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e-Session 523001 - 20th April 2020

Publishing and presenting your research findings

Prof Tannock: So good day, everybody, who is listening to this presentation. I'm Ian Tannock. I'm speak to you from Toronto, Canada, and I hope that you're all well despite the problems that the world is facing. So I'm going to talk today about publishing and presenting your research findings. There is my title. And what I hope to do in this presentation is to help you understand how to first of all plan a good oral presentation, how to design a poster, and then for the majority of the talk, to talk about actual publishing your research when your study comes to its end. So let's start with an oral presentation. And if I were face-to-face with you, I would ask you this question. I would ask you, how many of you have sat through very bad presentations? And I think you would all raise your hands. And then I would ask you, how many of you have sat through very good presentations? And again, I think most of you would raise your hands. And I think the way that you can best design your own presentations is to think of those differences. What were the reasons that some of the presentations that you heard were not very good. And some of those factors are, one, there was no clear plan. People put too much on the slide. There were far too much on them. And a good guide is that slides should not have multiple diagrams on them. If you use point form, you should only about five points and preferably about five words per point. The font size on the slide should always be at least 24 or minimum 20 bold. And if you're copying in figures, and the axes for example are too small to read, then use a text box to override them, so that people can read them. Many people, even experienced people use too many slides. A reasonable guideline is that if you've got a 10-minute presentation, then you should have about 10 slides that contain information. You can add your title and conflict of interest and acknowledgement slides, but no more than about one slide per minute that actually contains information. And likewise, if you have half an hour, you can use about 30. Many people speak too quickly and don't take time to explain. And usually wen we're speaking now at large meetings, there are a lot of people in the audience whose first language is not English, or if you're speaking Italian or French, not the language that you're speaking in. So you need to speak slowly enough and clearly enough that people could hear and understand what you're saying, and you shouldn't run overtime. Practice before you give a talk. And always think about who is your audience. So if you are, for example, an expert in lung cancer and you're speaking to an audience of other health professionals who have the same expertise, you would give a very different talk than if you're say talking to a group of patients or a group of health professionals who are in a more general area and don't know the details of the area that you're speaking about. So plan your

talk for your audience. They say plan to fit your allotted time, and plan the presentation like a paper, which we'll come to in a moment. It should tell a story. Obviously, different types of presentations tell different stories. But in general, if you're reporting a study, report its design, why you did it, report how you recruited patients or the methods. If it's a scientific paper, describe the methods not in enormous detail, but enough so that people can have a good idea of how things were done, give the main results, and then a discussion. Avoid necessary details and speak slowly and clearly. So similar things apply to posters. I'm sure many of you listening to this presentation will have been to major meetings like ESMO, or ESTRO, or ASCO. And many of you will have experienced the crowds of people that are surrounding the poster presentations. And often when you're trying to read the posters, you can't read what's on them because the font is too small. There are too many things on the poster. So here are the rules that I've set for my own trainees when they present a poster. First of all, you must be able to stand about three meters or 10 feet away from it, and you should be able to read every word on the poster. And that means that the smallest font size should again be about 24. Sometimes you're asked in a poster to include an abstract. That to me is stupid, because nobody can read an abstract on a poster. If they want an abstract, then have them as a handout that you can give to people. Use point form and not long sentences. And on a poster, you can only at most make about three main points. A good dictum is the more you put in, the less people will remember. You cannot give a whole detailed paper in a poster. You can only describe the main points and the rest will come in discussion. So let's talk about the main focus of this talk, which is publication. And first of all, you should recognise that publication is an ethical requirement. If you do a research project, if you do a clinical trial and you register it, you must publish it. And you must publish it whether it's positive or negative. If not, you will help to create a bias in the literature. So failure to publish breaks an implicit contract with patients who participate in clinical trial, or if you're a scientist with those who reviewed and funded the research. So you must publish research. We all recognise that there is publication and presentation bias. Ideally, publication should depend on the quality of the study, but we all know that you're not gonna get a negative study. Very rarely you're gonna get a negative study in the New England Journal, or Lancet, or in a top rank journal, but you can publish a well done study in a good journal, even if it is negative, when you need to do so. So we have to accept that there is publication and presentation bias. Again, meetings like to hear positive results. And we have to recognise that, and that bias in the literature. And it can sometimes lead to inappropriate management of patients. If for example 10 trials are done all addressing a particular topic, then given that, we tend to think of positive results as those having a p-value of less than .05. One in 20 trials is going to appear positive by chance alone, even if there is no difference in the things that you are comparing. So we have to recognise that. And unfortunately, the one that's positive among a group of trials or scientific experiments tends to be the one that's published in a higher ranking journal and published earlier. And negative studies either remain unpublished or published later. And if you act on that, that can lead to inappropriate management of patients or inappropriate conclusions in a scientific experiment. Some years ago, with Monika Krzyzanowska who is a staff member at Princess Margaret that time was a fellow. We looked at randomised trials, so this applies even to randomised trials, that had at least 200 patients that were reported in an ASCO meeting. And we looked at the rate at which the studies were published, these trials, randomised trials of at least 200 patients, with time after the ASCO presentation. And it was a very clear differences in the rate of publication of positive and negative studies. So illustrating that publication bias applies even to substantial trials. And then we need to think about ethical issues in publication. Duplicate publication is of course not something that should be allowed, unless of

