some of them just do science just to publish papers or just to get job. I think those kind of thing is not good.

So, in this morning session probably we'd like to discuss about those kinds of things as well. So, how about such kind of mega-correction? Mega-correction means that many, many data in the accepted paper are replaced with better quality of pictures or some picture is just kind of indirectly replaced or those kind of thing.

As I already told you that there're many editorial offices having a kind of checking system. But as she told us that there is suffering from a kind of limitation of the resources to check those kinds of thing. Not only Nature Cell Biology but also other journal as well encourage us to submit the raw data as well.

Actually, this is a case of the Journal of Cell Biology and also guidelines and probably some of you referred to those kinds of things as well. I just borrowed the picture from the Uemura-sensei in Kyoto University where he did use this PowerPoint slide for his lecture about the misconduct. In this case, this is a paper taken from this paper. The paper is shown like this but the raw data is like this and submit as a kind of supplementary information or like this.

The lane is put in here and those lanes put in here. So, not only the beautiful picture but also the raw data is also submitted as a kind of supplementary data as well like this. Also, he is emphasizing a kind of how to deal with the kind of statistical thing; so this is one dot that means one data and so this is a kind of standard deviation and so these are the P value.

This kind of representation is probably more actual, more closer to the real thing as well. But again, we don't know that this actual one dot is really true or not. If someone would like to need those kinds of data, I don't know about that. So, if those kind of thing happen, the original, the number of the value can be misconduct as well.

Okay. Our Society has worried about those kinds of thing for several years, so almost every year we did those kinds of educational symposium, especially for the younger students and the postdocs and what kind of the rule is important in doing research kind of thing.

This year, because we are doing those six series of the misconduct session, we didn't do this kind of thing, but in next annual meeting probably again we will hold those kinds of session as well.

Actually, after those kinds of symposium, some of the actual data is put on our website, so this is the message from Nakayama-sensei who is one of the organizers in this session, so you can download a PDF from our homepage.

Also, in the other resources as well and so this is from the JST and so occasional materials how to avoid the misconduct. So you can register and going to own. There is some questionnaire and by that way you learn how to avoid the misconduct.

Okay. Finally, I'd like to emphasize that we need respect for science and love for science. We should not forget the love for science or interest in science when we're doing excellent research and writing excellent papers. Respect for science is a key for both the scientist and the society to trust

and to be trusted, or I should switch it, to be trusted for the scientist and trust for the society. This is my final slide.

In this evening, we'll do a kind of get together dinner party at the restaurant in that Portopia Hotel to the second floor. Everyone can come and need some fee. So, please contact me directly after this session or please send me an email. More information is on my blog. Thank you very much.

(Shinohara) Thank you very much for nice presentation, Dr. Chou and Dr. Osumi. I would like to start the discussion. For the panel discussion please show up on the board.

Sure. Go ahead.

(Floor) I have a couple of questions to I-han. Dr. Osumi showed some example of the retraction from the 'Genes to Cells.' But in this case there is some fabrication of the images so it was quite simple and I would say very childish or immature way of fabrication and it's too obvious.

But the thing I want to ask you is we know that there are several, let's say, figures which nobody can reproduce, there's no fabrication with the images. But we know that some results are very difficult to reproduce, so almost nobody can reproduce. In that case, can you actually get some complaint or flag from readers or experts on that kind of complaint? If so, what would you do?

(I-han Chou) If it's just the case that somebody is not able to reproduce the data in a paper, it is very hard for us to act on that because there are many reasons why something may not be reproducible, that are not necessarily due to misconduct. So, unless there is a formal investigation either by the University Research Board or a Governing Research Body that tells us there is evidence of misconduct, we usually can't go forward with that.

Now, what we will do sometimes in the case of study that we think is very high impact or is likely to be very controversial, we may solicit a replication before we will publish the paper, but we do that under only very exceptional circumstances.

(Floor) Okay. I am an immunologist and during '80s there are many, I would say, as you said, the sloppy practice that the contamination of certain monoclonal antibodies to other monoclonal antibodies. So, you can precipitate something in that laboratory but other laboratory cannot do that. These kinds of things happen relatively often. So people have realized, so became more careful about it. But if somebody has raised a hand and then pointed out, it was very difficult to correct the daily practice.

