

EDITING THE 2004 *AMJ* BEST ARTICLE AWARD WINNER

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I was the action editor assigned to the submission that eventually became the 2004 *Academy of Management Journal* Best Article Award winner, “Knowledge Transfer through Inheritance: Spin-out Generation, Development, and Survival,” written by Rajshree Agarwal, Raj Echambadi, April Franco, and MB Sarkar. The current *AMJ* editor, Sara Rynes, has asked for my perspective on the review process for this article, as I observed the interactions between the authors and reviewers, worked with both parties, and made the editorial publication decisions. The essence of this review process can be captured quickly: it was intense, insightful, and inspiring. But in addition, the review process this award-winning paper went through offers some useful lessons that other authors and reviewers can use to improve their own work.

A REVIEW PROCESS HISTORY

The First Submission: December 18, 2001

I received a notice on the morning of December 18, 2001, that I was the *AMJ* action editor assigned to “Knowledge Transfer through Congenital Learning: Spin-out Generation, Growth, and Survival.” Tom Lee, the *AMJ* editor, had identified three very different reviewers. The first was familiar with the topic and the theory on which the paper’s arguments were based. The second had expertise in the submission’s topical area; and the third had written on international business, on the theory underlying the paper, and on some relevant antecedent conditions. On balance, the reviewer panel could comment on the specifics of the submission’s theory and topic, as well as on its overall contribution to the field. I felt that the initial conditions were in place to make an informed editorial decision.

The reviews came back quickly, arriving in mid-January. I read the manuscript closely, noting several impressive features. For instance, the theoretical model drew from some relatively new

perspectives, the data were interesting, the results were compelling, and the topical area appeared to represent a new organizational form. Given *AMJ*’s mission to publish articles that make substantial contributions to management theory (as well as strong empirical contributions), what concerned me most was whether these authors were doing anything new with the theory. Were the hypotheses extensions of existing logic to a relatively new corporate behavior? If so, the authors would need to do more; could I steer them to develop their conceptual model in a way that would represent a new advance for knowledge and explanation? Assuming methodological issues could be resolved, was there a new and interesting story here?

The reviewers’ private comments to me reflected guarded optimism with a fair amount of uncertainty:

I believe that this research has potential to make a contribution. However, I do have a number of issues to which I believe the author(s) needs to respond in order to enhance the potential value of the work. Some work is needed on the theory and I have several questions related to the methods and sample generation/definitions that I believe the author(s) should address. (reviewer 1)

I think this is an interesting paper and one that makes a useful contribution, primarily to scholars interested in knowledge transfer. It follows in the tradition of Christensen’s research on disruptive innovations. The historical method, while not being without weaknesses, does not suffer from the serious biases that plague much research in this area. While it has some flaws, they seem to be correctable. (reviewer 2)

I think this could be a really strong paper. This is a pretty new area of research, and so the paper is basically exploratory, although the authors present it as a hypotheses testing exercise. As a result, they are struggling to find good interpretations of their results, and to some degree this becomes a fishing expedition where they adopt ad-hoc arguments that do not really fit. . . . A second issue is that their results are strong, but it is hard to understand what the results really mean. I think the paper needs a better account of the phenomena of spin-outs and of knowledge development in firms. That is not easy. But there are some opportunities inherent in some control variable effects in their results, and perhaps the authors could make better use of these results to

I am grateful to Sara Rynes and R. Duane Ireland for their extensive comments and suggestions. This editorial was much improved through their hard work, expertise, and understanding of the publication process.

give a more complete story of what is going on. However this might be a bit too large to fit into this paper. So, I guess I am a bit torn here. On the one hand, I would hesitate to demand from the authors a comprehensive theory of spin-outs and a comprehensive theory of knowledge development. On the other, without better accounts of these two, their results are hard to make sense of. Hmm. Perhaps there is a middle ground—a few lines of solid argument that provide a plausible account of the results. (reviewer 3)

As these initial comments reveal, reviewer 2 was the most positive about the manuscript. His/her review consisted mostly of suggestions and supportive comments. In contrast, reviewers 1 and 3 both had substantial concerns about the manuscript, rating its theoretical adequacy and contribution as “marginal.” However, because they also felt that the manuscript had strong potential, they both recommended revision. Both reviews were detailed and challenging, yet also encouraging. Part of what made the reviews so good was that both reviewers 1 and 3 offered a variety of concrete suggestions as to how the authors might deal with the most important issues.