course it's a very general thing where it is acknowledged that an important announcement should be shown in other journals. Authorship has to be deserved. Ideally, when during a study, authorship or the order of authorship should be decided at the outcome. And too often we see authorship where somebody who's very well-known, a quote thought leader, is parachuted in to be the first author of the study in which he or she had a relatively small part. That shouldn't happen. Scientific misconduct, including plagiarism, of course is a gross ethical, unethical thing. Conflict of interest is something that we all have to declare. I have done a review of this and basically about 80% of medical oncologist at least are accepting some form of payment from companies. I do not think they should do that. I think that really we should avoid taking money from companies, at least personal money. I'm not talking about money that is required to actually undertake a trial, because it has the effect of taking away our independence. It's very hard to criticise people if you're taking their money. Before you start any trial or any scientific study, you really don't need to undertake a systematic review of the literature to see how your study fits with what is being done before, and that systematic review of the literature will provide you with the necessary references that you will need to acknowledge when you write your paper. So in doing that systematic review, you need to search database of publications, such as PubMed and the Cochrane Library. You need to reference the list of relative particles, and you can even write a review yourself if there isn't a good systematic review. And I find this rather old paper, 17 years old now, as a pretty good guide on how to do this, and you can also talk to your friendly librarian who will also help you. So when you're doing a study, when you come to publish it, you need to frame the question for review in a clear way. You need to identify relevant work using an extensive search. You need to assess the quality of studies, use a quality checklist to refine selected studies, and there are formal ways of grading studies, and then summarise the evidence. And if possible, produce a meta analysis and explore heterogeneity. Sometimes you will find that different studies studying rather similar things end up with different results, and you need to ask why that is. And then interpret the findings and consider sources of bias, and if possible, grade the recommendations. In developing principles of authorship. As I said just a couple of slides ago, ideally, authorship should be decided before a study is opened. All authors should've made a substantive contribution to the study, and there are guidelines for that among a committee of journal editors that have set guidelines for authorship. The study chair is usually responsible for drafting the manuscript, and all authors must approve the final manuscript. Other authors would usually include a statistician for a clinical trial and a lot of scientific study. Investigators who are active on the steering committee Not silent members, but those who are active. And if it's a clinical study, investigators who accrue more than a predefined number of patients. Some people contest that, but you can't really do a study unless you have the help and advice of your colleagues. What should you include in the abstract of a paper or a presentation? Well, first of all, every study should have a primary endpoint, and it should be defined explicitly. The concluding statement of the abstract should refer back to that primary endpoint. I'll come to ways in which that has been circumvented, poorly I think, in many papers. The abstract, if it's a clinical trial, should include a statement of major toxicity. unfortunately many reports, both in high level journals and the meetings of ESMO or ASCO don't meet these simple requirements. Franco Vera-Badillo who came originally from Mexico, but is now at the clinical trial centre in Kingston, Ontario in Canada studied this when he was a fellow working with me a few years ago. He wrote a couple of papers looking at bias in reporting. The first one was looking at all the randomised controlled trials that are being published for breast cancer in a period between 1995 and 2011, about 16 years. And about 2/3 of them didn't show a significant difference in their primary endpoint. About 1/3 of them

