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Research ideas matter: Guidance for research students and early career researchers

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ABSTRACT

This paper draws on the collective experience of the authors as researchers, supervisors and editors spanning the fields of medical science, psychology, analytics, entrepreneurship, accounting, and finance. We provide guidance about research idea formulation for novice researchers, both Higher Degree Research students (HDRs) such as PhD students, and Early Career Researchers (ECRs). Drawing on parallels with business opportunity formulation, we break down the process into three steps: generating, refining and then evaluating research ideas. We present strategies to help generate high-quality research ideas, including five key perspectives for identifying gaps in the literature that are opportunities to be addressed with novel research ideas. We also provide advice for refining research ideas, and provide guidance on evaluating and ranking those ideas. Despite the diverse background of the authorship team, there are many commonalities because of the rigorous scientific nature of the general academic research process. Helpful research tools and templates, as well as examples and suggested readings, are also presented to assist novice researchers in the challenging process of forming research ideas.

1. Introduction

What is a good topic to research for my thesis or my next project? Where do I find ideas, or how can I generate ideas? How can I select a topic to ensure that it will be publishable at the end? In essence, how can I maximise my chances of success in research? Trying to answer these questions as someone early in their research journey can be daunting. In this paper, we draw on the collective experience and wisdom of a cross-section of science and social science researchers to provide some suggested paths and strategies that we collectively employ in our research careers. The authors' research backgrounds encompass medical science, psychology, analytics, entrepreneurship, accounting, and finance. So, the examples and references reflect this diverse base of experience; an important finding is that there are many commonalities we can learn from on account of the rigorous scientific nature of the general academic research process.

This paper is directed towards novice researchers, both Higher Degree Research students (HDRs) such as PhD students, and Early Career Researchers (ECRs). The authors hope that such researchers find this material insightful and helpful, and encourage it to be considered as one part of a much larger program of reading about research, covering topics such as the scientific method (Popper, 1981), responsible science (Faff, 2021b), and templates for designing, communicating and visualising research (Faff, 2021a; Lodhia,

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2019; Faff and Kernbach, 2021), as just a few selected examples. Bui (2021) interviewed users and found that a popular Pitching Research (Faff, 2021a; Faff, 2015) template was most beneficial to novice researchers. The importance of generating and refining research ideas as part of the overall research design is demonstrated by its inclusion as a separate "(E) Idea" element (Faff, 2023c) in the template. However, Bui (2021) found that the template was least helpful for idea generation, arguably the most difficult part of a research project according to Bui. The guidance presented in this paper is directly relevant to this challenging part of the research process.

It is important to be working on something novel. Studies that are obvious investigations, or just data gathering, will not add much to the field or a researcher's career. Instead, it is worth spending the time and effort to develop good research ideas that ultimately result in published research shared with colleagues and other stakeholders around the world. If we take the publication of high quality research that contributes to our academic community as our focal objective, then we must develop research ideas that contribute to the literature. We can think of this in a reverse sense in that journal editors and reviewers will look at submitted manuscripts from the position of whether the manuscript has the potential to add to the stock of knowledge and is of interest to their readers, and thus is worthy of publication and circulation within the field. Experienced researchers know a common comment from reviewers is a version of "it is unclear how the manuscript contributes to the literature", which is usually accompanied by an email rejecting many hours and even years of work invested in the submitted paper. Hence, if we work back from the publication goal and think about how to develop research ideas that will make a substantial contribution to the literature, then we have a starting point for any new project; quality and high publication probability will be built-in right from the research idea stage.

To assist with research idea formulation, we draw on parallels with business opportunity formulation, and specifically adapt the Opportunity Formulation Process described by Lindsay and Craig (2002) as shown in Fig. 1. We largely follow this structure in the remainder of this paper as shown in Fig. 1. In between, in Section 3, we present lessons from an exemplar researcher. Key themes are then presented in the concluding Section 6.

2. How to generate research ideas

2.1. Use of literature reviews

An effective place for novice researchers to start is to identify recent review papers within the domain of interest. For example, if we were considering researching an emerging research area such as environmental, social and governance (ESG) finance, we could search for systematic literature reviews in the area and find the paper by Daugaard (2020). In some disciplines there are specific journals that publish integrated reviews of the literature such as the *Journal of Accounting Literature*. Review papers typically discuss the body of research over a defined period and thus provide a good summary of what has been done to date with integrated commentary. Such reviews also identify directions for future research (see Linnenluecke et al. (2017) and Benson et al. (2015) as examples). In the case of ESG finance, Daugaard (2020) provides five emerging themes that are described as fertile areas for future research. Reflecting on these future directions at the same time as utilising a researcher's own knowledge provides a sound starting point for potential research ideas that can be crafted over time into worthy research questions.

In domains without relevant high-quality review papers, one strategy is to write a critical literature review and from that identify the potential research gaps. Although not guaranteed nor essential, such a review paper may make a significant contribution worthy of

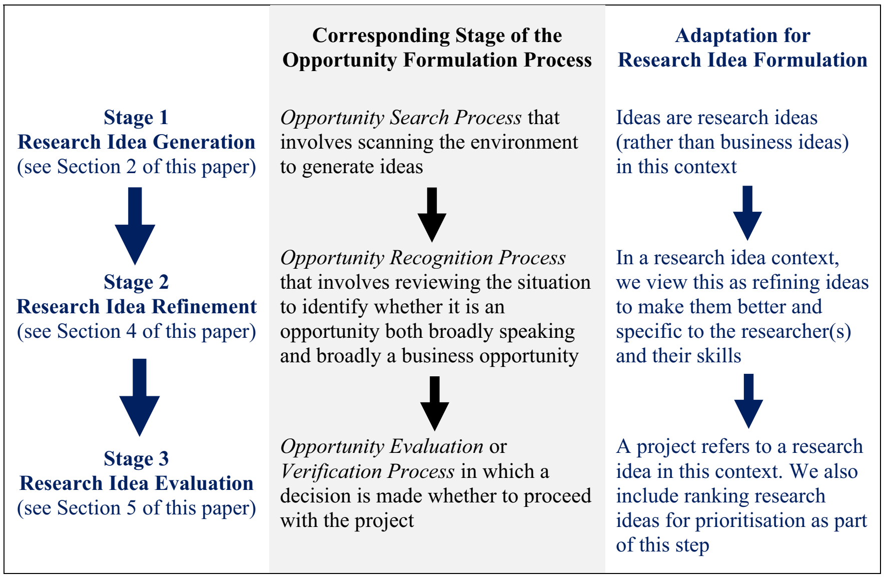

Fig. 1. Stages of Research Idea Formulation.

