CONTRIBUTIONS

Commentary

A Primer on How to Apply to and Get Admitted to Graduate School in Ecology and Evolutionary Biology

In my experience, most students considering graduate school have little knowledge of how to gain admission, how to choose a program, or how to find and select an advisor. Here, I try to remedy these problems with a basic step-by-step guide for the application process and for the prelude to that process. It is my hope that faculty and graduate students who read this and find it valuable will pass it on to interested undergraduates. This guide should get students started down the right track and allow them to ask more refined questions about the whole application process. Overall, this primer applies mostly to graduate programs in ecology, evolution, systematics, and natural resources. In general, students should know right off that applying to graduate school in these disciplines is much different than applying to universities from high school, or applying to medical school, law school, or even graduate programs in other areas of biology.

For the student, it is never too early to start thinking about graduate school. Before applying, however, you should be pretty confident that graduate school is right for you. It can be a long haul (typically 5-6 years for a Ph.D.) and complete commitment is required for success. If you are not sure, or if you are burned out, take a year or two off, gain some experience, travel, or get a job and bank some money, and then carefully consider postgraduate education.

I. Prelude 1: Grades and GREs

Most schools require that you take the Graduate Record Exam (GRE). Although your grade point average (GPA) and the GRE are not always good predictors of success in graduate school, universities will use these metrics to compare and evaluate applicants. Here is some advice:

1. *Try to graduate with at least a 3.0 GPA.* The vast majority of graduate schools have a 3.0 as their cut-off. This is reasonable and suggests that you took your coursework seriously and learned the basics. Still, if you are below this, all is not lost, so do not lose hope (see sections on the GRE and gaining research experience). Note that some programs will emphasize your GPA in the last two years of your degree program, or within your major. If your GPA is higher in these areas, emphasize this in your application. The best or most competitive programs will typically look for GPAs that are substantially higher than a 3.0, while smaller programs, and programs that only offer a Master's degree, may be somewhat less picky.

2. *Try to score well on the GRE.* Most universities or departments will require that you take the general GRE exam, which attempts to evaluate your quantitative, verbal, and analytical abilities. Some will require that you take the biology exam as well. Check with the prospective school or department to be sure.

Your score on the GRE will often be more important than your GPA because there is some belief that GRE compares students on a more equal footing than a GPA. A high score on the GRE can make up for a low GPA (or sometimes vice versa). Note that, like the GPA, most schools will have a cut-off or minimum acceptable score. Some guides to graduate schools or information provided by the university will specify acceptable scores, or the average scores of recently admitted students. Remember, however, that these are usually just targets, and students with lower scores are often admitted, so if you really want to go to Stanford, you might as will give it a try.

3. *Study for the GRE.* When you study for the GRE, you should at the very least purchase one of the many preparation guides available at local bookstores. Practice taking the test under the actual conditions of the exam until you feel comfortable with the format of the test, the speed at which you should work to finish each section, and the overall length of the test from start to finish. At most, if you can afford it, consider taking a formal course on preparing for the GRE (e.g., Kaplan) or check to see if your undergraduate institution offers free help and instruction on preparing

for the GRE. Studying and practicing for the GRE has been shown to significantly increase your score! Note that some universities and other funding agencies award multiyear fellowships and scholarships based on your performance on the GRE, so even a modest improvement in your score at the high end may help you qualify for one of these awards.

4. *Hang in there.* Overall, if your grades and GREs are both relatively low, but your ultimate goal is a Ph.D., do not despair. Consider trying to find a quality Master's program where your chance for admission might be higher. In a Master's program, you can conduct interesting research and demonstrate directly that you have the skills required to pursue a Ph.D. A quality Master's thesis, along with enthusiastic letters of recommendation, can more than make up for relatively low GRE scores and a mediocre GPA.

II. Prelude 2: Gaining experience

1. *Start doing or participating in actual scientific research early.* Know that classes are only one part of your education. You should begin to obtain real hands-on research experience as early as your sophomore year. *Research is the most important thing you can do to prepare yourself for graduate school* because it will teach you not only how to do research, but whether you like research and if so, what areas of research you enjoy the most. Try to obtain research experience by finding a graduate student or faculty member who is doing interesting work, and see if you can:

a) Volunteer.

b) Work as a paid field or laboratory assistant.

c) Conduct independent research (field or laboratory research project).

d) Conduct an independent study (library project that will require reading in the primary (journal) literature).

A note of caution is due here. Do not do any of these things if you are just trying to fill out your resume. You should be genuinely interested in the research project. If you are not, it will end up being a bad experience for you and the researcher. Overall, look around and try to find a lab that is doing research that interests you.

2. *Participate in a scientific meeting.* After gaining experience by one of the above means, try to attend and, if possible, present a paper or poster at a scientific meeting. A paper is usually a short 12-minute oral presentation of your research, while a poster displays your research with text and figures. There are many possible scientific meetings to choose from, beginning with more local meetings that are often sponsored by state-wide scientific academies, to national meetings such as the Ecological Society of America's meeting held annually at different locations around the US. Ask graduate students and professors for advice on which meetings to attend and see if you might be able to go along with them.

Even if you do not have independent research to present, you should still try to attend scientific meetings. Meetings typically last 2–4 days, and consist of a series of short scientific presentations on current research by both students and professors. Meetings will give you a flavor of the type of research that is out there, give you a chance to meet prospective advisors, and probably convince you that you can do interesting research. Most of all, meetings are fun!

3. *Write and try to publish a scientific paper.* This could result from your independent research or an independent library project; it will almost always require the help of a professor or graduate student. Do not think that this is beyond your ability, but it will require dedication and perseverance. Nothing impresses a prospective advisor or graduate school like a publication in a refereed scientific journal! This will no doubt help you get into a top program or is an excellent way to survive low GRE scores or a low GPA.

4. *Get to know your professors.* Recommendations that only include your performance in class will be considerably less influential than recommendations that evaluate your performance both in class and outside of class, conducting independent research, participating in an independent study, or working as a volunteer or paid field assistant. To gain admission to graduate school, you will need three recommendations and sometimes four. These recommendations are extremely important. Your professors are likely to be friends with, or at least acquaintances of, the professors that you are applying to work with. Potential graduate advisors will often trust the recommendation of a close colleague or scientific peer more than a GPA or GRE score.

5. *Participate in departmental events.* These could include departmental picnics or socials, undergraduate biology clubs, and perhaps most importantly, if your department has a weekly seminar series or journal club (an informal meeting of scientists to discuss recent scientific papers), by all means attend it. At first these meetings may seem boring or unintelligible, but with time, as you understand more, they will become more interesting and comprehensible.

6. *Enroll in graduate-level courses or seminars.* Do not think these courses will be over your head; often they are no more difficult than undergraduate courses. They can expose you to the flavor and tone of graduate school and will allow you to interact on a regular basis with graduate students. These courses can give you a window into the graduate school experience.

III. Applying

1. *Should you do a Master's degree first?* Graduate students at research universities typically plunge right into a Ph.D. program. However, don't turn your nose up at completing a Master's degree first. Consider completing a Master's degree if you are unsure whether you want to commit to a lengthy Ph.D. program, or if you are not sure if research is your thing. You will get much-needed experience, and will be able to choose a Ph.D. program with much greater insight.

2. *Application deadlines.* Applications are due usually from mid-December to early February for a program that begins the following September. Only a small number of programs accept graduate students in the middle of the year; thus, it is a once-a-year process!

3. *Choosing an area of research.* Identify the general area of research you would like to pursue. It should be more specific than just ecology or plant ecology. Seek advice from faculty and graduate students. Although it may be difficult, it is important to try to narrow your interests. This is also why it is important to gain exposure to different research areas as an undergraduate so that you can *begin* to narrow your interests.

4. *Selecting a potential advisor.* Identify 6–10 professors who might serve as your potential advisor in graduate school (begin by using the Internet). These should be professors who are conducting research in an area you are interested in, and at universities you are interested in attending. Do not go into this blind! Ask professors, graduate students, and anyone else you trust for advice on appropriate advisors. Your selection of an advisor is *the most important choice* you will make with regard to your graduate degree. It is almost always more important than your choice of a university. Although it may be possible to switch advisors once you enroll, switching advisors can often be awkward and politically difficult, and there may not be another professor who has an opening for a student or one who matches your research interests. Thus, choose your advisor wisely in the first place (for some advice, see *The interview* below).

5. *Selecting an institution.* Select a range of institutions in terms of quality, from major research universities to smaller colleges. You should choose at least one university where you are fairly certain of being admitted. Note: it is sometimes the case that large research universities may be less likely to accept Master's students, or that these applicants are given lower priority than students applying for doctoral programs. This varies by department and discipline, so check to be sure.

6. *Do your homework.* You should read the most recent scientific papers authored by the faculty member you are interested in working with, and find out whatever you can about this person. You will not necessarily be expected to fully comprehend these papers. Still, having a reasonable understanding of the research being conducted in the field or lab will allow you to ask better questions (during an interview, see below), make you seem more astute, and make you a better applicant. *Do not forget to do this!* The strongest applicants will be those who can discuss issues in their field of interest; these candidates will stand above the rest.

7. *The letter of introduction and resume.* Write a personal letter or send an e-mail to each faculty member with whom you are interested in working. This letter should go out well ahead of the application deadline (no later than mid-October to mid-November). In the e-mail, you should say briefly who you are, why you want to work with that person, and your background and experience. Find someone to read and edit this letter, preferably a graduate student or faculty member. In this letter, focus first on your research experience and secondarily on your academic performance. If you have research experience, give the name of the professor(s) with whom you have worked. Ask specifically whether the prospective faculty advisor will be taking on any students in the next academic year. This letter should be limited to one page. Include a resume or Curriculum Vitae (a long resume used in academics) at the end of the e-mail or appended to the letter. Ask advisors, graduate students, or faculty about how to construct a resume or Curriculum Vitae, or contact your placement office.

8. *The follow-up letter.* When you hear back from your initial letters of inquiry, follow whatever recommendations or advice they give you in the letter. If you do not hear anything, follow up your inquiry about 3 weeks later with a short and polite e-mail asking if they received your initial inquiry, and if so, whether they would consider you as a prospective graduate student. Faculty may be out of town for extended periods, so you might consider calling the department secretary, and inquiring about that faculty member's whereabouts.

