Commentary

In My Opinion

The Decline of Science

After a recent panel on "gender aspects of the science wars", someone persistently asked why such seemingly bad scholarship had attracted a following. I had no answer. Since then, the review (which appears in this issue) by Sandro Graffi of Russo's *The Forgotten Revolution* has given me new insight into this, and related, issues.

It is certainly important for the scientific community to be receptive to criticism, particularly regarding ethical issues. But I am concerned about those critiques which are based on the proposition that science itself is nothing more than a cultural construct—one that is white, male, and western. How, one wonders, can this be taken seriously? Is there any credible evidence that the force of gravitational attraction varies with gender or ethnicity? That electromagnetic phenomena in Africa are not in agreement with Maxwell's equations? Yet, Gross and Levitt's *Higher Superstition*[1], the hoax associated with the "Sokal affair"[1], and Bricmont and Sokal's recent *Impostures Intellectuelles*[2] leave little doubt that even some of the most preposterous cultural critiques have found a receptive audience. That this construct is suffused with male bias is a frequent theme in gender studies,[3] despite the lack of evidence that women prove different theorems. Indeed, those of us who participate in science and mathematics see so much evidence for its universal validity that we may forget that this is not obvious to others. However, among the general public a belief in the validity of science is being threatened by the appeal of alternative medicine, psychics, paranormal phenomena, etc.

Inadequacies of our educational system play a role. Consider, for example, the practice of trying to make physics attractive by eliminating even high school mathematics, depriving students of any possibility of seeing the logical development of physical principles. If, in addition, these students do not perform any simple experiments which illustrate basic phenomena, such as gravity, water pressure, magnetism, and the efficiency of pulleys, then listening to a (usually male) teacher lecture on such unfamiliar phenomena as black holes, relativity, strings, quarks, etc., is not likely to seem different from listening to a Druid priest. All connection with reality has been lost. With different motives, animal rights activists assert that the traditional dissection of frogs in biology can be replaced by computer simulations. But such simulations *are* unequivocally human constructs. Children are accustomed to using computer games which simulate a fantasy world of dungeons and dragons. A computer model may illustrate the location of the heart relative to the lungs, but it cannot teach students that real frogs have certain features in common, unaffected by whether or not those organs are included in a computer program. The role of dissection and animal experiments in education is a complex issue. But some experiments are necessary if students are to understand the notions of external world, "objective" reality, universality, scientific methodology, and "truth".

How can the public be expected to believe in scientific "truth" when supposedly "expert" witnesses present, under oath in court, absurdly divergent views as "scientific fact"? What is the public to make of the two video reconstructions with *different* trajectories that government bodies have presented of the 1996 crash of TWA flight 800? If this is truly a case in which an inverse problem does not have a unique solution, that should be admitted; if not, applied mathematicians with access to the data should set the record straight. Over twenty-five years ago almost half those surveyed believed that the moon landing they watched on television actually took place in Hollywood. Today, the credibility of science and engineering is reaching depths not experienced since the Dark Ages. Ethical responsibility alone should confine contradictory testimony to cases of bona fide ambiguity. Failure to do so undermines the very heart of science.

Experimental evidence can be used to counter the argument that science is a cultural construct. But what about mathematics? Despite its "unreasonable effectiveness" (as Eugene Wigner put it), mathematics *is* a human construct. We make definitions and examine their consequences. There is even a subjective human aspect to mathematical proof. Most number theorists were convinced of the essential validity of the Wiles strategy for proving Fermat's theorem before the details were available and even after a flaw was discovered. I would argue that the universality of mathematical truth is as valid and "unreasonable" a phenomenon as its widespread applicability. In a seminar, one previously math-phobic student wrote, "…the answers to *all* my questions are deep inside me. The problem isn't how to 'find' a solution, it's how to 'get it out' from within myself." Although this student had experienced the universal validity of mathematical truth, her words have been interpreted as supporting the notion[4] that "there are as many right answers as there are people listening to their own inner voices," i.e., that mathematical truth is a subjective personal construct. It may be tempting to dismiss this view as absurd. But it is a symptom of a serious and pervasive problem that merits our attention.

