Who Would Have Won the Fields Medal 150 Years Ago?

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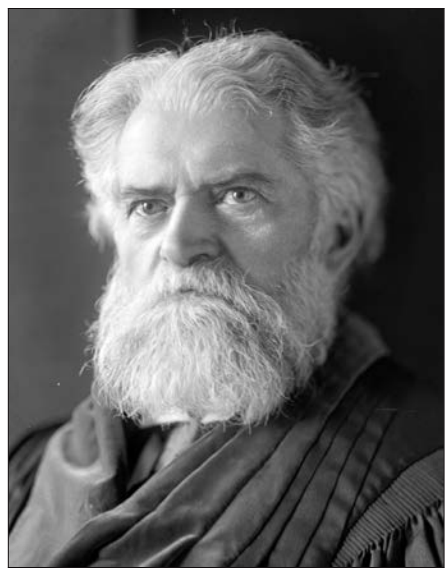
Introduction

Hypothetical histories are a way of shedding light both on what happened in the past and on our present ways of thinking. This article supposes that the Fields Medals had begun in 1866 rather than in 1936 and will come to some possibly surprising conclusions about who the first winners would have been. It will, I hope, prompt readers to consider how mathematical priorities change—and how hindsight can alter our view of the mathematical landscape.

Let us imagine that in 1864 the Canadian-American astronomer Simon Newcomb had seen past the horrors of Antietam and Gettysburg to a better world in which mathematics would take its place among other cultural values in the new republic. Let us further suppose that Newcomb, in his optimism for the future, had decided that there should be a prize awarded regularly to young mathematicians who had done exceptional work and that the first awards should be made in August 1866.

What might have motivated Newcomb to thus recognize exemplary mathematics research? Since 1861 Newcomb had worked at the Naval Observatory in Washington, DC, as astronomer and professor of mathematics. There Newcomb helped fill the void left when other academics, uncomfortable at being employed by a military institution during a time of war, resigned their positions at the observatory.



Simon Newcomb (1835–1909)

The US government tasked Newcomb with determining the positions of celestial objects. It was challenging work for Newcomb, who had had no formal education as a child and had learned mostly from his father before attending the Lawrence Scientific School at Harvard University and studying under Benjamin Pierce. It was this work which gave Newcomb his appreciation of mathematics.

To give the mathematics prize credibility, Newcomb would have realised that he would have to select a panel of sufficiently eminent judges. To find them, he would have to travel to Europe, so let us follow him to Paris in early 1865, the year he turned thirty.

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The French were then feeling an unaccustomed inferiority. For two generations the French had dominated mathematics. Laplace, in the five-volume *Traité de Mécanique Céleste* (1798–1825), had given conclusive reasons to believe that Newtonian gravity rules the solar system and could explain all apparently discordant observations. Lagrange had been the true successor of Euler in many fields, and what little he had set aside Legendre had taken up. These luminaries had been succeeded by Cauchy, Fourier, and Poisson, and Paris had drawn in many of the best young mathematicians from abroad, Abel and Dirichlet among them.

But these great figures had all died by 1865, and news from the German states had the French looking uncomfortably second rate. The influence of Gauss, who had died in 1855, the year before Cauchy, was ever more apparent. Gauss had revivified the theory of numbers—"the higher arithmetic," as he had called it—making it more substantial than Euler or Lagrange had managed to. Closely intertwined with Jacobi's elliptic function theory, the field had been further extended by Dirichlet. Although Jacobi and Dirichlet had both died by 1865, the University of Berlin was flourishing, and, it had to be admitted, the École Polytechnique was not. Apparently the glory days of Napoleon had taught better lessons to those whose countries he had conquered than to the French themselves, who found themselves with a complacent government unable or unwilling to keep up.

