Pitching Research®

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Pitching Research®

Abstract
Faff's (2015) "pitching" framework provides a succinct and methodical approach to conveying a new research proposal to an academic expert. The framework was identified in 2016 as one of 30 globally showcased "Innovations that Inspire" [*AACSB* network of Business Schools]. Now in its 15th version, the current paper is the ongoing "living" update of Faff (2015). Notably, I argue that the pitching tool can be used as: (a) a research **planning** tool (e.g. Chang and Wee, 2016; Menzies, Dixon and Rimmer, 2016); (b) a research **skills development** tool (Faff, 2016b); (c) a research **learning** tool (Faff, Ali, et al., 2016; Faff, Wallin, et al., 2016 and Ratiu, 2016); (d) a research **agenda setting** tool (Maxwell, 2017; Nguyen, Faff and Haq, 2017); (e) a research **mentoring** tool (Faff, Godfrey and Teng, 2016); (f) a research **collaboration** tool (Wallin and Spry, 2016); (g) research **engagement & impact** tool (Faff & Kastelle, 2016); (h) **research-led teaching** tool (Faff, Li, Nguyen & Ye, 2016); and (i) research **"discoverability"** tool (Faff, Alqahtani, et al., 2017). Moreover, the current paper points to an extensive array of ever-expanding supplementary online resources, designed to assist the **novice-mentor** research alliance – especially in the highly crucial formative stages of this important professional relationship (see Faff, 2017).

Keywords: new research ideas; pitching; template; research proposal; novice researcher advice; supervisor advice; research mentor advice; PhD coordinator advice; innovations that inspire

JEL classifications: G00; M00; B40; A20; B00; C00; D00; E00; F00; H00; I00; J00; L00; Q00; R00; Z00

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1. Introduction

Following a regularly updated revision process, the current paper now in its 15th version, should be viewed as a "dynamic" companion to the "static" Faff (2015).[1, 2] Faff's (2015) pitching research® framework[3] provides a simple, succinct and methodical tool – a 2-page or 1,000 word pitching template.[4] I argue that the tool provides multi-faceted benefits – its basic framework simultaneously serves as a research **planning** tool (various recent published articles acknowledge Faff's, 2015 template as a critical research planning tool, e.g. Chang and Wee, 2016; Menzies, Dixon and Rimmer, 2016; Dang and Henry, 2016; Mathuva, 2016; Sivathaasan, Ali, Liu and Haung, 2017); a research **skills development** tool (Faff, 2016b); a research learning tool (Faff, Ali, et al., 2016; Faff, Wallin, et al., 2016 and Ratiu, 2016); a research **mentoring** tool (Faff, Godfrey and Teng, 2016; Ratiu, Faff and Ratiu, 2016); a research **collaboration** tool (Wallin and Spry, 2016); a research **engagement & impact** tool (Faff & Kastelle, 2016); research-led **teaching** tool (Faff, Li, Nguyen & Ye, 2016); and a research "**discoverability**" tool (Faff, Alqahtani, et al., 2017).[5]

The broad motivation for "Pitching Research®" is as follows. In my experience the TWO biggest obstacles impeding any research project are, quite simply – starting it and finishing it. Moreover, by definition, the latter is only an issue if you manage to successfully negotiate the former. Hence, Pitching Research® is all about making a sound start. But, to start a research project "with purpose", you need to have a good grasp of where it is you are

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[1] As such, certain parts of the current paper borrow from Faff (2015).

[2] The original version of the "pitching research®" paper was lodged on SSRN on 3 July, 2014. By 16 August, 2017 the first through fourteenth versions of the paper (combined) have logged **10,301** downloads.

[3] The Pitching Research® logo is a registered Trademark in Australia, trade mark number 1694403.

[4] This project and its core tool, has been recently identified as one of 30 "Innovations that Inspire" across the AACSB network worldwide Business Schools. These projects were heralded at the 2016 ICAM conference in Boston. See online: http://bit.ly/29EUbX7
Also, for a 70-second video related to the AACSB accolade, see: http://bit.ly/1T1HggK

[5] The pitching research framework can also be viewed as a tool for dealing with "threshold concepts" in research education (Kiley and Wisker, 2009). A "threshold concept" is: "something distinct within what would typically be described as 'core concepts'; that is, more than a building block. A threshold concept is one that, once grasped, leads to a qualitatively different view of the subject matter and/or learning experience and of oneself as a learner." (Kiley and Wisker, 2009, p. 432). These authors further argue that threshold concepts are transformative, often "liminal", irreversible, integrative, bounded and likely represent "troublesome" knowledge. "Liminality" refers to a protracted period that precedes actual crossing of the threshold – a period in which "… students may mimic the language and behaviours that they perceive are required of them, prior to full understanding. It is while in this state that doctoral students are often likely to feel 'stuck', depressed, unable to continue, challenged and confused." [Kiley and Wisker (2009, p. 432)].

heading! So, how can you know with any confidence that you have identified a good/worthwhile research topic? More to the point, how can you figure this out very early in the planning process so that you avoid unduly wasting precious time and resources on something that (sadly) might ultimately be a "flimsy" addition to the relevant literature?

Accordingly, the core objective of the current paper is to give tangible advice in this regard.[6] My primary target audience is novice researchers engaged in empirical work – whether they are current doctoral students or (post-PhD) junior academics, with only limited publication experience in the very early phases of an academic career. My secondary, but equally important target audience comprises PhD supervisors, research mentors and senior research collaborators, since they should seek out all legitimate means to help fulfil their important leading role in any such research relationship.

To this end, I propose some key guidelines to creating a sound research proposal. Specifically, using Faff's (2015) pitching template, you (the "pitching" researcher) are challenged to concisely "populate" each section of the template with relevant material. Emphasizing the notion that "less is more", the task is to confine your efforts to just 2 pages (or 1,000 words).[7] How would you go about meeting this daunting challenge? What areas/aspects should you cover? In what detail? How can you best package this information for efficient consumption and assessment?

The basic logic is to provide essential, brief information across a broad range of essential dimensions that any collaborator would need, to make a reliable assessment of the

[6] In feedback on a previous version of this paper, it was quite reasonably suggested to me that while the template is helpful, even it can allow/encourage a considerable investment of "wasted" time if the core idea is "dumb". To an extent I agree, though I would argue that implicit in prior versions of this paper was the existence of some preliminary informal discussions between pitcher/pitchee regarding "deal breaker" issues on any given pitch. I now acknowledge and discus these concerns explicitly in Sub-section 2.2.4. Also, I refer readers to existing papers like Stokes (2013), who provides good advice/strategies on how to generate innovative research ideas. Stokes (2013) is freely available at: http://bit.ly/2jqpTKY

[7] In private conversations, Devraj Basu canvassed the view that in some circumstances, and particularly in the first instance when we are trying to capture initial attention, the time constraint might be much more severe than the "luxury" implied by my suggested 30 minutes. The most extreme version involves the so-called "elevator pitch" i.e. the pitcher has to elucidate the "value proposition" in about the time it takes for an elevator ride (30 seconds). This is more akin to the initial "selling" or "thinking" device, which might simply capture the key idea/motivation underlying the proposed research. Alternatively, this hyper-short pitch might be thought of as identifying the "irreducible contribution".

quality of and potential for the proposal. Notably, it is assumed that the goal of this exercise is to produce a solid plan which, once executed, would eventually lead to a quality research paper – published as a fully refereed article in a highly reputable international academic journal.

