I was invited here to elaborate on issues raised in an essay I published, along with a companion essay by Mark Roulston, in the Bulletin of the American Meteorological Society (BAMS) in May, 2006. My essay is entitled Deficiencies in the Present Funding Process in Meteorology; the nature of its subject matter is self explanatory by the title. Mark’s is entitled The Scientist’s Dilemma, which provides a simple mathematical underpinning for some of my ideas.

Response to the pair of essays was overwhelmingly positive; I received well over a score of e-mail messages and verbal replies from colleagues at Penn State and around the country, some of them quite passionately articulated. These responses underscore the fact that many in the meteorological community were already dissatisfied with the funding problems and outraged at its abuses. We clearly have a problem in our field.

I would like to begin this talk with a very brief historical background relevant to the issues at hand, and then summarize the points raised in BAMS, including some additional anecdotal material derived from my correspondence. I will touch on dangers resulting from the present state of affairs, and finally briefly suggest possible remedies. My overall emphasis here will be on systemic defects and their detrimental effects on the morale and well being of young faculty members. In BAMS, I begin by saying that the what I wrote was prompted less by own experiences than by the perception that younger faculty members were enduring an increasing and burdensome stress in trying to meet the demands of publishing, writing proposals, teaching and other duties.

According to William Clark in his book Academic Charisma and the Origins of the Research University, the research university began in Protestant Germany after the reformation and has continued to flourish ever since, though reaching its zenith in Europe over a hundred years ago. Clark Kerr, the former chancellor of the University of California, describes four phases in the development of the research university in the United States: (1) its
origins: 1810 - 1870; (2) slow growth: 1870 - 1940; (3) rapid expansion: 1940 - 1990, and (4) an era of constrained resources 1990 - 2015. The present ills in atmospheric funding which I will touch on arise from a disjoint between phase 3, which Stuart Rojstaczer in his book Gone for Good calls the ‘Golden Age’, and phase 4, the present.

Currently, we are experiencing a decline in research funding (in real dollars on a per researcher basis), a situation that was foreshadowed by a decrease in direct government subsidies to higher education during the 1990s. Clark Kerr states that, overall in the United States, direct government support of higher education decreased about 10% from 1989 to 1993, though a few states have experienced increases. By contrast academic research spending, largely from grants, increased by about a factor of 30 between 1962 and 1998 according to Howard Birnbaum, writing in Physics today (2002). Even so, academic research awards have still not kept up with the inflated costs of carrying out research during the past 40 years, although the cost of research greatly exceeded the increase in the cost of living during this same period.

Kerr specifically states that the maintenance of current levels of research funding will be ‘especially hard on many younger scholars trying to get established’. I was particularly struck by one younger faculty member at my institution telling me that his most satisfying moments in research were involved with unfunded ideas. I had already discovered this bit of enlightenment for myself. Other responses echoed this sentiment. One exceptionally bright young professor I know at another institution mentioned that he had given up proposal writing because the process was wasteful of his time and the results of his efforts were very discouraging. He too found that the greatest joy in research came from unfunded projects. As I state in BAMS ‘if lack of funds promotes creativity and joy in research, what does this say about the efficacy of research funding?’

At bottom, the detrimental effects of current funding practices, of which I wrote, were brought on by a decline in the changes in resource allocation by federal government and the concomitant change in attitudes (in the words of Clark Kerr, the ‘great change in spirit’) that occurred during the early 1990s. Yet, faced with declining success in grant writing, many faculty members do not have the luxury of what Stuart Rojstaczer calls ‘curiosity based research’ and, instead, have been forced to resort to various devices in order to bolster the declining odds against them. James Lovelock, in the preface to his book The Ages of Gaia (1988) states that scientists have become employees of
large institutions wherein they have foregone curiosity for conventional wisdom and have ‘traded freedom of thought for good working conditions, a steady income, tenure, and a pension.’

A colleague of mine mentioned a comment made to him by a program manager at a well known funding agency that the exponentially increasing numbers of proposals they received were of declining quality, as was the quality of the reviews. Another colleague in a national institution mentioned that as failure rates go up, so does the number of proposals. The situation evokes an image of runaway inflation in economics. As another colleague put it ‘underneath the mountain of proposals, too little of scientific importance is happening. We can play the game but we don’t like it.’