—Mary Beth Ruskai
Associate Editor

[1] *For reviews and commentary, see the articles by Sullivan and Harrell in the October 1996* Notices *or the Web site* http://www.physics.nyu.edu/faculty/sokal/index.html.

[2] *Planned for review in a future issue of the* Notices.

[3] *See, e.g., Allyn Jackson's article in the July/August 1989* Notices *or A. Koblitz's review of Claudia Henrion's* Women of Mathematics: The Addition of Difference *in the current* Notices.

[4] *M. F. Belenky et al.,* Epistemological development and the politics of talk in family life, *J. of Education* ***167*** *(1985), 9–27; see also M. F. Belenky, B. M. Clinchy, N. R. Goldberger, and J. Mattuck,* Women's ways of knowing, *Basic Books, 1986.*

Letters to the Editor

Comments on Sadosky's "Forum"

I was pleased to open my December issue of the *Notices* and see the article "On Issues of Immigration and Employment for Mathematicians" by Cora Sadosky in the "Forum". After a long silence from the *Notices* on the complex issues surrounding the current job market, I had hoped that Geoff Davis's excellent and thought-provoking article in the November issue marked the beginning of the *Notices* as a forum for thoughtful and frank discussion of these questions within the math community. I was particularly eager to hear considered arguments against changing current immigration law as it applies to scientists. I know many of the mathematicians advocating such changes and am familiar with their arguments, but have not heard much response to these arguments.

What I read was extremely disheartening. Sadosky speaks vaguely of "a few mathematicians" and makes brief reference to articles in the *Wall Street Journal* and the *Boston Globe* in which several mathematicians were interviewed and asserted that specific changes in immigration law as it pertains to scientists were largely responsible for the current job market crisis. Clearly she means to be responding to these people and these positions. Yet never in the entire article does she respond to any position I found remotely recognizable as being held by anyone I have heard speak on the subject. No mention is made of the recent changes in immigration law for scientists and engineers, nor are any arguments offered in favor of these changes. Instead, she creates a straw man of ridiculous and genuinely evil positions to argue against as a surrogate.

Those questioning the current status quo are compared to LePen's FNP and Nazis and caricatured by statements like "Now some would make foreigners rather than women the scapegoats." We are told that "surely mathematicians know better than to mistake correlation with causality in their efforts to deal with the current job crisis"; that "banning immigrant mathematicians" would create problems; that "asserting supposed birth rights of the U.S.-born in divisive chauvinistic quests, or with anti-immigration innuendo" is the wrong approach; and she concludes, "Let us work to eliminate the stereotype of foreigners as smart but unable to teach simply because they have accents." These assertions are inarguable, but if these are your only arguments against someone's position, you have in effect accused your opponent of racism, xenophobia, and the spreading of dangerous stereotypes.

Such accusations would require meticulous substantiation under any circumstances, and in an article solicited by the *Notices* in this close-knit and generally collegial community of mathematicians the standards should be far higher. This is all the more true since this is for the *Notices* a single isolated entry into the debate, and there appears to be no opportunity for those accused to defend themselves or state their positions. There was not a shred of substantiation for this smear. Even LePen was allowed to be damned by his own slogan rather than by having someone else's words put in his mouth, but no quotes or evidence of any kind were offered to condemn the accused racists and xenophobes.

Beyond being unsubstantiated, the accusations are also false: while I am sure somewhere in the world there are mathematicians who are racist, as in any profession, I have never heard any mathematician ever express any of the offensive sentiments Sadosky argues against.

I do not know what the *Notices*'s editorial policy is, but I expect and hope that making incendiary and unsubstantiated accusations against fellow mathematicians violates it and that the publication of this article is a mere oversight. I think an apology at least from the editorial board is called for. I also think that in an effort to undo some of the damage done here and to permit a productive discussion of what the *Notices* seems to feel is an important issue for mathematicians, it would be constructive to publish two pieces, making responsible and cogent cases on both sides of the immigration debate.

