Old and New Algorithms for π

This letter concerns Semjon Adlaj's article "An eloquent formula for the perimeter of an ellipse" (*Notices* **59**, September 2012, 1094–1099). In his comments on the "(so-called) Brent-Salamin algorithm" for computing π, Professor Adlaj misses some important points.

First, both Brent and Salamin acknowledged their debt to Gauss and Legendre. That the names "Brent-Salamin" or "Salamin-Brent" are widely used is probably due to the ambiguity of calling something new after Gauss and Legendre, e.g., a Google search for "Gauss-Legendre" gives many hits on Gauss-Legendre quadrature.

Second, although Euler discovered the special case of Legendre's relation that is used in the simplest Brent-Salamin algorithm ($k=k'=1/\sqrt{2}$), the more general form of Legendre's relation is needed for the members of the family of algorithms that arise from choosing $k \neq k'$. Since Legendre's relation is not attributed to Euler, it would be uninformative to use the name "Gauss-Euler" as Professor Adlaj suggests [footnote 4]. A Google search for "Gauss-Euler" gives even more hits than one for "Gauss-Legendre", but they are almost all irrelevant.

Third, and more important, none of those three great mathematicians of the past would have appreciated the significance of such an algorithm, because they lived in the days before electronic computers and fast algorithms, such as the Schönhage-Strassen algorithm, for multiplication of large integers. Without such technology and modern algorithms, the Brent-Salamin algorithm is a relatively poor algorithm for computing π-algorithms based on the Maclaurin series for arctan($1/n$), such as Machin's $\pi/4=4\arctan(1/5)-\arctan(1/239)$, are far superior (even today, they are competitive if combined with binary splitting and fast multiplication algorithms). Indeed, on reading Gauss's unpublished notebook entry of May 1809, it seems probable that he did not regard his discovery as an algorithm for computing π, since π only appears in the denominator of the right-hand side of the crucial equation. More likely Gauss regarded this equation as an interesting identity involving elliptic integrals, only incidentally involving the known constant π. (The relevant notebook entry is reproduced on page 99 of the book *Pi: Algorithmen, Computer, Arithmetik* by Arndt and Haenel.)

Finally, perhaps this emphasis on the computation of a single constant is unwarranted. Brent's 1975 and 1976 papers, not referenced by Professor Adlaj, showed that *all* elementary functions can be evaluated to given accuracy just as fast as π, up to a constant factor, by using the arithmetic-geometric mean. This of course includes the computation of an infinite set of constants such as e^π and π/e. No doubt this fact would have been of more interest to Euler, Legendre, and Gauss than yet another formula or algorithm for π.

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Yet Another Remark on Algebra Education

In the October 2012 issue of the *Notices*, Peter Johnson argued, citing studies, that algebra does not necessarily transfer critical thinking skills over to other areas.

Just as Dr. Johnson's article is not meant to discredit algebra but to induce skepticism, so is this point: it seems likely that, testing for transference, a study in educational psychology would consider only introductory college algebra classes, and not more rigorous upper-levels.

Such lower-level courses often eschew thoughtful development of algebraic structures, given concerns about time and the students' prerequisites, for rote application of methods. While this may transfer a little bit of critical thinking, certainly algebra's biggest contribution would come from classes like linear algebra and group theory, which are unlikely to be studied psychologically.

All of this is to say that, while a lack of documentation of algebra's role in improving critical thinking encourages skepticism, so does the fact that higher algebra is rarely if ever considered.

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In Response to Rob Kirby's "Whither Journals?"

I was saddened to read Rob Kirby's article "Whither Journals?" in the October 2012 issue of the *Notices*.

It seems that it is becoming a trend in the mathematical community to accept and endorse journals that collect "author processing charges" (APC). This trend has grown out of the frustration of the community with the fact that we have to pay a third party in order to read our papers; it has been decided that it makes more sense to pay a third party in order to publish our papers. I fail to see the logic.

One possible explanation for this trend is that moving to APC publication will make all papers "open-access". But that cannot be the explanation, because almost all papers are already published on people's websites or on the arXiv (and if some researchers refuse to put their publicly funded research on their website or on the arXiv, the answer for that might be legislation).

The only explanation I can find for this trend is that some of its proponents are people of such good intentions that they simply cannot see the obvious corruption and other twisted results that embracing APC publication will result in. For example, Rob Kirby suggested that his hypothetical journals XJM, YJM and ZJM "might correspond to A+, A, and A- papers…" and later, in parenthesis: "Papers graded C or lower could appear in journals in which no money changes hands and only volunteer work is done; B papers would fall somewhere in between." Note the educational message encapsulated here ("nothing is any good if no money changes hands"). From a more practical point of view, what I read is this: commercial publishers will charge higher prices from authors submitting to the more prestigious journals. I do not need to spell out the possible consequences.

If academic publishing is funded by public money, we must search for low-cost publishing models. Volunteer work and community effort are not bad words. Over the last two decades, many have "stepped up to the plate," and several excellent journals come to mind. Anyone can find them (and read them) online.

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