Boundary research: Tools and rules to impact emerging fields

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ABSTRACT

Boundary research can be risky, but it can also move academic disciplines into wider areas of influence. To help reduce the risk and increase the reward, this article describes new tools that boundary researchers can use to get started, published, and promoted. These include writing for surprising impact, positioning their research against a larger theme, developing a research impact matrix for promotion, and estimating a 10-year citation record. These tools can help move a boundary research area from fringe to focus. © 2016 The Authors. Journal of Consumer Behaviour published by John Wiley & Sons, Ltd.

INTRODUCTION

Most traditional research in academic disciplines is centrist. It focuses on research questions that are central to the evolution of a field. In the behavioral sciences, research areas such as attribution theory, behavioral decision theory, diffusion theory, and regulatory focus are just a few centrist topics that spawned hundreds of field-changing dissertations and articles. Yet not all started out as centrist. Many started on the periphery or boundary of what was then fashionable in their fields. Over time, however, they moved from idiosyncratic to impactful, from fringe to focus.

Boundary research exists on the fringes of all fields and sometimes ends up pushing these fields forward in spite of great resistance (such as evolutionary psychology – Buss, 1999), initial dismissal (such as behavioral economics – Myagkov and Plott, 1997), or general indifference (such as eating behavior – Rozin, 2007). Yet because such research often operates on blurred interdisciplinary lines, reviewers sometimes try and force it into a more centrist position, or they sometimes reject it as being a poor "fit" or "contribution" to the field even if they eventually end up being a classic (such as Griffin, 1997). As one boundary researcher (and former editor) wrote,

> I have found in my own research that my most innovative articles have had the toughest time in the review process. Those same articles, once published, have had the greatest impact (Rust, 2006).

In light of this, how does one persist with an innovative article so that "once published, it has the greatest impact"? By building on the professional personal experiences of successful scholars in a variety of social science disciplines,

this article offers tools that boundary researchers can use to help them move from being tentative to tenacious. It addresses the following issues:

- How many studies are enough?
- Writing for surprising impact
- Positioning alongside a larger theme
- Developing a research impact matrix for promotion
- Using the 3-year citation test to estimate a 10-year citation record
- Getting started in boundary research
- Avoiding career regrets

This begins with the encouraging history of how boundary research has broadened the impact of academia, and how it evolves from making more empirical contributions to making more theoretical contributions as it matures. We then describe specific tools boundary researchers can use to get started, published, promoted, and avoid career regret.

THE IMPORTANCE OF BOUNDARY RESEARCH

Boundary research is peripheral to the general focus of an academic field. Unlike a topic that is of narrow interest but has always been viewed as relevant to a field (such as scarcity theory in sociology, the Delboeuf illusion in psychology, or impulse buying in marketing), boundary research might appear to be tangential – maybe even unrelated – to the field of focus. It can be seen as too peripheral because it operates at the boundary of other disciplines (such as developmental psychology and music – Holbrook and Schindler, 1989, or anthropology and leisure studies – Arnould and Price, 1993). In other cases, it is peripheral because it is viewed as too applied. Such was the case with service quality, where the initial research in this area was widely rejected (Parasuraman *et al.,* 1988) but has now been cited over 20,000 times and helped move service-quality research from fringe to focus within only 5 years of its initial publication (Zeithaml, 2016).

Boundary research differs from transformative consumer research (Viswanathan, 2011; Mick, 2005; Mick *et al.,* 2012)

in that it is not necessarily aimed at improving social welfare, and it differs from activism research (Wansink, 2012, 2015) in that it does not necessarily need to be translational, practical, or application focused. Within its field, it might adopt or introduce new topics (behavioral economics), new perspectives (post-positivist explanations), new contexts (eating behaviors), or new methods (content analysis).

How boundary research evolves

Centrist research often examines a paradigm or general theory by identifying and testing moderating influences and possible mechanisms. In contrast, boundary research is often more exploratory. In the social sciences, boundary research seems to evolve in similar ways (e.g., Hanson, 1958; Kuhn, 1962). Scholars in a new area first define what they see (concepts), and they then organize these concepts into taxonomies (Figure 1). They next identify correlations between these concepts, test causality, and develop larger theories or paradigms.

The first stage of a field's development involves defining new concepts (e.g., reference points, health halos, experiential consumption, and market orientation). If the limits of our language are the limits of our world (Wittgenstein, 1922), developing new words for new concepts can provide a more useful tool for advanced or nuanced thinking. The second stage of a field's evolution involves usefully organizing the field into a taxonomy by distinguishing between different concepts, contexts (e.g., private versus public behavior), or outcomes (e.g., reaction time, attitude, behavioral intention, or consumption). Two illustrations of this are the recent taxonomies developed on identity-based consumer behavior (Reed *et al.*, 2012) and on package-based influences on overeating (Chandon, 2013).

After useful taxonomies and distinctions are created, the third stage of evolution is to discover how these concepts are related by searching for correlations. This can be carried out by using observational analyses, surveys, or existing databases. With enough correlational evidence, the fourth stage uses lab studies or field studies to test for causality. Given enough evidence of causal relationships, the fifth and final stage involves developing theories (and possibly even an eventual paradigm). While centrist research operates in the fourth or fifth stage of this evolutionary development in Figure 1, boundary research usually makes initial contributions in the first three stages.

This predictable evolution of science suggests why new subfields – like new seedling trees in a crowded forest – often struggle. Because they are still developing new definitions, distinctions, and taxonomies, they cannot stand up to a toe-to-toe comparison from reviewers and editors who are more familiar and comfortable with established centrist topic areas. Boundary research is at a different stage of development. Because of the struggle and the risk, young scholars are usually

advised to steer clear of boundary research by established scholars who do the same (with fortunate exceptions such as Belk *et al.*, 1989; Rust *et al.*, 2004; Mick *et al.*, 2012).

Because of the importance of boundary research to the development and impact of a field, some proponents might say that boundary research should be nurtured and perhaps the standards should be lowered in the review process. This would be wrong. Such affirmative editorial actions for boundary research would marginalize legitimate contributions of this work among those who saw it as being given an unfair advantage. There are other solutions for reviewers, editors, and researchers.

For reviewers, perhaps boundary research can be reviewed differently. Although it has a less established theoretical basis, it is not less rigorous. As will be seen in the next two sections, this often magnifies the importance of its empirical contribution.

For editors, the review process often forces boundary papers to become more centrist – often causing them to lose what makes them unique. One solution would be to assign one reviewer whose main task is to focus on the boundary nature of the research. That is, the editor would need to clearly frame that reviewer's task as being non-centrist. This might improve a manuscript's chance of publication and impact without sacrificing its quality or its voice.