publication itself (e.g. Daugaard (2020)). In this way, a literature review is a viable research methodology. There are many different types of reviews that can be conducted; they range from systematic through semi-systematic to integrative types and scoping reviews. For a more detailed discussion on using a literature review as a research methodology, refer to Snyder (2019) or Linnenluecke et al. (2020) specifically for systematic reviews.

When reviewing papers with quantitative results, techniques such as meta-analyses enable effects to be examined in aggregate across a body of research papers. Meta-analysis uses the distributional properties of estimated statistical parameters from a body of work to determine whether the effect of the phenomenon researched is statistically significant in aggregate (see Barton et al. (2022) as an example from marketing). Another example is Jeny and Moldovan's (2021) meta-analysis studying R&D accounting treatment (capitalisation vs expensing) that found an aggregate positive effect on value, but no clear finding on the determinants of the decision. Such identification of points of consensus and disagreement enables future research to focus on the areas that are contradictory. Resolving such conflicts in the literature is an important contribution. The meta-analysis technique is not often applied in finance and thus is a potential methodology for finance researchers – see Geyer-Klingeberg et al. (2020) for a discussion of the method and the opportunities and challenges with respect to the field of finance.

2.2. Perspectives on literature gaps

One way to think about research is as an investigation of the missing pieces that fill the gaps identified in the literature. While there are potentially many missing pieces in a domain, we present five key perspectives on generating research ideas that address gaps in the literature – see Fig. 2. The first four perspectives reflect key dimensions in fundamental research methodology. These are to consider the gaps first from a theoretical perspective, secondly in terms of the measurement and data employed, thirdly in terms of the research methodology, and fourthly in terms of the analysis conducted. A fifth perspective is to consider gaps from the perspective of questions that are relevant to industry.

2.3. Theory gaps and RQs

As academics we hear the expression "that's alright in theory, but will it work in practice?" and often laugh it off as a dig at academics, but experienced researchers turn the question round and ask, "that's alright in practice but will it work in theory?" So, how does this expression help us when thinking about generating research questions? It highlights the core thinking of researchers seeking to understand a phenomenon and explain it using existing or new theoretical frameworks. Note the process of identifying the phenomena to research is considered more in our discussion of industry perspectives on research gaps.

Focusing on theory first, we can examine if our empirical observations fit with our theoretical frameworks – if so, then we have a basis for understanding those observations. However, sometimes we need to ask whether the correct theory has been applied and whether a new theoretical perspective is needed. Considering this, a fruitful approach to developing research ideas is to study the theoretical frameworks used in the prior work. In some cases, it may be that the theoretical lens employed in the prior work is too narrow, and by adopting additional theoretical perspectives a better understanding of the underlining phenomenon can be developed by the researcher. For example, agency theory is often applied to finance topics such as leveraged buyouts from a control perspective, but venture capitalists see buyouts as entrepreneurial opportunities. Wright et al. (2001) show that applying an entrepreneurial lens leads to conceptual development and new empirical testing as a contribution to the literature.

Taking a new theoretical perspective may involve integrating multiple theories or extending existing theories, and this process can require researchers to look at other disciplines. For instance, research in behavioural accounting and finance, considering the effect of heuristics and biases in those decision-making processes, can be informed by understanding psychology research. A lot of research has studied non-rational decisions and theoretical concepts such as loss aversion, drawing on findings initially from psychology (see Thaler et al. (1997) and related work). In this case one size does not fit all when thinking about theoretical explanations for observed empirical phenomena. In finance and accounting research, researchers deal with market, organisational, and individual level decisions, and outcomes. Given that these units of analysis are not uniform, the path for any theoretical effect is likely to be nuanced, and researchers need to employ different theoretical perspectives to fully understand the observed phenomena. See, for example, Nguyen et al.'s (2020) multi-level and multi-theoretic review of women on boards and financial performance. Consequently, adopting multiple theoretical perspectives is a useful research strategy in developing good research ideas, but it also necessitates the need to upskill on multiple theoretical approaches.

Finally, some researchers specialise in developing theory. An example from the accounting discipline is Professor Jim Ohlson who is recognised by the American Accounting Association with The Notable Contribution to Accounting Literature Award. Other researchers may not have the skills or interest in developing such theories, but rather adopt the strategy of testing the empirical implications of the theories created by specialists like Professor Jim Ohlson.

2.4. Measurement gaps and RQs

Social science researchers are often interested in the relationships between abstract constructs such as motivation and their effect on job satisfaction, performance, or the intention to purchase. To measure these constructs, measures and scales are used as proxy for the underlying unobservable phenomena as depicted in Fig. 3. In this way, the unobservable theoretical construct A, job satisfaction, is measured on the empirical plane using a series of questions measured on a 5-point Likert scale. These scores might then simply be summed for an aggregate proxy measure of job satisfaction. The same concept applies for the measurement of construct B, motivation.

