

increase the interest in your work. The main investment in time is in figuring out how to download the first data series. Moreover, whether you run a regression for only one country or forty nowadays hardly makes a difference.

I am not saying testing the five different theories may not be good research. Just ask yourself whether you have the knowledge, skills and tools to do it. Why using Dutch data is a bad idea, I will explain later.

1.1.3 Proxies and concepts

Sometimes you can only use 'proxies' for your concepts as there are no data available. Again, let's consider an example.

"Does vacation behaviour of investors affect stock market returns?"

'Stock market returns' is a well-defined concept. The concept 'affect' will need more precision, but the real problem is with 'vacation behaviour of investors'. While it is a clearly defined concept, it will be hard to measure. We would need data on when investors take their holidays and for how long. These data are not available. We can come close if we are willing to make assumptions. We could for instance consider inbound and outbound travel data in different countries or the monthly number of airline passengers or the number of annual leave days in different countries. You get the idea. We may come close with our 'proxies' because all these variables are likely to be correlated with actual investor vacation behaviour. However, they remain 'proxies' as we need additional assumptions (investors all show similar vacation behaviour) to make it work. If you use proxies the less they align (or correlate) with what you want to measure the more of them you will need.

If you can only find one proxy to test your research question and it has low correlation with your concept, you may want to reconsider your research question to avoid ending up in trouble down the line. Suppose you find an article in the psychological literature suggesting that social people tend to use 'we' more than 'I'. You now want to test whether more social leaders make better managers (better defined in a clear way). You analyse conversations of managers and measure their usage of 'we'. You find a correlation between your proxy and management quality. You conclude more social managers are better managers. This would be overclaiming. That statement is too strong because the only thing you can really conclude is that people who say 'we' a lot make better managers. Other personal traits may also lead to higher 'we'-usage (which the psychological study might not have tested). So in this case you would definitely need more 'proxies', or a study that shows that no other traits can lead to higher use of the word 'we'.

Even in top journals researchers often have a tendency to assume results for their 'proxies' hold one on one for the 'concepts', incorrectly assuming that they are measuring the real thing. For instance, we do not observe 'investor expectations' as such, but very often we will use expectations based on some historical data or some sort of model as actual expectations of investors. Or, we use the predictions of analysts as expectations for all analysts, and these analysts are not all investors. These are, at best, proxies for the real thing and assuming that they are the real thing would, for most of these proxies, be silly.

This is an important distinction and confusion occurs frequently so let me give you another example, the so-called ‘equity premium’. This is the return investors expect above the risk free rate (which is proxied by the short term interest rate) to compensate them for risk. We often use long historical stock market returns in excess of the short-term interest rate as an indication, but that does not necessarily equate to the equity premium. Unfortunately, we do not know how high the equity premium is or even whether it exists (and historical premia can vary dramatically depending on whether you go back 30 or 300 years as many pension funds are currently discovering). Top senior researchers might get away with mixing proxies and concepts, but if I were you, I would not bet on it.

In my view, if you use proxies for concepts, carefully spell them out and where possible refer to your proxies and do not extrapolate this to your concepts. Otherwise, people may feel you are overclaiming. What you find for your proxies may not necessarily hold for concepts.⁵ Some proxies are accepted as everyone in your field realizes that it is the best you can do but be aware of their shortcomings throughout your writing.

1.1.4 Make things as simple as possible...

Some academics start out with the misconception that because we often deal with complicated issues, everything we do has to be or at least look complicated. Alternatively, you may be of the opinion that in order to look brilliant you have to make things look complicated. I prefer Einstein’s view: ‘make things as simple as possible but not simpler than that. Brilliance is in making things look simple. This is difficult enough as it is. The biggest compliment I can get is “that I can make things look so simple”. I work very hard at that’.⁶

In my view a good test of your research question is whether you can phrase it in a simple understandable question. Often, if you cannot, you have not thought enough about your research topic.

1.2 Contribution, Contribution, Contribution

The adage of real estate agents is ‘location, location, location’. Similarly, academics talk of ‘contribution, contribution, contribution’ or its closely related cousin ‘motivation’. I will use them interchangeably.⁷

⁵ Many economists in my experience have a similar tendency. They built models based on assumptions that are clearly violated in the real world (and they are happy to admit so) but subsequently they show how their model explains a phenomenon by their model and ignore the possibility that many aspects of the real world they happily assumed away might explain the same thing.

