Distinguished Scientific Contributions Award: How to Publish Like Heck and Maybe Even Enjoy It*

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Given the purpose of this award, I thought it was most appropriate to devote my talk to how to publish a lot of articles. I thought this might be of interest to young scholars in the field and maybe even a curiosity for established scholars. I considered giving a content-oriented talk, such as on my recent research on structured interviewing, but **Fred Morgeson** told me that people can read my research, so I should give a talk on how I got the research record to win this award. So if the speech is not interesting, it is his fault.

For ease of exposition, I will give my advice in terms of a list of specific suggestions, and I will break the list down in terms of subtopics.

How to Be Productive

1. *Work hard and long*. This may seem too obvious. Unless you are a lot smarter than other people (which is unlikely) or you get lucky (which you can't count on), the best way to produce more than other people is to work harder. I have found that the sustainable upper limit is 60 hours a week. More than that and you feel like you are working all the time.

2. *It is a marathon, not a sprint*. The best strategy is to work regular hours every day rather than work in spurts. Turtles are successful in this business, not hares.

3. *Do an increment a day*. One of the most successful work strategies I ever happened upon occurred to me when I was doing my first major project, my master's thesis in the 1970s. Most students stall out when doing their theses or dissertations. They usually get distracted by short-term deadlines, such as classes to teach, while always planning to spend days of future-focused effort that they never do. Months go by with no progress. An effective strategy is to instead set a goal to do some increment of progress every day, no matter how small, such as one table or paragraph. You must require yourself to achieve this goal or you cannot go home. The trick is that if you do an increment a day, you will eventually finish the project. And small achievable goals are very motivational. I think people get overwhelmed by the magnitude of the project when it comes to theses or dissertations, and so they procrastinate. A long journey is but a combination of a lot of small single steps, or however the saying goes. This insight occurred to **Bruce Avolio** and me while doing our masters theses in 1977, and we were the first among our peers to actually finish our theses because of it. My master's thesis was published as Campion and Lord (1982).

4. *Be totally focused.* Here are some test questions to see if you are focused: What do you do first thing in the morning? Do you check your e-mail? Do you prep for class? What if you are getting close to leaving for the day and you have 30 minutes left? What do you do? Do you work on little tasks that can get done in that amount of time? I suggest that you ask yourself, what really matters to my career success? Few people fail in their careers because they did not check their e-mail often enough. When you get to the office, you should go immediately to work on your research. Check your e-mail later on when you need a break. If you have 30 minutes left at the end of the day, spend it on your research. Even a little tiny bit of progress on something that matters is better than finishing some little task that does not matter. Do not be distracted by deadlines for less important tasks. If you have to teach later in the day, do not plan your class first thing in the morning. Work on your research until it is time to prepare for your class but not a minute before. Most people fail to finish graduate school or get tenure because of lack of progress on their research, rarely because of their teaching.

5. *Go Bulldog!* Although the incremental approach is useful advice earlier in the research project, there is a time when you must put out a lot of effort to get the paper out the door. I have found that a useful strategy when a project gets close to completion is to focus total effort. You must bite down and hang on. We call it "Going Bulldog!" We joke that we do not sleep, shave, wear deodorant, or do anything unnecessary until the work is done. This is a euphemism, of course, for focusing total effort on that single project until it is out the door. It is necessary because the last 20% of the project seems to take another 80% of the time. If you do not focus total effort, it seems like you can never finish a project.

6. *Practice, practice, practice.* Publishing is just like sex; you must practice, practice, practice, or at least that is what you tell your sweetie. Like any learned skill, publishing takes practice. Especially early in your career, like when you are a student, it is OK to write up datasets and send them off to low-grade journals to get practice writing complete articles, dealing with reviewers, and bringing work to completion. These will not make the key difference in terms of landing a great academic job, but they show good activity and they will help you refine your skills. Examples on my resume include Campion (1978, 1980). (But see the caveats below about not spending too much time on low quality research.)

How to Pick Research Topics

1. *Don't just study what interests you.* I have found that most topics become interesting once you start studying them. Using your native interests to guide you early in your career is misleading because you do not really know what interests you yet. It is amazing how things magically become really interesting if you are successful researching them. The more you learn about something, usually the more interesting it will get.

