THE ROOTS OF MATHEMATICAL THOUGHT

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Six years ago, I wrote the opinion piece [T2], titled 'Mathematics is a quest for truth'. My aim in this sequel is to expand upon the sentiment expressed there that 'the roots of mathematical thought lie within the deepest recesses of the human mind, where *logos* and *mythos* come together, in search of our very nature'. I will examine the moment when new mathematics is created, which is also a moment of discovery and revelation. There is a certain mystical quality to this event, which is to a large extent very personal. In conclusion, I will discuss what I believe mathematical research can tell us about ourselves, and our role in the world.

I agree with Dieudonné [D, II.6] and Hardy [H] that the main reason which compels us to do research in mathematics is intellectual curiosity, the attraction of enigmas, the desire to know the truth. Note however that neither [T2] nor the present paper claim that anything mathematical is actually true.

To give credence to my thoughts, I have to draw upon my own experience, which inevitably entails some self-promotion. I apologize in advance for the latter, and will have something more to say about that at the end. I feel compelled to write because I have not seen this topic discussed in the same way before, despite what I strongly suspect, that I am not alone. Eleven years ago, I solved my favorite problem in all of mathematics, in a manner so effortless that I did not realize the full significance of the event until much later. The story of how that happened will accompany the more important points to be made along the way.

I was reading books by my second birthday, and exhibited curiosity about all sorts of things. Around that time, my family moved to the United States from my native country of Greece, so I had two languages to play with. An incident that affected me occurred in fourth grade elementary school, in Skokie, Illinois, when our teacher taught us about area, and showed how to derive the formula πR^2 for the area of a circle. I remember the proof to this day. He cut the circle into two semicircles, and divided each of them using radii into a large and equal number of triangular wedges. The two halves of the circle unfold along these lines by straightening their outer rim, like a sliced orange, and fit together to form an (approximate) rectangle whose side lengths are πR and R, respectively.

I noticed that there was one shape our teacher had mentioned without giving us a formula for its area: the 'oval'. When I asked 'What is the area of an oval?', he said it requires calculus, and I would have to wait until college, or perhaps late in high school, to learn that. This led me on a quest to teach myself calculus and find the answer, which lasted for years. The plot thickened when we moved back to Greece a few years later, as I did not know the translation of Latin terms like 'calculus' and 'oval' into Greek. The few books I managed to gather on the subject

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were not very helpful, and I never understood the precise definition of a limit until it was taught to me in class, during my last year of high school.

In fact, a proof of the formula for the area of an ellipse does not require calculus. An ellipse with semiaxes of lengths a and b with $a < b$ can be defined as the intersection of a cylinder, whose horizontal cross section is a circle C of radius a, with a slanted plane. The ellipse is obtained by dividing C into two halves and stretching each half in the vertical direction by a factor of b/a. The area of the ellipse is thus b/a times the area πa^2 of C, or πab. This argument using similar triangles is not beyond the comprehension of a curious fourth grader. However I am glad that my teacher did not tell me this, because the journey I embarked on to discover the answer was much more valuable than the destination.

In secondary school, I found the answers that mathematics gave to the key question of 'why?' more satisfying than those offered in my science classes. Although mathematics and science are close relatives and cross-pollinate each other, there remains a fundamental distinction between them, in that scientific validity depends upon agreement with experiments, whereas mathematical facts rely on logically rigorous proofs from abstract first principles. As such, the latter are among the very few things that (so far) survive the test of time.
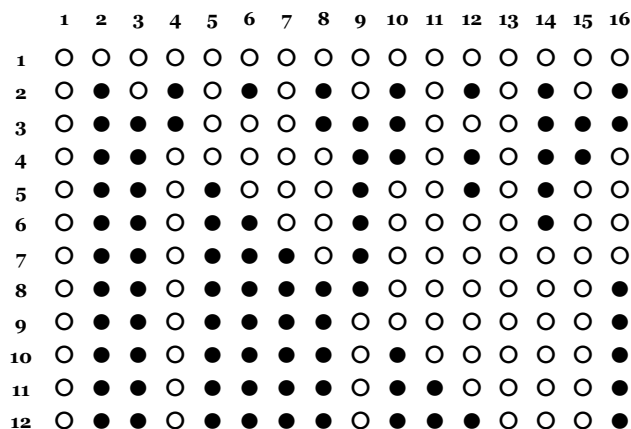
This proximity to truth and beauty was a major part of the attraction of mathematics for me. Another was my participation and success in math competitions, which exposed me to challenging problems. In downtown Athens, there were weekly informal training sessions for mathematical olympiads, organized by the Hellenic Mathematical Society, and I decided to go have a look. The very first problem I faced upon my arrival there was the following.

