**AM-Pt2-Stephen Taylor**

PROFESSOR NIELSEN:

Thank you everyone. If one were to google the old adage, if you want something done, ask a busy person, then one's search immediately leads one to Professor Stephen Taylor. Stephen is currently the Associate Dean Research in the UTS Business School. He's a director of A member of the CIFR Advisory Council. On top of those roles, he's the current chair of BARD's Nest, which is the Business Academics Research Director's network, with the Australian Business Dean's Council. Stephen is published in some of the highest quality journals in the accounting discipline He has been the recipient of both ARC Discovery and Linkage grants and has a wealth of editorial experience. We're very, very fortunate to have Steven and we've asked today he wouldn't even allow me to pay his air fare [LAUGH]. Will you please join me in welcoming Stephen Taylor.

[APPLAUSE]

STEPHEN TAYLOR:

Thanks. So that's working? Thanks, Ingrid. You didn't have to add the bit about the air fare on the end. But, you know, I'm a great believer in trying to do things as efficiently as possible. And basically one of the few advantages of being the associate dean of research is that I've got a travel fund which I don't ever actually seem to spend, because I don't really travel that much. And the time it would take To get the paperwork done for Deakin to reimburse UTS the return to Melbourne just doesn't make any sense at all. It's just better off if my ADR account pays for the airfare. This session is and thanks very much to Ingrid for asking me to do this and I was very happy to do it when I was asked because I guess. Like a lot of associate deans research in Australian Universities, no matter what the academic area, I'm just at a point in time where the amount of, kind of, busy work to do with grant administration, and in particular, era submissions, and I think I've turned my phone off, but if it rings it's going to be a very cranky deputy vice chancellor of research.[LAUGH] But, It's kind of nice to get back to talking about what I really like doing which is doing research. Basically what I wanted to do today was to try and encourage you to be as interactive as possible, it's a large room and there’s a lot more people here than I was expecting which is great. But please just put up your hand and ask questions as I go along. Don’t be timid. If I say something that you find a bit difficult to apply in the context in which you're working Then by all means throw that issue out there, and other people can make suggestions as well. But I call this session some basic but frequently forgotten issues, and the reason is because I actually do think these are very basic issues. And that comes back to what I see as a reviewer, as well as current and past experience in various editorial roles. I do think a lot of these issues get forgotten. And you see it a lot in terms of papers that people come and present in workshops and so on. At least they're presenting it in workshops and I’ll come back to the importance of that a bit later. But I think people just sometimes forget the basics Of doing a good job of writing in a way that gets an academic paper published in as good an outlet as you can reasonably expect, given the underlying nature of the research. Now, you can think carefully about some of the points that I'm making today, and think, am I doing that with my research? And if I'm not, well, that's going to make it much better and it’ll get published in better outlets. Well, maybe, but I mean I would make the point that the kind of things I'm talking about, to a large extent, what's that old analogy, can't create a silk purse out of a sow's ear? And you can't, it doesn't matter what you do. You could be the greatest salesperson in the world at what you're doing, but if the product's no good you're not going to sell it. And so you can't avoid that aspect of the problem as well. So what I wanted to do today was basically just focus on some of what I think are basic issues.

The first one that I see a lot in academic papers is a lack of clear structure. And I'll come back to what each of these is about. The second one is sometimes people do a terrible job. When they write a paper, writing it in a way that sells what they've done as well as possible. It’s not just about what you've done, it’s about why it's important, why it's interesting. If I read it as a referee, and ultimately they're the people you've got to get by. If we're talking about published publishing in good quality, academic journals, you need to get your work through referees. So the question a referee always asks themselves and I always ask myself is why is this important? Why should I care about what you've done? So that's particularly important Another thing that I find frustrating and this is particularly in the context in the type of the work I do and I’ll contextualise that on the next slide very quickly in just a moment, is lack of sufficient detail. So particularly people who were doing empirical research. Particularly archival empirical research like I do using data over extended periods. Large numbers of observations. And typically some sort of regression type analysis is going to appear somewhere in the back end of the paper. It’s very frustrating when I see papers that I can't figure out what they did. I can't understand exactly what the data is or I can’t understand exactly what they've done to produce the results. And, that's always a worry, when you're reading a paper as a referee. So, most of what I'm talking about today, try to think of yourself as the author and me as the referee. Or the editor. And in fact, all the editor usually does is work off what the referees say. So try to think of me talking from the perspective of a referee. Another problem that I often find is that people fail to properly discuss their results. They just think that the results of the paper speak for itself. OK, so very little discussion or contextualisation of their results. And a common problem, common problem that you see is the lack of any, and this doesn't matter if we're talking about theory or general research, is a lack of robustness, or sensitive analysis. In other words, people just run a set of results, think that's it, and move on. There’s no consideration of whether the results are sensitive to how they've tried to capture the construct of interest, how the particular research design they've used, and of course what that inevitably shows a referee is that you don't know the literature very well. If I can see problems that you haven't thought of, that are just obvious from my knowledge of the literature, and that's why I got the paper as a referee, because I've got some understanding of that particular literature. If I can straight away see things that. Pretty much a well-accepted issues that apply to your work and you haven't thought about how sensitive your results are to those issues, I'm gonna bin it. All right, let's be blunt I'm not gonna put a lot of effort into the paper because it sends me a signal that you don't know what you're doing as well as you need to. And the better the quality of journal you're pitching at, the quicker I'm legitimately gonna reach that conclusion. In terms of what journals I do refereeing for, in general they'd all be A-star or no worse than an A journal. and so if I look at the paper and it's just plainly obvious there's things that a reasonably competent researcher in that particular area of research would always check at some point and you haven't done it, that just tells me that this is probably a waste of time. I'm not going to, you know, invest a huge amount of time in writing some incredibly lengthy referee's report. And as an editor, I wouldn't expect me to either as the referee. And finally another point that I think is often a problem is that people don't acknowledge the limitations of their work. So, I'm really trying just to throw out all the primary issues here. You'll notice that I said people often fail to sell the work. Well, sometimes people do a very good job at selling the work. In fact, they do such a good job at selling the work that they just fail to ever step back and recognise there are some limitations. And this I know there are probably a lot of people from the law discipline as well here. So even in terms of legal research and writing, which often, perhaps, and I'm using the term that makes sense to me, maybe not to you, but often has a bit more of a kind of advocacy style approach to the writing.