Overall, then, this manuscript appeared to have promise, but it was unclear whether that promise could be realized within the initial conceptual and empirical frameworks. I decided that the appropriate decision was to invite a revision, while at the same time alerting the authors to the degree of risk involved.

My decision letter was a typical one. It highlighted problems with the theory (needed clearer and more consistent definitions, more development of the foundation and structure of the model, reworking of the theoretical logic), concerns about the method (reliability and validity issues, integration between theory and method, tangled relationships among variables, and questions about the analysis), and uncertainty with regard to the “value-added” (weak linkage between the results and the theory, lack of theoretical development based on the results). I highlighted this last issue as pivotal to our decision in the next round:

Although you are focusing on a relatively unexplored research topic and have assembled an interesting database, you do not explicate the value-added of your findings for theory development. Specifically, the Introduction notes limitations in prior research and suggests that combining literature streams would provide a “compelling avenue of research. . .” (page 2). However, your consideration does not focus on theory. You do not identify limitations in theoretical explanations of spin-outs. . . nor do you explain how your study’s findings will

address those limitations or how your revision or addition to theory will be valuable. The same concerns apply to the Discussion section. Here, you review the findings, but do not explain what the findings mean for theory development. Please explain what is new and unique about the model and study, emphasizing the contribution for theory development. Note that this is a serious issue, as a long-standing policy of the *Academy of Management Journal* is that every publication must make a substantive contribution to management theory and empirical knowledge. Unfortunately, these standards are not currently met and it is unclear whether they will become so with revision. Thus, I strongly encourage you to explicate the type of contribution and explain, carefully, the value of that contribution.

We had the pieces of a potentially very good manuscript here, and an excellent reviewer panel. If the authors could develop the story and contribution, I thought this submission could turn out well. But I also realized that attaining such an outcome would likely require that the authors and the reviewers work closely together.

The Second Submission: August 6, 2002

The revised manuscript, “Inheritance by the Unintended Child: The Impact of Knowledge Transfer on Spin-out Generation, Development, and Performance,” was resubmitted about six months after the initial decision letter. The authors wrote a promising note when they resubmitted their manuscript, and I was pleased that they seemed willing to work with our reviewers:

We could not help remarking among ourselves how much the quality of our paper has benefited by the editor and reviewer comments. The feedback has proved invaluable to us in our revision efforts, and indeed, we have found the review process very constructive and developmental. Thank you for all your efforts.

My reading of the revised manuscript left me with the feeling that the manuscript had improved in many important ways. It was clear that the authors had taken the reviewers’ comments quite seriously and that they had been diligent in their efforts. However, I also suspected that some matters were still unresolved. This manuscript was very complex, and I anticipated some additional work would be needed to uncover and clarify the overall story. There was much to like about this manuscript, but I still struggled with trying to understand its primary contribution.

The reviewers’ reports were similarly mixed. On the positive side, reviewer 2 now recommended

that the paper be accepted: "I have carefully read the paper three times and find no weaknesses. I believe it would make a nice contribution to this stream of literature." Reviewer 1 generally believed that the manuscript had gotten better and could probably make it to publication with an effective more revision:

Overall, I found the manuscript to be much improved over the previous version. As a result, my evaluation of this work and its potential contribution is higher than before. However, there are a few more issues, a few of them quite important, with which I believe the author(s) should deal prior to making the final decision. If the author can deal with these issues satisfactorily (and I suspect that it can be done), this work has a relatively high probability of being accepted for publication in *AMJ* (in my opinion).¹

On the other hand, reviewer 3 felt that the manuscript had actually gotten worse:

Overall, I am a bit disappointed with this revision. Many things which seemed to be clear in the first round now seem to be muddled. . . . I still like some of their ideas and findings, but there are just too many questions about the theory, the methods, and the data, and I think that a focused exploration of some issues might be more feasible and lead to a more pleasant paper with a clearer message.