A school has 1000 students. Each student has a locker, and these are numbered from 1 to 1000. The first student opens all the lockers. The second student closes every second locker. The third student goes to every third locker, closing those lockers which are open, and opening those which are closed. This continues in the same manner, with the nth student going to every locker whose number is a multiple of n, and opening/closing those which are closed/open, for each $n \leq 1000$. How many lockers will be open at the end of this procedure?

After overcoming my initial reaction, here is how I solved this: Consider a fixed locker, say locker number 12. The students touching this locker are numbers 1, 2, 3, 4, 6, and 12, which are the divisors of 12. The locker remains closed at the end because 12 has an *even* number of divisors. Observe that the divisors of any positive integer n can be organized in pairs of the form $(d, n/d)$, such as the pairs $(1, 12)$, $(2, 6)$, and $(3, 4)$ in our example. Therefore n will have an even number of divisors *unless* there is a divisor d of n such that $d = n/d$, that is, unless $n = d^2$ is a square. Hence the lockers numbered $1, 4, 9, 16, 25, \ldots$ will remain open, and, since there are 31 squares less than 1000, the answer to the problem is 31.

This delightful conundrum, whose solution requires only basic arithmetic, has two important lessons to teach us. The first is the utility of working on a specific example, and then trying to generalize from there. The second lies deeper. When confronted with the question, one's first impression is that of a long line of lockers, some of them changing their state with every passing student. It is a picture of dizzying complexity, similar to the famous sieve of Eratosthenes, which detects the primes. In order to solve the problem, one needs to concentrate on a fixed locker

and carefully analyze what happens to it. This involves changing your point of view, from a *global/horizontal* to a *local/vertical* one (see the figure below).

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        1   2   3   4   5   6   7   8   9  10  11  12  13  14  15  16
    1   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○   ○
    2   ○   ●   ○   ●   ○   ●   ○   ●   ○   ●   ○   ●   ○   ●   ○   ●
    3   ○   ●   ●   ●   ○   ○   ○   ●   ●   ●   ○   ○   ○   ●   ●   ●
    4   ○   ●   ●   ○   ○   ○   ○   ○   ●   ●   ○   ●   ○   ●   ●   ○
    5   ○   ●   ●   ○   ●   ○   ○   ○   ●   ○   ○   ●   ○   ●   ○   ○
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    7   ○   ●   ●   ○   ●   ●   ●   ○   ●   ○   ○   ○   ○   ○   ○   ○
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   10   ○   ●   ●   ○   ●   ●   ●   ●   ○   ●   ○   ○   ○   ○   ○   ●
   11   ○   ●   ●   ○   ●   ●   ●   ●   ○   ●   ○   ●   ○   ○   ○   ●
   12   ○   ●   ●   ○   ●   ●   ●   ●   ○   ●   ●   ●   ○   ○   ○   ●
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The importance of a new perspective shedding light in an area of mathematics will be a theme of this narrative. The examples are myriad, at every level of the subject, for instance the introduction of variable quantities and instantaneous rates of change, which turned the ancient problem of the computation of areas from a static to a dynamic one, and led to the fundamental theorem of calculus.