9. *The interview.* Hopefully some of the professors you contacted will be interested in you. Prior to being accepted, arrange a trip to any and all institutions you can afford to visit. Some universities will have money to fly in excellent prospective candidates for an interview. Wear clothes that are nice but casual. To get into many programs, and for you to evaluate the program, *an interview or informal visit is extremely important.* This visit or interview will:

a) Let you know if you want to work with this person. Major personality differences between a student and an advisor can become a disaster. Ask yourself what you want in an advisor. While at the interview, ask yourself the following questions: Can I get along and work comfortably with this person? How does this person currently interact with their students (regular lab meetings, daily guidance, moderate guidance, total independence)? Have past students done well? Did past students publish their research in good journals? Are students finding jobs on completion of their degree? How are students supported financially (part time teaching, research assistantships, Pizza Hut? see *Financial support* below).

Ask the graduate students what they think of their advisor and of the program in general. Get individual graduate students alone, one on one, so they can tell you what they really think, and so there is less fear that this information will leak out. Ask them if they had to do it all over again, would they? Remember, your selection of an advisor is *the most important choice* you will make with regard to your graduate degree. In general, if the graduate student population is excited and enthusiastic about their advisors and the program, then you have probably found a great place. A note of caution is in order here: many graduate programs will have a small number of disgruntled students who are often vocal and overly negative. Make sure you gauge the graduate population and program as a whole and not the sour comments of a few unhappy students. Nonetheless, a general negative tone from the graduate students is a bad sign.

b) Let the prospective advisor, graduate students, and laboratory personnel evaluate you and decide whether they want you hanging out in their lab. Note that current graduate students will likely have input into the decision on selecting new students. Additionally, you will likely meet with other faculty who will often have a say or vote in graduate admissions. Thus, before your interview, you should read up on the other most relevant faculty and their research interests. Reading some of their recent publications is highly recommended.

c) Allow you to inquire further about the program. You may want to ask such questions as: how many courses are required for the degree? How reasonable are the exams and hurdles associated with the degree? Graduate students are an excellent source for this information, but remember to query as many students as possible. A trip to the local pub may be helpful here.

10. *The application packet.* Fill out the application completely and type it. Make sure you get it in on time. Note that universities charge a fee to apply ($25–100). Most application packets will include an application form that will typically require you to write an essay about your goals or reasons for wanting to pursue a graduate degree. Consider your goals carefully and remember that most faculty are looking for committed, mature students, who will make research their priority. Generally, the more specific you can be in the essay the better. It is important to demonstrate that you have knowledge in the research area you hope to pursue.

11. *Recommendations.* You will need to secure three and sometimes four recommendations. These recommendations should come primarily from faculty, but one may also come from senior graduate students or job supervisors. Choose people who know your abilities both inside and outside the classroom. Ask each person if they are willing to write you a positive letter of recommendation (most will be quite frank). After choosing which programs to apply to, give each reference a brief description of your goals and interests, a copy of your resume, any forms they are required to fill out (typically, there is a formal recommendation form), and stamped envelopes addressed to each institution. Give them this information all at once and well before the application deadline (at least 3–4 weeks). Overall, these materials will allow your references to write a detailed and personal letter and get them in on time. Faculty can be notoriously bad about getting recommendations in on time. It is your job to insure that individuals who are writing your recommendations actually send them in. *Double check* this, preferably by contacting the universities you are applying to, not by asking the faculty member. If the letters have not arrived by close to the due date, contact the faculty member with an e-mail, phone call, or personal visit and request that they send the letter ASAP.

12. *Financial support.* Most institutions offer financial support in the form of Teaching Assistantships, Research Assistantships (sometimes provided directly by the professor), and Fellowships. This support often comes with full tuition remission (i.e., school is free) and a modest but usually livable salary in exchange for conducting research or teaching. A fellowship typically includes a salary and tuition remission with relatively few strings attached. The National Science Foundation offers prestigious 3-year fellowships that you can apply for in the year prior to enrolling or in your first year of graduate school (see <http://www.ehr.nsf.gov/EHR/DGE/grf.htm>). Find out whether you are likely to be awarded financial support upon admission. If so, what kind? Support can vary dramatically among institutions in terms of the actual amount of the salary, whether the salary comes with tuition remission, and how long the support will be guaranteed (from no guarantees to 5 years or more). *Find out the facts regarding your support!* Other questions to ask include: Will there be support during the summer and is there funding for graduate student research? Graduate students enrolled in the program are often a good source of information about whether the financial support is reliable and also livable. Support of $15,000 a year goes a long way in Beaumont, but not so far in New York City.

13. *Accepting an offer.* Once you have decided that a program is right for you, call them to accept their offer and send them a written acceptance. Do not accept an early offer as a "back-up" in case your preferred school declines your application; your acceptance means you agree to attend that school. If a deadline is approaching at one school and you still have not heard from other schools, call and see if you can obtain an extension.

14. *Declining an offer.* Once you have crossed a school off your list or have accepted an offer from another school, immediately contact the other schools and let them know you plan to go elsewhere. Write a short e-mail to each faculty member with whom you interviewed, thank them for considering your application, and let them know where you decided to enroll. Do not forget this simple courtesy; it will save you embarrassment when you run into them at scientific meetings. Additionally, there are students on waiting lists who will appreciate your timely decisions regarding these matters.

IV. Some concluding remarks

1. *Thoughts from a successful graduate student.* When I gave this to a number of graduate students to critique, one had this insightful commentary. Tell prospective students that "Graduate school is not for everyone. It is hard work at low pay,

and the few jobs available at the other end offer hard work at low pay. Do not go to graduate school because you like school; graduate school is very different from the undergraduate experience. Sometimes the choice not to go will be the right choice and send you off on an alternative and rewarding path." This is sound advice.

2. *Get advice from others.* Overall, this is just a primer on applying and getting accepted into graduate school. It reflects primarily my opinion and experiences. Seek out additional advice from professors, graduate students, and advisors. Procedures and strategies on admission can vary from one institution or discipline to another.

3. *Thrive in grad school and dodge the train.* Remember, you are trying to go from one who consumes knowledge to one who produces it. Make research your priority. Know that for every Ph.D. student, there *is* light at the end of the tunnel, but for many, that light will be the headlights of an oncoming train. To help yourself avoid the train, the following two articles are highly recommended and have been read by hundreds of graduate students.

Stearns, S.C. 1987. Some modest advice for graduate students. ESA Bulletin **68**:145–150.

Huey, R.B. 1987. Reply to Stearns: some acynical advice for graduate students. ESA Bulletin **68**:150–153.

These two articles offer a pithy and provocative exchange on how to be a successful graduate student. They each offer humorous advice and sage wisdom. *They should be read by all beginning graduate students.* For a lengthy and more formal treatise on surviving and thriving in graduate school, see: *Getting What You Came For: the Smart Student's Guide to Earning a Master's or a Ph.D.* Robert L. Peters, Noonday Press, 1997.

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Walter P. Carson
Program in Ecology and
Evolutionary Biology
Department of Biological Sciences
University of Pittsburgh
Pittsburgh, PA 15260
E-mail: Walt+@Pitt.edu

Research Productivity and Reputational Ratings at United States Ecology, Evolution, and Behavior Programs

> *Reputation is an idle and most false imposition; oft got without merit, and lost without deserving.*
> —Shakespeare

It is well known that the reputation of a research program has a major impact on its ability to attract top-caliber graduate students and faculty, to secure external funding from federal agencies, and to compete for resources within universities (Roush 1995). In 1995, the National Research Council (NRC) published a survey (Goldberger et al. 1995) of reputational ratings for 41 research fields at Ph.D.-granting universities in the United States. Included in this survey was a reputational rating of 127 Ecology, Evolution, and Behavior (EEB) programs across the US. One of the main stated objectives of the NRC survey was to "permit analysts to extend their work on the nature of 'reputational ratings' or the opinions of faculty peers about a program." This analysis will discuss the broader interpretation of the NRC ratings, and specifically, how these ratings relate to research productivity at the 63 top-rated Ecology, Evolution, and Behavior programs in the United States.

In the original NRC ratings, considerable discussion was devoted to the meaning of "reputation" and factors that might bias ratings. Authors of the NRC report were acutely aware that many factors besides the actual quality of a program might influence its NRC reputational rating. An important assumption of the NRC survey is that a program's reputation is related to its scholarly productivity. It was also clear from prior NRC reputational surveys that the size of a program is often correlated with its reputational rating. There are a number of fair and perhaps unfair reasons why this is generally the case (Goldberger et al. 1995). Other factors that might influence or bias a program's NRC rating include the presence of "stars," or "visibility," and the overall reputation of the university. Highly prestigious universities "may cast a 'halo' over [programs] which do not merit as lofty a reputation" (Goldberger et al. 1995).

Toutkoushian et al. (1998) recently published a general analysis of the NRC ratings across most research fields using data published in the NRC assessment. Their study found that NRC reputational ratings are positively correlated with the size (e.g., number of faculty) and per capita productivity (publications per faculty) of programs across all research fields. There is also a strong tendency for programs located at private and at prestigious universities to have substantially higher reputational ratings than expected based on their size and productivity alone. By comparing the ratings for fields examined in both the 1982 (Jones et al. 1982) and 1995 (Goldberger et al. 1995) NRC reputational assessments, Toutkoushian et al. (1998) found that program reputations change quite slowly; the best predictor of a program's 1995 reputational rating was its 1982 rating. (EEB was not one of the fields assessed in the 1982 survey.) The 1995

NRC survey found the assessments of program faculty quality (e.g., reputational rating) and program teaching/training effectiveness were generally very highly correlated (r^2 = 0.90). Toutkoushian et al. (1998) concluded that this was because reviewers had insufficient information to judge the teaching effectiveness of programs, and thus gave very similar assessments for reputation and teaching effectiveness.

In this study, we will examine how an independently derived measure of EEB program scholarly production correlates with reputational ratings. This measure of scholarly productivity was obtained by searching the electronic database BIOSIS for EEB publications in 226 EEB journals during 1988 to 1997 (see Fig. 1). Our analysis was restricted to the top-rated 50% of EEB programs in the United States (i.e., the top 63 programs). This literature search produced a productivity database with > 24,000 EEB journal articles. We also compared NRC reputational ratings to the size of programs, the university's overall reputation, whether the university was private, the average quality of journal articles, the number of program faculty who were members of the National Academy of Sciences, and change in program productivity during the period 1988–1997.

