

25 Epilogue: Trade-Offs among Editorial Goals in Complex Publishing Environments

William H. Starbuck, Herman Aquinis, Alison M. Konrad, and Yehuda Baruch

Trade-offs among editorial goals in complex publishing environments

The extrinsic rewards from editorial work can be erratic and disappointing. Even those who edit highly prestigious journals may find it difficult to persuade their deans to allocate resources to this activity, and their deans or department heads may advise them to publish more articles themselves instead of publishing articles by other authors. Those who receive payments from commercial publishers are likely to find that these payments translate into ludicrously low hourly wages. Those who edit journals published by their universities may hear their colleagues asking why resources are going to that specific field or topic rather than to other topics or fields.

Editors may also find it difficult to assess the impacts they are having at the time they are having them. Most editors receive respect and deference, and these rewards are likely to decline when they leave their editorial jobs. Editors, who spend many hours trying to help authors express their ideas, may hear later that some authors greatly appreciated their suggestions whereas others saw the editors' efforts as intrusive. Editors have opportunities to influence the development of knowledge and agendas for teaching and research. However, they may see these effects wane when their successors pursue different agendas and adopt different policies. Any effort to exert influence or to exercise judgment has the potential to upset someone, so all editors make some authors angry or upset and some editors discover that their editorial activities won them enemies.

Thus, editors need to think about their intrinsic rewards, and hence about their motives and goals. Why do you want to serve as an editor? What satisfactions do you hope to receive from the experience? Recognition? Visibility? Methodological change? Establishment of a nascent research domain? Incremental improvement in an established domain? Promote the development of a subdiscipline? This chapter offers an explicit analysis of some central choices that editors can make about their roles. Reflecting on

these choices can help editors to use their time and efforts more effectively and to see the advantages and disadvantages of their activities. This chapter also has implications for authors: by understanding the choices that specific editors make in their roles, authors can gain a better understanding of what to expect from the review process.

This book presents very diverse viewpoints that collectively show that there are many ways for an editor to achieve success ... and just as many ways for an editor to perform poorly. Because every editor has limited time, a limited supply of manuscripts, and personal strengths and weaknesses, no editor can attain excellence on every dimension of performance. An editor has to choose some foci. However, there is also no need for an editor to attain excellence on every possible dimension of performance. Journals have different reputations, occupy different market positions, possess different opportunities, espouse different missions, and need different developmental impulses. Thus, editors have latitude to adapt their talents and experience to the needs and opportunities of the journals they are editing.

At the same time, it is apparent that some editors achieve greater success than do others. Circulations, subscription revenues, downloads, and citations offer quantitative evidence. The historical statistics include instances in which citations to a specific journal have risen dramatically, or plummeted, during one editor's term and then stabilized when that editor left the job. Figure 25.1 illustrates some of the possibilities by graphing real data regarding the impact factors during the terms of four successive editors of a management journal. The dashed horizontal lines show the averages during the editors' terms, and the axes are unlabeled to mask the identities of the editors. Although documentation about reputations is nonexistent, every

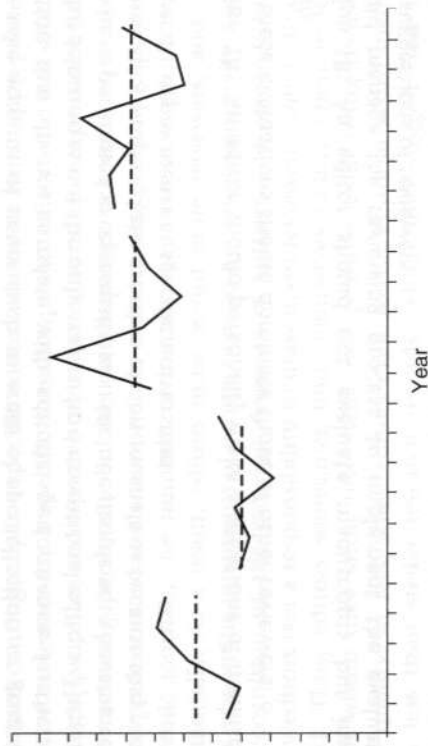


Figure 25.1 Impact factors with four editors

academic author has participated in conversations about the apparent editorial policies of various journals. Some journals, it is said, are open to new ideas; others, it is said, will consider only X or Y. Yet another indicator of editorial success may be the votes cast in the elections of professional societies. Because people vote for names they recognize and editorships create visibility, those who have recently been editors tend to attract votes. However, there are instances in which candidates who recently left editorships have attracted either remarkably many votes or shockingly few of them.

Journal editors face too many strategic and tactical options to enumerate here. However, four pairs of options receive frequent mention throughout this book. To make the pros and cons very clear, we present these options as sharply distinct alternatives. However, most editors mix these options in practice. Instead of consistently choosing option A in preference to not-A, editors choose A for some manuscripts and not-A for others. For example, an editor may have much more confidence in some reviewers than in other reviewers, or an editor may believe that she has much more expertise about some topics than about other topics.

Because many contingencies affect editorial actions and their consequences, and because some editors deviate from generalizations that describe many editors, all of the arguments to follow have exceptions and their validity depends upon the specifics of individual situations. For example, some journals try to select editors through systematic processes. They identify reviewers who offer especially useful and constructive advice, and invite these people to become associate editors. Then, they select editors from among the associate editors (Cascio, 2008, this volume). Although such processes tend to select editors who do not perform poorly, the selected editors do not all perform equally well, and different editors emphasize different aspects of their roles. The processes that winnow candidates may take account of issues such as areas of specialization or geographic location that do not correlate with editorial performance. Furthermore, careful selection is not the only way to find exceptional editors. The editors who have performed exceptionally well include people who became editors because they proposed the creation of new journals or because they worked in specific departments at specific universities.

Option 1A: An editor should personally make evaluations of manuscripts and these evaluations should dominate those of other reviewers.

versus

Option 1B: An editor should not evaluate manuscripts but instead should manage the reviewing process to implement the evaluations and suggestions of reviewers.

In Chapter 13, "The Case for an Activist Editorial Model," Jacobs (2008, this volume) argues that the decision following a first review should be a

crucial one. He advises editors to read carefully every manuscript and the reviewers' comments about them, and also to make sure that the reviews are "informative" and that they discuss all of the important issues. Editors' central goal, he says, should be to avoid encouraging revisions to manuscripts that have little chance of acceptance because authors find rejections more "painful" after they have made revisions. Jacobs also argues that early rejections save reviewers' time and authors may be able to publish their work sooner by resubmitting to an alternative journal.

