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Combinatorics with emphasis on the theory of graphs, by J. E. Graver and M. E. Watkins, Springer-Verlag, New York, Heidelberg, Berlin, 1977, xv + 351 pp., $24.00.

The book by Graver and Watkins is one of the two most ambitious accounts so far attempted in the field of combinatorial mathematics (the other being the two-volume *Kombinatorik* by M. Aigner, also recently brought out by Springer). Its publication is a notable event which affords the reviewer an opportunity to clarify his own ideas and to record his impressions of the present state of combinatorics. The reader of this notice will, I trust, bear with me if I do not plunge at once *in medias res* but try to place my conclusions in a fairly broad context. It is surely unnecessary to add that what I shall say has no claim whatever to originality–my sole purpose is to gain a reasonable perspective of the topics treated in the book under review.

Even a sleepy observer of the contemporary mathematical scene cannot but be struck by the spectacular growth of combinatorial studies in the very recent past. As little as, say, twenty years ago enthusiasm for work in this field was still regarded as a sign of mild eccentricity, and the problems investigated by combinatorialists were nowhere near the centre of the mathematical stage. Thus the late J. H. C. Whitehead probably did no more than express a tacit consensus when he described the theory of graphs as 'the slums of topology'. Nous avons changé tout cela–decisively and rapidly. Few universities anywhere in the world now fail to offer instruction in one or other aspect of combinatorics; several flourishing journals are wholly devoted to this discipline; and the number of conferences, of books, of papers, of graduate students, and of qualified practitioners has increased and is increasing at a rate which is probably a second order exponential. The reasons for this startling phenomenon are nevertheless not too dificult to discern. For one thing, combinatorial *methods* (as distinct from combinatorics as a *subject*) have naturally always constituted a vital ingredient of mathematical reasoning. It is therefore hardly a matter for surprise that, at some stage in the development of mathematics, a progressively conscious attempt should have been made to identify and isolate the specifically combinatorial arguments and to weld them into a coherent discipline (or range of disciplines). That this attempt should have got under way only recently rather than, say, fifty years ago is undoubtedly due to a plethora of questions of a combinatorial character thrown up in the last quarter of a century by subjects oriented towards practical applications, such as operational research, statistics, information theory and, above all, modern computer science. The avalanche, having once been set in motion, has been accelerating under its own momentum: every mathematician tends to transmit his own preoccupations to his pupils and colleagues and every paper that appears in print tends to provoke further research. Moreover, the change is not purely quantitative; when a collection of problems and methods reaches a certain degree of coherence, it assumes the character of an *autonomous* study which need not look for inspiration to extraneous sources. And finally, if a sufficient number of really able mathematicians devote their energies to a certain range of questions, there is every likelihood that their findings will become a focus of general mathematical interest. So with combinatorics: it is obviously no longer to be shrugged off as being merely of marginal significance–its presence is too strongly felt and its influence too pervasive for the continuation of Whitehead's dismissive stance. Indeed, every one of a score of outstanding mathematicians who have contributed crucial ideas to the recent development of combinatorics could assert with rightful pride: *exegi monumentum*. Voices have even been heard insisting on the fundamental role of combinatorics for the entire body of mathematical knowledge. Claims as far-reaching as these seem to me excessive; but it is symptomatic of the current prestige of the subject that they should be made at all.

