Letters to the Editor

More on Galois

Davide Bondoni ("Galois's first memoir", *Notices*, May 2013) suggests today's Galois theory is collective work, not purely Galois's creation. He suggests that we would best go indirectly at the "master's work".

Bondoni's article may rightly react to the recent C. Curtis review (*Notices*, December 2012) of P. Neumann's book *The Mathematical Writings of Evariste Galois*. Curtis restated the myth of Galois's death consequent on a duel. Mathematicians have long accepted that was over a "tart" the night after he "created his theory of groups." Bondoni refers only to Galois's Theory erupting overnight.

I address Galois's supposed lack of clarity.

1. His work wasn't just about groups (as we might guess from Curtis's review), or even groups delineating fields. 2. Where Galois aimed remains still unreached.

L. T. Rigatelli, *Evariste Galois: 1811–1832*, Italian-to-English translation by John Denton, *Vita Math.* **11** (Birkhäuser, Basel, 1996) remains strangely unacknowledged. Several use her cover picture of Galois while repeating myths she debunks. Rigatelli documents that the girl in question was hardly a tart. Also, Galois was more likely a suicide than a duel victim.

Galois used Abel's introduction of modular curves we now call $X_0(p)$. Thereby, Abel explained the smooth variation of his famous elliptic curve function theorem. Galois's unsolvability theorem showed these equations—excluding finitely many—were unsolvable in j invariant radicals.

Galois deftly connected finite groups (even profinite by introducing the groups $\mathrm{SL}_2(p^k)$) and systems of analytic spaces—before he was twenty-one and without the best education to boot.

Spaces, especially profinite systems, have components, cusps, differentials. These force you beyond profinite thinking. (Contrary to what Curtis once insisted in my UCI office.) Riemann, a certified genius, only partially fulfilled over thirty years later what Galois ended in 1832.

Even from Riemann—despite Gauss' early help—much remains unfinished. I still hear complaints about Riemann's clarity. For my meaning on finite groups among profinite situations, and Riemann's relevance, see M. Fried, "Alternating groups and moduli space lifting invariants", arXiv #0611591v4, *Israel J. Math.* **179** (2010), 57–125, (DOI 10.1007/s11856-010-0073-2), and my website's attached html file on modular towers and modular representations.

We gain much by looking back at the optimism of Galois, Abel, and others, to pursue results before they had a clue as to the outcome. Yes, newcomers would need help from those who have continued such topics.

A variant on Rigatelli is in my *Bulletin London Math. Soc.* **34** (2002), 109–112, review of Matzat and Malle's *Inverse Galois Theory*: `http://www.math.uci.edu/~mfried/booklist-vol/Matzat-MalleInvGal.pdf`.

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Reply to Mike Fried

Let me ponder two questions present in Mike Fried's letter.

1. As a matter of fact, all mathematicians do understand mathematical objects by relying on their own personal experience. We must thank people like Klein for a structural reading of the concept of *group*. While for Galois the concept of *group* was only instrumental in proving the nonresolution of a fifth degree equation by radicals, other mathematicians regarded the concept of *group* from a metamathematical perspective, i.e., as a concept of a theory useful for studying structured sets. Yet in 1874 the German mathematician Ernst Schröder in a short pamphlet [Sch74] accomplished an analysis of such mathematical structures, attaining the definitions of *loops*, *semi-groups*, and (*commutative*) *groups*, starting from his personal structural philosophy of mathematics.[1] According to such a point of view, an object in itself does not exist; it exists only in a given context. Mathematics is not neutral, but it is only context-dependent, as the various interpretations of Galois's work testify.

2. About Galois's death, I argue that it was a suicide in disguise caused by Galois's inability to integrate himself into a social group (be it academic, political, or other). Galois's suicide, *masked* as a duel, was the only way he could affirm control of his life. Apparent homicides were explained as suicides recently by Goeschel [Goe09] and by Emil Durkheim in his 1897 masterwork [Dur13].

[1] *I had occasion to write on this topic extensively (see* [Bon11], [Bon12] *and my introductory essay* Algebra, what else? *in* [Sch12, pp. ix–xli]).