Such an activist approach assumes that a specific editor has better judgment than do the reviewers of a specific manuscript. It assumes that the editor can and will make better decisions or more definitive decisions than the reviewers do concerning the issues that most need attention and the likelihood that a manuscript will eventually warrant publication. Such assumptions may not be valid when applied to many manuscripts on diverse topics, but they may be reasonable assumptions where an editor has especially relevant expertise or experience, or where reviewers lack relevant expertise or experience. Ironically, whereas the editor of a highly specialized journal might have relevant expertise more frequently, reviewers for a highly specialized journal might also have relevant expertise more frequently. Certainly, editors have stronger motivation than reviewers do to invest time and effort in making correct judgments. Anonymous reviewers do not have to take personal responsibility for the quality of published articles and reviewers may see the reading and evaluation of manuscripts as intrusions into their busy lives. Editors have their names publicly associated with the published products and they are performing editorial work because they feel personally committed to the specific journal.

Because journals, editors, reviewers, authors, and manuscripts are diverse, many contingencies affect the usefulness of an activist approach. One important contingency is a specific editor's ability to enlist appropriate and effective reviewers for a specific manuscript. An editor who has difficulty finding appropriate and effective reviewers must try to compensate by doing more reviewing personally. In the contemporary academic environment where professors around the world are experiencing pressure to publish in academic journals, the numbers of manuscripts to review are increasing substantially. As a result, editors must recruit more reviewers, and often must rely on the judgments of people they do not know personally. Indeed, in recognition of the growing diversity of authors and research topics, many editors feel a responsibility to draw upon reviewers from around the world. Thus, editors sometimes find themselves calling upon reviewers about whom they know little, with one possible result being reviews of poor quality. In fact, some editors even recruit editorial board members with less than stellar research records, which may suggest that these reviewers may not have the qualifications and experience needed to evaluate research produced by others (Bedeian, Van Fleet, & Hyman, 2007).

As well, reviewers sometimes do not complete their work promptly so that an editor may decide that further delay would be unreasonable, and one possible response is a review by the editor personally.

Another contingency is the size of the audience to which editors and authors seek to communicate. Some journals have identities that attract manuscripts on narrow ranges of topics, which implies that editors, reviewers, and authors come from specialized subpopulations. Editors in such situations can enlist reviewers from these subpopulations. However, much research indicates that people have different perceptions and different propensities for activism in familiar domains than in unfamiliar domains, and a few studies suggest that some editors treat manuscripts in their own specialties differently from other manuscripts (Ellison, 2002; Martinko, Campbell & Douglas, 2000; Miller & Perrucci, 2001). Such biases likely occur in reviewers' treatment of manuscripts in their own specialties. In addition, a study by Mahoney (1977, 1979) suggests that reviewers are more likely to approve of manuscripts that support the reviewers' own published studies and that they are more likely to disapprove of manuscripts that contradict the reviewers' own published studies. Thus, editors of specialized journals confront frequent challenges to their objectivity and fairness that arise from the homogeneity and small sizes of their audiences. The editors of general journals face inverse challenges that arise from the diversity and breadth of their audiences. When these editors find themselves looking at reviews by unknown people concerning manuscripts on unfamiliar topics, they have to regard submitted reviews as providing evidence about reviewers and manuscripts simultaneously (Konrad, 2008, this volume). This shifts the evaluation toward indirect evidence about the reviewers' evaluations: Does a reviewer appear to have read the manuscript carefully? Does a reviewer seem to have familiarity with relevant literature? As well, the communicative properties of manuscripts have more importance: Is the manuscript understandable by a diverse audience? Is the manuscript's topic of widespread interest? Overall, editors of general journals participate in editorial processes somewhat differently than do editors of specialized journals. Editors' willingness to ask questions about manuscripts outside their own specialties can help authors to communicate the value of their contribution to a wider audience. Indeed, forcing authors to explain the value of their research clearly to nonspecialists greatly improves the quality of writing and clarifies manuscripts' contribution even to specialist readers.

Strong editorial interventions are two-edged swords that can produce either better outcomes or worse ones. This attendant uncertainty arises from the many ways to perceive a manuscript and the many criteria that readers can apply to a manuscript. For example, Starbuck (2006a) reported how abnormal editorial actions affected several of his most-cited articles. Indeed, his most cited article received "rejects" by both of two reviewers but that journal published the article because the editor accepted it against the reviewers' advice. In another instance, an editor did not even consult

reviewers before accepting an article that later drew many citations. Of course, such interventions may work to an author's disadvantage as well. Starbuck has had a manuscript rejected by an editor despite receiving two "accepts" and a "revise" from three reviewers. Konrad (2008, this volume) points out that editors may offer recourse to authors when reviewers have missed a point, made incorrect assumptions, or held authors to excessive standards of excellence.

There are also various reasons why an editor might have a policy of abstaining from direct involvement in the evaluation of manuscripts. One of these reasons is to increase the involvement and commitment of members of editorial boards. For instance, when Starbuck became the editor of *Administrative Science Quarterly* (ASQ) in 1967, there was no field called "organization theory," and scholars regarded that journal as a representative of Cornell University's School of Business and Public Administration. Indeed, James D. Thompson and Edward Litchfield had created ASQ to promote the school's central theme that successful administration in business requires the same abilities as successful administration in public agencies or hospitals or military organizations. Although ASQ had published several articles by well-known authors, it had a diffuse identity that resulted from its efforts to encompass a wide range of administrative activities and settings. Starbuck saw ASQ as an opportunity to pursue a different agenda – the crystallization of organization theory. Although Simon had spoken of organization theory since the 1950s, March and Simon had published the book titled *Organizations*, and March had edited the *Handbook of Organizations*, in the mid-1960s, this potential field lacked a focal point. As Simon and March had visualized it, organization theory should draw participants and topics from all of the social sciences, but this heterogeneity meant that relevant research appeared in many specialized journals. Starbuck sought to give ASQ an identity as the focal journal for organizational research by anthropologists, economists, management theorists, political scientists, psychologists, and sociologists. He recruited a large editorial board of very well known researchers, who were almost the only people who reviewed manuscripts, and he made every effort to convince them that they had responsibility for the editorial decisions and recommendations to authors. His goal was to persuade the members of editorial board to identify with ASQ, expecting that this identification would induce them to submit their own manuscripts to the journal and to recommend the journal to their colleagues and students. Keeping himself in the background and demonstrating respect for the reviewers' opinions were important aspects of this persuasion process. Of course, this policy also meant that the journal sometimes published articles that Starbuck himself would not have chosen and it sometimes rejected manuscripts in which he saw promise. At the same time, members of the editorial board devoted many, many hours to reviewing and they often had insights and made discoveries that surprised Starbuck.