For researchers, knowing how boundary research can predictably evolve will call for more patience, persistent, and careful replication (Meyer, 2015). Moreover, knowing how it will be evaluated by reviewers and knowing what can and cannot be controlled can help minimize wasted effort and disappointment.

Raising our expectations for boundary research

Although the progress of an area of boundary research will generally evolve from empirical discoveries to larger general theories, some of the early contributions may be smaller or stylized theories. In helping boundary researchers better navigate a path toward impact, let us roughly categorize articles (the word "articles" will also refer to unpublished manuscripts) into two oversimplified categories: (i) articles that make more of a theoretical contribution (even though they involve empirical studies) or (ii) articles that make more of an empirical contribution (even though they may also offer a theory).

With theoretical contributions, "there is nothing more practical than a good theory" (Ray, 1982). Unfortunately, it has been lamented that many theories in the social sciences are often demonstrated in only narrow or stylized situations. This may be why some psychology theories have been difficult to replicate, with some reported attempts showing 40 per cent consistency (Hennessey, 2015). Yet for a theory
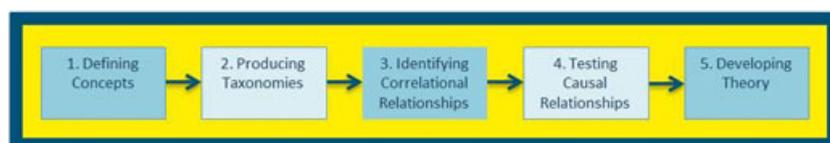

Figure 1. The research evolution process in boundary research. This figure is available in colour online at wileyonlinelibrary.com/journal/cb

to be both good *and* practical, it should be generalizable across contexts and should consistently predict and explain behavior in a variety of situations (Lynch *et al.*, 2015).

With empirical contributions, robust findings can lead to confident changes in lives and in society. Showing that the same result is consistently found across a wide number of contexts and populations – that it is found in lab situations, field situations, and observational studies – gives us confidence that this "finding is a fact." Five MTurk studies or three in-class 101 studies can show replication, but not rigor. Consider how pantry stockpiling might influence one's consumption rate of a food. An empirical contribution could involve a lab study that measures consumption likelihood, a store intercept study that measures consumption intentions, an in-home field study that measures reported consumption, and a scanner data study that infers consumption from repurchase.

Ideally, (Chandon and Wansink, 2002) all of our research would be both highly theoretically rigorous and highly empirically rigorous, and it would sit in the top right corner of Figure 2. Unfortunately, almost every published paper in even the best journals comes up short on one dimension or another. An article with a useful theory might have less convincing or generalizable empirical results, or an article that shows compelling results might only offer a plausible explanation for them.

For boundary research to be published in top journals, it needs to solidly be in the shaded areas of Figure 2. Either boundary research needs to have the empirical rigor that makes one confident of the robust findings (if not the mechanism), or it needs to have the theoretical rigor to make one confident of the mechanism (if not its general relevance).

How many studies are enough?
If even the best papers in the best journals come up a little bit short on either the theoretical or empirical dimension, how do you know if you need to add another study or redo the current one? When balancing the trade-offs between theoretical rigor and empirical rigor, editors and reviewers have their individual standards. Yet how can a researcher know when "enough is enough" and whether conducting an extra
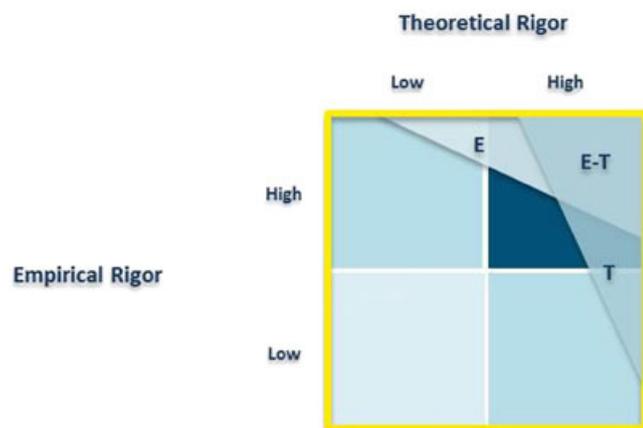
study would be worth it or a waste. The answer is different for centrist researchers than it is for boundary researchers. For centrist researchers, a top-down approach – theory to empirics – is efficient. Knowing the literature, the positioning, and the projected contribution can save the time and effort of running an unneeded study. While "Theory is King" with centrist research, the opposite might often be true with boundary research. If there is not much known about a field, it will probably need to evolve as illustrated in Figure 1. Initial contributions will define new concepts and offer new taxonomies, and empirical findings – correlational and then causal – will follow (Hardie *et al.*, 1993). Only after a variety of initial findings are discovered can they then be pieced together into useful larger theories.

The quality of the theory generally determines the quality of the journal that publishes it (e.g., Alba and Hutchinson, 1987; Kohli and Jaworski, 1990). Useful, robust theories are published in top general journals (e.g., *Science*) or in top discipline journals (e.g., the *Journal of Personality and Social Psychology* or the *Journal of Marketing Research*). More narrow theories tend to be published in top sub-field journals, and less robust theories tend to be published in specialty journals or lower tier journals.

While narrow theoretical articles are published in smaller (less prestigious) journals because they are of less general appeal, empirical articles are published in smaller (less prestigious) journals because they have a limitation. They might be marginally significant and poorly measured, or they might have multiple explanations, but unless there was a flaw in the study, they can always be published if one is persistent enough.

Back when journal articles were only available only in print, the quality of the journal largely determined the impact an article had. A larger number of influential people read higher quality journals, whereas lower quality journals or very specialized journals often had fewer readers.

The web has democratized the impact of research. Open-access journals (e.g., *PLOS One* and *BMC Public Health*) or working paper aggregators (e.g., the Social Science Research Network and ResearchGate) have broadened the reach of research both within fields and across fields. Searching for "social facilitation and habits" gives 47,600 results across dozens of fields: psychology, zoology, physiology, education, communication economics, sociology, family medicine, animal behavior, music education, biology, nursing, appetite, motor behavior, and so on.

With the democratization of research, the more useful the discovery, the more impactful it can become. A great theory article in a top journal might be even more influential across fields than it otherwise would have been, but a more specialized or stylized article will benefit less. One hypothesis (H_1) is that the impact of a theory might drop precipitously with the quality of the journal in which it was published (because "journal quality" may be a signal of "general usefulness").