⁶ Consider Steve Jobs’ quote in this respect: “That’s been one of my mantras — focus and simplicity. Simple can be harder than complex: You have to work hard to get your thinking clean to make it simple. But it’s worth it in the end because once you get there, you can move mountains.” BusinessWeek, May 25, 1998.

⁷ More precise we could define them as follows: **Motivation:** why is what you do interesting and to whom? “Who cares?” to put it bluntly. **Contribution:** What do you add to the literature that we do not already know? These are closely related because if what you want to do has already been done in the literature it is hard to answer the ‘why is this interesting’ question

The first thing editors and referees wonder about when they see your work is why your research is important, and what your research adds to the existing literature. The larger the contribution, the more likely you are to get your research published in top journals. If your research is sound and your paper is well written, then the only thing that determines success is your contribution as seen by the editor and the referees (although a bit of luck might play a role as well). Assuming you can research and write, then the contribution of your work and how to get your contribution across is the only thing to worry about.

Therefore I will discuss contribution at length and although I may bore you to death now, you can send me a thank you note later in your career. Let's start with what many researchers find difficult - figuring out your contribution.

1.2.1 What is your contribution?

So how to assess the contribution of your research question? A first test is the blunt: 'Who cares?' Once your research question is answered, how will your results change lives? And whose lives? Practitioners? Regulators? Academics? How is your result going to change behaviour, views or theory? The more detailed and specific you can make this the better. If you can think of ten different ways of motivating your paper, do it. There may be more than one motivation or contribution, so make sure you get them all. But also make sure you do not overclaim. This often happens if you start mixing up concepts and your proxies.

The 'who cares' question is often hard to answer, particularly if you are in the midst of your research. You know it is there, and you can feel your contribution, but it is hard to spell out. Often it helps to try and distance yourself from your research, get away from your computer, walk on the beach or use other means necessary to relax.

Assessing your contribution is especially difficult if you are new to this game but it might always remain an issue. Here are some tricks I have found useful.

Keep going and keep changing perspective: 'Who cares?' if you find the results that you find, 'Why?' and 'What does it mean (it=your results)?' These are questions you cannot ask yourself often enough from as many different perspectives as possible. So keep asking and keep changing perspective, imagine you are reading your results as an investor, as a regulator, as a fellow academic, as a penguin (if you are working on penguin research) or, if you dare, as a referee.

Time will tell: Revisiting your motivation and contribution every couple of months is another good trick. Your perspective changes over time (and in most cases years can do wonders for the views you hold). As an example, in some recent work my co-author and I looked at returns in ALL stock markets in the worlds using ALL historical market indices (including dividends) and ALL historical short-term interest rates available. We were so engulfed in our research question that we missed that as a by product we derived – what we believe is – one of the best historical upper bound estimates of the 'equity premium'

and if you find it hard to justify what you are doing you probably do not make much of a contribution.

available (if you are in finance : it's 3.7% ☺). That byproduct might for many researchers be more interesting than our main question.

Write a newspaper article: Another trick to tease out your motivation/contribution is to 1) take your research question 2) imagine the outcome you would like to see from your tests and 3) imagine you are a journalist writing a newspaper article about your research. What would your results look like, and what would the article look like? If need be it can be very helpful to actually try and write such an article yourself. Again, a change of perspective (here from researcher to journalist) may help to sort out your contribution and motivation.

Link with the existing literature: How does your research question (and/or results) relate to the literature; what is similar and what is different from what others have done? Do you use different concepts, proxies, assumptions, data, techniques and why? This positioning of your paper is extremely important and I will come back to this later, but writing down similarities and differences is a good start to get a grip on your contribution.

1.3 Size does matter

How big does your contribution need to be? When it comes to contribution bigger is better. As noted before, if you can do your job as a researchers, the size of your contribution and luck are the only determining factors as to whether you get a top publication or whether your work is more likely to end up in a lower level journal.⁸ So how to assess the size of your contribution?

First and foremost, keep in mind that the problem with contribution is that it is highly subjective. You will learn this lesson the hard way once you get your first rejection. But while referees and editors can be wrong, keep in mind that once you have a research question, its contribution is easily overestimated, particularly by the person who just came up with the question and especially if you are just starting out in this business. Most research questions tend to look really, really good immediately after the inspiration has just hit you.