did. And in 60% of those trials, they didn't show a difference in their primary endpoint. The concluding statement of the abstract use secondary endpoints to suggest benefit. There's a lot of bias, a lot of spin in the literature, and we should try to avoid that. And only about a third of them indicated the frequency of high grade toxicity in the abstract, and that was more common in the trials that were positive for their primary endpoint. So recommendations for what not to include in an abstract. Analysis of endpoints that are not pre-specified in the protocol, reports of subgroup analysis that are not pre-specified. And this is data dredging or data torturing, and can give results that are statistically invalid. The more you search, the more p-values that you apply, the more likely you will turn up something that could have occurred simply by chance alone. Then writing the paper. It's relatively straightforward. Background, keep it simple, keep it brief. Generally, one-page, double-spaced manuscript is enough. Just tell people why is the question. Why did you study it? Why is it important? What information is already known and what is not known? Not in great detail. You can go into a bit more detail in the discussion, but just a sentence or two at that background, how you've addressed this deficiency and what was the objectives of the research? Usually the final sentence of your background should be a statement of the objectives or hypothesis that you're addressing. Now the CONSORT has a statement of what should be reported in a clinical trial, and I've listed that here. It's fairly straightforward. The methods should describe the participants, interventions, etcetera, in sufficient detail that people can reproduce your study. Results should show the baseline data, the properties of the participants, the numbers analysed, the outcomes, particularly your primary analysis, and obviously the discussion you interpret. You look at the generalised ability, and you look at the benefits and problems of your study. So let's go to methods. How did you design the study? Which people were recruited and how? If it's a clinical study, what were the cell lines or animals that you use in the scientific study? You should indicate your tools, such as questionnaires, say if you're measuring quality of life, measures or devices. Now of course, many journals set a page limit, typically about 3,000 or 3,500 words. So often these can be in an online appendix with a link to that. You should have a statement about statistical methods. You should describe the ethics clearance, and it should be detailed enough together with an online appendix if necessary for someone else to reproduce what you did. If you're doing clinical study, use a CONSORT diagram for showing the flow of patients. So here's just a hypothetical sample one. How many people did you assess for eligibility for your trial? How many were excluded and for what basic reasons, without great detail. How did you allocate to the intervention here? We're just talking about a simple phase two type trial. How many actually received the treatment that you prescribed and how many didn't? How may were lost to follow up and discontinued? And how many you analysed? Ideally, your analysis should start right at the first box to indicate the representation of your data through that population. Results, report your main result, your primary endpoint. It's okay to have pre-specified subgroup analysis, but you should recognise that unless the study is very large, these are only hypothesis generating. They shouldn't be used as conclusions. Try to tabulate and graph. People can see things much more easily from a figure than they can even from the table, and more easily from the table than things loss in the text. Don't repeat what is in tables in a text. Just give a very brief summary in the text and refer to a table or a figure. Don't over inflate your data. If you are reviewing, many of you review papers as do I. And if I see somebody who's written a paper claiming that this is wonderful, its results, and very important, it makes me rather suspicious. Whereas if people are rather humble, then I think one tends to give them more the benefit of the doubt. And don't slip interpretation to results. Leave that to the discussion. Discussion, again, don't write too long a discussion. Start with a summary statement of the main finding, then describe why

