MY JOINT WORK WITH RICHARD RADO

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I first became aware of Richard Rado's existence in 1933 when his important paper *Studien zur Kombinatorik* appeared. I thought a great deal about the many fascinating and deep unsolved problems stated in this paper but I never succeeded to obtain any significant results here and since I have to report here about our joint work I will mostly ignore these questions. Our joint work extends to more than 50 years; we wrote 18 joint papers, several of them jointly with A. Hajnal, three with E. Milner, one with F. Galvin, one with Chao Ko, and we have a book on partition calculus with A. Hajnal and A. Máté. Our most important work is undoubtedly in set theory and, in particular, the creation of the partition calculus. The term partition calculus is, of course, due to Rado. Without him, I often would have been content in stating only special cases. We started this work in earnest in 1950 when I was at University College and Richard in King's College. We completed a fairly systematic study of this subject in 1956, but soon after this we started to collaborate with A. Hajnal, and by 1965 we published our GTP (Giant Triple Paper - this terminology was invented by Hajnal) which, I hope, will outlive the authors by a long time. I would like to write by centuries if the reader does not

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consider this as too immodest. Since this conference is more on finite combinatorics, I will speak more on our work on finite sets and where possible, I will restrict myself to countable sets. I have another reason for this: very many new results were proved in this subject, much of it using mathematical logic and forcing. Many mathematicians (e.g. Hajnal, Shelah, Galvin, Laver, Baumgartner and many others) are more competent than I to write about these results. Hajnal and I [1] published two long survey papers about 15 to 20 years ago about many of our solved and unsolved problems in set theory. Clearly a new paper or papers, perhaps a book on this subject, would be very desirable, but as I just stated, many others are now more competent than I to write such a paper or book. I will list in the references our joint papers with Richard and will refer to them by their number.

I started to correspond with Richard in late 1933 or early 1934 when he was a German refugee in Cambridge. We first met on October 1, 1934 when I first arrived at Cambridge from Budapest. Davenport and Richard met me at the railroad station in Cambridge and we immediately went to Trinity College and had our first long mathematical discussion.

In one of my first letters to Richard early in 1934, I posed the following question: Let S be an infinite set of power m. Split the countable subsets of S into two classes. Is it true that there always exists an infinite subset S_1 of S all of whose countable subsets are in the same class? This, if true, would be a far reaching generalization of Ramsey's theorem. Almost by return mail, Rado found the now well-known counterexample using the axiom of choice. Later on all this led to many interesting developments. The answer to my question becomes affirmative if we restrict the partition in various ways, e.g., we only permit Borel or analytic partitions, or if we do not permit the use of the axiom of choice. These results are connected with the names of Galvin, Prikry, Silver, Mathias and others. Magidor and I much later used some of these results. [2]

Actually our first joint paper was done with Chao Ko and was essentially finished in 1938. Curiously enough it was published only in 1961. One of the reasons for the delay was that at that time there was relatively little interest in combinatorics. Also in 1938, Ko returned to China, I went to Princeton and Rado stayed in England. I think we should have published the paper in 1938. This paper [XI] "Intersection theorems for systems of finite sets" became perhaps our most quoted result. Our principal result states as follows: Let $|S| = n$, $n \geqslant 2k$, $A_i \subset S$, $1 \leqslant i \leqslant t(n;k)$, be an intersecting family of subsets of S i.e., $A_i \cap A_j \neq \varnothing$ for every $1 \leqslant i \leqslant j \leqslant t(n;k)$. If we further assume $|A_i| = k$, then

$$t(n;k) \leqslant \binom{n-1}{k-1} \tag{1}$$

and there is equality in (1) if and only if all our sets A_i have a common element. We in fact proved that (1) holds if $|A_i| = k$ is replaced by $|A_i| \leqslant k$, $A_i \not\subset A_j$, i.e., that our family forms a Sperner system. We also proved that if $n > n_0(k,r)$ and we assume $|A_i \cap A_j| \geqslant r$, $1 \leqslant i \leqslant j \leqslant t(n;k,r)$, then

$$\max t(n;k,r) \geqslant \binom{n-r}{k-r} \tag{2}$$

Min first observed that the assumption $n > n_0(k,r)$ is really needed and our crude upper bound for $n_0(k,r)$ has been gradually improved by Frankl and Wilson and the exact value is now known. At the end of our paper, several further results are proved and problems are posed, which became very popular. Most of them were settled by Katona, Kleitman and others. Let me state one problem stated in our paper which seems deceptively simple but no progress has been made with it. Let $|S| = 4n$, $|A_i| = 2n$, $A_i \subset S$, $1 \leqslant i \leqslant T(n)$. Assume, $|A_i \cap A_j| \geqslant 2$, $1 \leqslant i \leqslant j \leqslant T(n)$. Is it then true that