Here are some reality checks on the actual size of your contribution:

1.3.1. *The textbook question*

⁸ This does not mean you should only do top level research. Generally it takes ten times more work to get published in an A* journal versus an A journal (unfortunately most universities fail to appreciate this). So even papers with a lesser contribution - if they do not take up too much time - can be beneficial (especially if you are at a university where they do not realize that A*=10A). Moreover, while you should not shy away from top publications at a younger age, they may be less likely. There are only so many spots available in these top journals. A bit of experience does help and top notch research questions are hard to find (more about how and where to find them later). So as long as you do not need to spend much time on them, even papers that do not have a top contribution might be of interest. There is always a trade-off between time and publication level. As a researcher, time tends to be your most valuable asset (and unfortunately too many people and institutions have no problem frivolously wasting it and that trend seems on the rise). Just to be sure, it may be good to repeat that one: as in life, as a researcher time is your most valuable asset. Spend it wisely.

A helpful tool to assess your contribution is to think of the ‘textbook question’. Look at the main textbook in your field. Suppose your research pans out as planned. Where and how in the textbook would it be discussed in say ten years time? Would it be referenced? Footnoted? If the answer is ‘yes’, you are probably talking top research already, particularly if you are thinking of an undergraduate textbook. Will your paper get a sentence or a paragraph? If you think it might be a chapter start thinking Nobel prize. But if you can tell now that none of the above will happen your paper may not be top publication material.

1.3.2 The relation with the foundations in your field

A second tool (which may also be helpful in coming up with research ideas) requires a bit more experience. But you can give it a go. Sit down and think about the major challenges in your field of research and see how your research relates. Let me give some examples in my field.

One of the fundamental concepts in finance is that risk and expected return should be related. Who would be willing to take on risk if there was no reward? Now, the problem is that while in theory this should be the case, empirically we are not so sure. The problem is that we use all sorts of proxies for risk (like volatility) and also for expected returns (like historical returns). In our field we believe so strongly that this relationship should exist that precious few people realise that the empirical evidence we have may be questionable.⁹ Now think about this again in terms of the concepts ‘risk’ and ‘expected returns’ for which we use all sort of proxies like volatility and historical returns. Suppose you can come up with a better proxy for one of these and then show that risk and return are strongly related.... So it may be worthwhile to see whether you can come up with one of the two.

To find your contribution, ask yourself in how far does your research question help our understanding of some of the fundamental hypotheses and relations in our field. Can your proxies overcome some of the main problems with generally used proxies?

Now let’s go back to the research question:

“Are stock market returns higher in January?”

Is this an interesting research question in terms of contribution? Well it used to be. To see why, you need to know another important hypothesis in finance. In the 1970s we thought financial markets were essentially unpredictable based on the so-called Efficient Market Hypothesis (information is efficiently priced in by market participants; only new information moves prices but new information is unpredictable by definition, otherwise it would not be new; therefore price changes are random). The hypothesis was derived from the empirical finding that markets were difficult to predict (yes, empirical results preceded theory). Several studies confirmed this. But if all (publicly available) information was priced in then surely this month’s information should not predict whether returns are higher or lower than they would be in in any other months. So if I find that stock market returns are significantly higher in January, that would suggest some predictability, or in other words, a violation of

⁹ See for instance: this video on “Asset Pricing explained”, by Eric Falkenstein.
<https://www.youtube.com/watch?v=OugUZzUL0WY>

the hypothesis, especially if I show that this is the case in many countries and different time periods.

In a world where people believe that markets are informationally efficient this might be an interesting 'seasonal anomaly'. If you were the first to test it, it would make a contribution. Although it is questionable whether it is still present and even whether it ever really was¹⁰, the January effect is still one of the best known 'seasonal anomalies' to date (Google Scholar reports over 5000 hits).

The January effect made a contribution not because it was interesting per se to know whether January returns were higher but because of the link with the theory. The more you can link results to the main theory and the more explicitly so, the larger your contribution tends to be.

1.3.3 Top papers

What papers are you referring to? If the main papers your work relates to are in top journals, your paper is more likely to have a bigger contribution. Or, maybe I should state this the other way around, if the main article your work relates to is not in a highly ranked journal your work is more likely to make less of a contribution.