There are numerous extant articles/books that give researchers general advice and valuable insights on how to get their research published and so such a perspective will not be repeated in any detail here. A critical distinction exists between the objective/context of such "advice" papers versus the current paper. Most notably, they assume that researchers already have a well-developed product (i.e. that they have a paper that is considerably beyond the first-draft stage), and the advice they then give is how to enhance and improve from this relatively advanced base. In contrast, in my paper, I am speaking to researchers who have embryonic notions which are yet to be formally explored, and for which the researcher is genuinely unsure of the underlying academic merit.

The remainder of the current paper evolves as follows. In Section 2, I outline Faff's (2015) pitching template and briefly guide the reader as to the underlying thinking behind each piece and how it might be completed. Section 3 provides some advice directed at the two main pitch stakeholders: the "pitcher" and the "pitchee", for completing/using the template. Section 4 briefly points to a range of online supplementary material and an update on the ever-growing extensive support and initiatives.[8] The final section concludes.

[8] From version 15 onwards of the current paper, a conscious decision was made to cut down the "clutter" in this paper and make it more "consumable" – in V14, it had grown to an almost 60-page "ugly monster". The simple strategy for achieving this reduction objective has largely been by creating a companion paper: Faff (2017), a "one-stop shop" – a "Resource Central". That is, Faff (2017) now gives a comprehensive update and latest information (including numerous actionable hyperlinks) on ALL resources currently available to support the pitching research® framework (see Section 4 below). **Readers are strongly encouraged to access Faff (2017) and exploit the benefits from this extensive resource pool.**

2. The Pitch Template

Faff's (2015) pitching template is shown in Figure 1 in blank format, Figure 2 presents a completed example of the pitch template for the pitching research® proposal itself, while Figure 3 repeats the template but now provides a series of prompting questions, as cues to induce the "pitcher" to think about a range of possible considerations under each heading.[9] I begin by discussing the components of the template and the basic philosophy/purpose behind each element. I also give some general guidance on how to populate each segment of the template. For ease of reference, the key elements of the pitch template are labeled "Item (A) - (K)".

The first thing to understand about the design of the template is a need to be concise and to the point. It is very safe to assume that the "pitchee" (e.g. potential research collaborator, Honours/PhD supervisor, research mentor) is a very busy person. He/she is time poor and in the first instance simply wants to know the essential ideas, without being bogged down by the details. With this in mind, my strong advice is to keep the completed pitch to a maximum of 2 pages. For a knowledgeable "pitchee", this limit will provide ample material to induce probing questions, leading to an informed judgment – and more detail can be called for once the pitch is deemed "successful"![10]

Indeed, the pitch can evolve. The very first version will very likely be rough and raw – and possibly incomplete. This is expected. There is no shame in this. Rather, the shame will be if the "pitcher" is always too "scared" to share their pitch with their potential "pitchee" because they fear embarrassment. Air your ideas early, so that they might flourish or die – whichever is appropriate. Lost time is a lost opportunity. Should your early ideas flourish, the

[9] A softcopy WORD file of the pitcher's cued version of the template is available from the authors webpage: http://www.business.uq.edu.au/staff/details/robert-faff (please scroll down the webpage until you find the download prompt). A web portal version is available at: PitchMyResearch.com

[10] There is no unique definition of "success" in this context. At one extreme, for a very early version of the pitch, success could simply mean that the senior collaborator wants to see a revised pitch that addresses some key areas in more detail. For an already heavily revised pitch, success would be indicated by the senior researcher agreeing to collaborate on the project, with an agreed division of duties on, for example, generating a detailed literature review and hypothesis development versus initial data collection and sampling – perhaps even staged via a "pilot" exercise.

pitch template can form a useful framework for development across several iterations, until that moment of metamorphosis is reached – when it is no longer a "pitch" – it becomes a fledgling project!

The template begins with stating the pitcher's identity – "ownership" is important.[11] Also a "field of research" (FoR) category clarifies the relevant "domain" and a date of completion of the pitch is provided – so that a clear time context can be given – especially useful in situations when the pitch is (re-)viewed/assessed with any significant delay.

The template is built around a "4-3-2-1" design – like a "countdown" – a useful "gimmick", if only because it is easily memorable (while only a little contrived).[12] At the top of the template, the "4" part relates to four broad, essential ingredients of which the reader wants immediate knowledge: (A) working title; (B) the basic research question; (C) the key paper(s) and (D) motivation/puzzle. Together these four items can be viewed largely as an exercise of "framing".

2.1 Preliminaries – Framing your Research

2.1.1 Template Item (A): Working Title

The "first" challenge is to decide on a working title. While stated as the "first" challenge, in most cases the "working title" evolves over time. As such, the title can be refined several times during the process of completing the template and it becomes more clearly shaped as more information is gathered and cognitively processed. Indeed, you do not necessarily have to begin at the top of the template and work systematically down. The task is best thought of as a dynamic and iterative process, in which the "path" to a completed pitch is non-linear and

[11] When it comes to "intellectual property" linked to research, a definitive statement of ownership is often problematic. Similar research ideas can be developed independently by different researchers – and it is quite possible that multiple "leaders" will be acknowledged in the literature. One way to stake an early claim to an idea is to make "public" your work in various forms as soon as possible e.g. by creating a working paper on SSRN; by delivering a research workshop at a university seminar program; or by presenting a paper at a recognised conference. Of course, if the idea is meritorious and potentially developed contemporaneously by several researchers, those who are too slow developing it to a mature state, risk being relegated as secondary players on the given issue.

[12] Simple arithmetic shows that 4-3-2-1 sums to 10, yet there are 11 substantive elements in the template design. As such, the template design is really "4-3-2-1" "+1".

unpredictable.[13] The ultimate title (of the paper that hopefully comes as a successful output from this pitching process) should be an appropriate balance between being informative, catchy and concise.[14]

2.1.2 Template Item (B): Basic Research Question

The next challenge[15] is to capture in one sentence, the key features of the chosen research question. It is often said that you should have passion about your research – here I say, be passionate about the question, but as a good scientist, be open-minded about the answer! It is very likely that the research question will be very similar to the working title (Item (A)) – but in most cases it will be more than subtly different, and slightly more expansive. While the question can take almost any form, it is typically "neutral" in its expression. Indeed it might not even be a question, in the literal sense. For example, it might be something like: *What are the (e.g. economic) determinants of "variable Y"*? or *To explore the empirical determinants of "variable Y"*. While such a research question does not identify any prediction(s) or hypothesis(es), it is readily connectable to the expression of such. Following on from the above example, the related hypothesis might be expressed as: *"Variable X" is a positive determinant of "variable Y"* (the opportunity to state a prediction/hypothesis comes later in the template under the Idea). In many contexts, such a statement will clarify the identity of the key dependent ("explained") variable and the key test/independent ("explanatory") variable(s).

[13] Indeed, we have captured some accumulated pitch completion data from our web portal (PitchMyResearch.com), that confirms this to some extent – though there is a significant degree of linearity too. See Figure 4, which characterises the 11 elements of the pitch template as a pitch completion "clock". Linear behaviour in using the web portal, is reflected by the thick black lines joining the items (presumably, travelling clockwise) around the outer edge of the clock. However, the prevalence of many "cross lines" in the figure, reflect a nontrivial incidence of iterative behaviour. The size of the dark outer edge circles denote average time spent on template items – the idea, motivation and data, seemingly occupy the three most time consuming elements.