Newcomb would make it his business to meet Joseph Liouville, who was the founder and editor of the important *Journal de Mathématiques Pures et Appliquées* and used its pages to keep the French mathematical community aware of what was happening abroad. Liouville had been involved in bringing to France Kummer's discovery of the failure of the prime factorisation of cyclotomic integers and his attempts to redefine "prime" to deal with this unexpected setback. Kummer had subsequently won the Grand Prix des Sciences Mathématiques of the Paris Academy of Sciences in 1857 for his work on the subject. As a successful editor, Liouville was the best person to ask about new and exciting mathematicians. And he would be fifty-six in 1865, too old to be considered for the prize himself.

Liouville had done important work on the theory of differential equations (Sturm–Liouville theory), potential theory, elliptic and complex functions, the shape of the earth, and other subjects. But Newcomb might well have been discouraged on consulting the most recent issues of the *Journal*, because they were full of Liouville's interminable and shallow investigations of the number theory of quadratic forms in several variables. In a sense, what Liouville's *Journal* and the *Journal* of the École Polytechnique showed was that mathematics in France was at a low ebb.

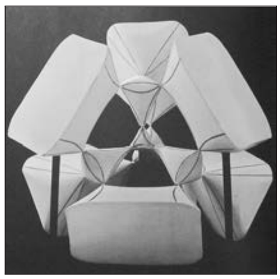
Joseph Liouville (1809–1882)

Liouville would surely have praised Charles Hermite highly, had Newcomb prompted him to suggest potential prizewinners. Hermite had studied with Liouville, and together they had come to a number of insights about elliptic functions. It was in this context that Liouville had discovered the theorem that still bears his name: a bounded complex analytic function that is defined everywhere in the plane is a constant. Hermite had gone on to explore the rich world of elliptic functions and in 1858 had used that theory and algebraic invariant theory to show that the general polynomial equation of degree five has solutions expressible in terms of elliptic modular functions. This work had drawn the attention of Kronecker and Brioschi and remained an insight that rewarded further attention. Galois's discovery that the general quintic equation was not solvable by radicals sat in the context of equations that are solvable by precise classes of analytic functions.

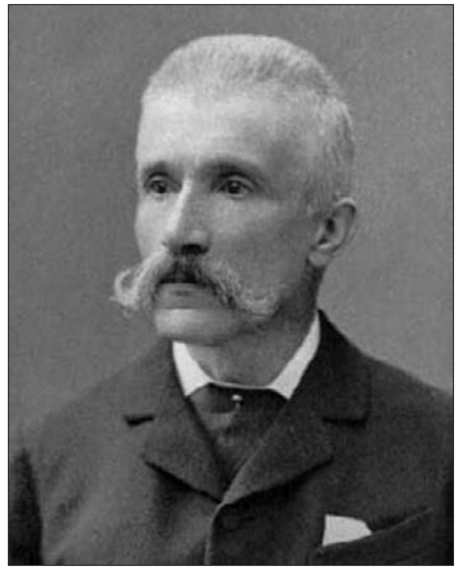
But Hermite was forty-two. Newcomb wanted to recognize younger mathematicians to point the way to the future, and he was beginning to realise that he needed more advisors than Liouville alone.

Kummer was the obvious choice. He was less than a year younger than Liouville, and he had worked on a variety of topics before deciding that Gaussian number theory was the rock on which to build a career. He had written on the hypergeometric equation (another Gaussian topic) and as recently as 1863 on a quartic surface with sixteen nodal points. Kummer had succeeded Dirichlet at Berlin in 1855, when Dirichlet moved to Göttingen to succeed Gauss, and had swiftly arranged for Weierstrass to be hired at Berlin. Weierstrass, who had just turned forty then, had only recently burst onto the mathematical scene with his theory of hyperelliptic functions and was now, with Kummer, establishing Berlin as the preeminent place to study mathematics. Furthermore, Kummer, as proof of his administrative ability, had just become dean of the University of Berlin. Newcomb would choose Kummer as the second member of his prize committee.