The mixed reactions of the reviewers created a dilemma, particularly since *AMJ's* editorial policy encourages editors to make a final decision after the first revision. On the one hand, reviewers 1 and 3 continued to have serious concerns. On the other hand, reviewers 1 and 2 both believed that some real progress had been made. Although I had been optimistic about the submission, I continued to have reservations and could see the merits of the opposing positions.

At this point, I reread the authors' comments and compared the initial and revised manuscripts. It was clear that the authors had tried in good faith to embrace our suggestions. It also seemed that many of our continued concerns could be characterized as "issues of uncertainty" rather than fatal flaws. Although a revised manuscript can certainly be rejected on the grounds that insufficient progress has been made, I decided to give the authors another opportunity. They had engaged us construc-

tively, their manuscript had several unusual qualities, and the potential remained for a significant contribution to be provided. Another point in their favor was that the large scope of the manuscript suggested it was perhaps unrealistic to expect that everything could be resolved with just one revision. We needed another data point.

The tone of the second decision letter required some balancing between being too tough versus too easy. I did not want to err on the side of being too discouraging, as that might dampen the positive affect the authors needed to have to make a constructive response. Yet I did not want to be too encouraging, which might cause the authors to underestimate the additional effort required. Weighing these factors, I tried to write a forthright assessment that described the significant problems that continued to exist:

The reviewers and I appreciate your efforts to address our concerns and believe that the revised manuscript is improved. We see clear progress in your revised manuscript, and we continue to be intrigued by the model and the findings. However, I am sorry to tell you that some of our initial concerns still persist and that the extensive revision efforts have created some new problems. The reviewers' interpretation of this situation is rather inconsistent. One reviewer is satisfied with the revised manuscript, another is cautiously optimistic, while the third is becoming more pessimistic. These latter two reviewers believe that another major revision is needed before a final publication decision is made. I agree with all three reviewers; there is much to like about your manuscript, but I also believe that we need to see if you can resolve several troubling matters. Therefore, I am writing to invite you to revise and resubmit the manuscript for continued consideration.

I included most of the same warnings as before, noting in particular, that the revision should continue to be considered major and risky. The authors would have to clarify conceptual terminology, strengthen theoretical links, more clearly integrate theory and method, improve the transparency of the study and, again, develop the contribution. I wanted the authors to know that the next review would be the last one and that the editorial decision could still go either way. My letter was emailed on October 4, 2002.

The Third Submission: May 20, 2003

The twice-revised manuscript, now entitled "Knowledge Transfer by Inheritance: Spin-out Generation, Development, and Survival" was resubmitted on May 20, 2003. It was returned only to re-

¹ These quotations are, again, from the reviewers' private comments to the editor. We ask that reviewers *not* state their recommendations or estimates of likely publishability to the authors, leaving that assessment to the editor, who has considerably more information about the entire file.

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viewers 1 and 3, as reviewer 2 had officially signed off after the first revision.

My reading of the revised manuscript led me to be hopeful. Many of my own concerns had been addressed, and I felt that other weak spots had been strengthened. Much progress had been made. Still, I sensed that the reviewers might not give a consistent recommendation.

Reviewer 1 seemed persuaded by the revisions and evaluated the submission as satisfactory on several dimensions, including conceptual and technical adequacy and implications for practical significance. The reviewer also believed that the potential significance of the contribution was strong. The only concerns pertained to the clarity of the exposition. This reviewer offered a number of additional suggestions but concluded:

Overall, I am quite positive about this manuscript and its potential contribution. I feel that the potential value-added contribution of this manuscript is quite strong, perhaps one of the strongest that I have read in several years.

These comments really caused me to take notice, as reviewer 1 had been one of our most demanding and rigorous reviewers over the years.

Unfortunately, reviewer 3 held the opposite view, evaluating the manuscript as weak on most of *AMJ*'s evaluative criteria (conceptual and technical adequacy, theoretical significance, and implications for practice). In general, reviewer 3 thought that key arguments were murky and not sufficiently developed, that the logic was not entirely convincing and, most importantly, that some of the key measures might be biased. Nevertheless, the reviewer still believed this research had promise, even if s/he didn't feel that it had yet optimized its potential. This situation, reviewer disagreement, is not uncommon, even with submissions at the third round of review. The manuscript on spin-out generation had changed in its form and purpose, was pushing new conceptual frontiers, and was very complex. Such major developments over the revision cycle often produce varying reactions, some supportive and others not. Editors at *AMJ* rely on reviews as inputs into the editorial decision, but we do not simply go with a majority vote.