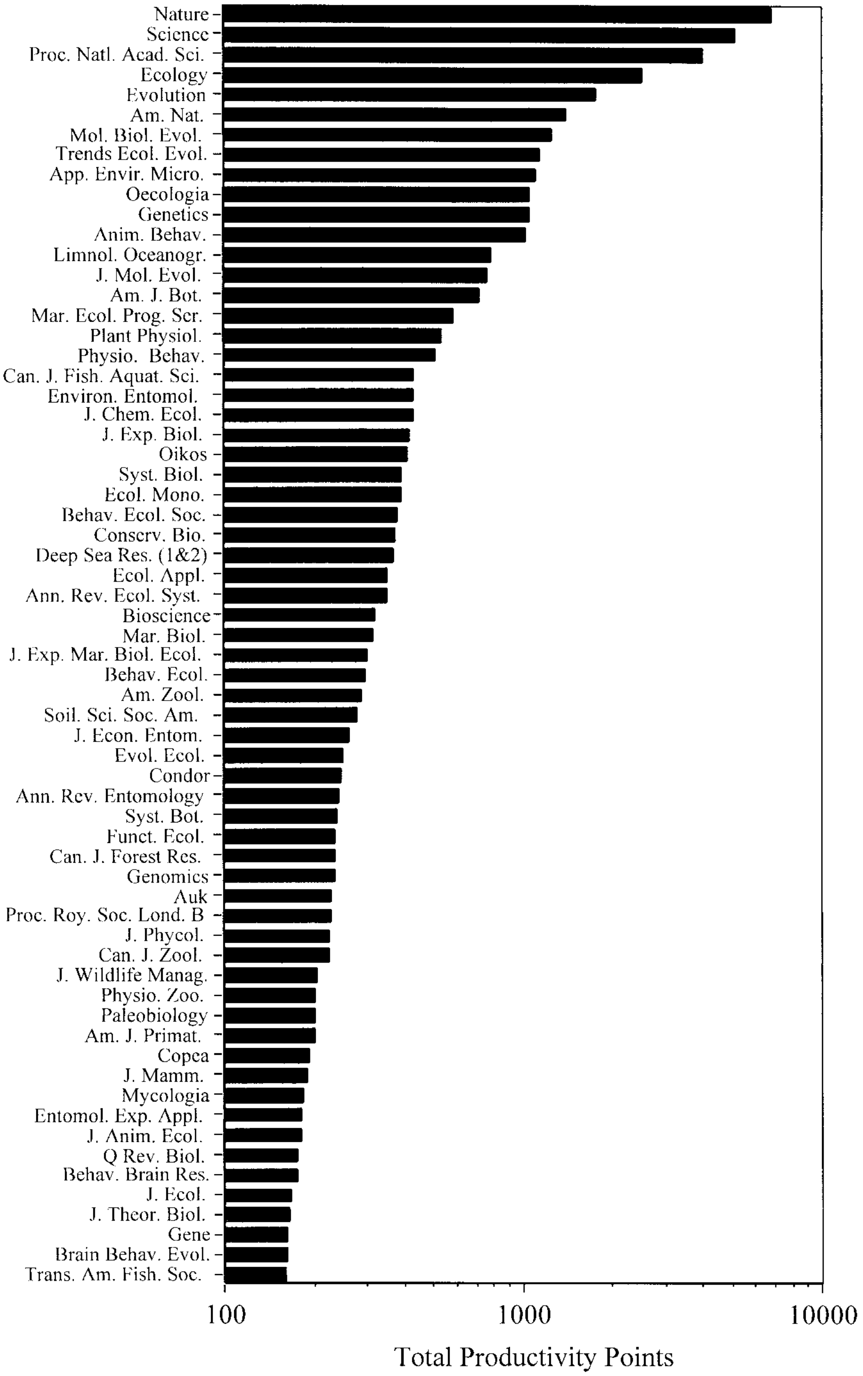

Fig. 1. Contribution of the top 64 journals of the 226 Ecology, Evolution, and Behavior (EEB) journals used in this survey to our measure of program productivity. The journals encompassed 85% of our entire sample of EEB scholarly production at the top-rated EEB programs.

Methods

Compilation of data

A more detailed description of our methods and the journals included in this analysis can be found at: <http://www.ce.washington.edu/NRCEEBsurvey.htm>.

The program reputational ratings used for our analysis were obtained from the NRC report by surveying approximately 100 reviewers per program, with each reviewer asked to rate the programs on a 0–5 scale (0 equal to low and 5 equal to high quality). Measures of uncertainty in NRC ratings for each program were also taken directly from the report. The overall NRC rating for universities included in this analysis was calculated as the median program percentile for all programs rated in the NRC survey at that university.

Our measure of publication productivity was calculated as the total number of publications in EEB for the period 1988–1997, weighted by the impact factor of each journal. We searched the electronic database BIOSIS for 226 EEB journals using a series of "affiliation" searches for each program. The NRC's measure of program productivity was calculated by multiplying the reported number of faculty in the NRC report by the ratio of publications per faculty in the NRC report. The number of program faculty was taken directly from the NRC report. The number of program faculty in the National Academy of Sciences Section 27 (Population Biology, Evolution, and Ecology) was compiled for each institution included in this survey. We calculated per capita productivity by dividing our measure of program productivity by the number of faculty. Change in the program's productivity was assessed by comparing our measure of program productivity for the periods 1988 to 1992, and 1993 to 1997.

The percentage productivity points from papers published in *Nature, Science,* and *The Proceedings of the National Academy of Sciences* was calculated as the portion of total productivity attributable to these journals. The average journal article quality was calculated as our tally of scholarly productivity divided by our tally of journal articles. We also calculated a Shannon's diversity index for the publication productivity data.

We compiled indices of program strengths for 21 subdisciplines (e.g., evolution, animal behavior, marine biology, entomology, ornithology, conservation biology, etc.) of EEB by totaling the number of productivity points within these categories for the various specialty journals. These indices were compiled by totaling the productivity for journal articles representing that subdiscipline and not by compiling actual papers from that subdiscipline. For example, if a paper on birds was published in a conservation biology journal, the program would be credited with productivity points in conservation biology, and not ornithology.

Analyses of data

First, we compared each of the main parameters described above against the NRC reputational ratings. Then, we developed a multivariate regression model to predict program NRC reputational rating. This was done by regressing one form of each of the major predictor variables against the NRC ratings. For scholarly productivity, we used our measure of scholarly output. For faculty size, we used the log-transformed (to normalize the distribution) count of total faculty as reported in the NRC report. For journal article quality, we used the average quality parameter. We also used log-transformed change in productivity, number of program faculty in the National Academy of Sciences, and whether the program was located at a private or a prestigious university. To avoid the most obvious collinearity problems, we did not include different versions of similar variables in our analysis (e.g., productivity, our measure of publications, and the NRC measure of publications).

Results and discussion

Scholarly productivity at U.S. EEB programs

Fig. 2 depicts the total scholarly productivity for 63 top-rated EEB programs in the United States. The five most productive EEB programs are located at UC Davis, Cornell University, UC Berkeley, UC San Diego, and the University of Georgia. This histogram shows that a few programs stand out as being much more productive, but many EEB programs have similar productiv-

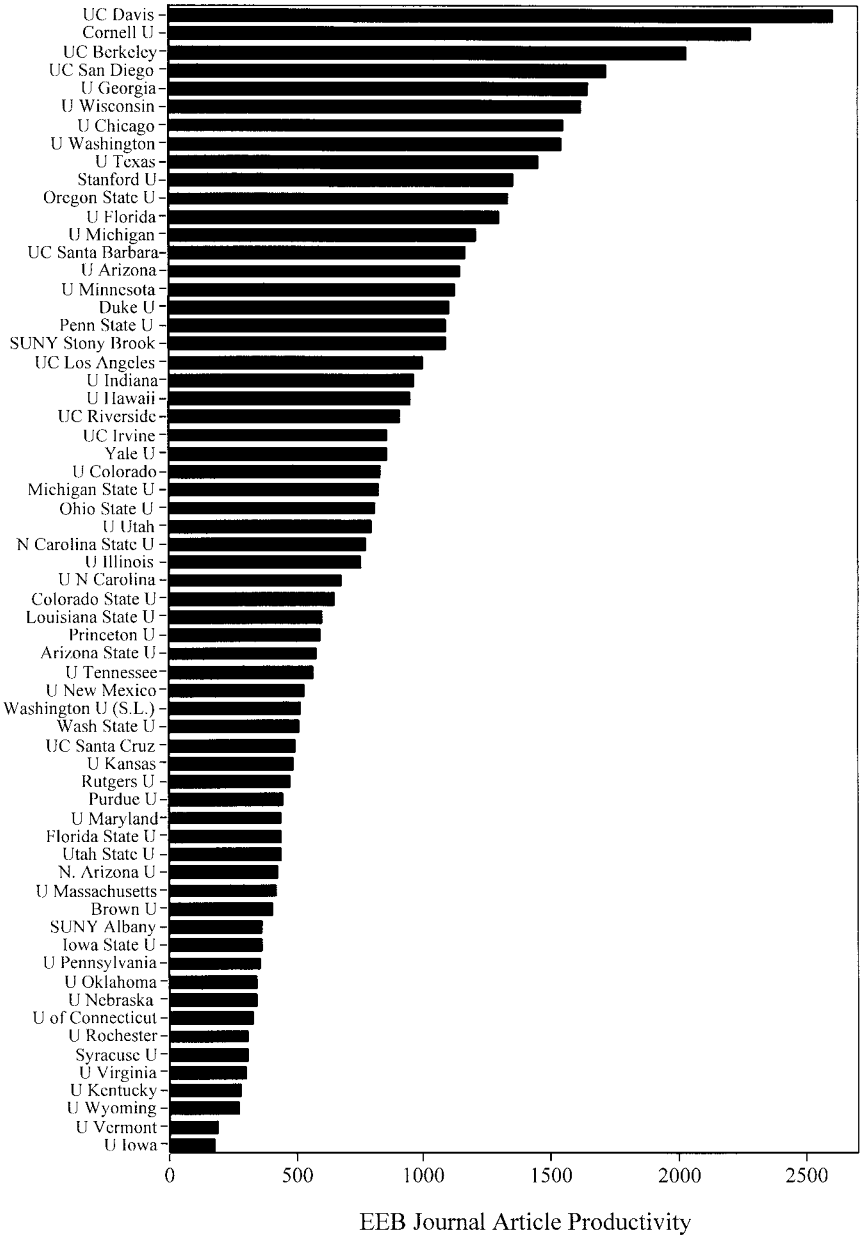

Fig. 2. Journal productivity (number of articles) at the top-rated 63 Ecology, Evolution, and Behavior programs.

ity. Table 1 lists these and other statistics for the top-rated 63 programs.