Ernst Eduard Kummer (1810–1893)



A quartic surface with sixteen nodal points



Francesco Brioschi (1824–1897)

By now Newcomb was becoming aware that the new state of Italy (unified only in 1861) was also producing important mathematicians. The Italian figures comparable to the German judges Kummer and Liouville were Enrico Betti and Francesco Brioschi, who had both just turned forty. Both had been actively involved in the unification of Italy and now led political lives: Betti had been elected to the Italian parliament in 1862, and Brioschi was to become a senator in 1865. Betti had just become the director of the Scuola Normale Superiore; Brioschi was the founder and director of the Istituto Tecnico Superiore in Milan. Both men were devoted to raising the standard of mathematics in Italy in both schools and universities; both were active in research. Betti, who had become a close friend of Riemann's when he stayed in Italy, was interested in extending Riemann's topological ideas and also worked on mechanics and theoretical physics. Brioschi had done important work in algebra, the theory of determinants, elliptic and hyperelliptic function theory, and he had taught many of the next generation of Italian mathematicians: Casorati, Cremona, and Beltrami among them. Newcomb, we shall suppose, would have decided that Brioschi was the man to keep him informed of the latest developments in the emerging domain of Italian mathematics.

Should that be enough, or should Newcomb make a trip to Britain? In pure mathematics this meant a visit to Cayley; in more applied fields there were several people at Cambridge who might be consulted. The mathematicians Newcomb had already met spoke well of Cayley and respected him as an inventive and well-read mathematician who spoke several languages. He was, along with his friend Sylvester, best appreciated for his exhaustive, and sometimes exhausting, investigations into invariant theory. But it did not seem that there was anyone in England in 1865 who could be considered for the prize, now that Cayley was in his early forties and thus ineligible himself.

Newcomb decided that three judges were already enough: Liouville, Kummer, and Brioschi. It was time to select a prizewinner.

Newcomb already knew one name. Everyone he spoke to told him about Bernard Riemann, a truly remarkable former student of Gauss in Göttingen. Riemann had published a remarkable paper on abelian functions in 1857 which was so innovative that Weierstrass had withdrawn a paper of his own on the subject, saying that he could not proceed until he had understood what Riemann had to put forward. That same year, Riemann had published a very difficult paper on the distribution of the primes in which he made considerable use of the novel and fundamentally geometric theory of complex analytic functions that he had developed and which was essential to the paper on abelian functions. There was also a paper in real analysis where he had developed a theory of trigonometric series to explore the difficult subject of nondifferentiable functions, and there was talk of a paper in which he was supposed to have completely rewritten the subject of geometry.

Riemann would turn thirty-nine in 1865, so Newcomb could agree that he was still, officially,

Bernhard Riemann (1826–1866)

Luigi Cremona (1830–1903)

A cubic surface with 27 lines

young. There was the disturbing problem of Riemann's health, however. He suffered from pleurisy and was said to have collapsed in 1862 and to be recuperating in Italy. Newcomb would have to stay informed.

As for the generation born in the 1830s, Liouville, Kummer, and Brioschi might well have given different reports.

Liouville, to his regret, would have had no one to suggest. Kummer, too, would have struggled to name a nominee. His former student Leopold Kronecker had just turned forty, and although there were some promising younger mathematicians at Berlin—Lazarus Fuchs sprang to mind—they had yet to do anything remarkable.

Brioschi, on the other hand, would have been optimistic. He could have suggested the names of Cremona, Casorati, and Beltrami. Cremona was already known for his work on projective and birational geometry, including the study of geometric (birational) transformations, and Brioschi could have assured the panel that Cremona was writing a major paper on the theory of cubic surfaces (it was to share the Steiner Prize in 1866—Kummer was one of the judges—and was published as (Cremona 1868)). Casorati was perhaps the leading complex analyst the Italians had produced, and Beltrami was emerging as a differential geometer in the manner of Riemann.

Over the summer of 1865 Newcomb would have faced a difficult decision. No one disputed Riemann's brilliance, although Kummer reported that Weierstrass was hinting that not all of Riemann's claims were fully established, and Liouville was saying that Hermite was hoping for direct proofs of some results that presently relied on Riemannian methods.