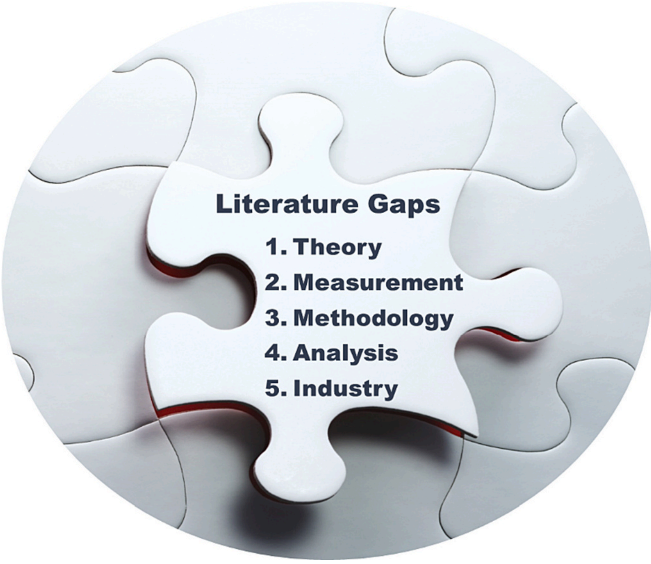

Fig. 2. Key perspectives on potential missing pieces in the literature.

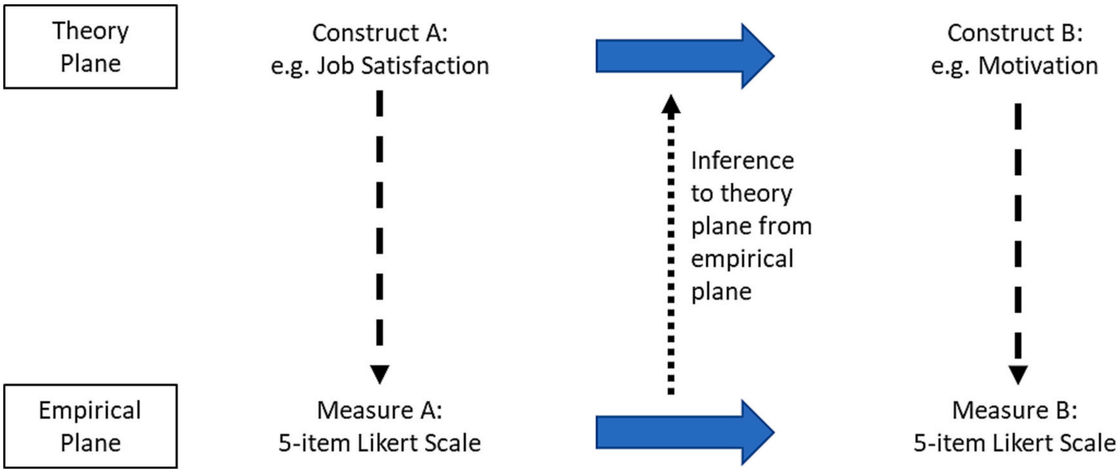

Fig. 3. Measurement of theoretical constructs.

The specific constructs and Likert-scale measures are not important, but merely examples. The key point is that when researchers observe the two proxy measures being statistically related on the empirical plane, they then infer that constructs A and B at the theorical plane are also related. Note, however, that we never observe the latter. It then follows that one reason for prior research, possibly discussed in a review paper, finding inconsistent results about a theoretical relationship is that the measures used at the empirical level poorly capture the underlying theoretical constructs.

Developing and testing an empirical measure to use for a theoretical construct is a potentially valuable research question. For example, early research on corporate governance focused on individual governance dimensions such as board size (John and Senbet, 1998) and board independence (Burton, 2000), before researchers interested in overall governance developed composite measures and formed them into an index (Gompers et al., 2003). As ECRs, Mathuva et al. (2019) applied the Pitching Research (Faff, 2021a; Faff, 2015) framework and received an Emerald Literati Outstanding Paper Award[1] for demonstrating the process of developing a corporate governance index in the context of Kenyan audited financial statements. Thus, valuable research insights can emerge by studying the measures used to proxy for underling constructs and developing multi-dimensional indices of the phenomena. In some cases, sub-dimensions of the construct might be the key. For example, Aldamen and Duncan (2016) measure 15 individual corporate

[1] Award winners listed at https://www.emeraldgrouppublishing.com/journal/jaee/journal-accounting-emerging-economies-literati-award-winners-2020

governance variables that they reduce via principal components analysis (PCA) to three factors and find only one of these sub-dimensions is statistically significant in explaining discretionary accruals during the Global Financial Crisis of 2007–2008. Alternatively, when a newly developed proxy measure is published, it can be fruitful to consider whether prior findings need to be re-investigated, particularly if the quality of the prior proxy measure is uncertain.

A review of the literature might also identify that the measure used to date for a construct is deficient and warrants a scientific approach to developing a new metric. A search of the literature in the domain of interest will identify papers that have solely or largely focused on developing new measures and these are a guide to the process. There are also more general papers such as MacKenzie et al. (2011) who detail the construct measurement and validation processes in the social sciences. Similarly, there are research areas where higher order properties of the data's distribution can add insight to a field. One example of a new measure that makes use a higher order distributional property is using negative skewness of returns as a proxy for information flows and price crash risk (Hu et al., 2020).

Another potential reason for finding inconsistent results about a theoretical relationship in prior literature is limitations or deficiencies with the data collected and analysed. Thus, one way to consider addressing measurement gaps in the literature is to focus on different data. It is critical to distinguish this approach of starting with a literature gap and then filling it by using different data, as opposed to simply applying an existing method to another data set, which may or may not actually result in a publishable contribution. The different data can be in the form of different subjects, different time periods, different markets, different variable definitions and so on. For instance, in financial research, we can use interest expense (an accrual) or interest paid (a cash flow) as an interest cost metric. When computing the interest rate on debt financing, then these two different definitions of the numerator will result in different outcome that may be important to the underlying study. Although if studying contracted interest cost (a concept relevant to a contract cost theoretical hypothesis) then the footnote disclosure of contracted debt rates is likely most applicable. Aldamen and Duncan (2016) demonstrate how such a simple refinement in variable measurement then reconciles conflicting results of prior literature. Variable definition and measurement can make a big difference to testing the theory. It may also be that the data necessary to answer some research questions are only available in some markets. For instance, in contrast with other markets, companies in China have since 2008 disclosed entertainment expenses, a potential proxy for perquisite consumption or corruption, making this a unique setting for many research ideas to be explored (see Hu (2021) as an example).

