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Another reason is that the journal review process is central to each scholar's growth and development. Thinking back over my own publication experiences, the high and low points were associated with journal reviews. A number of reviews were absolutely devastating. The reviewers seemed determined to be destructive, hurtful, and narrow minded. But I have also been buoyed, supported, cheered, helped, and encouraged by reviewers, and constructive criticism has improved my work dramatically. The review process can have enormous impact, either positive or negative, so it seems important to share views about it.

A final reason for analyzing the review process is that there are several points that need to be made, some tricks of the trade that should be passed on to authors. I find that reviewing is more subjective than objective. Manuscripts give off many cues, and these cues form a gestalt. Factors that influence this gestalt include such things as writing style, tone, and method of theory building. Subtle, intangible cues often cause me to like or dislike the paper, and hence to support or not support the paper for revision or publication. These intangibles are hard to put a finger on, and they are crucial to the paper's acceptance, yet are hard to explain in the written review given back to the author. The intangible side of the review process needs to be analyzed as one way to help authors get their work published.

The purpose of this chapter is to present my perspective on the review process. Because this is my personal perspective, I will put my biases on the table. My training was at the University of Chicago, where I was imprinted with the belief that the goal of research is theory development. Data collection and analyses are important, but data are intended to illuminate a path of insight into organizational behavior and processes. Theory gives meaning to data. I can also say that I am challenged and excited by the review process. I enjoy sharing my views and suggesting ways authors can improve their papers. I have been reviewing papers for journals for about six years, and I am not tired of it. Each paper is a new challenge. I enjoy the review process.

In this chapter, I will present an analysis of my reviews for journal manuscripts, and I will propose seven guides for overcoming common manuscript problems. My analysis and suggestions are written with the desire to shorten the publication cycle for colleagues, to crystallize some of the intangible elements that annoy and turn off reviewers, and to facilitate those high points of science—those successful researcher-reviewer transactions—that are exciting and constructive and lead to the publication of new ideas and important discoveries in the organization sciences.

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Why I Recommended That Your Manuscript Be Rejected and What You Can Do About It

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No one can learn to write an excellent paper based on examples of failure. No one can expect to have a paper accepted at a major journal by hearing about papers that have been rejected. Research and publication are learned through trial and error. Scholars learn by doing. Yet this chapter is about failure, the reasons for failure, in the journal submission process. Why concentrate on the shortcomings of papers previously submitted to journals in the organization sciences? There are several reasons.

For one thing, we are a community of scholars. In a community, people learn from one another. By sharing previous errors, the number of trials required for new scholars to publish their work may be reduced. Moreover, examples of excellent papers are already in the journals. The good papers are out there for everyone to see, but many colleagues do not have insight into the problems, mistakes, revisions, and previous rejections associated with excellent publications.

ANALYSIS OF REVIEWS

The approach used to bring order to my observations was to analyze the content of my own reviews of journal submissions. The reviews were limited to manuscripts submitted to *Administrative Science Quarterly* and *Academy of Management Journal* because these journals are in the mainstream of the organizational sciences. Most papers were on organization theory topics, although a few were in closely related areas such as business policy. Most papers were empirical and reflected traditional fieldwork methods, although several used what would be called qualitative methodology.

The sample for my analysis included 111 reviews over the last four years. Some overlap existed among these reviews because several papers were reviewed a second or even a third time. The revised manuscripts were included as separate entities in my analysis because a paper's gestalt can change substantially with a major revision. Solving one problem often calls attention to other problems.

My procedure was to read each review and note up to three reasons the paper was weak and needed a major revision or was rejected. The reasons listed were then consolidated into categories. The categories grossly oversimplified the unique characteristics of each paper, but the categories do identify common problems that existed in the papers sent to me by *AMJ* and *ASQ*.

Why I Recommended That Your Manuscript Be Rejected

The results from the analysis of 111 manuscript reviews are in Table 9.1. Table 9.1 lists 11 problems and the frequency of each problem. The content of these problems is described here.

No Theory. Theory means explaining what the variables mean and why they are related to one another in organizations. Fully one half of the papers I reviewed had little or no theory to explain relationships among variables. Theory need not be formal or complex—theory should simply explain why. Theory provides the story that gives data meaning. The measurement of variables, procedures for data collection, and techniques for data analysis are important parts of the research process, but they are not sufficient for publication. The essential point of research is to provide an understanding about human behavior and processes within or between organizations. The purpose of theory is to interpret data to provide insight into real behavior.