2. *Have your cake and eat it too*. Study topics that have practical implications. We are an "applied" science. We do not do "basic" research. Our role is to help organizations and workers be more effective. Study topics that companies and workers care about. This has the additional benefit of getting organizations to support your research and possibly even pay you for it. Also, it is a big kick to see your research influence hundreds or thousands of people's work lives. We can have an impact on the world in many ways for which we should be proud (Campion, 1986).

3. *Study topics that have broad readership (and avoid trends)*. If you study selection, thousands of people will read your research. If you study some esoteric attitude, few people will read it or cite it. As such, avoid the tendency to study new topics because they are new. Traditional topics are traditional because they are of long-term value to the field. Also, if you study trendy new topics, there is a good chance that your research will not be relevant in 20 years, and you will have the displeasure of realizing that you have not really had any lasting impact on the field.

4. *Consider the number of journal outlets*. Make sure there are at least two A-class journals that will publish on your topic so you will have at least two bites at the apple.

5. *Avoid the well-known pitfalls*. Lab studies are very hard to publish in our field. Common method variance, like collecting all your data on a single survey, is usually considered a fatal flaw by the top journals.

6. *Study what you can study well*. Articles get rejected more for methods than topics, so study topics that you can study well. See the Article Review Checklist for what reviewers look for (Campion, 1993).

7. *Study topics for which great data become available*. There is nothing wrong with picking topics to study because a good dataset becomes available to you.

8. *Swipe ideas from other fields*. How does that expression go? "Stealing from one is plagiarism, stealing from many is research." An excellent way to make a contribution to our field is to borrow theories, methods, or ideas from other related fields and apply them to the problems in our field. Remember, we are an "applied" science, meaning our role is to apply science, so this is what we are supposed to do. We borrow from other areas of psychology, of course, but also consider other fields that study related phenomena (e.g., other areas of business, other social sciences, engineering, etc.). For example, I have been able to contribute to I-O by borrowing ideas from human factors and engineering in the study of job design (Campion & Thayer, 1985), ergonomics and work physiology in the study of physically demanding jobs (Campion, 1983), economics in the study of compensation (Campion & Berger, 1990), and law in the study of international issues (Posthuma, Roehling, & Campion, 2006). A point of caution is necessary, however. Most top journals are oriented to specific disciplines, so pure interdisciplinary work where the contribu-

tion is simply the use of multiple disciplines may be hard to publish. You must borrow from other disciplines and bring the knowledge home to solve problems in your discipline rather than trying to stand in between.

9. *Study different topics.* Do not become "thematic" too early. Take on new projects and new topics. Try different things. There is a strong tendency to want to specialize in limited topics so that you can make an impact in an identifiable area, which is important to promotion in academic jobs. The problem is that this tendency occurs too early, such as when you are still in school, which limits exploration into new areas. You cannot find your true love without doing a little dating around. There is a tension between wanting to study thematic topics, so you can be identified with a unique contribution and because you know the literature, versus the many advantages of studying new topics. For example, some of the best insights occur when scientists approach a research area for the first time. You will not be encumbered by traditional views. Also, how do you grow professionally if you do not take on new topics? I am willing to study most everything, and I have. (By the way, this also makes you a more effective consultant.)

10. *Be creative methodologically.* Publications do not always have to be surveys, despite their omnipresence in our journals. In fact, surveys trying to test box-and-arrow models are a very weak methodology and have probably advanced the science as much as they can. It is time for new and better methodologies. Some of my best research used observations, interviews, aptitude tests, quasi-experiments, or other methods. You might learn some new tools and have some fun at the same time. If you have strong or creative methods, reviewers will help you make the article publishable. If you use a survey, they will be looking for a reason to reject you.

11. *Watch for megatrends.* One example in my life was teams. I saw the wave rising and got in on it in the early 1990s. The result was my most cited work (Campion, Medsker, & Higgs, 1993; Stevens & Campion, 1994, 1999). Another example is structured interviewing. We saw that one coming and got in on it early (Latham, Saari, Pursell, & Campion, 1980; Campion, Pursell, & Brown, 1987; Campion, Palmer, & Campion, 1997). We have also paid attention to job analysis trends, such as O*NET (Peterson et al., 2001) and competency modeling (Campion, et al., 2011). We are also trying to start some new trends ourselves, such as the study of inaccuracy in job analysis (Morgeson & Campion, 1997) and the study of faking in the interview (Levashina & Campion, 2007).