In contrast, a specific empirical finding can be helpful in supporting a theory in a totally different field – even if only correlational or with multiple explanations. As a result, empirical articles can be useful to others with less regard to

Figure 2. Theory versus empirical trade-offs in leading journals. This figure is available in colour online at wileyonlinelibrary.com/journal/cb

where they were published (H$_2$). For example, early research on the influence of the image of a product's region of origin (Parma ham, Champagne) on consumer product evaluation was widely cited in the food sciences, (agricultural) economics, nutrition dietetics, soil science, environmental studies, and urban studies. A study powerfully demonstrating how descriptive food names bias sensory perceptions and sales in restaurants was cited in food science technology, nutrition dietetics, behavioral sciences, public environmental and occupational health, psychology experimental, psychology social, psychology biology, physiology, neurosciences, and agricultural economics policy. This basic hypothesized relation is illustrated in Figure 3.

If Figure 3 is correct, its implications range from being encouraging to inspiring. It is encouraging to theoretical boundary researchers because it shows how they can use findings from other fields to build or support of their own theory. It is inspiring to empirical boundary researchers because it shows how these basic empirical articles – even if not published in a top journal – can have a broad impact far outside of their home field (as long as it uses the most relevant key words and search terms). Like many social science hypotheses, this suggested relationship (H$_1$ and H$_2$) could be empirically tested across fields.

GETTING PUBLISHED

Because of the unusual contexts in which some boundary research is conducted – restaurants, websites, convenience stores, vacation resorts, homeless shelters, and so forth – it can often be unfairly characterized by its context. Part of this is the reader's fault because they fail to see that these are single illustrations within *larger themes*. Part of this is the researcher's fault because we fail to clearly state how our single illustration actually does fit within a larger theme. Because the connection is not made with the larger theme, readers confuse the context versus the contribution. This raises two questions: How can boundary researchers write more clearly, and how can they better articulate the larger theme behind their work?
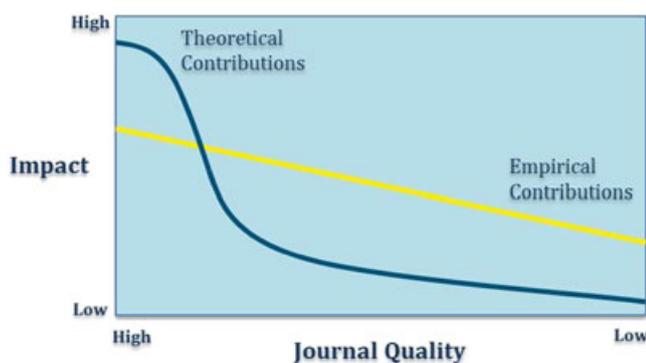

Figure 3. The hypothesized relative impact of theoretical and empirical contributions. This figure is available in colour online at wileyonlinelibrary.com/journal/cb

Writing for surprising academic impact

Advising a Ph.D. student to publish a rigorous theory with robust empirical results in the greatest journal in their field is no more helpful than advising a pole vaulter to go win a Gold Medal. A great goal requires a great plan and great execution.

Yet what is also needed is advice on how to write a more realistically publishable article that will still be useful or have an otherwise surprising impact. Articles in a boundary research area – because of its less defined pathway to success and still nascent appeal – might initially be mainly published in specialty journals or lower tier journals. If true, it would be useful to have advice on how to make these articles "sticky" (Huber, 2008) enough to have an impact wherever they are published.

For the past 10 years, one Advanced Consumer Research course has required its graduate students to personally interview admired researchers about their most *surprisingly* successful article. This interview was not to focus on the best article they ever published in their best journal. Instead, it was to focus on an article that was published in a less prestigious journal but which had a surprisingly successful impact on their career or on their field.

Of the 120 or so interviewed researchers, some offhandedly attributed the success of their article to the zeitgeist of the times or to their "instinct" of knowing what is important to their field. Others attributed the success to something external and uncontrollable, such as it being a lead article or winning an award.

But the more thoughtful researchers often talked about the *challenge* of the research question that led them to collect data in an unusual *context*, or to *communicate* it in a different way than they might otherwise have done. In short, they attributed their success to one or more of the following conditions (or 3Cs): (i) the challenge, (ii) the context, or (iii) the communication.

1. The challenge. Although it is obviously important to ask and answer an important research question, it was surprising that most of these questions did not originate in the literature of the field. Researchers often referred to a challenge that needed to be solved or "proven". The intent was sometimes to definitively make a statement (e.g., Young and Nestle, 2003) and not necessarily to investigate mediators or moderating conditions.

Many questions were actually divorced from the literature in the field. Instead, they originated in a conversation, a debate, a lawsuit, or as an assumption behind policy proposals. One successful boundary research approach has been to quickly answer difficult questions that follow new technologies, such as eye-tracking (Pieters and Wedel, 2004), GPS technology (Hui *et al.,* 2013), and smart shopping carts (Van Ittersum *et al.,* 2013). Another has been to explore more meaningful ways consumers can use technology, whether it be for entertainment (Brasel and Gips, 2011; Sciandra and Inman, 2014; Xu *et al.,* 2014) or meaningful and potentially powerful ways of using social media (Yoon *et al.,* 2006; Hennig-Thurau

et al., 2010; Schellekens *et al.,* 2010, 2013; Zhu *et al.,* 2012; Toubia *et al.,* 2014; Dahl *et al.,* 2015).

There are many valuable sources for groundbreaking research ideas. A casual conversation about closed-loop supply chains resulted in research demonstrating that deterring third-party competition via preemptive remanufacturing may actually reduce profits (Agrawal *et al.,* 2016). Economic litigation cases in the domain of copycatting triggered research to detect visual copycat brands (Satomura *et al.,* 2014). Lawsuits against weight discrimination lead to research on how the weight of waiters influenced how much people ordered and drank in chain restaurants (Doering *et al.,* 2016). Proposed cuts in sports program funding lead to research on the long-term correlation of sports participation and corporate leadership compared with other activities, such as being in student council or band (Kniffin *et al.,* 2015).

2. The context. Specific people and specific places can bring research to life and make it notable and quotable. Some of these papers studied a population that was generally interesting because of their profession or past. These groups have included burglars, World War II combat veterans, triathlon athletes, Thai prostitutes, jazz musicians, and chronic shoplifters. Other populations were notable because they were an expert population that should *not* have been influenced by what was under investigation (judges being biased by the time of day of a trial, police officers being influenced by car colors, nutrition experts being biased by the size of ice cream bowls, or bartenders being influenced by the shape of barware).

Evocative locations made for surprisingly impactful research as well as frequently encountered locations. Many natural behaviors in natural locations outside of a lab were often surprisingly memorable. This included extreme eating behavior at State Fairs, shoplifting at Wal-Mart, closing time behavior at bars, bullying on playgrounds, pouring cough medicine, consuming chicken wings during a Super Bowl party, and bargaining behavior at garage sales. Although the points made in the articles were often fairly generalizable, they were all examined in a colorful, evocative, memorable context.