Therefore, I was thinking if there is any good way to solve this kind of problem, particularly from the outside of the field it was very difficult. Because in '80s Nature had very high-impact paper in the field which

nobody was able to reproduce and everybody knew that. But from outside the field, many people have actually cited the paper, so there was this kind of things could have happened.

As you said, to maintain integrity of the paper, if somebody raised a question about the reproducibility, it would be better to have a good method to deal with those kinds of complaints.

(I-han Chou) I agree completely and if somebody can come — and this happens quite often, if somebody can come to us and say, well, we have attempted to replicate the experiments but we came to a different conclusion or we have an explanation that this might have been the reason why the authors observed the results they did, then we will consider that possibly as a communication that is then appended to the original paper along with the authors' reply.

I think the issue of replication though is incredibly important and I think what needs to happen is that there needs to be motivation for people to publish negative results and failures to replicate and for journals to highlight replications or lack of replication. But I think it also has to be an effort both on the part of the journals and the scientific community because right now there is not a lot of incentive for people to publish negative results. There are journals now that say in their mission that they will publish negative results or failures to replicate, and that this is part of their mission, such as PLOS One, also Nature Publishing Group's Scientific Reports, and others. I think there just needs to be more of a move for people to publish their negative results.

(Floor) Also in that case, I appreciate your recent movement to encourage people to submit a protocol to your Nature protocols so that if you have complicated but sophisticated techniques, you can actually publish in detail in that kind of journal so that helps people to easily reproduce the result.

(I-han Chou) Well, we are doing what we can to facilitate reproducibility and also publication of lack of replication. But I think it's also up to the community to see this as a way forward.

(Floor) And also because of limitation you always ask us to cut the length so that people cannot actually fully describe the important detail of the protocols. That's one of the difficult things for us.

(I-han Chou) Yeah. It's an evolution at least for Nature, from a print magazine with a physical number of pages to the new landscape of scientific publishing which is all online. So, we are taking measures to improve these things. We now have all our methods online and we have no space limitation on methods. We also have Nature Protocol Exchange where we invite authors to submit the full protocol of their procedures in complete detail so that people can reproduce your experiments.

We are also taking measures to increase data presentation, so we now have extended data that is integrated into the paper that allows authors to have more space to present more data.

(Shinohara) Any other discussion? -Please.

(Floor) I have a specific comment on 'Genes to Cells.' The case of Dr. Kato is quite unfortunate, but in my opinion that occurs inevitably. What I meant is I have several really sort of uncomfortable experiences with 'Genes to Cells' in the past, and after that I decided to decline all the request to review for the journal.

That journal, I remember originally it meant to be at the class of, let's say, MCB or something like that. But soon it declined to sort of the community journal for Japanese scientists – by Japanese scientists for Japanese science. Even worse, it became journal for editors for editors. What I meant is when I receive the paper from one editor's student, then another senior editor asked me to review gently. What that means 'Review Gently'?

I ignore that and send just the type of review I would send back to MCB or nature or whatever. Then, nearly all of my comments were ignored, let's say, overwritten by senior editor who has retired by now, so it doesn't matter.

But that was really, really unfortunate because I understood that Japan could have good journal. I originally hoped for that. But then after that it's another, let's say, in Japanese, Otasuke journal. See? So, I decide not to review it and I decide not to submit it. It has nothing to do with me. It may sound very severe but it's the reality.

Another reason is because the graduate students need a paper sometime very rapidly and they submit to maybe Nature or whatever, now time limit is approaching then editor find or friends of the editor find a good place in Genes to Cells. They need it to be accepted in 30 days or 40 days. Just I guess those of Dr. Kato's paper are few of those. I'm sorry this may sound very severe to the journal but that's my opinion and I really hope that you do something completely to change the system if you can.

(Kohara) You have any comment Osumi-sensei?

(Osumi) Actually, I'm not the 'Editor-in-Chief' and not in charge of those papers of Kato-sensei. All the data I showed today is open for public on the web, so I don't know much about what's going on in the editorial office. But, well, I think that there are too many journals actually recently. Not in Japan but also in the worldwide.

The reason is that there are more and more researchers and they need more and more papers and that is why we need more journals. That is why. So, there is competition among journals because the number of the journals has increased.

So, it's not good timing for me to say about the kind of how to deal with the kind of 'Genes to Cells' in my position, but I hope that if the submission of good papers is gathered in the 'Genes to Cells' I think that those kind of situation could be changed.