Other editors may defer to reviewers because the editors perceive themselves as having abnormal values or unusual biases that would make their own evaluations unrepresentative of their journals' audiences. For example, when authors asked one editor to intercede in the review process, this editor systematically replied by saying that he would render a judgment if the authors insisted but he nearly always wanted to reject manuscripts. Few editors have time to read carefully and comment on many manuscripts, which implies that the editors of journals that publish many articles need to restrict their personal involvements to tiny fractions of the manuscripts.

Like directors of research laboratories who do not conduct research themselves and instead rely on the expertise of the research scientists, editors who refrain from making evaluations have to focus on process management. Are manuscripts being reviewed promptly and generally fairly? Are the comments to authors polite and constructive? However, process management works better if the editor concentrates on preventing problems rather than on correcting problems. Probably the most important management activity is deciding who should be on the editorial board and which people should review each manuscript (cf. Bedeian, Van Fleet, & Hyman, 2007). By prescreening reviewers to identify people who show good judgment and write helpful reviews, editors can avoid situations in which authors feel they are receiving unjust treatment. Highly regarded journals are not only publishers of frequently cited research. They gain their strong reputations in part because they provide post-doctoral education in research methods and research presentation, and the members of their editorial boards and ad hoc reviewers function as faculty for this education process. Thus, selecting board members and reviewers deserves cautious care. Laband (1990) studied referees' comments and authors' reactions to these comments on 89 papers that appeared in top journals over five years. His statistical analyses led him to infer that editors' comments had had no discernible influence on later citations of the published papers but that reviewers' comments had significantly increased citations. Thus, he concluded that editors add value to manuscripts mainly by choosing reviewers well. Of course, just as some reviewers make more useful comments than others do, some individual editors doubtless make useful comments. Nevertheless, Laband's study says that editors add very little on average.

Option 2A: An editor's primary goal should be to help authors express their ideas and findings as effectively as possible.

versus

Option 2B: An editor should not attempt to advise authors about presentation style, but instead should allow authors to exercise their own judgment.

Konrad (2008, this volume) argues that authors greatly appreciate guidance from editors, who will have responsibility for making the final publication decisions on their manuscripts. Knowledge development entails difficulties,

especially ones arising from abstract and complex ideas. Editors can help authors by adjudicating the reviews, helping authors to sort out reviewers' demands that contradict one another, shedding light on a line of action that might address a particular reviewer's concerns, and sharing their own thoughts. Providing that sort of guidance, she says, gives authors better chances of focusing their efforts on areas likely to result in acceptance.

Just as editors have opinions about the research that manuscripts describe, they also have opinions about the ways in which authors describe their research and opinions about the advice offered by reviewers regarding presentation. Most of the contingencies that make it more or less reasonable for an editor to intervene between authors and reviewers apply to presentation style as well to research substance and methodology. Does the journal target a specialized audience or a general one, and hence, does the presentation speak better to a specialist audience or a general audience? Does the author have much experience and many publications, or is the author inexperienced and possibly looking for guidance? Are some of the comments of reviewers unclear or misguided?

The pros and cons of such editorial activities are visible in the story of an editor who made strong efforts to assist authors. A few years ago, an editor of a very prominent journal gained a reputation for very detailed commentary. This editor had once worked as a journalist and he expressed himself very well. He also sought to behave in a nurturing way. Thus, the editor routinely sent very long commentaries to authors; every author received at least two single-spaced pages and many received four or five pages. These commentaries restated the comments of the reviewers, emphasizing some of their points more than others, and they gave rather detailed suggestions about how to revise manuscripts. He even wrote long commentaries that were filled with suggestions for revision when he rejected manuscripts, and he phrased his comments so tactfully that some authors of rejected manuscripts thought he was asking them to revise-and-resubmit. Authors reacted variously but predictably to this behavior. For one thing, authors interpreted the editor's comments as contracts: they expected that if they did as the editor proposed, the editor would accept their manuscripts for publication. As a result, authors were shocked when they resubmitted manuscripts that they believed fulfilled the editor's requirements and then they subsequently received yet another revise-and-resubmit that was accompanied by another long, detailed commentary by the editor. For another thing, most authors objected to a few of the editor's suggestions, so they viewed these as unreasonable demands with which they had to comply if they wanted to publish in that journal. Authors who were very eager to achieve a publication in a very prestigious journal found themselves deeply conflicted: Their goal appeared to be within reach but to reach that goal, they would have to say things they did not believe or do things that made no sense to them. Some authors felt that this editor was demanding co-authorship of

their work, anonymously of course, but enforced by the implicit threat to reject noncompliance (Bedeian, 1996).

That editor could have caused less anger had he handled his relationships with authors in a more forthright way. However, his example highlights the incompatibility among four editorial roles: resource allocators, evaluators, mediators, and coaches. Editors ultimately have to allocate space in their journals. Which of the available manuscripts is going to occupy that space? With typical editorial practices and allowing for multiple revisions, the reviewers make definitive decisions concerning 55–65 percent of the manuscripts. Therefore, editors make the final decisions concerning 35–45 percent of the manuscripts (Starbuck, 2005). Editors must evaluate manuscripts carefully enough to choose among the candidates that reviewers leave unclear, and most editors prefer to involve themselves much earlier in the review process. Editors must also mediate between authors and reviewers. Authors may believe that they must do everything that every reviewer says in order to receive an acceptance, and they may find it difficult to distinguish between an inflexible demand and a casual suggestion. Authors want a court of appeal when they believe that reviewers are making incorrect or unreasonable demands, and they would like to think that editors provide fair mediation. Finally, editors have opportunities to act as coaches. Some authors genuinely want advice, and even those who do not want it may find it useful if phrased appropriately. When should authors be strongly encouraged to undertake revisions? Which of the various suggestions by reviewers are likely to prove most useful? Editors often fail to make clear which of these roles they are performing. Aguinis (2007) has pointed out that similar challenges and issues occur with other supervisory roles.

Editors sometimes forget that authors regard editors and reviewers with fear. Bedeian (2003) surveyed authors who had published in two highly regarded journals. One-third said that they had made revisions that expressed an editor's or reviewer's personal preferences, and one-fourth said that they had had to make statements that they believed to be incorrect. These findings show the importance of flexibility in editorial processes. A reviewer or an editor may believe that a particular change would substantially improve a manuscript. However, the author should have latitude to argue an alternative view, and the reviewer or editor should be open to changing their judgments. Indeed, such debates can strengthen the manuscript if authors include their arguments in the manuscript. If a reviewer or editor has questioned a particular conceptual or methodological point, other readers are likely to have the same question, and persuading those readers should be one of the author's goals (Starbuck, 2003). Bedeian (2004) has argued that the issue is less one of flexibility than of equality between authors, editors, and reviewers. Giving authors more equal standing, he argued, would enhance knowledge development.