2.5. Methodology gaps and RQs

When studying the existing literature or reading reviews, considering the methodologies employed so far might identify a research gap. For instance, if much of the work is analytical then perhaps adding some empirical work to test the prior analytical work is an appropriate way forward for the research. Additionally, in business many research questions have aspects that can be considered at different levels of analysis such as individual, group, organisational, firm, market, or country levels of analysis (e.g. Nguyen et al. (2020)). Extending the literature to a different level of analysis can contribute to the domain of interest. It may be that a literature review identifies that most of the research about a specific idea has used the same approach. For instance, in accounting and finance a lot of research focuses on archival data or data available from databases. While this is perfectly valid research, it is possible that there are diminishing returns regarding new insights gleaned from such data and novel insights might be uncovered by connecting with the target subjects directly. One approach is to conduct an experiment involving individual decision-making which Huber and Kirchler (2023) show is a limited but growing trend in finance research. Some researchers turn to survey methods, but they can suffer from non-response bias, and so interviews should also be considered for suitability given the research question being investigated. While interviews are more time consuming to design, initiate, collate, and analyse, they can provide unique insights on an existing research idea. For instance, in business, we might be interested in the viewpoint of a board of directors that might only be obtained by talking with them directly (see Trotman and Duncan (2018) as an example).

A lot of empirical research in business employs archival, survey and interview techniques. Often there is limited experimental work although this is changing with greater emphasis on the behavioural aspects of decision-making in domains such as finance and accounting (for a review of experimental methods in finance see Huber and Kirchler (2023)). Experimental research has an advantage in terms of internal control and thus can be very useful to test different theoretical predictions, such as where we want to address a gap in the literature by employing different theoretical perspectives. An example is the Non-GAAP earnings literature that was initially dominated by archival work but recent contributions are via experimental decision-making studies (see the review by Brosnan et al. (2023a) and associated experiment by Brosnan et al. (2023b)). Many journals and editors are inviting research that employs methodology such as experiments to add to the body of research in their domain. In a recent finance paper calling for more diverse methods, Kaczynski et al. (2014) on p. 127 state it this way: “Imagine the benefits to finance if we expand our empirical sources of data to include what people have to say, which then allows us to explore the complex reasoning behind these conversations.” An example are the insights gained via survey and interviews into real earnings management vs accrual earnings management by Graham et al. (2005). Much of the prior work was archival and this paper produced new insights that expanded the literature. Triangulation in research, using multiple research methods, adds to a discipline (Kaczynski et al., 2014).

2.6. Analysis gaps and RQs

When looking for gaps in the literature to fill with new research ideas, one approach is to consider innovations in analysis. Data analytics is an ever-expanding field that is also becoming more accessible through powerful computers and modern software, much of which is open source (freely available). Advances in data analytics allows researchers to ask questions that could not previously be addressed, and some review articles call for more research leveraging such advances. Gepp et al. (2018) provide such a review in which they suggest future research in the auditing domain takes better advantage of big data analytics such as utilising sentiment analysis and natural language processing. Analytic innovations have the potential to substantially affect our understanding of phenomena.

Models with less restrictive distributional assumptions are proving powerful in certain situations. Thinking about a conceptual relationship in terms of non-linear effects can be key to advancing a domain and sometimes help our theoretical perspective. If we take the concept of information overload as an example: more information enhances decision-making but only up to a turning point, called the overload point, past which additional information is not effectively used in decision-making. There might be phenomena in your field that have been studied using a linear approach that would benefit from more flexible models because the theory now indicates a non-linear effect, or the nature of the effect is as yet uncertain. An example of the latter is researchers developing better financial fraud detection models using more flexible techniques that can handle non-linearities and interactions between independent variables (Gepp et al., 2021). Another simple innovation is to test for negative effects when the literature has tended to focus on positive effects. An example of this is the contingency theory work on systems fit and performance in organisations where looking at the phenomenon in terms of lack of fit via say the residual magnitude from a fitted regression can provide insight for subsequent literature in the area (see Duncan and Moores (1989) as a seminal exemplar). These are merely a selection of examples to reveal that good research ideas can be generated from identifying weaknesses in the analysis employed by prior research.

Novice and expert researchers alike would benefit from staying current with methodological papers in their field. An example from the accounting and finance field is Gippel et al.'s (2015) framework to unify the current approaches to dealing with endogeneity issues in accounting and finance, which is also a novel solution using a natural experiment. In the same field, difference-in-difference analysis is becoming commonly required to confirm the differential effects of a phenomenon. This facilitates adopting a quasi-experimental design where researchers can explore the effect of an event or change where some of their sample is impacted, called the treatment group, and the rest of the sample is unimpacted by the change or event. For example, researchers might be interested in the effect of a regulatory change where only some firms are affected by the change (Hu et al., 2020). More recently, developments in artificial intelligence (AI) and machine leaning (ML) are emergent technologies in finance research that are offering new insights as discussed in the review by Goodell et al. (2021).

Collaborating with those in the data analytics field is another strategy that may help bring new insights to a research area. Such collaborators have also been termed *collaborating statisticians* and are discussed, amongst other excellent advice for analysis, by one of the great statisticians Brad Efron (Holmes et al., 2003).

2.7. Industry RQs

We started with the saying "that's alright in practice but will it work in theory". Practice or industry provide the phenomena for us to investigate by applying our bag of theory and research skills. Recently, there is an increasing call for academic research in business to become more relevant to industry. Many research funding agencies prioritise research with impact that is going to contribute to industry and the economy. This shift in emphasis represents an opportunity for researchers to identify new research ideas in their domain. We recommend engaging with industry, or a relevant group of professionals; they can be our teachers, our inspiration. Conversations with such people can identify complex problems and puzzles that need the expertise of academic researchers to document, explore and explain. These conversations are not set up about research ideas per se; they are more akin to regular *check ins* to find out challenges at the coal face that might inspire new research ideas.