So probably it is a kind of feedback, a kind of spiral problem. And I think that that sensei on the floor is mentioning some of the right point. But at this moment I am not the kind of the 'Editor-in-Chief' in the 'Genes to Cells' so I have no comment on that.

(Kohara) Any other comments?

(Shinohara) Yes, I have. So, that is an educational issue, an editor's issue as well, editor's report, too. So, again, we have to educate PI as a scientist, that is very important.

Okay that is another issue, so I would like to talk about mega-corrections we often see these days in top journals. What is the rationale of your journals to approve such mega-corrections in your journal? Because I think that is a kind of violation of peer review system because if you can easily replace your data later after acceptance of paper, so how can reviewer judge integrity of the data as well as conclusion of the paper in a scientific way.

So, would you explain some rationale over here? So I will show you some mega-corrections. So just this is a famous paper from Kato Laboratory, so you see multiple replacement of the images like here. As you know, this paper, this also has published correction first, then this paper was retracted. Even in these collected figures, there is some manipulation.

It is clear to you. So, that means probably – I am sorry to take this kind of bad example to you, but still I am just wondering how you can evaluate this kind of process in a fair way by asking reviewers about this integrity of this correction, something like that. Anyway, please, explain rationale of the mega-correction in your journals.

(I-han Chou) As I said, the rationale for a correction versus retraction is whether the conclusions of the papers stand and whether the authors are able to provide a reasonable explanation, and in some cases additional data to show that their conclusions are still valid. That is our principle.

(Floor) That means you do not need any peer review system because you have...

(I-han Chou) The corrected data may be peer reviewed, as well as additional data.

(Floor) That is – because the first submission is very critical, because as an honest scientist you have to prepare your paper with complete dataset with multiple controls, with multiple duplicate or triplicate. But in the case of mega-correction, maybe you can later carry out experiment to support or some kind of bad image later, so that is not good. So I think probably

if the first submission – if original paper contains lots of sloppy data, you have to recommend resubmission; that is a fair way for the journal.

(I-han Chou) I am not sure I understand your point of what is the difference between resubmitting versus actually having the data rechecked. In the case of these large corrections, it is usually the case that we have asked the authors to provide original data or supporting data and also an explanation for why there were irregularities in the figure in the first place. And our decisions are based on whether, assuming no fraud in the original conclusions of the paper, the main conclusions are still valid.

(Floor) Yeah, so that is one standard to approve such kind of mega corrections, but still I have to think about how you can revert such kind of arguments. Just give me a moment, so just talk about something else.

(Shinohara) Okay, please.

(Floor) I would just like to forward his question. Just imagine, if someone, he or she, happens to know someone's data, okay, and so he or she knows the conclusion, then he or she just make up a falsified data and submitted to Nature, and you just accept it. And finally so you find the data itself is not very genuine, but the conclusion is okay. So then what happens is he or she can get the priority for publication in Nature and then he or she can make corrections afterwards, right? So, that means this is not a good practice for science.

(I-han Chou) I think – and this gets back I think to the original question, for us the key is if we can establish that there was intent to commit fraud. And it is very difficult for us to establish that intent from the position of a journal, because the only evidence we have in front of us are the figures and the data. So unfortunately, the only thing we can go by is what we are given. And we have to rely on the institutions to do the investigation as to whether there was intent to commit fraud.

(Floor) I also have a related question. Probably you can ask the original data, but if the authors try to repeat or do the experiment once more and if it is allowed that, after the problem occurs because the first falsified data first up, the authors later give the probably corrected data and submit it as a revised one, how long can you wait for revised data if there is a problem?

(I-han Chou) So I think your question is if there is a problem with the paper and we ask the authors to explain the irregularity and...

(Floor) ...or may ask the original data, raw data, but the authors ask the waiting for a while, perhaps 2 or 3 months, maybe they can make the data or do experiment or...

(I-han Chou) Well, presumably – it is a complicated question. It depends on each case and we have to take each case as it comes and decide what

is appropriate for the kind of data and for what is being requested. There is not a straightforward answer I can give you.

(Osumi) Probably he is asking a more simple kind of case and just ask kind of original data, right, but authors did not submit the original data for about 3 months or so, what that mean, so that is his point.