Correlation matrix of predictor variables

A correlation analysis of predictor variables against NRC reputational ratings shows the single best predictor of a program's reputational rating was our measure of EEB program scholarly productivity, $r^2 = 0.59$; see Table 2. A simple tally of EEB publications (unweighted for impact) had a substantially weaker association with the NRC ratings, $r^2 = 0.41$. The NRC report's tally of EEB publications had a much weaker association with the reputational ratings, $r^2 = 0.18$.

Table 1. Various statistics for the 63 EEB programs examined in this analysis.

University	Publication Productivity	NCR Rating	Overall Rating	Type	Faculty	NAS Members	Article Quality	Change in Prod. (%)	NAS/Our Publications	Top 10 Subdisciplines
UC Davis	2605	4.42	37	Public	197	3	2.13	16	1.73	17
Cornell U	2282	4.44	16	Private	92	1	2.27	15	1.22	12
UC Berkeley	2029	4.29	7	Public	57	1	2.53	-8	0.72	9
UC San Diego	1719	3.82	16	Public	39	0	3.13	-1	0.74	4
U Georgia	1650	3.87	55	Public	84	3	1.94	26	1.14	13
U Wisconsin	1616	4.18	25	Public	90	1	2.03	50	1.40	15
U Chicago	1549	4.51	13	Private	30	0	3.91	-23	0.69	3
U Washington	1540	4.30	28	Public	41	4	1.97	17	0.50	10
U Texas	1453	4.12	24	Public	54	0	2.30	0	0.65	9
Stanford U	1351	4.51	14	Private	10	4	3.49	-8	0.65	4
Oregon State U	1335	3.74	56	Public	127	1	1.81	-2	1.29	9
U Florida	1301	3.57	47	Public	45	0	1.77	-29	0.56	12
U Michigan	1206	4.10	20	Public	52	2	2.00	-28	0.71	8
UC Santa Barbara	1165	3.81	39	Public	28	0	2.53	11	0.75	2
U Arizona	1143	3.80	35	Public	26	1	2.07	43	0.31	7
U Minnesota	1126	3.88	32	Public	66	2	2.19	17	1.17	6
Duke U	1102	4.49	27	Private	62	2	1.89	-8	1.00	6
Penn State U	1091	3.60	40	Public	93	2	2.60	26	2.29	3
SUNY Stony Broo	1090	4.12	40	Public	29	2	2.51	-10	0.71	3
UC Los Angeles	996	3.82	20	Public	41	2	2.59	-20	1.28	4
U Indiana	960	3.49	38	Public	27	0	3.01	-7	0.71	2
U Hawaii	950	2.94	62	Public	64	1	2.32	-25	0.78	2
UC Riverside	910	3.60	51	Public	26	2	1.99	4	0.52	2
UC Irvine	858	3.77	29	Public	21	2	2.96	30	0.88	2
Yale U	857	3.83	16	Private	59	1	3.01	-29	3.48	4
U Colorado	832	3.46	43	Public	38	0	2.20	-19	0.81	2
Michigan State U	825	3.41	48	Public	39	0	2.08	18	1.06	3
Ohio State U	808	3.27	41	Public	105	0	1.96	-35	1.36	4
U Utah	796	3.65	47	Public	16	0	3.12	-17	0.46	0
N Carolina State	772	3.20	43	Public	75	0	1.86	-35	1.44	4
U Illinois	754	3.52	29	Public	74	2	2.25	-33	1.72	1
U N Carolina	674	3.33	34	Public	28	0	2.64	1	0.69	0
Colorado State U	647	2.99	53	Public	17	1	1.41	18	0.35	4
Louisiana State U	598	2.91	65	Public	74	0	1.35	-19	1.36	3
Princeton U	590	4.34	9	Private	11	0	2.84	30	0.31	1
Arizona State U	579	3.41	51	Public	32	0	1.87	12	0.82	2
U Tennessee	563	3.35	68	Public	61	0	2.22	5	1.65	1
U New Mexico	529	3.24	37	Public	30	0	2.33	-12	0.96	0
Washington U (S.	516	3.94	36	Private	24	0	2.55	50	2.20	2
Wash State U	511	3.37	65	Public	84	0	1.92	-10	4.03	0
UC Santa Cruz	494	2.93	45	Public	11	0	2.46	5	0.49	1
U Kansas	487	3.46	60	Public	46	1	1.87	-25	0.93	0
Rutgers U	475	3.60	36	Public	39	0	1.74	9	1.86	2
Purdue U	450	3.10	31	Public	34	0	1.71	-20	0.96	1
U Maryland	443	3.28	44	Public	70	0	1.89	75	3.08	0
Florida State U	443	3.41	58	Public	16	0	1.52	-24	0.37	1
Utah State U	442	3.39	79	Public	70	0	2.29	-26	1.44	1
N. Arizona U	424	3.35	87	Public	22	0	1.98	3	0.91	1
U Massachusetts	420	3.39	46	Public	65	0	1.44	36	1.76	1
Brown U	406	3.30	32	Private	11	0	2.37	81	0.59	0
SUNY Albany	361	3.10	61	Public	8	0	3.61	43	0.50	0
Iowa State U	360	3.00	50	Public	25	0	1.59	55	0.82	1
U Pennsylvania	353	3.90	20	Private	55	1	2.15	-2	3.01	0
U Oklahoma	344	3.11	69	Public	41	0	1.71	-11	1.11	0
U Nebraska	339	2.96	70	Public	17	0	1.57	116	0.56	2
U of Connecticut	330	3.35	60	Public	28	0	1.88	-18	0.68	1
U Rochester	308	2.95	38	Private	12	0	3.22	-47	1.26	0
Syracuse U	308	3.09	55	Private	7	0	3.80	-30	0.94	0
U Virginia	299	3.14	33	Public	42	0	1.91	-4	1.97	0
U Kentucky	282	3.04	63	Public	17	0	1.50	28	1.00	0
U Wyoming	272	3.00	77	Public	39	0	1.09	4	1.33	1
U Vermont	186	3.04	63	Public	27	0	1.51	-10	1.39	0
U Iowa	175	2.94	46	Public	33	0	2.06	-46	3.22	0

Table 2. A correlation matrix of the main parameters measured in this analysis. Reported values are regression coefficients (r^2), boldface indicates that the regression was significant at the P >0.0001 level, italic indicates significance at the 0.05 level, and (-) indicates that the correlation coefficient (r) was negative.

	NRC Ratin	Production		
	r^2	r^2	Mean	SD
NRC Rating	-		3.56	0.47
Production	**0.59**	-	830	530
Log Production	**0.59**	**0.89**	2.84	0.27
Overall NRC Rating	**(-)0.47**	**(-)0.28**	42	18
Publications	**0.41**	**0.86**	379	235
Top 10 areas	**0.38**	**0.74**	3.03	4.06
NAS Members	**0.35**	**0.32**	0.66	1.05
NRC Publications	*0.18*	**0.41**	286	232
Production/Faculty	*0.17*	0.08	24	20
Private or Public	*0.14*	0.00	0.17	0.38
Percent Nat., Sci., PNA	*0.12*	0.14	28	12
Publication Quality	*0.11*	0.05	2.23	0.61
NRC Faculty	*0.08*	**0.30**	46	33
Log Faculty	*0.07*	**0.21**	1.56	0.31
Publication Diversity	0.01	0.05	3.53	0.38
Log Prod./Log Faculty	0.01	0.00	1.88	0.39
Change in Productivity	0.01	0.00	3	31

After productivity, the next best predictors of a program's NRC rating were the university's overall NRC ratings, the number of subdisciplines in the top 10, and the number of program faculty in the National Academy of Sciences. Production per faculty, whether the university was private or public, and average quality of publications had statistically significant, but moderately weak, associations with the NRC ratings. Scholarly productivity of EEB programs was correlated with the number of subdisciplines in the top 10, as well as the number of program faculty in the National Academy of Sciences, size of faculty, and the overall NRC ratings of the universities.

A multivariate model

A four-factor regression model explained 77% of the variability in NRC reputational ratings. The most important variable in this model was the publication productivity of the various programs, which accounted for 38.2% of the variability. Whether the university was private accounted for 14.8% of the variability, its overall NRC rating across all programs accounted for 12.3% of the variability, and the number of NAS members on its faculty accounted for 9.8% of the variability in the program NRC ratings.

The coefficients given in Table 3 can be converted to rank equivalencies to place these values in a more intuitive perspective. According to a regression between the rank ordering of programs and their NRC ratings, one rank in the program ordering is worth about 0.0252 points on the NRC 0–5 scale; rating = 4.37 - 0.0252 x rank, r^2 = 0.96. Thus, according to the coefficient for productivity, 500 productivity points would be worth approximately 9.2 ± 1.5 (± 1 SE) ranks. Similarly, being a private university would be worth approximately 11.3 ± 3.6 ranks. Being a moderately highly regarded university (the 25th percentile for this sample, and a median overall NRC percentile of 30) would confer a "halo" effect of approximately 6.5 ± 2.3 ranks compared to being a slightly below-average university (the 75th percentile in this sample and a median overall NRC percentile of 56). The value of having EEB National Academy of Sciences members on a program's faculty was approximately 3.4 ± 1.4 ranks per member.

Table 3. Results of the stepwise regression of program NRC rating vs. nine program characteristics. This gave a four-variable model that explained 77% of the variation in NRC ratings.

Variable:	Coefficient	Std. Err.	t-value	P	Partial r^2
Intercept	3.33				
Productivity	0.000463	0.000077	5.99	0.0001	0.382
Private/Public	0.284	0.090	3.17	0.0025	0.148
Overall NRC Ratin	-0.00625	0.00220	-2.85	0.0061	0.123
NAS Members	0.0869	0.0346	2.51	0.0148	0.098

Size, productivity, and quality

One of the more debated questions in the NRC assessment literature is the meaning of the frequent positive correlations between program size and reputational ratings. This relationship holds for virtually all fields assessed in the ratings (Goldberger et al. 1995). Some researchers argue that it is unfair that the ratings are generally positively correlated with the number of faculty, while others argue that size is a valid component of quality. We side with those who argue that a program with 60 strong faculty is inherently better than a program with 20 strong faculty, even if the production per faculty is similar for both programs. A program with more strong faculty has a greater opportunity for balance, variety, and specialization. However, the more interesting question is, "What do the NRC reviewers of EEB programs think about the general relationship between program size, productivity, and reputation?"