it might be important, surprising, concerning. How did it compare to other work? Refer to other major contributors to the area. Always include a paragraph that lists the main strengths and main limitations. Much better you list the limitations than reviewers discover them for you. Give the implications for practice, implications for future research, and in conclusion, give your main point. Use simple words. Some people write in a very flowery language. Words like however never really belong. So instead of a majority of, most. Now. In some cases, sometimes. Look at the examples there, they can all be replaced by simple words. Somebody said good writing is like sharpening a pencil. The shorter it is and the more direct, the easier it is for people to read it. What to avoid in your paper. Length. Nobody wants to read a long paper. We're all too busy, so we want to be able to assess it whether it's good or not, but in a short period as possible. Don't make conclusions that are not based on the primary endpoint. Don't include multiple significance test, analysis of subgroups. If you analyse subgroups at all, always with a caveat that they're not a major conclusion and they're only generating new hypothesis. Don't overstate your data. Don't claim to be first. Let other people judge that. So if you claim to be first, it's quite possible that you've missed something or somebody else is doing things and will be publishing before you. And avoid direct criticism of others. It's okay to give constructive criticism of other data that's been published, but don't make it person. Criticise the data, not the person. And don't compare single arm studies, because there are so many factors that go into the background of recruitment to single arm studies that they cannot really be compared. Multiple significance test or data torturing. You fish for p-values. They are used in comparing treatment effects between randomised groups due to use of multiple endpoints, or subgroup analysis, or repeated analysis of data after different times. And again, the more p-values you include, the more likely you'll fish something that occurs by chance alone. Here's one example. Years back when 5-FU and levamisole were first described as adjuvant treatment for Dukes C colon cancer, there were two major trials, the Mayo Clinic trial, which had a subgroup analysis showing that the treatment was more effective for men and older patients, and the Southwest Oncology Group trial that suggested that it was more effective for women and younger patients. And of course, almost certainly neither of those are true. There was an effect. There was an effect to that across all patients. And if you were doing the subgroup analysis, you could find by chance that something appears more effective in one group than another. And there's even been a paper where they've analysed outcomes by the sign of the zodiac under which patients were born. Selecting a journal. It's really a balance between ambition and common sense. It's reasonable to try first a journal with a high impact factor that publishes your type of research, but you're not gonna get a small phase two study in a journal like The New England or The Lancet. Often you will be rejected from your initial submission, and you'll need to submit to a journal with a lower impact factor. One thing to avoid a scam journals. Probably most of you get four or five emails a day asking you to submit to some new journals you've never heard of. These are scam journals, and you should submit only to those who are referenced in PubMed. And what to do with rejection? We all had multiple rejections. Don't take it personally. If the paper has been reviewed, I think it's useful to use the feedback provided, because sometimes reviewers do point out defects in your paper that can be addressed. If you can, refocus and resubmit quickly, or sometimes you might want to, if you're in a laboratory, do another experiment to address the criticism, and seek help of a senior mentor. So that's the end of my talk. Happy writing and presenting, everybody, and I look forward to reading your papers.

Prof Franco: Thank you. Thank you very much Professor Tannock for this terrific talk. It's been very, very clear and interesting. We can start a little bit of discussion if you agree.

Prof Tannock: Sure.

Prof Franco: I was intrigued by some of the points that you clearly stated in your presentation. The first one I would want to address is the ethical obligation in publication, right? I know that, for example, prominent bioethicist state that it's worth taking the risk of running a clinical research only if society can gain in terms of knowledge. And also like the Helsinki declaration, the Helsinki statement suggest that it's very, very important and it's like an ethical obligation to publish your result, but I want to know your opinion. And what is the core, the main core of this ethical obligation? It's because it is a respectful behaviour for the patient that have been involved in the research, for the people who funded the research. It is important because it provides knowledge to science, or you think it also belongs to the social aspect of science. It is because science is very meaningful if what is found in science can have a declination in the society and can make society better and people's lives better. So what is the core for you in this ethical obligation?

Prof Tannock: I think, Franco, Francesco, I think it's all of the above. But I think if you're doing a clinical trial, then the most important people to whom you have an ethical obligation are the participants, the patients who took part, because they will have signed a consent form, and that consent form will have stated that in taking part in this study, there may be no benefit at all to themselves. Maybe of course if the trial is positive, but consistently we have to tell patients that, in fact, the majority of trials do not lead to great benefit to the individual who is participating. And those consent forms will also state that there may however be benefits of the study for people like yourselves who are in a similar situation in the future. So that's part of the core consenting of patients to take part in a clinical trial. And clearly, you cannot have any benefit for patients who participate in trials in the future if you don't publish the findings of your study. And even if it's a negative study, a negative study can then actually prevent patients from receiving an ineffective treatment in the future. So in many ways, that's just as important as publishing a positive study. So I think the core at least for clinical research is to the patients. Now obviously that's a little bit different in experimental research. But even there, there have been a couple of papers, one in Nature, and I can't remember the other, but one form Berkeley, and Alice, and Nature, one from a German group, showing that reproducibility in early phase trials of drug development isn't very good. And I think, again, it's so important that all the results be published and published in a way so that the methods can be looked at, otherwise we can be misled down the road in clinical development, and of course there is an ethical obligation also as you say to those funding research, to society in general. To me, those are the core ones.