$$\max T(n) = \left(\binom{4n}{2n} - \binom{2n}{n}^2 \right) /2 \ ? \tag{3}$$

It is easy to see (and is contained in our paper) that (3) if true is best possible. This uses the original idea of Min. Let the set S be the integers $1, 2, \ldots, 4n$. $S_1 \cup S_2 = S$, $|S_1| = |S_2| = 2n$. The A_i's satisfy $|A_i| = 2n$, $|A_i \cap S_1| \geqslant n + 1$.

I offer 250 pounds for the proof or disproof of (3). This problem is more than 45 years old.

Due to our rapidly advancing age, it is doubtful if all three of us will ever be together again, (at least not in this world). I had the good fortune to see Ko in June 1986 in Beijing and found him in good health. I, of course, see Richard often and speak to him on the phone every few months.

As far as I know, Hilton and Milner wrote the first paper on our theorem. They proved that if $|S| = n$, $n \geqslant 2k$, $|A_i| = k$, is an intersecting family which is not a clique (i.e. there is no element which is contained in all the A's) then the size of our family is at most

$$\binom{n-1}{k-1} - \binom{n-k-1}{k-1} + 1 \tag{4}$$

and (4) is best possible. If we assume that our family A is intersecting but there is no set of r points so that one of the A's contains one of these points then for $r \geqslant 2$ the maximum size of our family is not known. Perhaps $r = k - 1$ is the most interesting case. See our paper with Lovász [3] and many papers of P. Frankl, Kleitman, Katona, Füredi, Pyber and others and also, a forthcoming paper of Aigner, Andreae and myself.

To end the discussions of this problem, I would like to state a few problems. In our paper we notice that if $|S| = n$, and F is an intersecting family of subsets of $|S|$ then trivially $\max F = 2^{n-1}$. We noticed that there are many such families which are not cliques. Hindman and I obtained upper and lower bounds for the number of such families ([4]).

We further observed that if $|F| = 2^{n-1}$ and every three sets in F have a non-empty intersection then F must be a clique.

Frankl and Füredi now considered the following problem: Let $|S| = n$, F a family of subsets and assume that every k of them have an intersection of size $\geqslant r$. Determine max F. In particular for which values of k and r is it true that max

$F = 2^{n-r}$? Frankl proved that this holds for $k = 2$, $r = 2$ and he conjectured that this holds also for $k = 3$, $r = 2$. Füredi pointed out to me that this certainly does not hold for $k = 2$ and r sufficiently large. Clearly many unsolved problems remain.

Here is a nice conjecture of Frankl and Pach: Let $|S| = n$, $|A_i| = k$, $1 \leqslant i \leqslant t_n$, be an intersecting family. If $t_n > \binom{n-1}{k-1}$ then there is an A_j so that for every $B \subset A_j$ there is an A_i for which $A_i \cap A_j = B$. They can prove this if $t_n > \binom{n}{k-1}$.

The final problem which I believe was first raised by Rothschild, Szemerédi and myself is this. Let $|S| = n$, and let F be an intersecting family of sets with $|A_i| = k$. Assume that every point of S has degree not exceeding cF, $c < 1$. What can one say about max $|F|$? The point of our degree condition is that no element can be contained in too many of our sets, i.e., our family F is very far from being a clique. Füredi solved this problem for many values of c ([5]).

Our first joint paper which actually appeared is on the canonical Ramsey theorem [I] which was the first paper in this direction and which also had a great deal of influence (see, e.g., the work of Graham, Leeb and Rothschild, the work of Deuber, Voigt, Prömel and Laufmann; and the book of Graham, Rothschild and Spencer on Ramsey theory [6]).

Here is our Theorem: Let N be the set of integers. Split the r-tuples of the integers into any number of classes. Then there always is a k, $1 \leqslant k \leqslant r$, and an infinite subset $u_1 < u_2 < \dots$ of N so that our distribution is canonical on $u_1 < u_2 < \dots$. In other words, two r-tuples $u_{i_1} < u_{i_2} < \dots < u_{i_k}; v_{j_1} < v_{j_2} < \dots < v_{j_k}$ are in the same class if and only if $u_{i_{v_1}} = v_{j_{v_1}}, \dots, u_{i_{v_k}} = v_{j_{v_k}}$. There are clearly 2^r canonical distributions and we obtain the classical Ramsey's theorem if the number of classes in finite; then there is only one canonical distribution when $k = n$, i.e., all r-tuples belong to the same class. Rado and I extended our theorem for infinite cardinals and Galvin and Taylor improved our results and, in fact, obtained best possible versions of it. Many generalizations and extensions of our theorem with Richard were obtained by many mathematicians, e.g., Deuber, Voigt, Laufmann and many others.