Option 3A: An editor should seek to publish manuscripts that are consistent with values and paradigms that currently dominate the thinking of readers of the editor's journal. That is, the editor should promote evolutionary change rather than revolutionary.
versus

Option 3B: An editor should seek to publish manuscripts that challenge the values and paradigms that currently dominate the thinking of readers of the editor's journal. That is, the editor should promote revolutionary change rather than evolutionary.

Option 3A's focus on evolutionary development may require adamant and persistent demands. Almost all of the editors who have attempted revolutionary change have had small impacts that might better be described as incremental evolution, and the changes produced by one editor have often been undone by the succeeding editor. To effect revolutionary changes, editors need help from textbooks, professional societies, and many other editors.

Consider the consequences of efforts to eliminate null hypothesis significance tests (Starbuck, 2006b). These tests are supposed to assess the adequacy of sample sizes, but researchers who are willing to continue gathering data until their samples are large enough can eventually show that any "finding" is statistically significant. Indeed, published correlations indicate that researchers do gather more data when their studies are yielding effects that do not reach conventional levels of statistical significance, and the average correlations computed from small samples are much larger than the average correlations computed from large samples (Webster & Starbuck, 1988). Some argue that because statistical significance is very easy to achieve, journals often publish statistically significant findings that have little or no substantive meaning. Another problem is that the public, journalists, MBA graduates, and researchers misunderstand the actual meaning of the tests, and so misinterpretations of test results are very prevalent. People often discuss statistical significance as if it indicates substantive importance. Other prevalent misinterpretations are that rejection of a null hypothesis indicates support for the alternative hypothesis that inspired data gathering, or that failure to reject a null hypothesis indicates that a variable has no effect. As well, people sometimes speak as if the 0.05 probability tail is the probability that a null hypothesis is true, whereas most null hypotheses could not be true on logical grounds, no matter what data research produces (Armstrong, 2007; Cascio & Aguinis, 2005).

The elimination of statistical significance tests from social-science articles has to be regarded as a revolutionary proposal because such tests are deeply imbedded in social-science traditions and many social scientists do not understand why these tests might be detrimental. The liabilities of significance tests have roused criticism since they were first proposed, but their

critics have found it very difficult to persuade social scientists to use other ways of assessing their research (Falk & Greenbaum, 1995). For example, in the mid-1990s, several respected psychometricians urged the American Psychological Association (APA) to ban significance tests from its journals (Starbuck, 2006b, p. 137). Subsequently, leading psychologists participated in symposia at the annual meetings of both APA and the Association for Psychological Science (then called American Psychological Society), and APA appointed a task force to assess the pros and cons of banning such tests. However, the APA task force met briefly and then announced that it "does not support any action that could be interpreted as banning the use of null hypothesis significance testing or p values." The task force did recommend that researchers should augment significance tests by also reporting confidence intervals for effect sizes.

Partly because of the APA task force, two dozen journals in psychology and education are now asking authors to estimate effect sizes (Thompson, 2007). Other journals ask authors to estimate confidence intervals rather than or in addition to point estimates. However, the evidence so far has been that merely asking authors to do this is insufficient to produce much compliance (Cumming et al., 2007). Authors have conformed symbolically, but not substantively. For example, authors may state effect sizes in tables but ignore effect sizes when they discuss their findings, or they may state confidence intervals in tables but say nothing about these in their discussions. For example, in 1996, 36 percent of the articles published in the *Journal of Consulting and Clinical Psychology (JCCP)* discussed the clinical significance of their findings (Fidler et al., 2005). That year, the APA task force made its recommendations, both American psychological societies held symposia on statistical evidence during their annual meetings, and *American Psychologist* published a debate about significance tests. In 1997, an editorial in *JCCP* asked authors to report effect sizes and indicators of clinical significance. In 1999, *JCCP* sought to promote the use of indicators of clinical significance by publishing a special forum about such indicators. Yet in 2001, 40 percent of the articles in *JCCP* discussed clinical significance, just 4 percent more than in 1996. Also in 2001, only 17 percent of the articles reported confidence intervals, and only 11 percent of the articles mentioned the confidence intervals in their discussions of results. As well, Leahey (2005) inferred that authors – especially, well-known researchers from prestigious departments – have had more influence than journal editors on the ways sociologists use significance tests.

Editors have rarely refused to publish manuscripts that did not conform to specific guidelines. However, such strong editorial interventions appear to have stimulated major changes in statistical reporting in medical journals. One extreme example occurred when Ken Rothman became an editor of the *American Journal of Public Health*. His revise-and-resubmit letters told authors: "All references to statistical hypothesis testing and statistical

significance should be removed from the papers. I ask that you delete p values as well as comments about statistical significance. If you do not agree with my standards (concerning the inappropriateness of significance tests) you should feel free to argue the point, or simply ignore what you may consider to be my misguided view, by publishing elsewhere." Later, Rothman became the editor of another journal, where he announced: "When writing for *Epidemiology*, you can enhance your prospects if you omit tests of statistical significance ... In *Epidemiology*, we do not publish them at all. Not only do we eschew publishing claims of the presence or absence of statistical significance, we discourage the use of this type of thinking in the data analysis, such as in the use of stepwise regression." Not surprisingly, these policies altered the frequencies of significance tests in these two journals dramatically. Somewhat surprisingly, Rothman's policies set behavioral patterns that persisted after he left those journals.

However, Rothman did not produce a revolution in medical reporting all by himself. Various medical researchers, journals, and societies had been campaigning against significance tests for many years. Although Rothman's actions drew attention and they influenced many people, a unity of efforts by many people appears to have been what brought about widespread and persistent change in medical research. After studying efforts to change statistical practices in ecology, medicine, and psychology, Fidler et al. (2004: 615) concluded: "The nature of the editorial policies and the degree of collaboration amongst editors are important factors in explaining the varying levels of reforms in these disciplines. But without efforts to also re-write textbooks, improve software and research understanding of alternative methods, it seems unlikely that editorial initiatives will achieve substantial statistical reform."