(I-han Chou) Usually when we contact authors and ask for supporting data behind a figure in the paper, we expect to receive a response right away.

(Shinohara) Any other questions? Please go ahead.

(Floor) Let me change the topic a bit. So, when a suspicious case was supported in a paper, the journal will ask the author to answer or respond properly. If the author is cooperative and immediately submits the original data, that will be fine. But in some case I would assume that the author may not be very well cooperative, maybe extend or delay the response, or do not provide the satisfactory answer.

And I guess that case could happen in many cases. For example, on the 'Genes to Cells' case, retraction notice was made about 1½ year later than the problem was first stated, so it took a long process. And I see that that author's response was not immediate. And I guess, prior to the author to finally present a retraction notice, there must be some long exchange. And I think for this kind of case, how much of the author's responsibility can be enforced. And in case authors are not cooperative, what the journal or institution can do? I think that this is one issue we should discuss here in order not to repeat this kind of case.

(Kohara) Do you have any comment? No?

(I-han Chou) If we make a request and we feel that it is not being met in a timely fashion, we will refer to the institution. So, I think I have to turn it over to somebody who has more experience in that regard as to how to take it from there.

(Osumi) So, in case of the 'Genes to Cells', as far as I know after Dr. Kato resigned – and I do not know what kind of communication between 'Editor-in-Chief' and Dr. Kato. But as far as I know, Kato-sensei says that he could not ask for retraction because it is an ongoing kind of evaluation or examination by the committee in the University of Tokyo. And that point is an inter-summary kind of thing is submitted. So, he got some report from the University of Tokyo and then he moved, he did a kind of retraction request to the journal, 'Genes to Cells'.

(Floor) So I-han said that if the problem cannot be answered, then the journal will hand out the responsibility to institute; and then according to Kato-san's explanation, he says the investigation as an excuse for not responding as a corresponding author or responding as a member of the scientific community to take a responsibility to make an immediate action to correct the falsified record.

And it is a great loss for this community to circulate the falsified paper one year that will influence a lot, especially from the leading scientists or especially the paper from a leading journal. So such an obvious record I think could be retracted or corrected properly independent of institutional investigation.

And I think it is really very difficult to enforce this kind of action, but since the scientific publication is sort of a practice of trust, and we always promise to the journal that the content is correct and produced in a fair scientific manner. So this is what we always do and what you are always asking for, right? And so based on that promise, I think the journal can take a more stronger action in the case that the authors are not cooperative enough to solve immediately the problem.

(Floor) I think there is a perfect solution. I think Dr. Kato could not retract because of the university request. University said, do not do anything. But meantime, I think what MCB did was just right. It said, this and that and that paper are under suspicion or whatever, I do not remember the word, but those papers are under investigation. There is no official conclusion, but the reader should be aware.

And that should be enough. The reader can judge and then the official decision may take a year or more. In Dr. Kato's case, there are more than 40 or 50 papers, and more than 100 students and investigators are involved. The retraction is fine but they have to find who is responsible. Is that all by Dr. Kato or many graduate students? Who did what? And that takes time.

Actually, I think all those 40 or 50 papers could be investigated in 1 year. I think just it was a great job for them. Actually I am from University of Tokyo but I have nothing to do with them, so there is no reason to praise them, but I think the investigation committee did a good job and it takes 1 year. Some people said it is slow, but if you think about the size of the problem, it was just a great job.

(Osumi) Well, I think that this session should not talk about only one case or one person or those kinds of things. Probably it is better for us to talk about a little bit wider range of the aspect, right?

(Floor) So let me ask you a related question. So Osumi-sensei told us that three persons: corresponding author, and general editor, and publisher agreed with the retraction. But is it also the case in Nature, so three persons should agree with the retraction? For example, if corresponding author disagree with retraction, what do you do if the image manipulation is obvious?

(I-han Chou) The fact that we are printing the retraction can generally be taken as our indication that we agree with the retraction. We do seek all authors for any correction or addendum or retraction. We do try to get all of the authors to sign the retraction. If anybody dissents or cannot be

contacted, we note that in the notice. So everybody has the opportunity to make their statement as to whether they agree or disagree.

(Shinohara) Please.

(Floor) So, the question is for I-han. So in your talk, at the beginning you said that the principle of the scientific publication is trust basically. And given this situation of I mean many false papers, so now I cannot trust anyone or any papers. So my question is are you going to change your principle, not based on the trust, but based on the fact that people can make up.