My undergraduate study at the University of Athens provided me with a solid background in analysis, which impressed my teachers during my first year of graduate school at the University of Chicago. It came as a surprise to me that five years later, I was writing my thesis in arithmetic algebraic geometry. My favorite subject was complex analysis, and under the direction of my first advisor Narasimhan, I went from the study of Riemann surfaces and Hodge theory to Grothendieck's theory of schemes and intersection theory in algebraic geometry. Narasimhan then suggested that I have a look at Gillet and Soulé's arithmetic intersection theory, a generalization of Arakelov theory to higher dimensions, which added number theory to the mix. Since the glue that held all this together was intersection theory, it made sense after my second topic exam to switch advisors to Fulton, who was also at Chicago, and whose brilliant book on the subject had kept me going.

One of the main difficulties with higher dimensional Arakelov theory is a lack of examples where explicit computations are possible. Maillot had succeeded in computing arithmetic intersection numbers on Grassmannians, and I decided to try to extend his work to more general flag manifolds. This involved moving beyond the case of hermitian symmetric spaces, which have a canonical theory of harmonic differential forms, and also learning a fair amount of combinatorics.

The advice I received from Fulton was to first try doing some small examples. This is certainly the way one should begin any such project, however I found to my frustration that I was unable to compute even *one* new example using the known methods. Instead, what was required was a different perspective: my Ph.D. dissertation was one of the first works to use *representation theory* in Arakelov geometry, and led the way to a new *arithmetic Schubert calculus*. During the long

road to its completion, I came to appreciate all that mathematics has to offer, and the beauty found in areas that seem far from one's initial interests.

I remember the moment when I realized that I could solve my thesis problem. I was alone in my living room, sitting on the couch, when my mind turned again, as it had so many times before, to the calculation I sought to achieve. And then, in a sudden flash of inspiration, I saw how I could do it. The solution combined in a novel way ingredients that I had developed gradually over the previous years, and proved that all the natural intersection numbers were rational numbers. On that occasion, I experienced the ineffable, incomparable feeling of exciting new mathematics taking shape right before (or rather, behind) my eyes.

The following excerpt from a famous poem by Dionysios Solomos describes in the best way that I know the exhilaration that the researchers of the unknown experience at the moment of mathematical discovery.[1]

> *Mother, magnanimous in suffering and glory,*
> *Though your children forever live in hidden mystery*
> *In reflection and in dream, what grace have these eyes,*
> *These very eyes, to behold thee in the deserted forest . . .*

The 'Mother' is the ontological mother – the mother that gives birth to us all. She is the *truth* – no matter how defined – that mathematicians seek in their research endeavors. Her children live in hidden mystery, alone and in the dark, searching for a pathway to that truth. We have two essential tools at our disposal: deductive reasoning (reflection) and guesses or conjectures, which are rooted in human intuition and fantasy (dreams). Much of the work happens subconsciously, over long periods of time. And when the moment of enlightenment finally comes, one is left speechless in admiration and awe. The only feeling then is one of wonder, joy, and gratitude for the gift that was bestowed upon you, to be present there with your mind's eye open, at the very moment when the Mother reveals a bit more of her light. We have here an essentially otherworldly experience, as the researcher understands that he or she is more the recipient or channel of knowledge, rather than its creator. No other area of inquiry illustrates in such a profound way the illusion of human agency: we apparently have original mathematical ideas, but the conclusions we obtain using them could not have been otherwise.

There is ample evidence that new mathematics is discovered, often independently by different people at different times, and is a revelation to us. A striking example of this occurred in my first joint paper with Kresch on the arithmetic Grassmannian. For such spaces, Gillet and Soulé formulated arithmetic analogues of Grothendieck's difficult standard conjectures on algebraic cycles.

Kresch and I used arithmetic Schubert calculus to prove these conjectures for the Grassmannian of lines in projective space [KT]. In the course of our work, we were led to make a conjecture of our own about a certain family of *Racah polynomials*. This indirect and miraculous connection between the problem we set out to solve, the theory of hypergeometric orthogonal polynomials, and the Racah coefficients or 6-j symbols in quantum mechanics came as a complete surprise.

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[1] I am indebted to the Greek philosopher Christos Malevitsis, who loved this passage and discussed its significance in his writings. The translation and interpretation here are my own.

To state our conjecture, let k, ℓ, and n be integers, and define

$$(1) \qquad R(k,\ell,n) := \sum_{i=0}^{n-1} (-1)^i \binom{k}{i}\binom{k+i}{i}\binom{\ell}{i}\binom{\ell+i}{i}\binom{n-1}{i}^{-1}\binom{n+i}{i}^{-1}.$$

**Conjecture 1.** *For any integers k, ℓ, n with $0 \le k, \ell \le n-1$, we have*

$$-1 \le R(k,\ell,n) \le 1.$$

We found much computer evidence in support of Conjecture 1, and proved it when $\ell \le 3$ or $\ell = n-1$. The latter case is interesting: although one can show, using the Wilf-Zeilberger method, that in general there is no 'closed form' for the sum defining $R(k,\ell,n)$, when $\ell = n-1$ there is such a formula, namely

$$(2) \qquad R(k,n-1,n) = \sum_{i=0}^{k} (-1)^i \frac{n}{n+i}\binom{k}{i}\binom{k+i}{i} = \frac{(1-n)(2-n)\cdots(k-n)}{(1+n)(2+n)\cdots(k+n)}.$$

The second equality in (2) is a special case of an identity proved by Pfaff, 220 years ago, and (independently!) by Saalschütz, 130 years ago.

The most vivid evidence for Conjecture 1 is the following picture, which was kindly provided to us by Wilf. In the figure, we let $n := 51$ and plot the values $R(k,\ell,51)$ on the lattice points $0 \le k, \ell \le 50$, then connect the resulting dots by line segments. In fact, the bound $|R(k,\ell,51)| \le 1$ fails badly if k and ℓ are taken to be *real* parameters lying in $[0,50]$. In other words, the conjecture depends essentially on the *integrality* of k and ℓ.
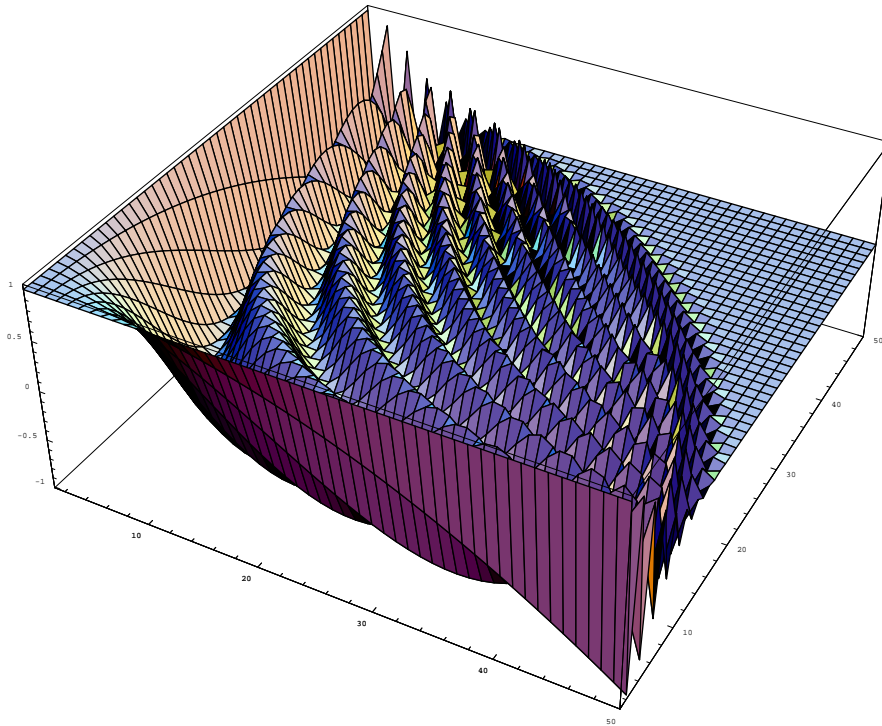
Conjecture 1, which surprised Askey, remains open as of this writing. Fortunately for Kresch and me, the arithmetic Hodge index conjecture for the Grassmannian $G(2, n)$ is equivalent to the weaker inequality

$$\sum_{k=1}^{n-1}(-1)^{k+1}R(k,\ell,n)\mathcal{H}_k < \sum_{k=1}^{n-1}\mathcal{H}_k$$

which was within our reach. Here $\mathcal{H}_k := 1 + \frac{1}{2} + \cdots + \frac{1}{k}$ is a *harmonic number*.

My research on the Arakelov theory of Grassmannians and flag manifolds was followed by further joint papers on their quantum cohomology rings. These exotic intersection theories are both deformations of the classical Schubert calculus. The latter is the study of the usual cohomology ring of the same spaces, and an old and rich subject. The work of twentieth century mathematicians provided algorithms to do computations in Schubert calculus, but I realized that there remained fundamental gaps in our understanding. A perplexing mystery beyond the Lie type A was the problem of *representing polynomials*, to which we turn next.

We begin with the example of the Grassmannian $G(m, n)$, consisting of all m-dimensional $\mathbb{C}$-linear subspaces of $\mathbb{C}^n$. This space has a natural decomposition

$$(3) \qquad\qquad G(m, n) = \bigcup_I X_I$$

into *Schubert cells* X_I, one for each subset $I := \{i_1, \cdots, i_m\}$ of $\{1, \ldots, n\}$ with $|I| = m$. Every subspace V in $G(m, n)$ can be represented uniquely by an $m \times n$ matrix A in reduced row echelon form with row space V. We have $V \in X_I$ if and only if the pivot '1's in A lie in columns $i_1, \ldots, i_m$. The *Schubert class* $[X_I]$ is the cohomology class of the closure of X_I. The cell decomposition (3) implies that the Schubert classes form an additive basis for the cohomology group of $G(m, n)$:

$$H^*(G(m, n), \mathbb{Z}) = \bigoplus_I \mathbb{Z}[X_I].$$

To understand the ring structure of $H^*(G(m, n), \mathbb{Z})$, it is better to parametrize the Schubert classes by integer partitions $\lambda = (\lambda_1 \geq \cdots \geq \lambda_m)$ with $\lambda_1 \leq n - m$ rather than by subsets I. The formula $\lambda_r := n - m + r - i_r$ for $1 \leq r \leq m$ gives a bijection between these two parameter spaces. If I corresponds to λ, we denote the Schubert class $[X_I]$ by σ_λ. For every integer $p \in [1, n-m]$, the class $c_p := \sigma_{(p,0,\ldots,0)}$ is known as a *special Schubert class*. Giambelli [G] was able to express a general Schubert class σ_λ as a polynomial in the special classes $c_1, \ldots, c_{n-m}$:

$$(4) \qquad\qquad \sigma_\lambda = \det(c_{\lambda_i+j-i})_{1\leq i,j\leq m}.$$

In the Giambelli formula (4), and in later equations, we set $c_0 := 1$ and $c_p := 0$ if $p \notin [0, n - m]$. The polynomials on the right hand side of (4) are algebraic representatives for the Schubert classes, and lead naturally to an algebraic model for the cohomology ring of Grassmannians.

In fact, the determinant in equation (4) had appeared in the work of Jacobi and Trudi, 60 years earlier. The symmetric functions studied by these authors were eventually named *Schur polynomials*, in honor of Schur's work relating them to the characters of the general linear group. Although this remarkable connection between the cohomology of Grassmannians and representation theory was (and

continues to be) influential, later research showed that it breaks down when one looks at more general homogeneous spaces.

In the twentieth century, the study of the Grassmannian and related symmetric spaces was enriched by writing them as quotients of Lie groups. The general linear group $\mathrm{GL}_n = \mathrm{GL}_n(\mathbb{C})$ acts transitively on $\mathrm{G}(m,n)$, and the stabilizer of the subspace $\langle e_1, \ldots, e_m \rangle$ under this action is the maximal parabolic subgroup P of invertible matrices in the block form

$$\left(\begin{array}{c|c} * & * \\ \hline 0 & * \end{array} \right)$$

where the lower left block is an $(n-m) \times m$ zero matrix. It follows that $\mathrm{G}(m,n) = \mathrm{GL}_n / P$. The Weyl group of GL_n is the symmetric group S_n, and if $B \subset P$ denotes the Borel subgroup of upper triangular matrices, then we have a decomposition

$$\mathrm{GL}_n / P = \bigcup_{w \in S_n^P} BwP/P,$$

where S_n^P is the set of permutations w such that $w(i) < w(i+1)$ for all $i \neq m$. This agrees with the Schubert cell decomposition (3) of $\mathrm{G}(m,n)$ given earlier.

The above picture generalizes to the case when G is a classical complex Lie group and P is any parabolic subgroup of G, so that the quotient space G/P is a compact manifold parametrizing (isotropic) partial flags of subspaces in $\mathbb{C}^N$. If $B \subset P$ is a Borel subgroup, then we have a cell decomposition

$$G/P = \bigcup_{w \in W^P} BwP/P$$

and a corresponding direct sum decomposition

$$\mathrm{H}^*(G/P, \mathbb{Z}) = \bigoplus_{w \in W^P} \mathbb{Z}\, \sigma_w \tag{5}$$

of the cohomology group of G/P, where W^P is a certain subset of the Weyl group of G. The *Giambelli problem* is to determine the analogue of formula (4), that is, to find canonical polynomial representatives for the Schubert classes σ_w on G/P.

Once again, the answer required a change in perspective. The most challenging and original part of this was joint work with Buch and Kresch, which examined the case of symplectic Grassmannians. Here we equip $\mathbb{C}^{2n}$ with a non-degenerate skew-symmetric bilinear form $(\ ,\ )$, and say that a linear subspace V of $\mathbb{C}^{2n}$ is *isotropic* if the restriction of $(\ ,\ )$ to V vanishes identically. We let $\mathrm{IG} = \mathrm{IG}(n-k, 2n) = \mathrm{Sp}_{2n} / P$ denote the symplectic Grassmannian consisting of all isotropic subspaces of dimension $n-k$, for some fixed $k \geq 0$. The Schubert classes σ_λ on IG can be indexed by *k-strict partitions* λ. The condition 'k-strict' means that all parts λ_i of λ with $\lambda_i > k$ are distinct, and reflects the fact that the rows of the matrices in reduced row echelon form which represent V must be pairwise orthogonal.

Using a Pieri rule for the products $c_p \cdot \sigma_\lambda$ of a special Schubert class $c_p := \sigma_{(p,0,\ldots,0)}$ with a general one, we proved that the c_p for $p \in [1, n+k]$ generate the ring $\mathrm{H}^*(\mathrm{IG}, \mathbb{Z})$, and could access the Schubert calculus there. With the help of a computer, we observed that known determinantal and Pfaffian formulas represented σ_λ in extreme cases, when all the parts of λ were at most k, or, respectively, greater than k. However, a search of the extensive literature for an operation on matrices which interpolates naturally between a determinant and a Pfaffian in the sense

required proved fruitless. To make matters worse, there was no a priori reason why there should be *any* nice formula for σ_λ, and, given the presence of relations among the c_p, there were plenty of reasons to be pessimistic. A situation all too familiar to those researching the unknown: we were groping in the dark.

Instead of the old language of determinants and Pfaffians, the answer we found to the Giambelli problem for IG employed Young's *raising operators*. Given an integer sequence $\alpha := (\alpha_1, \alpha_2, \ldots)$ with finite support and indices $i < j$, define

$$R_{ij}(\alpha) := (\alpha_1, \ldots, \alpha_i + 1, \ldots, \alpha_j - 1, \ldots).$$

A raising operator R is any monomial in the R_{ij}'s. Moreover, let $c_\alpha := c_{\alpha_1} c_{\alpha_2} \cdots$, and for any raising operator R, let $R\, c_\alpha := c_{R(\alpha)}$. The main theorem of [BKT] states that for any k-strict partition λ, we have

$$(6) \qquad \sigma_\lambda = \prod_{i<j}(1 - R_{ij}) \prod_{\lambda_i + \lambda_j > 2k+j-i} (1 + R_{ij})^{-1}\, c_\lambda$$

in $\mathrm{H}^*(\mathrm{IG}, \mathbb{Z})$, where the first product is over all pairs $i < j$ and second product is over pairs $i < j$ such that $\lambda_i + \lambda_j > 2k + j - i$.

If the partition λ satisfies $\lambda_i \le k$ for all i, then (6) becomes

$$(7) \qquad \sigma_\lambda = \prod_{i<j}(1 - R_{ij})\, c_\lambda = \det(c_{\lambda_i+j-i})_{i,j}$$

so we recover the Giambelli formula (4) in this case. The second equality in (7) is a formal consequence of the Vandermonde identity. For example, we have

$$(1 - R_{12})\, c_{(a,b)} = c_{(a,b)} - c_{(a+1,b-1)} = c_a c_b - c_{a+1} c_{b-1} = \begin{vmatrix} c_a & c_{a+1} \\ c_{b-1} & c_b \end{vmatrix}.$$

At the other extreme, if $\lambda_i > k$ for all non-zero parts λ_i of λ, then (6) becomes

$$\sigma_\lambda = \prod_{i<j} \frac{1 - R_{ij}}{1 + R_{ij}}\, c_\lambda = \mathrm{Pfaffian}\left(\frac{1 - R_{12}}{1 + R_{12}}\, c_{\lambda_i,\lambda_j} \right)_{i<j}.$$

Here the second equality follows from a classical result due to Schur.

Since Young introduced them in the 1930s, the raising operators R_{ij} made occasional appearances, notably in the theory of Hall-Littlewood functions, and in the book [M]. However, for both historical and mathematical reasons, they were hardly ever used. Our paper [BKT] was the first to show that raising operators play an essential role in geometry, in the Giambelli formulas for isotropic Grassmannians, and to employ them in their proofs. It was a lengthy process, with many ups and downs, from the moment in 2003 when formula (6) was conjectured, until our proof of it was complete. The raising operator expressions in (6), which depend on the indexing partition λ, do not enjoy the same alternating properties as determinants, and we had to learn how to work with them from scratch.

We called the polynomial on the right hand side of (6) a *theta polynomial*, and its even orthogonal counterpart an *eta polynomial*. The theta and eta polynomials are the analogues of the Schur polynomials in the symplectic and orthogonal Lie types, for the purposes of Schubert calculus, and of geometry more generally. The expository paper [T3] gives further information about this correspondence.

The work [BKT], along with several companion papers, was announced on arXiv in the fall of 2008. The following summer, I sensed that all the required tools were in

place to settle the Giambelli problem in the general case (5). I wrote the paper [T1] very quickly, during the first few weeks of July 2009. The solution combined many different strands of prior research, and also solved the problem when G/P varies in an algebraic family, to obtain formulas for the cohomology classes of *degeneracy loci of vector bundles*. The main theorems gave unique, combinatorially explicit, and *intrinsic* Chern class formulas for all the degeneracy loci involved.

I finished writing [T1] and stared in disbelief at the end result. Although it did require some new ingredients, the paper seemed almost trivial, with most proofs consisting of just a few lines. The article [T1] introduced a new, intrinsic point of view in Schubert calculus, one that came entirely naturally to me, the culmination of an understanding of the subject that had developed over many years.

We are not always so fortunate that the answers to our hardest questions turn out to be so simple. Nevertheless, I believe that simplicity is the hallmark of truth. I also began to appreciate, quoting E. Artin, that our difficulty is not in the proofs, but in learning *what* to prove. Indeed, the ability to ask the right questions is critical, and something that can only be taught by experience.

To the casual observer looking at the sequence of papers leading up to [T1], it appears as though the author had it all planned out, assembling the necessary components over time, until the final synthesis. Of course, this is completely false, as a detailed examination of the record shows. Many of the pieces were found while solving problems in Arakelov theory and quantum cohomology, not directly related to [T1]. Moreover, a large part of the work was intuitive, with some sections included in papers not because they were needed there, but because they were too beautiful to omit, and trusting there would be an application someday.

As I began my education, I was drawn to mathematics because I love truth, and sought it there. I soon realized that although mathematics is seemingly humanity's most credible attempt at finding permanence and truth, it has not reached that goal, not by a long shot. Nor does mathematics have answers yet to our deepest and most pressing existential questions. Since aquaintance with truth is the basis of all knowledge, I conclude with Socrates that in essence, *I know nothing.* Everything written below should therefore be taken with that caveat in mind.

Because I can't honestly claim to possess knowledge with any certainty, what I am left with are beliefs. Like any scientist, my beliefs are supported by evidence culled from my life experiences. The aforementioned ones are the most relevant for the purposes of this brief exposition, but many more remain unsaid.

I believe that the apparent existence and consistency of the mental structure we call mathematics is a miracle, as wondrous as the universe around us. The beautiful equations such as (1) and (6) which emerge out of our collective mind are as unforeseeable as any revelatory event. These miracles are unlike those that depend on religious faith: once seen, they can be reproduced, shared, and admired together with other human beings. However, they are far too surprising and otherworldly to be explained away as our inventions, or a product of solely rational thought.

I am under no illusion that I knew what to expect, or even that I was in the driver's seat, when [T1] was written. Like most of my research efforts, the work began with a pen, paper, and wishful thinking. Although the article soon materialized on my desk, I have no satisfactory explanation as to why all the different ingredients

required were there just when they were needed, and fit together like a charm, so that the proofs turned out to be so easy.

To paraphrase Gibran, it is not up to you to direct the course of mathematics, for *mathematics, if it finds you worthy, directs your course.* I therefore cannot in good faith take credit for [T1], or by extension any of my papers. This is not just because of the debt that [T1] owes, like all scientific research, to the many other works by various authors that preceded it. More importantly, I am convinced that the fact that [T1] was going to be written was *determined in advance.*

This is what my career has helped to teach me about the human condition: that the choices we make, consciously or not, are predetermined. Science supports the notion that our sense of self and freedom of the will are both illusory. Mathematicians are in a unique position to understand this, because their success depends on circumstances which are figments of their imagination, and yet assuredly beyond their control. Moreover, in both the research process and its fruits, mathematicians experience and perceive miracles. At the same time, as a poster in my daughter's elementary school points out, math is everywhere.

Assuming that human society can reconcile with determinism and overcome its anthropocentric posture without destroying itself in the process, what is left for the human race to strive for? Recall the importance of adopting the right perspective, and also the Mother, magnanimous in suffering and glory. In my opinion, what remains is to collectively move closer to Her point of view. This will require a humble reckoning with the truth about our endeavors, an admission of ignorance, and above all, bestowing this attitude towards life to our children, and looking to them – for forgiveness first, and, ultimately, for guidance.

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