Again, you've still gotta sell your legal research at the start of the paper. But at some point, you need to be a little bit modest and recognise there may be some limitations in the conclusion in your legal reasoning, particularly where you are disputing somebody else's reasoning. So there's a balance, in other words. You've gotta try and balance selling what you're doing, why it's important, why it's likely to be of significance. But on the other hand you do actually need to retain some degree of modesty as well. And then I'll just finish up with a few concluding comments. Just very quickly, the reason I thought it would be useful to cover my background really fast is just to give you an understanding of the perspective I'm looking at this from. So my work is in the intersection of financial accounting and what a lot of people might call finance. and so most of what of what I do is with large databases, and that ranges from a lot of stuff that's looked at Australian capital markets, and also corporate reporting in Australia across other countries in Asia and also using US data So the kind of topics I look at, how information affects prices, the economics of auditing, the measurement of earnings management and earnings manipulations, and the causes and consequences of that. And when I talk about a referee's perspective, I'm really talking about here from what you look at high quality journals. Which I would approximately define as the A star journals in my area. As well as, the major higher ranked A level journals. And this is also important not just for what you do in your own research, but what you try to impress upon your post-graduate research students as well. It's very frustrating to me when I see a PhD student present a seminar, and you look at those points I just said I'm gonna cover in this presentation, and pretty much all of them are issues. With what they've done. We shouldn't be producing PhD graduates that don't understand that they're important points. So, it's not just what we do ourselves, it's what we try and do to encourage our research students, as well. So, let me start by talking about how a lot of papers often have a lack of structure. As I said, please just put up your hand if you've got any questions or even an example that you might want to share. Now I'm pretty old and the older you get, age has its drawbacks I've gotta say. But it also has the odd plus. I'm always trying to impress on my children that age has some plusses, you know more than they do. Well, maybe not about information technology [LAUGH], or social media [LAUGH]. But there are some things I'm comforted I know more about than my kids. But here I think being old is a bit of an advantage. And the advantage is that I'm just old enough, and there's a really fine line in this, I'm just old enough to actually remember when re-writing something was an incredibly expensive exercise.

Ok, because what you had to do was actually either get liquid paper and use it or just start again. Right? Now, I want to ask some in the room, particularly the under forty brigade. Can you imagine think about the last time you were working on a piece, writing out a piece of academic research. How many times did you change your mind about what you'd written on the screen and just go delete, delete, delete, or just chop out a paragraph or something like that and start again Alright? Well, when you used a typewriter in the world before word processing which is basically from the kind of early, mid 80's onwards. So I'm just old enough, my honour's thesis was done on a typewriter. All right? It was actually typed. And that's 1982 I think. So I'm old enough to remember when you had to adjust the problems that we don't have, some people think we don't have now, because of word-processing. So what this has done, in my view, is breed a generation of researchers who don't plan what they're doing before they write it. In other words, people just sit down at a desk and go. No, that doesn't read right. What was I trying to say? Right, here's an alternative. Think you're back in the old days. What you had to do was construct a plan. As to what you are going to say. You have to have a plan. What am I going to say in the first paragraph? What am I going to say in the second paragraph? What am I going to say in the third paragraph? In other words, what you needed was a clear writing plan. Otherwise, you were just making a mess with the typewriter. You had to plan out. Now, what's the advantage of planning, clearly on a notepad, what you're going to do when you start writing a paper? You're putting some structure on it. And the first problem, that I have as a reviewer, is when I see papers that lack appropriate structure. Now that means you got to think about each section of the paper. First of all, there are some papers that I would do where the introduction is quite long and that is because it's the nature of the paper and such that it doesn't really need a kind of literature review section.

On the other hand, there is some where the introduction is pretty short, but then the next section is quite lengthy and there's a lot of literature to discuss, to kind of position what the paper is really about. Now, in either case, what you need to do is sit down and plan that out. Actually force yourself to sit there and do a dot point plan of the paper. and then try and write the first draft to the plan. Because you've thought about what you want to say first, and the logical order you're going to do it in, before you do it. Now just as an exercise I actually have looked at a couple of times recently at essays One of my kids who's doing the HSC has done this year for Extension English. And I actually stood and observed what he was doing for a bit. And he writes just like every 16 or 17 year old. He just sits there and types and then he changes his mind I said you know did you think about what you wanted? Oh yeah I knew I was going to say this this and this. I said but did you plan that out first? I mean, it's just becoming a lost art because they don't see a cost in changing their mind. But they change their mind so many times that in the end what you end up with is a mess. So the first thing you need to do in right in doing a good job of writing an academic paper is to have a very clear plan as to what that paper is going to look like. That's really the first challenge that I think people often fall down on. So, let's just take an example based on part of a paper. Let's talk about what we might generically term the literature review section, right, which will often be kinda the first section of a paper, people who do work like me, the first section of the paper after the introduction. Okay, well, how can a writing plan help me there? Well how many key points do I need to cover in the literature review? What do I need to draw out to the readers attention? What's important in terms of helping the reviewer understand what I'm doing and why I'm doing it? What are the key arguments? What are the key results in previous papers, what key innovations in research design might have occurred? Is there some sort of "common mistake," and I've put that in inverted commas, is there some kind of common mistake from your point of view, That a whole bunch of existing papers have. That the real point of your paper is to try and show that that is a mistake in earlier papers.

Now if you look at those sub dot points that I just put up there, those one, two, three, four, five dot points. Basically, how important each one of those might be is really a function of what the real purpose of your literature review is. Think about what it is your paper is trying to contribute. Are you basically trying to introduce a new line of argument? Are you trying to focus on a result that's quite different from other papers? Are you identifying a flaw in what people have tended to do in this type of research? What is the, if you've got the purpose of your paper clear in your mind, that would guide how you've planned the literature section. And I'll show you what it's like an absolute red flag to review as a good journalist. It's when you pick up a paper, and my normal approach as a reviewer is I read the abstract, I read the introduction, I read the conclusion, and I look at the tables. In fact, if the paper's really well-written, that's almost all you should have to read to understand what they've done. But, when you flip to the, kind of, section two of the paper, what I'm generically terming literature review, and, or hypothesis development, what you see as you flip the pages is a series of paragraphs, one after the other, and every paragraph starts by something like x and y and the year of the publication say. X and y showed. A and b found. You know? And someone, paragraph after paragraph after paragraph. Ok, if you're writing like that, then you're not thinking about the purpose of your literature review.