The problem was to decide who else might get the prize. There were several bright young mathematicians, but none of the highest calibre. Should Newcomb announce the existence of the prize, call for nominations, and risk disappointment? Or should he postpone it and give the younger mathematicians a better chance to shine?

And then there was the worrisome matter of Riemann's deteriorating health. Weak and prone to illness, he had spent the summer recuperating near Lake Maggiore and in Genoa and had returned to Göttingen in early October.

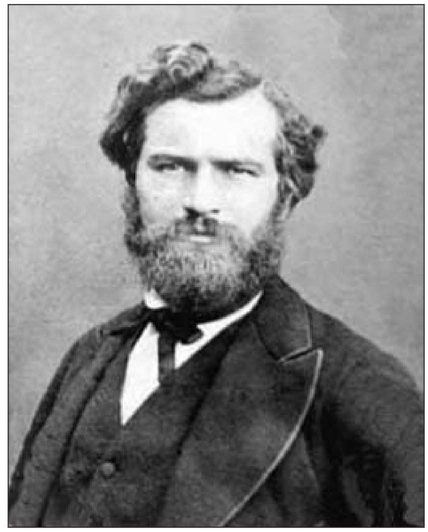
Let's suppose Newcomb decided to postpone the competition for four years.

Riemann died on July 20, 1866, within a month of returning to Lake Maggiore. He was thirty-nine. Among the papers published shortly after his death, the one entitled "The hypotheses that lie at the foundations of geometry" was to inspire Beltrami (born 1835) to publish his "Saggio", in which non-Euclidean geometry was described rigorously in print for the first time. The leading German physicist, Hermann von Helmholtz, was independently converted to the possibilities of spherical geometry and in correspondence with Beltrami came to advocate the possibilities of non-Euclidean ("hyperbolic") geometry as well.

Asked about candidates from Germany, Kummer could now offer three or four. The first was Rudolf Clebsch, who, with his colleague Paul Gordan, had devised an obscure but effective notation for invariants that had led to many new results, and he had applied himself successfully to the study of plane curves. He also had showed that elliptic functions can be used to parameterise cubic curves and in 1864 had begun to extend such ideas to curves of higher genus. Then, in 1868, he had opened the way to extending Riemann's ideas to the study of complex surfaces by defining the (geometric) genus of an algebraic surface.

The second was Lazarus Fuchs, a former student of Kummer's, who was now attached to Weierstrass and seemed poised to extend Riemann's ideas. So too was the third candidate, Hermann Amandus Schwarz, who was using Weierstrass's representations of minimal surfaces to tackle the Plateau

Eugenio Beltrami (1835–1900)



Camille Jordan (1838–1922)

Rudolf Clebsch (1833–1872)

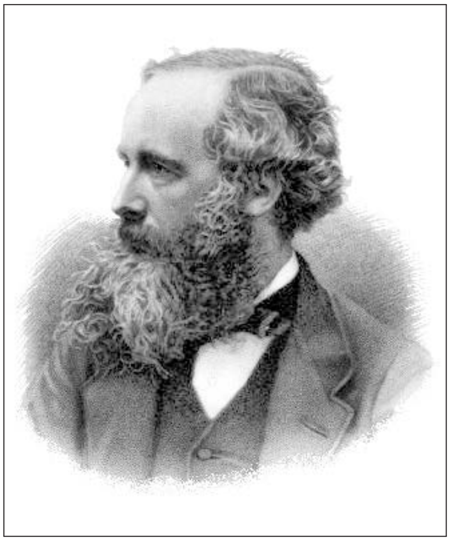
problem. He was also beginning to think about the Dirichlet problem.

Then there was the fourth candidate, Richard Dedekind, who was emerging as a number theorist in the tradition of Gauss and Dirichlet. But Kummer, and still more his colleague Kronecker, had their doubts about the highly abstract and not always explicit character of Dedekind's approach. It would seem appropriate to wait.