If we are to gain a clear picture of the nature of recent advances in combinatorics, we would do well to view them against the background of advances (say over the last 70 or 80 years) in other areas of mathematics. Once our intention has been formulated in these terms, we readily perceive a common pattern. The initial push is, as a rule, a messy and uncoordinated affair: the objectives have not yet been identified with any precision; there are no standard methods and techniques; individual mathematicians work in comparative isolation; and their conclusions inevitably exhibit both partial overlaps and differences of approach. However, when the bulk of available information reaches a critical mass, a qualitative transformation–sometimes slow and sometimes very rapid–takes place. The workers in the field become fully conscious of each other's efforts and eventually find themselves first groping and then striding towards a common goal. In this way a hotch-potch of loosely connected results turns into a study that before long acquires the characteristics of a systematic discipline. And at this point the fact that our grasp of the material has become sufficiently firm is often made manifest by the appearance of a comprehensive treatise which codifies the subject and gives it the authentic stamp of unity–an event which itself contributes decisively to subsequent progress. The crooked having now been made straight and the rough places plain, further work can be carried out in a much more effective fashion along clearly marked lines. The orderly advances then continues until some fresh upheaval disturbs the transient balance–possibly a merger with another discipline or the subordination to some more fundamental study (as in the take-over of algebraic geometry by algebra in the thirties and forties). The established framework is then shaken, but the Time of Troubles is brought to an end with the expected emergence of a new Universal State; and then the pattern of change described a few lines earlier is likely to repeat itself (at a higher level of sophistication). Such, in brief and allowing for local variations, seems to me to be the dynamic of advance in modern mathematics.

A few familiar examples might usefully serve to illustrate the pattern I have indicated. General topology is an obvious instance (and is, in fact, cited by Graver and Watkins). Towards the end of the nineteenth century, it was dimly perceived that analysis required a broader framework than was at that time available. The first step was the utilization of the concept of an n-dimensional euclidean space. This notion owed its existence more to geometry and algebra than to analysis; it first appeared explicitly in Grassmann's *Ausdehnungslehre* (first ed., 1844; second ed. 1862) though not in a form which it is easy to recognize today. Analysis made effective use of this concept, but this was not yet enough. The next stage was reached with the transition to metric spaces (Fréchet, 1906), in which the notion of distance is given an axiomatic form. Yet a still more general structure was needed to underpin a comprehensive discussion of analysis; and what emerged after innumerable experiments and trials was the concept of a 'topological space', where the basic object is the collection of open sets. The entire development took something like fifty years and, during the twenties and thirties, the contributions of the great Polish school proved decisive. By about 1940, the subject as we know it was, in all essentials, complete (although the most familiar book embodying the final conclusions, J. L. Kelley's *General topology*, did not appear till 1955).

Another instance is furnished by functional analysis. Here we study certain abstract 'spaces' endowed with a structure which generally has both topological and algebraic features. Naturally, these spaces are chosen so as to reflect the most interesting properties of various classes of functions. By special interpretations of the abstract entities, we can recover the theorems of classical analysis and at the same time note that the proofs become much more transparent in the general setting and that the same argument can often serve to prove several theorems that previously required separate approaches. This, of course, is only the starting point: very soon functional analysis reaches a sufficient degree of coherence to generate its own problems and its own methods. The beginnings of the subject might reasonably be assigned to the work of Volterra in the 1880's, but the strongest impulse derived from Hilbert's assault on linear integral equations some twenty years later; and the most effective tool for the applications to 'concrete' problems was provided by the Lebesgue integral. Naturally, the advance into new territory took place simultaneously on many different fronts, and the total effort needed to achieve any sort of systematization was prodigious. An early classic that must be mentioned is Banach's *Théorie des opérations linéaires* (1932). This is a fairly short but astonishingly 'modern' work, and although the subject has since grown to vast proportions, Banach's book already exhibits nearly all the distinctive features that we associate with the term 'functional analysis'. By now the available material is much too extensive to be confined between the covers of a single book, and I merely note the most outstanding post-war study that covers large tracts of functional analysis: the three volumes of *Linear operators* by Dunford and Schwartz.

My final, and possibly most telling, illustration is taken from algebra. Since the nineteenth century this subject had, in an even greater measure than other parts of mathematics, undergone a process of increasing abstraction and generalization; and the work of Dedekind, Hilbert, Steinitz, Emmy Noether, Artin, and others had resulted in the creation of a large body of knowledge incapsulating classical algebra but conceived in a fully axiomatic spirit. The new approach found its most complete and systematic expression in van der

Waerden's *Moderne Algebra* (2 volumes, first ed. 1930–1931). Here all the many strands are woven together into a tightly knit pattern; and with the exception of the two *Elements*, ancient and modern, van der Waerden's work is probably the most influential text-book ever written–it summarized the achievements of a lengthy process of development and became the manifesto of a new era in algebraic investigations. The axiomatic mode is now an integral part of our way of thinking, and it is difficult to remember that such has not always been the case. The contemporary habit of mind has been shaped, at least in part, by the impact of van der Waerden's presentation. Needless to say, a great deal of the algebra of our own day (such as homological methods and category theory) is absent from van der Waerden's treatise. For all that, his masterpiece remains a fount of living knowledge rather than a venerable monument.