Here I state the canonical van der Waerden theorem which was found independently by R. L. Graham and myself: Split the integers into a finite or infinite number of classes. Then for every k there is an arithmetic progression of length k all of whose terms are in the same class or all of whose terms are in different classes. I was told of this problem by Dr. Wilkie after a lecture of mine at the Open University. Both Graham and I used in our proof Szemerédi's celebrated theorem: every sequence of positive density contains arbitrarily long arithmetic progressions. Nešetřil and Rödl later found a proof of the canonical van der Waerden theorem which does not use Szemerédi's theorem.

Perhaps our canonical theorem and also later the discovery of the partition calculus explains the success of our collaboration. I was good at discovering perhaps difficult and interesting special cases and Richard was good at generalizing them and putting them in their proper prospective.

Now let me state one of our minor results [II]: A sequence $a_1 < a_2 < \ldots$ has property S if every infinite subsequence has two terms, one of which divides the other. We proved that if $a_1 < a_2 < \ldots$ has property S then the set of integers $\Pi a_i^{\alpha_i}$, $0 \leqslant \alpha_i < \infty$ also has property S. It turned out that Higman already had a more general result. In [VIII] we proved some results on partially well ordered sets of vectors and later Nash-Williams and others proved much more general and important results.

But now I have to return to serious Mathematics. Perhaps our most fascinating unsolved problem in finite combinatorics is about Δ-systems [IX]. A family of sets $\{A_\alpha\}$ is called a Δ-system if the intersection of any two of the A's equals $\cap A_\alpha$, i.e., the intersection of any two of them equals the intersection of all of them. Now we investigated and completely solved in the infinite case the following problem: Let $\{A_\alpha\}$ be a family of m sets each of size $\leqslant n$. When must it contain a Δ-system of size p? Our results had applications in topology, set theory and logic (e.g., in forcing). We solved the infinite problems even without the use of the continuum hypothesis. But if p, m and n are finite, surprising difficulties arise. Denote by $f(n;p)$ the smallest integer such that every family of $f(n;p)$ sets of size n contains a subfamily of size p which forms a Δ-system. Even for $p = 3$ the problem seems to be very difficult. I offer 1000 dollars for the proof or disproof of our old conjecture:

$$f(n;3) < c^n . \tag{5}$$

We only proved $f(n;3) < 2^n n!$ and $f(n;p) < (p-1)^n n!$ This was improved by Spencer to $f(n;3) < (1+o(1))^n n!$. $f(n;3) > 2^n$ was proved in [IX] and is in fact very easy. Let x_i, y_i, $1 \leqslant i \leqslant n$ be $2n$ elements. Our 2^n sets are defined as follows: Each of them contains exactly one of the elements x_i, y_i; $1 \leqslant i \leqslant n$. Clearly these 2^n sets do not contain a Δ-system of 3 members. Abbott and Hanson improved our bound by showing $f(n;3) > 10^{n/2}$, and Abbott showed $f(3,3) = 21$. I would like to call attention to the fact that it is not yet known that for $n > n_0$,

$$f(n;3) < n! \tag{6}$$

A family of sets $\{A_\alpha\}$ is called a weak Δ-system if the intersection of any two of our sets has the same size. In a triple paper with Milner [XVI] using the continuum hypothesis we solved all the infinite problems with the help of Hajnal, but

$$f_w(n;3) < c^n \tag{7}$$

is still open, where $f_w(n;3)$ is the smallest integer u so that every family of u sets of size n contains three sets which form a weak Δ-system. As far as I know even $f_w(n;3) < n!$ has not yet been proved and I am sure that $f_w(n;3) < (n!)^{1-\epsilon}$ is still open. After we wrote our paper we found out that Sanjin in the 1940's proved that if m is a regular cardinal and F is a family of m finite sets then this family contains a Δ-system of size m. Sanjin used this result in set-theoretic topology. The inequality

$$f(n;k) < (k-1)^n n!$$

is the basis of the star method which was developed by Frankl and Füredi for solving extremal problems concerning several intersecting type families. It will be surveyed in a forthcoming book by Frankl, Füredi and Katona. For a generalization of Δ-systems see the paper of Frankl and Pach [7]. As far as I know $f_w(n;3)^{1/n} \to 2$ has not yet been disproved.