TABLE 9.1 Problems Found in 111 Manuscripts Reviewed for *AMJ* and *ASQ*

Problem	N*	Percent of Problems	Percent of Manuscripts
1. No theory	56	(21.7)	(50.5)
2. Concepts and operationalization not in alignment	35	(13.6)	(31.5)
3. Insufficient definition—theory	27	(10.5)	(24.3)
4. Insufficient rationale—design	27	(10.5)	(24.3)
5. Macrostructure—organization and flow	26	(10.1)	(23.4)
6. Amateur style and tone	23	(8.9)	(20.7)
7. Inadequate research design	22	(8.5)	(19.8)
8. Not relevant to the field	20	(7.7)	(18.0)
9. Overengineering	11	(4.3)	(9.9)
10. Conclusions not in alignment	6	(2.3)	(5.4)
11. Cutting up the data	5	(1.9)	(4.5)
	258	(100)	(100)

*N = 258 major problems identified in the 111 manuscripts.

Consider, for example, a hypothetical study of resources, environmental contacts, centralization, and the introduction of new products. The investigator may hypothesize that fewer slack resources will be related to greater centralization and fewer environmental contacts, and environmental contact in turn will be positively related to new product introductions. The role of theory is to explain why these relationships exist. Perhaps resource scarcity leads to conflict among departments so that managers are forced to centralize decision making. Centralized decision making might mean that employees feel less responsibility for contact with customers. Customer contacts may be an important source of ideas for new products, so fewer contacts would mean fewer ideas and fewer new products.

This story could be developed in more detail, but some type of story must explain the relationships among variables. So many manuscripts miss the essential point of research, which is theory construction. Without a theory, there is nothing to pull the study together, nothing to justify why the variables should be studied. Simply reviewing the literature and showing that each variable appeared previously is not enough. The theory organizes the variables into a set and is the basis for new insight into organizations.

Concepts and Operationalization Not in Alignment. The frequency (35) of this problem surprised me because it seems so obvious, but often the operational base of the research did not reflect the variables or model under study. Sometimes

level of analysis was the problem. The investigator might propose to study organization technology and structure. Then the investigator surveyed individuals in a single organization and analyzed the responses for individuals rather than for departments or the organization as a whole. The sample thus precludes any opportunity to learn about the relationship between organization-level technology and structure.

Other examples of poor alignment included the use of number of hospital beds as a measure of organizational complexity, and percentage of university graduates as a measure of organizational control. Number of beds is an indicator of size, and size may be associated with complexity, but using beds as a measure of complexity requires a thoughtful and convincing rationale. To some extent, educational level may be associated with the extent of clan control, but educational level means a number of other things as well. Simply calling a variable "complexity" or "control" does not make it so, especially when the operationalization measures another concept.

No operationalization is perfect, and perfection is not expected. But authors often did not select measures or a sample to fit their concepts. Manuscripts sometimes read as if new labels were created for old data in the hope of getting published. But to attain publication, investigators have to maintain congruence between concepts and operationalization, between theory and research design.

Insufficient Definition—Theory. Insufficient definition is similar to the notion of no theory but is even more basic. This problem occurred when authors did not explain what the concepts meant. Authors did not provide definition, explanation, or reasoning for some of their variables. Instead, authors simply proposed variables because of appearance in previous studies or because the variables seemed like a good idea. If administrative ratio had been reported in previous publications, that was offered as sufficient rationale for studying it again, and the reader was expected to know what it meant and why it was important.

Concepts in the social sciences are fuzzy, and an explicit definition is usually required to let the reader know exactly what is meant. In a study of information processing, it helps to define information and to say how it differs from data. If the study pertains to information load, information density, or information form, each of these concepts must be made explicit. Frequently a "correct" definition is not available in the literature. The author should enact a definition. Otherwise the reviewer is in the dark about what the author is thinking and studying. Defining exactly what each variable means is an important part of the theory construction process.

Insufficient Rationale—Design. Again, insufficient rationale was a problem, but in this case the manuscripts lacked explanation of study procedures. The author should introduce the reader to the true operational base of the research. Simple things, like describing the sample, saying who completed the questionnaires, providing example questions from the questionnaire, and reporting means and standard deviations, all bring the reader close to the basic data. If, for example, the author elected to sample one firm in each of three industries and to survey 20 managers in each firm, the reasons for those selection decisions should be explained. Nothing is obvious to me as a reviewer. The author has to explain why the sample and procedure are appropriate to test the proposed research question.