I should add that as disturbed as I was by the accusations of racism that seemed to me to be the centerpiece of Sadosky's argument, I felt there were also a number of valid points. In particular, she offered a number of plausible explanations for the job crisis, although I would have liked to see a real case made based on hard data that these can explain it. She also makes the important and often overlooked point that the place in all of this where mathematicians can probably have the most positive effect is in supporting the wealth of young research mathematicians who find themselves at places where little research has gone on. This does not, however, free us from the responsibility of considering all aspects of the crisis and all coherent arguments (even those which seem frightening) in the critical, open-minded, thoughtful, and collegial way that I believe characterizes our discipline.

Stephen Sawin
Fairfield University

(Received December 15, 1997,
revised January 20, 1998)

Editor's Note: In the penultimate paragraph of his letter, Professor Sawin suggests that the *Notices* should publish two pieces representing both sides of the immigration debate. In fact, last year's Editorial Board attempted to do just that. Professor Sadosky's piece was to have been one of a pair of articles on immigration issues. The other author (whose offer to write on this topic led the Board to solicit the contrasting view from Professor Sadosky) produced a piece that was twice as long as the agreed-upon word limit and refused to shorten the piece. Out of fairness to Professor Sadosky, who had abided by the word limit, the Board decided to publish her piece alone. This year's *Notices* Editorial Board continues to look for ways to present various sides of the issues concerning immigration and employment.

Sadosky Replies

Professor Sawin is angry with my "Forum" piece on issues of immigration and employment. He expected an

article countering arguments advocating restrictions to scientific immigration. Instead, in what I thought was going to be one of a pair of position pieces, I concentrated on the danger of substituting anti-immigrant rhetoric for the serious consideration of the unemployment crisis faced by mathematicians.

I mentioned Le Pen's slogan not to counter specific arguments against scientific immigration but to warn against the recurrence of foreigners as scapegoats. After WWI France had a great need of manpower sated by a flow of refugees; during the economic crisis of the thirties some professional associations reacted with xenophobia instead of confronting the causes of unemployment. Such attitudes are not uniquely French. My contention is that xenophobic reactions divert attention from the real causes of real problems.

Professor Sawin complains that my case on possible causes of the job crisis is not made because it is "not based on hard data." That is not so. The strict word limit of my article pushed the supporting material (all easily accessible to the *Notices* readership) to the references. For instance, I mentioned the decrease in calculus enrollment of more than 100,000 students in five years, which is quite hard data and comes from a survey published in the September 1997 *Notices*. The article "Changes in Mathematics Faculty Composition: Fall 1990 to Fall 1996" in the November 1997 *Notices* summarizes extensive data from past AMS-IMS-MAA Surveys: in the five-year period covered, total tenure-track faculty declined by 1,400 despite about 2,600 retirements—how can that be blamed on immigration?

On the other hand, the newspaper accounts to which I also referred reported efforts to lobby Congress against scientific immigration but did not present the substantiating arguments. While such arguments may be familiar to Professor Sawin, to my knowledge they have yet to appear in print. Making available through the *Notices* the text of the statement sent to Congress would facilitate an informed debate on their substance. If Professor Sawin knows of cogent arguments in support of restrictive immigration laws and feels so strongly that I overlooked or misrepresented them, why did he not use some of his letter to state them? How long are the purported powerful anti-immigrant arguments to remain mere rumors?

Professor Sawin calls in his letter for "considering all aspects of the [job] crisis and all coherent arguments (even those which seem frightening) in the critical, open-minded, thoughtful, and collegial way" that he believes "characterizes our discipline." In the same letter he calls for an apology from the *Notices* Editorial Board for having solicited and published my "incendiary" article. It is hard to reconcile both calls.