Mark Roulston puts the frantic efforts to obtain funding into the context of game theory, citing Nash equilibrium as the ultimate stalemate in proposal writing; (Nash, of course, refers to John Nash of the movie and book, Beautiful Mind; Nash, one may recall, won the Nobel Prize for economics some years ago.) Roulston shows that if everyone writes the same number of proposals no one can improve their chances of success. Indeed, the chances of success would be identical if everyone submitted one or a large number of proposals, except that, in the case of a large number of submitted proposals, everyone loses. They lose because each proposal has a cost attached to it, a cost in time, energy and resources and in intangibles such as the quality of life and the drain on emotional and physical resources…… in other words in inspiration and insight needed to do good work. Mark’s arguments imply that if the Nash equilibrium level becomes sufficiently high – a large number of proposals submitted – the net cost to the system can exceed the benefit of the research.

Of course, the system has not yet evolved to a full Nash Equilibrium, and some professors play the system by submitting a large number of proposals. A colleague cited an instance in which a faculty member once submitted 33 proposals in a single year! Such egregious examples are unusual but probably not rare. Quantity does not always have its rewards. I know of several instances where faculty members have written eight or ten proposals before achieving a single success. (In once case, 17 submissions!) Clearly, the Nash game is taking its toll.

The examples cited above are not far from the norm. Stuart Rojstaczer, who entered Duke at the end of the Golden Age, mentions that he had six
proposals of 25 accepted during his first seven years as a faculty member in Hydrology. In so doing, Rojstaczer claims to have spent about 25 percent of his time writing proposals, which means that he spent almost 20% of his time writing failed proposals. Rojstaczer commented that this rate of success was not bad. I agree. My own rate of success (for a fewer number of proposals) may have been somewhat better than that during the 1970s and 80s, but that was during the Golden Age. The problem is that Rojstaczer began his professorial career at the end of the Golden Age, so that the success rate for proposals was already falling during his first seven years. (Since publishing his book, Professor Rojstaczer has subsequently left academia.) The rate of success is much worse now; I know of grant opportunities in which the acceptance rate was under 10%, even well under 10%. This ratio is typical, rather than anomalous. In the words of a young colleague, one can easily spend all one’s time writing proposals. Which is equivalent to say that one can spend most of one’s time writing failed proposals.

Such failure rates do not matter so much if one is entering a baking contest or even applying for a Fulbright or Rhodes Scholarship. One can carry on with one’s life and work after enduring a rejection in such non-essential competitions. But the game is highly destructive to a young faculty member whose very career and personal life hang precariously on the success rate of his or her proposal submissions.

Young faculty members are suffering not only because of the system itself but because of its abuses by a few who have become successful at the Nash game, such as the person who submitted 33 proposals. I cite some of these abuses. In one case I personally encountered, someone in another department asked a fellow faculty member permission to list that person as a co-PI on a proposal the day before it was submitted, on the condition that the person need do no work on the project. Presumably his name would bulk up the proposal with the reputation of an urban economist. A colleague in another institution cited a proposal he was on that had 8 PIs, 2 Federal Cooperatives, 8 collaborators, and one Federal fiscal representative. His impression was that the roster was artificial, bulked up to convey the interdisciplinary nature of the proposal. My correspondent expressed doubt that a coherent study could be conducted when the participants were scattered all over the West. Here the Nash game was being played with face cards, getting as many recognizable names as possible on the proposal to demonstrate expertise.
I have received a number of such anecdotal accounts from colleagues, some of which could be very amusing were their implications not so sad. Some questioned the integrity of the system. Although such abuses could be laid at the door of human weakness, the drivers for such behavior are institutional. One institution at fault is, of course, the university, which fosters the idea that the more proposals one submits the more money one gets…. at least until Nash equilibrium catches up. As I point out in BAMS, the scramble for big bucks leaves the researcher less time to attend to students and requires that the faculty members trust their graduate students and post docs to do the work. The person who has the most experience is therefore relegated to doing the least amount of research, while the student, both undergraduate and graduate, loses.