There are much larger fluctuations in the impact factors of small-circulation journals that change editors than in the impact factors of large-circulation journals that change editors. Presumably, these fluctuations reflect the stronger effects of editorial policies in small-circulation journals. Journals with small circulations also have greater incentive to develop policies that give them distinctive identities. However, there is a risk that idiosyncratic policies and values may alienate some potential authors and readers. On the other hand, the editors of journals with large circulations are very likely to feel that they have a responsibility to uphold norms that have wide acceptance, and those who choose editors for such journals are very likely to choose editors who subscribe to widely accepted norms. As well, large-circulation journals draw reviewers from diverse populations so these reviewers are unlikely to endorse manuscripts that espouse revolutionary ideas. Thus, the editors who have the greatest opportunities to attempt revolutionary change are ones who have limited influence on researchers in general, whereas those who have the greatest opportunities to exert influence face stronger moral and practical constraints.

It is possible that editors' abilities to bring about change fluctuate over time. Tushman and Romanelli (1994) observed that the strategic development of business firms shifts between brief periods of dramatic change and long periods of incremental convergence. Similarly, Kuhn (1970) argued that scientific knowledge fluctuates between brief periods of revolutionary change and long periods of widespread agreement and incremental development. Revolutions become increasingly likely as more and more people become dissatisfied with the shared beliefs that pervade incremental development. Widespread consensus might mean either that the current values and paradigms are well founded or that the field has grown stale and complacent. Thus, an editor who attempts revolutionary change may be a lone dissident who attracts few supporters or one of a rather large group of dissidents who support the change.

Option 4A: An editor should focus on publishing outstanding articles.

versus

Option 4B: An editor should focus on not publishing bad articles.

The pros and cons of these options have been evolving as the publishing industry has experienced dramatic changes over the last three decades. Publishing companies have merged and consolidated their operations. Printing technology has revolutionized, making it possible to publish books in small quantities. The journal-publishing segment has become much more commercial, focused on profit making. Many journals are now being delivered via the Internet, and journals on paper may soon become rare.

During the 1980s and 1990s, publishers introduced many, many new journals (Starbuck, 2005). Although each of these new journals gained a small market segment, collectively they took a significant share from the established journals. Because academic libraries had to acknowledge these new entrants, they shifted funds from books to journals. To make these shifts on defensible grounds, librarians focused on journals' impact factors. In reaction, journal publishers also began to pay much more attention to impact factors, which not only indicate salability to libraries but also the potential value of future reprints. The Institute for Scientific Information (ISI) stopped trying to compile citations in books and limited citation counts to citations in journals, which made the citation data more reliable but also increased the influence of journals over books. Thus, the success of journals and their editors has become a matter of citation counts. Articles that receive many citations encourage librarians to subscribe to journals and publishers to continue publishing them; articles that draw no citations may enhance their authors' résumés but they do not improve journals' visibility or economic viability.

The strongest correlate of journals' impact factors is their circulations. The larger the number of people who receive journals, the larger the number

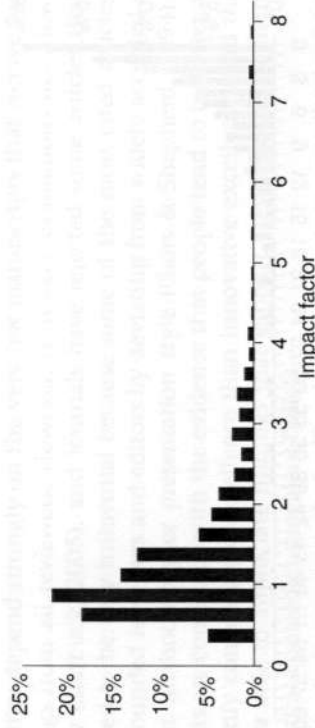


Figure 25.2 Percentages of business journals with different impact factors in 2004

of people who cite articles in those journals. However, journals published by professional associations have lower impact factors than their circulations would otherwise imply, presumably because some of their subscribers do not read them. Editors can use at least two tactics to increase the circulations of their journals: They can try to publish articles that attract a more diverse readership, and they can publish special issues that attract readers who are interested in specific topics. Other factors that correlate positively with impact factors are journals' reputations for high quality, the average lengths of articles, the prevalence of frequently cited authors, and emphasis on theoretical-review articles versus empirical articles (Laband & Piette, 1994; Medoff, 2003). Figure 25.2 presents the 2004 impact factors for 508 journals that business journals cited frequently during 2001 and 2002. These are all of the journals that received at least 12 citations over two years by the 150 journals that the ISI classified as business, business finance, industrial relations, or management. The distribution has a long tail to the right, and the impact factor for the most cited journal is over 34 times the average journal impact factor, which is only 0.85. That is, an average article in an average journal receives less than one citation during the first two years after its publication. A lack of submitted manuscripts may influence the editorial processes at little-cited journals.

The average impact factors that publishers and journals advertise and librarians study can be misleading. Impact factors give a misleading impression of the total citations to articles in that they focus on just the first two years following publication and they assume that some notes or letters might receive citations. Most published articles receive very few citations, and small fractions of the published articles attract most of the citations. Figure 25.3 shows total citations to full-fledged articles over many years – based on 1,000 articles published during the last eight months of the year

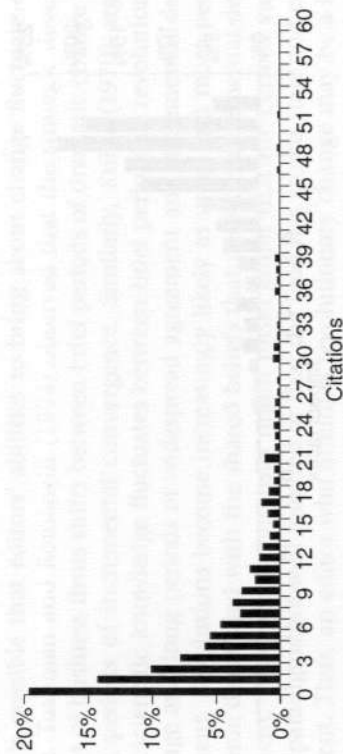


Figure 25.3 Percentages of articles that received different numbers of citations over 17 years

2,000 and having the words "compensation," "management," or "strategy" in their titles. Because these data encompass many journals that have diverse circulations and reputations, the articles published in any single journal would not have so much diversity. According to these data, an average article in an average journal received 7.3 citations during the first 17 years after its publication. However, 71 percent of the articles had below-average numbers of citations and 20 percent of the articles received no citations at all. The 29 percent of the articles that received more than seven citations accounted for 78 percent of all citations. Furthermore, the data in Figure 25.3 understate the effects of the most highly cited articles because this specific sample includes no articles that received more than 118 citations, whereas there do exist rare articles that draw hundreds and thousands of citations.

