

REVIEWER INSTRUCTIONS AND REFERENCE MATERIAL

Dear colleague,

Whether or not you are an experienced reviewer of conference/journal papers, please take note of the following suggestions:

1. Just like with journal reviewing, the goal is to provide constructive feedback, and each work deserves to be judged firstly by its merits and only secondly by its shortcomings. It is good practice to start the review with a few sentences that a) demonstrate that you have read and understood the manuscript, and b) shows appreciation of the strengths and/or potential of the paper.
2. Unlike journal reviewing, this review process is not going to be blind. Authors, the three reviewers (which include the facilitator/track chair) and other participants will collaborate on developing the paper at the workshop.
3. Unlike (most) journal reviewing, the reviewers in this review process are not always going to be experts on the subject matter of the paper. Nevertheless, you will get and be able to give valuable feedback on structure, clarity, general theory and method issues, etc. The closer to the core of entrepreneurship research your submission is, the more likely one or more of your reviewers will also have considerable expertise on the subject matter of your research.
4. Unlike (most) journal reviewing the paper may not be in a shape where the authors think it is ready for publication. As reviewer you will receive contextual information from the author. Please give developmental feedback with the intention of helping the paper from where it currently is to a suitable 'next level'.
5. Please prepare *written* reviews of at least the length of 1 A4 paper, single-spaced (that's about 600 words).
6. All papers and authors deserve fair treatment. It is a good idea to spend the first few lines making clear that you have carefully read and understood what the paper is about, and what its strengths are. (Just wanted to make sure you did not miss point 1 above...)
7. Do not hesitate to criticize all important shortcomings of the paper. If authors do not get a chance to learn about these they cannot improve. However, make sure your critique is developmental—that you can suggest alternatives, improvements, repositioning, extensions, alternative publication targets and/or possible collaboration partners—so that the author can get more leverage out of his/her ideas and data.
8. Try to avoid reviewing the paper 'page-by-page' or to mix important, fundamental issues with trivialities. Therefore, start the critical feedback part of the review with numbered 'major issues' (e.g., relating to the appropriateness of the suggested theory; chosen methods; suggested interpretation of the results; logical inconsistencies) followed by numbered 'minor issues' (such as typos, missing references and unclear phrasing).
9. Number your points in sequence so that it is easy to refer to a specific point of critique.
10. If you feel a need for 'role models' I enclose below some examples of previous bootcamp reviews and journal reviewer instructions. However, do not consider any of this a template that you follow slavishly. In essence the review should be an honest, friendly letter from you to the author about how this particular paper can realistically best be developed further by this particular author.
11. Some of what you may want to cover in your review is:

- a. What specific stream of literature to contribute to; which specific 'research gap' to address, and how to clearly communicate the contribution in the Introduction.
- b. Distinguishing between the author's own research journey, learning, and sense-making on the one hand, and true contributions to our collective knowledge on the other (it is the latter that most readers and journals want the authors to share)
- c. What audience and journal(s) to target
- d. Combining papers? Splitting up in several papers? Different 'packaging' for different audiences?
- e. Strengthening of theoretical background and literature review
- f. Elimination of detours
- g. Further development of data and method. Missing information? Excessive detail? Follows structure of target journal?
- h. What results to especially highlight
- i. Alternative interpretations of the presented results.
- j. What implications to elaborate on for what interest groups?
- k. Writing a powerful introduction to evoke reader interest and clarify the paper's contribution to existing research streams

NB! Friendly, colleagues 'previewers' and anonymous conference/journal reviewers sometimes have an important role in developing a paper. For this they are often acknowledged with a 'thank you' note—but their involvement does not amount to co-authorship. This is also the rule for our exercise—the help we give in the context of this workshop does not come with an expectation of getting recognition as co-author.

EXAMPLE review 1

Thanks for the chance to read your work. You seem to have done a nice job with the conjoint study and the pattern of your results makes a lot of sense to me and is interesting. I have a few comments and suggestions. These are not intended as judgments, but rather as points for discussion.