As I said, is it a highlight before of what people have done? Are you trying to introduce a new argument? Are you extending research in a fairly incremental way? The answer to that question would guide the way you structure the literature review. But your literature review should be around the theme that you are developing, not just literally paragraph after paragraph saying A said, B said, C said, D said, E said. What I'd encourage you to do is if you go and look at the best journals in your field Right? So from my point of view in the business field if you're in accounting or finance or marketing or economics or management go and pick the top three journals, pick the last issue of each one, and try and find one paper that does what I've said at the bottom there. A said B said C said D said You won't find that. You just don't see papers like that published in really good journals, because essentially the paper is written so that it's built around the purpose of the paper. It's not just kind of like a tacked in summary of a whole bunch of other people's work. Because that's essentially what people often tend to do. Because they haven't really forced themselves to think about what the purpose of the literature review type section is. Now I could construct a similar slide on each section of what, for me, would be a typical paper. I'm just using that as an example to try and highlight why I think it's so important to actually have a clear structure to your paper. And, that really means having a writing plan. And, one other little plea Sometimes, deep in the subcon, I think this must be deep in the subconscious. When we don't have a clear plan about what we're trying to write, somewhere deep in our subconscious, we know that. And so, what we're inclined to do is keep doubling back as we write. Because somehow we're worried the theme got lost to the reader. So every time we move to a different point, we like to remind the reader of what the point was that we'd made just above. Okay? If you're doing a good job of writing, using a good flow of argument You don't need to keep reminding the reader what you just said. Right? I won't have fallen asleep, I'll be wide awake and paying attention, so I don't need to be constantly reminded of what you said earlier in the paper. You know, if you read your, if you look back at what you're doing, and you keep noticing that you're referencing stuff earlier in the paper, Then that probably is like a warning bell for you that you're not really getting a logical flow clearly enough. And you're trying to kind of triage the paper by kinda constantly trying to help the reader remember what it was you'd said earlier. That's not gonna fly in good quality research outlets. Okay, let me now move to a second kind of issue that I think is always important to keep in mind. And that's failing to sell the work. When you send a paper off to a good academic journal, you know that there is a very high rate of rejection. So, you know, in the A star journals in accounting and finance, the rejection rates are over 90%. Now that's despite the fact that the papers themselves largely self-selected, it's not like we're talking about random scholarship from all over the world. The submission fee at the Journal of Accounting Research and the Journal of Accounting in Economics, I think is currently either 400 or 450 U.S. dollars. It's the same at the Journal of Financial Economics. So, most people aren't even prepared to pay that for a submission fee, if they're not reasonably confident that the paper must have some chance of getting published. And yet you still have rejection rates of over 90%. So what that tells you is this is a crowded field, and you've gotta be prepared for rejection. As an area of work, I always think being an academic, if you can't take rejection, you're in the wrong business. And if you say oh, my papers don't get rejected, they always get accepted. Then I'd probably argue that you're pitching your work far too low. Or you're pitching it at a level that's not really gonna help much in building the institutional reputation. So why is selling your work important? Well, The main reason is you've got to convince me as a reader when I'm reviewing the paper that your work is interesting. Why does it matter? Why is this really important Now, I'm going to sort of pick on an example here. So late last night I was reading a paper that's a workshop paper on my own department at UTS next week. All right?

Now this particular paper is at an early stage, so the author is to be congratulated from wanting to get out and present the thing. And he was keen to come and present it at our place because there's a group of us that I guess are among the better people at doing that kind of work, and I'm reading this paper, and this paper is about, essentially it's about the extent to which companies in Malaysia Who trigger a particular stock exchange disclosure requirement about being in financial distress. The extent to which they undertake actions designed to avoid having to hit that trigger. Now, The first problem I had when I read the paper is why do I care? - [LAUGH] - I mean, there's a lot of research on the mechanisms that they're looking at in the paper. Why do I care about this bunch of Malaysian firms? You know, this might be very interesting to Malaysians, All right? Maybe it might be interesting to the regulator of the Malaysian Stock Exchange, but I couldn't give a hoot. What's the interesting institutional detail? Maybe it is that there's this particular requirement that's kind of unusual relative to what happens on other major stock exchanges, but the authors don't say that at all. I mean if it was me I think that's the twist I'd be trying to put on the paper. But you look at it and you think, why is this likely to be of interest to anyone except someone who's particularly interested in what Malaysian companies do? Yet I'm quite sure the authors have got in mind that there'll be a broader, that they'd submit this to a journal that isn't just a Malaysian research journal. So you've gotta be able to make clear why your work is interesting. Is it that you've got some unique institutional twist? Is it that you've got a new idea, genuinely new idea? Is it that you can show that, or you believe you can show that there's some sort of fundamental issue, with some research? Is it that there's an important result out there that you have some evidence on, that questions our current understanding of that important result? So where am I'm going to find that? It's in the introduction. You need to tell me in the introduction why your paper is important, why I would be interested in reading it very carefully, and so you've got to be really clear there on what the paper really contributes. And let me add that if you're, we're under, in Australian business schools, we are under a lot of pressure to publish in the top level journals. And those top level journals, almost universally, are not Australian-based. All right, so In all probability, the majority of the referees who you might get on a random draw, are not going to be Australian referees. If I submit a paper to, say, The Accounting Review or The Review of Accounting Studies or The Journal of Accounting Research, it's highly unlikely that it's gonna be reviewed by an Australian referee, even if the paper uses Australian data.

All right, so the first thing I need to be very clear on is what this contribution really is. Because the first question a referee's gonna have is why do I care? This might be important to Australians, but why is it important more widely? Because this is a global i.e. in the case of accounting and finance, typically U.S. But this is a global journal. And so how is this issue of broader importance? Unless, of course, it's U.S., and that's typically seen in those journals as global. But there's another issue with trying to play that game that I'll come back to. So, it's very important to convince reviewers at non-Australian journals, if you're doing something that has a local flavour, By reason of the institutional environment that drives your research or which you're evaluating. Is this likely to be of interest more broadly? And if you believe it is, you've gotta do a very good job of selling why it's important more broadly. - Steve, can I just make a comment. - Yeah? Please do. I don't want to talk nonstop for 90 minutes. - I've had editors of journals say something similar in my field. I publish in tourism. One editor of a talk journal said don't put the word Australian in the title of the paper that prejudices people against your paper before you even get to square one. And I thought that was a really interesting set of advice, because you can still make the case by the end of the paper that this has been a really important application or turning the tables on what's always done in the US or whatever. I don't know what you think about that? It's important. - Well- - [LAUGH] - Yeah, I've heard that theory before and there may well be some validity to it. The flip side, of course, is that an overseas editor may see the word, Australian, in the title. And if there is somebody else who's typically good at doing that sort of work and publishing in major international journals, who is in Australia, That may actually mean they're more likely to send it to them as the reviewer. Then again, that person may not like you. - [LAUGH] - Anyway, so yeah, when you get down to that level, there's pluses and minuses that you can think of. And that's why And I'm not trying to be trite. That's why the focus on what you think about here is really fundamentally choose about what you're doing in terms of writing your search. Because if you go to the fundamentals, the rest will kinda look after itself. So Don't tell me what everyone else did to start off with. Have you ever picked up a paper, and the introduction starts off all about what other people have done, and it's like, when do I get to the bit where you make clear what you've done? So, you know, the introduction normally needs, the key thing is to tell the reader what you've done. at least insufficient data that they can contextualise it relative to the research literature. As an editor, currently I'm an associate editor of Accounting Horizons. And what I do in selecting referees is I read the introduction.

