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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.

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.

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. *Microstructure* 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 findings, 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 statement 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.

THE QUALITATIVE COUNTERPOINT

The above discussion assumed a traditional, theory-based, hypothesis-testing approach to empirical research. But an increasing number of qualitative studies are being submitted to such journals as *Administrative Science Quarterly* and the *Academy of Management Journal.* I reviewed several manuscripts that used qualitative procedures. The major shortcomings in qualitative manuscripts were the same as for traditional research, but the problems were revealed in a different way. The two biggest problems, lack of theory and incongruence between theory and method, can be understood by comparison with traditional methods.

No Theory. The single biggest problem I found with qualitative research was lack of theory, which surprised me because the purpose of qualitative research is to build theory. The problem was that the researchers did not define new concepts or create new theory. In qualitative research, concepts and models should be defined at the end of the manuscript. The point of going out to observe organizations is to construct theory based upon the investigator's observations and interviews. The research goal is to end up with a well-defined set of constructs and a model that can be used to guide future research.

The same rule applies to both qualitative and quantitative research—theory is more important than data. Theory is the contribution to knowledge. Researchers should use the paper to crystallize a model that explains their observations. Many authors seemed too timid to stick their necks out, to go beyond the data, to enact a model from the myriad details they observed. Without the final model, the paper came across as long and meandering without a point, without a conclusion. The paper was rejected not because referees did not like qualitative research but because the investigators had not used the manuscript to build theory, which was the purpose of the research in the first place.

Concepts and Operationalization Not in Alignment. Practically every reviewer will agree that qualitative procedures are as valid as quantitative procedures. But qualitative procedures have to be tailored to the right research problem.

Most research projects can be placed on an imaginary continuum that reflects the extent of previous theory development. On one end of the continuum are research topics for which a great deal of previous research and understanding exist. Theory is well developed, so the goal of new research is hypothesis testing. To test explicit hypotheses, data have to be gathered in a sufficiently quantitative way to permit systematic comparisons and hypothesis rejection.

The other end of the imaginary continuum reflects research topics that have little previous theory development or systematic knowledge. The goal of research on these topics is to develop a theory or model for future tests. A successful research outcome is a tentative model based on observation and conjecture that can facilitate new research on this topic.

Quantitative procedures work well for topics on the hypothesis-testing end of the continuum. When frameworks are explicit and hypotheses can be tested, it makes sense to gather data in a quantitative way so the hypotheses can be accepted or rejected based on statistical tests. Qualitative studies, on the other hand, fit on the exploratory, theory-building end of the continuum. Qualitative procedures provide the freedom to ransack one or more organizations for new ideas and to consolidate these ideas into a plausible model.

The design problem occurs when the wrong procedure is used. When there is an extensive research literature so the author can formulate explicit hypotheses, then a loose, open-ended, qualitative procedure is not adequate to accept or reject the hypotheses. The procedure seems impressionistic; qualitative findings are too vague to contribute new knowledge to a well-defined topic. At the other extreme, when quantitative studies are used to study topics that are poorly developed, the result is premature rationalization and oversimplification. The quantitative procedure lacks the richness to build new theory. Thus, when qualitative procedures are used to test explicit hypotheses or when quantitative procedures are used to explore new topics, the research design came across to me as inadequate for the research problem.

Learning to Play Golf

The 11 items in Table 9.1 capture the bulk of the serious problems discovered in the 111 manuscripts I reviewed for *ASQ* and *AMJ.* There are also two overall patterns in the data that should be noted.

Theory Versus Design. The first pattern is the disparity between theory development and research design as the cause of failure, as indicated in Table 9.2. A problem with theory was five times more prevalent than a problem with design. One reason theory was a frequent problem is that theory building is hard to learn. Textbooks tell us how to design studies, but theory is learned through experience with both organizational research and real organizations. Theory requires both creativity and practical insight. One reason many authors had trouble explaining what the data meant or writing a story about the relationship among variables was that they had never seen the phenomenon about which they wrote. The

TABLE 9.2 Theory Versus Design Problems as the Cause of Manuscript Failure

Theory development problems	114
Research design problems	22
Total	136

authors learned to do research from behind university walls and had no face-to-face experience with the organizational subject matter. Theory building is more difficult to learn than research design because it does not come from textbooks or classrooms. But theory building can be mastered through experience and effort once researchers see that their contribution to knowledge comes from explanation, interpretation, and theory construction.