The absence of rationale about design issues was frequently a cause of my conclusion of poor alignment between theory and method. Without full disclosure and openness about method, I could not understand whether the method was appropriate. For example, in a study of ideology, routine versus nonroutine technology was used as the measure of ideology without careful explanation, and in a study of business strategy the presence of a large computer in the organization was used as a measure of strategy. These design decisions must be explained. Without adequate rationale, the author's logic is suspect, and the reviewer is likely to reject the paper because the research procedures are unclear.

Macrostructure—Organization and Flow. Macrostructure means whether the various parts of the paper fit together into a coherent whole. *Macrostructure* pertains to individual sentences and paragraphs, which are satisfactory in most papers. But the macrostructure is a harder problem to solve. The theory portion of the paper may make sense by itself but be out of alignment with the conclusion section. The results section may be well written but not test each hypothesis proposed in the theory section. Scholars must make a special effort to visualize the entire paper—especially the interconnections among the parts—and be confident they are effectively constructed before submitting the paper for publication.

A number of clues indicated macrostructure problems in manuscripts I reviewed. The author might introduce measures in the method section for variables that were not identified in the theory section. Occasionally an author introduced new tables and analyses into the conclusion section, almost as an afterthought. Frequently the results section did not explicitly test each hypothesis raised in the theory section. Or the conclusion section might draw conclusions about theories and variables (e.g., organizational effectiveness) that were unrelated to the paper's explicit research question (e.g., information processing). In most cases the

author saw an implicit connection, but the reasoning was not made explicit to the reader.

Other indicators of macrostructure problems were an insufficient number of subheadings to provide an obvious road map for the trip through the research, frequent parenthetical statements or footnotes to explain things (frequent parenthetical statements are distracting), asking the reader to see other papers in order to understand what something meant (see Campbell, Daft, & Hulin, 1982, for details), referring ahead to future parts of the paper for explanations (I will explain this point in the conclusion section), or simply submitting a paper that was far too long for the research at hand.

Any of these elements gives the paper a disorganized, poorly conceived look. A good paper is extremely disciplined. A good paper does not jump around, is internally congruent, and doesn't open up new areas late in the paper. Author self-discipline is needed because the study itself may have been conducted in a disorganized way, as is most research. But that disorganization must be removed for the reader to understand what happened. The paper should take the reader from A to Z in a logical sequence without deviations. Then the paper can tie back to A in the final section by summarizing what new has been learned about the research question.

Amateur Style and Tone. Style and tone are intangibles, but they have enough impact on me as a reader to sometimes cause rejection. Style and tone can signal that the authors do not know what they are doing, that they are amateurs. One indication of amateurism was contrived emphasis—the frequent use of underlining or exclamation marks. If the point is made properly, contrived emphasis seems very silly and inappropriate, and actually *takes away* from the point. Another problem was the use of “straw men.” The importance of the research topic was grossly exaggerated to make the case for publication. One example was the argument that bureaucracy should be studied because bureaucratic processes are oppressing individuals in all organizations. The paper was written in direct response to Weber and ignored all the literature in between (loosely coupled systems, informal organization, garbage can model) that indicates bureaucracies are not as tight as Weber proposed. The avoidance of exaggeration is so critical that authors must understand it or they will never be published again!

Yet another indicator of amateurism was an overly negative approach to the previous literature. Authors often tore down previous work to justify their study rather than show how their paper built on previous findings. (That is, the reason this chapter is so good is that the other chapters left out many ideas, are poorly

developed, and their databases are smaller and less accurate than mine.) Previous work is always vulnerable. Criticizing is easy, and of little value; it is more important to explain how research builds upon previous findings than to claim previous research is inadequate and incompetent. A related problem was when amateur authors wrote as if their research project were going to correct all previous findings on the topic. They believed their study was going to prove once and for all that organization size was related to formalization and administrative ratio, or some such thing. The authors did not acknowledge the realistic limitations of their own research. Yet their findings were a function of their specific sample and measurement techniques and were not any more valuable than the previous research that was supposed to be corrected.

Inadequate Research Design. When this problem appeared, it was typically fatal. Design cannot be corrected because the research has already been executed in an invalid manner. Graduate schools must be doing something right, because this problem appeared in only about one fifth of the manuscripts I reviewed. Sometimes the true problem was lack of explanation. On the other hand, additional explanation often revealed the paucity of the design. But only about one fifth of the papers were rejected due to unsolvable design problems.