The abstract typically isn't enough, but I read the introduction And at that point, if it's not immediately obvious, it's what we would call a desk reject. Then you would pretty much get a sense of where your referees might be by looking at reading the introduction and then going to the reference list at the back of the paper and trying to figure that out. And that's why the introduction's important. Because it's gonna tell editors as well as reviewers quickly what it is you've actually done. So your introduction has to be very clear on what your contribution is. This is where you don't want to be bashful. All right. You need to be very clear on what your contribution is. Okay, because if it's just same old, same old, it's not going to fly at a good quality journal. You gotta be very clear on what contribution your paper is actually making. And as I say, the better the outlet to more important that is to the reviewer. in terms of that you really are making a contribution. I've had papers rejected at major journals, not on the basis that the paper was no good or it wasn't done right. Simply they didn't see it as a big enough contribution Alright, because the really top journals in any field, of course are very competitive with each other about their impact. So what they're looking for is papers that they see making a substantial contribution. Course, in saying that, don't ignore the conclusion of the paper either. Now, don't just retell the whole story at the backend of the paper. Sometimes people write these incredibly long, concluding sections to the paper. Where it's like they just want to repeat everything again. You don't need to do that. But, what you do need to do is essentially just basically pull it together. Focus again on the major contribution, but perhaps also display a little bit of modesty as well. And it's nice, I always think, in a conclusion, if you can not only remind the reader why your paper's important, but try to identify why your paper creates research opportunities. Alright? Because that's going to do two things. First, it's gonna play on the mind of the reviewer in helping them see that your paper is genuinely making a contribution. Secondly, editors love that because if you're right, it's gonna trigger more papers, all of which will cite you. Now, of course, in this environment in which we exist where everyone is obsessed with various measures of how many times you cited and all this stuff. That's a big deal to a lot of individuals as well. But, fundamentally, it's a big deal to the journal editors because if they think your paper is one that's likely to get cited they're much more likely to be enthusiastic about publishing it. So this is especially true where there are a lot more assessment of journal impact. And editors are conscious of trying to maximise journal impact. So they're much more likely to publish a paper if they think that others are going to cite the paper. Let me add, with one exception, not if they think people will cite it because it's wrong. Right? That's not the kind of citation...it actually helps drive impact factor, if you publish a paper and lots of people publish other papers citing it to show it's wrong, but in general, it's not something that, Is what journal editors wanna do. And, that gets me back again to the point, plan your work. Try to plan what you're doing, and again, have a good plan in terms of writing your conclusion. And, how can you help achieve that, and I just want to throw this point in, Get feedback, give your paper to people to read, go and present your paper. So first question I'll ask next Monday of our workshop presenter in accounting is, why did you do this? What's important about it? Okay, and then I'll ask a couple of other questions about the method they've used and how they can be confident they think they've done what they claim they've done.

But basically if you get feedback, it helps identify not just flaws in what you've done in terms of the actual research, and the method, and of course you want to know that before you submit it to a journal. You don't want referees pointing out basic flaws. I go back to my earlier point. The more basic the flaw that gets pointed out, the more likely it is that both the reviewer and the editor, consequently, draw the conclusion that this is not worth pursuing Because even though I don't know who the reader is. Well I do know who the authors are in a lot of cases, because especially these days with things SSRN, you're kinda well aware of papers and that's often a reason why you get it to review. But on the other hand even if I don't know who the authors are, and that does happen sometimes, I review papers and I don't know how the authors are It's not a matter of going and searching online. You can find it 99 times out of 100, if you don't know who they are. But you don't need to do that. If you just look at the paper and you can see there are obvious flaws. Well, it just tells you that there's no point in trying to push the authors. To develop this in a way that it could get into the publication that you're reviewing for because if they kind of were good enough to produce that work, it wouldn't have this obvious flaw in it to begin with. So that's one reason we workshop. But the other reason it's good to workshop is when you get up and present your work, You know, you've actually got to be able to explain what you did and why it's important. I always think of it just in the simple exercise of developing overheads to go with a paper and then go on and present it. When I do that, I often find the overheads I develop. Look quite different to what I've got in the paper, And then I think, that's actually more what I'm trying to say. That's really more, a better flow chat if you like, of what I'm trying to achieve in writing the paper than what I've done. So, workshopping doesn't just help you find the issues you need to deal with in the research itself, it's actually, I think, a very valuable exercise in helping you frame the research and write it up. Because it really forces you to set out the logic of what you've done in a more concise form than when you're actually writing the paper. So I guess if you've had a great writing plan to begin with, your slides should look roughly like your writing plan when you first go out on the road and present your work in a few places. And if it doesn't, well, why doesn't it? And, I think that's always a good thing to keep in mind. Okay, now I'll get into some things that, for those of you who do more empirical research, is probably more relevant But I guess for those of you who are in, for example, areas like law and who primarily do case analysis, nevertheless there are various forms of data that people use in that. And again, I think a lot of the issues would come through.

I find it incredibly frustrating when I can't make out what people really did. And I'm trying conscientiously as a reviewer to make sure I understand exactly what you did so that I can be confident that You've done it the right way. All right? That you haven't done something, that I don't quite understand what you did. But it possibly caused the result you found. So you need to describe what you did and the data you use in sufficient detail. Now a classic that we see in accounting and finance is people say here's my sample and I excluded all the following. Right? Why? Why did you exclude them. Now It's kind of funny when in one area that I do some work in, which is called earnings management. It's common in large samples to exclude what we call financial firms. And that's because the methodology that tends to get used relies on identifying a certain component of earnings that's really very different for financial firms compared to A widget manufacturer. So, you see a paper where they're actually testing some alleged cause of that behaviour, but specific, more specific to financial firms, and the first thing you see in the sample description is, we exclude all financial firms, and it's like...why? Why would you do that? So you need to be very clear on what you're excluding, as well as what you're including, okay? Just saying, we drop all observations at the 1st and 99th percentiles of our data, is that reasonable? You probably did it because somewhere in the back of your mind was, Oh, I should get rid of outliers. But, what if the story you're really trying to examine is actually more about the outliers? Shouldn't you be looking at the ends of your distribution? Isn't, is that where the action really is that you're really concerned with? So, very important to be clear about not just what you included, but what you excluded, and why you excluded it. The next thing is to explain the variables fully. Okay now. A lot of journals that publish empirical research in business disciplines and economics encourage you to, in the tables with your primary results, have very full explanations of what is in that table, what each variables, how it's measured, and so on. And if the journal that you're trying to target has that style, you should try to make sure you do that right from the outset. Don't leave that as an afterthought afterwards. Make sure you've clearly explained what you've done in terms of each variable.