Three Skills. The second pattern pertains to the skills needed to produce winning research. I have read that an excellent golf game requires the mastery of three separate games—the woods, the irons, and putting. Each part of the overall game requires a different skill, and all three skills must be mastered to be a top pro. Weekend golfers may do well on one or two parts of the game, but they do not excel at all three.

The golf analogy fits the research game. Three skills are needed to be a top pro in the organizational sciences—theory skills, writing skills, and design skills. Manuscripts are rejected because of insufficient skill at one or more parts of the research game. Table 9.3 organizes the data according to the skills needed to correct the deficiencies I found in the manuscripts.

For 149 of the criticisms, the publication solution was through additional theory building skills in the authors. Greater skill with theory would enable the authors to go beyond their current manuscript with respect to definition of concepts, telling the story that connects the concepts to one another, clarifying how concepts relate to the operationalization, and showing why the study contributes to the field of organization sciences. An important point is that many of the 149 problems could have been prevented before journal submission if the researcher had worked at theory building and acquired the skill. Learning to build theory takes time and requires several revisions and feedback from colleagues, but theory building skills can be learned.

Writing skill was the reason for 82 criticisms. Writing pertains to how things were expressed. The act of putting words into sentences or forming sentences

TABLE 9.3 Skills Needed to Overcome Manuscript Problems for Publication

	*N**	*Percent*
Theory Skills (define concepts, enact models, write stories, develop theory, integrate variables)	149	59
Writing Skills (flow, style, tone, integration of parts, rationale, openness, A to Z then back to A)	82	32
Design Skills (inadequate method, wrong method, no validity)	22	9
	253[a]	100

*N = 258 major problems identified in 111 manuscripts.
a. The five cases of cutting up the data are not included in the table.

into paragraphs was generally OK. The problem occurred when the paragraphs and sections did not convey specific insights to me and other readers, or when the sections did not complement one another. Writing skill means that concepts and issues are conveyed to the reader with precision and clarity. Writing skills are used to convey the rationale for design choices. Writing skills provide a consistent organization and flow, a professional tone, and a tight integration of theory, method, findings, and conclusions.

Finally, only 22 criticisms reflected poor research design skills. Research design skills were better developed than theory or writing skills for the manuscripts I reviewed. When poor research design occurred, however, the paper could not be improved through additional revisions or skill acquisition by the authors.

A paradox in Table 9.3 is that most problems were preventable. Theory and writing represent 90% of the problems, and they could be overcome through practice and revision. Yet only 10%-15% of manuscripts are accepted for publication. The explanation seems to be that researchers cannot learn theory building and writing skills easily. These skills take time, commitment, hard work, and an extension of self beyond the techniques learned in graduate school. These skills are not analyzable and teachable, as experience and practice are crucial, just as golfers must play every day to learn driving, approach, and putting skills.

AND WHAT YOU CAN DO ABOUT IT

Authors can do a number of specific things to increase their skill level and the probability of satisfying reviewers. The guidelines below can help authors over-

come most of the problems identified above. Some of these guidelines are quite easy to follow; others are more difficult. But each guideline is an explicit point against which a manuscript can be compared before it is submitted to a journal. Authors can check whether they have gone as far as possible toward accomplishing theory and writing skills.

Tell a Story

One technique I have found to overcome the lack of theory in a manuscript is for authors to think of each variable in the study as a character in a story. The author's responsibility is to fully describe each character, and then to explain how and why characters interact with one another. Storytelling explains the "why" of the data and gives meaning to observed relationships. Storytelling is difficult because we are trained to be rigorous and precise and to stick to the data in literal fashion. Storytelling requires conjecture and going beyond the data; it is the opportunity to fill in the blanks between variables. The story provides a larger framework within which each variable has a logical place. The explanation gives us insight into organizational processes. The story explains the why of relationships in organizational terms. The why is important, and researchers should be creative and ruthless in pursuit of it to solve the theory problem (Weick, 1974). The why, not the data, is the contribution to knowledge.