So much, then, for general remarks on the phases through which mathematical theories often pass in the process of becoming fully articulated. Turning now, more specifically, to combinatorics one may as well begin, quite artlessly, by asking: what precisely constitutes this subject? The question does not admit of a very satisfactory answer (and neither do analogous questions relating to other areas of mathematics). As I see it, combinatorics is a range of linked studies which have something in common and yet diverge widely in their objectives, their methods, and the degree of coherence they have attained. Most are concerned with criteria for the existence of certain 'patterns' or 'arrangements' or 'configurations', where these terms need to be interpreted in a very broad sense. Further, in many problems, the emphasis is on quantitative aspects: we seek to determine or estimate the number of objects of a specified kind or to characterize the patterns which are, in some way, extremal. Now the expert can live a happy and useful life without the aid of my fumbling attempts at elucidation while the mathematician unfamiliar with combinatorial problems will not derive from them any perceptible enlightenment. By the shades of Euclid and Aristotle! I have evidently not supplied much of a definition–but is there a better one?

Let us not stay for an answer but rather turn to the more profitable task of surveying, however superficially, some of the theories that between them make up what is now known as 'combinatorics'. The most intensively cultivated area of combinatorial investigations is undoubtedly the theory of graphs. Here individual questions have a long history: we need merely recall Euler's problem of the Königsberg bridges or Cayley's determination of the number of 'labelled trees on n vertices'. However, the creation of the theory of graphs as a unitary discipline is the achievement of Dénes König, of whose *Theorie der endlichen und unendlichen Graphen* (1936) all the numerous recent books on the subject are recognizably lineal descendants. König himself applied the theory of graphs to derive certain results on matrices (and thereby provoked an ill-tempered and ill-judged outburst on the part of Frobenius). History has vindicated König: the theory of graphs has found many applications in other parts of mathematics and, in particular, its use in the study of nonnegative matrices is now a standard technique. During the past decade, objects more general than graphs, namely hypergraphs, have made a strong bid for supremacy. Again, within the realm of the theory of graphs, various conglomerations of problems have gradually acquired an unchallenged title to local autonomy. Thus an exploitation and quantification of ideas inherent in Menger's 'separation theorem' (1927) led to the development of 'network theory', formulated by Ford and Fulkerson in their *Flows in networks* (1962). Again, Turán's remarkable paper of 1941 (in which it is proved, inter alia, that a graph on n vertices which has at least $[n^2/4] + 1$ edges must contain a triangle) inaugurated the study of extremal graphs, a luxuriantly flourishing branch of the tree of graph theory from which Paul Erdös has plucked the most attractive blossoms. I might also, in passing, refer to the equally productive extremal theory of finite sets; these investigations, which sprang from a simple but arresting theorem due to Sperner (1928), currently enjoy the attention of a large number of mathematicians (among them Erdös, Katona, Kleitman, and Lovász).

Extremal problems (and optimization problems) in combinatorics are, in fact, strewn as thick on the ground as the autumnal leaves in Vallombrosa. Many results assert the equality of a maximum and a minimum, and it is therefore hardly surprising that the duality theorem of linear programming should have been found an effective tool in combinatorics. Here the subject owes much to A. J. Hoffman, H. W. Tucker, and D. Gale and, more recently, to D. R. Woodall. I do not believe, however, that the precise nature of the links between linear programming and combinatorial mathematics is as yet fully understood; to me, at any rate, there is still darkness at the heart of the matter.