12. *Listen to smart students with applied experience.* Stan Malos was an attorney returning to school for a PhD. He had some real insight into how professional service firms like law firms worked, which he turned into an *AMR* and an *AMJ* (Malos & Campion, 1995, 2000). Carl Maertz was an internal consultant in a major corporation with some real insight into turnover, which he turned into an *AMJ* and several other articles (Maertz & Campion, 2004; Maertz, Stevens, & Campion, 2003).

13. *Publish your applied projects.* This is perhaps my most effective strategy. Out of the 114 publications on my resumé today, 33 (30%) came directly or indirectly from applied projects.

How to Publish Applied Projects

1. *There's a pony in there somewhere.* You should look at every applied project as though there is a publication in it, and your job is to find it. If you cannot find it, you should simply continue to think about it until you figure it out.

2. *Don't ask permission.* The topic of asking for "permission" to publish a dataset merits brief discussion. First, never ask corporate attorneys for permission. Attorneys will virtually always say no because it is their job to say no to most everything. They are only rewarded for minimizing risk regardless of the potential benefit to science. My preference is to disguise the organization's identity in the article to such an extent that asking for permission is not necessary. This may require being vague about some details of the sample, jobs, and setting, but this is a reasonable tradeoff compared to not publishing the study at all. If the reviewers ask for more details, you can tell them in your response letter, but do not put the details in the article. I have found that editors will usually understand this explanation. There are a couple more key points about permission. If you must ask permission, you might let the company sponsor see some examples of published research articles. They usually think of publishing as something in the newspaper (i.e., highly visible, focusing on the extremes, and taken out of context). They have no concept of scientific articles (i.e., technical, thick, boring, and uninterpretable to the layperson). Scientific articles are a sure cure for insomnia, as one of my clients once put it. Finally, our articles are not like publishing trade secrets. They usually focus on theories and methods, and the actual details of the setting and the specific results are of less interest. Besides, using the findings of our studies requires dedicated managers, significant investment, and technical expertise, and cannot simply be stolen like a formula for a new product.

3. *Coauthorship is free.* Liberally include your bosses, company sponsors, and others who can help you get data. This also gives the company some good public relations benefit because they are sharing knowledge, which may make them more likely to take an interest in the project, sponsor your research, and allow the data to be published.

4. *Link up at SIOP.* One of the many wonderful things about our science is that we are still at the stage where most of us are both scientists and practitioners whether we are in academic or applied employment contexts. However, this creates a dilemma. If you are in academe, you often lack access to datasets and research sites, but if you are in applied settings, you do not have the time or reward system to publish. The key is scientist–practitioner linkups, and SIOP is the place to make that happen. Seek those in employment settings opposite your own for your mutual benefit. I have had a great

many link ups; examples include Campion, Fink, Ruggeberg, Carr, Phillips, and Odman (2011); Maertz, Wiley, LeRouge, and Campion (2010); Morgeson, Delaney-Klinger, Mayfield, Ferrara, and Campion (2004); Bauer, Truxillo, Paronto, Campion, and Weekley (2004); and Bauer, Truxillo, Sanchez, Craig, Ferrara, and Campion (2001).

5. *Don't be afraid to ask for data*. When you meet executives, ask for their help. You might be surprised at what they are willing to do. Helping universities and college professors is a good thing that many organizations want to do. Take advantage of it. I once had an executive in a *Fortune* 500 company send me all the data on their compensable factors for all their jobs, something that lower level managers thought was too sensitive, which I parlayed into a *PPsych* article (Campion & Berger, 1990).

6. *Use favors*. When I left IBM to go to Purdue in 1986, I offered to do a particularly difficult job for them if they would do me a favor—help me get some data. I collected enough data to get a *JAP* and two *PPsychs* (Campion, 1988, 1989; Campion & Berger, 1990)

7. *Get there before data collection begins*. The problem with publishing existing datasets is that there is often some key weakness that could have been avoided if planned in advance, such as a missing measure or research design limitation. The old expression about an ounce of prevention being better than a pound of cure applies here.

8. *Be looking for multiple needles in the haystack*. Be willing to study different topics. Be flexible theoretically. If you only want to study a narrow set of topics (or if you only know a small amount of the literature), coupled with the fact that organizations often define their needs very narrowly, it is like two needles trying to find each other in a haystack. Be willing to study the organization's problem. Not only might you have important insight when you approach a new topic, a point noted previously, but it is much more fun to get paid and get a publication at the same time.