I went through both sets of reviewer comments carefully and found that each and every comment had merit. However, I felt that the most significant concerns reviewer 3 raised were ones that we could live with (given some additional work by the authors) and that the type of contribution the manuscript offered would still be acceptable even if the continuing problems weren't completely resolved. I reread the manuscript a few more times to ensure

that I was comfortable with it before writing the conditional acceptance letter:

Overall, your revised manuscript now satisfies two of the reviewers and they recommend acceptance subject to some minor revisions. The other reviewer remains critical about the manuscript's potential to reach the *AMJ* publication standards. My own reading has led me to agree with many of the concerns raised by the remaining reviewers, and I agree that some work still remains before this manuscript is finished. However, I also believe that you have adequately resolved our more serious concerns and that the remaining ones can be addressed in a satisfactory manner.

I was unsuccessful in my efforts to reach the authors by phone to congratulate them. Somewhere, they would check their e-mail and get the marvelous news.

The authors revised their manuscript once more, sent it back to me, and I wrote a final acceptance letter on December 7, 2003, nearly two years to the day after their initial submission. The article subsequently appeared in the August 2004 issue.

POSTSCRIPT: LOOKING BACK, TWO YEARS LATER

As I reflect back on this review process, several key points emerge that might be useful to consider. A caveat first: I cannot provide comments on how to write an award-winning *AMJ* article, as I was not a member of the award committee. But I think we can learn from the authors' experiences about how to successfully navigate a stringent evaluation process.

First, the reviewers played a vital role in the development of this manuscript. It follows, then, that authors should try to help editors to identify the best reviewers for their work. Authors can do this by specifying clearly how their work builds on earlier contributions. This step might help editors choose competent reviewers who are more likely to take the extra step to push the development of a manuscript. I think that Tom Lee deserves much credit for assembling a very strong board from which to draw such reviewers.

Second, authors help themselves by engaging reviewers in a positive and constructive way. Some authors view reviewers as adversaries to be taken on in the review process. That approach is not usually very effective. Reviewers have a right to their reactions, and most will see a manuscript in a different way than its author anticipated. In the case of the manuscript discussed here, the differences between the initial and final versions were very considerable. We had three very smart and

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capable reviewers who carefully critiqued the manuscript, and four energetic authors who successfully engaged them. It seems only natural that the interactions among them would produce a better product. Instead of trying to impede these developments, authors can benefit from working with them. This is not to imply that authors need to accept every reviewer suggestion, but rather to suggest that authors engage reviewers in a thorough and active manner.

The authors' statement in this editors' forum, "REAP REWARDS: Maximizing Benefits from Reviewer Comments," provides some exemplary ideas about how to address reviewer concerns. In addition, as the editor of their submission, I might be able to offer some other insights about how we might all learn from their experiences. For example, although the authors' revisions produced a stronger manuscript in the end, they struggled with reviewer 3 throughout the process. I suspect this may have happened for two reasons:

- As described in "REAP REWARDS," the authors' revision process was heavily focused on specialization and production. To some extent, the fact that this was a four-person, geographically dispersed author team may have necessitated the production focus. However, the authors' chosen method of having subgroups revise individual pieces of the manuscript, even though they all collaborated subsequently, may have created a manuscript that left reviewer 3 struggling to see how all of the pieces fit together.
- The authors note that they made a decision to treat some reviewer comments as "tangential." This raises a larger issue: how to evaluate and respond to reviewers' comments, as authors of revised manuscripts have to decide how they are going to address the reviewers' concerns. And I understand that not all reviewer comments have the same weight and consequence. In my experience, authors usually benefit when they are careful about how they characterize and respond to a reviewer's points. My sense is that it is in an author's best interests to treat all reviewer comments objectively and equally. Of course, there are always some reviewer points that an author will see as minor and/or not agree with or wish to incorporate. In those situations, I suggest that authors indicate in their letter to a reviewer how they thoughtfully considered the pertinence of his or her comment and then positively engage the reviewer by explaining their position. Authors can disagree with reviewers, and they are likely to be the most persuasive when they provide a clear and developed argument for their

position. Remember, the editor is the ultimate decision maker and will be reading both the reviewer and author perspectives. It is to the authors' benefit to find a way to constructively communicate to reviewers how each and every one of their points was considered.