1. I think that your theory development section could do a much better job of setting up your study.
 - a. In my opinion, the framing of a conjoint study needs to make a compelling case for why you have chosen the particular factors you included in your study. It is not clear to me why you chose newness, relatedness and financial gain. Why, for example, did you not include a direct factor for risk? Or resource requirements? Or difficulty of competitor imitation? Etc. I think that you need to make a good case for the choice you made. One way to do this might be to develop theory about risk and perhaps resource requirements and argue that your "newness" measures proxy for these.
 - b. Much of your theoretical argument applies directly to what Shane and his colleagues call "opportunity discovery." But I think (I'm not positive, because I haven't seen the actual scenarios you presented) that your study presumes an opportunity has been "discovered" and really focuses on the subsequent step or moment of opportunity "evaluation." Thus, much of your argumentation based on prior experience does not really fit the point you are trying to make. I think that you need to focus your argument more on how

prior experience is likely to affect opportunity evaluation. Entrepreneurs can only evaluate opportunities they have thought of, so much of the effect of prior experience has occurred prior to opportunity evaluation (remember – in Shane, 2000 the people who started different businesses off of the single technology had not considered and rejected the other opportunities, they had never thought of them at all).

- c. Your theory development includes claims and tangents that do little to make your argument, but set you up for gratuitous reviewer objections. For example, the nature of entrepreneurial opportunities (e.g. discovery versus creation) and the relationship between “idea” and opportunity are somewhat controversial, and you make implicit claims about these issues that are completely unnecessary to your primary argument, but which will invite reviewer objections.
 - d. I don’t see the connection you are trying to make to effectuation and indeed your approach to financial gain and your results would seem either inconsistent with or orthogonal to “affordable loss.”
 - e. I think it is hard to maintain in the face of Dean Shepherd and his colleagues body of work that there has been little conjoint analysis in entrepreneurship research. You should, I think, build more directly on Dean’s stuff.
 - f. The whole history of individual traits and the transition to work on entrepreneurial behavior is an unnecessary aside. Also, I don’t follow the logic when you state “Gartner calls for a behavioural approach...” and follow this with “In other words attitudes of entrepreneurs reflect their attractiveness for the creation of good and services.” How do you get from Gartner’s call to focus on behavior to a focus on attitudes?
 - g. Your brief (and, I think, unnecessary) review of Schumpeter versus Kirzner seems to suggest that Schumpeter was big on newness and Kirzner was big on imitation. You “distill” the two views into a focus on “newness.” What I think you mean is that they represent distinctive positions on the importance (and perhaps attractiveness) of newness to entrepreneurship. I think the ambiguity here leads to some confusion in the rest of the paper such that even though the overall pattern of your results suggests (with interesting variation across the factors) that the folks you are studying are avoiding most novelty, you still end up concluding that “newness” is very important to venture attractiveness.
2. I think it’s great that you paid attention to Schumpeter’s insight that there are various types of novelty important to e’ship. Your operationalization of newness makes a lot of sense for products/services. But for production methods, new customers/markets and promotion, the “new to the world, new to the market, substantial improvement and imitations” categorization is a little bit less natural. In particular, with regards to market newness, I have trouble visualizing what is meant by/distinguishing between: “Totally not served by other businesses,” “Not served by other businesses” and “Substantially different from what others offer.”
 3. Please provide an example of the instrument your respondents actually looked at and evaluated.

4. Rather than focusing so precisely on the actual utility estimates, I think you might consider interpreting your results at a slightly more conceptual level, backed by the numeric result. To me, it looks like your entrepreneurs avoid both opportunities to introduce novel product and opportunities to duplicate existing products but are drawn to opportunities to introduce improvements to existing products. They strongly prefer to imitate existing production methods, have some attraction to making improvements to existing methods, but strongly avoid introducing new production methods. They want neither to pioneer brand new markets nor to enter highly competitive existing markets. They have a preference for moderate novelty in how they promote their products and services (I'm not exactly sure what this means in practice), and they prefer opportunities that are closely related to what they already know, that require resources like those they already control and that provide a chance to make a lot of money.

