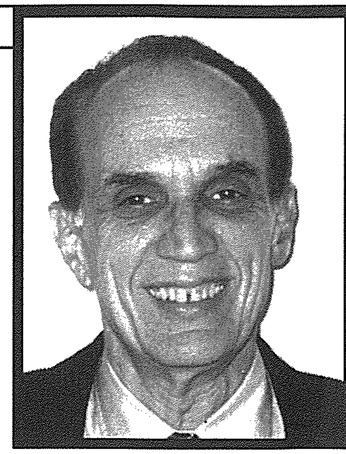
Statistics as a Unified Discipline



Bradley Efron

Many surprises await the neophyte ASA President-Elect. Most surprising for me was a call from the Alexandria headquarters asking what theme I had chosen for the 2004 Joint Statistical Meetings in Toronto. Now, I had always assumed that these themes (San Francisco's "Statistics—A Bridge to Discovery and Knowledge," for instance) were selected by some large and responsible deliberative body. No, it was my choice alone, and moreover it would also serve as the de facto theme for my entire year as president. Quickly rejecting "Statsters Inc." as frivolous, and "A Full Century of Steady Employment" as uninspiring, I spent a couple of days thinking about what ideal I would like to convey, in six words or less, to the thousands of Toronto-bound statisticians from the ASA and our sister organizations.

"Statistics as a Unified Discipline" was my choice, and when I thought about it, it summarized the reasons I had wanted to be president in the first place. The first thing to say in its favor is that it is broadly true: statistics *is* a unified discipline, in the important sense that all statisticians receive fundamentally the same type of advanced training, and bring a common set of tools and a common scientific sensibility to their daily workplace. I'm really interested in only a small fraction of the JSM talks among the thousands offered, but I could attend most of them with being totally mystified; most of us could do so.

Unification is neither a small fact nor an unimportant one. Many of the most celebrated intellectual disciplines are not unified these days. Physics, for example, is sharply split between experimenters and theoreticians, and split further among string theorists and those whose theory connects more immediately to the real world. Moreover, statistics, among all disciplines, might be considered as a ripe target for fragmentation. We are an information science, the first and most successful information science, and as such we don't have our subject matter predefined by nature, as with rocks for geologists, money for economists, stars for astronomers, and so on.

In fact, unification did not come easily to statistics. An early Bayesian consensus, following the flag of Laplace, fell apart in the 19th century. Statistics in the 1900s was, in the main, a collection of ad hoc methods applied separately to different client fields, rather in the spirit of contemporary computer science. Heroic work by the prewar giants—Pearson, Fisher, Neyman, Wald, and a few others—codified the methodology into a recognizable mathematical discipline. The weapons we confidently wield today in the war against random noise, maximum likelihood, sufficiency, efficiency, ANOVA, optimal testing, and discriminant analysis were forged in the first half of the 20th century and founded so firmly that we know they cannot be improved upon, at least not within the considerable range of problems they attack.

The second half of the 20th century saw the subjectivist Bayesians, led by Savage and De Finetti, mount a serious challenge to the Fisherian-frequentist school. I think it is fair to say that subjectivism broadened the base of statistical thinking without fragmenting it. Its offspring, the more objectivist Bayesian school that centers algorithmically around MCMC and Gibbs sampling, has inched closer to frequentism as it more fully engages genuine statistical applications. A neo-Bayesian data analysis, using the power of modern computation to execute its formidable methodology, looks a lot like the cross-validations, bootstraps, smoothing splines, p values, etc., of its frequentist counterpart. Again, I would say that the statistical viewpoint has been broadened rather than split.

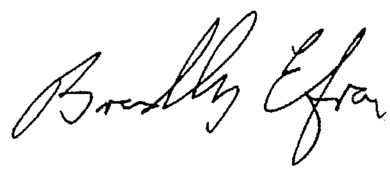
Why should a university have a statistics department? Why should a pharmaceutical company have a statistics group? Why should a medical school have a division in biostatistics? The first answer has to be that there is a unified viewpoint called "statistics" that has provided, and continues to generate, useful answers to an important set of scientific problems. The unification of statistics as an intellectual discipline had an enormous impact on the 20th century scientific enterprise. Statistics has become the primary mode of quantitative thinking in literally dozens of fields, from economics to biomedical research. The statistical tide continues to roll in, now lapping at the previously unreachable shores of the hard sciences. This September I attended "Phystat2003" at the Stanford Linear Accelerator Center, a joint conference for physicists and statisticians. Phystat2003 was at least partially inspired by the success of Penn State's "Astrostat" conference series. Yes, confidence intervals apply as well to neutrino masses as to disease rates, and raise the same interpretive questions, too.