Even a simple approach of surveying industry colleagues can produce unexpected results. Although not the catalyst for doing the research, conducting a survey of accounting firms about outsourcing (Duncan et al., 2018) identified a need for a separate future project to investigate how accountants were dealing with technological advances such as artificial intelligence. Other methods of engagement include interviewing industry leaders, attending industry conferences, workshops, and seminars, and reviewing trends in industry publications. Over time, one should aim to develop a personal network of contacts in industry. This can lead to another strategy, which is to work directly with an industry partner on their complex problem. This last strategy can provide access to novel ideas, unique data and potentially to funding, as well as the potential to flow on to subsequent industry partners and larger grants. It is important to remain a responsible scientist (Faff, 2021b) with this approach, including documenting and addressing any potential conflicts of interest with the scientific research process.

2.8. Research generates quality research ideas

In contrast to the external sources of research ideas presented in the previous section, here we present an internal source – that is, good research ideas emerge from an active program of research and engaging in the process of research. While clearly relevant to experienced researchers, there are also lessons for novice researchers. From the outset of your research career, consider thinking about developing your own program of research, not just a single research project in complete isolation. For some researchers, their PhD thesis develops into a program of research that defines their career. For others, they might develop all the basic research skills during their PhD, but it is their subsequent work on a different topic area that becomes the theme of their program of research. Either way, maintain high-levels of curiosity in your own research, question your results and approach – you might be surprised what you can learn from reflecting on your own work, even at an early stage.

It is also clear that good research ideas come from skilled researchers. We see this when an author becomes known in the field for an increasing number of excellent papers reporting outstanding research findings. These skilled researchers are not only knowledgeable in the field, but they are also insightful, using intuition to guide their decisions on experimentation, and they are active and focused on project outcomes. A novice researcher who has not yet established a personal program of research may benefit from joining an existing skilled team. Alternatively the novice researcher could work with an established researcher to reverse-engineer a pitch for a leading academic paper; Faff et al. (2016) document an exemplar of this process in finance working with a visiting ECR to develop their idea-generation skills. Engagement in the research process, and access to skilled researchers, can also be achieved through conference participation attendance and discussions with experienced research colleagues. Exposure to the work of others, seeing what work others are doing, and what techniques they are using, is a common source of research ideas. Aspiring finance professors might also learn from the documented career insights of professors in the field such as Edmans (2022).

The brain innovates in interesting ways. Stokes (2013) explores innovation in accounting research and advocates the value of finding the intersection of our personal research passion, interest from top journals and the skills we have. Much can also be learned from the science of research studying creativity; importantly, creativity is a skill and as such can be improved. One study in that area is by Fletcher and Benveniste (2022) who describe a new way to train creativity using narrative theory – it is also noteworthy that they do an excellent job of articulating the way their novel approach fills a gap in the prior literature. A simple tip based on the experience of the authors of this paper is to take periods of rest from thinking about research to allow fresh ideas to emerge. Anecdotally, many researchers admit to the idea-in-the-shower phenomenon in which they think of a good research idea while not consciously trying. In our experience, downtime for exercise, music, movies and the like, can be the time when complex issues gain clarity, and new research ideas emerge.

A word of caution at this point. Many novice researchers can spend excessive time on the take-off phase looking for a perfect research idea before commencing. A research career will not hinge on having a shelf full of unactioned great research ideas. Any researcher knows that projects evolve over their duration. This is why the advice "just write and write often" is so useful. The hardest thing to edit is a blank page. Failure to launch while thinking can be a huge impediment to developing as a researcher. One can't progress to refining ideas (discussed in Section 4) unless there are ideas written down to refine. For those prone to getting stuck in the take-off phase, consider applying the lean entrepreneurial start up model to research ideas (see Still (2017) as an exemplar). With today's tools, the lean research start-up model as shown in Fig. 4 can be stepped through in a matter of hours to determine whether a project is viable. First, the research idea could come from any of the processes discussed thus far. Then using a search tool such as Google Scholar see if the idea has already been addressed and if not, identify the top 1–3 studies. Is the typical methodology in these papers within your skillset? If so, the next step is to determine whether you can get access to suitable data or collect it yourself. The last step is to decide whether a result would be publishable. How might the project contribute to the target journal(s)? Ideally with a co-researcher or mentor, this lean start-up model can be completed in a few hours. Successful research start-ups can begin and be more rigorously refined (see Section 4) along the way. To maintain progress, we recommend committing to regular meetings or milestones, and targeting a conference, presentation or special issue with a fixed submission date.

Overall, engagement in the research process is critical for building a research career. Ideas are fuelled by one's own curiosity, by critical analysis and questioning of knowledge or interpretations. Good research ideas also come from a close knowledge of the field, what is published and what work is in progress by others in the field. This helps in identifying the missing puzzle pieces, the research gaps in Fig. 2 that need to be filled by new projects.

3. Learning from an experienced researcher exemplar

Novice researchers can benefit greatly from looking to research mentors and exemplar careers, and thinking about how others became successful researchers. We document the reflection of an experienced researcher as an exemplar of the journey research careers can take, and note some key success traits in generating new research ideas.

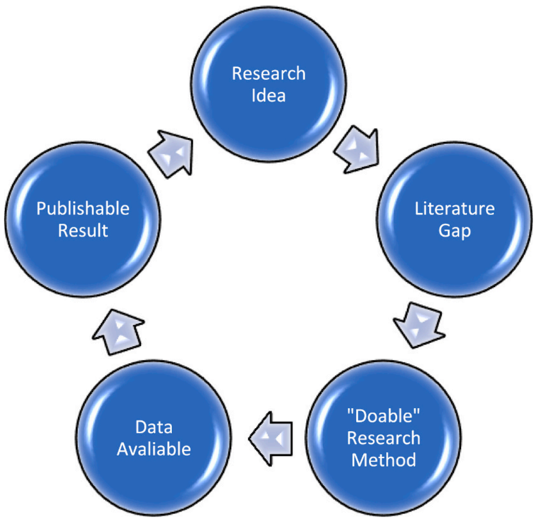

Fig. 4. Lean research start-up.