Particularly interesting to me is that your entrepreneurs come across as relatively conservative incrementalists, which fits lots of prior evidence, but does not match the recurrent emphasis on novelty as a key and essential characteristic of entrepreneurship. It seems to me that if you developed more fully your theory about novelty up front, your results might give you the opportunity to say something useful about the often limited and contingent role of novelty in entrepreneurship (and even couch this in terms of Schump v. Kirzner, if that's your preference). Of course, some essentialists might say this means that the people you studies aren't really entrepreneurs!

I look forward to the chance to talk with you about this project.

EXAMPLE Review 2

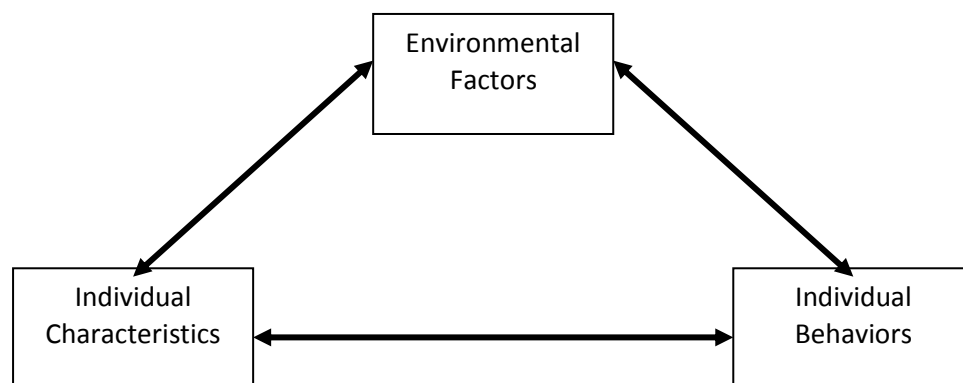
This theoretical paper makes the case for the importance of "context" within the entrepreneurial opportunity recognition process. I offer the following comments to hopefully help you to move the manuscript closer to submission to a journal:

1. Be careful not to criticize extant literature. For example, you criticize Shane (2000) for overlooking context within his study, but that wasn't really the purpose of what he was doing. He was simply trying to demonstrate that the backgrounds of individuals affect the types of opportunities that they perceive. To this end, I think he did a pretty good job. If you think his findings would have been different if he had taken into account certain contextual variables, then you need to state that explicitly. Otherwise your point seems to be lost. Instead of pointing out flaws, try to develop your story around how you can "build upon" prior studies. Sometimes this is simply a matter of tone to consider within your writing. You never know who your reviewers are going to be! So you always want to be complimentary!
2. In your introduction, I think you've missed where the field is headed (or has already arrived) in terms of defining the entrepreneurship. Entrepreneurship is generally about any behaviors that results in "value creation." I am fairly confident that this is a view that is going to hold over the long-run.

3. I think on the second page you mean to cite Shane and Venkataraman (2000), rather than Shane and Verk (2000).
4. The paper reads like a running commentary on the case for considering the context (in different forms) within the entrepreneurial opportunity recognition process. You need a framework to pull all of your ideas together in which you can provide a more coherent story along with substantive recommendations for what future studies can better incorporate context into the examination of the entrepreneurial recognition process. You through out so many different was in which context can be considered, and seem to insinuate that they should all be considered jointly. But is this realistic? Are there instances in which certain aspects of the context are more important than others? Can you help to focus researchers thoughts in a realistic way on which contextual variables would be most fruitful for them to consider.
5. Social cognition theory might provide you with an appropriate framework. See Wood and Bandura (1989)

Wood, R., & Bandura, A. (1989). Social cognitive theory of organizational management. Academy of Management Review, 14(3), 361-384.

Essentially SCT considers the bi-reciprocal relationships between (1) the environment, (2) individual characteristics, and (3) individual behaviors.



So you could essentially plug what you think are the most important variables for each of these categories and propose how they influence each other in respect to opportunity recognition.