If we could take the unity of our field as a given then there wouldn't be much purpose in using it as a theme for the Toronto meetings, but we can't take it for granted. Statistics is a discipline with an enormous frontier—all those client fields that depend on statistical methodology—compared to a relatively small central core of work on the development of general ideas. It's easy to imagine a world without joint statistical meetings, with only separate conferences for biostatisticians, geostatisticians, mathematical statisticians, Bayesians, public policy theorists, and other subject-related specialties. Of course, these groups do, and should, hold their own separate meetings, but it's important and valuable that we can still talk to each other, and even enjoy meeting together at least once a year.

Not all statistical ideas come from within our own community. Data-rich scientific environments often generate their own statistical methodology. Right now we can see this happening in the machine-learning community, which has aggressively pursued a host of automatic model-building algorithms such as neural networks. To say we are unified doesn't mean we are a union shop. An important component of our meetings is the recognition of new statistical problems and methodologies, and their incorporation into our world. With neural nets in particular it has been heartening to see a mutual process of understanding growing between the originators and classical statistics; witness *The Elements of Statistical Learning*, by Hastie, Tibshirani, and Friedman, a book that reaches into both worlds.

The one problematic word in my theme is "discipline." It's hard to argue with "statistics" or "unified," but wouldn't "profession" be friendlier than "discipline"? In truth, I've already received one complaint along those lines. My defense was that it was the intellectual discipline of statistics, not its very pleasant professional manifestation, that most profits from a unified viewpoint. As always, there are strong centrifugal forces pulling at statistics, from the diverse needs of our client fields, from competition in academe, from funding pressures in the modern scientific world. What keeps us together (literally, at the JSM) is our core of shared ideas, and that's what I most hope will maintain it's modest but genuine unity.

Besides choosing a theme, the ASA president gets a slice of monthly space in *Amstat News*. This year part of that space will be shared with a panel of guest columnists, with the goal of examining the statistics discipline, and profession, from several different points of view. Of course, I hope that this examination will validate the unity theme, but we shall see. As statisticians, we can never afford to flinch from the data.



Bradley Efron
ASA President

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MARCH COLUMN. MEGAN: THE BRACKETS << >> INDICATE ITALICS, IN THE TITLE AND ONCE IN THE

BUT WHAT DO STATISTICIANS <<DO>> ?

At a recent birthday party a friend produced a fake photo of me sitting next to Fidel Castro, with the caption "Brad tries to look interested as Fidel explains that statistics was his worst class in college." There is a depressing frequency to this sort of conversation, for me and I suspect for most of you too. What is it about statistics that elicits confessions of incompetence? It's hard to imagine "I was a complete dunce in English" as common party chatter, but statistics seems to be fair game.

These thoughts came back to me recently when I was interviewed for an ongoing column in the Stanford Magazine called "What do they do?". The nice (and smart) lady interviewer told me that this was a fairly new feature designed to explain "unusual" campus professions to the Stanford readership. The previous interviewee was the campus gardener, whom I expect had an easier time than I conveying his duties. He could point (authoritatively, the campus looks great) to trees and flowerbeds and pristine paths, things that most people know and treasure. What does a statistician point to?

Well, I tried my hardest. An ongoing consulting job at the medical school seemed promising. It involved 20000 genes and 88 microarrays, in a massive experiment concerning the genetic basis of atherosclerosis. I hauled out graphs and charts and computer displays, trying to convey the dangers of answering "which genes are important?" in a context with 20,000 candidates. I think, or maybe just hope, that some feeling for the collaboration between scientists and statisticians came across. But the fact is that it was a pretty complicated story, and my interviewer will need advanced reporting skills not to get lost in a maze of microarrays before ever getting to the statistics part.

Which brings us to the nub of the "what do statisticians do" problem. My research collaborators are trying to cure heart disease. I'm trying to help them understand the data they've collected in trying to cure heart disease. Statisticians work at least two levels away from nature. (Three levels when we write JASA papers intended to help other statisticians help scientists understand nature.) In its essence our work is more abstract than most of science, and science itself is scary enough for most people.

Another way to say this is that statistics is an information science, the first information science in fact, and the most fully developed. In the first four decades of the Twentieth Century an enormously ambitious and successful intellectual effort produced a theory of inference that cuts across individual scientific disciplines. It was by no means obvious that such a theory should even exist-- why should a method like regression apply to economics as well as astronomy, to geology as well as medicine? But it does exist, much to the benefit of science, as I was trying to get across with the microarray story. We don't need to construct a special theory for each new area of application, though context usually determines the specific details. Modern statisticians have combined Twentieth Century theory with Twenty-First Century computing power to build a general inference machine of impressive force. This machine has become the principal mode of scientific inference in literally dozens of fields, and the list keeps growing.

Which makes it more surprising how little many scientists themselves seem to know about statistics. Unless they have had occasion to need our services they are likely to think of statistics as a computer program, something like TurboTax, or maybe a dimly remembered formula for the standard error. My rule of thumb has been that scientific colleagues can do quite well with probability models but are not to be trusted with statistical inference. We are the only ones trained in the arduous kind of reverse logic required to go from data back to an unseen model.