A third piece of advice about navigating the evaluation process we can take from the history of Agarwal and colleagues' experience is that a reviewer has responsibilities to both the authors and the other reviewers of each paper. I think the spin-out manuscript was successful not only because of the authors' acceptance of and commitment to the reviewers' observations, but also because the reviewers were similarly involved in a positive, constructive, and developmental manner. Although authors and reviewers may tend to view each other suspiciously, neither party fell into this trap at any stage in the review. To be sure, the manuscript had numerous and impressive strengths. But only through the commitment, sweat, and dedication of the reviewers and the authors did it survive an arduous review. It would not have been accepted if the pieces of the puzzle had not all eventually fitted together. All the parties deserve credit for the outcome.

Fourth, the review process can be humbling. Reviewers probe nooks and crannies, and issues arise that may not have been foreseen or anticipated. Reacting to reviewer comments will likely lead authors to drop some pet ideas while developing others. I think these authors were successful in part because they were willing to suspend their own egos and work with the reviewers to build a stronger manuscript, as indicated in their own description of the review process. I applaud them for their positive attitude and commitment toward that goal.

Fifth, authors can help themselves by pushing harder on their ideas before the initial submission of their manuscripts. I am not criticizing the authors of the paper discussed here, but noting that one of the persistent challenges was defining and explaining its contribution. I think the problem arose because the contribution became a bit of a moving target; the proposed advance was clarified and developed throughout the successive stages, and this trajectory forced all to think through the potential of this manuscript. Since *AMJ* requires both theoretical and empirical contributions, I strongly urge reviewers and authors to think hard about how study results inform explanations. This requires thinking at theoretical and empirical levels. It also requires clarity in thinking about how theory is being used in a manuscript. Is theory being extended in any important way? If not, can it

be? Doing this work up front might help some authors get more favorable reactions to their work in early stages of submission. I am fortunate to have had opportunities to work on many manuscripts, and I am also lucky to have edited an award-winning *AMJ* article. One might wonder what differentiates a merely good from a truly distinguished article. What initially struck me about this award winner was its focus on a unique organizational action and form. We researchers know very little about spin-outs, either what they are, what motivates them, or what implications they have. So one key distinction this article had was its novel topic: the reviewers and I all learned something new from reading it. Another differentiating feature of this article was its data. The authors developed a data set that was unusual and rich. It was obvious that they had invested significant resources into constructing their data and did not look for easy ways out. Moreover, these authors worked especially hard with their data to get new insights, going above and beyond what may have been their initial intentions. Far too often I have seen great data sets squandered on pedestrian or less interesting ideas.

In addition, the article drew on a collection of related thoughts, rather than having a narrow, highly focused idea and argument. The breadth of this article produced a range of potentially new research opportunities spanning several different topic areas, including strategy, entrepreneurship, organizational behavior and theory, technology management, and the influence of founding conditions. Further, the article integrated recent and traditional perspectives into a cohesive framework and an insightful set of arguments. Finally, the contribution of this article became more developed and substantive with each iteration. I urge authors, reviewers, and editors to always look hard for the glimmer of potential in every submission.

I have often been asked how authors can best deal with reviewers who want to challenge their ideas. I sense that many authors fear that reviewers will have different visions of what their papers should look like and how they should be written and that the only way to be successful is to acquiesce. I think the answer to this important concern involves two points: (1) The best ideas and arguments should prevail, and when the author-reviewer discourse is respectful, clear, and honest, the best ideas will rise to the top. (2) It is critical that both parties work toward the same goal and do not compete against each other. As with any negotiation, once the parties are joined, great things can happen—and did, in the case discussed here.

I congratulate the authors on the success of their article. They had something very rare: a set of excellent ideas, unusual data, and an ability to work very hard and effectively. I believe that the combination of these factors led to the success of their submission.

I also extend my thanks and admiration to Tom Lee and the reviewers, who worked anonymously so a greater good could be accomplished. I hope that each of you knows how much you mattered.



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