(I-han Chou) I cannot speak for the journal but I will tell you, personally I think once you do that, the system will break down. I think we have to go forward and the only way that science works is to educate people to the standards that are expected and work with them. You have to enable people to report up to those standards, but I think if you start not trusting people and if we go into a situation where we take every paper as suspected fabrication, I think the review of every paper would immediately turn into a 2 or 3 year process and I don't think the system can work that way.

(Floor) I do not think education works, so maybe there should be more stricter rule. I came here to expect some idea coming from the journals. For example, asking the raw data submission, not just encouraging but just making it mandatory. So what do you think about it?

(I-han Chou) I guess the question is, from a reviewer standpoint can you handle the raw data and can you handle checking the entire body of data...

(Floor) You do not need to check the raw data. For example, anyway, the raw data should be submitted on the public domain so that everyone, all the readers can check. So I would like to suggest changing some system.

(I-han Chou) So you want to see more hosting of more supporting data for the papers.

(Floor) Yeah. So if necessary I would like to check raw data, so even the picture of the lab note.

(I-han Chou) Well, I think the field is slowly moving in that direction, but it is very slow. We certainly do not make it mandatory, as I said. Part of it is that it is just an incredibly complex problem of how you would support that amount of data, and what kind of data do you support for what kind of paper. There are external sites now like Figshare that allow you to upload data and additional figures. We will see, if the community shows a strong interest in uptake, then I think that would push journals to consider integrating more data into their papers. But I think we are at the very early stages in changes in presentation and how people show their data. So I think we will have to see how it goes forward.

(Floor) So another thing is, you said that as long as the conclusion stands, you prefer correction rather than the retraction. But I think in many cases making up the data usually occurs in the kind of minor point, not the main data. So if the main data was made up, of course that should be retracted, but if the falsification was minor point, usually I think it would not change the conclusion. But still, if it is an intentional misconduct, I think that should be retracted actually. That is my opinion.

(I-han Chou) Well, every case is different and every journal has a different set of processes for making their decisions and different thresholds, so I think it is just too complicated. I think everyone has their own set point.

(Floor) I completely agree with his opinion. So I think point is intentional or just a mistake. If it is intentional even in a minor figure, the trust is lost, so it should be retracted I think because it is highly likely that main figure is manipulated. What do you think about that?

(I-han Chou) I think that is a valid point but that is not the principle with which – I mean I think all of these are very complicated. It is very hard for me to give a clear yes/no answer because in every case it is about how much was affected, what is the nature of the irregularity. Everything has to be taken into consideration.

(Floor) It must be important who decides this is due to the intention or...

(Floor) I do not think so, I do not think so.

(Floor) ...it is very clear, but if Author does not say anything about that, it is very difficult to judge.

(Floor) For example in the 'Genes to Cells' case, Dr. Shigeaki Kato's paper, there is a white board in the figures; it must be intentional manipulation I think, so if such figures...

(Floor) So, the authors realize such a mistake has been done...

(Floor) It must be realized that a white plate cannot be automatically put in the figure I think.

(Floor) They might say someone else, right.

(Floor) I do not think that is point.

(Floor) Basically I do not agree, but such cases. So I think the decision of the organization, for example, the university or institution, national institute, the decision of such organization must be very important. But for such case, it needs quite many years, so I think, as he requested, as he introduced once some example, I think top journal quickly must do some announcement prior to the final decision.

(Floor) A related point is that there are any kind of communication among journals about the kind of, oh, we have this kind of retraction from this author and those information can be spread in all the kind of top journals.

(I-han Chou) At the moment there is not because each journal generally considers the review process to be confidential and so certainly any complaint about a paper would certainly be considered confidential as well.

(Floor) I have a related question, actually comment. I understand that each journal has its own policy of how to handle papers and how to handle the question about the either retraction or corrections. But as Dr. Xxxx pointed out, if somebody really intentionally manipulated the data, it is very difficult to see that.

For example, I don't know if you were young so you may not know, but in 1970 there was a wonderful paper published in the journal 'Cell' resolving the molecular mechanisms of Warburg effect, written by Racker and Spector, so beautiful gel patterns showing the phosphorylation cascade of proteins downstream of the growth factor receptors. But everything was actually came out from the autoradiography of iodinated proteins with the same molecular weight. But nobody can actually figure out by just reading, but nobody can of course reproduce the result. So it took a year or so to figure out what happened.

