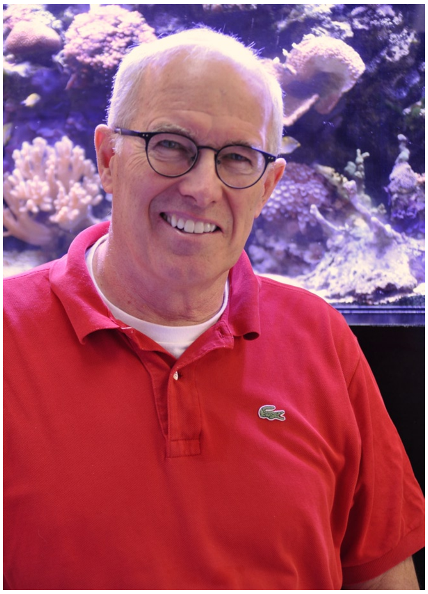
An enduring enthusiasm for academic science, but with concerns

John R. Pringle
Department of Genetics, Stanford University School of Medicine, Stanford, CA 94305

INTRODUCTION

I turned 70 this year and started graduate school shortly after turning 20. Thus, at this writing, I have spent 71.3% of my life in academic science. I have no regrets and hope to continue for years to come, because I remain an unabashed enthusiast for both the research and education components of this profession. The high honor of the E. B. Wilson Medal and the accompanying request to write this essay give me an opportunity to reflect on why this is so, as well as on my concerns about the fouling of the nest that has made it harder for any of us—but particularly for younger people—to enjoy academic science as I have over the past 50 years.

My enthusiasm should not be misunderstood as naïveté. I well understand that not every aspect of the job is fun; some tasks are inevitably tedious or even painful. It hasn't always been easy for me personally; like most people, I struggle with my own limitations, notably an incurable perfectionism and the frequently linked traits of avoidance and procrastination, which have often made it difficult for me to finish papers and other tasks. I have had papers and grants rejected and seemingly great ideas that turned out to be lousy when actually tested. As a graduate student, postdoc on two continents, faculty member at three universities, and dean for graduate students and postdocs, I have seen plenty of nasty stuff: subcompetent people obstructing progress at all levels; arrogance and self-centered insensitivity to the feelings of others; exploitation of students and postdocs by faculty; dishonesty ranging from exaggerated claims and the concealment of inconvenient data to gross fabrication; pointless and demeaning squabbles about priority and authorship; abuse of the still-critical tenure system; inappropriate behavior by editors and reviewers; and behavior driven by lust for power, money, and fame rather than by any desire to understand nature and (perhaps) improve human well-being in the process. And, in recent decades, I have seen the environment in which science is done erode in ways that I deplore.



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Nonetheless, I still feel that doing science is fundamentally Good (even noble) and that universities are wonderful places. I still look forward (almost) every day to going to the lab, and I have liked (to various degrees) almost every scientist I have known over a long career. How can this be? My attempt at explanation follows.

A DEEP AND ABIDING BELIEF IN THE SCIENTIFIC ENTERPRISE

Already as a child, I realized (sometimes during conflicts with my teachers) that I simply hated opinions that were illogical, irrational, or based on appeals to authority. Thus, I was ineluctably drawn to math and science and the rule of reason and evidence. I haven't changed, and I feel deeply proud to be, in a small way, a successor to Galileo, Darwin, Pasteur, E. B. Wilson, and the host of others who have helped to push back the fog of ignorance and open ever more of the universe to human understanding. As I watched the Nobel Prize ceremonies in 2001, I found tears running down my cheeks, not because of my personal connections to the laureates and the work for which they were being honored, but because the language of the citations—about the importance of science for the human spirit and the betterment of the human condition—was so incredibly moving. It was a vivid reminder that the scientific enterprise is bigger and grander than its often-flawed individual practitioners, and it is our shared commitment to this enterprise that it has made it so easy for me to like other scientists regardless of their personal idiosyncrasies. This all may seem sentimental and wildly idealistic, but I think it is the truth, and it still sustains me during times of frustration.