Okay, now, here's an example of this. This is from a paper that I refereed a while ago. For a very good journal. Now, there's a statement in the paper that says, uncertainty is measured as the variation in analysts' earnings forecasts. Now if you're doing research where you want to capture a measure Of the degree to which investors might be uncertain about future outcomes for individual firms. Intuitively, that statement broadly makes sense, right? Because the variation in analysts' forecasts is probably a good proxy for the extent of uncertainty. The extent to which they disagree. We might assume it's a good proxy for the extent to which investors generally might disagree at a point in time. But that's all they said. So that left me wondering over what period are they measuring the uncertainty in analyst's forecasts Is it forecasts just before earnings are released? Or is it forecasts of next year's earnings, a year before that earnings figure was released? Is the measure the variance or the standard deviation? Didn't even tell me that. Are they using what we call a consensus forecast number that available in the database they referred to, which is the average of all the individual forecasts for a given firm at that point in time? Or are they actually looking at the individual analyst forecasts that are available from the same database, and using those individual measures to then create a measure of disagreement. So in other words, is this uncertainty measure a cross-sectional one or a time series one? Now as it turns out, for the particular research question they were looking at, that is a very important issue. Right? I don't know what they did. I just can't work it out. Right? Yet a few simple sentences, at most Addressing what I just described would've told me exactly what they did. So, I'm left wondering whether they've done it in a particular way, that actually creates the primary result they're so excited about in their paper. Now, as it turned out, that's exactly what had happened. Right? That that query kinda killed the paper in its existing form. But it was a query that had to get back to the author, so I wrote a review saying, I don't know whether I buy this or not. Because I can't figure out what they did. They could've done this, or they could've done that. If they did this. This is potentially interesting if they did that, the results contrived, and so, essentially, you know, the editor wrote him a letter saying, here's the referee's report, the referee has a very fundamental concern, if this is what drives your result, you need to think again. If not, you know, we continue on. But you shouldn't have to do that. You need to make sure you carefully explain what you did, and as you can sense from just listening to me describe it, it's really irritating as a reviewer when you sit there and you spend a lot of time trying to figure out what they did. That should be clear. And in fact, there was another point I forgot on that, that I couldn't--the real worry was that I couldn't figure out whether it was cross section or time series, to the extent where they were repeatedly observing the same firm's forecast in the way they were constructing the measure. OK? So, that was the bottom line, I just couldn't figure that out because they weren't giving me enough detail. So if I can't clearly understand what you've done, you've sent me an awful negative signal. And that negative signal is, if I can't figure out what you did in the paper on important methodological points, it kinda makes me wonder if you understand what you did. well enough to be confident that this is going to go further. If I write a three or four page review of the paper, here's all the things you need to think about, or as an editor I write a letter to go with that sort of review, here's what you need to do, we'd like you to have another go at this Then is that a waste of time? Because the higher the level you're pitching your work, that's more and more a question in the minds of reviewers and editors. Does this have legs, as it were? Is that likely to end up in a publication at that level?

Okay, next point is failure to discuss the results. - [INAUDIBLE] - Yeah, yeah. - Seems pretty obvious but in recent editions of the journal. Find where my methodologies [INAUDIBLE] - Yeah. what exactly what's been reported over a period of time to make sure I'm including all those things that editors are [INAUDIBLE] want to be presented. - Yeah. Well, that's just an awareness and understanding of where the literature's at. And so the method you're using, now let me just give you one flip side to that. When I was a PhD student and I was presenting my PhD proposal. I was asked a question, not by an accounting or finance professor, by a management professor, about a statistical test that I was using, what I had partly done and was proposing to continue doing to completion. And I was very frustrated because he'd asked a couple of other questions that I thought were really quite negative. He didn't like accountants or finance people. So I turned to him, and I looked at him, and I said, look. Would it make you happy if I showed you three recent papers in the accounting review. All of which use this test. All right? Now he then took the next 15 minutes to systematically demolish me on the basis that the test was, in fact, wrong. And that drove home the point just because something's published doesn't mean it's right. Okay, and that experience, painful though it may be at the time, has stuck with me in my mind forever since. Just because it's published doesn't mean it's right. And so, one of the things you need to be careful of is that you don't say because a, b, and c do this, I do it as well. If there's room for debate around whether that's the right way or the wrong way to do it, you get a referee who is in the other camp, and you're dead straight away. Okay? Because you can't just rely on the fact that something's published. You've gotta understand. The area of research, don't just rely on saying this is what some, I know it's quite what you were saying. But it's an extrapolation that some people do make. And all I'm trying to do in this presentation is be like the Devil's advocate and point out all the issues that people can sometimes find in your work. So always be careful of that. What I often find frustrating is that a paper will contain some predictive statements, or hypotheses, and then when you get to the results section of the paper There's very little attempt to actually, carefully tie the results back to the hypothesis. Okay? And it's just a part of good writing is discuss the results in consistent, in a way that you set up an interest in the paper initially. Sometimes people have very specific hypotheses that they see themselves as testing. But then they have a whole bunch of results at the back of the paper, and they don't really tie those results to the hypotheses, as such. Well, why have those hypotheses there at the start if you're not actually going to tie the results back to them? Again, just as I talked about the data description earlier, how easy it for the reviewer to find questions in your results that you seemed to have ignored. The more easily that I can spot things in your results that you've ignored. The less confident I am about whether this paper's going to advance towards publication in the outlet that you've chosen to submit it to. Here's one a bit like that earlier example of A says, B says, C says. Again. If you do empirical research, go to the top journals in your area, and see how many papers you can find in those journals where the results section looks like as follows. Table 1 shows, Table 2 shows, Table 3 shows, Table 4 shows. What are table one, two, three, four, what are they intended to tell me? You should write the discussion of the results with reference to the tables. But, writing it in a way that makes clear what the result is intended to show. Okay? Whereas a lot of people just go, table one shows That, you know, the following result. Table 2 shows the following result and so on. There's no real attempt to draw this together in a way that again ties back to the hypotheses. And if you can try to do that, if you can tie your results back and discuss your results, don't discuss tables, discuss results, That's really the difference. Ask yourself, when you're writing a first draft of a paper and you look at your results section, did I discuss the tables or did I discuss the results? Okay? Because you wanna discuss the results. You wanna explain what it is that you see in terms of the results. And in that sense, that's why I say keep in mind what you claim the paper contributes, or the purpose of the paper. Write the results. Write your discussion of the results consistent with what the purpose of the paper or the motivation really is. Don't just write the staccato Description of the test that's reported in each table. That's not a very convincing way to convey the message on what you've done. Now let me just add for many in business disciplines who do empirical research. An interesting question is You might get very excited when you see that something is statistically significant. So let's assume you've used a methodology that the reviewer and others who are experts in the field would think is entirely appropriate for what you've done. No debate about that. And the result is that, using the appropriate statistical test, you can reject the null hypothesis. Because typically what we do is publish papers that reject the null hypothesis. That's an interesting bias of itself. But that's a whole extra issue that is not for today. But imagine that you are able to reject the null hypothesis and you're very excited about that because that's really what you're trying to do. But you probably want to also ask yourself is the result economically significant? You know, people often fail to think about The economic significance of the result that they're reporting. And sometimes, if they did, they'd realise the result is really trivial. But at other times, it might caution researchers to think about what they're doing because the result's the exact opposite. It suggests an order of economic magnitude that's implausible. I'll use two examples. It's very common in what I call earnings management research to find that sitting underneath, Whatever the test is associating a stimulus or a consequence of that behaviour, that the behaviour itself is massive.