1.3.4. The debate

An often neglected contribution to the literature is if your paper adds somehow to a debate. Surprisingly, you often see studies that only focus on discussing the studies in the literature that are in line with the conclusion of the authors. This is a bad idea for three reasons. Science is hardly ever one sided - it suggests that you are biased. The referee will most likely know the literature and be surprised if you only pay attention to studies confirming your results. But most importantly, if there is debate going on (and when isn't there?) adding to a debate can be a huge contribution. Just confirming one side of the debate is probably less so.

1.4. Research Question Mistakes

1.4.1. Don't do research on Dutch data

Probably the worst research questions are the ones that result in papers that have something like the "New Zealand evidence", "the case of the UK" or the "Dutch experience" in the title. Unfortunately, if your research confirms a well-known phenomenon in your country, it is a so-called replication study and unlikely to get you published in a top journal.

Here is the important lesson. Using data from a country just because you happen to live in or originate from that country is a very bad research idea. Let me say that again. Using data from a country just because you originate from or live in that country is a very bad research idea. Use the conventional data from conventional countries in your field unless

¹⁰ Nowadays, there are many more tests a so-called anomaly needs to pass and it is questionable whether the January effect would have done so. For instance, as there was at the time no reason to believe that January should have been different it might have been a case of what we would now call datamining. In any given sample some months will have higher returns than others and if you have 12 months....

your dataset can tell something about a research topic no other conventional dataset can.¹¹

In most finance research, data from the United States are the default data sets. Not because we necessarily like the United States but these are simply the data everyone knows and the three top journals reside there. You could argue that by just looking at the United States we may have all sorts of biases. The US has been a superpower with hardly any military struggle within its borders so de facto we are looking at what might be an outlier. And yes, I would agree with you. You could even argue that the automatic assumption that it is ok to use US data might even make for bad science. Again, I would agree. Unfortunately, most top researchers don't and data from the US are the go-to-data (and generally the best cleaned data around when it comes to other biases).¹²

If you need an alternative, I like to use international data as much as I can (makes the study interesting to as many people as possible). But looking at many different countries is not always easy. Good data may not be always available or may be computationally burdensome if you want to look at individual stocks.

Sometimes some countries have data that allow for studies the conventional data do not. Finland is a good example. In finance there are a lot of studies using Finnish data, not because the Finnish are special when it comes to investing or because we particularly care about Finnish investors or the Finnish evidence but because we can measure things in Finland we cannot measure in the US.

Here is the important lesson: your research question should dictate the data to use and if your research question can be done using conventional data (or a conventional data gathering approach) in your field, use these. If you cannot, then use data that can best answer your research question.

For instance, I have a paper using UK data, but only because we needed a very long data series and the UK data went back to 1692 when the US did not have a stock market. In another paper we use data on New Zealand investors simply because we can measure things about their behaviour we cannot measure elsewhere.

My advice would be that if you now are working on a research idea and are thinking of using something like the 'Malaysian Evidence' or the 'Japanese experience' consider ripping it up. You limit yourself to B or C journals if you are lucky. Using the wrong data has been my biggest time waster. I have had some good papers go down the drain because in my very early days some of them were replication studies and later - even though they did something new - because they used the wrong Dutch data and referees felt unfamiliar with them.

¹¹ Of course, if in your field it is perfectly normal to use data from the country you live in, then it is perfectly fine. However, if you feel a need to add something like the Dutch evidence in the title of your paper, that may be a signal you are doing a replication study and you should worry.

¹² What might become more and more accepted in finance is Chinese data. But if you are a young researcher and even if you are Chinese - or maybe especially if you are Chinese - I would be very careful.

As this is the most common mistake let me elaborate a bit more.

The motivation for a study on the Mauritius stock market I encountered once was 'We are the first study to look at the evidence from Mauritius'. The fact they were the first to study the Mauritius stock market did not come as a surprise to me. Many people would not know Mauritius existed, let alone that it had a stock market. Unfortunately, that does not make the study interesting. The study tested a specific theory on stock markets. This theory originated in the US where millions of stocks are traded every nanosecond. A more interesting motivation might have been how this theory would hold up in a very illiquid market where a stock hardly ever changed hand (as I would expect could happen in Mauritius).