Prof Franco: Thank you, thank you. So you mention about the need to publish also negative results and inconclusive result I would say. So that leaves us to talk about the publication bias. So I was surprised because I read a few papers and review, that we are comparing the number of literature review reporting positive trial compared to the positive FDA review for antidepressant agent. And there was a striking difference. 94% of the literature review were positive compared to 51 FDA positive review. So there's a striking difference when assessing positive results if we take a look at the literature compared to what is really into clinical practice. I was just wondering, who do you think is this fault? It's on the authors? It's on the editors? I really think like the editor should really rely and judge on the quality of the research, of methods, not on the conclusiveness of the results.

So who do you think is the mostly responsible for this publication bias which is present in the literature?

Prof Tannock: Well, there's one other group that you haven't mentioned, and that's pharmaceutical companies of course. I mean again, there is responsibility for all, but I think, first of all, the authors, because the authors of negative studies, I can understand that they submit to a journal. The editor may reject the paper because he doesn't find it interesting enough. I mean the editors of major journals are stating, "We like to publish things that will influence clinical practice, and a positive study is more likely to do that than a negative study." But with the ability to do a review, anything that is now in PubMed can be found. So you may want to go. You didn't get your paper into a journal that has say an impact factor in 20s, but you can publish it in a PubMed review journal with an impact factor that may be between five and 10. So I think the ultimate responsibility does lie with the authors. But authors are also, you know, the like to be known for having prominent papers in high impact journals. They're gonna be more enthusiastic about publishing the positive results. There is certainly pressure from pharmaceutical companies to publish positive results. And you mentioned, I think you said antidepressants. I think it's there that this have been found to be huge pressure from companies to publish positive results. And only when you do a more complete review, you may see that they are counterbalanced between the benefits of particular product and some of the toxicities of that product. So I think there is some pressure upon people to publish particularly when they're doing pharmacological, pharmaceutical responsive studies. I've been the PI on both positive and negative studies that are being sponsored by pharmaceutical companies in prostate cancer. I didn't actually find it difficult to publish the negative study, but there certainly is less enthusiasm from the company to get it out. You have to really persevere.

Prof Franco: That's fair. So you stated that of course is important to publish positive results. But at the same, it is important to publish negative result. I want to know your opinion also on the importance to publish inconclusive result, at least for people doing research. To me, my feeling that it's also very important to publish inconclusive result because it gives the researcher a perspective on where to do properly design and study research, right? It's like a hiatus of knowledge that you can feel with your new research. So my feeling is it's also important to publish inconclusive result, not only positive or negative.

Prof Tannock: Yeah, I agree with you. Absolutely. I mean probably the best example of this is where clinical trials have started. They don't meet their recruitment goals so that the study is underpowered, and it may or may not show a trend to benefit in an outcome-like survival, but not a statistically significant result. I think it is important to put those into the literature and particularly for underpowered randomised controlled trials. It also offers the opportunity for people subsequently to include those trials in some form of either meta analysis or systematic reviews. I think the information is still useful. I mean let me think about a specific example. If we think of trials of adjuvant therapy for bladder cancer, give that I'm largely a GU and breast oncologist. There being several trials that have been published which many of them have been of the order of 200 randomised patients, large enough to show or exclude an important difference in outcome. Most of them have a trend towards benefit, and that does allow to say the Cochrane collaboration to take those trials and make some conclusions. Yes, that probably is a benefit of that therapy. So I do think that's important that we publish, I mean not trivial trials. I mean you're not gonna be interested in a 10-patient inconclusive phase two trial than a common type of cancer, but trials that have some

substance but haven't reached their target goals. It's important they also be in the literature. And generally, journals are willing to publish those.

Prof Franco: It makes a lot of sense because that I think like if you had like similar trial with similar trial design, with similar endpoints, I mean trial investigating the same clinical scenario and clinical outcome, and maybe they're under power because of a wrong simple size calculation on low accrual, then hypothetically, then in the future, then you can combine all the trial on an individual patient meta analysis, and then there you can properly analyse the outcome, and you can come out with a solution and with a reliable result, so I think that makes sense.

Prof Tannock: Yes, I agree.

Prof Franco: I was also interested in knowing your opinion on reporting methodology in result of studies. You mentioned the CONSORT statement and the standard of reporting. That I would say mostly apply to prospective trial. So it was interested in knowing your opinion on the way of reporting and the quality of reporting methodology result and data in general for retrospective or even observational trial. For example, I'm aware of the STROBE statement, which is the strengthening the reporting of observation study in epidemiology. So I would say the quality of reporting, does it apply also to observational study and retrospective study or only to perspective studies?