In another paper [III] where we already started to discuss problems of partition calculus we obtained the first reasonable upper bounds for the general Ramsey function. Denote by $f(n;k,r)$ the smallest number so that if we divide the r-tuples of the integers $1 \leqslant j \leqslant f(n;k,r)$ into k classes then there always is a subset of the integers $\leqslant f(n;k,r)$ of size n all whose r-tuples are in the same class. Before our paper there were only very poor upper bounds for $f(n;k,r)$ for $r > 2$. By using a ramification system we obtained an upper bound for $f(n;k,r)$ as an $(r-1)$-times iterated exponential. In fact we proved

$$f(n;k;r)^{1/n} < \left. k^{k^{\cdot^{\cdot^{k}}}} \right] r-1 \, .$$

Hajnal, Rado and I [XII] later showed that in fact $f(n;k,r)$ is greater than an $(r-2)$-times iterated exponential. It seems likely that the $(r-1)$-times iterated exponential gives the correct bound. Let us restrict ourselves for the moment to $r = 3$. The probability method gives without any difficulty

$$f(n;2,3) > 2^{cn^2} \tag{8}$$

and Hajnal proved more than 20 years ago that (see [XVIII])

$$f(n;4,3) > 2^{c \cdot 2^{n/2}} \tag{9}$$

Hajnal and I have a slightly better upper bound then (8) for $f(n;3,3)$. Probably

$$f(n;2,2) > 2^{2^{cn}} \tag{10}$$

but, unfortunately, on this no progress has been made.

This is one of the outstanding open problems of the subject. I offer 500 pounds for a proof or disproof of (10). If (10) holds then it would follow from our methods that $f(n;k,r)$ increases as an $(r-1)$-times iterated exponential.

Hajnal and I have a forthcoming new paper on $f(n;k,3)$ which will show that in many ways $f(n;k,3)$ behaves differently then $f(n;k,2)$ which perhaps will further increase the interest in the fundamental conjecture (10).

In [III] we give the first non-trivial lower bound for the van der Waerden function $W(n)$. $W(n)$ is the smallest integer for which if we divide the integers not exceeding $W(n)$ into two classes then at least one of the classes contains an arithmetic progression of n terms. The only known upper bound for $W(n)$ increases as fast as Ackermann's function. We easily showed by the probability method that

$$W(n) > 2^{n/2}$$

This was improved by Wolfgang Schmidt to $W(n) > 2^{1+o(1))n}$. Berlekamp showed that if $n=p$ is a prime then $W(p+1) > p2^p$. Lovász and I noticed [3] that the local Lemma of Lovász gives, for every n, $W(n) > c2^n/n$. In some of my papers I somewhat carelessly stated that in fact we get $W(n) > c2^n$. As far as I know it has never been proved (see e.g., the survey paper of Graham and Rödl which appears in the same volume as this paper.) The first task would be to prove

$$W(n) > c2^n \tag{11}$$

and then to prove

$$W(n)/2^n \to \infty \tag{12}$$

(11) and (12) will perhaps not be difficult and I offer 25 pounds for a proof. It seems very likely that in fact $W(n)^{1/n} \to \infty$, but perhaps the proof will require a significant new idea.

For a long time all of us believed that $W(n)$ certainly increases much slower than Ackermann's function. As far as I know Solovay was the first who expressed doubts about this. The large majority still believes that the order of magnitude of $W(n)$ is much less than Ackermann's function. The very surprising results of Paris and Harrington showed that simple combinatorial problems can lead to functions which increase much faster than Ackermann's function. Denote by $f^*(n;k,r)$ the smallest integer for which if we divide the r-tuples of the integers not exceeding $f^*(n;k,r)$ into k classes then there always is a sequence $a_1 < a_2 < \ldots < a_n \leqslant f^*(n;k,r)$, $a_1 < n$, all whose r-tuples are in the same class. The harmless looking extra condition $a_1 < n$ changes the situation completely. Paris and Harrington first show by a simple compactness argument that $f^*(n;k,r)$ is finite, but then comes the surprise: $f^*(n;k,r)$ increases much faster than Ackermann's function, and in fact the existence of $f^*(n;k,r)$ cannot be proved from the Peano axioms. They proved that for every fixed k and r this is possible, but that no such proof exists for all values of k and r. It was of course known since Gödel that this situation is possible but it was a great surprise and a great achievement that such simple problems can lead to such seemingly paradoxial results. Many other results of this type are now known. To end this discussion I just remark that Mills and I proved that

$$2^{2^{\frac{n}{2}(1-\varepsilon)}} < f^*(n;2,2) < 2^{n!^2} \tag{13}$$

Solovay and Ketonen proved that if k or r are > 2 then $f^*(n;k,r)$ grows already at least as fast as Ackermann's function.