Sometimes students realize they have the wrong dataset and try to justify it ex post by claiming that they look at how a theory holds up in 'a emerging market'. Beware that the problem then becomes why they did choose that specific country as an emerging market. There are many emerging markets around so you will need a better, or more specific, reason. So before you pick a dataset think carefully. If Mauritius has a competitor as the most illiquid stock market in the world, people will wonder why you did not use that one.

The country argument also goes for groups of countries or regions. If your research does not specify that a group of countries should be prone to a phenomenon then there may be no point in studying this group. While studying emerging markets might make sense, studying African countries may be less so. Just to prevent confusion, there is nothing wrong with African countries. It all depends whether African countries are relevant for your research question.

Here are some arguments you frequently hear when you ask people why they chose the wrong data.

But I am the first to use Dutch data

Being the first can be good news or bad news.

But I am a Masters student

There is no law preventing Master students from publishing in top journals. Why would you disregard that option from the start? I have been lucky enough to have top publications with some of my Master students all using international data and this has helped them in their careers in the real world.

But not only do I use different data, I also do different tests....

Researchers sometimes argue that the dataset is fine because they are going to add more hypotheses and tests than there were in the original study. Unfortunately, that only makes the water murkier. The reader will be left wondering whether the results are caused by the difference in datasets or by the change in tests? If you have new tests to add, it is better to use the conventional dataset. If you want to do both, then first show what the impact is of the data and then what the impact is of the new tests, method or hypotheses you want to add. (Start with the reference point, test and dataset the reader is accustomed to, if you can. If on top of that you can replicate more or less existing results in your own paper, that shows you know what you are doing and builds confidence that your own results are correct.)

You can introduce more than one change in a paper (and changing proxies/assumptions can lead to interesting follow up studies). However, just like in a laboratory experiment, if you feel that a change to previous results in the literature can have an impact, change only one thing at the time so you can trace what is causing the difference.

But the Dutch are a great people...

"I know, I know.", to quote Graham Norton. I could not agree more. And the world has an awful lot to thank the Dutch for.¹³ And more research should be done about their greatness and their influence on the world and the world would be a better place if we all were Dutch. But that still does not mean you should use Dutch data.

But I work in a Dutch university - isn't it my duty to work on Dutch data?

I am all for giving back to society but the biggest gift you can give is top notch research. There are two possibilities, which are both good news. 1) The Dutch are not likely to behave any differently with respect to your research topic. This is good news. You can just take international evidence and generalize that to the Dutch case. 2) The Dutch are special with respect to your research question. Basically good news again. You can use Dutch data and they may also add something to our generic understanding. However, I would be very careful before you do this as a young researcher.

Now there is nothing wrong with doing a bit of analysis on a Dutch dataset for local purposes. But see it for what it is: a bit of consultancy. Do not mix it up with your goal of top research.

1.4.2. Pseudo motivation

Many researchers only pay lip service to contribution/motivation. Let us go back to the vacation behaviour. A paper may start with 'Vacations become increasingly important in society. We investigate the impact of vacations of investors on stock market behaviour'. The problem here is that this does not explain why it is important to look at the link between vacations of investors and their impact on stock market behaviour. You motivate one of your concepts in the research question but not the research question itself: "pseudo motivation". You see this very, very often. Many papers start with the assertion that A, B, C or D is becoming more and more important so this warrants the study. That is fine – although not very original - if A, B, C or D directly goes to the heart of your research question, but not if A, B, C or D goes back to one of the concepts in your research question. To drive this point home consider "Are men more optimistic than women" as a research question and the nonsense of the paper starting 'The population of men and women is growing rapidly and they are becoming increasingly important; therefore we study whether men are more optimistic than women'.

1.4.3. Lazy motivating

Next up is another mistake of lazy motivating as in 'studying the impact of vacation behaviour on investment decisions is important for risk management of investor portfolios'. This is obviously true but the problem is that it is very hard to come up with a research question in finance which is not somehow important for "risk management". Finance is to a

¹³ See Russell Shorto's: Amsterdam: A History of the World's Most Liberal City (2013) for an overview.

large extent risk management. So it is a hollow phrase. If you can show how (in detail) vacations affect stock market behaviour and how that translates (in detail) in a way that would affect risk to an investor portfolio, and how investors could manage that risk to prevent this behaviour having a major effect on the portfolio, you are getting the point. The simpler and the more specific the better.