So you'll often find in a big pool times series and cross section of firm year results That the average amount of earnings management is 2% of total assets on average. Now if you just go through any of the major databases the major Australian one, or Compustat which is the major US one and you calculate that measure. You calculate income over total assets you find it comes out at about 4%. So in other words, what the paper is starting from is an assumption that half, on average, of reported income is manipulation. That's rubbish. That's rubbish. That can't possibly be true. So that's where economic significance comes in to play. We should ask ourselves, it's not a matter of can I demonstrate something statistically different from an expected coefficient of zero under the null hypothesis. But what's the economic significance? I'll give you another example. There's a branch of research that tries to understand what sort of factors impact the way markets react to earnings releases by firms. And there's one particular branch of literature that asks Does the quality of your auditor impact the extent to which the market reacts to your earnings news? So in other words, for a given unit of earnings surprise that wasn't already anticipated, it that reaction per unit bigger? If the earnings number is somehow more credible because you have a higher quality auditor. There's a whole bunch of studies that document a statistically significant result, but they don't look at economic significance. So in fact, when you translate the coefficients into economic significance, if you take the median then in the distribution, look at it's market capitalisation. And then look at the extra amount of reaction that's being documented in the study. It's maybe 20 times the value of the audit fee. So if the auditor adds so much value, how come the auditor doesn't get paid more? Right? The numbers don't line up. They don't pass a kind of economic smell test.

So one thing that's often very useful is not just to ask yourself or get exclusively carried away with statistical significance of what you're doing. But to look at the economic significance of the work as well. If the nature of your work, particularly if it's empirical, if it lends itself to actually putting dollar values, ask yourself what those dollar values are. And does that look reasonable? Next point was robustness. This applies not just to the kind of work someone like me does but to everything everyone does in the room. So if we're talking, for example, about somebody who does qualitative work, or somebody who essentially does regal research of the type that's most common in Australian Law Schools. Any kind of research needs evidence of robustness and sensitivity analysis. Every kind of research it doesn't matter what the style of research actually is. Have you thought about alternative explanations? Have you thought about alternative perspectives, if you like, if that's a more appropriate term to your research? Because if it's just obvious to the reviewer that you haven't thought carefully about alternative explanations Or alternative perspectives haven't been appropriately covered off in what you've done. That sends a really negative signal about what the future of the paper might be. So what we're really getting at here is that you've gotta really show that you've thought about issues that are likely to impact on the validity of what your preferred conclusion is. Can I offer an alternative explanation for your results? And sometimes that's not about the method you used, sometimes it's about the data you used. Have you got a result Simply because of the way you created the data that you didn't examine, okay. Or have you reached a conclusion because of the way you approach a question would drive you towards that conclusion. So, if for example, and this may sound a little silly. Hypothetical. But, if you were doing case based legal research, and you are prompted by a particular legal question, and you reasoned that the correct answer is x, and in doing that, you selected a series of cases, which are necessarily representative of all cases that have examined the question you're concerned with That's a good example of a data driven outcome. You reached the conclusion you reached because you slanted your examination towards a particular set of cases. Likewise, in archival empirical research, you reached the conclusion you reached because of the data you chose to use. Or what, if you're a theoretical modeller, you reached the conclusion you reached largely because of the assumptions you made in the first place. My favourite theory is you see papers that say, let's assume the following thing is a signal of some outcome. Now I'm going to assume that the world can be fully described by the mean and the variance of what happens. And the end result is the paper pops out a conclusion that you get a fully revealing equilibrium if there's one other signal. Well, if it's a main variance world, you'll always have an equilibrium with two ambiguous signals because one covers off the main and one covers off the variance. [LAUGH] People spend a lot of time deriving these models, but basically, the result's gotta--if the result didn't come out that way, they've made a mistake with the calculus. Right? So the assumption drove the conclusion. So doesn't matter what style of research you do, the same issue really arises. The issue could be with the data you've relied on, and I'm using the term data broadly, as it could be with the method. If you're trying to capture some behaviour in a variable To what extent are there other measures that you could use? Do you find the same result? How robust is your result if you replace the measure you relied on with some competing alternative measure of what you're trying to capture? And that's always particularly a worry when people find they got a different result. So here just be a little bit cautious, you know I was emphasising selling the paper. If you see the selling point of your paper as I got a different result, I mean, you need to be really clear that you didn't get a different result just because you used different data. All right, unless there's very good reason why you used different data. Just saying my result's different, well how does that help resolve the issue? You've really more gotta demonstrate that your way is the right way or your conclusion is the more robust conclusion. And just as an example of that, I've a paper with one of my former PhD students, actually in one of the major Australian journals, Accounting and Finance, where we examined something that is very well known. Issue that's hard to explain in the way stock markets behave. And it's called the accrual anomaly. And this is something that pretty much every major quantitative funds manager uses, to some extent, in their portfolio decisions. And what we actually did in that paper, we took the different ways you can measure accruals. The different ways you can form portfolios, the different ways you can measure returns, and what we basically showed was you can get any result you want depending on what combination of those things you use. Which probably explains why there are some papers that say there isn't a cruel anomaly There are other papers that claim there isn't an accrual anomaly. And the bottom line is whether you find it or you don't find it, depends on the choices you make as you design the experiment. And that's a worry. It's not resolving the issue because. There are arguments about why each of the measures that we twist a dial on is right or wrong. It's up to the reader to decide, but the point of the paper is to show that we can show you a really good result or we can show you a really good non-result. It just depends what choices we make, and There's debate about what the right set of choices actually is. So, last kinda point in the paper. Having encouraged you to be positive in how you sell your research and made the point that this is really critical in terms of persuading reviewers That what you're doing is important and interesting. It's always good to have a little bit of modesty built in there somewhere, as well. So, don't claim what you haven't shown convincingly. If there are still subsequent issues that arise from what you've done. Recognise those issues, point to how that could be a topic for future research. And that checks that box that often an editor is thinking about more than a reviewer. Reviewers tend to be more like my job is turn You know I work out whether this is a good paper or not. Editors, of course, are going to have that extra bit of bias about what the likely citation impact of the paper is going to be. So if you're going to dispute somebody else's conclusion, be careful to recognise how your argument might also be open to dispute. Okay? Recognise that you might think you've back slammed someone else's research. But are you really in a position where somebody could back slam you? So you need to be very careful about going over the top in attacking somebody. Now, as I said, try to identify opportunities for future research. Be quite explicit on that, just a sentence or two might be quite sufficient in the conclusion. But where does this lead? All right, is the purpose of your paper to show that maybe researchers in your field shouldn't be doing something they're currently doing? Or if that's the case, then an obvious extension would be to go and do it the way you say it should be done on issues that have been investigated in the manner that you think is wrong because you're not going to go back and replicate everybody else's work in your paper. So, maybe that's what people should do. Should they take what you've done and apply it elsewhere? Because, what you think is wrongly or wrong conclusion has been applied in a whole area of research. Don't overstate the significance of your work. There I'm being a real devil's advocate. On one hand, I'm telling you, you've gotta sell your work. And on the other hand, I'm saying, don't ever state the significance of your results. So, it's about finding the right balance. Okay, you don't want to leave the reviewer thinking that you're not convinced, yourself about what you did, but as I say, in the conclusion, particularly, there's room for some humility, About what you've actually done. And don't bait potential reviews. All right? It you've written a paper that said X is wrong. And X is a paper published in a very good journal. All right? Then there's a very good chance if you're trying to publish your paper in that same journal, or a similar hard quality journal, that x might be the referee. And I have an anecdotal story about this. Again You learn over time, right? I told you why I never say I did something because somebody else did it, right? It was published in a good journal. I learned that lesson a long time ago. Bit later on in my career, I learned this particular lesson about baiting reviewers. I wrote a paper with a couple of my colleagues And the whole reason for doing the paper was, there was a paper came out in one of the world's really top finance journals, the Journal of Financial Economics, that just had to be wrong.