We addressed this question by performing a multivariate regression between these parameters. If reviewers discount programs that obtain high productivity via high numbers of program faculty and not via high faculty productivity per se, then we would find a positive association between productivity and rating and a negative association between size and rating in this multivariate analysis. Our analysis showed reviewers do tend to take a program's size into consideration; however, this tendency was weak. The coefficient for size was negative, but the improvement in variability explained over a single factor regression with program productivity as the sole predictor variable was only 2.4% (from $r^2 = 0.590$ to $r^2 = 0.614$), and the t test for the program size term was only marginally significant ($P = 0.0561$).

We examined the residuals of this two-factor regression to determine whether programs were overrated or underrated relative to their productivity and size. The six most overrated (i.e., high NRC ratings relative to their productivity and size) programs were all located at prestigious private institutions. However, not all private universities were overrated. The less prestigious private institutions were usually not overrated, and Cornell University, which is both private and one of the most prestigious universities included in this survey, was in fact somewhat underrated.

Unexplained variability

Although few researchers studying the NRC assessment would support absolutist interpretations of the NRC ratings, it is common practice for academics who have not studied them to ask questions like "Why didn't our department crack the top 10? We are just as good or even better than several of the programs rated ahead of us." As discussed previously, there are a number of reasons why one program might be more highly rated than another. The average standard error for the NRC ratings of the top 63 programs was ± 0.23 ratings points. Thus, if this survey were conducted twice using similar pools of reviewers, a program's final mean rating would be expected to vary by approximately ± 9 ranks simply due to luck in the draw of reviewers. Coincidentally, the residual unexplained variability in the multivariate regression analysis was also ± 0.23 (± 1 SD), or ± 9 ranks. Based on these results, it should be clear that the NRC rating may simply be too coarse to justify fine-scale interpretations.

Summary

This analysis of the National Research Council's assessment of reputation at Ecology, Evolution, and Behavior programs provides insights that should aid those attempting to decipher the broader meaning of these ratings. Our measure of total program productivity also provides a new parameter by which those interested in EEB programs can judge their "quality."

Reassuringly, the single best predictor of a program's reputational rating was its research productivity in scholarly journals. This result suggests more detailed analyses of program productivity as provided in this analysis may be warranted when assessing the quality of various research programs across the U.S. Although our assessment of total EEB scholarly productivity was quite time consuming, we assume economics of scale would apply if this were attempted on a broader scale, such as during the next round of NRC reputational assessments.

On the negative side, our study provides strong evidence that the reviewers polled in the NRC survey were biased in favor of private or otherwise highly regarded universities, and especially highly regarded private universities. This result is virtually identical to that obtained in an overall analysis of the factors influencing the NRC reputational ratings across all programs. To quote Toutkoushian et al. (1998), these "results seem to say that faculty form their impressions of 'good' and 'bad' programs based on many factors, some of which are related to underlying quality and others of which clearly are not." The bias in favor of prestigious private universities will presumably have a negative impact on the opportunities afforded EEB programs at less prestigious public universities. It may be more difficult for these programs to compete for the best graduate students, postgraduate researchers, and faculty, as well as for research funds in national competitions like solicitations to the National Science Foundation.

The substantial unexplained variability in our analysis and uncertainty in reviewer assessment of program quality suggests that these ratings are not wholly deterministic, and they should not be interpreted too literally. It is quite likely that the rightful position of a specific program in the hierarchy within a field of research may be 10+ ranks higher or lower than one might expect based on the NRC ratings. This result is consistent with those who have argued that the NRC ratings should only be seen as broad indices that distinguish between the truly outstanding, the average, and the marginal research programs (Mac Lane 1996, Stigler 1996).

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Michael T. Brett
Department of Civil and
Environmental Engineering
Box 352700, 301 More Hall
University of Washington
Seattle, WA 98195-2700
E-mail: mtbrett@uwashington.edu

Liesbeth Brouwer
and Lindzie A. Brett
Department of Environmental
Science and Policy
University of California
Davis, CA 95616

Incentives for Prompt Reviewers

I am sympathetic to Mack's (1999) complaint about tardy reviewers—I have been a frequent victim of them myself, and in my darkest moments have also thought that tardy reviewers should be somehow singled out in the list of reviewers. However, all of the editors I discussed this with echoed Baldwin et al.'s (1999) response: stigmatizing tardy reviewers might make it even harder to find willing reviewers. Most editors know which reviewers tend to be tardy, but in many specialized subdisciplines, the choice is often between a tardy reviewer and no reviewer at all.

I propose that instead of punishing tardy reviewers, ESA develop incentives for prompt reviewers. Rewards for reviews are not unprecedented: for example, the Proceedings of the Royal Society sends reviewers a certificate worth some number of free reprints of the next paper they publish in that journal. A reward that would be both appropriate and valuable to the reviewers, and would not cost ESA any money, would be to provide publication priority to the prompt reviewer's next *n* submissions to an ESA journal. In other words, at each stage of the publication process, a prompt reviewer's manuscript would be moved ahead of earlier submissions that were not by prompt reviewers.

As usual, the devil is in the details. How large should *n* be (probably 1, but possibly more)? Should the priority apply only to first-authored manuscripts, or to any manuscript on which the prompt reviewer is an author. (If the latter, then perhaps the prompt reviewer should be given a choice about which manuscript to apply the priority to?) Should the prompt reviewer's manuscript be moved ahead of all other manuscripts, or merely be advanced a certain number of places in the queue? A problem may also arise where a reviewer dashes off a prompt but contentless review solely to receive the incentive—in which case a minimal standard for review quality may become necessary (but I hope that most of us are not that cynical about reviews). If successful in achieving its goals, the incentive faces a new set of problems: when most submitters are prompt reviewers, then papers by nonreviewers (including most students) will languish forever. This might require a modification such that a given paper can be trumped by a prompt reviewer only a certain number of times, with greater protection for student papers.

Nevertheless, I think that these details could be worked out, perhaps through an adaptive management program. The incentive would have an added bonus: the only way to be a prompt reviewer is to be a reviewer, so more individuals will be willing to review ESA manuscripts and contribute to the publication process.

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Bruce E. Kendall
Donald Bren School for Environmental Science and Management
University of California
Santa Barbara, CA 93106

The Succession of Succession: A Lexical Chronology

The following essay is an expansion of remarks at a Celebratory Symposium, 9–10 April 1999, marking the 100th anniversary of the publication of Henry Chandler Cowles' premier studies of succession on the Indiana dunes of Lake Michigan. The symposium was sponsored by the Field Museum of Chicago, the Indiana Dunes Environmental Learning Center, the Indiana Dunes National Lakeshore, and Chicago Wilderness. It was supported by a major grant through the will of A. Watson Armour.

Recognition of change in nature was long familiar to naturalists, but the term *succession* was coined by H. D. Thoreau to describe the changes in forest trees (Thoreau 1860). Like the word *ecology,* coined in 1866, *succession* lay fallow until they were both resurrected in the late years of the 19th Century (McIntosh 1985). The major concepts of early ecology were community, development, or succession, and stability, or climax, the end of change. Ecologists strove to identify and classify communities in space, and the corollary was to examine change of a community in time, determine what stages it went through, and if, and when, it became stable or climax. Henry Chandler Cowles, one of the first expositors of "dynamic" ecology (Cittadino 1993), ambitiously sought "the laws which govern" the order of succession and produced a physiographic theory of ecology (Cowles 1899). His work was praised in a Festschrift by Sir Arthur Tansley (1935) who wrote: "During the first decade of this century Cowles did far more than anyone else to create and increase our knowledge of succession and deduce its general laws." Cowles attempted to arrange plant societies in order of development, and wrote that they approach the mesophytic forest climax of the Lake Michigan dunes. However, he specified that "Succession is not a straight-line process. Its stages may be slow or rapid, direct or tortuous and often they are retrogressive." He added his famous dictum that succession is "a variable approaching a variable," (Cowles 1901), an attribute that plagues ecologists, managers, and conservationists to the present.

As in any substantial development in science, currently called paradigm change, fin de siecle ecologists adapted their language to their purposes, a process that continues unabated. I have provided a selected lexical chronology, in English, of the concept of succession as it has itself undergone succession in the 20th century (Table 1). The several rows of terms around 1900 were among the standard usages in the earliest publications on succession, and most survive, often with different or multiple meanings. A recent review of stability terminology listed some 176 words or phrases used loosely (Grimm 1996). *Climax* persists to the present in different guises. *Law and theory of succession* are still a holy grail for some ecologists, but others are skeptical about either. The concept of *eutrophication*, the enrichment and aging of lakes, was an early extension of succession into aquatic systems. Some terms, like *disturbance*, have achieved new significance in recent decades.

Table 1. A lexical chronology of selected terms relevant to succession in approximate order of appearance by decades.

1900	Community, development, succession, dynamic, stability, equilibrium, climax, disturbance, pioneer, convergence, retrogressive, progressive, organism, association, secular, zonation, chronosequence, law, theory, eutrophication
1910	Primary, secondary, biome, seasonal succession, xerarch, hydrarch, mosaic, patch, nudation, migration, ecesis, reaction, coaction (competition), individualistic, colonization
1920	Biogeochemistry, autogenic, allogenic, gap dynamics, holism
1930	Ecosystem
1940	Energy, trophic-dynamic
1950	Continuum, gradient analysis, assemblage, assembly, initial-relay floristics, holistic, thermodynamics, systems, information, cybernetics, longitudinal succession, fugitive
1960	*r* and *K* selection, computer models, perturbation, keystone species, strategy
1970	Transition probability, gap models, turnover rates, transient, nutrient retention, assembly rule, resilience, resistance, facilitation, tolerance, inhibition, recovery, intermediate disturbance, sylvigenesis
1980	River continuum, resource-ratio, ascendancy
1990	Ecological law of thermodynamics (ELT), complex ecology, complex adaptive systems
2000	Nirvana

Cowles' undoubted contributions to succession were overshadowed by the somewhat dogmatic theory of his contemporary, Frederic Clements, who described laws of succession dictating an orderly, predictable, progressive, convergent development, to a self-perpetuating climax having the properties of an organism, or even a superorganism. Clements' writings on succession (Clements 1904, 1905, 1916, 1928, 1938) were more prolific than Cowles' and dominated American ecology textbooks until the 1950s (Egler 1951). Clements, in his 1916 magnum opus on succession, added to the lexicon of succession, the words *primary, secondary,* and *biome* and outlined a suite of processes as the basis of secondary succession.