Another aid to storytelling is a visual figure or model. If the investigator creates a contingency table or series of boxes and arrows to summarize the theory, then storytelling is easier. The story explains the reasons behind each box and arrow. For traditional hypothesis-testing studies, a visual representation strengthens the early part of the manuscript and can be revised in the conclusion section. For qualitative studies, a visual representation at the end of the manuscript is an ideal way to crystallize the theory developed from personal observations.

Discuss Fully Your Procedures and Thought Processes

Openness is a refreshing and positive characteristic in a manuscript. Many authors seem to hide the details of their research procedures, as if they fear that reviewers will criticize and reject the paper for those weaknesses. There is no better way to defuse criticism than to admit the weaknesses and point out the problems. Describing thought processes and rationale may be difficult when decisions were made intuitively. But the rationale for the selection of variables, design decisions, and analysis procedures needs to be explained. Reviewers gain a more positive impression from full disclosure, including weaknesses and problems, than from partial disclosure that appears to hide something.

Concentrate on Macrostructure

Many problems in a manuscript are caused by poor coordination among sections of the manuscript. The theory has to be congruent with the method, the method with the results, the results with the discussion section, and all sections with each other. The paper should flow logically in a straight line of thought, without digression. Each section should come across as a self-contained unit, and the sections must add up so the entire paper is a coherent meaning unit. Most writers find the macrostructure hard to manage, and several revisions may be required. If the macrostructure is clear, the microstructure will normally take care of itself. Make sure the overall logic and flow are in order before sending the manuscript to a journal.

Find the Operational Base of Your Research and Stick to It

The core of an empirical research paper is the operational base of the research methodology. The theory, results, and discussion must all correspond to the operational base. Authors who think in terms of the operational base make a clearer presentation than authors who think in terms of abstract concepts. Descriptive information on the organizations in the sample, questionnaire items, means, standard deviations, procedures, and all relevant details about the operational base should be included in the manuscript. Other parts of the paper should correspond to this operational base. If the study measures organizational size in the method section, there is no reason to propose a hypothesis about organizational complexity in the theory section. If the operational base measures technology, there is no reason to write a concluding section about organizational ideology. The operational base is the focal point for the research, and the other parts of the paper must be coordinated with it.

The operational base should also be reflected in writing style and interpretation. For a questionnaire survey that reports a positive correlation between elements of structure, the finding can be accurately reported as follows: "Perceived formalization was associated with perceived decentralization." The operational base does not measure actual constructs when we rely on the perceptions of others. Correct operational interpretations and appropriate phrasing suggest that researchers are in touch with the true operational base of the study.

Listen to Your Reviewers

Authors become supremely involved in the internal logic of their manuscripts. They are often dismayed when reviewers do not receive the message that was sent. Reviewers provide important feedback because they do not know the

background of the study; they see only the written manuscript. Reviewers evaluate it with the cold eye of objectivity. If the author's message does not get through, then the message should be changed. Do not blame the reviewer. Reviewer feedback can help you revise the manuscript toward the right message and content. Remember, reviewers are on your side. Reviewers are practically out searching for good material; they enjoy helping transform a good paper into an excellent one. Also keep in mind that sometimes a paper simply is not very good. Sometimes a design has flaws that cannot be corrected. Research is trial and error, and some trials don't work. When the study is flawed, reviewers will be quick to point it out, but the flaw is not their fault. Incorporate their suggestions as best you can. If the paper is unpublishable, use it as a learning experience and move on to the next research project.

Allow the Manuscript to Ripen Naturally

The analysis of manuscript problems in Table 9.3 indicated that most papers have difficulty with theory development and writing. These skills require thought and practice rather than mechanical technique. Theory development takes time; good theory follows a maturation process. The same is true for good writing. It takes time to draw out the implicit reasoning underlying research decisions, to see all the insights, to discover all of the important factors within the research. With each revision, the paper ripens. Expose your paper to the fresh air and sunshine of collegial feedback. With each discussion, new ideas emerge. The ripening process is facilitated with hard work and frequent revisions. When a paper fares badly with reviewers, the paper was still green—it probably was submitted prematurely. Allow the paper to ripen naturally with the passage of time, lots of sunshine, and many revisions as a way to develop theory and writing skills.