We start by working backwards from the list of journal publications – our research goal from the start of this paper. From this we can establish the theme of work over the last two decades and distil lessons for research idea generation. Sticking to the often-quoted statement by TQM pioneer, W. Edwards Deming, that "without data you are just another person with an opinion", we take to Google Scholar. From this we establish that 18 of the researcher's publications are cited more than 100 times, which suggests that the research ideas are good, but there is no information on how they were generated. As we look further, we discover that 14 of the 18 papers were presented at conferences. Further, it took on average four years from conference presentation to high-quality journal publication. Considering the review process in the field, this tells us the papers first went under review at a journal approximately 12 months after being presented at a conference – enough time to allow for meaningful contemplation of critical feedback before implementing improvements.

Undertaking this exercise enabled the exemplar researcher to reflect on many of these projects. More specifically, it reinforced that any success as a researcher, which assumedly means involvement in projects grounded in good research ideas, can be attributed to understanding early that a successful social scientist, as well as having a good understanding of methodologies, processes and writing to academic audiences, is also part detective and part entrepreneur. While this may not be widely appreciated, the exemplar researcher believes that their understanding of the entrepreneurial process and exposure to entrepreneurship as a manager and business owner prior to their academic career, laid the foundation for being able to survive and thrive as a researcher. This reinforces the mantra adopted by colleague and renowned scholar Professor Tom Lumpkin, who said that "you don't have to be an entrepreneur to be entrepreneurial."

As far as the detective analogy goes, the exemplar researcher is of a generation that has affection for the 1970s television character Colombo, played by Peter Falk.[2] Colombo was unorthodox and consummately, even annoyingly, inquisitive. These are characteristics of a social scientist, and our exemplar researcher. If we focus on a couple of publications, we discover deeper insights as to where the ideas came from. Three high-quality publications about family business have resulted from the researcher asking "I wonder if anyone has studied that?" when they saw a bakery truck on the highway promoting the slogan "from our family to yours." This fits with the notions of taking a break from academic thinking and that industry can sometimes be the fuel for new research ideas. Another research idea, which has resulted in two publications so far, came from pursuing a viewpoint contrary to an emerging theoretical perspective in the field. This reinforces the importance of curiosity and critical analysis, and that a different theoretical lens can address a gap in the existing literature. One of the researcher's most rewarding, and most difficult, research resulted from doggedly pursuing a project in which they measured concepts canvassed in a conceptual paper in a top-tier journal. This contributed to a measurement gap in the literature.

Finally, the exemplar researcher suggests that a contributing factor to success can be linked to the ability to work with colleagues on a tenure-track in the American system. The reason cited is learning from these colleagues who had access to better research training than provided in a small regional institution in Australasia. The researcher's inner *detective* and inner *entrepreneur* were necessary, but not sufficient, to get their ideas into decent publications. That process also required building a network of like-minded, similarly focused, but better trained, collaborators who have subsequently become friends. This reinforces the earlier-stated benefits of engagement in the research process and collaboration. The exemplar cited conferences, research seminars, talking with visiting academics and being a visiting academic as leading to collaborative projects. For further lived experiences see Walker (2019) for a multi-dimensional perspective on research impact and ECRs.

The next section considers enhancing already-generated ideas, and for this the exemplar researcher spends a lot of time in front of a whiteboard discussing conceptual maps of research ideas with many boxes and arrows being drawn and moved numerous times. In their experience, the whiteboard model has been extremely beneficial to some researchers. In fact, many breakthrough moments have come late in the day sitting talking around a whiteboard model of the concepts. Their research students also confirm that something big often happens around 5:00 p.m. after a day of seemingly little progress in front of the whiteboard. Intuition first requires engagement in the research process and in this case the whiteboard model is a common base from which conversations and contemplation can follow to refine research ideas.

4. How to refine research ideas to make them better

Once a potential research idea is identified, more work needs to be done to improve it and in doing so increase the likelihood of high-quality publication at the end. Feedback is crucial in this process, specifically critical feedback. Seek out those who will provide *tough love*; Edmans' (2022) section on "collaborating to create knowledge" is worth reading. Think of presenting research as an invitation to a deeper conversation with those interested in the topic. Consider delivering a seminar – be proactive and seek such opportunities. Additionally, ask your trusted network of researchers for feedback – and this reinforces the importance of developing such a network. For research students still growing their network, this is where your supervisors come in. It is vital for novice researchers to seek feedback to refine their ideas, but they also need to be conscious of the busy schedule and limited time available from experienced researchers. That is, efficient communication of research ideas is key. Using a whiteboard model as discussed above can be very useful in this way. There are also research templates and tools available that are designed to efficiently communicate research in written form (Faff, 2021a; Faff, 2015; Lodhia, 2019), oral presentation (Faff, 2023b) or visualisation (Faff and Kernbach, 2021). Researchers, particularly novice ones, have found such templates to promote effective communication and clarity (Bui, 2021), which

[2] See https://en.wikipedia.org/wiki/Columbo

helps obtain critical feedback.

A formal approach for latter-stage idea refinement is to present it at academic conferences or formal seminars at other institutions. Informal discussion at such events can be as valuable, or more valuable, than the presentation itself, although this additional value is questionable for virtual events. This does raise another question about what conferences or institutions to attend. Once again, we suggest looking to literature reviews that highlight the key studies and identify the corresponding authors, their institutions and conferences commonly attended. These are the researchers and institutions you want to spend time with. Feed these names and locations into your conference strategy. Attending their sessions is a good way to make contact. You might be surprised how often these other researchers are interested in hearing your ideas and providing feedback.

For experimental researchers, the research experiments can be part of the feedback process. Refinement of a research idea may require iterative experimentation, the design of distinct but supportive experiments, or the ability to get an outcome by deduction, such as by refuting the alternative hypothesis. Again, this requires engagement in the process of research and having belief that the process will lead to an outcome, albeit that outcome is currently unknown.