1) *Nudation*—the reduction of biomass by disturbance

2) *Migration*—arrival of organisms on a site

3) *Ecesis*—establishment of organisms

4) *Coaction*—interaction among organisms (subsequently largely confined to competition)

5) *Reaction*—modification of a site by organisms

6) *Stabilization or climax* on a large, regional basis

These are surprisingly comprehensive and have been resurrected by ecologists in recent years. Clements (Pound and Clements 1898) wisely urged ecologists to "not indulge in more neologies than imperative," but subsequently indulged in some 28 neologies in reference to climax alone. Unfortunately, ecologists, and others, have ignored his good advice (Wali 1999). Clements' theories came to be described as the "classical succession paradigm," which permeated conservation and management thinking by perpetuating the age-old tradition of balance of nature as the essence of preservation. Only in recent decades have ecologists, conservationists, and land managers come to realize that you can't lock up a site and preserve it. It is essential to deal with disturbance and succession and follow the variable approaching a variable, rather than look for the stable ideal climax or balance of nature. In recent years, Clements' (1916) magnum opus on succession is cited three to four times as often as Cowles' (1899) masterpiece (Table 2). Ironically, the high frequency of citation of Clements is probably because his, now much maligned, theories are cited in order to be berated by later students of succession.

Clements produced more books, but Cowles produced more students, among them the cream of the second generation of ecologists. Several of these wrote on succession, notably W. S. Cooper (1913), who, along with A. S. Watt (1924), in Britain, anticipated the ideas of gap and mosaic, later to counter the traditions of homogeneity, climax, and stability that permeated ecology. Cooper's version of the forests of Isle Royale was a "*mosaic,*" "*patchwork,*" even a "*kaleidoscope*" of different ages. He wrote, "The forest as a whole remains the same, the changes in various parts balancing the other."

Plant ecologists dominated the early study of succession, but animal ecologists, like Victor Shelford, wrote on succession of beetles (1907) and fish (1911), and C. C. Adams (1908) wrote on succession of birds. Adams quoted J. S. Mill: "Of all truths relating to phenomena, the most valuable to us are those which relate to their order of succession," which placed a heavy burden on truth-seeking ecologists.

Studies in desert, alpine, arctic, and diverse aquatic habitats created new problems and new terms for succession. Limnologists discovered seasonal succession of plankton opening up a new time and space scale. Hydrarch succession of wet habitats and zerarch succession of dry sites posed different problems. Techniques changed. Cowles deemed quantitative measurement unhelpful. Cooper, Watt, and some of their contemporaries used quantitative methods extensively, and numerical measures of species composition became the essence of successional studies. Woodruffe (1912) produced an early microcosm study of the sequence of protozoan fauna.

H. A. Gleason's 1917 and 1926 versions of his individualistic concept of community and succession (1927) were ignored or even denied. He wrote, "succession is an extraordinary mobile phenomenon, whose processes are not to be stated as fixed laws ... and whose results need not and frequently do not ensue in any definitely predictable way," flying in the face of conventional theory. Biogeochemistry appeared, unheralded, in the mid-1920s (Gorham 1991), but would later come into its own with the rise of ecosystems ecology. Holism was coined by General J. C.

Table 2. Citations of Clements and Cowles in the Science Citation Index, 1994–1997.

	Science Citation Index			
Reference	1994	1995	1996	1997
Clements				
1916	25	24	21	25
Total	55	51	60	55
Cowles				
1899	6	4	5	8
1901		2	2	
Total	8	6	9	8

Notes: Clements 1916 refers to Clements, F. E. 1916. Plant succession: an analysis of the development of vegetation. Carnegie Institution of Washington Publication Number **242.** Cowles 1899 refers to Cowles, H. C. 1899. The ecological relations of vegetation on the sand dunes of Lake Michigan. Botanical Gazette **27:** 95–117, 167–202, 281–308, 361–391. Cowles 1901 refers to Cowles, H. C. 1901.The physiographic ecology of Chicago and vicinity. Botanical Gazette **31:** 73–108.

Smuts (1926), who was influenced by Clements, just in time for the introduction of *ecosystem* by A. G. Tansley (1935). Tansley (1920) had contributed the words *allogenic* and *autogenic* to discourse on succession, but the ecosystem concept was to change the face of ecological science and *holism* became its watchword. *Ecosystem* and *succession* were first and second, respectively, in a survey of 230 concepts in ecology produced for the 75th anniversary of the British Ecological Society in 1985 (Cherrett 1989), testimony to their continuing significance for ecologists.

Perhaps lost in its revolutionary analysis of the trophic–dynamic aspect of ecology is the fact that Raymond Lindeman's (1942) opus was a study of lake succession. Lindeman wrote, "Quantitative productivity data provide a basis for enunciating certain trophic principles, and shed new light on the dynamics of ecological succession." Key to his idea was the "energy availing" aspect of succession: productivity and efficiency increase in the early stages of lake succession, decline with lake senescence, but rise again in the terrestrial stage. Gutierrez and Fey (1980) wrote, "with Lindeman's work the articulation of a general self-contained hypothesis for ecological succession was essentially complete," a very optimistic assessment.

A. S. Watt (1947), in *Pattern and Process in the Plant Community*, brought to the fore his and Cooper's ideas of gap and mosaic formed by the death of a dominant tree. According to Watt, the repeated gaps formed a definite proportion and a "phasic equilibrium." Watt's pattern–process concept of community and concept of phasic-equilibrium was described as a "paradigm shift" to "hierarchical patch dynamics" (Wu and Loucks 1995), which, they said, would overcome old constraints in ecology. Watt, prophetically, quoted T. S. Eliot, "We must know all of it in order to know any of it." The search for "all of it" in recent holistic studies of succession persists in spite of pessimistic comments by many ecologists frustrated by the complexity of succession and the lack of unifying ecological theory.

Another paradigm change in the 1950s was the resurrection of H. A. Gleason's long-submerged individualistic hypothesis, in the gradient analysis of Robert Whittaker, and the continuum of John Curtis (Barbour 1996). Whittaker demolished Clements' monoclimax theory and arrived at a climax mosaic theory based on continuous gradient change. Whittaker (1953) wrote: "Succession may thus be thought to occur not as a series of distinct steps, but as a highly variable and irregular change of populations through time, lacking orderliness or conformity in detail though marked by certain fairly uniform overall tendencies." A myriad of studies of succession had complicated general understanding. Frank Egler (1954) provided two themes, *initial floristics,* in which succession proceeded from sorting an original set of propagules on a site, and *relay floristics,* in which groups of species appeared sequentially, the earlier ones preparing the way for the later ones, essentially a Clementsian view. Stream ecologists introduced longitudinal succession, occurring in a spatial sequence downstream and complicating the usual concept of succession on a given site.

A fitting successor to Cowles' pioneer work appeared 60 years later in a study of the Indiana Dunes area by a third-generation, University of Chicago ecologist, Jerry Olson. Olson (1958) seized on new techniques and observations based on radiocarbon dating and biogeochemistry to age the dunes and to analyze soil development, and rates of accumulation of organic carbon and nitrogen as well as physical properties. Like Cowles, he described alternative pathways, but, unlike Cowles, he suggested that succession to mesophytic forest might be inhibited by decreasing nutritional conditions of the soil on older dunes. Olson gave quantitative meaning to Cowles' "variable approaching a variable."

A major development in the 1950s was the emphasis on ecosystems stimulated by the first (1953) edition of Gene Odum's textbook. Following hard on this was the reintroduction of thermodynamics into ecology in 1955 by H. T. Odum and R. C. Pinkerton, harking back to A. J. Lotka's law of maximum energy. These marked the entrance into ecology of systems theory, which took institutional form in the International Biological Programme in the 1960s (McIntosh 1985). Systems theory was described by Robert Rosen (1972). "The developing family of ideas and concepts which fall roughly under the rubric of systems theory amounts to a profound revolution in science—a revolution which will transform human thought as deeply as did the earlier ones of Galileo and Newton." This made it a hard act to follow. Ramon Margalef (1963, 1968) reinterpreted succession in the late 1950s and 1960s in terms of information theory and the energetics of ecosystems. Ecosystem maturity was to be determined by the average number of bits per individual, and maximization of energy flow became a measure of succession and climax. E. P. Odum (1968) wrote, "Ecoenergetics is the core of ecosystem analysis."

The 1960s were a watershed in ecology, marked by much attention to theoretical, mathematical, and biogeochemical ecology. The MacArthur school produced r and K selection that attributed r properties, such as high reproductive and dispersal rates and tolerance of severe conditions, to early successional species, and K properties, such as slower reproduction, long life, and greater control of environment, to species characteristic of late stages of succession. The *keystone species* concept was introduced by Robert Paine (1969) in studies of succession in the marine littoral zone to describe species whose removal or addition had maximal influence on a community. The 1950s had produced a new tool for studying biogeochemical cycles—isotopic tracers—and studies of the interaction of organisms and the inorganic environment in ecosystem production and succession flourished in the 1960s. Part of this watershed was Gene Odum's

(1969) Strategy of Ecosystem Development, in which he offered "twenty-four trends to be expected in development of ecosystems." Odum's trends incorporated energetics and nutrient cycling along with biological traits seen as changing with succession. These have been frequently questioned by some ecologists, but widely approved by others of the developing thermodynamic school of ecology. Christensen and Peet (1981) wrote, "Indeed, Odum's (1969) strategy of ecosystem development presupposed most of Clements' doctrines." Many plant ecologists focused attention on population and species criteria. Pickett et al. (1987) asserted that "succession is fundamentally a process of individual replacement and a change in performance of individuals. Systems ecologists emphasized energy or materials changes. Robert O'Neill (1976) wrote: "The identity of the system remains through successional changes in species....There is no reason to believe that explanations of ecosystem phenomena are to be found by examining populations." The defining term for systems ecology was "holistic."

Beginning in the 1960s, and exploding in the 1970s, mathematical modeling provided ecologists with a bewildering array of ways in which to consider succession. Shugart and O'Neill (1979) wrote, "The single most reliable field mark of systems ecologists ... would be the use of mathematical models as a tool." An additional diagnostic characteristic, they said, "is the zeal for building a theoretical science that is the real motivation." Models and zeal for theory permeate the ecological literature from the 1970s on. One class of models considered transfers of large areas of defined landscape types and simulated the change in extent of these over time. Stand models were constructed to simulate succession in relatively small areas, and used life history attributes of species to simulate establishment, growth, and survival of species in relation to environmental attributes such as light, moisture, and nutrients. Shugart and West (1981) wrote that before 1970 there was only a handful of forest models, but by 1981 there were over 100. The granddaddy forest model, based on Appalachian hardwood forest, was named JABOWA (Botkin et al. 1972). It was followed by numerous offspring based on other forest types with similar acronyms, even to ZELIG, but did not stop there. One model had a recognizable name, LOKI. LOKI, perhaps appropriately, is the name of a Scandinavian god of evil, a counterpart of Satan and an enemy of good gods. Such models have developed the gap concept of Cooper and Watt, are specific in applicability, and have been useful in predicting succession in given forest types and the effects of environmental change as a guide to management. Yet another class of models, predicated on the assumption that succession was adequately described by a statistical process, the Markov Chain, was advanced by Horn (1975). Some raised questions about the validity of this approach (Usher 1979), and even Horn (1976), after several studies of succession, commented, "The only sweeping generalization that can safely be made about succession is that it shows a bewildering variety of patterns."