9. *Bring multiple theories to bear*. Make solving the applied problem your central focus. Start by digging into the literature, finding literature that is relevant, and applying the literature to the problem. The applicability and limitations of our theories and methods will reveal publishable research topics. Comparing how different theories address applied projects is almost always interesting and can often be a contribution to the literature because it pits the theories against each other. That was the key insight in my job design research (Campion & Thayer, 1985) and also my research on turnover (Campion & Mitchell, 1986; Campion, 1991).

10 . *Be open to insight*. Don't let a total focus on your favorite theory and hypotheses blind you of fortuitous insight. For example, I was once doing a study on training needs, but interviews with executives revealed that the real driver of career development was the movement of employees between jobs. The result was an *AMJ* on job rotation (Campion, Cheraskin, & Stevens, 1994) and a long-term understanding of how work assignments are the most

important developer of management talent, which paid off in many subsequent research and consulting projects.

11. *Study anomalies*. If you see something odd in an organization that does not fit our current theories, study it. You might discover something new. For example, I once encountered a situation where employees were passing up seniority-based promotions. We called them "frozen employees." We did a little study and got a *JVB* publication (Campion, Lord, & Pursell, 1981). Another time we observed that temporary employees were sometimes chronically underemployed. We called them "marginal temps" in contrast to "satisfactory temps" for whom the temporary work fit their current employment needs. We discovered that these marginal temps were also associated with counterproductive work behavior and published our findings in *Industrial Relations* (Posthuma, Campion, & Vargas, 2005).

12. *Take advantage of potential natural field experiments*. Quasi-experiments are virtually always better than cross-sectional designs, and they are possible more often than you think if you look for them and if you try to argue for their value to the organization. For example, we once did a quasi-experiment on an interviewee training program when I was at IBM because, quite honestly, we did not know if it would work, and we did not want to roll the training out across the organization without some proof. We reported the result in *PPsych* (Campion & Campion, 1987). In another instance, management at Allstate agreed that it was important to find out the best new job design before it would be rolled out across the organization. Different geographically dispersed units were trying different things. We used this natural field experiment to discover the best design that we reported in *JAP* (Campion & McClelland, 1991). In yet another example, we were implementing work teams in a Donnelley factory. We were able to use a sister factory as the control group, which we also reported in *JAP* (Morgeson, Johnson, Campion, Medsker, & Mumford, 2006). In a final example, we were redesigning jobs at Eli Lilly, which we reported in *PPsych* (Morgeson & Campion, 2002).

How to Be Productive as a Student

1. *Publish term papers*. Students should view every term paper as a publication opportunity. I published several articles that came out of term papers. Several types of publications can come out of term papers. The first is the traditional review of the literature where you summarize the literature, critique it, and propose areas for future research. An example from my resumé is Campion (1983). The second is a practitioner paper where you summarize "best practices." These latter papers do not help you get an academic job, but they show activity, they help with your teaching, and they are good for consulting. An example from my resumé is Campion and Phelan (1981). A final type is simply a writeup of a small study or dataset you analyzed for a class. An example from my resumé is Campion and Goldfinch (1983).

2. *Only try to do A-class research*. Never start a research project that, if it turns out well, will still only be a B-class publication. First, the payoff from A-class publications is infinitely larger. In fact, publications in the top journals (e.g., *JAP, PPsych*) are the most important factor to promotion at the top schools. No amount of lower level publications will be equivalent to an A-class hit. It is like the difference between jumping a 2-foot hurdle (which most everyone can do) and a 4-foot hurdle (which few can do). Second, you may underestimate what it takes. Research studies intended for A-class outlets often end up in B-class outlets. Those intended only for B-class outlets at the start may end up in a C-class outlet or being unpublishable, and you have wasted your time.

3. *Cut the data thick.* Avoid the tendency to slice up a dataset into multiple related publications. The world of work is complex and multivariate, and studying underspecified models does not advance our knowledge. Plus, you may slice the baloney too thin, making the contribution too little to be publishable. It is hard to get into the top journals, so you should give them everything you have each time. Also, reviewers are watchful for this problem and will punish you for it. The *APA Publication Manual* expressly forbids piecemeal publication. I thank Don Schwab for this insight early in my career.