Prof Tannock: No. I agree with what you're saying there that for any study, we need to know how it was done, even if it was a retrospective study. For us to judge how valid it might be, we need to know how, if it's a clear retrospective clinical trial, how the patients were selected, what was the larger number of patients, how representative might they be for the patients that we see in our own clinic. So I think it's very, very important to report exactly what was done. In very rare diseases, we often have to rely on retrospective findings or smaller perspective trials. And to look at those, we really do need to understand how the patients are recruited. One thing to mention here is the efficacy effectiveness gap, which incidentally we just had with some others, we just had a commentary on in JCO. And what that is is a recognition that patients in clinical trials are quite strongly selected. And particularly pharmaceutically sponsored clinical trials, generally speaking, they're recruiting patients, patients with cancer who are otherwise well, patients without comorbidity. Yet when we go to our daily clinics, we're treating people with hypertension, who have a myocardial infarction may have some liberal kidney abnormalities, many of whom would not get into the clinical trial, or simply are older. Many trials have an age cut off, which may be say 70 or something like that, and yet we treat with the same types of treatment patients who didn't go in the trial. And the efficacy effectiveness gap, and there are many examples of it in the literature, shows that the outcomes are invariably poorer. The positive outcomes like survival have a response rate of poorer in the unselected population than in the clinical trial, and the toxicity is greater. So when we come to review retrospective series or real world series, then it's important to know how they're selected so that we can judge how representative they are. And I think ultimately, in adopting a new treatment, so one that's been shown to have some but not a huge benefit in a clinical trial, then the health outcome reports, the registry or hospital registry-based studies, are important for other information. Because if you only have a small benefit in the trial, you may lose that completely in the real world. That's not just likely if you have a large effect size and a randomised trial. Well, let's face it, most of our cancer clinical trials don't show large effect sizes. A few do, but most do not.

Prof Franco: Yeah, thank you. That's very interesting. Another point I would want to address, if you agree, is the value of subset analysis in clinical trials. So pre-specified subset analysis can be reliable in terms of outcome. I mean you can rely on the outcome that is provided by the subset analysis. If the subset analysis is not preplanned, how is your feeling? Do you think it's completely useless, or it could be a proof of principle for new study design or for a properly selected trial design, which could be run thereafter.

Prof Tannock: Yeah, I'm not even sure that I agree with your first statement here, Francisco. Most trials are only powered to look at their primary endpoint for the whole group of patients. I mean it's certainly reasonable to look at pre-specified subgroups. They might be men vs women. They might be older versus young patient, a pre and post menopausal in a breast cancer trial. But usually the studies are not powered to give you a definitive answer about that, and showed you the example from the original adjuvant trials and colorectal cancer. So I think even there you have to be careful. They obviously have more statistical foundation if they're pre-specified, because usually that means a limited number of findings. But often there, there's still, they still are mainly, unless the problem is very large or part of a meta analysis, they're probably more reasonably to give a hypothesis for a future study. If you don't pre-specify, you can look at an endless number of subgroups. Years ago, I actually wrote a paper in JNCI where I worked out the number of inferred p-values in a number of reports, and often just by the number of things that they look at, and often the number of inferred p-values in the paper was more than a hundred, even though they weren't necessary quoted in the paper, because you could tell from the methods that they compare lots of different things, and they could generate lots of p-value. And when we're talking about p-values, really there's a statement from the American Statistical Association on their website, and there's a paper, say really we shouldn't be judging outcomes of clinical research on the basis of a p-value. We should be judging it on the importance. A p-value doesn't tell you about effect size for example. One of the reasons that our trials have got bigger and bigger is mostly are pharma sponsored, and they know that EMA and FDA will approve new treatments on the base of a p-value. So if you do a large enough study, even a relatively small finding can give you a positive p-value. What we should, in my opinion, be moving towards as a value judgment, using his measure of clinical benefit scale or the ASCO value scale, rather than the p-value, and many statistically positive trials do not meet criteria of value. So I think we have to be coming back to subsets, so we have to be particularly careful about how we interpret them.