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John R. Pringle is the recipient of the 2013 E. B. Wilson Award from the American Society for Cell Biology.
Address correspondence to: John R. Pringle (jpringle@stanford.edu).

Meanwhile, the human condition seems more precarious than ever. I'm not sure that science can save us, but I am confident that it can help, and I am equally confident that irrationality and superstitions will not. Scientific medicine saves lives (mine, for one), science-based engineering creates marvelous devices (like the laptop computer on which I write this), and rational planning can save local ecosystems (e.g., the Guanacaste Dry Forest in Costa Rica), so there is surely hope for our species and our planet. To succeed, however, we will need more good scientists and—even more importantly—a public that understands scientific evidence and reason, which is of course why more and better science education is so critically important for our collective future.

A CAREER BASED ON LUCK AND JUDGMENT

That I have enjoyed my own career so much seems to me a matter of good luck with an occasional infusion of good judgment. I wasn't exactly to the manor born. My parents suffered greatly during the Depression of the 1930s, leaving them very focused on financial security but also with a reverence for the education they had missed. Given his own experiences and concerns, it is remarkable that my father advised me repeatedly (it is among my earliest memories) to "find something to do that you would do for free, and hope that you can make a living at it." Growing up near Chicago, I first planned to follow this advice by succeeding my hero Ernie Banks as the Cubs shortstop, but I had the good judgment to realize that my inability to hit my weight would be an obstacle. When I was 14, we moved to Evanston, where the honors classes made school fun for the first time and put an academic spin on my ambitions.

High-school math was easy and fun, so I decided to become a mathematician. But by good luck, I went to Harvard, where comparisons to some classmates suggested that I did not have big-league talent in this field either. Fortunately, a spring break spent with my roommate's evolutionary-biology books (when I was supposed to be doing math and physics) revealed the fascination of biology just as E. O. Wilson was trolling for math students to recruit into population biology, and I was soon accepted for graduate study despite my scanty preparation. The department addressed my deficiencies with a five-course load during my first year that included organic chemistry but also, luckily, spectacular courses in genetics (Matt Meselson and Nick Gillham) and cell biology (Keith Porter), and I soon realized that my interests, talents, and temperament were better suited to these fields than to ecology and evolutionary biology.

But I was also temperamentally unsuited to join the crowds then studying *E. coli* and its phages and mammalian cell biology, and I had also glimpsed the potential of yeast in the laboratory portion of the genetics course. There were no yeast groups in the Boston area, but I convinced protein chemist Guido Guidotti to sponsor me for study of "some interesting yeast protein." But protein chemistry wasn't right either, and I struggled mightily before producing a thesis (mostly on proteolytic artifacts). In the midst of my agonies, I somehow convinced Herschel Roman to accept me for postdoctoral work in Seattle, then the only center of yeast genetics in the country. Herschel soon began nudging me toward his new recruit Lee Hartwell, but I was not excited by Lee's studies of RNA and protein synthesis. However, during a September 1969 visit to my medical-resident girlfriend, I heard about the first cell cycle mutants and had the good judgment to sign on immediately. When I joined the lab in 1970, I somewhat perversely veered away from study of the *cdc* mutants to focus on control of the cell-cycle by nutrition, cell size, and pheromones. Lee was initially dismayed, I think, but it had a happy ending in the development of the concept of Start (Hartwell *et al.*, 1974).

After two more years of work on the nutritional control of cell proliferation (and much broadening of my knowledge of the world) in Zürich with Armin Fiechter, I started my faculty career at Michigan in 1975. Both Lee and everyone else seemed to be interested in the nuclear events of the cell cycle, so wanting, as always, to be different, I decided to focus on events in the cytoplasm, namely bud formation and cytokinesis. It was not clear that the mechanisms would be of any "general interest," but I didn't care, as I found the problems interesting myself, and they were essentially unexplored but clearly approachable using the powerful methods of yeast genetics. The department was not strong in genetics or cell biology, and I had a heavy teaching load, but I believe that these two "problems" were also blessings in disguise. I never worried at all about promotion, a sound strategy for Assistant Professors in general, I think, but one that is no doubt harder to follow in some situations. The teaching was very satisfying (particularly "Biology for Nonscientists") and educational, and the nine-month salary that accompanied it freed me from excessive worry about grants. Thirty-eight years later, I have shifted focus many times (actin and microtubules, septins, Rho proteins, positional signals in the cell cortex, cell-wall synthesis, evolution of cytokinesis mechanisms, etc.) and moved twice (to North Carolina in 1991 and to Stanford in 2005). I have also started a totally different project (attempting to transform coral biology by developing, more or less from scratch, a proper model system for its study) that now consumes most of the effort of my lab. But we continue to learn interesting new things from yeast, most of which—mirabile dictu—continue to be very broadly relevant.