A legitimate answer to the what-do-statisticians-do question is "Everything." Within the past few months I've heard statistic deparment seminars touching on medicine, biology, sociology, geology (image analysis), particle physics, and chemistry. Statistics departments tend to annoy university and business administrators by not fitting neatly into the organization chart, sprawling across division and school lines. One of the real charms of statistics is the opportunites if affords to peek into everyone else's science business. In an age of specialization we might be the last remaining scientific generalists.

Of course "I'm a generalist" doesn't get one very far at the cocktail party. Most people seem to know, or think they know, what biologists or physicists or economists do for a living. Even computer scientists have a movie-made persona these days, the geeky guy eating pizza in front of multiple screens while he hacks into secret websites. Statisticians don't have a signature piece of equipment to fall back upon, like those computer screens. Our profession is defined by its ideas and methodology, rather than specific subject matter or technology, which is noble enough but difficult to explain.

"How do statisticians <<think>>?" is a better question than what do we do. I still remember how difficult it was for me, an undergraduate mathematician, to understand why something like ANOVA made sense in analyzing real scientific problems. NFL cornerbacks learn the difficult skill of running backwards; we have to learn to think backwards, from the specific instance to the general rule. Along the way we also learn proper respect for the power of randomness to confuse the human mind. Statisticians are good at spotting patterns hidden in random noise, and even better at not being fooled by apparent patterns.

There are a few of us, not me I'm sorry to say, who do have the gift of conveying statistical ideas to the general public. (Fred Mostellor comes to mind.) Telling people what statisticians do is an important job, for us and also for society as a whole, which could definitely use a good deal more clear statistical thinking about crucial issues like environmental policy, terrorism, and preventive medicine. Those few journalists who can write clearly about statistical topics, Gina Kolata for one but there aren't many more, are our prime allies in the war against ignorance. To this end the ASA has instituted an Excellence in Statistical Reporting Award (Erisa). The selection committee, Don Berry, Betz Halloran, and John Rolph, is looking right now for the first winner among "members of the communications media who best display a committment to statistics and to advancing the role of the media in the science of statistics in public life". You can find out more on the ASA web page, www.amstat.org, and even nominate your own favorite journalist.

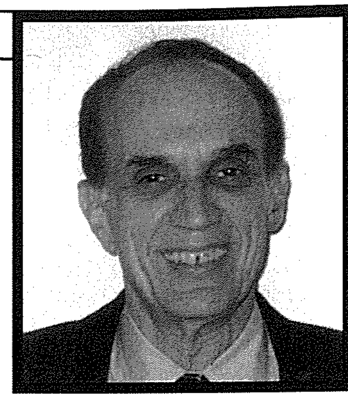
I hope that one of my guest columnists will have something helpful to say about dealing with that awkward pause at the cocktail party, just after you've answered "I'm a statistician." To this end I can only report my only success along this line, which occurred at a university gathering for winners of a minor award. Here is the conversation in full:

Nice Lady: "And what do you do?"
Me: "I'm a statistician"
Lady (after a pause): "Uh... what did you win for?"
Me: "I invented the mean".

She left perfectly happy.

....Bradley Efron

Statistics and the Rules of Science



Bradley Efron

It's June 1 and I'm looking at a polar projection of the earth, beautifully drawn by Richard Proctor. The projection shows precisely where it will be daylight, twilight, or night-time during the June 8, 2004, transit of Venus. Mr. Proctor drew the map in 1874*.

This is 19th century science at its best, predicting the affairs of the world with jaw-dropping accuracy, exemplifying the prestige and power of what "science" connotes in modern society. Science has changed a lot since 1874, moving in the direction of probability, statistics, and less precise but more widely useful predictions.

However, the rules of science, the underpinning of Proctor's, and our, approach to understanding an apparently inscrutable universe, have remained constant. These rules, which have governed the astounding ascent of science over the past 500 years, are elusive and even controversial. I wanted to say the rules out loud here, or at least my version of them, and also comment a little on their relation to statistics.

Modern science emerged in renaissance Europe as the convergence of three methodologies: the Greek legacy of mathematical logic, experimental empiricism (i.e., actually looking at nature—not a Greek strong point), and the craft tradition of applied technology. It's easy to forget the last of these, but technological enhancement of our senses is a cornerstone of science. Galileo needed his telescope just as modern biologists require the ability to work and see at the molecular level, and modern statisticians depend on electronic computation. Technology precedes scientific understanding more often than it follows.