3. The communication. Surprisingly impactful articles were often written differently than an author's typical articles. In some cases, they were written in a more succinct or pointed way. One theoretical article with two studies was rejected 14 times in psychology and marketing journals. Eventually, it was edited into a one-study paper that made one simple point, and it was accepted shortly thereafter by a leading medical journal. Similarly, another article was written as a review for a first year Ph.D. seminar. Because it was straightforwardly written and offered simple propositions, when it was eventually submitted to a journal and published, it greatly appealed to non-experts wanting to better understand the field.

Some articles became impactful because of their figures or tables. Other academics copied their simple and useful figures into lecture slides, modified them for other articles, or reprinted them in books. Tables that summarized literature or gave examples of implications stimulated new uses for the article's theory or findings. In other cases, the ideas and illustrations in these tables were specific and generous enough to serve as an effective call for future related research.

Again, the best guarantee of research impact is to publish a rigorous theory with robust empirical results in the best journal in a field. Yet most boundary researchers will probably not progress and be promoted by waiting for this perfect convergence (Figure 2). Yet these 3Cs might provide a solidly deserving finding or theory the additional boost it needs to be surprisingly impactful even if published in a lower profile journal.

What's the *larger theme*?

Each October, the Royal Swedish Academy presents five Nobel Prizes for transforming research in five fields. Each September, the *Journal of Irreproducible Results* presents 10 Ig Nobel Prizes in 10 fields for "research that first gets you to laugh and then to think." It is a 3-day media circus of sold-out presentations at Harvard and MIT and a series of receptions, dinners, and parties.

The Ig Nobel prizes are research at its funniest. Some of these Ig Nobel prize-winning articles are truly ridiculous: a medical journal article about the types of injuries sustained by circus sword swallowers or a food science article on making ice cream out of cow dung. Yet while many others seem equally ridiculous – mathematically modeling wrinkle patterns on sheets or training pigeons to differentiate between paintings of Picasso and Monet – they end up being widely cited, having over 100 citations within their first 10 years of publication (Waber *et al.,* 2008).

How does a widely cited, influential article become the punch line of a joke to the educated public? One explanation is that these Ig Nobel awards focus on the context of the research (wrinkly sheets) and not the larger theme of what the research represents (a demonstration of related topographical nonlinearities). For instance, one award winner showed that "bottomless" soup bowls caused people to overeat – making the point we eat with our eyes and not our stomach (Wansink *et al.,* 2005). Another showed that controlling a full bladder decreases impulsive decisions – making the point that inhibitory signals are not domain specific (Tuk *et al.,* 2011).

In short, each of these Ig Nobel award-winning articles provided a useful empirical insight that illuminates a larger theme. What is important about some of these articles – and why they had such a big impact on their field – is that their authors did not leave it to chance that readers would see these articles as illustrations of or contributions to a larger theme. They made that connection vivid and highly salient, such as magnifying the sound of a potato chip crunch to show how sound influences taste, thus illustrating the exaggerations

that occur with multi-sensory experiences (Zampini and Spence, 2003).

Many boundary research papers do not clearly articulate the larger theme to which they contribute. They position themselves in the context or as a mid-level theory. Yet more successful papers have taken a clear finding or theory and have shown how it relates to a larger theme that has implications for theory, application, or policy. To illustrate,

Table 1 shows how insightful recent publications in this journal reflect larger themes that could expand the value and use their findings.

"Larger themes" are not "future research." While most papers have "Future Research" sections, they usually read like afterthoughts. They languidly suggest there should be replications or extensions, or they make a weak, vague call for related research. These sections are seldom generous with

Table 1. Illustrations of research conclusions and their larger theme

Title	Research conclusions	Larger themes
"Influencing light versus heavy engagers of harmful behaviour to curb their habits through positive and negative ad imagery" (Burton *et al.,* 2015)	• Positive (gain-framed) messages reduce texting while driving and gambling intentions among heavy engagers • Heavy engagers resist negative (loss-framed) messages and find them uninvolving or not credible • Light engagers respond better to negative (loss-framed) messages	• Reactance to negative messages • Gain vs. loss seeking in public health
"The stability and sales contribution of heavy-buying households" (Romaniuk and Wight, 2015)	• 50% of heavy buyers of brands are not heavy buyers in a year, and 33% of them will not even be heavy category buyers in a year • Heavy buying is not stable and may not be easily targeted or worth the long-term effort	• Habit change • Intertemporal household purchase patterns
"Antecedents of young adults' materialistic values" (Grougiou and Moschis, 2015)	• Disruptive family incidents in early life impair socioeconomic status and self-esteem and correlate with greater materialism at later life stages • Low self-esteem alone has no effect • A childhood that emphasizes autonomy and individualism (over compliance) correlates with higher materialism in young adults	• Psychographic predictors of preference • Family materialism and expectations of children
"Foreign brands in local cultures: a socio-cultural perspective of postmodern brandscapes" (Al-Mutawa *et al.,* 2015)	• Social-cultural dynamics in Kuwait (religion and tradition) restrict sexual expression and dating • Muslim women use luxury brands to express their sexuality, value, and expectations in alternative ways	• Sexual expression in nonsexual ways • Personalizing the meaning of brands
Living with terrorism or withdrawing in terror: perceived control and consumer avoidance (Herzenstein *et al.,* 2015)	• Terrorism increases one's desire for control and can lead to avoidance behavior • When perception of control is low, preferences and consumption patterns change	• Coping with personal crises • Long-term effects of traumatic results
"Effects of consumer embarrassment on shopping basket size and value: A study of the millennial consumer" (Nichols *et al.,* 2015)	• Consumers "mask" to avoid shopping embarrassment • Masking leads to larger market baskets	• Consumption identity and impression management • Embarrassment avoidance strategies
"Children's consumption behavior in response to food product placements in movies" (Matthes and Naderer, 2015)	• Frequent placement of food in movies had no influence on children's attitudes toward the brand • Frequent placement did influence immediate consumption • There was no relation between attitude and behavior with 6–14-year-old children	• Predicting impulsivity • Marketing research and children
"Extraordinary consumer experiences: why immersion and transformation cause trouble" (Lindberg and Østergaard, 2015)	• Co-created extraordinary experiences (wilderness canoeing) may not be better than more ordinary experiences • Such experiences can contain unwanted paradoxes and ambiguity that exceed a consumers processing resources • Role conflicts may interfere with transformation and immersion	• Consumer self-regulation • Co-creation and enjoyment

ideas, and they seldom point at the larger theme to which their conclusions contribute.

As academics, we not only miss seeing the forest because we are staring at the trees. We often even miss seeing the trees because we are staring at their bark. Most of us never see the larger theme when we start a research study, and sometimes this only begins to emerge (if at all) after we have published a number of papers in that area.