Cora Sadosky
Howard University

(Received February 11, 1998)

Hold Departments Accountable

Cora Sadosky's recent article on immigration makes an important point that bears repeating: immigration is *not* the real issue facing the mathematics community. Recent changes in immigration patterns are but one of a number of factors that have affected the balance of supply and demand for Ph.D.s. The real issue we must address as a community is that of how to adapt to the broad range of changes that are taking place in the environment in which we operate.

Sadosky's article evokes images of an idealized job market in which Ph.D.s engage in a Darwinian struggle for employment, a market in which survival is based purely on merit. While the evolutionary metaphor suggests that the ferocious competition amongst job-seeking doctorates leads to improvements in mathematics, there is a fallacy in this notion. It is *individuals* who feel and must respond to the selective pressures, not departments. The unfortunate reality is that many individuals are adapting to the present climate by leaving mathematics altogether. Departments, in contrast, have little incentive to change their ways.

A change in perspective is in order: consider what might happen if we were to expand the arena of competition to include departments. Suppose that not only would doctorates compete for jobs, their departments would also compete to ensure that their graduates obtained the best possible positions. Innovative initiatives such as the Preparing Future Faculty program and NSF's new VIGRE grants provide the means for departments to increase their graduates' chances of finding meaningful employment. Competitive pressures would give them the motivation to participate in these programs or to explore their own avenues of change. Some departments might add breadth to their doctoral curricula; others might create courses of study to prepare students for new, nonacademic careers; still others might hone teacher-training programs. The result would be an expansion of opportunities for new doctorates rather than the present contraction and retrenchment.

An annual departmental report card providing such information as placement rates for each department's recent graduates, attrition rates, and average times to degree would provide departments with incentives to adapt. Report cards would steer prospective students to the departments best suited to fulfilling their career goals. To attract the best students, departments would not only have to strive for excellence in research but also for excellence in their preparation of their students for their careers, be they inside or outside of academia. The Web site `http://www.phds.org/ratings/` illustrates how this data can be used.

The level of accountability embodied in departmental report cards is common in other professions. Detailed information on placement rates is readily available for business and law schools. Trials of hospital report cards have been so successful that the *Journal of the American Medical Association* has recently called for the provision of report cards for both hospitals and individual doctors. A March 3 workshop I am organizing with the AAAS Commission on Professionals in Science and Technology will lay the groundwork for the gathering of outcome data for doctorates in all of the sciences.

Holding ourselves to a higher standard through greater accountability is a first step towards alleviating the

current labor market problems and preventing future ones. Increased accountability provides incentives to adapt without making any distinctions regarding nationality of doctorates and without mandating any cutbacks in enrollments. Prospective graduate students must invest years of their lives in a difficult course of study. These students are of tremendous benefit to faculty members: they help teach our classes and help us with our research. For the graduate students, however, the investment is a risky one. There are no guarantees that they will be able to fulfill their career goals upon obtaining a degree. It is the graduate student and not the department who ultimately pays the price for inappropriate training. Do we owe our students anything less than full information?

Geoff Davis
Dartmouth College

(Received January 20, 1998)

Vershik Work Needs Acknowledgement

I was astonished not to find in the paper "Random Combinatorial Structures and Prime Factorization" by R. Arratia, A. D. Barbour, S. Tavare (*Notices*, September 1997, 903–910) any reference to the works by A. M. Vershik (and his school) [i–iv] quoted below:

[i] A. Vershik and A. Schmidt, *Symmetric groups of higher degree*, Soviet Math. Dokl. **206** (1972), 269–272.

[ii] A. Vershik and A. Schmidt, *Limit measures that arise in the asymptotics of symmetric groups, I, II*, Theoret. Veroyatn. i Prim. **22** (1977), 72–88; **23** (1978), 42–54.

[iii] A. Vershik, *Asymptotic distribution of decompositions of natural numbers into prime divisors*, Soviet Math. Dokl. **289** (1986), 269–272.

[iv] A. Vershik, *Asymptotic combinatorics and algebraic analysis*, Proceedings Internat. Congr. Math., Zürich, Switzerland, 1994, vol. 2, Birkhäuser-Verlag, Basel, Switzerland, 1995, pp. 1383–1394.