The other great areas of human thought, religion and theology, arts and letters, and law and government, preceded science by millennia. The success of science has depended as much on what it rejected from its predecessors as on its own innovations. A key rejection was that of the sentient universe. A scientist at work relies on the assumption that nature has no will and runs by rules that make no exceptions: no magic, no miracles, no answered prayers, no appeals to higher authority; a "clockwork universe" in Laplace's vision. That the clockwork is less than perfect has a lot to do with the rise of statistical thinking, but, if anything, probability models have moved science even further away from the comfort of a caring universe.

It is emotionally difficult for most people to accept an uncaring universe, leading to familiar stereotypes of evil scientists or soul-less statisticians, and the fundamentalists' hundred-year war against Darwinian evolution. Depriving the natural world of will has, however, one enormous redeeming feature: simplicity. Science assumes that the world is basically simple, or at least simple enough for the human mind to understand. The arrogance of this assumption infuriates anti-scientists but it is difficult to argue against successes like those of Newton that enabled Proctor to say exactly what will happen on an afternoon 130 years in the future.

From an historical viewpoint the scientific revolution began with the simpler aspects of nature and has progressed toward more complicated phenomena: mechanics—astronomy—physics—chemistry—biology—medicine. The role of probability and statistics has grown with science's attack on more complex situations.

Fields like economics and psychology, where human will actually does play a major role, have proved the most difficult for scientific progress, and the most statistical in their methodology.

Another great scientific theme is the unity of nature: the rules of physics apply to chemistry, the rules of chemistry to biology, biology to evolution; people are made of the same stuff as stars.

One hundred years ago many good scientists thought that life itself might be exempt from common physical rules. The discovery of a universal genetic code is perhaps the ultimate vindication for unified science. A statistician like myself who applies linear regressions to quasars as quickly as to kidneys must be a firm believer in scientific unity.

"Scientific Method" conjures up in the public mind visions of carefully kept notebooks, precise titrations, and robot-like objectivity. Actual science is less codified and more interesting. No single prescription is wholly successful but a few general guidelines can at least help avoid common pitfalls. A good first guideline might be that analysis precedes synthesis. A hallmark of quack science is sweeping conclusions based on meagre investigation. True science attacks complicated phenomena by breaking them down into smaller components, exhaustively examining the pieces, and only at the final stage reassembling them into a general theory. Great syntheses we now take for granted, like the double helix or electricity and magnetism, required sci-

FOOTNOTE

* Proctor's map is from his book "*Transits of Venus: a Popular Account of Past and Coming Transits from the First Observed by Horrocks in A.D. 1639 to the Transit of A.D. 2012.*" It appears in the June issue of *Sky and Telescope* magazine.

entist-centuries of dedicated analysis before the triumphant conclusion. Great genius often attends the final step, but even Newton needed Galileo, Kepler, Copernicus, and Tycho Brahe.

Occam's razor says that simple explanations are better than complicated ones, at least if they do the same amount of explaining. In some ways this dictum works against aesthetics. We don't prefer Warhol's "Marilyn Monroe" to the Mona Lisa because Warhol uses fewer pixels. But if I produce an intricately clever 30-page proof of the prime number theorem while you give me a straightforward one-liner, the one-liner wins. Science has a minimalist quality in common with good engineering. It proceeds by making seemingly complicated things easy to think about, and the one-liner will probably make for easier understanding.

There is a utilitarian principle lurking here. Science has to *work*, not just look pretty. I get a little nervous when colleagues talk about the beauty of this or that theory, and a lot nervous if beauty is used to excuse a lack of contact with reality ("String theory is too beautiful not to be true.") Another vital scientific rule is that empirical evidence is the final arbiter of validity.

It doesn't matter how subtle my calculations may be, if they don't agree with empirical fact then into the shredder they go. Statistics might be considered a reality-challenged discipline. Our theorems and methods usually apply at a second level, to the output of scientists studying nature rather than to nature itself. The cold light of empirical fact takes longer to shine on statistical methodology. A new telescope either works or it doesn't, but what about a new statistical theory like false discovery rates or support vector machines? Applied statisticians, in self-defense, tend to stick with tried and true old procedures such as ANOVA or t-tests. My own rule of thumb is that the more elaborate a new statistical methodology, the longer it takes to show its true value, and the more caution is required.

Are scientists *objective*? The public certainly thinks we are, and feels betrayed when it turns out that we're just as prone as anyone else to prefer our own ideas to our competitors'. This misses a crucial point of scientific society: individual scientists don't have to be objective, but science as a whole does. From an operational viewpoint, the most important rules of science are open criticism and a free competition of ideas. Administrative structures like blind refereeing and peer review help the process along, but our real bulwark is a five-century tradition of genuinely free expression. Wrong ideas, the health benefits of hormone therapy being a recent example, are mercilessly exposed, no matter what the economic or political consequences.