How then can we challenge ourselves to see this larger theme? One way is to choose a target journal in your field and to imagine they were going to publish a special issue that would be the perfect fit for your specific paper. Next, write down the other topics of articles that you could also imagine being in that issue and that would compliment – but not du-plicate – your article. By doing this exercise, you can usually find the larger theme related to your work because (i) it will be directly reflected in the title of the special issue you imagined or (ii) it will help identify larger commonalities between the articles you imagine would compliment yours in that issue. Interestingly, a sizable number of high-profile journals happen to regularly have special issues on specific themes. Two examples are the recent special issue of the *Journal of Macromarketing* on "Subsistence Marketplaces" (Viswanathan *et al.,* 2014) as well as the special issue on "multi-channel retailing" of the *Journal of Retailing* (Verhoef *et al.,* 2015). Other journals – such as the *Journal of the Association of Consumer Research* – dedicate each issue to a specific theme (Van Ittersum and Wansink, 2016). Trying to imagine the types of similar papers that could synergistically be published in a single issue of a journal – and not be redun-dant – can help us visualize and articulate the larger theme of our contribution.

GETTING PROMOTED

When many academics around the world are evaluated for promotion, their promotion case will be in a gray zone of uncertainty that is in the middle between the two black and white extremes. At the one dark extreme, there is the researcher who did not have a viable case for promotion and who would have been already coached to leave the university or would have been given a terminal contract. At the bright other extreme, there is the rare researcher with an unambiguously strong case who has either left for a larger or stronger department or who had a competing offer. Those who remain in between these extremes are in the unsettling gray zone of tenure or promotion. This is the important step where one goes from job insecurity to security. At different universities in differ-ent countries, this comes in different forms – it can be in the form of a new title, and new position, a longer contract, or permanent employment.

For these researchers – particularly if they are boundary researchers – it is critical for them to (i) highlight their productivity and past impact and (ii) show their promise for future impact. While every researcher attempts to write a research statement for promotion that highlights their past pro-ductivity and future impact, they usually do so subjectively

and qualitatively. However, this research statement can also be written more precisely and less subjectively, and it can be more quantitatively compelling by using a research impact matrix and the 3-year citation test.

Developing a research impact matrix
Determining standards for promotion or tenure can be unpredictable when centrist researchers are in the gray zone, but it is even more volatile with boundary researchers. Boundary research records are often heterogeneous. Articles are published in different fields and in journals of varying quality; academic citations or media mentions can vary widely across some papers, and other papers can be completely ignored.

The contribution of boundary research is seldom clear, and senior researchers – committee members, reviewers, or voting faculty – might be unsure how to equate a two-page article in *Science*, a four-page article in *Pediatrics*, a quirky Brief Report in *Psychological Science*, an interestingly titled article in *Environment & Behavior*, and a methodological article in *Marketing Science*. It is like comparing apples, oranges, pineapples, kiwi, and watermelons – the articles are different lengths, from different fields, and with different purposes.

To prevent senior faculty from simply defaulting to "counting A-level publications," a set of comparable metrics is needed. A two-page article in *Science* might trump a 20-page article in *Marketing Science*. A four-page article in *Pediatrics* that helps change medicine dosing advice to parents may trump a lead article in the *Journal of Personality and Social Psychology* that refines a construct.

Providing key metrics is critical for a boundary researcher going up for promotion. Consider four general metrics upon which heterogeneous articles can be compared: (i) journal quality, (ii) academic impact, (iii) media impact, and (iv) additional impact. Not all of these are relevant for all articles, but they can provide a "pick and choose" approach depend-ing on the intent of a scholar's research.

1. Journal quality. The gold standard of journal quality is its Impact Factor from the Social Science Citation Index (SSCI) database. This objective score indicates how many times the average article in that journal is cited each year. Whereas the average journal article in *Pediatrics* might be cited 9.2 times per year, the average journal article in *Journal of the Academy of Nutrition and Dietetics* might be cited only 3.9 times.

   A second measure is the Rank Order within a field and the quartile into which the journal falls. This is useful in smaller fields where the journals have a low SSCI index (because of fewer scholars) but are still considered top quality in their field. (For instance, both *Pediatrics* and the *Journal of the Academy of Nutrition and Dietetics* are in the top quartile for their respective fields.)

2. Academic impact. One way to track one's academic impact is to use the number of Web of Science citations

(WebofKnowledge.com Eisend and Lehmann 2014). This service tracks citations in peer-reviewed journals that have an established level of quality. It shows when and by whom the article has been cited. Web of Science citations are a conservative measure of an article's impact because it does not include how many times it might have been cited by unindexed journals (e.g., those that are new, have low circulations, or are of lower quality) or how many times it has been cited in books or on websites or blogs. Google Scholar (scholar.google.com) tracks these other citations, but it has been criticized for low quality control for including any unscreened citation of the article on the web. As a rough rule of thumb, Google citations will be at least twice as high as the Web of Science citations for an indexed journal. But for very popular articles (e.g., highly cited scales, frameworks, or review papers), Google citations can be four to five times higher than Web of Science citations.

Other article aggregators (e.g., SSRN or ResearchGate) can provide evidence of academic impact because they track the number of times a working paper has been downloaded and how many times that abstract was viewed. These different measures of impact can all be reported because they triangulate on academic impact because each comes from a slightly different focus: The Web of Science focuses on *quality* of citations, Google Scholar focuses on the *breadth* of citations, and SSRN and ResearchGate focus on *immediacy* of citations.

3. Media impact. Media impact is relevant for some articles (such as those with a consumer interest), but not for others (those that are more technical or methodological). One way to note media impact is to simply count the number of media outlets that have mentioned the story. The misleading problem with this number is that it can sometimes be artificially high if a media aggregator (India Times or Medical Xpress) mentions it and if 20 other websites simply republish the same story. One way to provide better quality control, therefore, is to mention the top media sites that reported on the story in a unique manner (such as having an original title and first paragraph).

When accounting for social media impact, one of the more justifiable measures to use is an article's Altmetric Score (altmetric.com). This provides a weighted number of how many times the web address for the published journal article (the DOI) has been linked to or reported in news articles, posts, Tweets, blogs, and so on. While a person can also note the number of times the article was tweeted or mentioned on Facebook, this could trivialize the article in the eyes of some. If something like this is noted, it might be better to mention it more casually in the next section, "Additional impact."

4. Additional impact. This section can be stylized to whatever other types of impact a researcher wants an article to make. It can also provide a quick visual reference of how this article has been received in the field. It could include universities where this research has been presented, courses

in which it is used, or the number of direct requests for working papers or reprints. It could involve how a finding may have been implemented or adopted by consumers, companies, or policy makers (although this rarely happens very quickly). This could also include awards the paper has obtained or direct quotes from media or other opinion leaders.