4. *Work with different people.* This profession, as well as most sciences, has an apprenticeship training model. You learn initially by working with an established member of the profession. As a student, you should work with several different faculty members to learn their different approaches and specialty areas. However, you should do the same after you finish school. Seek out other people you find interesting. It is appropriate to approach a stranger, either in person at a conference or by e-mail, to discuss their research and develop a relationship. This could be someone else at your career stage or a more senior person. Senior folks often have datasets and key insights as to the publication game but no time to write up the articles. It is a perfect match. Examples from my career include **Chad Van Iddekinge** and Julie McCarthy, both of whom I met at SIOP. And, of course, you should also work with junior faculty at your school. Although I have not done this extensively, a good example would be **Deidra Schleicher** at Purdue. We have all had a fun and productive relationship that resulted in four A-class publications (McCarthy, Van Iddekinge, & Campion, 2010; Schleicher, Venkataramani, Morgeson, & Campion, 2006; Schleicher, Van Iddekinge, Morgeson, & Campion, 2010; Van Iddekinge, Morgeson, Schleicher, & Campion, in press).

5. *Go study the animal in its natural environment.* Get out of the office and into the field. Most every new area of research should start with gathering qualitative data. Go interview managers, talk with employees, observe the work, ask lots of questions, and keep your eyes open for potential insight. This will make your theories more accurate, your research more relevant, and your stories in class more interesting. It may also get you consulting projects, and it is a good time.

6. *Don't put all your eggs in one basket, but don't have too many baskets.* Students developing a research record should carefully manage their number of eggs and baskets. Getting involved in too many projects may result in making insufficient progress on any of them, getting involved in too few may leave you with an empty basket. I have always thought that a good rule for students is to have three high-quality projects going at any given time.

7. *Do not play the probability game.* Many students look at the probability of getting an article accepted in a top journal and presume that the best strategy is to submit a lot of articles. This is a precarious strategy because if do marginal quality research, all of your articles will be rejected, but if you do only top-quality work, they will all find a good outlet.

8. *The odds are better than they appear.* The low likelihood of acceptance into the top journals (e.g., 15%) may seem discouraging. However, there are several top quality journals, and you will likely resubmit your article to another journal if it is rejected. As such, your chances of an A-class hit are probably more like a third. Probably another third get into good B-class journals. Therefore, if you keep at the publishing game long enough and do not give up, you will succeed. This also suggests that you should study topics that fit the mission of multiple good journals so you have multiple opportunities to resubmit, as noted previously.

9. *The secret to dealing with reviewers is to wear them down.* Basically, articles get accepted when the reviewers can no longer find a good reason to reject them. Therefore, it is somewhat of an endurance game. Unless the editor tells you specifically that you cannot resubmit, you should always revise and resubmit. Don't worry if the editor says it is "high risk." This is just editor speak for wanting you to try really hard to address all the problems raised. You know your study better than the reviewers ever will, so you have the advantage. Moreover, you have everything to gain. Keep revising and resubmitting until you wear them down. This lesson learned must be properly credited to **Fred Morgeson**, my former student and frequent coauthor, who ironically became a journal editor himself at *PPsych*. Now authors are wearing him down.

10. *Give them what they ask for.* If editors ever say, "If you had only done X, the article might have been publishable," you should go do X immediately and resubmit it. For example, if they say you should have collected some additional measure, go collect that additional measure. It does not have to be another full study but perhaps a small supplementary study to address some specific issues. If they criticize you for doing a lab study, do a little field survey to show that people in the real world have some similar thoughts. If they criticize you for common method variance, collect some additional data on a key measure to show convergence between methods. It may not fully solve the problem, but it shows a good effort. If you do this additional study, which is virtually always easier than a complete new study but more effort than other authors are willing to do, the editor is in a tough spot to reject your paper. **Talya Bauer**

was fantastic at this. She got many publications this way. Ironically, she is the editor of *JOM* today. I wonder if she ever says this in her decision letters.

11. *Publishing is sort of like cats fighting in the night*. Do not be put off by the apparent negativity of the publication process with what feels like a sole focus on criticisms. That is simply the way scientific contributions are judged. Like cats fighting in the night, it may seem like they are killing each other but they are actually making more cats. Scientists argue, but their real goal is to make more and better science and scientists.

