Letters to the Editor

Hedgehogs and Foxes, Not Birds and Frogs

Freeman Dyson's Einstein Lecture (*Notices*, February 2009) is a beautiful meditation on the distinction between two types of mathematical thinkers. But in calling them "birds" and "frogs", Dyson contravenes a metaphor that predates his by several thousand years. The common terminology is that referenced by the late philosopher Sir Isaiah Berlin in his essay "The Fox and the hedgehog", which comes from the words of the ancient Greek poet Archilocus: "The fox knows many things, but the hedgehog knows one big thing".

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Zoological Metaphors for Mathematicians

Freeman Dyson's impressive "Birds and frogs" (*Notices*, February 2009) reminds me of another zoological typology, proposed by Francis Bacon in 1620, in his *Novum Organum* (Aphorism I, 95): ants, spiders, and bees:

"Empiricists are like ants, who only collect things and make use of them. Rationalists are like spiders, who weave webs out of their own bodies. But the bee has a middle policy; it extracts material from the flowers of the gardens and meadows, and digests and transforms it by its own powers. The genuine task of philosophy is much the same. It does not depend on or mainly on the powers of the mind; nor does it deposit the raw materials supplied by natural history and mechanical observations in the memory just as they are, but as they have been worked over and transformed by the understanding. Therefore there is much to be hoped for from a closer marriage (which has not yet taken place) between these faculties, namely the experiential and the rational."

One can reconsider Bacon's metaphors as follows: ants are those scholars who remain involved in a particular field, trying to compensate by deepness what they miss by lack of extension and variety: Antoni Zygmund. The spider type is Georg Cantor, proposing a personal construction, with little reference to other authors. To the bee type belongs Paul Erdős, moving permanently from flower to flower, changing always his problems.

Dyson's and Bacon's typologies can be combined: Bolyai: ant and frog; von Neumann: frog and bee; Bourbaki: bird and spider; Hilbert: bird and bee; Gödel: ant and bird; Poincaré: bird and bee. Any researcher combines in various proportions different types, at different periods.

Open questions: Can we transfer these metaphors from individuals to historical periods? For instance, in the field of analysis, can we claim that the eighteenth century was preponderently frog and ant, while the second half of the nineteenth century and the beginning of the twentieth century were predominantly a bee and a bird? Can we describe in such terms the move, in algebraic geometry, from Castelnuovo and Severi to Zariski? etc.

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Some Comments on "Period Three Implies Chaos"

Freeman Dyson's beautiful article "Birds and frogs" (*Notices*, February 2009) refers to the well-known paper "Period three implies chaos" by Li and Yorke (1975). This paper is at the origin of the current use of the word chaos for differentiable dynamical systems. Li and Yorke proved that, for certain maps of the interval, the existence of a periodic orbit of period three implies the existence of periodic orbits of all periods. This is what Li and Yorke called chaos. The use, however, has changed and, as stated by Dyson, is now that "neighboring trajectories diverge exponentially". Most of the periodic orbits arising in the Li-Yorke theorem are unstable, i.e., a trajectory close to such an unstable periodic orbit diverges exponentially from it. Is this chaos? No, chaos occurs if the exponential divergence is present for long-term behavior, i.e., on an attractor. Unstable periodic orbits in a repeller are physically invisible, and do not imply chaos. So, with the modern use of the word chaos, period three does not imply chaos!

Interestingly, the theorem of Li and Yorke is a special case of a theorem by the Ukrainian mathematician Sharkovsky (1964). In its glorious simplicity, the theorem of Oleksandr Mikolaiovich Sharkovsky states that, if a continuous map of the real line to itself has a periodic point of (least) period m, then it also has a point of period n whenever n is to the right of m in the following unconventional ordering of the natural numbers: 3, 5, 7, 9,…, 2.3, 2.5, 2.7,…, 4.3, 4.5,…, 16, 8, 4, 2, 1 (we start with the odd numbers in increasing order, then have the odds multiplied by 2, 4, 8,…, and finally the powers of 2 in decreasing order).

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Separating Mathematicians

In the February 2009 *Notices*, Freeman Dyson elaborates on the division "birds" versus "frogs" among mathematicians. Usually, that division is described as "bird's eye view" versus "worm's eye view", but then Dyson avoids it, lest he may himself call a "worm", since he is considering himself to be a "frog".