[14] Other things equal, having a short title can attract initial attention. For example, while Benson and Faff (2013), titled "β", and Faff (2014) "α" hold the unofficial world record for the shortest title possible and thus has some "curiosity" value, ultimately, papers like these with similarly curt titles, can only sustain attention based on their real academic content.

[15] Similarly, the "research question" evolves over time. The initial view is often rudimentary and overly simplistic, and it too becomes more clearly shaped as more of the plan comes together.

2.1.3 Template Item (C): The Key Papers

A sufficiently deep immersion within the relevant literature is essential to coming up with and confirming a good research topic. I use a light-hearted metaphor to explain how to attack the literature challenge – what I term the "cocktail glass" approach. Imagine a fancy cocktail glass that is very broad at the top, narrows down to a small diameter – say, a third the way from the bottom and then fans out at the base – but much less so than the top. Such a glass is depicted in Figure 5. Symbolically, drinking from the full cocktail glass is like beginning the literature search on a broad topic – there is typically a big literature to traverse, characterized by the big diameter at the top of the glass. As you spend time reading, filtering of the papers takes place, coincident with the refinement of the potential topic – quite likely an iterative process. Like the slow consumption of the cocktail (savoring the taste), the drink level descends toward the narrow part of glass – analogous to the narrowing in ones thinking about which papers within the relevant literature are the most important and critical foundation stones for your research topic. When you get to the narrow part of the glass, you have identified the small set of papers that really help you focus your attention on what is currently "known" and what is yet unknown. These are the "key" papers – the **three** key papers. Later, should the project advance, an expanded set of the most relevant papers is identified as your reference list – like the cocktail glass, these represent the foundation upon which the paper (glass) rests.

I suggest that in answering the question posed in item (C) of the template – namely, what are the "key" foundational papers for your proposal, as foreshadowed above, limit your answer here to just **three** papers! You might ask: what "characteristics" should these critical paper(s) possess? Absent any specific considerations to the contrary, I suggest three rules of thumb. First, the key papers should be quite recent – say, no older than 3 years.[16] Ideally, they

[16] An obvious (seeming) concern with this "currency" advice, is that it excludes choosing a seminal paper. The counterargument is that we can take the seminal paper as "given" and, moreover, that well-chosen recent papers will explicitly and critically build on such seminal work. Nevertheless, a simple adaptation of the advice on key papers is e.g. to allow the seminal paper plus three others.

should be published in the Top Tier journals in the relevant field, or if they are not, then they should be very recent unpublished papers available on SSRN and preferably authored by "gurus" in the relevant field. Collectively, all these conditions serve as heuristics for "currency" and quality.[17] Ideally, we should also see some diversity in terms of the "guru" authors and journals e.g. we should avoid the extreme case of choosing three papers written by the same author, published in the same journal.

2.1.4 Template Item (D): The Motivation

The final "preliminary" consideration in the pitch template is the motivation (at item (D)). All high quality papers come with impressive motivation(s). One way to view this challenge – taking a "big picture" perspective – is to identify what broad piece is currently missing from the accumulated knowledge base in a field. Moreover, the identified missing "piece" should be one that we can argue scholars (notionally) have a strong "demand" for an answer. While this motivation should emanate from the academic literature itself, in the social sciences it is often also linked to (unexpected) observed (e.g. agent) behavior or actual (e.g. industry) patterns or real market imperatives or current regulation/policy debates. Indeed, one really good strategy for motivating a paper is isolating a meaningful and relevant "puzzle" – which, for example, might be observed in recent (e.g. market) trends that show curious patterns or actual decision-making that defies conventional wisdom.[18]

The core of the template tool is built around the next six elements, which constitute the "3-2-1" of the "countdown" identified above. "Three" represents the three essential ingredients of the Idea, the Data and the Tools. "Two" represents the two basic questions that a successful researcher always convincingly answers: "What's new?" and "So what?"[19] And,

[17] Of course, any other objective means of telling that an unpublished paper will soon be an influential one in the Top Tier journals can be used – but the rules of thumb stated in the main text seem reasonably "safe" suggestions.

[18] It is worth noting that many research papers do not identify a "puzzle" in the sense that I have in mind here – namely, there is really a puzzling phenomenon that is observed in "real world" settings, which is not readily explained by the conventional theory/models in a given relevant discipline.

[19] We should always remind ourselves of "cultural sensitivities" – and this is one such case. I have on good authority that from a Chinese perspective, the question "so what?" can be seen as quite offensive. Of course, while I want to challenge the "pitcher", I do not wish to cause offence! An alternative way of expressing the

finally, "One" represents the "holy grail" – the Contribution! Ultimately the merits of any paper must stand on both its actual and perceived contribution to the literature. Each element of the "3-2-1" design is discussed in the following sections.[20]

2.2 Three Dimensions – Idea, Data and Tools

Any empirical paper has three critical dimensions: (1) the Idea; (2) the Data; and (3) the Tools. Faff (2013) proposes a "cheeky" acronym based on the first letters of Idea, Data and Tools – the so-called "IDioTs" guide to empirical research.[21] These are the "building blocks" of the research plan. While the three elements are, for expositional convenience, presented here as being independent considerations, in practice they are often interrelated.

2.2.1 Template Item (E): The Idea

Absent a good idea, irrespective of how impressive everything else is, it is hard to imagine how a worthwhile paper can be created. As stated in Figure 3, against item (E) the main cue asks you to identify the core idea – the essential concept/notion/proposition that drives the intellectual content of your chosen research topic. Moreover, the template prompts for a brief articulation of the central hypothesis and also asks is there any theoretical tension involved? "Theoretical tension" reflects the situation in which there are meaningful contrasting predictions from two (or more) pockets of theory relevant to the research question.

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question is to ask: "who cares?" I thank Yong Li for bringing this issue to my attention and for suggesting the alternative form of the question.

[20] These sections are strongly inspired by and very closely aligned to Section 2 of Faff (2013). Interestingly, in Faff (2013), the purpose at hand – namely, to assess a well-developed paper – is naturally compatible with the reverse order of attack – "1-2-3". Ultimately, this reversal is innocuous – the essential elements and message remain robust.

[21] In anonymous feedback received on an earlier version of the current paper, the reasonable point was made that these three labels ("idea", "data", "tools") don't work across all areas of research. For example, in psychology a more accepted labelling might be "hypothesis/research question", "sample" and "statistical analysis" (HSS). As another example, you might substitute "design" for "data". In such a case, (research) design would in part capture "data", but in a broader setting (e.g. qualitative research) allow the thinking to usefully extend beyond this narrower focus. Whatever the case, in my mind, these variations are more about semantics than content. My attraction to "IDioT" is the broad scope that each element conveys, as well as the ease with which we can (collectively) remember them via the light-hearted acronym. It should further be acknowledged that the alternatively suggested labels are also widely used in finance research and elsewhere, though different disciplines might exhibit varying degrees of relaxation with which they are interpreted/applied. Finally, the other positive thing to note here is – whatever the concepts are labelled, it seems that a similarly motivated "triad" is all purposeful. Rather than dismissing on these grounds, a simple mapping from "IDioT" to "HSS" is encouraged!