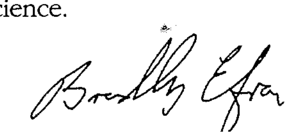
This process is easiest to appreciate when it goes wrong. When politicians blunder into science, as they have with stem cells, the results are usually bad for both parties. Proprietary research, now the norm in biomedical technology, threatens the free flow of scientific ideas. Happily, our society, more often than not, supports free science against the controlling hand of politics, religion, and economics. It takes a confident society to permit scientific freedom, and it is no accident that science has flourished only under the shield of such confidence.

Statistics has become the modern-day enforcer of scientific objectivity. Terms like randomization, blinding, and .05 significance wield a no doubt effective objectivity nightstick, but it is not a role most of us relish. My guess is that as a group we prefer the more positive aspects of statistical collaboration, where we help our scientist colleagues see what *is* in their data, rather than what isn't. That being said, the "isn't" part performs a real service for the scientific community.

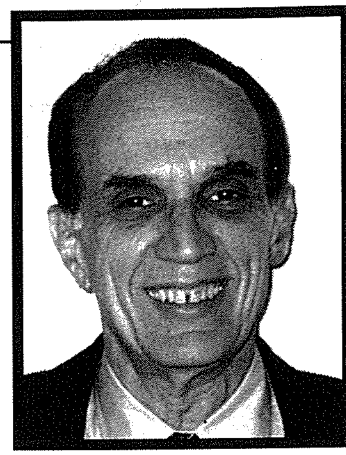
There is a corollary to a culture of open and free criticism: authority counts for little in science. The greenest lab assistant can challenge Einstein, and be quickly elevated if correct. One of Hollywood's choice set pieces shows the "established scientists" banding together to suppress the hero's new ideas. In practice, scientists get paid to overthrow the old paradigms, sometimes their own. Even now our physicist colleagues are searching desperately for "new physics," holes in general relativity or the standard theory of particles. Alas, the old paradigms are too good here—so far they've stood up to every challenge. Old science has been stubbornly successful in statistics, too; most of our basic theory remains pre-1950, although the methodology is greatly magnified.

Well, as I said, the rules of science are controversial and hard to pin down. Any proposed list can be tested against the historical reactions of scientists to unorthodox theories such as creationism, ESP, and cold fusion. But, perhaps out of timidity, I won't do any testing on my own list, at least not here.

By the time this article appears the transit of Venus will have come and gone. It will be astounding and disturbing news if it doesn't go exactly as Proctor predicted, such is our faith in the power of science.



Are Print Journals Obsolete?



Bradley Efron

My first copy of *JASA* is the June 1964 issue, half the size of the current megalith, featuring articles by Frank Graybill, John Pratt, Howard Raiffa, Leo* Goodman, and 25 others. I joined the American Statistical Association in '64 for one and only one purpose: to receive *JASA*; my first copies of *Biometrika*, the *Annals* (then of "mathematical" statistics), and the *Journal of the Royal Statistical Society* date from the same year. The accrual rate for the four journals has been about one foot per year, leaving me 40 shelf-feet of valuable statistics literature to deal with. This week I'm moving some of the older shelf-feet to an "off-site storage area" (my basement), which is sweaty work by academic standards.

All this unfamiliar exertion has put me in a mood to question the wisdom of amassing journals, "hard copies" in the current argot, in an age when any computer terminal puts an entire library in your office. My younger colleagues (that's all of them) seem to live off the computer screen and the printer without much need for the journals themselves. A graph of journal shelf-space versus age in Sequoia Hall would suggest that the end of print journals is just around the demographic corner. Even I, a defender of the old guard, guiltily download reprints from JSTOR (including my own papers!) in preference to my shelved journals. They look so nice, it's so convenient, the paper isn't disintegrating, it's easier for trips, etc., etc.

And yet, and yet... I'm not at all certain that print journals are on their way out, and if they are I'm afraid that there is much to be lost to the scientific world. More is at stake here than faux nostalgia for the good old days of mom and pop stores that supermarkets put out of business. The journal system has served science well since the original issue of the *London Philosophical Society* in March 1665. (The article by Robert Boyle is titled "An account of a very odd monstrous calf.")

The obvious purpose of scientific journals, and still the main one, is the dissemination of scientific information. Journals replaced personal letters in 1665, taking a huge step forward in effective communication and the democratization of science. But dissemination is the role most immediately threatened by electronic media. If quick and fast is the criterion, then one shouldn't bet on the tortoise. I remember a visit to Australia several years ago when the latest issue of *Biometrika* arrived—six months after publication. One pictures the great ship leaving England with Australia's *Biometrika*s in its hold, bravely plowing the seas on the long slow voyage to Sydney. Even nowadays in the United States, the ASA journals and magazines, including *Amstat News*, arrive a month late on the west coast.