In [III] we also considered the following interesting problem: Let $|S| = m \geqslant \aleph_0$ be an infinite set. For which m is it possible to divide all finite subsets of S into two classes in such a way that every infinite subset S_1 of S contains two finite subsets of S_1 of the same size which belong to different classes? We proved that this is possible if $|S| \leqslant c$ (the cardinality of the continuum). Hajnal and I [9] later proved that this is in fact possible if the power of S is less than the first strongly inaccessible cardinal, but that it can not be done if $|S|$ is a measurable cardinal. (In those dark and prehistoric times it was not yet known that the first strongly inaccessible cardinal can not be measurable i.e. it was BHT [before Hanf-Tarski]). Silver later proved that our decomposition is in fact possible for very much larger cardinals. He showed that the first cardinal for which such a decomposition is impossible is much larger than the smallest weakly compact cardinal (i.e., for which $m \to (m,m)_2^2$ holds), but it is much smaller than the first measurable cardinal. These investigations of Silver and others had important applications in mathematical logic.

In our paper [VI] we started a systematic investigation of the partition relation $a \rightarrow (b_1, b_2)^2_2$ and its generalizations. The partition symbol first occurs in [IV]. In this discussion we will restrict ourselves as much as possible to denumerable sets but first of all I must give the general definition of the partition symbol (see, e.g., our book [XVIII].)

$$a \rightarrow (b_h)^r_{h \in H} \tag{14}$$

where a and b_h is a cardinal or an ordinal or an order type, and H is an arbitrary set. In human language (14) means that if we divide the r-tuples of a into H classes in an arbitrary way then for some $h \in H$ there is a subset of type b_h all whose r-tuples are in the same class. $a \nrightarrow (b_h)^r_{h \in H}$ means that one can split the r-tuples into H classes so that these should be no set of type b_h all whose r-tuples are in the same class. Before we started our investigations several results of this kind were already known, but were expressed in a different language.

$$\aleph_0 \rightarrow (\aleph_0)^r_k \ (r < \aleph_0, k < \aleph_0)$$

is Ramsey's theorem. In human language: if we split the r-tuples of a denumerable set into k classes (r and k are finite) then there always is an infinite set all whose r-tuples are in the same class.

$$c \nrightarrow (\aleph_1, \aleph_1)^2_2$$

is a well known result of Sierpinski (which was discovered a little later independently by Kurepa). It states that one can partition the pairs of real numbers into two classes so that every subset of power $\aleph_1$ contains a pair from both classes. Finally if m is any infinite cardinal than Dushnik, Miller and I proved

$$m \rightarrow (m, \aleph_0)^2_2$$

Perhaps it is nicer to express this result in the language of graphs. If G is a graph of m vertices which does not contain a complete subgraph of m vertices then it contains an infinite independent set. As stated previously, the partition symbol introduced by Rado proved to be immensely useful in expressing in a clear and short way many old and new results and lead to many new problems.

Hajnal, Rado and I in [XII] obtained many further results on the partition symbol and its generalizations and raised a very large number of new and interesting problems which had a great deal of influence on the development of set theory. Many of the problems posed in our papers were solved positively or negatively by Prikry, Galvin, Laver, Baumgartner, Larson, Milner, Shelah, Todorcevic and many others. In many cases undecidability raised its ugly head and more and more often our problems turned out to be undecidable. I personally regret this but at the moment (and perhaps forever) we have to accept it as a fact.

One of our first results in [IV], [V] and [VI] was $\eta \rightarrow (\eta, \aleph_0)^2_2$. In fact we obtained a slightly stronger result. If G is a graph whose vertices are the rational numbers, then either G contains an infinite complete graph or an independent set which is dense in an interval. Later this result was extended and generalized enormously by Galvin and Laver. Our next result was $\lambda \rightarrow (\omega+n, \omega+n)^2_2$ where λ denotes the order type of the continuum. In 1951-52 I was at University College and Richard at King's College. Davenport arranged that my room at College should have a telephone and we had endless mathematical discussions on the 'phone.