While a critical aspect of a good research idea might very likely come from theory, the motivating idea might not necessarily be exclusively theoretical. As argued by Faff (2013, p. 952), "… the idea might involve an innovative blending of existing theory, or it might actually relate to a clever way of exploiting institutional differences or recognising unique exogenous events that allow reliable identification of causality. The idea might relate to the identification of a "gap", for which we can't reliably deduce the answer from the existing literature."

2.2.2 Template Item (F): Data

A research paper cannot claim to be truly empirical without data – data can be either quantitative or qualitative. Item (F) in the template aims to expose key questions around the data and sampling, with a key focus on establishing feasibility of the project – both in terms of an adequate sample size ("quantity") and veracity of the data source/compilation ("quality"). By challenging the "2 Qs", the current focus is centred on giving confidence that reliable inferences regarding the question at hand are ultimately deliverable. Item (F) of the template poses a (non-exhaustive) series of data-related questions. Question 1 largely prompts consideration of the chosen unit of analysis – either or both in a longitudinal/time series and a cross-sectional sense. Question 2 can in part be viewed as making us think about statistical validity, since sample size is a key factor.[22] Question 3, probes more on any likely (non-random) structure in the data – e.g. if the data have so-called "panel" properties, the effective degree of independent observations is diminished from the "headline" pooled sample size. Question 4 is strongly asking us to confront feasibility – sources of data whether commercial or hand collected or created by survey methods, pose potentially "deal breaking" issues in terms of prohibitive costs (either monetary or time). Questions 5 and 6 both connect to the veracity issue – missing data, or ambiguous data or "unclean" data. All data are an

[22] Clarkson (2012) argues that four dimensions of validity constitute the "cornerstone of scientific rigor": (a) internal validity – do we have a fully-specified model?; (b) construct validity – do we have compelling linkage between empirical proxies and economic variables?; (c) statistical validity – do we have appropriate data, sampling and tests?; and (d) external validity – will our results be generalizable?

unknown weighting of signal/information versus "noise", and concerns reflected in these questions can push the perceived noise/signal ratio beyond levels too high for comfort. As the old saying goes: "garbage in garbage out". Question (7) in this template item, asks us to contemplate any "other data obstacles?" While this could relate to anything of relevance, it helps prompt thoughts of other validity issues – e.g. external validity: does the sample of data provide a representative and meaningful view of the underlying (and relevant) population?, or construct validity: are the feasible proxies compelling constructs for the underlying theoretical variables in question?

2.2.3 Template Item (G): Tools

Item (G) reminds us that without adequate tools/techniques, data and ideas are useless. A critical part of academic rigour is having systematic and formally designed statistical analysis that gives reliability/credibility to any/all inferences drawn. An empirical study that is purely descriptive or one that is based on univariate tests, will find little favour in the mainstream literature. In essence, the "toolkit" comprises the techniques, econometric models, software and so on, that collectively allows us to objectively "ask" the data for answers to the key research question and its related predictions/hypotheses. For example, Item (G) asks the very basic question of whether a regression approach will be used. Or will it require survey-based tools (e.g. survey/questionnaire instrument design) or involve interviewing design/techniques? Further, questioning which software (e.g. econometric, text analytics, qualitative) are fit for purpose, prompts the related questions of software availability and training. There is also a question of "connectivity" between tools and all other aspects of the proposed framework – indeed, an overall consideration is that a common thread runs right through the pitch.[23]

As emphasised by Faff (2013, p. 953) novel tools "… can provide added "leverage" to a research question, that helps create new insights not possible with standard techniques that

[23] I enjoyed discussions with Marc De Ceuster along these lines in which he always asks his students "… what is the *story*?"

are well-worn in a given literature. One example of such potential leverage is when a researcher transports an established technique from another discipline, and shows how it can give new insights, that for whatever reason are obscured by the existing "old" approaches."

2.2.4 The "Deal-breakers"

As foreshadowed in the Introduction, a critique of my advocated approach is that it, too, can allow/encourage a considerable investment of "wasted" time and effort. To minimise this concern, I assume (indeed, advocate that) there (should) be an "sufficent" level of preliminary discussion between pitcher/pitchee regarding the possible existence of any "deal breaker" issues relating to any potential research question before it is fully "embraced" by the template. In the current context, this can most simply be linked back to the "IDioT" principle.

Regarding the Idea, informal "due diligence" should be directed (based on "within reason" efforts) to ruling out: (a) "replication" risk – that you will avoid effectively replicating an existing study; (b) that the answer is already known (directly or indirectly); or (c) that it is a "dumb" idea. Regarding Data, the most common and obvious "deal breaker" to be ruled out is that you do not have access to (or, simply, there do not exist) sufficient quantity/quality data for a reliable and representative sample, relevant to the question. Regarding Tools, it is a question of knowing that the necessary tools are available to do the job. Very likely, it is the Idea and/or the Data which will give most anxiety at this "deal breaker" stage, as modern tools are in abundance. While it is true that all of these "deal-breaker" aspects are relevant to the full pitch template exercise itself, in that context we are interested very much more in specific details for developing the pitch.

2.3 Two Questions – What's New? and So What?

Yes, any "IDioT" can tell you that empirical papers are characterised by three critical dimensions: Idea; Data; and Tools. But, you can use these dimensions either well or poorly – how can you plan to achieve the former and avoid the latter? I suggest the answer lies in two questions! First, ask yourself, what is new? Second, ask so what?

2.3.1 Template Item (H): What is New?

Faff (2013, p. 951-2) argues that a meaningful contribution should tell us something new, "… something that we did not already know based on an informed reading of the extant literature. If there is no novelty in the empirical work – for example, a straight replication of an existing paper, then it seems straightforward to conclude that there is no contribution." Moreover, Faff (2013) highlights that novice researchers often fall for the "trap" of taking a very literal interpretation of the word "new".

Consider a hypothetical illustration, in which a series of single country studies are historically common across a given literature. Viewing this situation, novice researchers can naively fall for the trap of excitedly targeting the "missing" country as a new study. That is, while the relevant literature already documents clear and consistent evidence for country "X", country "Y" and country "Z", a perceived "gap" is identified because nothing has been published in the author's chosen setting of country "A". Yes, in the narrow (literal) sense, generating a test for country A is "new". However, the novelty is likely to be deemed trivial – the fallacy here is that an informed reader of this literature (with minimal effort) might be able to take a synthesised view of the collective extant research and reasonably infer what will be applicable to country "A" (and, indeed, to a range of other similar countries). Thus, to establish meaningful novelty in such a single country study, the researcher needs to make a compelling case as to why it is "dangerous" to extrapolate the distilled evidence from X, Y and Z to country A (or to other similar jurisdictions).