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Mathematics and Historical Chronology

The *Notices* published in August 2013 two letters related to my invited article "Mathematical methods in the study of historical chronology", [*Notices,* April 2013], which was duly reviewed by seven anonymous referees. This piece is based on my book *The Lost Millennium—History's Timetables Under Siege*, an objective account of the debate between historians and revisionists of the established chronology, the latter group known to include many cranks, such as Immanuel Velikovsky. Among other issues, I analyze there the controversial work of Anatoli Fomenko and his collaborators, which I divide into three categories: good

(follows established mathematical results and has not been refuted, so far), mediocre or weak (unconventional or questionable applications of otherwise sound mathematics), and blunders (conclusions drawn using false premises or reasoning). In my *Notices* article I focus on some mathematical methods used in chronology that have already helped historians with dating events or may do so in the future. But in the eyes of the letter writers, Fomenko steals the show.

Claude LeBrun openly trashes Fomenko's work without providing any evidence against my article, which he nevertheless dislikes, mostly for not having any mathematical content. Well, had he read what I wrote, he might have learned that one of the issues I deal with is the motion of the moon, so it embodies results in celestial mechanics (mainly differential equations) obtained by Newton, Lagrange, Laplace, Poisson, Jacobi, Poincaré, Hill, and others. My article also refers to other branches of mathematics.

The letter by Alex Emerenko and Victor Grinberg attempts, at least, to argue against the publication of my article. But the way they do it reminds me of a joke about a man who was trying to find his keys around a lamppost, though aware that he had lost them in the dark, a block away. Instead of discussing the issues I present, they take my statements out of context, apply them to all of Fomenko's work, then quote some of Fomenko's blunders, and finally let the reader decide whether my article is worth its salt. Arguments to the point? Zero. If Emerenko and Grinberg are so eager to defend the truth, they should consider sticking to the facts, abstain from distorting other people's statements, nurture some fairness, and use logic even when expressing an opinion outside their expertise.

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The Incorrect DNA Structures and Mathematicians' Contribution to DNA Topology

2013 marks the sixtieth anniversary of Watson and Crick's publication of the double helix DNA structure [1]. It is worth emphasizing that arguments against the intertwined helical structure also played important roles in making it firmly accepted. And the establishment of a rigorous mathematical foundation of circular DNA topology further supports the helix structure.

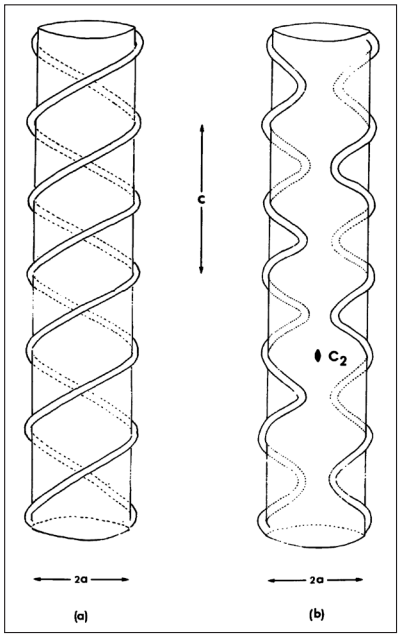
Pauling and Corey proposed a triple helix DNA structure in 1953, which turned out to be wrong. Watson and Crick had considered an incorrect DNA model before the double helix; in their earlier incorrect model, the negatively charged phosphate groups of two strands of DNA interact by binding with the magnesium ion between two phosphates, and four bases (adenine and thymine; guanine and cytosine) have no interaction.

The double helix is intertwining and topologically constrained for circular DNA. A big difficulty is the separation of strands during replication. Therefore the double helix was challenged even after Watson and Crick had been awarded the Nobel Prize in 1962, and the side-by-side model of DNA was proposed (see accompanying figure) [2, 3]. The exclusion of the side-by-side model was recalled by Crick [4]:

"At about this time Bill Pohl, a pure mathematician, got into the act. He pointed out, quite correctly, that unless something very special happened, the most likely result of replicating a piece of circular DNA would be two interlocked daughter circles rather than two separate ones. From this he deduced that the DNA chains could not be intertwined, as we had suggested, but had to lie side by side.

"Fortunately some brilliant work by Walter Keller and by Jim Wang on the 'linking number' of circular DNA molecules proved that all these side-by-side models must be wrong."

In addition to William F. Pohl, the mathematical relationship of the linking number L of a circular DNA with the twist T and the writhing number W was mainly accomplished by two mathematicians, F. Brock Fuller and James H. White [5, 6, 7]. The equation $L = T + W$ is not a derivative of known mathematical or physical laws; it is unique for DNA topology and geometry. This equation is one more example of Eugene Wigner's "unreasonable effectiveness of mathematics in the natural sciences."



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