Readers seem to have very poor abilities to judge the impacts of below-median articles, but better abilities to judge the impacts of above-median articles. Gottfredson (1978) related citations to evaluations made by knowledgeable readers. The logarithms of citations correlated 0.21 with readers' judgments of articles' quality and 0.27 with readers' judgments of articles' impact on their fields. Because readers made their judgments retrospectively, both correlations might overstate reviewers' abilities to make predictive judgments. As well, the practical correlation is nil for many articles. When Gottfredson separated the more-cited and less-cited articles, he found that reviewers' judgments of impact correlated 0.03 with citations for articles that had fewer citations than the median, and correlated 0.36 with citations for articles that had more citations than the median. These differences suggest that readers may be better able to judge and to agree with each other about the small fraction of manuscripts that attract unusually many citations. If so, an implication would be that journals' visibility and economic

viability depend strongly on the very few manuscripts that receive "accept" ratings from all reviewers. However, reviewers' evaluations have low reliability (Starbuck, 2005), and journals have rejected some articles that later proved to be very influential because some of the most cited articles have confounded reviewers and editors by deviating from widely accepted norms about methodology or presentation style (Gans & Shepherd, 1994). Such events are consistent with the evidence that people tend to be better able to identify conventional excellence than innovative excellence, and they are better able to appreciate findings that support prevalent beliefs than findings that contradict prevalent beliefs.

Two studies suggest that editors' most important contributions to their journals come from interpersonal networking. A survey by Sherrell, Hair, and Griffin (1989) found that professors considered the two least ethical editorial practices to be (1) favoritism toward friends and colleagues and (2) biasing reviews toward acceptance or rejection by selecting reviewers with strong biases about a manuscript's content or methodology. Laband & Piette (1994) investigated the effects of favoritism. They found that articles by authors who had personal connections with editors were more likely to be lead articles and these articles received significantly more citations after publication. Therefore, Laband and Piette (1994) inferred that "although journal editors occasionally publish subpar papers authored by colleagues and former graduate students, on balance their use of professional connections enables them to identify and 'capture' high-impact papers for publication" (p. 194). Medoff (2003) also studied the effects of favoritism and drew a similar conclusion. Medoff concluded "that articles authored by those with editorial connections, particularly serving on the publishing journal's editorial board, are both statistically and numerically of higher quality. ... The empirical results support the proposition that journal editors, in order to reduce the search costs involved in identifying high-quality manuscripts, use personal ties and institutional connections to persuade high-quality authors to submit their papers to them. Journal editors/coeditors attract these submissions by inducing high-quality authors to serve on their editorial boards as well as by offering constructive comments and suggestions on a high-quality author's paper, reducing the author's transaction cost of publishing" (p. 434).

Yet, editors and reviewers devote nearly all of their attention to trying to improve normal manuscripts. A focus on identifying defects and suggesting corrections means that reviewers and editors spend their time on the manuscripts that have many defects and need many corrections. Although improving a not-quite-good-enough manuscript until it is publishable may contribute to the general development of academic research, but it does not raise a journal's standing to a significant degree. Editors also devote time to rejecting some manuscripts without sending them to reviewers – manuscripts that aim at another audience, that focus on topics outside the

journal's range, or that seem to be of low quality. Unfortunately, empirical studies say that readers' judgments (including the judgments of editors and reviewers) about manuscripts' quality have very low correlations with citations to published articles, and these unreliable judgments mean that journals likely reject about half of the best manuscripts that they receive (Gottfredson, 1978; Starbuck, 2005). Of course, this unreliability also means that about half of the best articles appear in second-tier or third-tier journals.

The foregoing studies imply that in an era of "impact factor" dominance, editors' most significant contributions may be to use interpersonal networking to search for and to attract unusually high-quality manuscripts or manuscripts that promise to create breakthroughs. Both strategies entail risk. The former strategy violates cherished professional ethics. The latter strategy relies on editors' having better judgment and better insight than most people.

Striking realistic balances

This chapter has critically reviewed four dimensions on which editors can make strategic choices. Although the alternatives are phrased as polar opposites, it is very unlikely that any editor would choose one of these extremes. More likely, an editor would blend the alternative strategies even within the handling of one specific manuscript.

For example, Options 1a versus 1b contrast a strategy in which an editor participates in evaluation so dominantly that reviewers have little say, with a strategy in which an editor merely manages the reviewing process and defers completely to reviewers on substantive issues. These two strategies might be policies that an editor applies consistently to every manuscript. However, an editor might use one extreme policy for some manuscripts and the other extreme policy for others, and might deal with most manuscripts by asserting the editor's own opinions on some points while yielding to reviewers on other points. An appropriate mixture of strategies depends upon the editor's abilities and goals, the editor's confidence in each reviewer, each reviewer's expertise, the topic and methodology of the manuscript, the author's reputation and expertise, and properties of the journal such as its age, circulation, or ownership.

Likewise, an editor might perceive that one manuscript could become much more informative and persuasive if the author would change some presentational elements, but that another manuscript is already exceedingly well written. Or, one editor might perceive that conditions are right to attempt a revolutionary change – support from like-minded editors and textbook writers, possibly an influential professional society that is advocating change – whereas another editor might believe that no matter how desirable a change might be, the timing is not right or support is lacking. Or, even an editor who devotes much time to soliciting manuscripts likely also devotes

some time to weeding out unsolicited manuscripts that deviate from that journal's topics.

Because editors have so many ways to use their time and effort, it is hard for them to behave poorly, but it is also hard for them to behave optimally. Optimization requires awareness of the many strategic options and the contingencies that make strategies more or less effective. Because each manuscript entails somewhat different contingencies, an editor that seeks to optimize both macroscopically and microscopically would need to reassess endlessly. Shades of Herbert Simon! This changing complexity raises the issue of what heuristics editors adopt and what biases editors exhibit.

Editors need to beware of the so-called "fundamental attribution bias," a propensity for people to overestimate their own influence on events and to underestimate external or situational influences. For instance, Meindl and Ehrlich (1987) and Meindl, Ehrlich, and Dukerich (1985) argued that both researchers and the general populace attribute too much control and influence to leaders; they inferred that these attributions become more frequent after successful performances. A related and prevalent phenomenon is the "self-serving bias," a propensity for people to overestimate their own influence on successes and to overestimate external or situational influences on failures (Heider, 1958). People also tend to exhibit the opposite biases when interpreting other people's successes and failures. For instance, Wagner and Gooding (1997) found that managers who face equivocal information about their own businesses tend to attribute positive outcomes to strengths in their own organizations, while they blame negative outcomes on environmental circumstances. However, when managers are asked to interpret information about businesses managed by others, they attribute positive outcomes to opportunities in the environment and negative outcomes to organizational weakness.