Faff (2013, pp. 954-5) emphasises a simple device to help assess research novelty – the so-called "Mickey Mouse" diagram (i.e. Venn diagram). The idea is that based on a characterisation of the relevant literature, you define (e.g. three) circles of research attention that meaningfully overlap,[24] in ways that have not been completely explored in the extant literature. Figure 6 depicts a generic version of Mickey Mouse, in which two circles are at the

[24] There is no fixed requirement for what these circles might represent – they might be any combination of idea(s); data; tools; or relate to market features, regulation, … anything that makes sense. There is no right or wrong answer here – it is simply a matter of whatever works.

top (i.e. "considerations" A and B) representing Mickey's ears and one circle is at the bottom (i.e. "consideration" C) representing his head. Typically, for projects in which such a characterisation makes sense, the area of novelty is defined by the triple intersection zone i.e. "X marks the spot".[25]

2.3.2 Template Item (I): So What?

Simply being new or novel is not enough! Many "new" things have no special consequence – they are unimportant. Accordingly, Item (I) in the pitch template poses the question, "so what"? Yes, so let's assume that you have posed a novel research question. But, then the critical follow-up question is – why is it important to know the answer? Is it likely to have "first order" or only "second order" effects? How will major decisions/behaviour/activity and or other relevant phenomenon, be influenced by the outcome of this research? If it is not sufficiently important, then no one will care. To express this concern differently – we should never embark on a research project that is effectively targeting a journal of "irrelevant results".

Building on the previous discussion, one potentially fruitful way of successfully invoking a "novelty" dimension into a single country study is to identify some unusual (e.g. financial) market behaviour or unusual relevant phenomenon or unique institutional feature or regulatory event(s) that would meaningfully distinguish the chosen new country setting from prior research. But, simply being different to e.g. the US (being the world's dominant market) does not guarantee a fertile ground for new research. The critical reader (e.g. dissertation examiner or journal referee) will need to be convinced of the importance and relevance of any identified unique features to advancing knowledge in the discipline area. In other words, they will ask the "so what" question.

[25] In a sense, the Venn diagram device helps stimulate our thinking toward "innovation" rather than "invention". While few would argue that "inventing" something very new and path-breaking is not highly valued, for most researchers major "inventions" are purely aspirational and beyond reach. In contrast, innovating with guidance from our friend Mickey is very broadly attainable – and can still deliver fantastic and highly influential outcomes. In the context of doctoral study, the sentiment I convey here is consistent with Mullins and Kiley (2002) – it's a PhD, not a Nobel prize!

2.4 Template Item (J): One Contribution

The "holy grail" for any research topic is to make a contribution – this is the **NUMBER ONE** goal. Thus, while few researchers would have trouble agreeing with this statement, no matter how experienced we become at doing research, the challenge of establishing contribution seemingly never becomes any easier. One reason for this is that as we become more experienced, we become more ambitious with our targeted journal – the higher the quality of the journal, the higher is the threshold standard for the required incremental contribution.

Thus, completing the penultimate section of the pitch template is bound to leave us all feeling unsatisfied or even a little disillusioned – but these are not good reasons to leave this item blank or for it to create a "road block". One comforting thought is that good responses to all of the previous parts of the pitch template, help to define the contribution. In other words, by the time you end up at Item (J), you have thought seriously about all the constituent parts needed for contribution. Now you are faced with the challenge of distilling this into a short statement about the primary force. Often times, it will be inextricably linked to the Idea. But, the Data and the Tools will also play their part. Where is the essence of the novelty? Again, is the Idea new? Is there any novelty in the Data? Is it in the Tools? But, beyond novelty in any of these dimensions, what is the importance? Why should we care? This latter consideration can often invoke thinking around likely economic significance of possible findings. The prospect of finding statistical significance, absent economic significance, is a hollow victory.

Another important angle on the contribution, is to recognise the uncertainty of the research process – as true scientists, we never really know what we will find until the research is actually executed. So, at the time of conceiving the plan, we should try and think about reasonable scenarios – and, if possible, aim to express our contribution message in terms of the (hoped for) "upside" scenario versus the (dreaded) "downside" scenario. While the likely outcome is somewhere in between these two, our decision-making around research

priorities could very well be influenced by a subjective balancing of how *dire* is the perceived "downside" contribution versus how *alluring* is the potential "upside" contribution.

2.5 Template Item (K): Other Considerations?

Item (K) in Faff's (2015) template is a residual or "catchall" – it presents a time for posing any other relevant final reflections. Various suggestions are offered in the template. Is collaboration needed/desirable? For doctoral students such a question will be a sensitive one – in many cases only limited collaboration will be permitted. Also, the issue of collaboration will give rise to a discussion of roles, expectations and timelines.

What are your target audience or target output or target journal(s)? Are these targets realistic? Are they relevant? Sufficiently ambitious? Or, too ambitious? Linking back to Item (C), "key papers", I would argue that there is what I would label a "ceiling effect" in play here. That is, in many cases the journal quality in which the key papers are published represents a "ceiling" for the target journal of your planned research. For example, if you choose key papers that are all "A" journals, then it seems illogical to then have an "A*" journal as your target. Moreover, it would be quite likely that your target journal is a journal in which one of your key papers is published. However, in the counter case, while all your key papers might be published in Tier 1 journals, your realistic target might well be a lesser quality outlet.

Also, what about a "risk" assessment? While totally subjective, can you make a judgment on whether the proposed project has "low" vs. "moderate" vs. "high" risk, in certain respects? For example, the risk of "insignificant results"?[26] Or that of "competitor" risk (i.e. being beaten to publication by a strong competitor)? Or the risk of "obsolescence"? Or is there a "personal agenda" or "independence" risk – the risk that one becomes an

[26] In conversations with Devraj Basu, I was reminded of a strategy that many of us have used to circumvent the "no results" risk, that might also be seen as a "deal-breaker" consideration. Specifically, we can ask for a "proof of concept" analysis in which just one basic table and/or graph of basic results is required – often within a challenging but feasible timeframe (e.g. delivery required within 4 weeks or else the collaboration offer dissolves).

advocate, rather than an objective "scientist". [27] Or is there "political enemy" risk?[28] Is there any other major (research) risk exposure? Also, are there any serious challenge(s) that you face in executing this plan? If so, what are they? Are they related to the Idea? The Data? The Tools?

Finally, what about the implied scope of proposed analysis? Is the scope appropriate for the purpose or goal? Should it be narrower thereby allowing a deeper examination versus being broader and more shallow? These considerations of scope are often at issue in "conversations" between Honours/PhD students and their supervisors.[29]

3. General Advice on Using the Template

3.1 Advice to the "Pitcher" – PhD students and Novice Researchers

To this point, since the current paper has predominantly been written with the pitcher in mind, further detailed commentary under this heading is unnecessary. As already stated above, I plead with the pitcher – don't be scared! Treat the pitch template as your "friend", here to help you start a "conversation" with a relevant expert – a supervisor, a mentor, a potential collaborator. Among other things, I argue that this offers a big advantage in inducing better, more targeted feedback on your ideas. But, please take early and serious notice of the potential "deal breakers" (see Sub-section 2.2.4). Be concise and focused – "less is more", until "more" is requested. Think of it from the pitchee's point of view – what would you like to know if you were on the "other side"? View the pitch as a starting point only –

[27] Putting it another way, if you think that you already "know" the answer before you even start – that your task is simply to find the set of tests that confirm your firmly held beliefs, then you should not embark upon this path at all. Research is not a "religion". The reason is simple – you have no (actual and/or perceived) objectivity and, thus, your research (though very passionate) will not be "truly" independent or "scientific". This is what you might call "bad" passion. We should strive to harness "good" passion – the passion simpatico with objectivity and true science.