6. Finally, I think that one of the most neglect facets of opportunity recognition is the fact that opportunities are dynamic (i.e., they are consistently changing). Therefore static model of opportunity recognition that does not take into account "time" would seem to be fundamentally flawed. Entrepreneurs need to continuously be able to scan their environment and modify their opportunities or recognize when to drop certain opportunities and move on to others. Here are some self-serving references that might be helpful to you – but each of them probably suffer from many of the issues that you have brought up in your paper. (abstracts suppressed *PD*)

EXAMPLE Review 3

This is the most developed of the papers I've read for the bootcamp and it obviously has a lot of promise. The empirics seem pretty tight, the theory a little bit less so. I'll comment first on the question you have posed about where to publish. I'll then point out my most general and impressionistic criticism of the paper and suggest – very loosely for discussion – whether you mightn't reconsider your conceptualization/operationalizations of growth. Finally, I'll provide a laundry list of some other things you might think about. I won't say much about writing because I think that whatever path you take, you'll need to engage in some substantial restructuring. This is not intended as a "review" but rather as topic for discussion.

- A. If this were me, I would do the substantial work required to try to get this into shape for AMJ. As with any paper, I think this would be risky, but I think there's a non-trivial chance that this paper could be developed for AMJ or another very good "theory" journal. The work required would be pay dividends even were you to end up sending it to JIBS or JBV. Alternatively, I think the paper could be gotten into shape for JBV pretty quickly, and for JIBS with slightly more work.

That said, I would have a slightly different take on framing strategies 1,2 and 3 as outlined in Per's email included with the paper. I think that for JIBS the core would be (rather than interest in the phenomenon of SME internationalization) trying to disentangle and perhaps integrate (which you don't try to do now, although you note they are complementary) the three groups of theoretical perspectives on internationalization, at least with regard to their lessons for the causal links between international activity, knowledge (learning) and growth. This would include a reconceptualization and clarification of forms of growth.

For AMJ, I think you might consider making this a paper much more generally about firm growth, from a Penrosian perspective, with a focus on how firms learn to make better use of their resources and how this intertwines with the opportunities they can 'see' and pursue. Your theory would then be developed at a higher/more abstract level, e.g. relying more on something like the KBV or on other extensions of Penrose. From this perspective all of your contextual factors – Sweden and SMES and internationalization activities could be justified in terms of their usefulness in examining/testing your theory. Sweden because of the superior data and the level of international activity. SMEs because we are more likely to detect change effects over short periods of time than we are in bigcos. International activity because it is a discrete and observable learning opportunity.

I think that to pull this off, you would need to do a much stronger job of theorizing (and operationalizing – but you can probably do this with the data at hand) distinctive growth paths/outcomes. (As I'll note below, right now I not sure your three types of growth are all that distinctive.) I think that the flow of your theory would then be something like:

1. Penrose etc. say... learning → growth
2. We can build on this to theorize learning(x,y,z) → growth (a,b,c) and these different forms of learning and growth are important because...

Within this framework, most or all of the stuff about IE, Uppsala, etc. becomes simply supporting logic evidence for your theoretical claims about $x \rightarrow a$; $y \rightarrow b$; $z \rightarrow c$.

Thus, you would be developing theory linking knowledge to growth and testing it, rather than trying to "sort out" distinctions between internationalization theory strands. You could still note that one of your contributions will be to clarify some of the other stuff, but you would then be doing so in the context of higher level theory that you have developed. I think this might be clearer and lead to the ability to claim a bigger theoretical contribution.

- B. Despite what I see as its promise, at the moment the paper seems very long and flabby; it lacks a clear narrative line. More importantly, it lacks theoretical "tension." To be blunt: by the time you have finished developing your hypotheses, I don't care as much as I'd like to about what your answers will be and I can't explain why I should care.

In large part I think the lack of tension is due to points that Per has already made, i.e., the fact that the paper hasn't quite made up its mind whether it wants to live at a general theory journal, an international journal or an e'ship journal. Simply resolving this issue of focus will help a lot. But, in addition, I wonder whether the lack of a clear theoretical narrative line might be due in part to the very way you conceive of different kinds of growth. Your forms of growth appear to me, both conceptually and as operationalized to leak over into one another.