In looking back, I think that I have been fortunate in many ways. First, for reasons of deep and incompletely understood psychology, I have always been attracted to problems that other people were ignoring (the blank spots on the map), and I think that this has made science more fun in several ways (the sense of adventure, the lack of any sense of competition). What would be the point of doing the same thing as someone else, even if I could do it a little faster (unlikely in my case anyway)? As a student and postdoc, I had mentors who were supportive but willing to let me find my own way, and as a young faculty member, I felt free to ignore what little advice the senior faculty had the temerity to give me. As a result, I have never felt that I had a "boss," and I can only imagine how unpleasant I would find this. Although sticking for many years to one general area of research, I shifted focus often enough to stay excited, and at the first hint of boredom in my early 60s, I started something totally new, which was thoroughly rejuvenating. I decided early that science would be more enjoyable (not to mention more effective) if I were always open and willing to share ideas, materials, and credit, and I have never once regretted this decision. I have for the most part had people in my lab who accepted my ideas about how science should be done, so that my interactions with lab members (and alumni/ae) have mostly been harmonious. In each move, I left a fine institution and good friends behind, but was stimulated by the new environment. And last, but not least, I had the good luck to find a partner (that medical resident, now a professor of medicine and cancer-center director) who was both passionate enough about her own work to understand my passion for mine and brave enough to join me in the enormously rewarding collaboration of raising two children, which we did without feeling that we were cheating either them or our work.

SOME PROBLEMS WITH ACADEMIC SCIENCE TODAY

Despite my own unabated enthusiasm, I feel that the scientific environment has deteriorated considerably during my 50 years of involvement, and I am ashamed that my generation has allowed this to happen on our watch. The problems are complex, interrelated, and not all easily solved, and there is not space here to discuss them all. So I will focus on some lowlights, as I see them (while stressing that no other individual or organization bears responsibility for my views!).