Next, I must mention transversal theory and matching theory. (The two terms are not entirely synonymous but cover, in part, the same ground.) This line of investigation runs straight from Philip Hall's theorem (1935) on 'distinct representatives'. (There were earlier theorems close to it in substance though not in formulation.) Transversal theory is, in essence, the study of results based on and extending Hall's theorem. By about 1965, it became evident that such results are most appropriately discussed in the context of matroid theory. The notion of a 'matroid' had been introduced and subjected to a profound analysis by Hassler Whitney in 1935. However, the initial impact of Whitney's ideas was almost negligible. The insight that made possible the accomodation of transversal theory in the more general setting and so gave it an added dimension came from an early and very remarkable generalization (1942), due to R. Rado, of Hall's theorem. Rado's work, like Whitney's, was to all intents and purposes ignored for a long time; but, with the wisdom born of hindsight, we must acknowledge it as the birth certificate of *modern* (i.e. post–1965) transversal theory–an unusual birth certificate, admittedly, seeing that it was issued twenty odd years before the birth. By now it is clear that the matroid structure and its subsequent extensions constitute the really significant objects of study–within this context transversal theory is a detail. Matroid theory is at present unmistakably a growth area in combinatorics; in this sphere we owe the most decisive contributions to W. Tutte.

Another large and steadily growing component is known as the theory of 'combinatorial designs'. The genesis of this topic lies in the need to devise and assess the significance of statistical experiments, an area of work in which the strongest initiative came from the British geneticist R. A. Fisher. Since its beginnings (in the early twenties), this study has naturally both expanded and deepened; it has assimilated many individual results (e.g. on Latin squares, Hadamard matrices, and Steiner triples) into a more comprehensive theory; and it has established firm contacts with other subjects, such as projective geometry. A very interesting and much more recent line of inquiry is 'coding theory', whose origin and purpose are indicated sufficiently by the nomenclature, and which is closely associated with the analysis of combinatorial designs.

The most vital and exciting aspect of modern combinatorics is possibly the research stimulated by a famous theorem of F. P. Ramsey (1930), a theorem which possesses both finite and infinite variants. (Earlier but narrower results of the same general type had been given by Schur in 1916 and by van der Waerden in 1927, but they were not known to Ramsey.) Ramsey needed his theorem in the strictly limited context of the study of a logical decision problem, and the long-term consequences of his work could not have been foreseen by anyone–but such is often the stuff of the very best research. The mathematical community, with a few notable exceptions, did not recognize the potentialities of Ramsey's theorem for a long time. In the late forties, Rado and Erdös mounted a systematic and determined attack on problems suggested by the infinite form of Ramsey's theorem. This work resulted in the creation of the 'partition calculus', essentially a department of set theory which, in particular, raises interesting and difficult questions on the frontier between 'ordinary' mathematics and mathematical logic. Thus, certain problems in the partition calculus can only be settled on the basis of the continuum hypothesis or the generalized continuum hypothesis. (Perhaps future research will be directed to establishing whether the solution of some as yet open problems requires, say, the axiom of constructibility or the axiom of determinateness.) A methodical and lengthy exposition of the findings of Rado and Erdös appeared in this *Bulletin* in 1956; with this publication, the partition calculus was well and truly launched. The finite form of Ramsey's theorem was slower to gain detailed attention but, in the last ten years, the subject has sprung suddenly into feverish life; and hundreds of variations have, probably, by now been composed on this theme. The sum total of the resulting knowledge has come to be known as 'Ramsey theory'. Put in extravagantly imprecise terms, theorems in this field have something like the following form: if S is a certain type of system composed of a *sufficiently large* number of elements, and if the set of these elements is partitioned arbitrarily into (a finite number of) classes, then S possesses at least one 'subsystem' all of whose elements belong to the same class. The relevance of such a result for graph theory is fairly plain; and in every case one naturally wishes to know how large is 'sufficiently large'. The reader will have guessed that many problems which emerge have a quantitative aspect–the determination of a 'Ramsey number'. This task is almost invariably extremely hard and comparatively few general methods are yet available. Possibly the most promising feature of Ramsey theory is the fact that, whereas Ramsey's original theorem deals with sets plain and unvarnished, a number of results have been proved in recent years involving sets which carry a structure (say that of an abelian group or a vector space). The conceptual enrichment of the theory achieved in this way augurs great things for the future. The present material, though extensive, is still largely unorganized. However, it is an open secret that two American mathematicians propose to give a systematic exposition of Ramsey theory in book form. I, for one, am looking forward to this publication with an impatience which I make no effort to conceal.