All right, when you looked at what they did, the data they used could not possibly let you reach the conclusion they claimed to be able to reach. Okay, because the only way you could address the issue is if you actually had data that enabled you to see how many people at any point in time wanted to buy the shares. Right, this was an issue surrounding firms when they first go public. Now what they had was data that was a very poor proxy for the degree to which individual investors wanted to buy shares and how that changed over time. Now, we had access to some data which actually was from the Singapore Stock Exchange, was quite unique at the time. But which let us address exactly that issue, but looking at companies listing on the Singapore Stock Exchange, not the New York Stock Exchange. But, prima facie, there's no reason to expect the explanations should differ between the two, right? So we wrote the paper, and we were very aggressive in the way we wrote the paper. We just said this paper by and Weiss is wrong. Here's the correct result, here's the right data to do this, and you reach the opposite conclusion. And we send it to the Journal of Financial Economics. Now, The submission fee, I don't know what it was then, but today I think the submission fee for the JFE may be over 500 US dollars. And what we got back for our money was about a four line letter from one of the editors, Cliff Smith, who actually was the editor at the JFE who had accepted the paper we were pulling apart, right? He told us that what we'd done was just rubbish and a referees report which wasn't much longer than that which I subsequently found out by accident was in fact by Kathleen Hannelly who was a former colleague of mine when I was at the University of Michigan for a couple of years. Who Actually had gone to the University of Maryland and yet she was the referee. And she didn't take kindly to it either, right, being told that this is pretty much rubbish. I mean, you can't tease out this argument from the data that they had on US firms. Because it didn't let you see the underlying demand schedule for the stock. Now, we thought we were pretty hard done by. Sat there and sort of looked at each other and like, wow, can you believe this? We didn't even get a revise and resubmit, it's just out. Thanks very much but no thanks. And we really don't like this. We haven't even got any reason for the paper being rejected other than the referee and the editor don't like it. And yeah, then you start to put two and two together and you think well Cliff Smith probably didn't kindly to us basically saying you're and idiot Cliff. You made a really bad editorial decision. So, you know, we went away, and we thought some more about it, and I had a conversation, just by chance, not long after with the managing editor of one of the other top finance journals, the JFQA, and, he said, of course, you've got this paper that you think shows that a paper in the JFE is plain wrong. he goes well, why don't you send it to us? You know. Competition between journal editors. So our first lesson was it's very hard to publish your paper in a journal where the motivation of the paper is to show that journal made a bad editorial decision. That was probably a dumb thing to do. But the real point of this example is that over, probably, three or four revisions And working with an editor and a referee, and we know the referee was a guy called Ivo Welch. He's one of the top, top finance researchers in the U.S., in terms of initial public offerings. That paper improved out of sight and when I look back at what the papers says and the way it's written, it is far more useful And far more, has far wider relevance than just basically being like a missile fired straight through the Hanley and Weiss paper. And With the benefit of time and age and experience I can say that we didn't adopt the smartest approach in the way we first approached it. We were really gung ho to being with. It's like how the hell can the top journal in the world publish something this wrong? This unreasonable in terms of how they're trying to make a conclusion from data that doesn't show you what you really want to know in the first place. And to a paper that had a much broader remit, right? So the point I'd make there is don't bait reviewers and editors If you really think somethings wrong in a particular journal, and it's the same management team, and this is particularly if it's a propriety journal where you do tend to get continuity of editors at association journals you get turnover. And most people understand that association journals you have to manage in a slightly more Shall we say unbiased manner? I mean if I send a paper to the Journal of Accounting Research, I recently had one rejected there, which I think it's a really bad decision. [LAUGH] You know, at the end of the day, it's their journal. It belongs to the University of Chicago. They can do what they like with it. They can absolutely do what they like with it. I had a paper rejected at the Journal of Accounting and Economics on the fourth round. Right, which is really hurts. It got into another good journal, but at the end of the day, JAE is a proprietary journal again. The editors can do what they like. If they really do a bad job, sooner or later the journal ranking will drop. but we were experiencing a [INAUDIBLE] so far. He really is a role maker in the longitudinally designed- - Yes. - How do you do that? - I wouldn't choose the journal that published or is most associated with what I think is wrong. Okay? I'd look for an alternative outlet. Okay I would look for an alternative outlet because I think that you're bating the reviewer or you're certainly baiting the editors I should have included editor in that statement. You're baiting the editor, you're just being provocative to begin with people don't like being told their wrong. I mean how many times have you ever read a rejection on a paper and thought, they're right? - [LAUGH] - I've been going at this a while and I still haven't seen a rejection that I thought was correct. - [LAUGH] - But that gets me, of course, to some concluding comments. You know, don't blame the reviewer or the editor.

Now I must admit in the example I was just describing, I did have some uncharitable things to say about Cliff Smith and Kathleen Hanley. But I got over them eventually. Human behaviour is human behaviour. People don't like being told they're wrong. And when you basically do it in a way that just pokes a stick in the editor's eye, [LAUGH] that probably is pushing your luck. Especially when none of us are anywhere near as important as Cliff Smith is in the finance discipline. So don't blame the reviewer and the editor. Try to understand why they said what they did. Okay, keep in mind what I was saying. What's the rejection rate at the journal you're trying to publish in? The really, really top accounting, finance, and marketing journals all have rejection rates of above 90%. I always say to people, you've got to live with the rejections. Have a great time when you get an acceptance. Enjoy it because it doesn't happen very often. And that's particularly the case if you're not part of, in many cases, a very small group of people who will tend to dominate publications in the top journals. So in the top six journals in accounting globally, there are only something like 300 people currently employed with more than one publication in one of those top six journals. It's a pretty staggering statistic. Now that was done about five or six years ago, so maybe there's 400 now or something, but it's surprising how few people there are With multiple publications and that really small number that we would really say are the really top journals in the discipline. Another point I'll make is don't expect the reviewer or editor to re-write the paper for you. That's not their job. Okay, my job as a reviewer is to point out the issues with the paper that exist. And I think it's not unreasonable to expect a reviewer to also offer some insight into how they thing you deal with the problems. There's not point saying I don't like the method you used, it's wrong. That's not fair. Why is it wrong? Why is it inappropriate? You know, I should explain myself and offer some alternative. But don't expect the reviewer or editor to write the paper. That's not their job. You know, and don't blame them for telling you that it wasn't a very good paper. Even if you're convinced it is a good paper, look at what they said, and try and take what they've said and improve what you've done. Don't throw negative reviews and editors' letters in the bin. No matter how cranky you are, no matter how much you might wanna just tear them apart And I had one just recently that is an all-time bell ringer for me.