Table 2 gives an example of a research impact matrix that contributed to a successful tenured case in the USA. The names of the articles and publication dates are disguised, but the range of the journals, their quality, and their reception are instructive. What is important to note is that heterogeneous articles from heterogeneous journals can be summarized for reviewers and committees in a way that helps focus their evaluation. When used as a table or appendix in a tenure or promotion packet, this matrix can provide a useful narrative structure for describing the impact of a boundary research record.

The 3-year (Web of Science) citation test: will this article matter in 10 years?

Two key criteria for tenure and promotion are (i) how much have you published and (ii) how much will you publish? In general, you will be evaluated on your potential to have an impact on your field. Estimating the future impact of boundary research – or any research – is challenging in one's early years. Most articles are recently published within the last 3 years and, therefore, have very few citations. Whereas a candidate may argue their research is on the cutting edge of a new trend, a senior researcher might instead simply view as faddish or idiosyncratic and that it will have been largely ignored 10 years in the future.

A recent tenure case at a large US university generated some useful insights as to how to answer this question. A boundary researcher was focusing on an unusual topic and one that had political implications for diversity hiring. As an example (although this was not the topic), suppose the research is on how high-functioning teams under life-threatening situations (astronauts when there is a Apollo 13-level malfunction, Navy Seals when there is a Black Hawk Down disaster, and so on) differentially weight the opinions of team members who are dissimilar from them in a way they do not fully understand – such as being gay or being a viewed as a religious zealot. This researcher only had five articles but made the case that this topic would explode in importance over the next years. As a result, they would be the premier leader in a new sub-field of research that would have impact in multiple areas (discrimination, judgment making, social cognition, and public policy). Outside letters were mixed – some very positive and some negative – and the case had been rejected, appealed, accepted, rejected at a higher level, and appealed again. One key question was whether this research was going to be influential in the future. One pointed question was "Ten years from now, how many of these articles will have been cited at least 100 times?" This was shortened during discussions to being "Will these be Centennially-cited papers?"

During tenure and promotion discussions, it would have been useful to have an objective rule of thumb as to whether

Table 2. An illustration of a research impact matrix developed for promotion and tenure[a]

Title of article[a]	Journal and Date	1. Journal quality		2. Academic impact				3. Media impact		4. Additional impact
		Journal impact factor (SSCI database)	Rank order within its field (e.g., "2nd of 23")	# SSCI citations	# Google Scholar citations	# SSRN or ResearchGate downloads	# media outlets citing work on Google News	Sample of media outlets uniquely citing (up to top 10)	Social media impact Altmetric Score[b]	
"A path analysis model of interstellar trade"	*Journal of Marketing*, 2011	3.819	Q1 Business	7	11	93	10+	Atlanta Journal Constitution, Scientific American, Toronto Star (Canada), Het Laatste Nieuws (Belgium)	67	• 2 conference present • 14 requests for copies • Presentation at OSU
"Mediation analysis: did we do it right this time?"	*Psychological Reports*, 2012.	0.402	Q4 Multidisciplinary Psychology	9	16		50+	Reuters, New York Times, Fox News, ABC News Radio, LA Times, Huffington Post, RedOrbit, BusinessWeek, US News, Money Talks News, Yahoo! News	83	• 2 invited keynotes at company retreats • Filmed for a Dateline episode that was not broadcast
"Ignorance and apathy when reading this table: who knows and who cares?"	*British Medical Journal*, 2012.	6.783	Q1 Medicine, General & Internal	41	76	3	50+	The Wall Street Journal, USA Today, Washington Post, ABC News, San Francisco Chronicle, Newsday, LA Times, National Public Radio, The Times of London, BBC News, Canadian Broadcasting Corporation	103	• Listed as one of the 10 best new findings in its area by Business Insider • Contributed to Applebee's changing their glassware
"Punchy or pathetic: article titles with colons are passé and should not be accepted"	*PLOS ONE*, 2013	3.534	Q1 Multidisciplinary Sciences	7	9	6	50+	ABC News ("The Early Show"), Business Week, CBS News, MSNBC, NPR, United Press International, New York Times, The Washington Post, LA Times, WebMD	56	
"How redundancy and restatement reduce interest in and reading of ads, web pages, journal articles, and so forth, etc."	*Journal of Consumer Research*, 2012	2.783	Q1 Business	31	62	202	50+	ABC News, Wall Street Journal, Good Morning America, CNN, Fox News, Huffington, Scientific American, Yahoo! News, Forbes, NPR	81	• Website for paper receives an average of 1200 unique visitors/month • Page as been Tweeting 142 times
"A literature review involving	*Journal of Experimental*	2.426	Q1 Applied Psychology	3	19	9	25+		40	• 4 of 5 studies are included in 2 new meta-analyses on this topic

(Continues)

Table 2. (Continued)

Title of article[a]	1. Journal quality			2. Academic impact				3. Media impact	Social media impact Altmetric Score[b]	4. Additional impact
	Journal and Date	Journal impact factor (SSCI database)	Rank order within its field (e.g., "2nd of 23")	# SSCI citations	# Google Scholar citations	# SSRN or ResearchGate downloads	# media outlets citing work on Google News	Sample of media outlets uniquely citing (up to top 10)		
"Sixth time a charm"	*Journal of Food Science*, 2013	1.791	Q2 Food Science & Technology (48th of 120)	19	44	-	10+	-	0	• Is included in the Dietary Guidelines • Library database regarding food labeling • Used in Ph.D. courses (USC, UIUC, Cornell)
"Does professional editing effect the believableness and credibility of clinical findings?"	*Journal of Pediatrics*, 2014	3.736	Q1 Pediatrics (6th of 111)	3	10	8	50+	Reuters, Huffington Post, RedOrbit, Yahoo News, The Guardian, Science World News, Healthy Kids Today, Business Insider Australia	181	• Referenced in editor's front-issue-editorial saying, "although clinically unproven it is potentially useful and holds little risk"

[a]While this is an actual impact matrix of an actual tenure case, the paper titles and journal publication dates have been altered for privacy, but all of the remaining columns are actual entries.
[b]The Altmetric Score is a measure of internet activity that directly links to the original home of the journal article (altmetric.com).

these articles would have a substantial impact in the future or whether they would still be "findings on the fringe," as one reviewer had described them. To try and develop one rule of thumb, an analysis was conducted of 200 scholars in eight fields from the top 50 Ph.D. granting international universities (determined by the Gorman Report). For each scholar, one of their top-tier publications was randomly selected, and one of their lower tier publications was randomly selected. The number of Web of Science Web of Science annual citations for each article was then collected for each of the first 10 years of its history (Year 0 to 10).[1] The goal was to estimate how many citations a new article would have in 10 years based on only the initial 2–3 years of its citation data.