The authors are wrong in attributing on pages 903–5 the results on the asymptotic distributions of normalized length of cycles of random permutations to [29, 7, 9] (their references) and others. The strongest and deepest results in this direction were announced in 1972 in [i], and the proof was published in two papers [ii].

I know that Vershik's ideas in this area were very fresh and were accepted by many mathematicians with enthusiasm and used by many others. In a sense Vershik has developed the pioneering ideas of the 1940s of a Russian mathematician, V. Goncharoff (who also is not quoted in the *Notices* article). Vershik thus called the crucial functional equation which he obtained "the Dickman-Goncharov equation".

Later Vershik found the link between that problem and the statistics of the prime factorization [iv]. The joint distribution of the prime divisors was found in the previous papers (unknown to Vershik), but the coincidence of the two statistics was mentioned first not in [1] as the authors have claimed but in [iii]. In his invited talk at ICM 94 in Zürich [iv] Vershik gave a survey on this and related topics and further references.

The results and papers of Vershik were undoubtedly known to the authors of the *Notices* paper.

The present situation is not the first case of the—I would say irresponsible—attitude of Western (especially American) mathematicians toward their helpless Russian colleagues. I have the impression that a segment of the Western mathematical community accepts the attitude of those mathematicians who quote results and ideas only of people who might invite them to their university (institution, conference, Congress, Prize) or might become the reviewers of their works for publication or might be useful in job hunting.

Since Russians have no money to pay them and are unlikely reviewers, it is safe not to quote Russians at all. In other domains of science and technology there exists a legal system of punishment for thieves (patents systems, courts, ...). In mathematics it is too easy to make a reputation just by repeating classical Russian results and by publicizing them under wrong names. I might list dozens of cases like the one provided by the disastrous *Notices* article.

People are still stealing the results of, say, my teachers, like Andronov, Kolmogorov, Pontryagin, Petrovskii, as well as those of my students (being, it seems, afraid to steal my own results, however).

I think that the main reason for this attitude is the tradition of an unfortunate tolerance of the mathematical community, especially in the USA, toward the unethical conduct of its members. If everyone would loudly protest whenever he sees the wrong attribution (as I am doing now), people would find it profitable to quote the correct references rather than to propagate unfair publicity.

V. Arnold
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and Université de Paris, Dauphine

(Received January 11,1998)

Comments on Arnold's Letter

I am grateful to Professor V. I. Arnold for his observation about the article of my old colleagues R. Arratia, A. Barbour, and S. Tavaré. Indeed, it is too bad that the authors forgot to mention the papers by V. Goncharoff and by my coauthors and me that have contained serious results in the area, especially papers [ii, iii], of which they definitely had knowledge.

But I am not in agreement with the sharp criticisms and terms that Professor Arnold used. My impression is that unfortunately such a gap in memory is common in different communities and is an ordinary lapse that occurs very often with many of us worldwide. On the other hand, it is true that the situation with authors from Russia is special because of the former long period of an absence of contacts.

Anatoly Vershik
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Academy of Science,
St. Petersburg branch

(Received February 1, 1998)

Reply to Arnold Letter

In response to Arnold's letter, we wish to make the following points. First,

the *Notices* articles are intended as an introduction to a topic rather than as reviews, and editorial policy is that references should be designed as pointers to the immediately relevant literature and not as exhaustive lists. We mention this explicitly in the text. Our theme is the unified view deriving from the conditioning property and the logarithmic asymptotics; Vershik's approach is quite different and as such is not central to our theme.

To Arnold's specific points. The Poisson–Dirichlet approximation for the large prime factors we attribute, correctly, to Billingsley (1972). For combinatorial structures, our interest is precisely in the fact that the Poisson–Dirichlet approximation holds for a huge class of structures and not just for isolated instances, for which we cite the appropriate reference, Hansen (1994). It would have been polite to have mentioned Vershik's theorem for uniformly distributed random permutations at this juncture, but hardly essential. The fact that this limiting structure is common to primes and combinatorial structures we attribute not to ourselves, but to Knuth and Trabb Pardo (1976), a reference ten years earlier than that quoted by Arnold.