We conjectured that for every ordinal $\alpha < \omega_1, \lambda \to (\alpha, \alpha)^2_2$ and $\omega_1 \to (\alpha, \alpha)^2_2$, but could not even prove $\lambda \to (\omega 2, \omega 2)^2_2$. This was proved by Hajnal and a little later Galvin proved $\lambda \to (\alpha, \alpha)^2_2$.

We first heard of Martin's axiom in the late 1960's from Juhász and at first we did not take it too seriously. But then Hajnal and Baumgartner proved $\omega_1 \to (\alpha, \alpha)^2_2$ by Martin's axiom and then observed that if it follows from Martin's axiom it is in fact true in ZFC. This triumph of course changed our opinion on Martin's axiom. Later Galvin proved a result stronger than $\omega_1 \to (\alpha, \alpha)^2_2$ without using Martin's axiom.

In those early days we thought that perhaps for every $\alpha < \omega_1, \omega^\alpha \to (\omega^\alpha, n)^2_2$. The truth turned out to be much more complicated. First of all Specker proved that $\omega^2 \to (\omega^2, n)^2_2$. I hope the reader will forgive a very old man for some reminiscences. I passed through Zurich on the way to Israel in November 1934. I met Specker, already an old friend, at the ETH and told him that I offer 20 dollars for a proof or disproof of our conjecture with Richard $\omega^2 \to (\omega^2, n)^2_2$. A few days later Specker sent me his now well known proof of the conjecture. At first I thought that I can prove $\omega^n \to (\omega^n, 3)^2_2$, but in fact the proof only gave $\omega^{2n} \to (\omega^{n+1}, 4)^2_2$ and soon Specker found his well known counterexample $\omega^n \not\to (\omega^n, 3)^2_2$ for all n, $3 \leqslant n < \omega$. Neither Specker's proof nor his counterexample worked for

$$\omega^\omega \rightarrow (\omega^\omega, 3)_2^2 \tag{15}$$

In the late 1960's I offered 250 dollars for a proof or disproof of (15). Chang in 1969 proved (15) and I gladly handed him the well deserved prize. Milner somewhat simplified Chang's very complicated proof and also showed $\omega^\omega \rightarrow (\omega^\omega, n)_2^2$. Finally Jean Larson obtained a relatively simple proof of $\omega^\omega \rightarrow (\omega^\omega, n)_2^2$ and proved many related results. The first open problem now is: $\omega^{\omega^2} \rightarrow (\omega^{\omega^2}, n)_2^2$.

I offer 250 pounds for a proof or disproof of $\omega^{\omega^2} \rightarrow (\omega^{\omega^2}, 3)_2^2$ and 1000 pounds for the complete characterization of the values of α for which

$$\omega^{\omega^\alpha} \rightarrow (\omega^{\omega^\alpha}, n)_2^2$$

holds. We conjectured that if $\alpha \rightarrow (\alpha, 3)_2^2$ holds then for every n also $\alpha \rightarrow (\alpha, n)_2^2$ holds. This conjecture if true would be useful both for finite and infinite combinatorics. The following problem should be mentioned here: For which values of n, k and l does

$$\omega^n \rightarrow (\omega^k, l)_2^2$$

hold? Galvin, Hajnal and independently Haddad and Sabbagh reduced this problem to a finite combinatorial problem and Eva Nosal nearly completely settled it.

In [XII] if we use the continuum hypothesis and exclude large cardinals and restrict ourselves to cardinal numbers we settled nearly all the problems about the truth of the partition relation $a \rightarrow (b,c)_2^2$. In our book [XVIII] we give a fairly detailed investigation how far we can get if the continuum hypothesis is dropped, but as stated earlier I ignore here these investigations. I would only like to mention one striking open problem. In [XII] we prove that if $c = \aleph_1$ then $c \not\rightarrow [c]_c^2$. In other words one can color the pairs of a set of power $\aleph_1$ by $\aleph_1$ colors so that every subset of power $\aleph_1$ has edges of all the colors. In fact we show that every bipartite $K(\aleph_1, \aleph_0)$ contains edges of all the colors. Let us now not admit the continuum hypothesis. Is it true that $c \rightarrow [\aleph_1]_3^2$ holds? In other words can one color the pairs of real number by three colors so that every set of power $\aleph_1$ should contain edges of all three colors? In view of the simplicity of the old proof of Sierpinski for $c \not\rightarrow (\aleph_1, \aleph_1)_2^2$ it is very surprising why this simple question should be so difficult. In fact it is generally believed that the problem is undecidable. Galvin and Shelah proved $\aleph_1 \not\rightarrow [\aleph_1]_4^2$ and $2^{\eta_0} \not\rightarrow [2^{\eta_0}]_{\eta_0}^2$. Very recently Todorcevic proved $\aleph_1 \not\rightarrow [\aleph_1]_{\eta_1}^2$. This certainly is an unexpected and sensational result.