Biases in reviewing processes also confront editors with challenging ethical and political issues. Prominent among these is the treatment of authors' social statuses. Some of the studies mentioned above imply that editors can increase their journals' visibility and influence by making special efforts to attract manuscripts by prestigious authors. One of the most cited journals, the *Journal of Economic Literature*, has an acceptance rate close to 100 percent because nearly all of its articles are invited ones written by very well-known authors. Similarly, the *Annual Review of Psychology*, which includes invited articles only, has the second highest impact factor (i.e., 11.71) among 99 journals included in the "psychology-multidisciplinary" category (ISI rankings, 2007). However, some professional norms assert that all manuscripts should receive evaluations that are independent of their authorship. Thus, Peters and Ceci (1982) roused strong responses when they resubmitted 12 articles to the journals that had published them just 18 to 32 months earlier. All 12 of these journals were prestigious ones, and the articles had originally been authored by researchers from highly rated psychology

departments. However, Peters and Ceci gave the resubmissions fictitious authors and return addresses at obscure institutions. The submissions went to 38 editors and reviewers. Three editors and reviewers detected that the manuscripts had already appeared in print, which reduced the study to nine manuscripts that had 18 reviewers. Sixteen reviewers recommended rejection, and editors rejected eight manuscripts, leaving only one accepted. Of course, the original editorial processes may have erred by accepting undeserving manuscripts that had been written in prestigious departments, or the experimental editorial processes may have erred by rejecting deserving manuscripts that had been written in obscure departments, or the quality of manuscripts may be so ambiguous that reviewers and editors eagerly grasp for external evidence to guide them such as the imputed reputations of authors. During Peters and Ceci's experiment, the most prevalent reasons for rejection were "serious methodological flaws," including inappropriate statistical analyses and faulty study design. As Mahoney (1977) found, methodology seems to serve as an all-purpose façade for other considerations.

Thus, editors' most valuable assets may be awareness of their own limitations and the limitations of their coeditors and reviewers. Editing is above all an imperfect activity that incorporates human weaknesses and errors and has no optimal solutions. However, editors can do more and feel better about their effects if they make realistic assessments of their personal capabilities and situations.

Acknowledgment

We thank Arthur Bedeian for help in locating some relevant research. This research was conducted in part while Herman Aguinis was on sabbatical leave from the University of Colorado at Denver and Health Sciences Center and holding a visiting appointment at the University of Salamanca (Spain).

References

- Aguinis, H. (2007). *Performance Management*. Upper Saddle River, NJ: Pearson Prentice Hall.
- Armstrong, J. S. (2007). Significance tests harm progress in forecasting. *International Journal of Forecasting*, 23, 321-7.
- Bedeian, A. G. (1996). Improving the journal review process: The question of ghost-writing. *American Psychologist*, 51, 1189.
- Bedeian, A. G. (2003). The manuscript review process: The proper roles of authors, referees, and editors. *Journal of Management Inquiry*, 12, 331-8.
- Bedeian, A. G. (2004). Peer review and the social construction of knowledge in the management discipline. *Academy of Management Learning and Education*, 3, 198-216.
- Bedeian, A. G., Van Fleet, D. D., & Hyman, H. H. (2007). Scientific achievement and editorial-board membership. *Organizational Research Methods*.
- Cascio, W. F. (2008). How editors are selected. In Y. Baruch, A. M. Konrad, H. Aguinis, and W. H. Starbuck (Eds.), *Opening the black box of editorship*. Basingstoke, London: Palgrave Macmillan.
- Cascio, W. F., & Aguinis, H. (2005). Test development and use: New twists on old questions. *Human Resource Management*, 44, 219-35.
- Cumming, G., Fidler, F., Leonard, M., Kalinowski, P., Christiansen, A., Kleinig, A., Lo, J., McMenamin, N., & Wilson, S. (2007). Statistical reform in psychology: Is anything changing? *Psychological Science*, 18, 230-2.
- Elison, G. (2002). The slowdown of the economics publishing process. *Journal of Political Economy*, 110, 947-93.
- Falk, R., & Greenbaum, C. W. (1995). Significance tests die hard: The amazing persistence of a probabilistic misconception. *Theory & Psychology*, 5, 75-98.
- Fidler, F., Cumming, G., Burgman, M., & Thomason, N. (2004). Statistical reform in medicine, psychology and ecology. *Journal of Socio-Economics*, 33, 615-30.
- Fidler, F., Cumming, G., Thomason, N., Pannuzzo, D., Smith, J., Fyffe, P., Edmonds, H., Harrington, C., & Schmitt, R. (2005). Evaluating the effectiveness of editorial policy to improve statistical practice: The case of the Journal of Consulting and Clinical Psychology. *Journal of Consulting and Clinical Psychology*, 73, 136-43.
- Gans, J. S., & Shepherd, G. B. (1994). How are the mighty fallen: Rejected classic articles by leading economists. *Journal of Economic Perspectives*, 8, 165-79.
- Gottfredson, S. D. (1978). Evaluating psychological research reports: Dimensions, reliability, and correlates of quality judgments. *American Psychologist*, 33 (10), 920-34.
- Heider, F. (1958). *The psychology of interpersonal relations*. New York: Wiley.
- ISI rankings (2007). Reports on Impact Factor for the year 2006. Retrieved from <http://www.annualreviews.org/catalog/isi-rankings.aspx> on September 26, 2007.
- Jacobs, J. A. (2008). The case for an activist editorial model. In Y. Baruch, A. M. Konrad, H. Aguinis, & W. H. Starbuck (Eds.), *Opening the black box of editorship*. Basingstoke, London: Palgrave Macmillan.
- Konrad, A. M. (2008). Knowledge creation and the journal editor's role. In Y. Baruch, A. M. Konrad, H. Aguinis, & W. H. Starbuck (Eds.), *Opening the black box of editorship*. Basingstoke, London: Palgrave Macmillan.
- Kuhn, T. S. (1970). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
- Laband, D. N. (1990). Is there value-added from the review process in Economics? Preliminary evidence from authors. *The Quarterly Journal of Economics*, 105, 341-52.
- Laband, D. N., & Plette, M. J. (1994). Favoritism versus search for good papers: Empirical evidence regarding the behavior of journal editors. *The Journal of Political Economy*, 102, 194-203.
- Leahy, E. (2005). Alphas and asterisks: The development of statistical significance testing standards in sociology. *Social Forces*, 84, 1-24.
- Mahoney, M. J. (1977). Publication prejudices: An experimental study of confirmatory bias in the peer review system. *Cognitive Therapy and Research*, 1, 161-75.
- Mahoney, M. J. (1979). Psychology of the scientist: An evaluative review. *Social Studies of Science*, 9(3), 349-75.
- Martinko, M. J., Campbell, C. R., & Douglas, S. C. (2000). Bias in the social science publication process: Are there exceptions? *Journal of Social Behavior and Personality*, 15, 1-18.
- Medoff, M. H. (2003). Editorial favoritism in Economics? *Southern Economic Journal*, 70, 425-34.
- Meindl, J. R., & Ehrlich, S. B. (1987). The romance of leadership and the evaluation of organizational performance. *Academy of Management Journal*, 30, 91-109.

