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Interview with Van Vu

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Photo by Nam Long

Van Vu finished high school in Vietnam, where he was born. He received his Bachelor degree at Eötvös Loránd University in Budapest in 1994. He obtained his Ph.D. at Yale in 1998 under the guidance of László Lovász. He is currently the Percey F. Smith chair of mathematics at Yale University. Before his position at Yale, he spent some years at the Institute of Advanced Studies at Princeton, Microsoft Research, University of California San Diego, and Rutgers University. Professor Vu has given talks at many conferences, including an invited talk at the International Congress of Mathematicians in 2014. Professor Vu has received several awards, including the Sloan Fellowship in 2002, Polya Prize in 2008 and Fulkerson Prize in 2012. In 2012, he became an inaugural fellow of the American Mathematical Society, and in 2020, a fellow of Institute of Mathematical Statistics. He is on the editorial board of Combinatorica and Journal of Combinatorial Theory, Series A.

Mansour: Professor Vu, first of all, we would like to thank you for accepting this interview. Would you tell us broadly what combinatorics is?

Vu: To me, combinatorics is more of a style rather than a separate area of mathematics. Roughly speaking, we think discretely. In the beginning, this applies, naturally, only to discrete objects, such as graphs. Later, general principles have been formed (a good example is Szemeredi's regularity lemma[1]) which apply across many fields.

Mansour: What do you think about the development of the relations between combinatorics and the rest of mathematics?

Vu: In recent years, combinatorial thoughts have been used in many branches of mathematics, sometimes with astonishing efficacy. In many studies in different areas, it has turned out that at the very core of the subject there is a deep combinatorial problem, and ideas and tools from combinatorics became very essential. For instance, the notion of pseudo-randomness (originated from works of Thomasson[2] and Graham-Chung-Wilson[3] on graphs) is crucial in the Green[4]-Tao[5] proof of the existence of long arithmetic progressions in primes. Szemeredi's lemma is used quite frequently in analysis and probability also. Many of my own works on eigenvalues of random matrices and roots of random functions rely on

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[1]E. Szemerédi, *Regular partitions of graphs*, in: Proc. Colloque Inter. CNRS (J.-C. Bermond, J. -C. Fournier, M. Las Vergnas and D. Sotteau eds.), 1978, 399–401.

[2]A. Thomason, *Pseudo-random graphs*, Annals of Discrete Math. 33 (1987), 307–331.

[3]F. R. K. Chung, R. L. Graham and R. M. Wilson, *Quasi-Random Graphs*, Comb. 9:4 (1989), 345–362.

[4]B. Green, *Long arithmetic progressions of primes*, in W. Duke, Y. Tschinkel (eds.), Analytic number theory, Clay Mathematics Proceeding 7, Providence, RI: American Math. Soc. (2007), 149–167.

[5]T. Tao, *Arithmetic progressions and the primes*, Collectanea Mathematica, Vol. Extra (2006), 37–88.

[6]G.A. Freiman, *Inverse problems of additive number theory, VI. On the addition of finite sets, III*, Izv. Vysš. Učebn. Zaved. Matematika 3 (1962), 151–157.

modern anti-concentration theory, which has its roots in subset sums and Freiman's inverse theorem[6] in combinatorial number theory.

In the beginning, combinatorialists borrowed lots of tools from other areas to prove our theorems (probabilistic method, topological method etc). Now, with some pride, I can say that we have started to return the favor.

Mansour: What have been some of the main goals of your research?

Vu: Like everybody, I like to solve big/famous problems. However, to me, it is more important to develop new tools and methods or notions along the way, which other researchers can use for different purposes. This is what really makes mathematics move forward, not the fact that certain conjectures are true.

Mansour: We would like to ask you about your formative years. What were your early experiences with mathematics? Did that happen under the influence of your family or some other people?

Vu: Like most mathematicians, I went to a special school for gifted children from an early age (around 10 or so). It was my elementary school teacher who recognized that I could be good at math and told my parents, who then encouraged me to change schools.

Mansour: Were there specific problems that made you first interested in combinatorics?