In [VI] we proved

$$\lambda \rightarrow (\omega+n, 4)_2^3 \tag{16}$$

The proof was quite complicated and we never could get a stronger result and could never prove $\omega_1 \rightarrow (\omega+n, 4)_2^3$ and for a long time (16) remained the strongest result. It was conjectured that perhaps for every ordinal $\alpha < \omega_1$ and integer n, $\lambda \rightarrow (\alpha, n)_2^3$ and $\omega_1 \rightarrow (\alpha, n)_2^3$ holds. Very recently Milner and Prikry using some recent results of Todorcevic proved

$$\omega_1 \to (\omega.2+1, 4)^3_2 .$$

Thus most of the problems here are still open after more than 30 years. For the order types λ and ω_1 there are no interesting positive results if we split the four-tuples.

I talked about our results on partition calculus in October 1953 at a meeting of the American Mathematical Society in New York. I stated many of our theorems and open problems and deplored that our results did not find any applications in other branches of mathematics. In the meantime the situation greatly improved. As far as I know Hajnal and Juhász were the first to use partition calculus to solve problems in set-theoretical topology and among them, a problem of de Groot. Juhász tells me that ramification systems were used by Alexandroff and Urysohn in the 1920's. Various results of partition calculus and Δ-systems were used in mathematical logic, and Δ-systems were often used in combinatorics and also occasionally in number theory. In fact the conjecture [5] was discovered because of applications on the greatest prime factors of polynomials and also on problems of combinatorial number theory.

Incidentally Rado and I were the first to prove that for every infinite cardinal number m there exists a graph G of power m and chromatic number m which contains no triangle ([VII], [X]). In our proof we used the construction of Specker by which he proved $\omega^3 \not\to (\omega^3, 3)^2_2$. Perhaps I should mention here a few problems and results on chromatic graphs. The chromatic number k of a graph G is the smallest integer so that the vertices of G can be colored by k colors so that two vertices of the same color are not joined. At first mathematicians became interested in the chromatic number of graphs because of the four-color theorem (at that time it was the four-color problem), but soon it was realized that there are many interesting problems on the chromatic number of graphs which are independent of the four-color problem. Tutte was the first to prove that for every k there is a graph G which has no triangle and which has chromatic number $\geqslant k$ (Ungár, Zykov and Mycielski obtained the same result independently). Tutte sometimes published his results under the pseudonym Blanche Descartes, and in one of my papers quoting this result I referred to Tutte. Smith wrote me a letter saying that Blanche Descartes will be annoyed that I attributed her results to Tutte (he clearly was joking since he knew that I know the facts), but Richard was very precise and when in our paper I wanted to refer to Tutte, Richard only agreed after I got a letter from Smith stating that my interpretation of the facts was correct. After our result with Richard I proved by using the probability method that for every r there is a graph of n vertices the smallest odd circuit of which has size $\geqslant 2r+1$ and whose chromatic number is $> n^{\epsilon_r}$; in fact whose largest independent set is of size $< n^{1-\epsilon_r}$. The order of magnitude of ϵ_r is known only for $r = 1$.

Here it is known that if $G(n)$ has n vertices and contains no triangle then its chromatic number is less than $\dfrac{n^{1/2}}{(\log n)^{c_1}}$ but can be greater than $\dfrac{n^{1/2}}{(\log n)^{c_2}}$. It would be desirable to get an asymptotic formula.

Some of these theorems could later be proved by constructive methods, the first such proof was due to Lovász and later in a sharper form by Nešetřil and Rödl, but some of the sharper results have not yet been obtained without the probability method.

In a later paper Hajnal and I proved that for every cardinal number m and integer r there is a graph of power m, chromatic number m the smallest odd circuit of which has size $\geqslant 2r+1$ and we also proved that every graph of chromatic number $\geqslant \aleph_1$ contains a $k(n;\aleph_1)$ but does not have to contain a $k(\aleph_0, \aleph_0)$

Unfortunately all of us missed the beautiful and fundamental conjecture of Walter Taylor. Let G be any graph of chromatic number $\aleph_1$. Then for every cardinal m there is a graph G_m of chromatic number m for which all finite subgraphs of G_m are subgraphs of G too.