It was found the first two full years following the year in which a paper is published provided both an intuitive and compelling estimate for the committee. Suppose a paper is published in June of 2016. A decent estimate of how many citations it will have in 10 years (2026) can be extrapolated from the total number of Web of Science citations it receives in the remainder of the year in which it was published (2016) as well as in the next two full years (2017 and 2018).[2]

Papers that were highly cited after 10 years tended to also be relatively highly cited (including self-citations) immediately after they were published. If they received four to five citations in each of their first 2–3 years, they were likely to have 100 citations in 10 years. If they received one or two citations in each of these years, they were more likely to have 30 citations in 10 years. Although a Bass diffusion model yielded higher predictiveness, the simple rule of thumb was good enough for the committee. A paper that has 12+ citations in the Web of Science in years 0–2 after publication was 90 per cent likely to have over 100 total citations in 10 years (Figure 4). A paper that has five to 10 citations in years 0–2 after publication was 70 per cent likely to have between 40 and 80 publications. A paper with less than three citations was 80 per cent likely to have fewer than 30 in 10 years (Wansink *et al.,* 2016).

What about papers that are "slow starters"? Most academics have papers that have very few citations in the first years but which we believe will eventually catch fire and explode in terms of their academic impact. Sadly, this seldom happened with this sample. Nine of 10 highly cited papers were relatively quickly cited within their first 2–3 years (three to four citations per year). That is, they had 12 or more Web of Science citations in their first 3 years (including self-citations). Those with fewer than five total citations within the first two full years following the publication year were 90 per cent likely to follow the flat "typical citation trajectory" in Figure 4.

[1]The analysis was conducted using the Web of Science instead of Google Scholar for two reasons. First, the Web of Science conservatively counts citations in qualified journals and cannot be spuriously influenced (or "gamed") by unmerited mentions of the article publically on legitimate or less legitimate websites. Second, Web of Science citation counts are available year by year, whereas Google Scholar currently only reports the total number of citations to date and not the trend of these citations for a specific paper (although they are privately available to the scholar themself).
[2]Additional adjustments could be performed in the first year the article was published (January versus June). Initial analyses aggregated across the different months of the first year. In practice, these extra months do not seem to substantially matter. Papers that are will be highly cited distinguish themselves in the first 24 months of being published.
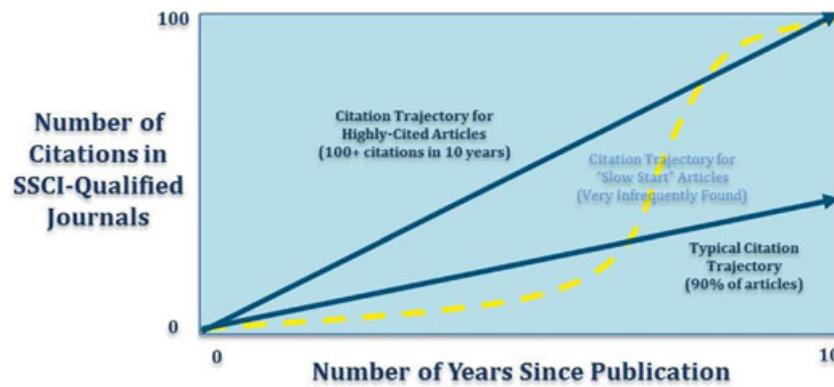
Figure 4. Predicting citations in 10 years using the three-year (SSCI) citation test. This figure is available in colour online at wileyonlinelibrary.com/journal/cb

Table 3. Getting started with boundary research

Career stage (and objective)	One possible plan	The rationale
Junior faculty (keep the fire burning)	1. Answer the question and then find the journal.	If you compellingly answer an important question, your paper will eventually be published. But scholars often first target a journal (e.g., "Let's write a paper for JPSP") and then start the research process. This can unnaturally constrain and bias the research question, context, and independent variables. Starting with the right question can give the right insight, even if you have to do another add-on study to make it JPSP worthy.
	2. Think of a portfolio of target journals.	Write and submit to a variety of journals where you think your ideas will have the biggest impact. Even if these journals are not all premier journals, this strategy has three advantages: (i) it extends your ideas to multiple audiences, (ii) the publications still "count" toward tenure (except at the most elite institutions), and (iii) it keeps you in the game, it keeps you motivated, and it sharpens your skills as a researcher. Without some early publication victories, even at less prestigious journals, it is easy to become discouraged and let the fire burn out.
	3. Team up with a senior scholar.	When you team up with an academically productive senior scholar, your joint work will almost certainly be published somewhere. When approaching this person, you need to clearly demonstrate what your value would be to the project and to their over-programmed schedule. Being prepared to do 85% of the legwork is a good start. Additionally, the right person can be a valuable confidant and advocate as you grow and move through the field.
	4. Which school's best?	Some professors are academic migrant workers. They start at one school and move until they find a school that is "a good fit." Being at a school where you feel appreciated and productive is worth deceptively more than being at a more prestigious school where you feel neither. If it means being at the 45th-ranked school in the world instead of the 25th-ranked school, the difference is probably worth the trade-off for both your sense of well-being and your productivity. A difference of 20–30 ranking points is probably worth the trade-off. Beyond that, maybe not.

(Continues)

Table 3. (Continued)

Career stage (and objective)	One possible plan	The rationale
Senior faculty (find a new spark)	1. Broaden Your view of "acceptable" journals.	Focusing only on premier journals can keep us from asking the most important or useful questions in this new area (that is, we ask the questions that are most publishable instead of most useful). Also, a portfolio of articles in different types of journals would broaden the academic market for your ideas. In one study, when academics were asked about a paper they had written that had their biggest impact outside of academia, about half claimed it was published in a journal where they usually do not publish (Wansink, 2012). Submitting articles to an unfamiliar journal or working with a new partner requires rebuilding our credibility.
	2. Attend unfamiliar conferences (or sessions).	Although reading unfamiliar journals is valuable, going to unfamiliar conferences in this new area is even more useful. These conferences are forums for a wider range of topics and questions that are relevant to decision makers in that area. Although you might be an unknown person at the party, it can be liberating not to have to attend the same types of conference sessions one usually would. Unfortunately, attending unfamiliar conferences can be expensive in both dollars and hours. A second best solution can be to attend unfamiliar sessions at a conference you already attend (Lehmann 1993). With new conferences, it is humbling to see how little influence most of us we have outside our field. This also points to how little we have to lose by moving toward boundary research.
	3. Do some of your own field work.	Senior professors often "leverage" their time by having research assistants to do the field work, interviews, data collection, and to be a main contact person with the research partners. This often results in noisy data, instead of useful serendipitous insights. Even the best trained graduate students and research assistants are inexperienced with the unexpected debriefing glitch, equipment malfunction, or an unruly participant, and they will seldom make the same judgment call that you would have made. Furthermore, the hypothesis-driven mission you gave them can lead to a tunnel vision that prevents them from seeing an unanticipated – but far more interesting – pattern of results that you might recognize.