[28] I do agree with Eliza Wu who, in a private communication, warned that the focus on "tension" in Item (E) of the template can bring the real risk of naively "picking a fight" against a strong and established researcher or research group in which there is little chance of "success". Experienced mentors are well placed to assess this risk early on, and particularly whether it might be so significant that it becomes a "deal breaker".

[29] Of course, there is an important distinction between the appropriate scope of an Honours thesis versus a PhD – most simply thought of in terms of the differential timeframe constraint, 9 months versus 36 months. For example, it is not uncommon for an Honours student to be advised that their topic is too broad – "… hey, that's a PhD – you won't have time to do justice to that topic … we need to cut this down."

don't suffer from the pitfall of "perfectionism" – particularly at such an early stage of the research process, just get your core ideas down. Appreciate the benefit that the template gives in terms of organizing your thoughts in a concise/structured way.

3.2 Advice to the "Pitchee" – Supervisors/Research Mentors

Hey! It's a two-way street! As a "pitchee" you need to know how to help the pitcher get the best from the exercise – my argument is simple: if this process helps "start a conversation", then you already have a "win". Above all be supportive and encouraging. But, please also be vigilant and pro-active on the question of potential "deal breakers" (see Sub-section 2.2.4) – this is where your experience and expertise are vitally important! Any *bona fide* effort – that produces a seriously completed pitch, however "flawed" it may be, is a success! In the embryonic stages, these exercises help us more quickly and efficiently move on a positive research trajectory. As such, the pitch template offers big advantages to you, the pitchee. Used wisely, if nothing else, it can help save you a lot of time and avoid much frustration. To assist even further in this regard, in the online material I provide a counterpart pitchee's version of the (pitcher's) cued template in Figure 3.[30]

As a pitchee, you have a "duty of care" to the potential pitcher.[31] As such, you should devise a "pre-pitch" strategy in which you aim to help minimise the chance of an early/any repeated "dead end(s)" for your protégé. To this end, they will want early guidance on what ideas are worth thinking more about and which ones are not? They will want guidance on how to efficiently generate a "pool" of potential research directions. In this regard, there are several strands of advice I can offer.

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[30] A softcopy WORD file of the pitchee's cued version of the template is available from the authors webpage: http://www.business.uq.edu.au/staff/details/robert-faff (please scroll down the webpage until you find the download prompt).

[31] Not everyone agrees with the implied "risk averse" stance that I take here as a supervisor/mentor. Some argue that such an approach could easily stifle a brilliant student/brilliant topic that could lead to a major seminal work in a given field. There is no right or wrong answer here – it is a judgment call that we all need to make for ourselves, in terms of how we execute our "duty of care".

First, emphasize very early on to the pitcher the need to follow a "smart" (cocktail glass) approach to reading the literature and to quickly run ideas past you. Second, advise your pitcher to read works like Stokes (2013) to gain a strategic mindset that can enhance their ability e.g. to scan the literature. Third, recommend that the pitcher seek out recent survey articles written by "gurus" in the field relating to their broad topic areas of interest.[32] Finally, particularly with Honours or MSc students in mind, you could apply the "four-eyes" (4 x "i"s) principle, where "i" here prompts layered questioning around the student's academic discipline-related "interests".[33] The first "i" asks the student what Major in their coursework study do they find most *interesting*? Second, within that major what subject is most *interesting*? Third, within that subject what topic is most *interesting*? And fourth, within that topic what subtopic or vexing issue is most *interesting*? This simple, "drill down" approach can help usefully narrow the field, which can further be filtered by questions of their current/potential skillset (e.g. which areas did the student get their best marks) and their "confidence/comfort" (e.g. which area does the student feel most confident about studying in great depth from a research perspective).

A few further words of advice, particularly to novice/junior pitchees. Try and think of it from the pitcher's point of view – in particular, from a position of: perceived/actual ignorance about the technical aspects of the topic, a fear of being foolish and not knowing what is really important at the beginning. Once a completed pitch is in hand, identify the strengths/weaknesses. Applaud the strengths! Make it clear why such aspects are deemed strengths. Offer guidance on the weaknesses – specific or general. Aim to help develop the pitch to be uniformly strong.

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[32] An excellent source of such review articles spanning a broad range of discipline areas is *Annual Reviews* [http://www.annualreviews.org/], for example, including (a) biomedical/life sciences: biochemistry, biophysics, clinical psychology, genetics, marine science, medicine, physiology, virology; (b) physical sciences: biophysics, computer science, fluid mechanics, physical chemistry; (c) social sciences: anthropology, economics, financial economics, political science, psychology, sociology, to name but a few. As stated on their website, the "… mission of Annual Reviews is to provide systematic, periodic examinations of scholarly advances in a number of fields of science through critical authoritative reviews. The comprehensive critical review not only summarizes a topic but also roots out errors of fact or concept and provokes discussion that will lead to new research activity."

[33] I thank my colleague, Barry Oliver, for suggesting (and naming) this simple but effective approach.

4. Pitching Resource Center Support

To take the lengthy details out of the current paper, in a new companion paper, Faff (2017) now gives a comprehensive update and latest information (including numerous actionable hyperlinks) on **ALL** resources currently available to support the pitching research® framework. In other words, Faff (2107) is the "one-stop shop" – **"Resource Central"**. Specifically, and most notably, **V1** of Faff (2017) provides details relating to: (a) an e-library of **196** worked pitching template examples;[34] (b) a separate listing of **61** worked examples relevant to finance research topics (with individual hyperlinks); (c) a separate listing of **35** worked examples relevant to accounting research topics (with individual hyperlinks); (d) the associated webportal, "PitchMyResearch.com"; (e) a stable of **19** associated SSRN "pitching" papers;[35] (f) doctoral symposia and coursework applications of the pitching research® framework; (g) research grant application of the pitching research® framework; (h) YouTube video resources; (i) publication opportunities, via "pitching research letters"; (j) pitching "ambassador" and research digest initiatives. **Readers are strongly encouraged to access Faff (2017) and exploit the benefits from this extensive resource pool.**

[34] The e-library can be accessed at (please scroll down the webpage until you find the relevant weblink prompt): http://www.business.uq.edu.au/supplementary-material-pitching-research

[35] Faff (2016a); Faff (2016b); Faff, Alqahtani, et al., (2017); Faff, Godfrey and Teng (2016); Faff, Ali, et al. (2016); Faff, Babakhani, et al, (2017); Faff, Babakhani, Dallest et al, (2017); Faff, Baladi, et al, (2017); Faff, Gill, et al (2017); Faff and Kastelle (2016); Faff, Li, Nguyen and Ye (2016); Faff, Wallin, et al. (2016); Faff (2016c); Faff, Lay and Smith (2017); Faff, Carrick, et al. (2017a); Faff, Carrick, et al. (2017b); Faff, Carrick, et al. (2017c); Nguyen, Faff and Haq (2017); Teng and Faff (2017).

5. Conclusion

Following a regularly updated revision process, the current paper, along with Faff (2017), are "dynamic" companions to Faff (2015). Collectively, these papers further explore a methodical approach to pitching a new research proposal, enabled by Faff's pitching template framework. This simple template is designed to allow a researcher in virtually any academic discipline to identify the core elements of a viable and worthwhile empirical research proposal. The template is built around the core "gimmick" of a "4-3-2-1" countdown design.