An inadequate design revealed itself in various ways. A closed-ended questionnaire survey was mailed out to a random sample of managers to study subtle and intangible political or decision-making processes. Survey questions cannot capture these equivocal processes, and the whole procedure lacked face validity. An investigator surveyed top managers and asked questions pertaining to details of departmental activities and technology about which the respondents would have little information or insight. Another example was to use an undergraduate student sample to analyze the selection of business strategies by corporate executives. Undergraduates have virtually no experience at upper levels of organizations, and they often have a hard time even understanding strategy concepts. To use undergraduate students as representative of senior managers is grossly inappropriate. In each of these examples, the design error was basic and major, the study lacked validity, and the problem could not be corrected after the fact.

Not Relevant to the Field. Sometimes papers simply were inappropriate or irrelevant to the organization sciences. Sometimes papers were written from a finance or economic orientation, almost as if the papers were rejected from journals in those disciplines and were retooled toward organization theory as a way to get published. These papers typically lacked depth and insight for organization the-

ory questions. Sometimes papers had a strong mathematical base and attempted to understand organizational processes through mathematical proofs. This approach was valid enough but was of no value if the author did not discuss organizations or organizational relationships. Some papers simply came across as a rehash of old issues. No single flaw killed the paper, but the parts did not add up to sufficient new knowledge to warrant publication.

One hidden factor that influences a paper's contribution is the maturity of the topic matter. Research topics behave like the product life cycle described in marketing. When the topic is new, a lot of research activity is generated, and most projects contribute new knowledge. But as the product matures, and a large number of studies have been published, it becomes more difficult to conduct a study that produces genuinely new insights. In organization theory, size and administrative ratio is a mature topic that has been overstudied. In organizational behavior, the topics of motivation and job satisfaction have matured. A new study on a mature topic may use a novel sample or organizations, or include a new variable or two, but the insight into organizational processes is typically small. The case for publication is easier if the topic is new, fresh, and poorly understood rather than mature and overstudied.

Overengineering. Sometimes authors overdid methodology so that it became an end in itself. The strength of the study was the operationalization of perhaps 50 or 100 variables. Or perhaps the authors used exotic and sophisticated statistical techniques to analyze data. In this type of manuscript, the engineering mechanics were emphasized to the exclusion of what the data meant. Sometimes the case for publication could be made for a well-engineered study, but typically the emphasis on engineering took away from the underlying theoretical contribution. As data were combined through factor analytical techniques and were run through interactive data analyses, their meaning was further and further abstracted from the operational base of the organization sample. Sophisticated techniques are fine, but when the concepts become far removed from organizations, new insight into organizational processes is impossible. The ultimate justification for a study is to learn about organizations. Simply measuring and manipulating variables, no matter how sophisticated the techniques, does not provide new understanding sufficient for publication.

Conclusion Not in Alignment. This problem occurs just often enough to be worth mentioning. A publishable paper should have a strong concluding section that tells the reader what the findings mean. This section should interpret the find-

ings, show how the data add to or modify the original theory, and state explicitly how the study adds to the developing knowledge base within the field. Sometimes the conclusion section was limited to a paragraph of the papers I reviewed. The authors wrapped up as if they were in a hurry to get away from the research. They left it up to me to figure out what the findings meant. Other times the conclusion section generalized far beyond the data. Some generalization is important, because authors need freedom to think beyond the data. But jumping into unrelated topic areas, or using findings from a single study to reorganize the field, typically struck me as unrealistic. The discussion should not become too far removed from the operational base of the research. Some statements about research limitations is also worthwhile, but the concluding section should not dwell on methodological issues. The important thing is to use the conclusion section to fully develop the theoretical contribution and to point out the new understanding from the study. The conclusion section should build on and be congruent with previous parts of the manuscript. The conclusion section deserves as much attention as the theory, method, and results sections, because the conclusion section explains what it all means.

Cutting up the Data. This problem occurred when the paper under review for one journal overlapped by 80% a paper under review for another journal. Sometimes the paper contained the same data as previously published papers but under somewhat different names or with slight modifications. This did not happen often, but when it did the impression on me was terrible. Other reviewers and I called it to the attention of the editor, who immediately went back to the author. Attempting to multiply publications from a single database wastes everyone's time and is a breach of professional ethics.

There are well-established precedents for publishing multiple articles from a single database. The Aston group studies of organization structure during the late 1960s and the early 1970s are an example. Each article was a complete meaning unit that contained a significant portion of the overall study and was directed toward a specific theoretical topic. Follow-up papers made explicit reference to previous publications and stated exactly how the new research added to the previous paper in a building block manner. When this procedure is followed, reviewers have no problem with multiple publications from the same database, and indeed will admire the author for undertaking a large study. But when a small study is analyzed to death to get multiple publications, everyone involved is left with a bad taste.