The International Biological Programme of the 1960–1970s advanced the concepts of systems science in ecology and produced large models of what some called "transient behavior." Gutierrez and Fey (1975) developed a grassland model of 17 integral and 66 algebraic equations that, its authors claimed, "provides a precisely formulated hypothesis of secondary succession ... that appears to be consistent with information currently available on ecological succession." Very large models of ecosystems, such as the Elm model of the grassland biome, were generally unsuccessful. Some commentators on systems models were unkind. May (1973) wrote that some systems models "could benefit most from the installation of an on-line incinerator."

Although some successional models had considerable success, Van Hulst (1992) wrote, "It is important to remember that modeling of vegetation dynamics is still an immature discipline ... one may hope that some progress can be achieved in succession modeling once mathematical models of sufficient sophistication are being used."

Much traditional succession study focused on plants, especially forests, but studies of animals, particularly in aquatic settings, flourished in the 1960s and after. Notable in this context were studies of aquatic systems stressing the importance of herbivores and predators. Some argued that the effects of consumers, called *cascades,* were critical to the trajectory and the end point of succession. This generated a widely discussed analysis (Connell and Slatyer 1977) that provided three mechanisms for succession: *facilitation* (preparation of a site by early species for the advantage of others), *inhibition* (early species restrict the entry of later species), and *tolerance* (short-lived species are replaced by long-lived species), but none fitted all successions. Succession in streams posed new problems. Stream ecologists had introduced *longitudinal succession* and later the *river continuum* and *nutrient spiraling,* which stress the flow of materials downstream. The longitudinal sequence in space seems contradictory to the classical in situ view, and one stream ecologist (Fisher 1983) suggested "redefinition of succession is thus in order"—a difficult order. Succession of wetlands of all types added some special problems. Walker (1970) examined British wetland succession and said that any stage could go to any other stage, although some sequences were more frequent. This was a very different view of conventional succession and fit no theory.

In the wake of the widespread questioning, if not the demise, of Clements' organismic theories of succession and extended studies of succession of organisms and ecosystem attributes, much progress had been made, but many ecologists were pessimistic about prospects for unifying theory. Robert Whittaker and Simon Levin (1977) wrote, "The failure of unifying statements on succession may be not only historical but predictive."

Table 3. Frequency of occurrence of selected terms in titles or text of four ecological journals stored electronically in JSTOR.

Term	1956–1965	1966–1975	1976–1985	1986–1995
Assemblage				
Ecological Monographs	16	27	54	55
Ecology	53	90	190	>200
Annual Review of Ecology and Systematics	0	19	35	27
Ecological Applications	0	0	0	30
	69	136	279	>312
Assembly				
Ecological Monographs	1	5	18	14
Ecology	21	30	53	108
Annual Review of Ecology and Systematics	0	2	7	15
Ecological Applications	0	0	0	11
	22	37	78	148
Disturbance				
Ecological Monographs	68	70	105	98
Ecology	184	>200	>200	>200
Annual Review of Ecology and Systematics	0	17	43	40
Ecological Applications	0	0	0	126
	252	>287	>348	>464
Perturbation				
Ecological Monographs	0	9	31	18
Ecology	3	22	108	126
Annual Review of Ecology and Systematics	0	14	21	22
Ecological Applications	0	0	0	29
	3	45	160	195

John Miles (1979) saw "no all embracing hypothesis for succession" and wrote, "I doubt if this will ever be possible." Norman Christensen (1988) said that the demise of classical Clementsian theory "has led to an uncertainty and even cynicism as to whether a comprehensive theory of community change is feasible." Others were more hopeful. Tilman (1985) provided the resource-ratio hypothesis that he described as "an alternative, simple theory of succession." Reiners (1986) lamented the absence of ecosystem theory but saw prospects in energetics, as introduced by Lindeman, and stoichiometry.

Not to worry! Hope springs eternal in some ecologists' breasts. One thing to do was change the ecologists' lexicon. If you can't solve *community, disturbance, succession,* and *climax,* change them to *assemblage, assembly, perturbation,* and *ascendancy.* Community was difficult to define, and assemblage was mostly inserted as a synonym without definition, although Charles Elton (1927) had long ago said "a community is not a mere assemblage." These terms have appeared with increasing frequency in recent decades, but it is not clear that these neologies have aided ecology any more than Clements' neologies (Table 3).

Perturbation was, I suspect, introduced as perturbation analysis to assuage "physics envy" (Cohen 1992), and included addition or removal of a species or a change in nutrient supply. It has not replaced disturbance, as Vogl (1980) suggested, but now appears as a synonym; an old-fashioned disturbance, such as fire or windthrow, becomes a perturbation.

Assembly is a word that implies something being assembled, and it took on new significance when Jared Diamond (1975) coined the phrase "assembly rule" in an article "The Assembly of Species Communities." Diamond's rules were much debated, even ridiculed as tautologies (Connor and Simberloff 1979), but assembly, assembly rules, and even meta-rules proliferated. Berryman (1989) wrote that the viewpoint of general systems was that "ecological dynamics are governed by universal rules of change." Drake (1990) wrote that when "succession is considered in terms of assembly dynamics the factors responsible for community organization emerge," and allowed that assembly and succession have similar processes. In addition to assembly dynamics, the ecological lexicon is embellished with assembly grammar, assembly algorithm, assembly routes, assembly trajectories, assembly space, and assembly steps, all presumably according to assembly rules. Grover (1994) wrote that "it may be feared that community ecology will be plagued by unpredictability and inexplicability." The good news, he said, is that such fears can be countered by understanding assembly rules, offering the caveat that to study the assembly process "we need a mathematical breakthrough." Tanner et al. (1994) referred to "dynamics of assemblages that are recovering from a recent disturbance," which reminds one of secondary succession. Grossman et al. (1998), in an article on "Assemblage Organization in Streams," listed "community organization" in the key words, but not assemblage.

Confidence in the merits of assembly rules fluctuates. Wilson (1994) asserted that without assembly rules "vegetation science is reduced to stamp collecting," but Wilson et al.

(1998) lamented," it is difficult to see simple assembly rules in plant communities." Assembly has connotations of mechanics in an assembly line wherein prefabricated parts are fitted into a designed body, which produce what may be called ensemble constructs. The question is if the ensemble is a Rolls Royce or a Yugo.

The 1980s and 1990s, which have been called the Age of Ecology, produced what some regarded as a rejuvenated ecology under the rubric "Complex Ecology." Complex ecology came as no surprise to most ecologists. The pioneer limnologists, E. A. Birge and the aptly named Chancey Juday (1911), had wondered if the complexities of limnology would elude the rational man. Stanley Cain had, in the 1950s, commented to the effect that ecology may not only be more complex than we think, it may be more complex than we can think, and many ecologists expressed frustration at the complexity of ecological systems. Nevertheless, the renewed perception of complex ecology brought forth new approaches, terminology, and institutions to deal with its complexity. A volume, *Complex Ecology* (Patten and Jorgenson 1995), was described by its editors as "a science that does not shy away from some of the most obvious facts that are well recognized in scientific holism." Ulanowicz (1997*a*) wrote "after a decade of relative quiescence the field of ecology is again becoming an arena for lively debate, as new and sometimes radical concepts appear and old, cherished ideas are vigorously challenged." This assessment of recent ecology might surprise many ecologists who survived the 1970s and 1980s. Several of the purportedly new or radical concepts pertain to succession and have as a common denominator Gene Odum's familiar "trends to be expected," which described ecosystem attributes closely related to succession, although numerous ecologists had questioned the generality of the "trends" (Vitousek and Reiners 1975, Sousa 1979, MacMahon 1980, Peet and Christensen 1980, Christensen and Peet 1981, Peet 1992).

Gutierrez and Fey (1980) started from the "trends" and the "contemporary hypothesis" of succession as a process of development that is reasonably directional, predictable, community controlled, and provided an "endogenic theory of secondary succession." Ulanowicz (1980) introduced the hypothesis of "ascendancy," which he expanded in later volumes (1986, 1997*b*). Ulanowicz's (1997*b*) ascendancy originates in Odum's "trends," each "a separate manifestation of increasing mutual information in trophic networks ... showing parallel increases in ascendancy." Ascendancy is measured as the product of total system throughput ($\mathrm{mg} \cdot \mathrm{m}^{-2} \cdot \mathrm{yr}^{-1}$ of C) and average mutual information (Shannon-Wiener H'). Ascendancy has a somewhat private lexicon not familiar to most ecologists. Although Ulanowicz denies that ascendancy smacks of Clements' superorganism, his discourse is redolent of Clementsian ideas. In any event, he asserts "But the issue, bearing on nature of superorganismal organization ranks today as one of the most philosophically intriguing in all science" (Ulanowicz 1997*b*). Ulanowicz has high hopes for ecology. Ecology—"The long-neglected stepchild is about to take center stage. Ecology is on its way to becoming the ascendant perspective of the next century." To which ecologists can only respond—Right on! Which ecology?

Jorgenson (1997) also adopts Odum's trends. He propounds an ecological law of thermodynamics (ELT) which is "a thermodynamic translation of Darwin's theory, an evolving superorganism." Jorgenson's law of ecosystem theory also serves as a "tentative" Fourth Law of thermodynamics.

Ecology has recently been introduced to the Institute for the Study of Complex Systems in Santa Fe, New Mexico, and ecosystems are recognized as "Complex Adaptive Systems" (CAS; Brown 1994), which, unsurprisingly, has multiple meanings. Brown (1995) writes, "Recently it has become increasingly apparent that the activities of organisms play major roles in the structure and function of ecosystems." This unsurprising observation is also implied in a volume, *Linking Species and Ecosystems* (Jones and Lawton 1995), which makes one wonder how they ever got disassembled.