Don't Exaggerate

An underlying law of reviewer reaction is that understatement is more persuasive than overstatement. Nothing kills an argument so quickly as exaggeration. Nothing will motivate a reviewer to find flaws like overstatements and contrived emphasis about research results and importance. Exaggeration tells the reviewer that the author is not in touch with the true base of the study. Understatement, by contrast, can engage the reviewer on your side. The reviewer can see that the results are even stronger than you suggest and can agree with and support your interpretation.

To implement this guideline, avoid statements like "the findings prove," "certainly," "obviously," and "very strong relationships." No matter how strong the correlations, it is better to substitute phrasing like "the findings suggest," "tentatively," and to talk about "moderate relationships." This phrasing is more accurate and defendable for social science research. Everything we find is tentative. I think overstatement often is inadvertent rather than intentional. But exaggeration inserts a tone of amateurism to the paper and says that authors are not aware of what they are doing. Exaggerations can be excised from the paper and replaced with tentative understatements, which are more accurate and persuasive and will increase opportunity for publication.

CONCLUSIONS: THE RESEARCH DUALITY

No one can write an excellent paper by hearing about failures. No one can be certain of having a paper accepted at a major journal by reading about papers that were rejected. Yet this chapter was about reasons for failure and rejection. My purpose in this chapter was to illuminate reasons for manuscript rejection. I analyzed the problems defined in my reviews of 111 manuscripts submitted to *AMJ* and *ASQ*, which revealed 11 major reasons for failure. My analysis suggested that most failures were due to theory rather than to method, and that three distinct skills—theory, writing, design—are needed to produce an excellent manuscript. Seven guidelines were proposed for developing manuscripts that can be published in major journals.

The findings from this analysis parallel two other projects of mine that examined the research process. The projects involved interviews with prominent scholars about significant and not-so-significant organizational research (Campbell et al., 1982) and a proposed model of research as craftmanship (Daft, 1983). The outcome of the interviews with prominent scholars suggested that research is significant when it embraces a special kind of duality. Significant research excelled at both method and theory and was characterized by both objective and subjective elements, by both organic and mechanistic processes. The description of research as a craft implied something similar—that the soft, theoretical side of research had to be integrated with the hard, methodological side to achieve significant outcomes.

The duality also appears in the analysis in this chapter. The findings suggest that good research requires both a theoretical base and a solid methodology. An excellent manuscript masters the intangible factors, including tone, style, and integration, along with the tangible factors of research description and solid re-

search design. A well-written paper sticks to the operational base of the research, yet tells a story that goes beyond the data.

The research duality creates a tension for scholars. The dual elements may seem mutually exclusive. How can an author stick to the operational base and go beyond the data at the same time? This is the challenge facing authors. The finding from my analysis is that most manuscripts did not meet the challenge. They fell short on the subjective, theoretical side of the duality. Most of my criticisms pertained to theory rather than to method, to tone and style rather than to design. This suggests to me that the softer, theoretical side of the research duality is more difficult to learn. To master our craft, we must master theory and writing skills in addition to research design skills. The skills on the intangible side of the duality take more time, more work, and cannot be taught through formalized course work in graduate school. Yet mastery of this side of the duality is what distinguishes the truly excellent papers and allows them to be published in the leading journals.

The emphasis within the duality required for publication probably depends upon respective disciplines and journals. A journal such as *Administrative Science Quarterly* emphasizes the theoretical end of the duality. Without strong theoretical development, a paper is less likely to be published in *ASQ*. The *Journal of Applied Psychology* or *Management Science* emphasize the end of the duality where method and design procedures are important. But excellent papers typically capture both aspects of the duality to some extent when they are successful and have impact.

Most papers I reviewed handled the design and method portion of the duality satisfactorily, which is important. But they fell down on storytelling, the coordination of one part of the paper with other parts, tone, rationale, or letting the paper ripen naturally. Most authors need to work hard at these elements before submitting their papers for publication. Improving the theory and writing will please and delight the reviewers and increase the chances for publication.

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