Recently, a similar division in "seers" versus "craftspeople" was promoted by Lee Smolin in his amusing book *The Trouble with Physics*. And one of my good old and articulate colleagues likes to go even farther by dividing scientists into those who "think" versus those who "stink". No doubt, it is an irresistible urge of many a human intellect to discriminate, classify, and segregate; and then of course, judge, sentence, and when possible, why not, also execute. And the most primitive and brutal way, needless to say, is to divide in merely two categories. Dyson appears to enjoy himself quite a bit with this "apartheid" venture, and on top of it, seems to be convinced to be doing something useful, if not in fact, even important. Among others, he judges von Neumann to be a "frog" and not a "bird", just like his old professor Abram Besicovitch at Cambridge. Beyond all such "apartheid" excursions, however, Dyson misses quite a few crucial facts related to the work of some of the mathematicians he sets out to segregate. With von Neumann, for instance, he misses two of his extraordinary insights. One of them is the basis of present and future computation, namely, that a computer program is allowed to act not only upon the data, but also upon itself, and do so in ways dependent on the data. This manifestly self-referential nature of computer programs has only come recently to a more systematic attention within a wider mathematical context, namely, with the emerging theory of the so-called non-well-founded sets, presented in the 1996 book *Vicious Circles* of Jon Barwise and Lawrence Moss. Indeed, ever since the ancient Greek Paradox of the Liar, not to mention its modern set theoretic version in Russell's Paradox, there has been a considerable reluctance among mathematicians to deal with any form of self-reference. After all, it indeed cannot be treated lightly, being nothing else but the name of God in Exodus 3:14 of *The Old Testament*. Well, von Neumann not only introduced self-referential programs into effective computation, but managed to do even one better when he proved the existence of self-reproducing automata, and showed that such automata can in fact be rather simple, having less than a few hundred elements. So much for applying hard and fast segregation methods of "apartheid" to truly remarkable scientists.

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Replies and Correction

I am grateful to the authors of the above letters for their criticisms and corrections. I am especially grateful to David Ruelle for telling us about the Sharkovsky theorem and explaining its meaning. I am sad to learn that the clarion statement of Yorke and Lee, "Period three implies chaos", is no longer true.

Surprisingly, none of these four letters calls attention to the worst errors in my lecture, which were pointed out by two other authors, Adrian Bondy and Manjit Bhatia, in personal letters to me. I am grateful to these two gentlemen for identifying my mistakes, which occur in the discussion of the $P = NP$ problem on page 217. To set the record straight, here is a description of the mistakes. I was wrong to say that the traveling-salesman problem, as usually formulated, the problem being to find the shortest route visiting a given set of cities, is NP. To find the shortest route is probably harder than NP. To obtain an NP problem, one should ask a more modest question, for example, whether there exists a route visiting the cities and not exceeding a given length. In addition to this mistake, I made a second mistake when I said that the traveling salesman problem is conjectured to be an example of a problem that is P but not NP. Here I should have said, "NP but not P". These mistakes occurred because I fell into the trap of talking about a subject of which I am ignorant, quoting some remarks that I heard from a friend who is equally ignorant. Thanks to Adrian Bondy and Manjit Bhatia, I am now a little less ignorant.

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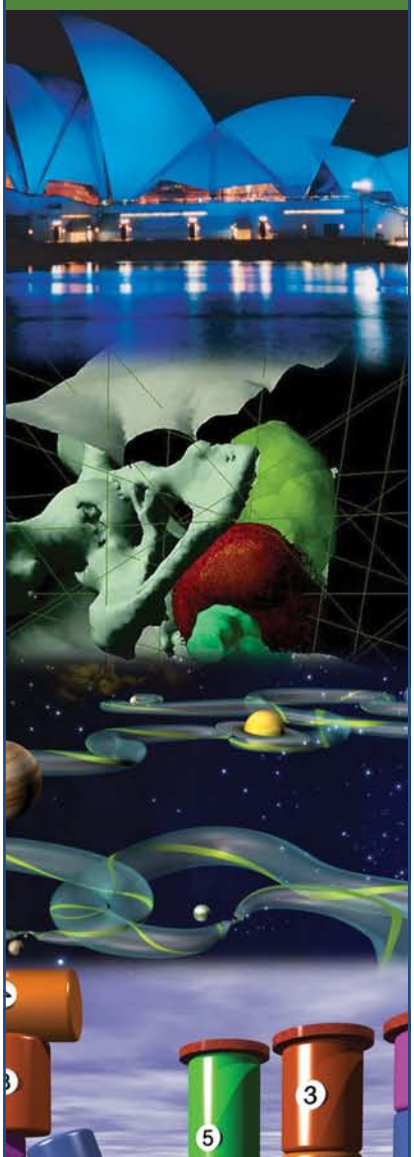
Human Understanding and Formal Proof