Additionally, when refining and enhancing research ideas based on feedback, it is important to consider your own, or your team's, research skills as recommended by Stokes (2013). This does not mean that we ignore constructive feedback if it is difficult to implement, but it does mean identifying any skills gaps and planning to upskill or recruit additional researchers accordingly. It is also common that filling these skills gaps will result in further refinement of the research idea based on the additional expertise.

5. How to evaluate and rank research ideas

Fig. 5 summarises some key elements of the third and final stage of the Research Idea Formulation process.

Unashamedly adopt a strict quality filter that retains only good research; research time is a scarce and valuable resource and it should be allocated accordingly. Remember to target high-quality publication outlets and do not regress to a lower mean. Implement what is known as Pasteur's Quadrant (Tushman and O'Reilly, 2007); this ensures that our work is *rigorous* and *relevant* by contributing to fundamental understanding as well as considering how the findings will be used. If you heed the network-building advice stated earlier, then your network of trusted researchers is well placed to help ensure your work rigorously contributes to fundamental understanding, while your industry-based network is well suited to ensure the research findings will be relevant. Additionally, get a quality mentor – every researcher can benefit from a mentor; they are a special trusted member of your network. Sands et al. (1991) provides a useful empirical study on the various roles senior mentors play in the careers of junior faculty. Reverse pitch mentoring is an exemplar of a practical activity that might help develop research skills. Overall, much of the advice for refining ideas applies to the evaluation stage, but the goal now shifts from finding ways to improve the research idea to determining whether the refined research idea is worthy of investigation now, later, or never. Even if we determine that the idea is unworthy, it is good practice to keep our notes as the thoughts involved might help with a different topic in the future; it is also possible that new information might change our

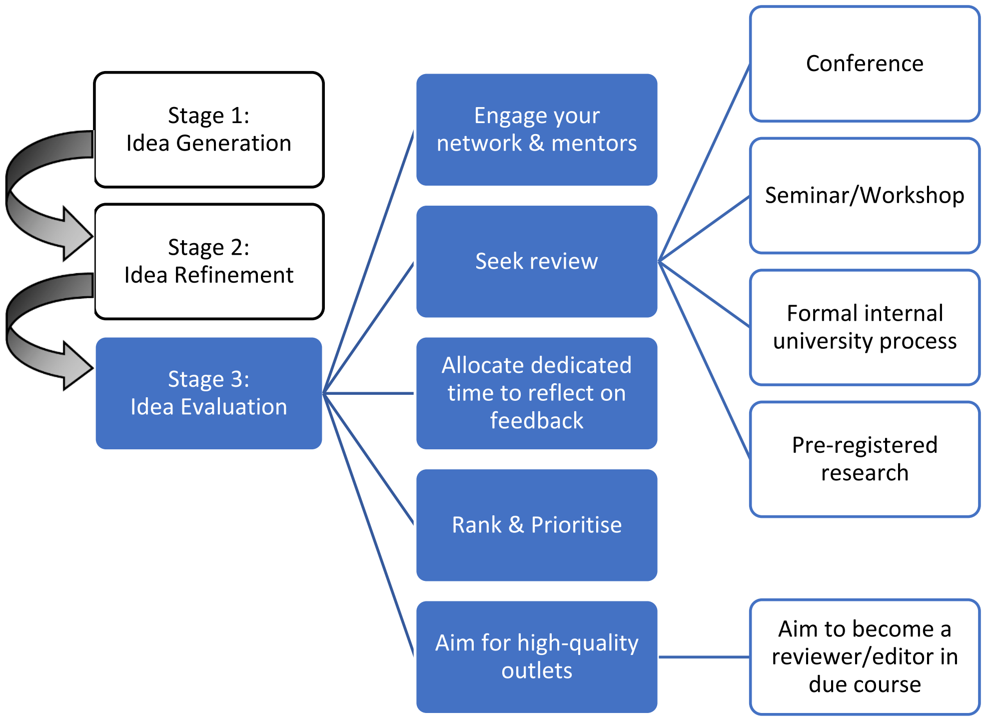

Fig. 5. Evaluation Stage of the Research Idea Formation process.

evaluation in the future.

The field of science supports peer review as the gold standard for review of research ideas and outcomes. However, it is also important to evaluate ideas before reviewer assessment since by then a lot of time and effort has been allocated. Conferences, workshops, and seminars are great platforms as already discussed. Even earlier than this, scientists regularly evaluate ideas through rigorous discussion of their ideas with peers and colleagues, ranging from local area to overseas, often by leveraging the online connected world. While online conversations are useful at expanding the pool of potential feedback, the value of in-person conversations should not be overlooked. This might simply be informal conversations with co-authors or other trusted colleagues, or setting meetings with a mentor or other member of our network specifically to evaluate one or more research ideas. We also need to allocate time without distraction to reflect on the feedback and ultimately come to an evaluation; we strongly recommend this is dedicated time rather than moments in between replying to emails, for example. In addition, the recent development of a pre-registered research pathway also offers an exciting new way to receive independent peer-review on a research idea and proposed methodology before conducting the research; Faff (2023a) explains the initiative. The first published example is on operational risk management in financial institutions: initially Cornwell et al. (2023a) published the peer-reviewed research idea and methodology, before completing the research and publishing the results and conclusions in a latter paper (Cornwell et al., 2023b).

There may also be formal internal research idea evaluation processes that can be joined at your university. In fact, for projects linked with grant applications, many universities will require an interval evaluation process before they are externally evaluated via the competitive grant process. As is the case with acceptances in top journals, please note that large grants are not awarded for simple extension work or investigations for which the answer is obvious. The idea needs to be particularly interesting and novel, and perhaps even establish a new paradigm. Regardless of the evaluation method, each research idea should be scrutinised for novelty, inventiveness, rigor, and relevance.