There seems little point in discussing the remainder of Arnold's letter. We should, however, mention that Vershik has been an honoured guest, not only in Zürich, but also at the University of Southern California, as have innumerable other Russian mathematicians. Also, that incorrect attribution does not work exclusively against Russians, as evidenced by continuing reference to that most useful and widely quoted probability inequality as Chebyshev's.

Richard Arratia, Andrew Barbour, Simon Tavaré
University of Southern California, University of Zürich, University of Southern California, respectively

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How to Teach Limits and Continuity

In recent issues of the *Notices* (May 1997, pp. 559–563; September 1997, pp. 893 and 932–934; and January 1998, p. 6) there are interesting letters of David Mumford, Saunders Mac Lane, Leonard Gillman, and Peter D. Lax about ways of teaching limits, continuity, and uniform continuity. But I believe that the authors do not point their fingers at the root of the difficulty and how to overcome it.

The difficulty, I believe, comes from the fact that quantifiers constitute an advanced linguistic tool whose goal is to avoid the introduction of ad hoc names and symbols. The students whom we teach are not sufficiently used to this tool, and hence they find it difficult to overcome this linguistic barrier.

As always, the solution is to explain to the students what we have in mind. Thus when we claim that $a_n \to a$, we mean that we have a function $N(\epsilon)$ from positive reals to positive integers, such that

$$(*) \quad n > N(\epsilon) \text{ implies } |a_n - a| < \epsilon.$$

When we say that f is continuous, we mean that we have a function $\delta(x, \epsilon)$, where x is any element in the domain of f and ϵ is any positive real, such that

$$(**) \quad |x - y| < \delta(x, \epsilon) \text{ implies } |f(x) - f(y)| < \epsilon.$$

After all, the only way to prove $a_n \to a$ or to prove that f is continuous is to build the appropriate functions $N(\epsilon)$ or $\delta(x, \epsilon)$ respectively and to prove (*) or (**) for those functions. And I believe that a student who is not able to prove that, say, $1/n^{1/2} \to 0$ or that $x^{1/2}$ is continuous does not understand convergence or continuity. On the other hand, a student who is able to prove a few theorems of that kind does understand those concepts.

One final remark: A naive interpretation of quantifiers accepts Platonism or physicalism, namely, a Platonic or a physical existence of all the elements of the domains to which the quantifiers are referring. Operations such as $N(\epsilon)$ and $\delta(x, \epsilon)$, which are called Skolem functions, justify the use of quantifiers without forcing us to accept Platonism or physicalism. Indeed, we construct those operations in our brains, but we do not (and cannot) build all the elements of their domains or ranges, since, as a rule, those sets are too large. Thus quantifiers are mere abbreviations by means of which we can avoid (but not always) naming or denoting some Skolem functions.

Jan Mycielski
University of Colorado

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Keep Young Scholars Programs Running

The NSF's decision to cut the funding for the Young Scholars program ("The Demise of the Young Scholars Program", *Notices*, March 1998) is a tragic mistake. The total cost of these programs, $10 million a year, is relatively small, yet the potential benefit to society is enormous. The argument that "these students are already highly talented and motivated and such programs simply add to their advantages" badly misses the mark. In fact, many of these youngsters are intellectually and socially isolated in their school environments, and it is by no means certain that they will, without help, fulfill their potential. These programs affirm that what they are interested in is worthwhile and valued and not merely "weird". My daughter, Lenore, who is a mathematician, was in David Kelly's fine program at Hampshire, and I am by no means certain that she would be in our profession if it weren't for that experience.

If NSF funding cannot be restored, other sources should be pursued. (Bill Gates, are you listening?) Also, those of us who can afford to do so should contemplate making donations to those programs which are still in existence. It is money well spent!

Robert Cowen
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