There is some curvilinearity in these relationships, and there is some difference across fields. Still, this basic rule of thumb will be most useful to most boundary researchers who are putting together a case for their promotion. It can also be useful for committees making a decision on the future impact of recently published research.

GETTING STARTED

There is a temptation in academia to view publishing with a short time horizon. This is reasonable. If a dissertation is not completed on time, you lose your funding. If no papers are completed during a Ph.D. program, you do not get a good job. If there are not enough submissions as a third-year assistant professor, your contract is not renewed. If there are not enough acceptances after 7 years, you do not get promoted.

While a short-term focus is understandable, it could come at a cost. Centrist researchers have a more easily visualized path to quicker publication: Although those areas are crowded and competitive, the methodology is clear, the importance is clear, the critical references are clear, and the positioning is clear.

Yet if a person chooses a research topic because it seems like a quicker route to publication (or because their advisor suggested it), they are being externally motivated. Based on what we know about attribution theory, once the external motivation is taken away (once one becomes promoted or given tenure), there is a risk one's motivation could die. Although people commonly hope that after they are securely

promoted, they can "then do the research they really want," by that point, it is uncommon to see drastic changes. When these infrequent exceptions do occur, they are typically the exceptions that prove the rule (such as Belk, 1974, 1975, versus Belk, 1985). If a person was rewarded with tenure or promotion by working on a topic that their advisor – or the zeitgeist of the time – indicated was a good topic 10 years earlier, many will not stray more than a couple steps from that area for the rest of their career.

A notable academic once said, "We are not in academia to publish ten articles. We're in it to publish a hundred" (Morrison, 1988). If one goal is to conduct research that will inspire you for a 100-article lifetime – and if that area happens to be boundary research – how should you start? Observations on how to begin differ for younger versus more established researchers – whether you are a junior faculty or senior faculty. Table 3 summarizes some different observations based on the personal experiences of some of the scholars who are thanked in the acknowledgements.

CONCLUSION: WHAT WILL BE YOUR BIGGEST REGRET?

In 2002, a professor from a top department at a top Big-Ten school was receiving a career contribution award at the annual spring meeting of the university's Business Advisory Committee. He was one of the most notable economists at the university. He occupied a rare niche at the intersection of economics, real estate, finance, and law. He was widely published and widely influential, and people often spoke of him in awe. He had won numerous awards, and the rumor was that he was one of the most highly paid faculty in the business school. This year was his retirement year, and his speech would perhaps be his "Last Waltz" in front of a group like this. His talk went well, and the closing reception after the talk was well attended and filled with congratulations and appreciation. Later, as the crowd thinned out and private conversations were possible, he was asked, "In light of all of the remarkable things you've accomplished in your career, what's your biggest professional regret?" After an uncomfortable pause, he said he had one regret:

> My work lies at the intersection of four areas – economics, finance, real estate, and law. I have a very complete picture of how these interact and how they influence everything from real estate prices in ghettos to land speculation prices in the middle of nowhere. The problem is that I'm the only one who sees the big picture. My papers are published in econ journals and finance journals, while others are published in real estate journals and law journals. Nobody else sees the big picture because they only read one type of journal.

When then asked,

> Would it be easier for people to see your bigger picture if you were to write a book that pulled all of this together? That way, everything would be in one place and you could connect all the dots for them.

He chuckled and said, "In economics, they don't reward books."

After 45 years of research, here was a person who was retiring with one needless regret. Yet what he let get in his way was how he would be rewarded or whether a colleague might think he was simplifying his research for the uninitiated. For him, writing a book could have been a potentially transforming project. It could have focused on solutions, it could have clarified a series of debates, and it might have erased his regret. Given the relevance of this area, it could have become game changing.

Many examples of transforming books that fit the above take-away can be identified (such as Griffin *et al.,* 2012; Otnes and Maclaran, 2015). However, the metaphor that is relevant for us is not necessarily a book. It is any project that might ratchet up our level of influence in the area we are most passionate about. It is any project that might not be "rewarded with the respect of the professor next door," but that we think is critically important. In fact, it might be actively derided. That is what happened to a number of metaphorical books. It happened to Carl Sagan's award-winning "Cosmos" series on PBS, to Gary Becker's *Business Week* columns, to Richard Posner's federal judge appointment, and to Stephen Ambrose's World War II Museum. Although they were not boundary areas of research, they were boundary projects that had lasting if not transforming influences on people. They launched science careers, changed investment strategies, increased individual freedoms, and gave a voice to the *Greatest Generation*.

This economist's unwritten book can be a useful metaphor. Many researchers have at least one metaphorical book that would take their ideas to a new level of influence. It might be starting a website and blog, presenting research in front of a House Subcommittee in order to propose a law, making class modules for science teachers, writing a review article in a related field, or starting a new class and turning the notes into a MOOC. Transforming behavior is what many of us dream of doing. But, it cannot be guaranteed. Yet when we start with passion – even it is an area of boundary research – people notice.

One last comment that this economist made is particularly powerful when thinking about whether to follow an area of research passion before or after tenure or promotion. Later that night, after the reception, he spoke about how quickly his research years had passed. He said after he graduated with his Ph.D., he blinked and he had tenure; he blinked again and had an endowed chair; he blinked again and he was traveling to this event to make his speech. Waiting to follow your research passion only when "the time is right" is an opportunity that could disappear in three blinks of an eye.

BIOGRAPHICAL NOTES

Brian Wansink, is Professor and Director of the famed Cornell University Food and Brand Lab, where he is a leading expert in changing eating behavior - both on individual level and on a mass scale - using principles of behavioral science. He is the author of *Mindless Eating* and *Slim by Design* (which have been translated into over 25 languages) as well as over 200 peer-reviewed journal articles. From 2007 to 2009, he was White House appointed as

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Koert van Ittersum is a Professor of Marketing and Consumer Well-being at the Faculty of Economics and Business, University of Groningen, the Netherlands. Van Ittersum conducts substantive research to help improve consumer well-being - a state of flourishing that involves health, happiness, and prosperity. His research represents a unique multidisciplinary blend of consumer psychology and marketing on the interface of in-store decision making, food consumption, and obesity. His work is widely cited in marketing, medicine, nutrition and dietetics, food science and technology, public, environmental, and occupation health, psychology, and economics and has drawn global media attention. He received his PhD from Wageningen University.

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