My rough inventory of combinatorial topics would be certainly incomplete if I failed to refer to enumerative analysis. In essence, this goes back to Euler (or, perhaps, even to Leibniz) and is concerned with the determination of the number of objects of a prescribed type. The formal manipulation of generating functions–usually power series–played a dominant role in classical research in this field; this method was given a definitive shape in the two volumes of MacMahon's *Combinatory analysis* (1916). Naturally, the subject has not stood still since. More recent writers, in particular Pólya and after him de Bruijn, introduced new devices based largely on ideas drawn from the theory of groups. At the same time, methods both from asymptotic analysis and from the theory of probability were pressed into effective service.

My remarks convey (I hope) some impression of the great diversity of combinatorial problems, but the impression can hardly be adequate since almost every topic has fully-fledged sub-topics. (For example, I did not even mention the theory of 'tournaments', a special class of graphs whose properties have been explored pretty thoroughly.) Moreover, many questions in combinatorics have two aspects, finite and infinite; and the approaches in the two cases are often quite different. However, it is not diversity and richness alone that characterize modern combinatorics, but equally the fact that the subject has now been brought into close relation with other mathematical theories. The relevance of linear programming, of projective geometry, and of probability theory has already been noted. But, above all, modern combinatorics draws on a whole range of resources from algebra; and groups, finite fields, partially ordered sets, Boolean algebras, lattices, vector spaces, and categories have all been harnessed successfully to the task of establishing combinatorial results. The forging of so many links is itself an index of the growing maturity of the subject.

And now, at long last, I come to the book by Graver and Watkins. The authors disclaim any intent at encyclopaedic coverage and, in particular, they only concern themselves with finite systems. Their aim is not to treat every significant result in (or even every significant area of) combinatorics but rather to present the topics of their choice as parts of a unified and coherent discipline. In short, they seek to achieve in the domain of finite combinatorics what van der Waerden had done with such superb aplomb for algebra. And this inevitably raises the question whether the 'subject' (I use inverted commas so as not to prejudge the issue) is ripe for such treatment. In a strictly literal sense the answer is, of course, 'No'. Combinatorics is far too wide-ranging a study to allow, in its entirety, a genuine unification. Even as well-established a range of theories as those that constitute analysis would fail this test. We must therefore judge the enterprise of Graver and Watkins by less comprehensive standards: are the topics discussed *by them* sufficiently developed to permit inclusion within a unified framework? My own tentative feeling–an instrument of questionable reliability–prompts me to express some doubt. However, an a priori judgement on the feasibility of a project is almost pointless if the results of the project are available for inspection. It is to them we need to turn.

Graver and Watkins are very clear in the statement of their objectives, and in the preface to the book they indicate the spirit in which they approach their work by quoting G.-C. Rota's dictum that 'combinatorics needs fewer theorems and more theory'. (For myself, I find the dichotomy set up by this formula a little too sharp: in some cases, though naturally not in all, 'more theory' will follow as a consequence of '*more* theorems'. Still, one sees what Rota has in mind; and to quarrel too insistently with his way of putting it would show a spirit at once pedantic and captious.) To the task of codifying combinatorial mathematics, the authors bring impressive learning, an exceptional ability to mould their material, and rare expository skill. They cover a wide spectrum of topics, and they are undoubtedly right to build their conceptual framework primarily with an eye to the theory of graphs. The result of their labours is a remarkable book. It does not, to be sure, make easy reading–one needs to be determined and persevering to get the best out of it. But the reader who has succeeded in mastering the technicalities will find that his investment in time and trouble pays a handsome dividend.