Liouville, too, would have had a new candidate to put forward as the 1860s came to an end: Camille Jordan. Jordan had published a series of papers that he was now drawing together in his book *Théorie des Substitutions et des Équations Algébriques*. In these papers and again in the book, he set out a theory of groups of substitutions (permutation groups) of great generality and showed how to use it to derive all of Galois's results systematically. He had then gone on to use it in a number of geometrical settings, finding, for example, the groups of the twenty-seven lines on a cubic surface and Kummer's surface with its sixteen nodal points. He outlined in over three hundred pages a programme to find all finite groups. Not everyone was convinced of the need for such a big new idea, but it was bold and rich in applications to topics that were known to be interesting.

And what of James Clerk Maxwell? Could his best work even be called mathematics? He had written on many subjects, but his major 1864 paper "A dynamical theory of the electromagnetic field" and a 1866 paper in which he suggested that electromagnetic phenomena travel at the speed of light (thus implying that light is just such a phenomenon) displayed a considerable mastery of the difficult mathematics they involved. He had also published his second major paper on the dynamical theory of gases, which did much to establish the statistical approach to physics.

So Newcomb would have had four candidates: Beltrami, Clebsch, Jordan, and Maxwell. Under pressure, Brioschi would have had to admit that Beltrami had published only one remarkable result amid a stream of good ones, mostly in differential geometry. But at least his work was independent of Riemann's, as Cremona would attest. Clebsch was another heir of Riemann's, but he worked in a tradition that was opposed in some ways to Kummer's way of doing things. Jordan was the youngest, and his advocacy of substitution group theory was controversial. Some found it a fine addition to the geometrical way of thinking, and some were to see in it the way to rewrite Galois theory the way Galois might have meant it, but others were to find it needlessly abstract and



James Clerk Maxwell

almost unnecessary even for Galois theory (and preferred the language of field extensions).

As for Maxwell, electricity and magnetism had been the major topics of mathematical physics for the preceding fifty years, but no one in continental Europe understood Maxwell's ideas. In particular, they found it incomprehensible that electric current was a discontinuity in a field and not the passage of a (possibly mysterious) substance. It was too risky to choose Maxwell.

What might Brioschi, Kummer, Liouville, and Newcomb then decide? From today's perspective, it seems they should have rewarded (1) conclusive proof of the existence of a new, possible geometry of space (when the work of Bolyai and Lobachevskii was almost completely forgotten), and (2) a new and highly abstract structure (the group) that seemed to have many applications, although many important details remained to be published. Beltrami and Jordan might well be our modern choices.

But I suggest that Newcomb and his advisors would have chosen differently. Kummer had a great sympathy for the study of algebraic surfaces and a high opinion of Cremona's work. Brioschi could have agreed that it offered a way into the general study of algebraic geometry, to which Clebsch had also made a contribution, and that the new, non-Euclidean geometry, however remarkable, had yet to lead to new results. If Cremona represented the opening up of a subject that had long challenged mathematicians, then Jordan could embody the spirit of radical innovation, one that also led to insights into geometry. But such a decision would be strongly opposed by Kronecker and Hermite, who were powerful advocates of invariant theory, and their views would be well known to Kummer and Liouville. That would have made Clebsch a contender, not so much for his Riemannian work as for his development of invariant theory.

Newcomb, who was to publish a paper in 1877 on spaces of constant positive curvature, might have pushed hard for Beltrami. But in the 1860s his own work was firmly in mathematical astronomy, so I suppose that he would have let himself be guided by the counsel of his chosen judges. He surely would have wanted to see that Riemann's ideas were being carried forward, but the citation for Clebsch, the leading advocate of Riemann's ideas, would take care of that. The prizes, I conclude, would have gone to Cremona and Clebsch.

End Note

The real Fields Medal was established in the bequest of John Charles Fields, a Canadian mathematician who had been active in the International Mathematical Union. It was his wish that the prize be awarded every four years to two young mathematicians, and although he did not define "young," this has come to mean under forty, and that guideline has therefore been preserved here. The first Fields Medals were awarded in 1936 to Lars Ahlfors and Jesse Douglas.

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