Prof Franco: So you would be very, very cautious in judging reliable, the data or the outcomes coming from a subset and all, even if preplanned. And the other point you address is like being very careful when, to multiple testing hypothesis within, particularly for slender sample sizes.

Prof Tannock: I mean if you put together like the Early Breast Cancer Trialist Group have put together large numbers of reports and studies to finding, and they have thousands of patients and a limited number of subgroups there can still include two or 3,000 patients, then I think you have fair confidence on the findings. But in most single trials, I don't think you do have that confidence.

Prof Franco: Okay, thank you. So I have one more question or let's say one more point to address, which is about a way to cope with the rejection. We say that normally we shouldn't be too much affected by the rejection of their study of their paper, which is like a normal phenomenon in life and for those involved in research, which is fair. But you know, my experience is that sometimes it's very hard, sometimes I mean you do your research, you submit your paper, and you wait for an answer

from the journal and from the reviewer. Maybe you can even expect that your paper will be rejected, but what you want is that your paper will undergo a proper reviewing process, because that will give you the chance to improve your manuscript, to learn and to let's say be a better scientist somehow, but sometimes you just get the answer from the editorial staff. So your paper is not of a high priority for the journal, so you, Okay, you get a rejection, or even you get like a formal review, and the reviewer would say, "Okay, this is like not a good paper." The paper is gonna be rejected. So you don't have, Let's say you don't feel that your paper got precise review process, and that you feel that it doesn't help you. So you think there is also an ethical obligation for the journal, for the editorial staff of the journal to provide you with a reasonably detailed reviewing process or it is not like this.

Prof Tannock: I think it depends on the journal. If you go to the high-ranking journals, I mean I don't know the numbers, but I think a journal like New England, Lancet, JAMA, they probably send no more than 10% of their patients out for review. And let's face it, it's often hard to find reviewers, particularly for lower level journals, because everybody is overburdened. So I think we just have to accept that the editors at least of a higher ranking journals are going to make decisions. I was recently at meeting with David Collingridge who I know well, who is the editor of Lancet Oncology, and he was saying that, you know, he and his co-editors first make a decision about whether the paper is appropriate to their journal, and I can't remember the numbers, but it's not more than about 20%. You have to see that they've got a job to do, and getting reviews from the other 80% is not gonna help them, and it's going to completely swamp their pool of reviewers. I think you have to be sensible about where you submit. I mean you got a large randomised controlled trial and you're the chair or the principal person. You send it to a high level journal. But if you're publishing a phase two study with a more limited number of patients in a common area or if you're studying a scientific paper that is interesting but is not gonna be earth-shattering, I think you need to send it to a journal that publishes papers like yours. If they are publishing papers of a similar type and size of your study, then I think it is reasonable to expect that they will give you a review from which you can learn. Obviously, the reviews are as good as the reviewers, and some reviewers are very good. Some journals, which I think is a very good thing, actually rate reviews and will give you CME credit, continued medical education credit, if you do a good review. I think we need more of that. We need to encourage good reviews, but one of the problems... I mean I refuse. I only will review papers for journals that I actually read. That's the sort of criteria I set, but we all get a lot of papers for us to review, and we could only do a certain number of them. There should be feedback to review. If I see a review will come back, like the last one you said, this isn't a very good paper and I don't think it's suitable for the journal. Well, that isn't telling you anything. At least, if you read the paper, you can at least make a few comments as to why you don't think it's a very good paper or why you think it shouldn't be accepted.

Prof Franco: Yeah, thank you, thank you, thank you, Ian. This is very helpful. I don't have any other point to discuss. So maybe, Ian, if you want to address some final remarks for the audience.

Prof Tannock: No, I don't think so. We've concentrated on publication. I guess I would just emphasise presentation, because I know we all read papers and we can read good and bad papers, and learn. But we all sit through large meetings, and I think as well as the quality of the science, it's very good for your own future and your own planning of presentations, particularly of younger

people, to think, was that a good presentation? Was that bad? And how can I design my own so that I am regarded as a good teacher and presenter?

Prof Franco: Yeah, presentation is also very challenging, very important. It's not easy to be at the same time like scientifically reliable, clear, but somehow entertaining, right? It's not easy. I think it takes experience to come up and find a balance between the scientific reliability but also the entertaining part of the presenting part.

Prof Tannock: Yep, I agree. Thank you.

Prof Franco: Thank you, thank you very much, Ian. It's been terrific.