I do not propose to analyse in detail the structure of the book; but a brief indication of its contents must, of course, be given. Chapters 1 and 2 develop the (quite formidable) set-theoretic and algebraic preliminaries. As the authors had already explained in the preface, the basic 'system' which is to underlie their discussion of combinatorics is a triple, consisting of a set (of 'vertices'), another set (of 'blocks'), and a mapping which assigns to each block a subset of the set of vertices. The source of this structure in graph theory is evident, and Graver and Watkins demonstrate convincingly that extended areas of combinatorics which lie outside graph theory can nevertheless be presented within the same basic framework.

As the title of the book indicates, the main weight of the discussion rests (rightly) on graph theory, to various aspects of which Chapters 3–7 are devoted. The climax of Chapter 3 is (essentially) Kuratowski's criterion for planarity of graphs. Chapter 4 develops the network theory, and here the max-flow min-cut theorem of Ford and Fulkerson has naturally pride of place. In Chapter 6, separation and connectivity are studied. Chapter 7 deals with 'chromatic' problems, i.e. problems which arise when the set of vertices, or the set of edges, is partitioned into disjoint classes (such that two connected vertices or two incident edges belong to different classes). Chapter 5, which is concerned with matching theory, is of a more mixed character; in addition to strictly graph-theoretic results, there is discussion of Philip Hall's theorem, of combinatorial properties of matrices, and of other related topics. Chapter 9 is devoted to combinatorial designs and Chapter 11 to matroids.

All the topics mentioned in the preceding paragraph are fully integrated into the basic scheme of the book, although occasionally the authors feel themselves compelled to adopt Procrustean tactics: for example, Dilworth's decomposition theorem (1950) is presented under the auspices of network theory, and the proof, even after all the preliminaries have been disposed of, runs to two pages; there exist, of course, much brisker ad hoc demonstrations. Yet, in the main, the authors' strategy is very successful in relation to the material considered so far. Two chapters seem, however, to stand largely outdside the scheme. Chapter 8 is primarily concerned with the finite form of Ramsey's theorem. The consequences for graph theory of this outstanding result are touched on, but the discussion is not carried very far. In Chapter 11, questions of enumerative combinatorial analysis are treated, with the emphasis on two topics: the classical method of generating functions (which goes back to Euler), and Pólya's celebrated enumeration theorem (1937). It is not easy to see how the enumerative theory could comfortably fit into the framework chosen by the authors. The chapter, and the book, conclude with a very brief introduction to the Möbius function associated with partially ordered sets, a topic that seems to me to merit more extensive treatment.

Coding theory is conspicuously absent from the discussion, although (since it is closely allied to the theory of designs), it could conceivably be accommodated within the existing scheme. I am much less certain about extremal problems of graph theory and of finite set theory (nor are these problems discussed here at all). A feature of the text that caused me regret is the comparative isolation of Ramsey's theorem. No echo of the clamorous advance in Ramsey theory is heard in the book.

Yet, when the worst I can think of has been said, there remains the firm conviction that any shortcomings of the work of Graver and Watkins are trifling by comparison with its merits, its innovative character, and the writers' unflinching determination to meet head-on the principal difficulties of modern combinatorial mathematics. Although not every problem discussed by them forms an integral part of their grand design and although some fascinating topics are ignored or are only accorded a grudging welcome, nevertheless the book represents an achievement which I salute with stunned admiration. Graver and Watkins have deserved well of their fellow-combinatorialists and I judge that, all in all, while they have not produced a definitive treatise on combinatorics, they have assuredly brought nearer the day when such a treatise might be written.

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A history of numerical analysis from the 16*th through the* 19*th century*, by Herman H. Goldstine, Studies in the History of Mathematics and Physical Sciences 2, Springer-Verlag, New York, Heidelberg, Berlin, 1977, xiv + 348 pp., $24.80.

"Let us now praise famous men and our fathers who begat us"

Ecclesiasticus XLIV

It has been said that numerical analysis is the last refuge of classical analysis in modern universities. Be that as it may the impressive volume