12. *Publish even if you go into practice, and do some applied work even if you are an academic*. Following from the above, everyone should strive to live in both worlds to some extent. This is not only good for the profession by making both our research and practice better but also can be good for your career. In these uncertain times, it is good to have career options, and you might tire of one career and want to try something new. I am a good example. I spent 8 years in industry before going academic, and in my latter career as an academic, I have become extensively involved in consulting. The key to making the transition is to have both types of skills. If you want an academic job, you must have a publication record; if you want to do applied work, you must have experience doing real-world work.

How to Sustain Your Productivity in Later Career

1. *First assist, then lead, and finally coach*. Scholars can and should play different roles at different points in their career. If you are too busy to lead in your later career, then it is fine to coach others and take a junior author role. In fact, that may be the most appropriate role for senior scholars. I am the last author on everything I do these days.

2. *Publish applied projects*. There is a natural tendency to focus more attention on earning money and somewhat less on just publishing articles as you grow older. What many productive I-O psychologists do is publish their consulting projects. **Ben Schneider** called this his "consulting research."

3. *Review the literature.* At later career stages, you may be in the best position to see trends over time and put things in perspective due to your vast experience, so this is a perfect time of life to review the literature. Moreover, in consulting projects and expert witness court cases, it is always good to start with a review of the literature, so publish the literature review. There are many examples on my resumé (e.g., Campion, Posthuma, & Guerrero, in press; Levashina, & Campion, 2009; Posthuma, Morgeson, & Campion, 2002; Posthuma, & Campion, 2009).

4. *Stir things up a little.* Sometimes at a later career stage, you are in a good position to identify and draw out controversy in the field. These controversies should be published to document them in the hope that this will lead to their resolution (e.g., Campion, Outtz, Zedeck, Schmidt, Kehoe, Murphy, & Guion, 2001; Morgeson, Campion, Dipboye, Hollenbeck, Murphy, &

Schmitt, 2007). You will notice that Kevin Murphy is a coauthor in both articles. That illustrates another lesson. If you are going to a fight, bring along big strong friends who aren't afraid to mix it up.

5. *Work in teams*. Let's be totally honest. Working in teams increases your number of publications because it is less work per article than sole authoring. Promotion committees are much more focused on counting numbers of articles than numbers of coauthors. As long as you are first author enough times, people will not criticize you for having coauthors. Teams also have many other meaningful advantages, such as improving the quality of the research by the greater mental resources brought to bear, increasing the size and scope of research projects that can be undertaken due to greater resources, improving the quality of the writing from many copy editors, and moving the research along more quickly through the motivating effects of peer-based encouragement and deadlines. Of course, be watchful for the downsides of teams (e.g., shirking, conflict) and be willing to disband a dysfunctional team.

6. *Seek out those with complementary needs.* Late-career folks have data and interest but no time and should seek out early-career folks who need data and have the time to devote.

7. *There is no shame in helping publish someone else's dissertation or thesis*. Many dissertations are not published because the student is off to a job or sick and tired of the study. There is nothing wrong with helping someone publish their dissertation and getting a junior authorship for your efforts. These projects are great opportunities because much of the work is done. This does not apply to just your students but also to others (e.g., Gollub-Williamson, Campion, Malos, Roehling, & Campion, 1997). Sometimes I have also needed help publishing the dissertations of my students (Mumford, Van Iddekinge, Morgeson, & Campion, 2008).

8. *Revise stalled out projects by bringing in fresh talent.* Many projects do not succeed initially because researchers run out of energy, get distracted, or cannot figure it out. Also, new coauthors bring new insights and fresh enthusiasm. My resumé has many of these examples. One example is a paper that was revised three times by adding coauthors (Morgeson, Johnson, Campion, Medsker, & Mumford, 2006).

9. *Always replicate.* Any good publication is worth repeating. It is also good science because it confirms the findings. But remember, like a sequel in movies, it must add some new drama or excitement, which in the research business we call a "constructive replication." For examples, see Campion (1988), Campion & McClelland (1993), and others.

10. *Always try to continue working with your students beyond their dissertations*. Most young scholars are not really ready to publish well on their own at the time of graduation. It is good for their development and good for your continued productivity to continue to work with them. There is a key point to note, however. It is all right to expect them to do more of the work-

but not all of it like with a dissertation. Also, they are now peers, not subordinates, and they must be treated that way.

11. *Find a Fred Morgeson or a Talya Bauer.* When you have those rare great students, continue to work with them if they are willing. They keep you young and motivated.

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