Four stands for the four "framing" items that begin the template: Working Title, Key Research Question, Key Papers, Motivation/Puzzle – collectively, these four pieces serve to give broad context to what then follows as more "specific" project-based information.

Three represents the essential "building blocks" of Idea, Data and Tools.

Two represents the two basic questions a researcher has to convincingly answer: "What's new?" and "So what?"

One represents the "holy grail" Contribution! I hope that this template will be of great use as a training tool for developing strong research proposals by the leading researchers of the future.

While the current paper is now in its fifteenth major version, I will continue striving hard to broaden its appeal to all possible areas of academic endeavour.

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Figure 1: Faff (2015) Pitching Template

Pitcher's Name		**FoR category**		**Date Completed**	
(A) Working Title					
(B) Basic Research Question					
(C) Key paper(s)					
(D) Motivation/Puzzle					
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide				
(E) Idea?					
(F) Data?					
(G) Tools?					
TWO	**Two** key questions				
(H) What's New?					
(I) So What?					
ONE	**One** bottom line				
(J) Contribution?					
(K) Other Considerations					

Source: Faff (2015).

Figure 2: Pitching the "Pitch Research" Project

Pitcher's Name	Robert Faff	**FoR category**	Higher Education	**Date Completed**	18/12/14
(A) Working Title	"Pitching Research"				
(B) Basic Research Question	Create a tool/mindset that captures the essential information needed to give a sound basis for starting a new research project				
(C) Key paper(s)	Stokes, D., (2013), "Generating Innovative Research Ideas", Journal of Accounting and Management Information Systems 12, 145-155.				
(D) Motivation/Puzzle	The hardest thing about doing research is starting it. Finishing the research is also difficult, but unless you begin, finishing is irrelevant. Novice researchers rarely know where to start – they often suffer from being overwhelmed. Novice researchers never know what are the essential items of information that would be convincing to their potential research mentor (or supervisor). Everyone is busy – especially supervisors and research mentors. Creating a more effective means to "pitch" a research topic would be beneficial for all concerned.				
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide				
(E) Idea?	Its all about the "pitch". The relationship between the two parties to the "pitch" is central and critical – hence, I purposefully draw attention to this linkage by choosing the paired terms "pitcher"/"pitchee". Then, the core idea here is developing a pitch "template" – a succinctly formatted device that is logically designed, builds in its flow and allows a clear and coherent message to be conveyed between the "pitcher" and the "pitchee"				
(F) Data?	Normally in research we expect to see "data". The nature of data in this project is very different. In a sense the data are the worked examples of the template showing novice researchers in a very real and practical way "proof of concept" – how it can work in their field of interest.				
(G) Tools?	The core tool here is the "naked" pitch template itself. This is supplemented by: **Short term:** • advice on use; • a version of the template with "cues" **Long term:** • evolving library of examples; • expanding set of Internet resources including a Youtube video; appendices; PowerPoint slides and Prezi presentation template; • technology enhanced delivery of template technology via web-based application.				
TWO	**Two** key questions				
(H) What's New?	Novelty can be thought of in a few ways. First, focusing attention on the common challenge faced by novice researchers: to initiate a "conversation" [i.e. meaningfully convey essential information] with a mentor in a simple and clear way regarding a new research idea. Second, the novelty is around the simple template device – not new in its constituent parts, but new in its overall design by bringing together cohesively, essential ingredients that create a simple "synergistic" package. The template "tool" is a big driver, but this is inextricably linked to the "idea" as well. The worked examples, as "data", are also very important for inducing wide takeup of the concept.				
(I) So What?	My pitching template research is important because it will lead to major efficiencies in the research process – efficiencies that can be characterised by substantial savings in time at the beginning of the research journey – for BOTH novice and seasoned researchers (mentors). This saving in time will have positive psychological/motivational effects that help magnify the benefits going forward. These benefits will manifest in: higher quality research outcomes; more timely PhD/paper completions and help create good long-term research habits that will give a "sustainability" dimension.				
ONE	**One** bottom line				
(J) Contribution?	FREE provision of a simple tool and deep support … across the full spectrum of academic research … with many potential applications … finance, accounting, management, CSR, chemistry, physics, healthcare, psychology … short-term and long-term benefits to all researchers. Extensive impact on research that is NOT discipline constrained				
(K) Other Considerations	No direct Collaboration – but extensive support "collaboration" critical eg provision of examples to populate an expanding library; workshops/seminars/pitch day events Target Journal: ultimately - highest profile/quality education-type journal, relevant to higher education/research. "Risk" assessment: (1) "competitor" risk - low; (2) risk of "obsolescence" – low, involves an issue of enduring concern relevant to ALL research fields; (3) "no result" risk – low. Other challenge(s)? getting people to "listen" and "invest" a little time reading what is being offered – the "salesman" dilemma. Is the scope appropriate? As potential examples expand, exploit the online angle. Perfect template is unattainable – convince audience of core benefit, encourage adaptation to personal preference. Need to confront various negative "syndromes": (a) "in house" templates/"I already do this!"; (b) Too good to be true; (c) Too simple to be useful; (d) Nothing new, so little value.				

Source: Faff (2015).

Figure 3: Faff (2015) Pitching Template with Cues for the Pitcher

Pitcher's Name	Your name here[37]	**FoR category**	Field of research?	**Date Completed**	Insert date here
(A) Working Title	Succinct/informative title here				
(B) Basic Research Question	IN one sentence, define the **key** features of the research question.				
(C) Key paper(s)	Identify the key paper(s) which most critically underpin the topic (just standard reference details). Ideally one paper, but at most 3 papers. Ideally, by "gurus" in the field, either recently published in Tier 1 journal(s) or recent working paper e.g. on SSRN.				
(D) Motivation/Puzzle	IN one short paragraph (say a max of 100 words) capture the core motivation – which may include identifying a "puzzle" that you hope to resolve.				
THREE	**Three** core aspects of any empirical research project i.e. the "**ID**io**T**s" guide				
(E) Idea?	Identify the "core" idea that drives the intellectual content of this research topic. If possible, articulate the central hypothesis(es). Identify the key dependent ("explained") variable and the key test/independent ("explanatory") variable(s). Is there any serious threat from endogeneity here? If so, what is the identification strategy? EG: is there a natural experiment or exogenous shock that can be exploited? Is there any theoretical "tension" that can be exploited?				
(F) Data?	(1) What data do you propose to use? e.g. country/setting; Why? Unit of analysis? Individuals, firms, portfolios, industries, countries …? sample period; sampling interval? Daily, weekly, monthly, quarterly, annual, … Type of data: firm specific vs. industry vs. macro vs. …? (2) What sample size do you expect? Cross-sectionally? In Time-series/longitudinal? (3)Is it a panel dataset? (4) Data Sources? Are the data commercially available? Any hand-collecting required? Are the data to be created based on your own survey instrument? Or by interviews? Timeframe? Research assistance needed? Funding/grants? Are they novel new data? (5) Will there be any problem with missing data/observations? Database merge issues? Data manipulation/"cleansing" issues? (6) Will your "test" variables exhibit adequate ("meaningful") variation to give good power? Quality/reliability of data? (7) Other data obstacles? E.g. external validity? construct validity?				
(G) Tools?	Basic empirical framework and research design? Is it a regression model approach? Survey instrument issues/design? Interview design? Econometric software needed/appropriate for job? Accessible through normal channels? Knowledge of implementation of appropriate or best statistical/econometric tests? Compatibility of data with planned empirical framework? Is statistical validity an issue?				
TWO	**Two** key questions				
(H) What's New?	Is the novelty in the idea/data/tools? Which is the "driver", and are the "passengers" likely to pull their weight? Is this "Mickey Mouse" [i.e. can you draw a simple Venn diagram to depict the novelty in your proposal?]				
(I) So What?	Why is it important to know the answer? How will major decisions/behaviour/activity etc be influenced by the outcome of this research?				
ONE	**One** bottom line				
(J) Contribution?	What is the primary source of the contribution to the relevant research literature?				
(K) Other Considerations	Is **Collaboration** needed/desirable? – idea/data/tools? (either internal or external to your institution) **Target** Journal(s)? Realistic? Sufficiently ambitious? "**Risk**" assessment ["low" vs. "moderate" vs. "high": "no result" risk; "competitor" risk (ie being beaten by a competitor); risk of "obsolescence"; other risks? Are there any serious challenge(s) that you face in executing this plan? What are they? Are they related to the Idea? The Data? The Tools? Are there ethical considerations? Ethics clearance? Is the **scope** appropriate? Not too narrow, not too broad.				