In the introduction, it's:

1. Firm grows more than its competitors (linked to rents?)
 2. Firm increases its internationalization (regulating role)
 3. Firm grows through offering a market something new (entrepreneurial?)
- And I think this is what you currently trying to measure in the paper.

In the discussion, it's:

1. Growth through targeting unserved segments (new customers in existing geography)
2. Growth through geographic expansion
3. Growth through offering new products and services

"2" and "3" are pretty close, but the mismatch on "1" is confusing. More importantly, it strikes me: a. that the second set are more distinctly different forms of growth (relative to the first set) and b. that it would be easier (compared to the current theory development) to develop clear theoretical rationales for why we might expect different patterns of knowledge-based causes for these three forms. Moreover, although I have tried to piece it together, I think that you can measure these in your data with the stuff you already have.

I haven't thought this through, but alternatively, you could probably also theorize something about profitable versus non-profitable growth -- or you could do this post hoc in the discussion. I don't think you can get away with making your point about "profitable" growth as a measurement issue at you do now; I think this is pretty clearly a theoretical issue given your theoretical framing, and that it needs to be developed as such.

- C. The rest of this is just a quick impressionistic list of responses I had reading the manuscript in its current form.

1. Your opening sections too much toward reciting what scholars claim and too little toward building on descriptions of compelling empirical findings that support or fail to support their claims.
2. "Growing more than the majority of your competitors, even if profitably, does NOT IMPLY RENTS, as your argument claims.
3. The paragraphs introducing the internationalization and Uppsala perspectives end with nice firm level summary statements. The IE perspective section does not.
4. Stuff leading to H3c is unconvincing and I don't think this h buys you anything theoretically anyway
5. I think you need a table saying what data for what firms came from what source in what year. I could not follow this in your narrative description of the two sets of phone interviews and two surveys. The idea of asking SME owners how much their firm is worth strikes me as fraught. More importantly, it's hard to see how firm value is more about "expansion" than it is about "financial" aspects of growth, esp. given that theories of firm value are pretty much all based on expectations of future cash flows.
6. I don't know what you mean (concretely) that you measured growth relative to competitors in international markets separately for growth of foreign sales and profitability of foreign operations.
7. The elements of "entrepreneurial growth" you use imply what seems to me an odd meaning of "entrepreneurial." If serving new customers, developing new products and moving into new markets are all entrepreneurial, then very few forms or sources of growth are NOT entrepreneurial. What does this leave? Selling more of the same stuff (maybe improved versions of the same stuff) to the same customers you already have?
8. I think your results suggest that knowledge from internationalization is of limited malleability.
9. I think you need to introduce know-how/know-what (procedural/declarative) explicitly early on in your theorizing if you want to talk about it in the discussion; same thing with "complementarity" between market and technological knowledge.
10. You need descriptives and a correlation table for your measures (I'd like to see, for example, whether on average your respondents think they are above average).

Thanks for the chance to read this paper. I look forward to the chance to talk about it.

The Journal of Business Venturing

MANUSCRIPT EVALUATION FORM

Please email reviews to: shepherd@indiana.edu and SubramonyS@Darden.virginia.edu

Manuscript #: 05.190.1

Reviewer #: 1

Date Due: 01.16.06

I. PLEASE RATE THE MANUSCRIPT ON THE FOLLOWING CRITERIA (CIRCLE RESPONSE)

A. Significance of Contribution to Field	NONE	TRIVIAL	MODEST	SIGNIFICANT
B. Contribution to Theory	NONE	TRIVIAL	MODEST	SIGNIFICANT
C. Conceptual Adequacy	NONE	POOR	MODEST	EXCELLENT
D. Technical Adequacy	NONE	POOR	MODEST	EXCELLENT
E. Appropriateness	NONE	LOW	MODEST	HIGH
F. Clarity of Presentation	NONE	POOR	MODEST	EXCELLENT