But this is an extreme case. Eventually it was revealed and then the paper was retracted. But if somebody actually had the similar intention to show a panel figure, just a panel upon the request of the tough reviewer of Nature in order to be accepted you need this data.

Of course, as somebody said, the entire story is probably correct and the conclusion will stand, but to publish the paper in Nature, one can be tempted to do that kind of things. But still, do you agree to correct the data or do you retract the paper? So those are the things that people have been wondering. I understand it depends, but at the same time it is very difficult to figure either this is sloppy or intentional, but everybody is quite eager to know how each journal responds to this kind of misconduct.

(I-han Chou) But it sounds like what you are laying out is quite straightforward that the data where perhaps possibly in this case we are talking about a case of potential data fabrication, in which case an investigation would show very quickly the authors would not be able to produce original supporting data.

(Floor) No, because even if I give you the autoradiography, it is impossible to tell whether it was because of the P-32 or gamma ray. So therefore, the data you cannot tell for example. I know this is an extreme case but as long as if you have the appropriate protein, you can run autoradiography many times and get the same result, for example, if you really intentionally do that. So that kind...

(I-han Chou) But in that case it would only come up – that is a case where we would have to rely on the university to make that investigation and to

uncover it. That is something beyond our power. And if that were the case, in this case it does sound like a piece of the main conclusion, the key data of the paper being fabricated, I think that would be a straightforward case.

(Floor) But in that case you would not ask the institution to investigate, right, because this is only minor portion of the data and main conclusion will stand, then most of the data just fine.

(I-han Chou) Any time there is an irregularity, we...

(Floor) You request the institution to...

(I-han Chou) If it is significantly serious, we will request the institution.

(Osumi) So, a related issue had already been talked yesterday, so if the wrong paper just appear but within long period probably it disappeared – probably it is there in the journal but nobody talked about it anymore by that way, so the previous fake paper is ignored or forgotten.

And to accelerate those kinds of things, one way would be that we discussed yesterday is that like system of the open-access and submitting comments so that the readers can make comments like the Amazon system or Facebook and those kinds of things can be integrated in the journal system as well. What do you think about that?

(I-han Chou) We have commenting on all papers in Nature Publishing Group research journals. I think as with all other journals, it is not the most used function that we have, and this I think is something that all the journals are grappling with, because we hear from the community that there is a need for some way to give and share feedback on papers. I think both the journals and the community are still trying to find the best way to do so in a way that people feel comfortable with.

There are some anonymous commenting sites now available like PubPeer. PubPeer is a site where you can go and make a comment, anonymous or under your own name, on any published paper. I think right now it is only in Life Sciences but I am not sure.

And people are using that a little bit more I think than the official commenting channels on papers, but still I think it is still in very early stages.

(Takahashi) Let me add just a couple of comments but slightly different aspect because what we have been discussing today is like a punishment after this cheating or fabrication has occurred, whatever the word is.

Thank you, but it is not really the point that I would like to make – or penalty maybe. But what I would like to say is our basic researchers; we do not like to be told not to do this or to do that. Then the most important thing here is that our community should be supported by trust and not by the regulation or not by the law. So, of course it is important

what we have to do after this kind of problem has occurred. But I think to me the more important thing is that how we prevent those unfortunate things by the activity of the science community or society.

So from that aspect I would like to make or let me make comments, because as Noriko Osumi pointed out, maybe it might have occurred because of a lack of free discussion or communication between labs or communities or between individuals of graduate students or postdocs or young people or even senior scientists. So unfortunately, I have an impression that this field is getting more and more competitive, too much competitive, it is getting even like business-like.

Do you like business-like science? I do not like. So, then maybe we have to remember that as Noriko's beautifully comments pointed out why we are doing this kind of basic science. So maybe let us encourage each other, not really education, let us keep encouraging each other to really discuss our data freely, let us disclose what we have discovered or what we have found. And then I think this kind of daily based effort will prevent I believe these unfortunate things. And, for example, concretely, I am afraid saying that, for example, in this MBSJ annual meeting, this is huge and big audience, but how many discussions, how many active discussions we are having. I have to say that the discussions by the audience in the floor should be much more. We expect more discussions than we have had during this meeting or during the series of the meetings in the past decade, let us say. Is that the way it should be? Maybe no.