Source: Faff (2015).

[37] The guidelines in red should be deleted and replaced by the best available "answers" in relation to the proposed research topic, obeying an overall 2-page (1,000 word) limit.

Figure 4: Indicative Pitch Item Completion "Clock" from PitchMyResearch.com Web Portal

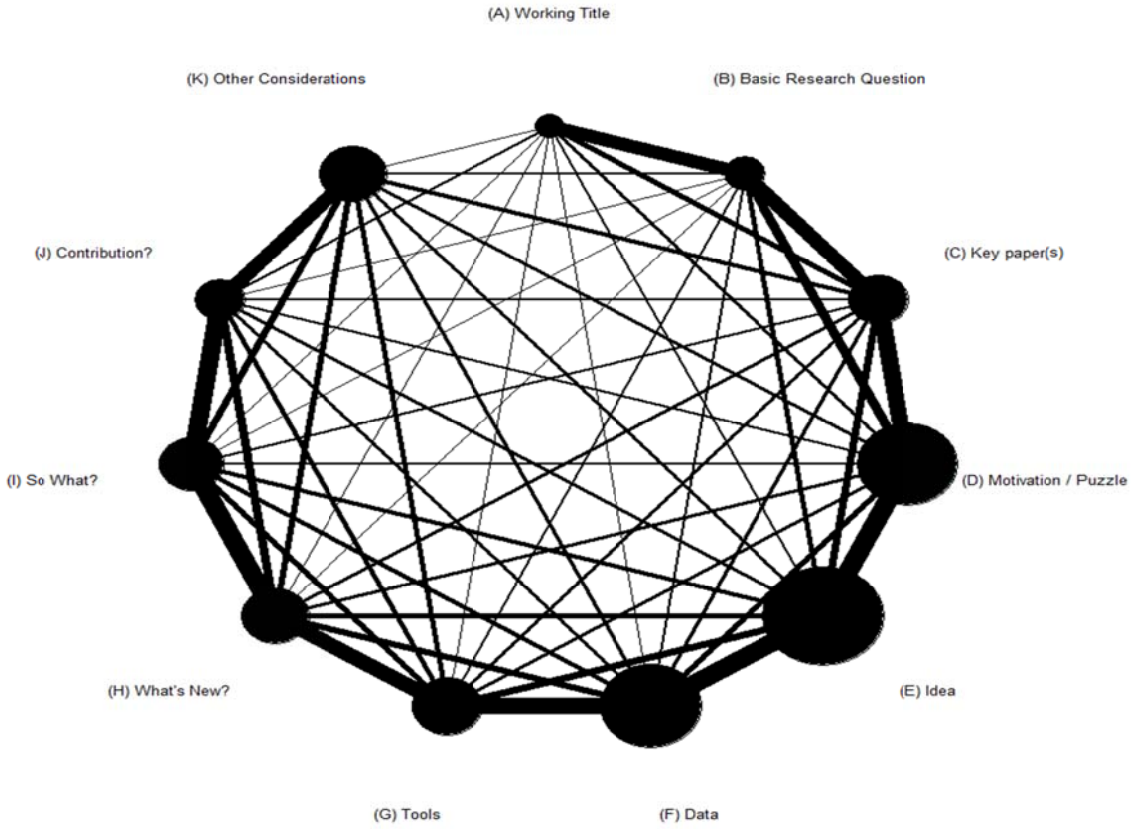
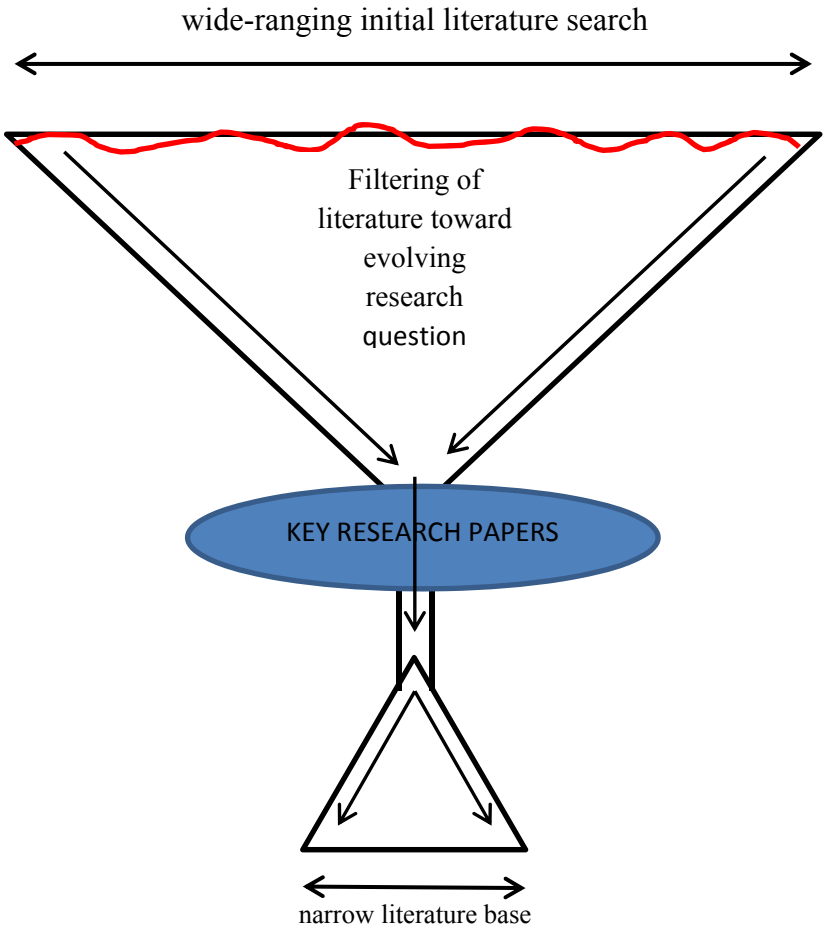


Figure 5: Faff's (2015) Cocktail Glass Approach to Reading/Filtering the Literature



Source: Faff (2015).

Figure 6: A Generic Characterisation of how Mickey Mouse might help to identify Novelty in Research