1.4.4. The paradigm shift.....

This is not really a mistake but it may be good to discuss it. Taking my words on the size of the contribution here to the extreme you could think that if you can come up with an earth shaking contribution that would turn current knowledge upside down (and all tests are correct and your paper is well written) you will get published in a top journal.

Beware. Keep in mind that no matter how good your paper is, life in academia is not always fair. Many good papers do not make it into top journals simply because of bad luck. The editor does not like it, or referees do not like it, for no clear reason. Unfortunately it does happen quite often. Research is also sensitive to trends. Some topics are hot and your research may just be on a not so hot topic no matter your contribution. On top of that academics are (almost) human. They have - sometimes incorrect - kneejerk responses. They are risk averse and tend to be sceptical of research that is too confrontational. So ideally your results should conform to some extent to mainstream research but not completely. But if it is completely orthogonal to mainstream be aware that old paradigms die hard.

A good example is the so-called behavioural finance literature (the notion that investors might not be strictly rational, even collectively). No matter how solid and correct the research results might have been in the early days, behavioural finance was considered a paradigm shift and much frowned upon. Nowadays this may seem hard to believe but the early behavioural finance people had to carefully motivate their results from a main stream perspective and carefully align it suggesting that some people might not be rational all the time.

So your research must be different but not too different. The younger you are the less different it should be. But finding the right angle between mainstream and new work can be difficult (I still struggle but maybe because I am too much of a Frank Zappa fan: without deviation from the norm, progress is not possible). This may be frustrating because it suggests that only more mature researchers are allowed to take a 'walk on the wild side' to quote another great musician. But then again, when you think about it a bit longer, maybe that is for good reason.

Having said that, the older I get the more I would argue that one of the reasons we have not made much progress in our understanding of financial decision making in our field is to a large extent based on too much trust in mathematics and a reluctance to accept research that goes off the beaten track. But that is another story.

1.4.5 Doing bad research for the wrong reasons

When you ask researchers about a bad research question some of the arguments that they give are 'at least it is a good learning experience' and 'we wanted to do a paper together'. Life is short and if you must waste it on bad research that is of course your

choice. But why not try to do something that is good research and has a bigger contribution instead.

2.0 How to find good research questions?

2.1 Question everything

I like to question everything - preferably conventional wisdom - and tend to wonder 'Why?' a lot. While it does not always make for easy living (for me or the people around me) it does generate a lot of research ideas. Of course, ninety percent of these ideas do not make it past the drawing board, but the ones that do have made for some nice research.

So my first advice would be to no longer go with the flow and start questioning everything. I am biased but strongly believe that a nice side effect would no doubt be that it leads to a better world if everyone did so. But not everyone might agree.

If you feel that this is a step too far, start questioning the main principles that currently guide your field. When I started out in investments, I simply could not believe that markets were almost completely unpredictable (the current thinking at the time). In those days fellow academics in finance would raise an eyebrow if you wanted to do research to the contrary. I got quite a few papers out of my willingness to question that hypothesis.

2.2. 'All the experts' agree

Keep in mind that in academia fashion plays a role; trends come and go and nothing remains always true. For instance, it is generally accepted at the moment that smoking is bad for your health. That may be true but would it be bad for all people all the time? Or could there be examples where the health benefits outweigh the health costs to an individual - if only for some people? How do we measure health as a society? Life expectancy? And is that a good proxy for happiness? What behaviour do people show after they quit and is that really healthier than smoking?

Or, to put it more bluntly in the words of Joe Jackson:

"It has become 'common knowledge' that smoking is one of the worst things you can possibly do to yourself; 'all the experts agree'. Of course, 'all the experts' once agreed that masturbation caused blindness, that homosexuality was a disease, and that marijuana turned people into homicidal maniacs." (Smoke, Lies and the Nanny State, Joe Jackson, www.joejackson.com)

2.3. Literature

What may work well is to go back to your textbook and see which topics intrigue you. A similar approach would be to have a look at the top journals in your field and see what issues are of interest. There are trends in research. The downside is that this may be dated (and the trend may have passed). Still, if you can find up to date sources of academic working papers (like SSRN (www.ssrn.com) for the social sciences) that may be a good start. A second problem is that there may be no longer any low hanging fruit in that part of the orchard. If many academics are working in an area all the easy pickings are probably gone. A third problem is that unbeknown to you someone may be working on a similar idea. In most cases in my experience the chances of other researchers coming up with exactly the same idea as you are remote and if they do the approach to that research

question is likely to be very different from yours. However, some ideas do have a first mover advantage so you are doomed if you come second. I know that a lot of people use the literature as a starting point but I personally try to look for places in the orchard where no others are searching.