Pressure to bring in the bucks seems to be trumping scholarship. One professor at another institution received an award for bringing in more funding than anyone else. I regard my own department’s attitude as very enlightened in its regard for scholarship.) Yet at a recent college-wide gathering, faculty members who brought in over a half million dollars were asked to stand up and receive applause from the rest of us. This achievement is, of course, worthy of some praise but it reveals a certain attitude toward the importance of funding. Such displays of commercialism exemplify what one colleague of mine calls the ‘selling culture’ in which university faculty are being turned into what Rojstaczer calls ‘managers of scholars’.

Although some have drawn an unfavorable comparison between the process of obtaining funding in science and the way corporate business is conducted, the two magisteria differ in one fundamental way. As an illustration of this difference, a dean was heard to boast of how much money his college had ‘generated’. Yet, universities do not generate money, they consume it, as my colleague Mark Roulston pointed out. In this respect, university research differs markedly from corporate business, which places emphasis on profits (output), whereas the university is less interested in profits (papers, patents, etc.) than in revenue (grant money). This attitude leads to what I call userless science.

Besides bringing in the bucks, another important criterion in academia for tenure and promotion is the number of PhDs one produces. I have heard of one institution that requires a faculty member to have at least eight graduate students to get promoted. Emphasis on doctorate production only
exacerbates the funding problem by creating a growing population of grant applicants. In the Golden Age this system helped to fuel the impressive expansion of all sciences. In the post Golden Age it contributes to a population explosion, which is leading to a desperate scramble for resources.

Yet, quantity does not necessarily equate to quality. The scramble for resources may be yielding a decrease in the quality of our scientists as well as of the proposals. Since a lowering of standards for the PhD undoubtedly makes academics uncomfortable, a solution to the problem has been to admit more and more foreign students as the numbers of native born Americans coming into science are diminishing. Phillip Greenspun, a physicist who teaches acoustics at MIT, wrote a web-based article (February 2006) aimed primarily at women students. Greenspun questioned why anyone would want to become academics because (I paraphrase here) working conditions for young faculty are so difficult and so insecure that, for most Americans, it is not worth it to endure a period, first as a post doc and then in academia waiting for tenure, only to find that, having been denied [it], one is too old to do anything else. Greenspun goes on to state that (and I again quote approximately) ‘fewer and fewer Americans and more and more foreign male students are applying to PhD programs’. Of course, nothing is wrong with having foreign students, but Greenspun strikes a chord with much of what I am trying to say here.

Funding agencies themselves encourage the Nash game. Someone explained to me that such low rates of awards granted by funding agencies are a way to increase their funding in Congress. A field littered with failed proposals makes a good case for need. I received several anguished letters from scientists at national institutions, confessing discouragement and bemoaning the fact that they too were required to compete with the universities and to raise huge sums of money for research; numbers such as $250 or $300 K per year were actually mentioned by one member of a government lab (referring also to his and another government institution) as being a minimum requirement in those institutions for the practicing scientist to be retained. This person also cited the uncontrolled growth of proposals in the face of constrained funding being responsible for the present situation, which he claims is a ‘recipe for disaster’. He referred to the ‘waste in human time and energy….while acceptance rates are ludicrously low… (the) university or lab forces you to do it anyway’.
While some may praise a system of scientific (as opposed to social) Darwinism to help select the best proposals, the system of proposal evaluation is far from flawless. The process of evaluation by reviewers and agency panels is necessarily hasty and fraught with personal prejudices and institution politics. Moreover, reviewers can be taken in by glib writing and overstated claims. Birnbaum attributes this decline in review quality to the mushrooming size of multi-investigator grants. One of my correspondents cited a colleague who boasted that he could get any proposal accepted on any subject whether he knew anything about it or not. Reviewers, for the most part, do the best they can under the circumstances. Yet even under the best of circumstances, it is impossible to be sure that the ten proposals selected for funding from one hundred submissions really represent the best science or are all better than the 90 rejected proposals. Lovelock is especially critical of the peer review system.

The problem also arises at middle levels in the funding agency with the program manager. Program managers are also removed from the cutting edge of research and so tend to adhere to routine ideas currently favored by society or congress or the press,— ‘safe’ projects in the words of Stuart Rojstaczer. Program managers may or may not be interested or knowledgeable about the work they support and may be justifiably more concerned with their own careers. In many cases these managers move on to bigger and better pastures, including universities, using their influence and more photogenic successes as leverage, just as in academia, where some professors use their ability to wheedle funds as levers to higher salaries in their institutions or better positions in competing ones. Thus, grant money becomes a ticket to promotion and higher income within the agencies.