If multiple research ideas progress past the evaluation hurdle, then we might need to rank them for prioritising our time. From the experience of the authors, the most important research ideas that should be tackled first are those with long-term value, for which we have the skills and resources (tools and data) to proceed now (or very soon), and for which the time for completion is evident and acceptable. This approach ensures that outcomes of the work will be achievable, timely, and of importance for publication. Being timely matters, because we are not the only researchers in the field interested in the topic. So, as with any entrepreneurial pursuit the best first-to-market paper is going to be the benchmark. The exemplar research career discussed earlier highlighted the substantial time to publication and thus it is important to keep projects rolling to completion. Projects plural is noteworthy, because choosing to be involved in multiple projects simultaneously has advantages if one of the projects hits a temporary or permanent barrier. However, novice researchers also need to be careful not to spread themselves too thin with involvement in too many projects – mentors and your research network can help in finding the right balance for you as there is no one-size-fits-all recommendation. It is also noteworthy that if the same research template is used to plan multiple projects then the ranking process might be easier because Bui (2021) found use of the Pitching Research template (Faff, 2021a; Faff, 2015) promotes comparability between research ideas.

We improve at evaluating and ranking research ideas by helping others do the same. Be active participants in conversations and presentations about ideas from other researchers. ECRs should also seek to become an active reviewer. The exemplar researcher discussed earlier has spent numerous years as an Associate Editor in a leading journal. This is one way to stay at the vanguard of the thinking in the domain, improving both the breadth and the depth of understanding. Being a guest editor of special issues is a path some take towards becoming a full-time editor.

6. Key themes on research ideas

The strategy of using published review papers as a source of both summaries of existing knowledge and potential future directions is a useful starting point to identify research gaps. We discussed five perspectives for investigating the existing literature for gaps that are worthy of being addressed by new research ideas.

1. Look for potential new theoretical contributions, which may include taking a multi-theoretic perspective or applying a different theoretical lens to a research problem.
2. Consider how the theoretical constructs have been measured and the data used to empirically test the relationships between the measures, and thus infer properties of the constructs. Evaluating and improving proxy measures is one way to extend current understanding, as is providing new insights by using data from a relevant novel context.
3. Critically assess current research methodologies and consider whether an alternative approach would provide new insights or help to integrate a range of prior evidence. For example, a field relying on archival data might benefit from survey, interview or experimental work for insights that help to triangulate our understanding of the focal phenomenon from multiple sources.
4. Advances in analytical techniques can facilitate enlightening research insights. Research progress is always limited by technology. Consequently, keeping current with technology trends, either yourself or via your team, can help identify ideas that are now actionable because of technological advances such as big data analytic techniques.
5. Another fruitful approach for generating new and interesting research ideas is by consulting industry – specifically looking for questions without answers either in practice or research. This includes considering published research priorities from government agencies, professional bodies, and other research funders.

Simply put, engage with the research process, because research generates new research ideas. Sometimes the new ideas might be generated systematically, but sometimes the idea will seemingly come by accident while talking to a colleague, reading a research paper, being at a conference, or just watching a favourite sporting team. However, such inspiration is generally on the back of engaging in the research process and knowing enough about a domain that the brain has the inputs to fire and generate a new idea. So, while as a novice researcher you will not yet have a portfolio of ongoing research projects, you can read published research and engage with other researchers as part of your preparation for inspiration. It is beneficial to go both deep into the literature specific to your field, as well as broad in search of lessons from other fields that might be both applicable and novel in your field. Just as entrepreneurs identify new business opportunities by noticing connections between seemingly unrelated events or trends in the world (Baron, 2006), when broadly educating yourself you are looking to connect the dots between multiple pieces of information. Examples of this process include integrating a theory into your field that has been developed in another field (perspective #1) or matching an open question from practitioners with an academic theory that needs to be empirically tested (perspective #5) and might require a modern flexible data analytic technique to address the complex non-linear theoretical relationship (perspective #4).

In addition to engaging deeply and broadly with existing research, the following recommendations are emphasised in this paper.

- Generating innovative research ideas is a creative skill that can be improved. Not all ideas generated will successfully progress through the refinement, evaluation, and prioritisation stages. It is natural that not all ideas will be undertaken; there is risk involved and we need to be willing to fail sometimes. Creative work always comes from the individual, while translational work, or innovation with the development of an existing idea, requires collaboration.
- ECRs should remember the essential importance of being distinctive, or carving out a niche. Take a long-term view, particularly when evaluating and prioritising ideas. From the start of your research career, we advocate having a plan to develop a program of research, not just a project, even if the research focus is currently still vague. Such a long-term view necessitates prioritising quality over expediency.
- Establish academic and industry networks, including at least one mentor. From these networks and beyond, have conversations, seek critical constructive feedback, and collaborate. We do not have to agree with all feedback we receive, but rather carefully and thoroughly reflect on it, and be able to clearly articulate the reasoning if we disagree – something we might need to do if a reviewer shares this opinion.
- Improve your skills by helping others to refine, evaluate and prioritise their research ideas. Be active at seminars, volunteer as a discussant at conferences, and aim to take on reviewer and then editorial roles in due course.

We started this paper talking about the publication objective. Do not become a researcher with lots of great projects sitting on the shelf unpublished. As a responsible scientist (Faff, 2021b), one has a responsibility to report findings, particularly when they are funded by the public or donated money. As there is no such thing as the perfect research idea or the perfect completed research project, one needs to publish at a point where it is possible to write a worthwhile story that will contribute to the advancement of the field. While writing a research article is not the focus on this paper, we nevertheless make two important recommendations. First, do not fix on writing research as originally planned; it must be written consistent with the findings from the research. And secondly, remember the golden rule that we all forget at some point, we are writing for the reader.

Researchers contribute new insights to the body of existing knowledge known to humanity. We should not forget that it is a privilege to be involved in such an important endeavour, but successes are earned and do not come easily. Consequently, it is important to own and celebrate successes when they come; do not fall into the imposter syndrome trap – read Bothello and Roulet's (2019) paper instead. Conducting novel research, and developing research ideas in particular, is difficult, and challenges are to be expected along the way, but the rewards are worth it, in the opinions of this paper's authors. Use the strategies in this paper (and from other sources such as Brennan's (2019) 100 rules for research) to generate, refine and evaluate research ideas to ensure our efforts are focused on high quality research that is both rigorous and relevant, and consequently ends up being published.

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