A more appropriate successor to the dune succession studies of Cowles and Olson is one on Lake Michigan dunes at the Straits of Mackinac, 300 miles north of their sites, and published 99 years after Cowles' and 40 years after Olson's studies (Lichter 1998). This area, less disturbed than the Indiana dunes, provided a series of 72 neatly parallel dunes formed over 2375 years, in a clearly dated sequence, plus some older dunes. It is a tribute to Cowles and Olson that the succession on younger dunes is similar to that they described on the Indiana dunes. Lichter described clear patterns of species turnover, successional changes in species diversity, aboveground biomass and litter production, net ecosystem production, nutrient pools, and cycling. He noted a number of constraints in developing forest such as light, cationic nutrients, cool soil temperatures, and thick litter layers. In the all-too-familiar assessment of succession, Lichter summarized, "These numerous potential environmental constraints suggest a considerable complexity in this ostensibly simple ecosystem." In the immortal words of Yogi Berra, "It's deja vu all over again." Nevertheless, the celebration of complexity and the eternal hope of further breakthroughs in mathematics maintain the eternal hope of a unifying theory of succession, and ecology at large, and we may hope for NIRVANA in the 21st Century.

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Robert P. McIntosh
Department of Biological Sciences
University of Notre Dame
Notre Dame, IN 46556
E-mail: ammidnat.1@nd.edu

How to Manage Data Badly (Part 1)

In a landmark article in *The American Statistician*, Howard Wainer (1984) presented ideas for "How to Display Data Badly," wherein good data are ruined by bad graphics. Wainer presumed too much. In this essay, I extend his concept by presenting ideas and examples of how scientific data can be managed badly so that they never even make it to the graphics stage. Modern database management software, continually improving hardware and networks, and many sound recommendations for managing ecological data (e.g., ESA 1995, NRC 1995) are making it increasingly difficult to manage data badly. It can still be done, however, and judging by various so-called "horror stories" one hears about adventures with ecological and other scientific databases, is done frequently. Those people still having trouble mismanaging data, whether they are database managers, administrators, or scientists, will find the following techniques helpful.

Techniques for database managers

Rule 1. "One world, one database"

Two time-honored techniques used by database managers are: (1) make it hard to get data into the system, and (2) make it hard to get data out of the system. Although experts often use both techniques, novices may wish to start with just one. A data system that is far more complex than is necessary will usually do the trick. In *Principles of Data-Base Management,* James Martin (1976) suggested that one reason for failure of long-term databases is "Plans for the installation of a grandiose all-embracing system." This strategy is effective because it makes development time long, data loading slow, and data queries difficult to formulate. Discouraged data collectors and users will seek solutions elsewhere. In one case, a national marine water quality database used such stringent quality assurance procedures that data collectors were loath to enter their data. Consequently, the users learned a lot about data quality but very little about water quality. Another organization thought that merging their scientific data systems with their administrative data systems would be easy "because they both use Oracle software."

Rule 2. Users are losers

Practitioners of the mystical cult of database management do not need meddlesome ideas from potential users of a data system, such as those gathered during those boring system requirements exercises. Do not compromise your design or processing efficiency by consideration of the system's usefulness to scientists. If scientists insist on contributing to the design, invite them to an Information Management Needs and Requirements Workshop (the name alone is scary), where you can use tips from Zave and Jackson (1997) on four dark corners of data system requirements engineering to stymie input.

Rule 3. What's good for General Motors is good for science

Design data systems for research projects the same way you would for a bank or an insurance company. The paths of scientific inquiry are as fixed as the steps followed in manufacturing and selling cars. It does not matter if the system must process thousands of transactions per day or one batch load per month. After all, a byte is a byte is a byte, no matter where it is found. Commercial software for database management systems (DBMS) is driven by the commercial market. More than one research group has found that their database designer was unaware of the differences between business and scientific databases (Pfaltz 1990), and that the design recommended was not suitable for their less structured data, less formal organization, and less predictable user needs.

Rule 4. Reinvent the wheel

Another powerful technique is to resist the efforts of unimaginative people who promote standards for formatting and exchanging data. Surely you can think of more interesting codes for species than the Integrated Taxonomic Information System and more clever codes for chemicals than the Chemical Abstracts Service. You can always develop better software than is available off the shelf. Moreover, it is unlikely that any previous data system built by others will be of any value to you. After all, if we did not reinvent wheels, they would still be made of stone. One organization, in a burst of creativity, let each of its branches come up with their own coding system for fish names, thereby making more work when they later wanted to search and merge species catch data.

Rule 5. Data governance: totalitarian or anarchist

People who collect data cannot be trusted to manage them. Seize control of all data and get them into a centralized system. This allows data sources to disavow all ownership and responsibility, and therefore not bother with subsequent corrections and updates. To avoid bias, metadata (information about data) should be written by people not familiar with the scientific discipline. This can provide much needed comic relief, as when software engineers interpreted "pH" as a code for telephone and then wondered why the value had only two digits separated by a decimal point. This same group was so keen to integrate data that they insisted that one group studying lakes and another studying estuaries use identical formats for pH. That made it easy to calculate the mean pH for the nation's waters, should anyone ever want to know the answer (e.g., Country—USA; Area—9.36 x 10^6 km^2; pH—6.9).

When adopting data and metadata standards, avoid the middle road. You can get along with scarcely any standards (an absence of any data policies common to all groups can let data sources express themselves freely and preserve our rich data diversity) or, conversely, lay them on thick. You can require metadata with formats so onerous they will be ignored (admirable, but common). On the other hand, you can do what one group did and simply include a FAQ (Frequently Asked Questions) file with the data. (Usually FAQs have not been, but the authors probably liked the answers anyway.)

An infallible way to frustrate data users is to let them choose from multiple, mutually inconsistent versions of the same data set. For example, Schmidt (1998) had to invest considerable detective work to get a consistent data set from published and underground versions of data from the Geochemical Ocean Sections program.

Although engineers keep developing new algorithms for detecting and rejecting bad data (Zhang et al. 1992, Baldick et al. 1997), blindly relying on computer programs to validate data can lead to trouble. In one well-known misapplication of computerized range checks, NASA computers programmed to delete concentrations of ozone below a certain value to eliminate "noise" failed to detect the ozone hole over Antarctica (Edwards 1998).

Rule 6. Silicon is thicker than DNA

Communicating with computers is easier than with humans. Despite complaints about the writing of computer people, a few phrases of computer jargon and acronyms can express a thought that would take plain English many sentences to explain. And compared with computer languages, English is a frail, illogical language that is full of conflicting and inconsistent rules. Much of the poor communication between computer people and normal people results from use of this imprecise language. English even has the peculiarity that if enough people violate a rule of grammar for enough time, the rule changes to meet the practice! (Try that method with syntax errors in a computer language.) Machine language, the ultimate in clarity and conciseness, has no tolerance for solecisms.

Data modeling techniques, which are used to design databases, can provide fertile ground for confusing scientists and administrators. Entityrelationship diagrams can be translated into computer files far more easily than into scientists' and administrators' heads. Hay (1998) points out that database managers can proudly take most of the credit for the bad reputation of data modeling; too often, the data modeling software receives all the glory.

Rule 7. Sell! Sell! Sell!

Database managers must promote their systems vigorously. Whenever anyone asks for a certain feature or has a data set they would like to add—no matter how irrelevant or unsuitable for the system—you should promise to add it. Successful database managers, anxious to please anyone with a glimmer of interest, will never say no to a single bit of data and will try to make their data systems be all things to all people. An associated rule, so commonly used that it hardly needs to be stated, is "Always underestimate the time needed to bring a data system online."

Rule 8. Mapping administrators

If getting scientists to manage data has been likened to herding cats, then getting administrators to pay attention to data management is like herding lemmings—they are going to plunge off some cliff no matter what you suggest. At least a cat can sometimes be lured with a bowl of milk. When working with administrators, remember that their one weakness is an unnatural fondness for Geographic Information System (GIS) maps. Axiom: Never show an administrator a table of data; always use a GIS map instead. Even data collected in a single laboratory experiment can be plotted on a state map with an arrow pointing to the location of the lab building. In his seminal book *How to Lie with Maps*, Monmonier (1991) shows some of the clever things that can be done with maps. Dreadful data can often be swept under the rug of a colorful map.

Techniques for administrators

Rule 9. Talk the talk

With everyone from the President on down talking about a national information infrastructure, be sure to join the national fervor for getting data flying all over the place. But be careful not to get involved in the painful task of creating an effective data management system in your organization. One of the biggest disadvantages of using database management software packages is that they burden the project with the need to get organized, to make decisions, to coordinate, and to be consistent. Take special care to avoid CASE (Computer-Aided Software Engineering) tools used for designing databases; these are particularly insidious in demanding feedback from administrators about organizational procedures. Data managers may be slow to recognize your perspicuity, but you will know what you want for a data system when you see it. Another good use of "messy" organizational problems is that they can be as effective as technical ones in inhibiting data sharing (Evans and Ferreira 1995).

The considerable work needed to create a sound scientific database, much of it done during the early stages, is worthwhile only when there is long-term intent to maintain the database. Improvident administrators can take advantage of this weakness in the system life cycle by limiting themselves to short-term commitments. Quint (1998) described the mounting death toll of databases. Even if the data system succeeds, the costs of long-term maintenance are often not included in project budgets (ESA 1995, Farrey et al. 1999).

Rule 10. Do less with less

Clearly, one of the best ways to create poor data sets is to underfund data management, and, in fact, this technique is commonly used. If every study of research projects ever done since the invention of computers has recommended that 10–20% of the project budget be spent on managing the data, why not impress your budget people by allotting only 5% in your study? This will put the data system on a death march (Yourdon 1997). Often, administrators can apparently achieve a data management system solely with hardware and software, thereby overcoming the superstition that qualified people are a crucial commodity. National data centers have been called "data cemeteries" because of inadequate funding to handle the flood of incoming data (French 1990).

Frequently, administrators can indulge in one of their favorite pastimes (technology transfer), where the objective is to leave sophisticated data systems and equipment with groups that do not have the trained personnel or budget to operate them. One of the nicest GIS applications ever developed for managing natural resources is gathering mold in a tropical jungle because the donors forgot to add people to operate the system.

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Note: Part 2 of this essay, to appear in the next issue, covers techniques for scientists who do not wish to fall behind database managers and administrators in managing data badly. Then it shows the synergy that can result from all three groups working together to mangle data. An epilogue confesses that this article was written in an ironic tone, and provides a few simple suggestions on how to manage data well.

Stephen S. Hale
Atlantic Ecology Division
U.S. Environmental Protection Agency
27 Tarzwell Drive
Narragansett, RI 02882