Hajnal, Shelah and I have a triple paper on this subject where we prove some partial results and recently Hajnal and Komjáth [10] have a paper in which they prove many further interesting results on finite and denumerable subgraphs of graphs of chromatic number $\geqslant \aleph_1$. In a triple paper of Hajnal, Szemerédi and myself [11] we prove many interesting theorems and raise many problems which I hope will lead to further interesting results. Thus our old paper with Richard leads to many developments and I am sure will continue to do so.

In [XII], many results are proved and very many unsolved problems are posed but their discussion on the one hand would lead too far into set theory and also a proper discussion of them would need a better knowledge of the many recent results on undecidable problems with which I am not so well acquainted. Hajnal, Shelah and many others could do a better job of this than I. Thus I will restrict myself to a very small sample. Hajnal often observed that to prove positive results in partition calculus we essentially use only two tools. The ramification systems and the canonization Lemma. In its most general form the canonization Lemma is stated in XVIII p. 164 (see also [XII]). This Lemma was one of our most original contributions to set theory and it was very useful in many applications. Shelah has a very significant improvement of our Lemma for $r = 2$ (see p. 159 of [XVIII]). To avoid a complicated formalism we state our Lemma in only a special case: Let ω_α be a regular cardinal. Let S_β, $1 \leqslant \beta < \omega_\alpha$, be a rapidly increasing sequence of cardinals. Split the r-tuples of $\bigcup_{\beta < \omega_\alpha} S_\beta$ into fewer than ω_α classes (each r-tuple meets each S_β in at most one point). Then there is an $A_\beta \subset S_\beta$, $\bigcup_{\beta < \alpha} A_\beta = \bigcup_{\beta < \alpha} S_\beta$, for which the distribution is canonical. In other words if $(x_1 x_2, \ldots, x_n)$ is an r-tuple of $\underset{\beta}{U} A_\beta$ and $X_i \in A_{\beta_i}$ then the class of $(x_1, \ldots, x_r)$ only depends on $(\beta_1, \ldots, \beta_r)$. The first triumph of our Lemma was our proof of $\aleph_a \rightarrow [\aleph_a]^3_2$. In human language: If one splits the pairs of a set of power $\aleph_a$ into three classes there always exists a subset of power $\aleph_a$ all pairs of which are in only two of our classes.

To end this paper I just state a random selection of some problems and results which came out of our investigations. Richard and I proved [XIII] that for every pair of integers m and n there is a smallest $l_\alpha(m, n)$ so that

$$\omega_\alpha l_\alpha (m, n) \rightarrow (m, \omega_\alpha n)^2_2 .$$

We conjectured that $l_\alpha(m, n) = l_o(m, n)$ but proved this only for

$m \leqslant 4$, $n \leqslant 2$. Our conjecture was proved by Baumgartner. In [XIII] we also ask: Is it true that $\omega_1 \omega \to (\omega_1 \omega, 3)_2^2$? Hajnal and I later showed

$$\omega_1 \omega \not\to (\omega_1 \omega, 3)_2^2$$

but that

$$\omega_1^2 \to (\omega_1 \omega, 3)_2^2 .$$

We could never decide if $\omega_1^2 \to (\omega_1 \omega, 4)_2^2$ is true. Two years ago Baumgartner and Hajnal proved that the answer is negative. Several unsolved problems remain some of which are discussed in a recent paper of mine. Hajnal and I could not decide whether $\omega_2 \omega \to (\omega_2 \omega, 3)_1^2$ is true. This was recently proved by Shelah and Stanley.

Perhaps I should mention the very useful theorem of Milner and Rado which we used a great deal: Let $\alpha > 0$, $\delta < \omega_{\alpha+1}$. Then

$$\delta \not\to (w_\alpha^{(n)})_{n<\omega}^1 .$$

In human language: One can decompose a well ordered set of order type $\delta < \omega_{\alpha+1}$ into the union of countably many sets each of which has order type $< \omega_\alpha^\omega$. In our triple paper with Milner we used and generalized this important theorem a great deal. Several further problems are stated in papers of Hajnal and myself and Hajnal, Milner and myself.

To end this paper let me state a finite problem: In one of our innumerable mathematical discussions Richard and I observed that if we color the edges of a complete graph $K(m)$ with two colors then in at least one of the colors the resulting graph is connected and contains all the vertices of $K(m)$. This holds both for finite and infinite graphs. The proof is trivial. We wondered what happens if we use more than two colors. This problem was recently considered in several forthcoming papers by Gyárfás and myself but several interesting finite and infinite problems remain which I hope will be further investigated.

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