If you do want to pursue an idea already getting traction in the literature make sure you look at working papers or papers presented at conferences and seminars rather than the already published articles.

2.4 Daily life

Daily life, newspapers, Ted talks and practitioner publications may be other interesting sources of inspiration. But with news events you have to watch out. The 2008 Financial crisis stirred a flutter of academic activity in finance. The problem is that events like these also attract the famous and the really smart researchers so it will be very hard to offer a fresh perspective.

Also, while 'case' studies can be interesting, case studies might be perceived by some as a bit 'one observation' research. In my view good research should generalize widely, so the more observations you can rely on the better. With case studies there is always the risk that the results are very specific. I like research with many observations (although we now seem to enter an area where we have so many observations that significant levels seem to become meaningless). But sometimes it can be interesting to do a case study on a specific phenomenon like a company or a crash or a crisis, as long as you keep in mind that you do have only one observation. Always consider if you do a study like this, whether the lessons learned can be generalized.

2.5. Data sets

In many fields new data opportunities manifest themselves on an ongoing basis. Sometimes just having a look at what type of data is on offer, even going back to well known databases, might generate a new research idea. But in the 'deep web' (http://en.wikipedia.org/wiki/Deep_Web) you may be able to find all sorts of interesting data. Sometimes great data maybe freely available if you look for it. Even data on international political crises ☺.

2.6. Get ahead of the curve

Try to predict where your field of research is heading. There is now more and more research that suggests that financial market returns are to some extent predictable. So just finding a new predictor in itself becomes less and less interesting. Instead the focus has moved to the economics: what is driving the predictability? Alternatively, you need predictability with a twist (for instance time varying), gradually moving to the 'how to predict' and problems related to that (statistical biases, model uncertainty) or to papers that focus on what happens to variables that seemed to predict in the past. At the time of writing, papers in these areas are already popping up.

2.7. The big issues

Try to think of the big problems in your research area and try to make a contribution: 'how to measure risk in other ways than using volatility?' 'Can we measure expectations of investors?' are two questions I ask myself at the moment.

Somehow, even though they should know better, some academics seem to ignore the possibility that significant results are spurious but immediately cast their eye on the one significant observation out of the twenty numbers or so on the slide.

4.10 A last word of caution

Very often your results may not only be 'consistent with' the explanation you give but also 'consistent' with many other explanations you may have not explored in your paper. Some researchers seem to find it hard to accept that possibility once they have found significant results. They tend to become strong believers in just that one explanation. Keep an open mind. It is called 're-search' for a reason.

End of Part 1.

This concludes my two cents worth on research. May you have fun on your research journey!

Some more research and writing references

Good references (lots of it from the JFE website):

Tips of Rene Stulz:

<http://jfe.rochester.edu/tips.htm>

Joint Editorial Comments

<http://jfe.rochester.edu/jointed.htm>

<http://jfe.rochester.edu/jointed13.htm>

Paper submission check list

<http://jfe.rochester.edu/checklist.pdf>

Editor of the AER:

<http://vita.mcafee.cc/PDF/EditorExperiences.pdf>

Writing tips from John Cochrane

http://faculty.chicagobooth.edu/john.cochrane/research/Papers/phd_paper_writing.pdf

Pitching Research Robert Faff

http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2462059

From the editors: Publishing in *Academy of Management Journal*. Seven Part series. The first one is *Topic Choice* and appeared in 2011, Vol. 54, No. 3, 432–435.

<http://people.few.eur.nl/kole/WritingAdvice.pdf> by Erik Kole

Deirdre McCloskey: *Economical Writing*, Waveland Press Inc. 2000.

Ben Goldacre: *Bad Science* (visit <http://www.badscience.net>)