So, I think that the daily based activity we have to make efforts to encourage students and encourage ourselves to disclose your data, my data, and not hiding data. Unfortunately, we hear more and more that like gene X. Well, of course we understand in certain circumstances you need to hide the name of your gene, but let us make it very, very exceptional, but not like common things. It should not be a Joshiki.

So, the discoveries we make should be something that can be disclosed even in this kind of conference. If not, so what is the aim of this conference is for...

(Kohara) Takahashi-sensei, please refer to the journal responsibility in this session please.

(Takahashi) Is that the main purpose? I am sorry because Noriko Osumi, I think the comment what Noriko pointed, I think this is the more important thing, then that is related to how we make up, how we write, how we submit to a journal, okay. That is it.

(Kohara) Any other comment?

(Nakayama) I want to agree with Takahashi-sensei but I really think it is too idealistic. We should recognize that science is now business, not that I like it but science is business. And if it is business, it needs proper regulation. I do not like regulation as you don't. But if we leave it on the trust basis, there is always someone who cheated and make the complete

system corrupt. So, even to protect our community, we need certain regulation – not what is given by some higher up, but we have to make our own regulation. I think that is the solution. Based on education and the free trust won't work anymore. It is not 50 years ago.

(Kohara) Thank you. Any other comments, questions, discussion?

(Floor) So I think, actually Takahashi-sensei's comment was too idealistic. Of course, I agree with you but now I think it does not work because there are different moral standards from person to person. So I believe I have a quite high, but unfortunately a small number of people in this society who have very low standard, low moral standard, who can do anything to publish.

And I came here to hear something that change this situation, so I mean the system should change so that that kind of people with low moral standard should be eliminated from this society.

(Osumi) Well, let us protect a majority of the society who are doing very well and who have very high expectations or like a dream or lots of passion, just because of a very minor population are misconducting. This is my perspective. And also, then concerning journal. I am supposed to talk about journal, Nakayama-san, am I? Okay, so let us do that.

(Floor) That minor population is disrupting this system...

(Shinohara) Okay, we will discuss such issues in the afternoon session because we invited the Nature editor so we would like to use the time for more specifically to journal responsibility.

(Osumi) Do I stop or do I continue? Just one last quick comment.

(Kohara) Quick comment please.

(Osumi) Everybody knows that getting our paper in Nature or Science is not the goal of our life; I think this is what we have to share.

(Kohara) Any other thought?

(Floor) I agree with Takahashi-sensei.

(Floor) Yes. So I think that point is...

Peer reviewing system might be disrupted.

Yeah, what is the point of the journal? You disagree with peer review system? What is the point...

(Floor) Just like he said, and in the case of...

(Shinohara) Do you have any suggestions, alternate suggestions?

(Floor) To change – it is very difficult.

(Shinohara) So now it is time to close. Okay, the last question.

(Floor) It may just ask one question to I-han? How much percentage of the referees agree to review the paper submitted to Nature if you can tell. You may ask someone, maybe someone decline then you find next one, so in total how much percentage of people you ask?

(I-han Chou) It is very hard to say a percentage because I have never counted, but it is quite high. I mean, obviously people are busy, people travel.

(Floor) Because nowadays scientists are too busy and sometimes they decline, so that means the paper may not be reviewed by a very proper reviewer, so that is one thing. I mean...

(I-han Chou) Well, that is our job as the editors at Nature - to find appropriate reviewers and to find people with the correct expertise. And we do our best to get people who are experienced reviewers with whom we have experience so that we can properly put them in context. When they give us a comment that is critical, we understand the context based on that person's background or the kinds of comments that they have given to us in the past. That is our job to keep up.

(Kohara) Okay, so thank you very much. I am sorry but time is limited and it is time to close. Some points will be continued to discuss in the later sessions, so thank you very much for your joining. Thank you very much.

(Osumi) 夕食を 7 時 30 分からイーハンと一緒にポートピアホテルでとります。も今日は皆さん英語でしゃべりましたが、彼女は日本語も大変上手ですので、もし時間があれば気軽にご参加いただけたらと思います。会費は 3200 円です。

今、このセッションのときに来ていただくか、大隅宛に E メールをください。