II. PLEASE ANSWER THE FOLLOWING QUESTIONS BY CIRCLING YOUR RESPONSE.

- A. Is the **title** adequate? YES / NO
- B. Is the **Abstract** adequate? YES / NO
- C. Is the **Executive** Summary adequate? YES / NO
- D. Are the **conclusions** adequate? YES / NO
- E. Is the **arrangement of the material** suitable? YES / NO
- F. Is adequate **reference** made to other work in the field? YES / NO
- G. Is any **text or figure** unnecessary? YES / NO

III. PLEASE GIVE YOUR OVERALL RECOMMENDATION.

ACCEPT: This paper is publishable as is, with normal copyediting.

Accept subject to MINOR revisions: This paper is publishable if the authors make the minor changes discussed in my comments.

MAJOR revision needed: Considerable work is needed, but there is a good chance this paper will be publishable if the authors solve the problems discussed in my comments. The core ideas are basically sound, but more development is needed.

I believe there is an excellent chance the authors can satisfy my concerns in the next revision.

[] **SUBSTANTIAL revision needed:** Although the core ideas of the paper are important, the authors' approach is fundamentally flawed. They should be encouraged to basically start anew, following the suggestions in my comments. Overall, this is a high risk revision.

[] **REJECT:** Although parts of the paper may have merit, the core ideas don't contribute much to the current thinking on this topic and/or the authors' approach is irreparably flawed.

Date _____

Signature

Journal of Business Venturing

GUIDELINES FOR REVIEWERS

The Journal of Business Venturing: International Entrepreneurship, New Business Development, Technology, and Innovation provides a forum for the dissemination of superior empirical, and rigorously developed theoretical findings that advance our knowledge in the aforementioned areas. The editorial policy of JBV requires that submitted articles contribute increased understanding to the process of creating new wealth in society. Articles should be either rigorous theoretical contributions or empirically based attempts to advance theory; i.e., they should test theory in the light of observations of entrepreneurial phenomena. This does not mean that only multivariate analyses of large-sample studies are acceptable. It is entirely appropriate to test some aspects of theory by observation of several cases, or of a single case over time. The issues of reliability, validity, and generalizability should be addressed. All articles must be prefaced by a two-page Executive Summary and a 100-word abstract.

Guidelines for the Reviewer's Evaluation Form.

We ask that you clearly mark your answers without elaborating upon them (save your detailed comments for the "Comments to the Author(s) form.") To help you in making your judgments in part I of the form, we provide definitions of the factors below.

- A. Significance of contribution to field includes:
 1. originality of the ideas.
 2. strength of findings.
 3. theoretical and practical importance of ideas and findings.

- B. Contribution to theory development includes:
 1. extent to which articles draws on existing literature.
 2. extent to which formal hypotheses are developed and tested.
 3. extent to which results are used to extend the existing literature.

- C. Conceptual adequacy includes:
 1. how well concepts are defined.
 2. their appropriateness to content.
 3. how well they are tied into past theory and research.
 4. the extent to which new terms are necessary and useful additions to the field.

- D. Technical adequacy includes:
 - 1. appropriateness and robustness of research design, sampling, measurement, and methods of analysis.
 - 2. issues of reliability, generalizability, and validity, including external validity.
 - 3. in general, accuracy and completeness in reporting the results and all procedures leading to them.

- E. Appropriateness of topics includes:
 - 1. timeliness of the topics investigated.
 - 2. their interest value for JBV's subscribers (personal and institutional).

- F. Clarity of presentation includes:
 - 1. clarity of writing style.
 - 2. organization of material.
 - 3. clarity of tables and figures.
 - 4. accuracy and completeness of information given.

Guidelines for the Comments to the Author(s) Form

On this form we ask that you consider any other factors you think are pertinent to this manuscript and elaborate upon your comments on the Reviewer's Evaluation Form. Reviews should be thorough but fair. A good review generally contains a balance between general and detailed comments (1 page minimum). Your comments should point out current problems and deficiencies as well as make specific suggestions