1. The inability of some highly qualified people to find academic positions. Despite the undoubted numbers crunch, which has deep roots and is not easily solved, until recently I felt that the young scientists with the greatest potential could always find positions that would allow them to blossom as independent investigators, teachers, or both. Now I am doubtful, and I think that the problem is due in part to institutions' hiring not on the basis of candidates' potential for truly creative work but on whether they are working in fields perceived to be "hot" and well funded, which to me automatically means "less left to be done" and "vulnerable to future changes in funding fashion." It is also a true shame, given the enormous world of fascinating biology left to be explored. I also suspect that the current financially driven stampede toward massive online courses will worsen this situation, perhaps catastrophically, and will degrade the quality of education and even the understanding of what a good education comprises.
2. Poor performance by most funding agencies. Getting a reasonable amount of funding to do genuinely novel basic research is harder than ever, despite some well-intentioned efforts to counter this problem. Among the reasons are the commitment of excessive portions of the agencies' total budgets to projects that are top-down, directed toward overly specific practical goals, and/or excessively large (and thus almost inevitably wasteful), rather than to the investigator-initiated, small-group projects that have always brought most genuinely new discoveries (and the technologies that follow them).
3. The rise to power of commercial and non–peer-edited journals. Hartwell *et al*. (1974) was rejected without review by *Nature*, leaving a bad taste that has lasted, and I have never again submitted a paper there by choice. I have also watched with horror as a host of other journals has appeared for which the best interests of science and scientists are secondary to financial profit and promotion of the power and influence of the journal itself, and at which decisions on whether to publish, and with exactly what content, are all too often made by people who are ill prepared to make good ones. Even more appalling has been to watch these journals gain influence almost in proportion to how poorly they serve the true interests of science. Until 2000 or 2001, I could truthfully tell young scientists that in all the many search-committee, promotion-committee, and study-section meetings in which I had participated, I had never once heard the names of journals (much less their so-called "impact factors"—a metric so deeply flawed as to be ludicrous) used as a criterion in judgment, but, sadly, this is no longer the case. Fortunately, there is a growing realization that we need to wrest control of our most important decisions back from people who have neither the competence (nor, typically, the motivation) to make those decisions well (Bertuzzi and Drubin, 2013; Johnston, 2013; www.ascb.org/SFdeclaration.html). Reducing the perceived importance of publishing in certain journals should also reduce the temptations toward overstatement, concealment of nuance and doubt, and outright fakery.
4. The increased agonies of publication. Publishing one's work used to be (mostly) fun and satisfying, but it is now too frequently an excruciating ordeal for all concerned. The multiple reasons for this include: 1) Unrealistic expectations. High standards are fine, but science is a journey, not a destination, and no one paper will provide definitive answers to all of the questions in its purview, so that reviewers and editors need to be reasonable. 2) Too many editors who do not have the confidence, or will not invest the time, to evaluate carefully the reviews that they get and give appropriate decisions and instructions to the authors. 3) Closely linked to the preceding point is the routine use of three reviewers, a new and pernicious practice that slows down the reviewing process, adds to the burden on conscientious reviewers, and typically just adds aggravation for the authors without producing any real improvement in the quality of the paper. My own observation is that a third reviewer should be needed <10% of the time and only when the first two reviewers disagree wildly and the editor does not have the competence to adjudicate the matter (which, ideally, should not happen very often at a well-run journal).
5. The decreased quality of publications. I could rant at length but will restrict myself to just a few of the problems that I find most aggravating. 1) The fundamental error of confusing brevity per se with tight writing. Scientific publications should not waste a single word or figure panel, but arbitrary length limits encourage both the splitting of what should be one thorough paper into two or more logically incomplete ones and the omission (or, almost as bad, dumping into supplemental material) of detailed methods, valuable subsidiary results, and important caveats. Despite the effort required, the authors, reviewers, and editor should share the burden of producing a paper that is just the right length for its content. 2) Careless and sloppy writing. Good, clear writing is hard work, especially if it is not in one's native language, but anything else is a disservice to the community, as well as to the authors (whose experiments may reasonably be assumed to be as careful, or as sloppy, as their writing). Our programs need to provide more training in good writing, which is also hard work (on both sides), but critical. 3) The devaluation of Materials and Methods sections. The essence of science is its reproducibility, and you can't reproduce it if you don't know how it was done. I still almost always read the methods section first (when I can find it!), because I want to know whether there is any reason to believe the results. I think that the movement of the methods section to the rear of the paper sends entirely the wrong message and should be reversed in those journals in which it is allowed or even required. 4) The trend to declarative-sentence titles. Such titles, which were essentially unknown before 1980 (Rosner, 1990), are in part a sequela of the perceptions that it is important to publish in certain journals and that a bold claim about what you have (perhaps) discovered will help you get there. They are not actually needed to convey the most important contents of a paper, and they are objectionable for two reasons. First, they almost always overstate the solidity of the main conclusion(s), whereas a decent humility in the face of the complexity of nature is a more becoming posture for a scientist. And, of course, sometimes the bold statement turns out to be simply wrong, yet it lives on in the databases to confuse others for many years to come. Second, very few papers are so one-dimensional that a single sentence can describe them, so that such a title serves to obscure the true richness of the paper's contents.

CONCLUSION

Academic science has never been a paradise on earth; there have always been problems and frustrations. Although some of these have become worse over the past 50 years, many of our current problems could be solved, at least in part, by community will and action. In any case, if I were 20 again, I would take my chances and pick the same career, not only because it suits me personally but also because I still believe unshakably in its deep and abiding value to our species.

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