But of course quick and fast is not the only criterion. Slow and steady have something to say for themselves, too. JSTOR might go out of business next year, but the physical existence of all those hard copy journals (and books, too, of course) would ensure the continuity of our discipline. A more realistic point is that JSTOR, or Project Euclid, would have nothing to retrieve if it were not for our journal record. A print journal keeps disseminating its information for decades after its arrival. Some of my friends keep their old journals at home (not in the basement) where they can browse the back issues at leisure. Maybe *JASA* and *Technometrics* and the *Annals* are like fine wine, with connoisseurs musing, "56 was a very good year...."

The browse factor continues to perplex computer engineers, those who work on the "human-machine interface." Even electronic stalwarts don't really like to read off the screen. There is something perfect about the fit of the codex book format to human perception that is missing from the electronic world, even electronics supplemented by single-article downloads. That's now, but we can imagine a not-too-distant future in which it's easy to download a high-quality bound volume of, say, current microarray papers, choosing them yourself or perhaps relying on a random search engine, a scholarly version of the iPod® "shuffle" feature.

The worst feature of journals from an author's standpoint, the lack of space that limits acceptances (*JASA* is under 25%) and abbreviates those papers that do make it into print, is a boon to the profession as a whole. An editorial staff is under strict constraints to limit the number of printed articles. Choices must be made, choices that award a blue ribbon to a small percentage of the profession's output. In this sense journals act as magazines that direct our field's attention rather than just report it. Anonymous peer review is a crucial aspect of the journal system. Electronic journals have peer review too, but I fear that without physical constraints a kind of grade inflation will inevitably expand the table of contents to flabby proportions.

The blue ribbon aspect is much in evidence in university promotion and tenure decisions. Candidates without representation in a short list of top-tier journals are likely to find themselves among the academic homeless. Choice conference proceedings substitute for journals in some avant garde fields such as information retrieval, but the basic elements are the same: strict refereeing, limited acceptances, and eventual hardcopy publication.

All of this concern about print journals could be dismissed as airy academic hand-wringing except for one fact: the ASA depends heavily on its journals for financial support, and, more seriously, for membership. *JASA*, the world's most cited mathematical sciences journal, is a particularly valuable property. It, along with the *Annals* and *Technometrics* and *Biometrics* and other key journals, embodies the academic side of statistics in the United States, and helps define the profession for many of us. What will happen to the ASA, and our professional sense of identity, if trends in electronic communication render print journals obsolete?

The trouble with trying to answer this question is that nobody seems certain what these trends might be. Or rather, many are certain, but almost as many disagree. This year the ASA has a task force examining the issue of electronic publication, under the experienced leadership of Karen Kafadar. Task forces are only appointed for a year at a time, but this is the fourth year in a row for "E-pubs," as the Association tries to face the publication future without forgetting the past. And, in fact, this year we do seem closer to at least some action on electronic journal access.

One possible action concerns "arXiv," an electronic repository for paper and preprints. (Imagine a CIS where the entire article is available; see *www.arxiv.org*.) Begun in the physics community a dozen years ago, arXiv now serves a wide range of mathematical disciplines. Recently a physics friend complained to me how inaccessible the statistics literature seemed without an arXiv culture. Maybe this won't be true for much longer. The Institute of Mathematical Statistics now plans to place all *Annals* papers in arXiv for universal access, and the ASA is considering following suit. The goal is to encourage a thriving arXiv section for statistics (probability is already up and running) where everyone would deposit preprints as well as published material. ArXiv doesn't seem to have put the physics journals out of business, but it seems likely to undercut their primacy as reporters of the physics scene.

The arXiv proposal raises the question of full electronic access for our journals. Currently, ASA members enjoy electronic access to the journals they subscribe to but not our other publications. The IMS has just changed their policy to make all IMS journals available to members, regardless of their print subscriptions. Access will come through Project Euclid and JSTOR. Complete electronic access is a good deal more drastic than arXiv-style access to individual articles, where there is likely to be delays and there will certainly be a loss of convenience.

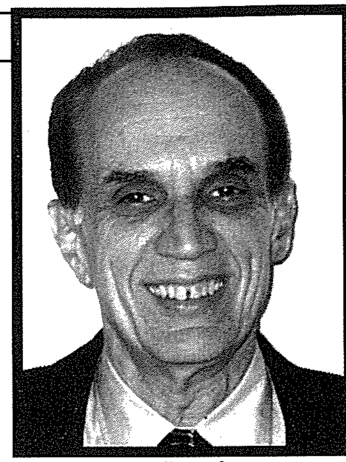
Still more drastic options lurk in the future. Legislators in both England and the U.S., reacting to the excesses of commercial publishers (Elsevier being the most frequently accused), are beginning to push for universal open access, with the NIH taking the lead. The NIH's recent proposal says that within six months of publication, papers must be placed in PubMed Central, an arXiv-like repository. On hearing this, the senior managing editor for the American Academy of Pediatrics commented that under the PubMed plan "nobody is going to purchase a subscription." Maybe that's overblown, but there is the danger that in pushing to punish the commercial publishers Congress will succeed mainly in wreaking havoc on academic societies like the ASA, which have been good scholarly citizens: the ASA charges less than one-tenth as much per page as some of the commercial journals. One of the ironies of what might be the end stage for print journals is that the commercial houses, having seized control of the "blue-ribbon" process in fields like cell biology, have raised journal prices to astounding levels.