- Meindl, J. R., Ehrlich, S. B., & Dukerich, J. M. (1985). The romance of leadership. *Administrative Science Quarterly*, 30, 78-102.
- Miller, J. & Perrucci, R. (2001). Back stage at "Social Problems": An analysis of the editorial decision process, 1993-1996. *Social Problems*, 48, 93-110.
- Peters, D. P. & Ceci, S. J. (1982). Peer-review practices of psychological journals: The fate of published articles, submitted again. *Behavioral and Brain Sciences*, 5, 187-255 (page range includes 50 pages of comments by others and a response by Peters & Ceci).
- Sherrell, D. L.; Hair, J. F., Jr.; & Griffin, M. (1989). Marketing academicians' perceptions of ethical research and publishing behavior. *Journal of the Academy of Marketing Science*, 17, 315-24.
- Starbuck, W. H. (2003). Turning lemons into lemonade: Where is the value in peer reviews? *Journal of Management Inquiry*, 12, 344-51.
- Starbuck, W. H. (2005). How much better are the most-prestigious journals? The statistics of academic publication. *Organization Science*, 16, 180-200.
- Starbuck, W. H. (2006a). Preface: Realistic perspectives on organizing and strategizing. In *Organizational realities: Studies of strategizing and organizing*. Oxford: Oxford University Press.
- Starbuck, W. H. (2006b). *The production of knowledge: The challenge of social science research*. New York: Oxford University Press.
- Thompson, B. (2007). Various editorial policies regarding statistical significance tests and effect sizes. Retrieved from <http://www.coc.tamu.edu/~bthompson/journals.htm> on September 26, 2007.
- Tushman, M. & Romanelli, E. (1994). Organization transformation as punctuated equilibrium: An empirical test. *Academy of Management Journal*, 34, 1141-66.
- Wagner, J. A. III & Gooding, R. Z. (1997). Equivocal information and attribution: An investigation of patterns of managerial sensemaking. *Strategic Management Journal*, 18, 275-86.
- Webster, E. J., & Starbuck, W. H. (1988). Theory building in industrial and organizational psychology. In C. L. Cooper and I. Robertson (Eds.), *International Review of Industrial and Organizational Psychology*, 93-138. London: Wiley.

Author Index

- Agarwal, R. 61, 67, 119, 123
- Aguinis, H. xii, xxvi, 3, 14, 250, 258, 259, 268, 269
- Aiken, L. S. 118, 123, 138, 142
- Allscheid, S. P. 34, 38
- Altman, G. xxv
- Anderson, N. 11, 113
- Argyris, C. 4, 15, 167
- Armstrong, J. S. 259, 268
- Arnsward, U. 175
- Ashtforth, B. E. 28, 37
- Bacharach, S. 23, 26
- Bachrach, D. G. 38, 147, 156
- Bakos, Y. 178, 186
- Bamford, H. 197, 204
- Barbuto, J. E. 35, 37
- Barley, S. R. xiii, xxiii, 39, 99, 103, 119, 123, 179, 186
- Barnes-Farrell, J. L. 33, 38
- Barron, F. 26
- Bartunek, J. xiii, xxiii, 3, 11, 59, 88, 89, 119, 123, 222
- Baruch, Y. xii, xxii, xxiv, xxv, 15, 37, 38, 56, 88, 96, 101, 103, 175, 209, 210, 216, 222, 239, 249, 250, 269
- Bazerman, M. 87
- Bedeian, A. G. xiv, xxi, xxiii, xxv, 29, 37, 80, 87, 134-142, 181, 187, 246, 253, 256, 258, 268
- Beebe, J. 139, 142
- Belcher, W. 135, 136, 142
- Bem, D. J. 101, 103
- Bergh, D. D. xiv, xxiii, 13, 114, 115, 119, 122, 123, 139, 243, 247, 249
- Bergstrom, C. T. 202, 205
- Bergstrom, T. C. 202, 205
- Beyer, J. 94
- Bhappu, A. D. 34, 37
- Biagioli, M. 134, 135, 142
- Bies, R. 77, 87
- Biggart, N. 171, 175
- Bitner, M. J. 33, 37
- Black, D. 216, 222
- Bolino, M. C. 31, 37
- Booms, B. H. 33, 37
- Borchgrevink, C. P. 34, 38
- Bowen, D. E. 32, 37, 38
- Brett, J. M. 91, 96
- Brockner, J. 77, 87
- Brody, T. 113
- Brooks, L. 230
- Brown, D. 230
- Brynjolfsson, E. 178, 186,
- Buchler, J. 16, 26,
- Burgman, M. 269
- Calhoun, M. A. 4, 12, 15
- Callero, P. L. 38
- Campbell, C. R. 254, 269
- Campbell, D. T. 23, 26
- Caplow, T. 88, 96
- Cardinal, L. B. xxii, xxv
- Carley, K. 210, 222
- Carr, L. 113
- Cascio, W. F. xii, xiv, xxiv, 89, 93, 216, 222, 231, 238, 252, 259, 269
- Ceci, S. J. 267, 268, 270
- Chan, L. 202, 205
- Chang, C. 33, 37
- Cherry, B. 34, 37
- Christiansen, A. 269
- Clark, T. xiv, xxiv, 176, 177, 180, 181, 186
- Clary, E. G. 28, 37
- Cohen, J. 20, 138, 142
- Cohen, P. 138, 142
- Colquitt, J. 77, 87, 96
- Conlon, D. 87
- Cook, T. D. 23, 26
- Cooper, C. L. 270
- Courpasson, D. 170
- Cranage, D. 33, 38
- Creamer, V. 33, 38
- Cummings, G. 260, 269
- Cummings, L. L. xxi, xxv, 67, 239, 249