That I literally, when I printed it off, it was hard not to just rip it because it's the most incompetent review I've ever seen from a quality journal in my life. But at the end of the day In there are a couple of points that it is worth picking up on. Looking at the review and saying hell on Earth, hell on Earth could the reviewers start by saying what we did is the same as x. When right at the start of the paper, the whole Russian half of the paper is that what x did isn't a very powerful test of what they're trying to identify. How could you say that? Well, on the one hand, that's probably a fair criticism of the review. It is a very poor review and I'm staggered that it would come from a quality journal. So has the editor, now that I've written back to him too. He's quite staggered. But the editor didn't handle the paper, one of the associate editors did. But on the other hand, in the associate editor's letter, incompetent though it is, you can see a couple of points that you go and you think well, How could they think that that's what we did? And, then we started to think, well, we kind of have tied the rationale behind what we're doing to that other paper. If you really read what we did superficially, maybe you'd just say, oh, that's what they did. It drove home the point that we need to differentiate ourselves from x's paper much more carefully. Before we re-submit the paper. All right, so even in what to me is, I gotta be quite honest, the most frustrating experience I've had in my whole academic career in terms of what I would expect for the quality of the journal. Even in that case, there's still something we can take away from that and try and do before we try another good quality journal with the paper.

And that gets me to my last point. Don't recycle papers without addressing the comments of the reviewer. So recently I had for the third time in my career an experience where a paper I recommended rejection as a reviewer, at a very good journal, then came to me at another very good journal to review. The reason I got it was because it smack, bang in one of my areas particularly because it comes off of The most, of all my papers, by far the most highly cited one. And they just, it's the same paper, they hadn't done anything. Right, so the last two times this has happened, I have actually contacted the editors concerned and said, I've already reviewed this paper. So why don't I just give you my review from journal X and you can send it as well. And that's what they both did. And in one case, with a very strongly worded letter saying, how many times do you need to see the same comments before it sinks in that there's some legitimacy to this? Just as inevitably must have happened at the other journal you sent it to first, we're rejecting it on the first round as well. Alright, pay attention to what they say, even if you don't like what you're being told, try at some point to calm down, not be worked up like I was about this particular instance. Just let a little bit of time past right? Don't try to deal with it the same day or even in the same week. Maybe just give it a couple of weeks. Let your head clear. Do some other stuff. Then sit down and look at it and say, but how could they have come up with such wacky stuff statements? What can I do to make sure the same thing doesn't happen again? So take some time to think about things before you submit elsewhere. - In your discipline and in your experience, when you receive a referees report and with some of the comments you agree and some of the comments are really his or her opinion. - Yep. - Now - And if you're given the opportunity to revise and resubmit how do you take those comments that you don't agree with the reviewer's comments. It's not a mistake. It's all [INAUDIBLE] - Yeah. Look, ideally you don't want to pick fights with reviewers. And particularly if it's clear from the editor's correspondence that they agree with the reviewer.

[LAUGH] You need to think about whether this is a battle you can win. - [INAUDIBLE] - Yeah. Okay. So I'll give you an example. I recently had a paper that came out in one of the top accounting journals. - And the initial version of that paper used a technique, used a regression analysis technique, whereas some earlier papers used an approach called variance decomposition analysis. Now, I would argue and argue and argue that the way we did it is more intuitive. But almost inevitably you'll get the same result. Now one of the two reviewers was determined we would have to do variance decomposition analysis, the editor was kind of impartial. So what you end up with is a paper that is a long paper, it's longer than it needs to be because it's also got the variance decomposition analysis in there. Now, you know, we thought about arguing with the referee and saying you really don't need this. There's really no reason to do this, and here's the reasons why. But, you know, at the end of the day if that was the make or break issue on whether the paper was going to be accepted, I'll just pull my head in. Run the analysis, the reviewer can see that's the way it should be done. It's in the paper. And, you know, the reader can decide. But if you really think the referee is wrong then you should say that but you don't do it in way that really just pokes a stick in their eye. You know I mean, why, there are ways that you can have an argument without antagonising people. You know, I mean it's like, you know if The worse thing I can ever say to my wife is, you're just like your mother. - [LAUGH] There's no argument that I've ever had that was improved by saying that right. - Nope, that's the one thing you do not do, right? Once or twice in twenty something years of marriage, I know, I 've said that. And I wished I hadn't' after I had, but - [LAUGH] It's the same thing with dealing with reviewers. There are ways of trying to make the point that you think what you're doing is right or you're being asked to do something that's not right. Without antagonising them. Don't antagonise people unnecessarily. Yeah. - I just wanted your advice and comments on how [INAUDIBLE] - Comments, and thoughts on converting a thesis into publication, and what are the issues that might need to be addressed - My view is, perhaps, a bit of a US centric one, don't write the thesis in traditional thesis form, or write it as papers to begin with, and generally - PhD students that I've worked with particularly over the last decade have written their theses as you know two or three papers because I kind of find it a bit unproductive or a bit of a waste of time. People try to write something that they plan on trying to publish as a paper or papers And they try and recast it in a different way solely so you can put it in nice, red cardboard, or blue cardboard or whatever colour it is here, and then you pull it all apart again and put it in as papers. And I don't get that. Why we do we do that to ourselves? Why not just have your Ph.D. as a series of papers. That's the norm at major US business schools, because that's what you're trying to do. Unless you really are writing a book, why make it look like a book. Why not accept that what you're trying to do is show that you can do Good research and the way you normally show it is in most of our disciplines is by publishing in quality journals. Yeah. - In today's presentation research. In terms of theory I think you need to actually be clear that what you are doing is genuinely incremental. All right? I think a lot of theory research falls down. Because it's not really an advance.

And, be careful what I say here. We have one group at UTS in a particular area where whenever I ask how come you don't get publications in the really top journals in your area It's because it's very hard to get publications on theory papers. Well, I could go to the journals in the area, what should I say Because it's not an area I don't know anything about. I can go to the major journals in that area and there's lots of theory papers. The problem is that they're writing theory that's incredibly incremental and that's why it doesn't get published in major journals, right? If you choose to work in an area, you've gotta look at what the issues are with working in that area. So if you're doing what is basically theoretical work, and you're saying, this isn't fair. It's so hard to publish good theory. Well, no it's not. People are always interested in good theory, right? But what does that theory do? How does it advance existing theory, and just as importantly, and here's my bias. How does it point the way for things that impirisis can do? You know, how do we apply the theory and actually test it? Because that's often a key point I think that limits the prospects for theory work. Qualitative work generally, I think the same rules pretty much apply. I've had this discussion with a few very good qualitative researchers. And I think the bare bones of what I was trying to cover really are true, irrespective of the style of research that you do. I know it's easy for me to lapse into examples that are more kind of like Consistent with the archival empirical work, but I can't help that. That's the only stuff I can do well. Well, moderately well, sometimes. So that's naturally where my focus tends to be. I think I'm standing between the room and lunch.

- [LAUGH]

- [APPLAUSE]