A host of variations is available on the open access theme: delayed access like the PubMed plan, 30-day open access following publication, the Public Library of Science model where authors bear the publication expenses. Some of these envision print as well as electronic versions, but others are totally web-based. As I tried to say earlier, losing print journals would have hidden costs in terms of lost shared experience. The journals, and the societies that publish them, have helped integrate far-flung, sparsely populated disciplines such as statistics. We have reason to think carefully before discarding the print model. Readers who wish to learn more about open access can visit Jim Pitman's web site at *www.stat.Berkeley.edu/users/pitman/strategy.html*.

Well, TV was supposed to kill the movies but it didn't. Movie producers fought back with improved technology, wide screen formats, that TV couldn't match. Print journals probably won't disappear either, but they may have to improve to survive. In a world with color web access and color office printers, it must be time for the journals to go full color. Nice shiny magazine paper would help a lot, too. (Take a look at the Lee, Lin, and Wahba article in March's *JASA*.)

A couple of months ago I made a confident prediction that the ASA would still be in business 150 years from now. I'm not so certain about the 250th print volume of *JASA*. On the other hand, the *London Philosophical Society*, now the *Royal Society*, is still going strong, having published volume 359 this year, so it may be a little early to say farewell to all those big blue *JASA* volumes.

Bradley Efron



Bradley Efron

Life in a Random Universe

December in Palo Alto brings with it two notable natural phenomena: the days get short and it starts to rain a lot. These are both "scientific facts" but they involve quite different kinds of science. Shorter days exemplify hard-edged science, precise and so predictable that you can sell an almanac saying exactly how short each day will be, down to the nearest second. The almanacs try to predict rainfall, too, but they're not nearly so successful. Rainfall is a famously random phenomenon, as centuries of unhappy farmers can testify. (My father's almanac went further, predicting good or bad fishing weather for each day, indicated by a full fish icon, an empty fish, or a half fish for borderline days.)

Hard-edged science still dominates public perceptions, but the attention of modern scientists has swung heavily toward rainfall-like subjects, the kind where random behavior plays a major role. A cartoon history of western thought might recognize three eras: an unpredictable pre-scientific world ruled by willful gods and magic; the precise clockwork universe of Newton and Laplace; and the modern scientific perspective of an understandable world, but one where predictability is tempered by a heavy dose of randomness. Deterministic Newtonian science is majestic, and the base is modern science ,too, but a few hundred years of it pretty much exhausted nature's storehouse of precisely predictable events. Subjects like biology, medicine, and economics require a more flexible scientific worldview, the kind we statisticians are trained to understand.

These thoughts were very much in my mind at Phystat2003, a conference of particle physicists and statisticians held at the Stanford Linear Accelerator Center last year. It's at least slightly amazing to me that the physicists, who were the convening force, were eager to confer with us. One can't imagine Phystat1903, back when the physics world disdained statistics. "If your experiment, needs statistics you ought to have done a better experiment" in Lord Rutherford's words. (It may be a mistake to ever call a scientist "Lord.")

Rutherford lived in a rich man's world of scientific experimentation, in which nature generously provided boatloads of data, enough for the law of large numbers to squelch any noise. Nature has gotten more tight-fisted with modern physicists. They are asking harder questions, ones in which the data is thin on the ground and where efficient inference becomes a necessity. In short, they have started playing in our ballpark.

The question of greatest interest at Phystat2003 concerned the mass of the neutrino, a famously elusive particle that is much lighter than an electron, and may weigh almost nothing at all. Heroic experiments, involving house-sized vats of cleaning fluid in abandoned mine shafts, yielded only a few dozen or a few hundred neutrinos. This left lots of room for experimental noise, and in fact the best unbiased estimate of neutrino mass turned out to be negative. Mass itself can't be negative, of course. Given a negative estimate, the physicists wished to establish a statistical upper bound for the true mass, the smaller the better from the point of view of further experimentation. As a result the particle physics literature now contains a healthy debate on Bayesian versus frequentist ways of setting the bound. The current favorite is the "Feldman-Cousins" method, developed by two prominent physicists, a likelihood-ratio-based system of one-sided confidence intervals.

It took enormously long for the statistical point of view to develop. Two thousand years separate Aristotelian logic from Bayes theorem, its natural probabilistic extension. Another 150 years went past before Fisher, Neyman, and other frequentists developed a statistical theory satisfactory for general scientific use in situations where Bayes's theorem is difficult to apply. The truth is that statistical reasoning does not come naturally to the human brain. We are cause-and -effect thinkers, ideal perhaps for avoiding the jaws of the sabre-toothed tiger, but less effective in dealing with partial correlation or regression to the mean.