I don’t wish to impugn all program managers. I’m sure that there are many competent and concerned individuals in those positions. Yet, their presence implies that the science is being generated from top down through a level of bureaucracy even farther removed from the cutting edge in science than the overburdened PIs. This top-down science is programmed by this bureaucracy to suit political and administrative ends. (One wag said that meteorology is no longer a science but a collection of programs.) Program managers favor large, splashy research projects with plenty of crowd appeal. I’ve heard it said that large programs are necessary because today’s problems require a cartel of scientists to solve important problems. I don’t think any scientific historian would agree that Nature was any easier to probe in the days of Newton or even Rossby than it is today.
In fact, large programs are often counter productive. Large grants tend to drive out smaller ones with good ideas, as noted by one of my correspondents. It is probably true that the larger the grant the smaller the number of refereed papers per funding dollar published in reputable journals. (I’ve had agreement on this contention from various people I have talked with, including a friend who is a civil engineer in another institution. This person acknowledged the inverse relationship between grant size and productivity and admitted that data on the number of journal papers per dollar was used for purposes of tenure and promotion in his department. I was unable to access this information -- for good reasons, however.) Much of the output from large grants today is in the form of public relations frippery, (what Birnbaum calls ‘social engineering), such as fancy web sites that impress but which no one actually uses and which no one bothers to evaluate. As I have just said, neither the university nor funding agency show much intrinsic interest in these ‘products’, except as beans to be counted.

Program managers favor the Request for Proposal (RFP) mode of advertising the availability of funding --‘mission based research’ in Birnbaum’s words. I call it science for hire. In an essay extracted from a physics journal, one I read several years ago, the RFP was referred to as being detrimental to scientific research in that it can inhibit good research rather than encourage it. When an RFP is published, it tends to attract proposals from many groups who are simply looking to maintain their own programs and who then try their skill at convincing the reviewers that these programs exactly fit the conditions of the RFP. The outpouring of proposals engendered by the RFP, that epitome of top down science, tends to swamp the smaller numbers of proposals submitted by scientists who had been seriously involved in the subject matter for many years. Moreover, most RFPs read like legal documents, requiring considerable effort in simply meeting a range of picayune criteria. I was once almost on a proposal in watershed hydrology that was never submitted because the RFP required that a sociologist be part of the investigation and such an individual could not be found. This is top down science at its worst. Once upon a time during the Golden Age, I could get a good idea, submit a proposal to an agency of choice without any specific RFP and hope that the agency would be sufficiently interested to fund it. No longer!

It might be a useful expedient for academic deans and department heads to exhort their faculty to try harder to achieve greater success in grant writing,
perhaps suggesting seminars on grant writing, various interpersonal devices such as personal contact with program managers, etc. But such activities, besides wasting yet more precious time for research, are doomed to failure. They only beg the point. The Nash game prescribes more and more effort for the same return. Realizing that, institutions and individuals turn to various forms of subterfuge, some of which border on the illegal. Stuart Rojstaczer mentions some of these scandals in his book.

What is to be done? Too often the cure being prescribed is an increase in funds, a nostalgic call for a return to the Golden Age. Yet this wish is simply a fantasy and may not even be a good thing. If the situation continues as I’ve described it, the strains on university faculty and university finances (and university ethics) will increase. Clark Kerr worries about the deleterious effects of these strains on small, liberal arts universities, where, as he puts it, much of academic diversity still remains. He asserted (in 2001) that productivity gains are not keeping up with costs. Small colleges which, in the past emphasized teaching and student participation, have upgraded their status to that of University and have begun to encourage or perhaps pressure their faculty into the proposal writing game. Yet, one wonders if this practice can be sustained or if small colleges playing the Nash game will face the same dilemma in their academic programs that some have faced in their football programs. Welch Suggs, writing in the Chronicle of Higher Education (2003), describes how an increasing expense of maintaining competitive football programs has forced some small colleges to eliminate football as a sport.