By devoting a special issue to an extended discussion of prospects for formalization of mathematics, the *Notices* has done its readers a great service. The articles' authors have taken pains to motivate the long-term goals of their project as well as to present the state of the art in its accomplishments as well as limitations; they have managed the difficult feat of writing clearly about this highly technical subject for non-specialist readers while providing enough substance to make formalization credible. Whether or not Wiedijk's prediction is realistic that, in "a few decades suddenly all mathematicians will start using formalization for their proofs," I have no doubt that a project capable of attracting so many talented people around such clearly defined objectives for one of our central activities will ultimately change the practice of our profession in ways that are both profound and unpredictable.

Writing for an audience of mathematicians, the authors may be forgetting that among their readers will be those who do not necessarily share or sympathize with a common assumption they see no need to make explicit, namely that human understanding of proofs is of interest for its own sake. I am not mainly thinking of future mechanical proof assistants themselves, whose coming role in determining our priorities is scarcely addressed. Of more immediate concern are decision-makers who may well be convinced by the special issue to take the attainment of a given benchmark in the mechanization of mathematics as a signal to begin phasing out human mathematical research as a superfluous luxury. Harrison writes that a formalized proof "can be presented to others in a high-level conceptual way," but a pure cost-benefit analysis might see this as an unnecessary expense.

Harrison sees "the traditional social process" for verifying correctness of proofs as "an anachronism to be swept away by formalization." I would argue that this "woolly community process" is precisely what gives meaning to the peculiar practice of proving theorems. On the alternate view, human intervention in proof-making can easily be construed as a temporary inconvenience. The risk is real that we will lose one of the few fragile means we have evolved to come to terms with our experience



of the given world, and even more so of the virtual reality we all inhabit as participants in the society of human beings.

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Reply to Harris

Michael Harris reminds us that it is sometimes beneficial to say things that go without saying. For the record, I did not intend to disparage the understanding of proof for its own sake, nor the creative activity of human mathematicians generally. Neither, I'm sure, did any of the other authors of papers in the special issue.

I would draw a sharp distinction between (i) verification of a proof, and either (ii) its conceptual understanding, or (iii) the creative process that led to it in the first place. My critique of the "social process" relates solely to its role in verifying the correctness of proofs, as the text following the "anachronism" remark tried to make clear. As a vehicle for conveying understanding, I cannot seriously contemplate an alternative to communication between people. My goal with mechanical proof-checking isn't to put mathematicians out of work or eliminate the need for human creativity. On the contrary, the goal is to free the creative spirit from worrying about whether great imaginative constructs are invalidated by small errors in detail.

Riemann is supposed to have said "If only I had the theorems! Then I should find the proofs easily enough." I would like the mathematicians of the future (particularly those who are not of Riemann's caliber) to be able to say: "If only I had the broad outline of a proof! Then I should have my proof checker verify the details easily enough."

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Submitting Letters to the Editor

The *Notices* invites readers to submit letters and opinion pieces on topics related to mathematics. Electronic submissions are preferred (notices-letters@ams.org); see the masthead for postal mail addresses. Opinion pieces are usually one printed page in length (about 800 words). Letters are normally less than one page long, and shorter letters are preferred.

Correction

The September 2008 issue of the *Notices* carried a brief article I wrote about Grothendieck and the 75th anniversary of the Institut des Hautes Etudes Scientifiques (IHES). The article called the occasion the "sesquicentennial" of the IHES. Thanks to Jordan Bell, a mathematics graduate student at the University of Toronto, for pointing out that the word "sesquicentennial" refers to a 150th anniversary, not a 75th. Bell knows his Latin: He has translated forty of Euler's papers from the Latin and posted them on the arXiv.org with the author name "Euler".

—*Allyn Jackson*