Once it caught on, though, statistical reasoning proved to be a scientific success story. Starting from just about zero in 1900, statistics spread from one field to another, becoming the dominant mode of quantitative thinking in literally dozens of fields, from agriculture, education, and psychology to medicine, biology, and economics, with the hard sciences knocking on our door now. A graph of statistics during the 20th century shows a steadily rising curve of activity and influence. Statisticians, a naturally modest bunch, tend to think of their field as a small one, but it is a discipline with a long arm, reaching into almost every area of science and social science these days.

Our curve has taken a bend upwards in the 21st century. A new generation of scientific devices, typified by microarrays, produce data on a gargantuan scale—with millions of data points and thousands of parameters to consider at the same time. These experiments are "deeply statistical." Common sense, and even good scientific intuition, won't do the job by themselves. Careful statistical reasoning is the only way to see through the haze of randomness to the structure underneath. Massive data collection, in astronomy, psychology, biology, medicine, and commerce, is a fact of 21st century science, and a good reason to buy statistics futures if they are ever offered on the NASDAQ.

Several years ago I served a term as associate dean for science—a mouse training to be a rat as the old saying goes, though I never graduted to rat status. It was an interesting job that gave me a chance to see what life was like for our fellow scientists. Most of the stories were happy ones, with lots of good work and scientific progress in view, but there were serious problems, too. Biologists and chemists work in big expensive teams these days, putting senior scientists on a constant treadmill of grant requests and project supervision. Physics seems to have an overpopulation problem, with too many smart people chasing too little data. (Perhaps that is why the physics stories I see in *Scientific American* have taken on a science fiction aspect: "You may be able to travel backwards in time through worm holes.") Mathematics has become an inward-looking field, very successful in solving the problems it sets for itself, but dangerously cut off from the larger world of science.

My dean experiences led me to write down a list of three conditions for a healthy scientific discipline:

1. An outside demand for answers in the discipline's chosen area.

2. Some evidence of past success in answering such questions.

3. An ongoing production of useful new ideas.

This list reflects the importance of both the inside and the outside of a scientific discipline. The inside part, the internal development of the field along new directions, is what makes a field fun to work in. But without outside demands for answers, demands that test new ideas in the fire of genuine applications, the fun can turn solopsistic, drifting into "angels on the head of a pin" territory. This was my concern about mathematics.

Statistics has its own problems. It still has junior status in the science world, with less history and a less clearly defined subject area than the traditional heavyweights. All in all, though, my dean time made me happy to return to statistics, a smaller field but one on the way up, not overpopulated, not driven by major funding or equipment needs, with lots of interesting problems to work on, and a healthy (almost overly healthy) outside demand for answers.

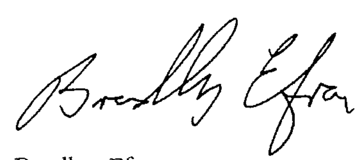
I find the microarray story particularly encouraging for statistics. The first fact is that the biologists *did* come to us for answers to the inference problems raised by their avalanche of microarray data. This is our payoff for being helpful colleagues in the past, doing all those ANOVAs, t-tests, and randomized clinical trials that have become a standard part of biomedical research. And indeed we seem to be helping again, providing a solid set of new analytic tools for microarray experiments. The benefit goes both ways. Microarrays are helping out inside our field too, raising difficult new problems in large-scale simultaneous inference, stimulating a new burst of methodology and theory, and refocussing our attention on underdeveloped areas like empirical Bayes.

Ken Alder's 2002 book, *The Measure of All Things*, brilliantly relates the story of the meter, one ten-millionth the distance from the equator to the pole, and how its length was determined in post-revolutionary France. Most of the book concerns the difficulties of the "savants" in carrying out their arduous astronomical-geographical measurements. One savant, Pierre Mechain, couldn't quite reconcile his readings, and wound up fudging the answers, driving himself to near-madness and death.

Near the conclusion of *Measure*, Alder suddenly springs his main point, forgiving Mechain as laboring under an obsolete, overly precise notion of scientific reality:

"Approach the world instead through the veil of uncertainty and science would never be the same. And nor would savants. During the course of the next century science learned to manage uncertainty. The field of statistics that would one day emerge from the insights to Legendre, Laplace, and Gauss would transform the physical sciences, inspire the biological sciences, and give birth to the social sciences. In the process 'savants' became 'scientists.'"

Right on, Ken! Alder's new world of science has been a long time emerging but there is no doubt that 21st century scientists are committed to the statistical point of view. This puts the pressure on us, the statisticians, to fulfill our end of the bargain. We have been up to the task in the past and I suspect we will succeed again, though it may take a couple more Fishers, Neymans, and Walds to do the trick.



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