Instead of calling for increases in national funding, a more realistic approach within the university would be to foster an awareness of the present situation and decide how to adjust to a changing social and financial environment without damaging their faculty. Birnbaum claims that universities may actually lose money in on federally supported research where overhead rates are capped (as with NSF), the rationale being that they still gain prestige and thereby attract good faculty. If this is so, universities might still be able to extricate themselves from the present vicious circle.

What are some possible remedies for the present ills as I have described them? First, we need to recognize that the Golden Age is gone....Gone for Good in the words of Stuart Rojstaczer. Mark Roulston suggested restricting proposal submissions to one per year from a given faculty member. I’m not
sure this very reasonable solution could be practically implemented in view of the inevitable resistance to such an imposed constraint.

I suggested in BAMS that funding agencies could provide the faculty member a nominal sum, say, 10 or $20,000 each year, while the university could furnish one graduate student for the duration of that student’s academic program. These grants would be subject to a short proposal to the university and to the funding agency. The proposal, however, would not be in competition with others but evaluated on the basis as to whether the research described therein appears to be worthwhile. Additional funding for larger programs would still be available by existing means --- subject to a competitive proposal --- but the research would be initiated from the faculty member not the funding agency. In other words, bottom up not top down science. Tenure and promotion would be based primarily on teaching and on research, but the latter criterion would rest not on bulk numbers but on a few of the candidate’s best papers as well as on outside reviews by colleagues.

Birnbaum similarly emphasizes quality over quantity in the number of papers evaluated for tenure and promotion. By providing a low level of research funding and a graduate student, and a tenure and promotion scheme based primarily on quality of research and teaching, rather than on quantity and on funding level, many faculty members would be willing to eschew writing a flood of proposals merely to keep themselves afloat. They would have time to think and to generate better science. The number of journal papers would diminish, the volume of proposals would be reduced, the quality of both would increase, and everyone would have more time for serious research and for students, the other big losers besides young faculty in the Nash game.

A cost analysis should be done on this suggestion, but I suspect that, if implemented, funding agencies would save some otherwise wasted time and money. Yet, at present, university and funding agency remain locked in a pattern of mutually dependent addition, the latter depending on the university for its raison d’etre and the university depending on the money its receives to continue its growth or even to survive. Only awareness, and good intentions will help break this pattern; Birnbaum suggests collaboration among universities. My own perception is that universities must remove funding level as a criteria in tenure and promotion and perhaps to lower the overhead rate on small grants.
The situation I have described was articulated to me in a message from a friend and colleague, a senior faculty member, who has been highly successful in funding his own excellent research. I think this excerpt typifies the intensity and content expressed in the many messages I received following the publication in BAMS.

‘I find myself actually outraged by the things I keep hearing, not just lately but for the past ten years or more, about the explicit and implicit expectations put on new faculty to bring in money. At (institution name deleted) I heard they tell [the faculty] explicitly: “you are expected to bring in $300K or more”. Some other places, similar story, but even larger amounts. This should raise the hackles of anyone who remembers what an intellectual environment is like and how poisonous the introduction of crass money criteria into such an environment is: I don’t care what excuses are offered--- it’s just wrong, wrong wrong. Science, at its best, is a life of relatively quiet contemplation………………Now, without any dialog about the nature of science or a scientist, these arbitrary fund-raising criteria have been applied willy-nilly across the entire academic landscape. …………The more I think about it, the more I wonder…could we have chosen a better way to hamstring young faculty, full of creative energy, than to turn them into businessmen? And hold up our field’s millionaires, as exemplars, rather than the thoughtful academics of yore?’

In summary, the present situation is wasteful, counter productive, stifling of creativity and joy in research, and especially stressful on young faculty members. It has come about because (1) national funding has decreased considerably over the years since the end of the Golden Age, especially in terms of dollars per scientist; (2) more PhD’s are on the market who are writing grants; (3) universities continue to grow as they did during the cold war, that is during the Golden Age; and (4) there is increased competition between universities and scientists, resulting in a hapless entrapment in Nash equilibrium.

Perhaps we can’t all be like Einstein who lamented that he felt guilty taking money for doing physics. But one can surely search for a balance between reward and scholarship.
List of References:


