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# Report on the present state of combinatorics

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## 1. Introduction

Thank you. I feel deeply honored to be here today. I am sure you are all wondering what I am going to say to live up to the unusual title of this lecture. As you know, such titles are agreed upon so far in advance of the actual meeting, that one is led to accept any title whatsoever, secretly believing that somehow things will work themselves out.

I should like to take advantage of the indulgence that is generously granted to those who have entered their seventh decade of age, to inaugurate a somewhat different style of address than the one we are used to in mathematics meetings.

To be sure, the temptation for me to follow the expected style of delivery of a mathematics lecture is hard to resist. The opportunity to present, before a distinguished public as you are, what one considers to be the latest and most significant results in one's own work in mathematics, is not easily passed over. But two objections to this kind of presentation should be borne in mind. Firstly, our latest results — what we in our naivete invariably consider to be the most important of our career — are unlikely to be formulated in a manner that can be followed by a mathematical audience, even the most sophisticated. It takes years of rethinking, redrafting and rearranging, before we ourselves understand the authentic significance — if any — of what we have done; more probably, it will be someone other than ourselves who will point out the relevance, if any, of the work.

Erdős has stated that no one blames a mathematician if his or her first solution of a problem is messy. One might round off Erdős's remark by adding that, in mathematics, the process of tidying up that invariably follows the first clumsy breakthrough is as indispensable as the original discovery.

Mathematicians, and combinatorialists most of all, are sometimes subdivided into two classes: problem solvers and theorizers. As is the case with all subdivisions, each of the two classes would occasionally like to believe that the other is superfluous. But we know deep in our hearts that the two kinds of activity are not only complementary,


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but indispensable to each other. No theory would exist, were it not for the initial impulse that has come from the motivation of a problem to be solved, a new jewel to be painfully wrested from the primeval mine of brute nature. But every jewel needs to be polished. New methods, new patterns of thinking are invariably discovered in the process of solution of a problem, and inevitably these methods will find their way to invaluable practical applications.

Paradoxically, we could state that the most valuable part of mathematics consists of the definitions. The learning of mathematics is largely the learning of definitions. Of course, learning the meaning of a definition is a far cry from merely grasping the statement of the definition; the sense of a definition slowly emerges as we master the theory which the definition is intended to bring to life. Such a process of learning may take years, even a lifetime.

Thus, I should like to claim the right of my seventies' decade to briefly discuss with you not the solution of any latest problem, nor even the latest definition or the latest theorem of which I may have become enamored. Instead, I should like to survey with a broad brush the present state of our field, of combinatorics. I hasten to add that this survey will of necessity be limited and biased. It is impossible today to keep track of the manifold ramifications of what only 30 years ago was a small budding branch of mathematics cultivated by a half dozen friends. It would be presumptuous for me to pretend to cover within the span of this address any but a short range of today's combinatorics. My hope is that the ground will be laid upon which others, who will address you in following years, will use as a precedent for other addresses in a similar vein. I apologize to those who will not find any mention of areas or problems which they consider to be relevant. Any such omission is due entirely to my own ignorance or carelessness, or both. I am also deliberately restricting this survey to the chapter of combinatorics that has come to be called algebraic combinatorics, with a short excursion into probability.

It was George Polya who stated that every mathematical result must be repeated three times. First, you must announce what you are going to say. Second, you must say it. Third, you must explain what you have really said.

We mathematicians have been reluctant to follow this injunction of Polya's. Much too often we fall under the spell of the results we have discovered, and we delude ourselves that their beauty will enrapture the reader or listener, and thereby exempt us from providing any kind of motivation, application or discussion of the sense of the mathematics we are presenting.

But, today more than ever, the survival of mathematics depends on our being able to carry our message as far and wide as we can. Our prejudices in favor of the standard theorem/proof presentation of mathematics are still very strong, and whoever strays into exposition, speculation or popularization is likely to be rewarded in the way our undergraduates at MIT label any course in which no formulas are used, namely, by being accused of presenting 'hot air'.

I am afraid that you are in for a few minutes of hot air. Where are we at in combinatorics? What are the problems that agitate in today's combinatorial waters? More to the point, what are the real problems behind the show case problems?

## 2. Combinatorics and algebra

The development of combinatorics in our times follows one of the leading trends that is visible in all present-day mathematics. This trend could be labeled the 'return to concreteness'. At first sight, it seems that the abstractions of the first half of this century, that culminated in the sixties with the successful developments in functional analysis, in algebraic topology, in algebraic geometry and in differential geometry, seem to have given ground to a new movement, that gives preference to specific problems, to computable algorithms, and to concrete results that appear to disregard the generalities of the past.

But I should like to claim, however timidly, that this view may be a hasty one. The relationship between combinatorics and algebra today, for example, is not a forgetting of the past.

A closer look will disclose that some outstanding work in combinatorics that is going on today is greatly benefiting the algebra of yesterday. Algebraic combinatorics has succeeded in providing non-trivial examples, fulfilling instances of a host of classical results in what used to be called abstract algebra.

The theory of toric varieties, for example, has provided new examples of varieties that might not have been generated in the process of internal evolution of algebraic geometry. Until toric varieties came along, the main class of varieties to which algebraic geometry was applied was the class of determinantal varieties and their ilk. But the theorems of algebraic geometry now find new and unexpected life in the newly added class of toric varieties, coming from an altogether different source, a combinatorial source to be sure.

What is more exciting, the discovery of toric varieties has provided a dictionary whereby results of algebraic geometry can be translated into results of convex integral geometry in the style of Minkowski and Hadwiger. An instance of such a translation has been the reformulation, discovered almost at the same time by Khovanskii and Morelli, of the Riemann–Roch theorem as a theorem in convex geometry regarding the enumeration of integral points lying in certain convex sets. Once this translation had been carried out, it became clear that the Riemann–Roch theorem could be given a purely combinatorial formulation which dispensed with algebraic geometry altogether. In so doing, Khovanskii was led to discover that the combinatorial analog of the Riemann–Roch theorem was no more or less than a multivariate generalization of the classical Euler–MacLaurin summation formula of asymptotic analysis. The discovery of such a connection between one of the central results of algebraic geometry, and one of the standard expansion theorems in the calculus of finite differences may well presage further combinatorialization of other geometric theorems, that may appeal to some of the other numerous expansions of the umbral calculus. Needless to add, I would rejoice if such speculations turned out to be even minimally true.

On another entirely different line, the recent monograph by Fulton and Lang on Riemann–Roch algebra shows how this chapter of what used to be algebraic geometry could greatly benefit by a more skillful use of the algebra of symmetric functions. In the work of Fomin, Garsia, Greene, Haiman, Hanlon, Kerber, Lascoux, MacDonald, Schutzenberger, Stanley, Stembridge and many others, this theory has acquired a depth and range that now places it at the very forefront of mathematics.

Grothendieck's beautiful definition of a lambda-ring is another idea that began in the abstract reaches of algebraic geometry, and that is proving to be fruitful in combinatorics. The recent discovery by Morelli of a lambda-ring structure for the ring of convex polyhedra introduced by McMullen is a case in point. It has been suggested by Lascoux that the algebra of symmetric functions might be enriched by what one would get by imitating the K-theory of vector bundles. Lascoux himself provided what was at the time the first example of such a situation, in his early construction of Grassmanian extensions of lambda rings. Sometime soon a combinatorial theory will come along, that will dispense with the topology of vector bundles much like convex geometry can now do without the recourse to toric varieties.

This phenomenon, of combinatorics coming to the aid of algebraic theories of long standing which suffered from a serious case of constipation in regards to examples, can be observed in several instances. Mike Artin has hinted, for example, that even the primary decomposition theory of commutative rings, a theory which was brought to abstract perfection in the twenties and thirties by Emmy Noether and her school, but which had remained as distinguished by its beauty as it was by the scarcity of concrete examples, is now receiving a much needed injection of newly minted examples from combinatorics.

Let us conclude this telegraphic survey of the relationship between algebra and combinatorics by recalling a wholly different instance of an unexpected connection. The notion of Hopf algebra was painfully abstracted from the puzzling phenomenon whereby the cohomology ring of a topological space was endowed with an algebraic structure, while the homology ring did not seem to have one. Since the time when in the Cartan Seminar the notion of a Hopf algebra was first explicitly brought to light, the formalism of Hopf algebras has been steadily winding its way into every nook and cranny of combinatorics, and the latest generation of combinatorialists has been forced kicking and screaming to learn it.

Three instances. *First instance*: the nineteenth-century work of McMahon and Hammond on differential operators on the ring of symmetric functions is now ensconced in the self-dual character of the Hopf algebra of symmetric functions. In fact, the detailed study of the self-dual Hopf algebra of symmetric functions is proceeding at the present time at full speed in the hand of Lascoux, Mendez and several others. Perhaps a Hopf algebraic formalism will be developed in connection with Schubert polynomials.

*Second instance*: the enormous variety of Hopf algebras that arise as the reduced incidence algebras of locally finite partially ordered sets provide what is perhaps the central examples of Hopf algebras having highly non-trivial antipodes, as has been shown by Schmitt. The antipodes of incidence Hopf algebras can be viewed as generalizations of the Mobius function of a partially ordered set, and provide what is probably the right level of generalization of the notion of a Mobius function. There is at least a ray of hope that incidence Hopf algebras will provide a unifying thread for the jungle of Hopf algebras that have been discovered over the years in algebraic topology.

*Third example*: the theory of linear recursive sequences has found its natural habitat in one of the classical Hopf algebras, thanks to an original idea of Earl Taft, which has been ably developed by the Sardinian school of Cerlienco, Mureddu and Piras. It may not be too wild to speculate that difference equations with polynomial coefficients will some day also be made to fit, if not in the mold of ordinary Hopf algebras, at least in a suitable extension of the concept that may be waiting in the wings. If difference equations can be dealt with by such methods of algebraic combinatorics, differential equations cannot be far behind.

Stanley has proposed a ranking of formal power series, from polynomials to rational functions to algebraic functions to solutions of differential equations with polynomial coefficients; it is tempting to speculate that his ranking will be matching by a concomitant ranking of algebraic structures.

## 3. Combinatorics and probability

The role of vitalization by unexpected examples that combinatorics is playing in algebra can be observed in probability theory as well. Let us consider two instances.

The theory of sums of independent random variables, which seemed to have achieved perfection shortly after probability came onto its own, is now displaying a renewed vitality, thanks to the finer properties of random walk that emerge when one studies random walks on finite or infinite graphs, or on groups. The connection between random walks and diffusion is being extended to graphs, and the Laplace equation on graphs, studied by Fan Chung and Shlomo Sternberg, displays combinatorial analogs of the classical Sturm–Liouville theory.

At the limit of refinement of random walk we find nowadays the theory of Mahonian statistics, which can be interpreted probabilistically, but which, we dare surmize, would never have been arrived at without the work of Foata, Gessel, Garsia, Wachs and several others. Furthermore, the work of the latest generation of probabilists, such as Aldous, Diaconis and Pitman has reopened an august chapter of probability that might probably not have renewed itself internally. The invariance theorems inaugurated with Donsker's thesis in the forties are now finding renewed vitality in the inspired limiting results of Aldous and several others.

But perhaps the outstanding contribution of combinatorics to probability to this day is Erdős's idea of establishing the existence of objects with certain prescribed combinatorial properties using a purely probabilistic existence proof leading to the conclusion that the probability that an object with such prescribed properties exists is positive. To this day, there are quite a number of combinatorial constructs whose existence can be established only by probabilistic methods.

It is amusing to note that professional philosophers of mathematics have not taken notice of the philosophical implications of such a concrete instance of non-constructivity in mathematics. It is widely conjectured that an algorithm should exist that would transform an existence proof obtained by Erdős's probabilistic method into an ordinary constructive logical proof; but to the best of my knowledge no such explicit translation algorithm has been obtained. Joel Spencer, who has extensively worked in this subject, has conjectured that a new kind of logic may be involved here.

The notion of randomness, also introduced into probability by Erdős in the case of graphs, is proving to be one of the most fertile sources of applications of mathematics. Perhaps the most striking recent work on combinatorial randomness is the discovery, made by Ron Graham and Fan Chung, that properties pertaining to, say, length of circuits, eigenvalues of the incidence matrix and the like which are true of random graphs can be used to approximate randomness; in other words, a graph whose incidence matrix has a spectrum similar to the spectrum of the incidence matrix of a random graph may be expected to share other properties of a random graph. This discovery is finding substantial practical applications in the simulation of randomness that is often required in engineering work.

## 4. Graph theory and matroid theory

The time is long past when mathematicians used to look disparagingly at graph theory. It may not be remiss to recall that the late Hassler Whitney stated (to Gleason, from whom I learned this fact) that his greatest contribution to mathematics was his theorem stating that every planar triangulation (satisfying certain trivial technical properties) has a Hamiltonian circuit. Tutte once told me that the motivation of a great deal of his work in graph theory, and even in matroid theory, was the search for a satisfying explanation of Whitney's theorem, whose original proof has not been notably simplified to this day. Nor has a satisfactory matroid generalization of the theorem been found.

Graph theory shares a unique distinction with perhaps only one other branch of mathematics, namely number theory; in both fields we find an unusually high ratio of nontrivial theorems to definitions.

The profound theory of Robertson and Seymour, establishing that the existence of certain features of graphs depend on the absence of a finite number of excluded minors in its lattice of contractions, is in my opinion one of the deepest contributions to mathematics of the latter half of this century. Robertson and Seymour's use of the notion of a partially well-ordered set, improbable as it may have once seemed, is reminiscent of another great breakthrough in the last century, i.e. the Hilbert basis theorem, which is based upon a similar lemma of Gordan, stating that the partially ordered set which is a finite product of copies of the natural integers has no infinite antichains.

The highest payoff for the new method Robertson and Seymour have introduced came last year, when the authors established the truth of Hadwiger's conjecture. To be sure, their proof of Hadwiger's conjecture relies on the truth of the four-color conjecture for planar graphs. This conjecture has been verified by Haken and Appel using a computer program. I am sure we all hope that some day an argument can be found that can be followed by mathematicians.

The idea of defining a dual object to an arbitrary graph, that in the case of planar graphs reduces to the ordinary dual graph, was the original motivation that led Hassler Whitney to develop the theory of matroids. Having written a book and some papers on the theory of matroids, I must still honestly admit that I do not know what a matroid really is. One may view matroids as a generalization of sets of points in projective space or as a generalization of the set of circuits of a graphs. The equivalence of the two definitions allows to replace linear algebra arguments by graph-theoretic arguments, and vice versa. Astonishingly, new facts of linear algebra are disclosed by taking the circuit point of view. Thus, the matroid point of view is immensely benefiting linear algebra, by providing new techniques that algebraists had not thought of.

Robertson and Seymour's theory of excluded minors goes a long way towards clarifying the deepest properties of matroids of our day. It handsomely complements the theory of varieties of matroids, developed by Kahn and Kung a few years ago. Still, the most baffling and tantalizing of matroid problems, to my mind, is the critical problem, which, if you allow me an oversimplified statement, asks for a relationship between the location of certain zeros of the characteristic polynomial of a matroid and combinatorial features, in the way of excluded minors or whatever, of the geometric lattice of the matroid. By an unlucky accident, the problem of coloring a graph was historically the first critical problem, and probably misled combinatorialists from easier instances which might have been more revealing.

Little progress has been made in our understanding of the critical problem, and one suspects that all the resources of algebra, as well as of combinatorics, will have to be put to work if we are to make a dent in it. Stanley's discovery of the interpretation of the values of the characteristic polynomial of a graph for negative values of the argument is perhaps the biggest leap forward made in the seventies, as is the discovery, also due to Stanley, that every modular element of a geometric lattice yields a factorization of the characteristic polynomial. The theory of supersolvable lattices is now the only finished chapter in the attack of the critical problem.

The reformulation of matroid theory in terms of arrangements of hyperplanes, which began with Zaslavsky's thesis and which is now drawing even algebraic geometers into the combinatorial fold, indicates a close connection between the characteristic polynomial of a matroid and some sort of Hilbert polynomial. The work of Orlik, Salomon, Terao, Ziegler and several others increasingly points to the truth of the conjecture that any insight into the critical problem will depend on the fine structure of resolutions of whatever ring structures we can associate with a matroid. There are at present several such candidates, but no sure winner as yet. Perhaps Kung's recent theory of Radon transforms on geometric lattices will further contribute to our understanding, from a more strictly combinatorial angle.

The axiomatization of arrangements of hyperplanes has recently been extended to an elegant axiomatization of arrangements of arbitrary sets of linear varieties, by Barnabei, Nicoletti and Pezzoli and independently by Bjorner, in the wake of Edmonds's notion of a polymatroid. It may well be the case that some of the problems of today's matroid theory will reveal their secrets only when looked at in this new and more proper setting.

Perhaps the most successful idea to come out of matroid theory is the Tutte–Grothendieck ring, developed by Tutte, Brylawski and by several others, too many to mention. Allow me to digress with a personal anecdote. In 1973, I was invited to deliver the Hardy lecture at Oxford, and I chose the Tutte–Grothendieck rings as the subject of the lecture. At the end, Michael Atiyah came up to me and said: "Nice stuff, but I frankly cannot see how it will ever be applied outside combinatorics", or something to that effect.

Last year, 19 years later that is, I met Sir Michael Atiyah in Cambridge, England, and I reminded him of his remark. "Now you see where the Tutte polynomial is being applied!", I added.

And indeed, from von Neumann algebras to knot theory, braid theory, and statistical mechanics, the Tutte polynomial has become endemic. Again, allow me to conjecture that those analysts, topologists and algebraists who are now busily applying the Tutte polynomial will eventually recognize actual matroids in their work behind the Tutte polynomial disguise.

The adaptation of matroid theory to other areas of mathematics is now in full swing in the work of Gelfand, Goresky, MacPherson and several others, which has led to the creation of a beautiful new theory of combinatorial manifolds, as well as to new kinds of matroids associated to each of the classical Lie algebras. Other striking applications of the theory of matroids are, first, the theory of oriented matroids, and the theory of rigidity of structures developed by Crapo, White and Whiteley, where some problems of mechanics which had been open since the past century (since Maxwell that is) have been elegantly and definitively settled.

## 5. Invariant theory

It is not a coincidence that the first combinatorialists were also invariant theorists, as the term was then understood. The names of McMahon, Hammond, Kempe, Petersen are now largely remembered because of their work in combinatorics, but their motivation came from classical invariant theory. Since nowadays the expression 'invariant theory' is used in so many senses as to have become practically meaningless, I hasten to add that by the expression 'invariant theory' I mean the continuation of the program of classical invariant theory that began in the 1840s, that came to an abrupt stop sometime in the twenties, and that in the last twenty-odd years has come back to vigorous new life.

I cannot resist the temptation to digress at this point, namely, to explain why the sentence 'Hilbert killed invariant theory', which is still sometimes sheepishly repeated today, is false. Actually, invariant theory was not killed (temporarily of course) by Hilbert, but by the joint efforts of van der Waerden and Emmy Noether. Each of them had an axe to grind against their dissertation supervisor. Emmy Noether was a student of the great invariant theorist Paul Gordan. Unfortunately, she was not able to solve the problem her thesis advisor had posed to her as a thesis problem, namely, the problem of classifying the invariants of a ternary quartic. As a consequence, she hated invariant theory for the rest of her life, and after she published the results of her thesis she made sure that the name would never appear in any of her subsequent writings. Van der Waerden was a student of General Weitzenbock. Weitzenbock was one of the first mathematicians in the Twentieth Century to understand the significance of Grassmann's work, as well as the relationship between tensor algebra and invariant theory, and some of his work was ahead of its time. He was active in the Nazi Party in the latter half of his life, and as a consequence his mathematical work became taboo reading.

There is reason to believe that Weitzenbock's overwhelming personality did not help his relation with his student van der Waerden. None of the several editions of van der Waerden's 'Modern Algebra' contains any mention of multilinear algebra or exterior algebra, let alone the word 'invariant'.

Invariant theory is intimately related to representation theory, an equally active area of combinatorics today. Central to representation theory is the construction, to be carried out as explicitly and as efficiently as possible, of the irreducible representations as well as the projective representations of the classical groups. The main tools of combinatorial representation theory are the theory of symmetric functions and their generalizations, such as Schubert polynomials, as well as various versions of the classical Schensted algorithm. Only two generalizations of the Schensted algorithm shall be mentioned here, because of lack of time. The first is the supersymmetric generalization of the plactic monoid, recently carried out by Bonetti, Senato and Venezia; the second is perhaps one of the most beautiful theorems inspired by the Schensted algorithm, namely, the theorem independently discovered by Fomin and Greene for finite partially ordered sets.

There is however a difference not only of style but of substance between representation theory and classical invariant theory. One way to visualize this difference is by analogizing it with the difference between probability theory and measure theory. One can stare all of one's life at measurable functions, without ever discovering the normal distribution. Similarly, one can stare all of one's life at the representations of the general linear group, without ever discovering the invariant-theoretic solution of a cubic equation.

Four basic ingredients make present-day classical invariant theory a promising area of combinatorics, to wit:

1. Richard Feynman had the genial idea of representing monomials in noncommutative algebras by replacing each variable by a pair of variables, the first being the original variable, and the second designating the place occupied by the variable in the given (noncommutative) monomial. In this way, the pair of variables can be viewed as a single variable, now called a letterplace. Letterplaces generate a commutative ring called the letterplace algebra, and problems of noncommutative algebra can be recast as problem in commutative algebra using the letterplace algebra. The use of letterplaces has turned out to be useful in invariant theory, together with fundamental straightening algorithm that may be viewed as a multilinear algebra analogs of the Schensted algorithm. Again, I cannot refrain from telling you another story. I met Feynman for the last time at the firm 'Thinking machines', on the occasion of the inauguration of the first connection machine. Most of the young computer scientists working at 'Thinking machines' had at some time or other taken my course in probability at MIT, and my being invited to the inauguration ceremony had therefore become a statistical certainty.

I mentioned to Feynman that I had used his idea in several papers. Immediately he left the cluster of reporters who were interviewing him and took me to a corner. "I am glad to hear that, because I consider time-ordering to be the best idea I have ever had, better than the Feynman integral", he stated in no uncertain terms. He then proceeded to explain to me another idea of his, which he had never published, and which he sketched on a piece of paper the size of a postage stamp. I put the piece of paper in my pocket, thinking I would study it later. To my great chagrin, it somehow slipped out of my pocket before I could look at it again, and since that time I have been wondering what Feynman's last idea was about.

2. The straightening algorithms of letterplace algebras are enriched by the introduction of supersymmetric variables, i.e. by replacing exterior algebras by tensor products of exterior algebras and divided powers algebras. Such a mixture of commuting and anticommuting variables has long been used in physics. However, two powerful ingredients have recently been added. The first is polarization of positive variables into negative variables, an idea developed by Andrea Brini; the second is an umbral operator that allows the representation of invariants of skew-symmetric tensors by polynomials in a commutative algebra.

These new techniques have led to the solution of a number of classical problems, as well as to simplifications in the representation theory of the classical Lie algebras, in the able work of Brini, Grosshans, Huang, Stein and several others.

Brini's simplification and extension of the classical Gordan–Capelli expansion, made possible by the language of supersymmetric algebra, leads us to hope that this fundamental expansion will soon take its rightful place side by side with Taylor's formula as part and parcel of the baggage of ordinary mathematicians.

3. Again, the use of straightening algorithms with supersymmetric variables has made possible the solution of an old standing problem, i.e., the extension to arbitrary characteristic of the projective resolution of Weyl and Schur modules, first obtained by Lascoux in characteristic zero. Although Buchsbaum and I have so far published the method in the rather simple case of two-rowed Young diagrams, it seems clear that the method is going to work in general.

4. Finally, it may not be remiss to counter the prophets of doom, who might be inclined to dismiss as intractable all invariant-theoretic classification problems that are not proved 'tame'. I should like to call your attention to the notion of perpetuant, a generalization of the notion of invariant that was only partially developed in the last century. A good half of the second volume of McMahon's collected papers is dedicated to the study of perpetuants of binary forms, and to their complete classification. McMahon and Stroh showed very clearly how among all invariants of a binary quantic only perpetuants have an interpretable significance in terms of geometric, combinatorial or algebraic properties of the quantic. It seems reasonable to conjecture that, whereas the classification of invariants of a quantic is not in general tame, the classification of perpetuants will turn out to be tame. I am happy to announce that a precise definition of the notion of perpetuant for arbitrary quantics has at last been given. It can be found in Frank Grosshans's latest paper, which appeared only last month.

## 6. Species and bijective combinatorics

The notion of species, introduced by Andre Joyal just over ten years ago, and brilliantly developed by the Quebec school, was a decisive step in the systematic program of making combinatorics bijective. Briefly, one wishes to work with the objects themselves and with operations performed on objects, rather than with derived constructs such as generating functions.

The resistance that is still put up against adopting the language of species reminds me of two other similar resistances. The first was the introduction of random variables to replace probability distributions, a revolution that was just about over in the early fifties. Some notable mathematicians clung to distribution functions, claiming that the notion of a random variable was superfluous. One such mathematician was Aurel Wintner of Johns Hopkins University, who wrote a treatise of probability in which the notion of a probability distribution was exclusively used. In this treatise, several important results were proved, that were later to be rediscovered in the language of random variables. However, Wintner did not receive any credit for his contributions.

The second instance is the introduction of the theory of distributions of Laurent Schwartz, early in the fifties. For years one could hear the trite objection "What can you do with distributions that you cannot do without them?", an objection that serves only to lay bare the objector's ignorance of the way mathematics progresses. In the end, however, those who refused to go along with the new and superior notation were cast aside.

The language of species is at present being considerably enriched, and our notion of what constitutes an acceptable bijective proof is gradually being enriched. For example, it seems clear, after futile attempts lasting several years, that a bijective interpretation of Schur functions is not to be had in the naive interpretation of bijectivity. The notion of bijectivity will have to acknowledge some construction that corresponds to what homological algebraists call a resolution, and the role of the minus sign in bijective proofs will have to be understood. The theory of Mobius species of Mendez and Yang is a step in such a direction; perhaps one of the next steps will be a bijective understanding of the semisimplicial world, that has met with such a striking success in re-combinatorializing algebraic topology.

Some of the outstanding successes to date of the theory of species are:

1. the bijective formula, obtained by Ehrenborg and Mendez, for the plethystic inverse of a formal power series;
2. Gilbert Labelle's recent theory of acyclic enumeration.
3. The bijective interpretation of orthogonal polynomials (Viennot, Foata, Gilbert and Jacques Labelle, and others), which cries for the language of species.

A good test for the effectiveness of the theory of species will be whether or not it succeeds in producing a simpler bijective proof of the Rogers–Ramanujan identities than the one given by Garsia and Milne. I am willing to bet such a proof will be given within a short time.

## 7. Special functions

Early in this century, the Reverend F.H. Jackson, a British mathematician whose name is seldom mentioned, spent all his life deriving $q$-analogs of formulas of classical analysis. He found, for example, a $q$-analog of integration, now recognized as the right kind of integration for $p$-adic fields, and a $q$-analog of the gamma function, later rediscovered by better-known mathematicians, who got all the credit. I wonder what the Reverend would say if he watched the present craze for $q$-analogs and quantum groups.

Quantum groups, which, to be honest, are neither quantum nor groups, are the first example of a Hopf algebra which is neither commutative nor cocommutative, and which nevertheless allows the standard constructions that one expects of groups. I am told by Victor Kac that $q$-analogs are the only possible deformations of the group algebra of the general linear group; this fact alone should keep $q$-analogs alive.

Quantum groups are at present the melting pot of some of the most promising ideas of combinatorics: braid groups, the Yang–Baxter equations, and $q$-hypergeometric functions. We can foresee in the theory of quantum groups a profound extension of the notion of group representation in which both groups and representations may be forgotten, but what takes their place will unquestionably deserve it.

In the field of special functions, as elsewhere in combinatorics, we are confronted with too many nontrivial identities and too few concepts with which to understand them. The ultimate test will be whether the theory of quantum groups will fulfill the long-standing promise of discovering a unified theory of hypergeometric functions and basic hypergeometric functions.

Perhaps the physicists who are now working on the quantum theory of angular momentum, led by Biedenharn and Louck, have discovered the key to relate hypergeometric functions with representation theory. The fine points of representation theory that began with the Racah–Wigner algebra, and that are now being generalized to $n$ dimensions by physicists inspired by the mystique of quantum mechanics, is one area of representation theory that we in combinatorics have unjustly ignored.

It is remarkable what new life the field of special functions has been given by the advent of sophisticated computer programs like Mathematika. And to think that in the early days of computer science there were mathematicians who predicted the death of special functions after the advent of the computer!

It is impossible here to do justice to the numerous trends that are now current in the theory of special functions. I am, for example, forced by time limitations to omit all mention of the combinatorial relevance of elliptic functions, as well as of the remarkable discoveries of Gelfand and his school, Gessel, Gulden, Jackson, Stanton, Zeilberger, and several others. There is one aspect of the theory, however, on whose importance everyone agrees, and that is the role of positivity. It seems that some of the deepest identities that have been obtained over the years and that are being obtained now are those identities that provide expansions with positive coefficients. We need hardly be reminded of the fact that one such expansion in positive coefficients, due to Askey and Gasper, was a crucial step in De Branges's proof of the Bieberbach conjecture.

Positivity, monotonicity, and unimodality problems have always haunted combinatorics, and to this day we do not have general enough methods to attack them, though some courageous younger mathematicians like Brenti are bravely battling with them. It would be interesting, for example, to obtain a combinatorially oriented proof of Edrei's structure theorem for totally positive matrices, perhaps using continuous analogs of Young diagrams, as Vershik has suggested. And I need hardly remind you of the oldest unsolved positivity problem, which my teacher William Feller used to call the crying shame of mathematics, namely, a characterization by simple inequalities of the cumulants of a random variable. Despite its probabilistic tone, this is really a problem in invariant theory, as Thiele, who first proposed it, had already realized.

## 8. Other directions

Of the great many subjects which must of necessity remain untouched, I will try to give a brief synopsis. Brief as my mention must of necessity be, these problems are no less important than the ones discussed at greater length above. Quite the contrary: it is in these primordial combinatorics problems that the life of combinatorics lies.

1. Convex polytopes. The analysis of $f$-vectors and $h$-vectors of convex polytopes and of triangulations was initiated by Stanley, who obtained some of the most beautiful results of extemal combinatorics, by settling the upper-bound and the $g$-conjecture; another instance in point is the work of Bjorner and Kalai characterizing simplicial complexes having given $f$-vectors and given Betti numbers, which uses some clever computations with exterior algebra.

In another direction, Harper's extremal combinatorics, extending to the cube results that were known for simplices, establishes an unexpected link between finite extremal problems and the classical calculus of variations.

2. Ramsey theory, in the able hands of Graham, Leeb, Rothschild and many others, has reached such a degree of perfection that even philosophers are taking notice, and using it in their speculations on the origin of order from chaos. Shockingly enough, we are still missing a probabilistic proof of Ramsey's theorem. The bounds, useful as they are, keep going in the wrong direction.

3. The combinatorics of finite lattices has made great strides in recent years; suffice it to mention Gelfand's pioneering works on the fine structure of the free modular lattice, Haiman's proof theory for lattices of commuting equivalence relations, and the theory of two-distributive lattices of Herrmann and Wild, which extends in a wholly unexpected direction Birkhoff's classical theorem on the classification of finite distributive lattices, and Bruce Sagan's enumerative studies.

4. Universal algebra. Ternary operations, and operations of higher arities, have been ominously absent from classical algebra, and until a short while ago such operations seemed to be oddities devised by perverse universal algebraists.

Let me tell you another story. The late Emil Post worked for several years to develop an elaborate theory of multigroups based upon $n$-ary operations. After reading the galleys of his two-hundred page long paper, which appeared in the Transactions of the American Mathematical Society, he realized to his chagrin that his basic $n$-ary operation could be expressed by a concatenation of binary operations. He hastily sent in some footnotes to be added in page proofs. I do not believe anyone has ever read Post's paper, which is in many ways remarkable.

But now the tables are turning, and operations of arbitrary arities may soon come to the fore not only from computer science, but even in the most conservative chapter of algebra, namely, in the theory of representations of the general linear group. Recent combinatorial techniques introduced into invariant theory make it very likely that we will at last reach an understanding of the new algebraic structures that may lurk behind tensors of arbitrary symmetry classes. Such structures are very likely to require operations of higher arities, and they will inject further life into universal algebra, much like Clifford and Heisenberg algebras injected new life in symmetric and skew-symmetric tensors.

Techniques of universal algebra are also coming to the fore in the theory of combinatorics on words initiated by Schutzenberger, in the classification of varieties of semigroups. And lastly, Thurston and Conway have reduced the study of tilings in the plane to decision problems in combinatorial group theory.

5. Let me stick out my neck, and submit to you that an algebraic structure that will play a pivotal role in algebraic combinatorics in the next few years, is the notion of resultant. Long neglected in favor of abstract methods, resultants are now beginning to flourish, in the hands of Gelfand, Jouanolou, Zelevinsky and several others. Much of the lowly spadework that made determinants into household tools remains to be done with resultants, and there is no doubt that new combinatorial applications are in store, as they were when determinants came of age. Perhaps a new matching theory can be gotten out of resultants, if you allow me a wild conjecture.

6. A great many of the conjectures on finite geometry made by Beniamino Segre still remain open. Some of these conjectures rank in beauty and depth on a par with the Weil conjectures of number theory, and are providing a strong motivation for the intensive work of the school of finite geometers in Italy, Belgium, Britain, Germany and Canada.

7. Combinatorics is distinguished among all mathematical endeavors by the enormous variety of deep but elementary-sounding problems. Some such classical problems are the postage stamp problem, the enumeration of finite distributive lattices, self-avoiding random walk, the hard spheres problem, the enumeration of chains in partially ordered set with elements at specified levels, the extension to Coxeter groups of the fine theory of the symmetric group, the never-ending surprises provided by Young diagrams, and so on too many to mention and most of all too late to mention, because my time is up, and I must thank you for patiently following this lengthy tirade. Thank you.

Florence, 23 June 1993