Vu: After high school, I got a fellowship to go to study in Budapest, but in an engineering school. There, one of my math teachers, Kati Vesztergombi (Laci Lovász's wife) ran a math circle and showed me an Erdős problem (I think it was the distinct distances problem). I really liked it (but could only achieve some progress 10 years later). I also took part in the famous Schweitzer competition and after a year or so Kati and Laci convinced and supported me to switch to studying mathematics.

Mansour: What was the reason you chose Yale University for your Ph.D. and your advisor László Lovász?

At the time, Eastern Europe had just stepped out of the iron curtain, and I did not know much about research and American schools. I chose Lovász as my advisor not only because it was natural, given our history, but also because his style of doing mathematics impressed me so much.

Combinatorics was new at Yale then (1994), and most of the time my fellow students did not know what I was doing. When I applied, I also got an offer from Massachusetts Institute of Technology (MIT). I remember that the chair of the department called me and asked if I wanted to come to MIT. He sounded very confused when I told him that I chose Yale to study combinatorics, so he asked who would be my advisor. When I told him it was Lovász, he seemed relieved. "Oh, you will be in good hands," he said.

Mansour: What was the problem you worked on in your thesis?

Vu: There were two problems. The first is to determine how big the inverse of a $(0, 1)$ matrix of size n can be. This originated from a paper[7] of Dmitry Kozlov and mine on a seemingly innocent coin weighing problem. We solved this problem with Noga Alon[8]. The second was Serge's arc problem in finite geometry. What is the minimal size of a maximal arc on a finite projective plane? (A maximal arc is a set of points with no three on a line and maximal with respect to that property.) I worked on this with Jeong Han Kim[9]. I still like the results we obtained back then.

Mansour: What would guides you in your research? A general theoretical question or a specific problem?

Vu: Mostly, I like specific problems which are, or seem to be, at the heart of a bigger theory. Of course, when one solves a good problem, one gets some recognition and that helps one's career. But the real value of the solution is the new ideas and tools that were introduced and that other people can use on other, sometimes totally different, problems.

Mansour: When you are working on a problem, do you feel that something is true even before you have proof?

Vu: Most of our conjectures are true. I do not doubt the Riemann hypothesis or the twin prime conjecture. The real challenge is to find out why. I guess at the end what really matters is the new phenomenon or viewpoint that we

[7] D. N. Kozlov and V. H. Vu, *Coins and Cones*, J. Comb. Theory, Ser. A 78:1 (1997), 1–14.
[8] N. Alon, D. N. Kozlov and V. H. Vu, *The geometry of coin-weighing problems*, FOCS (1996), 524–532.
[9] J. H. Kim and V. H. Vu, *Small complete arcs in projective planes*, Comb. 23:2 (2003) 311–363.

discover, which contains the problem (or the technical heart of it) as a special case. I mentioned the work of Green[4] and Tao[5] on primes earlier. The stunning thing that they found out is that this is a phenomenon about arithmetic progressions rather than about primes. Basically, they give a necessary condition for sets of integers to contain arbitrarily long arithmetic progressions. That was the real innovation, done with the help of the notion of pseudo-randomness from combinatorics. The part of verifying these conditions for primes can be done by standard tools from analytic number theory.

Mansour: What three results do you consider the most influential in combinatorics during the last thirty years?

Vu: I should mention the Freiman inverse theorem[6]. The other is Szemeredi's regularity lemma[1]. Both were proved more than 30 years ago, but their impact is most profoundly felt in the last 30 years and in so many different areas of math. The third one would be the development of the nibble method and its variants and refinements (such as the differential method or absorbing method). It started with a paper by Ajtai, Komlos, and Szemerédi[10] (I think), but most leading researchers in probabilistic combinatorics worked on it and added many important new ideas to make it more powerful.

Mansour: What are the top three open questions in your list?

Vu: Well, all of us would love to see why Riemann's hypothesis is true, I guess. Could there be a different approach to the Four color theorem (and many other problems of this nature)? An approach that convinces us that the theorem is true is based on some fundamental fact. Finally, as it brings back fond memories (from my Budapest days), a solution to the distinct distances in all dimensions. (The dimension 2 case was solved recently by Guth and Katz[11].)

Mansour: What kind of mathematics would you like to see in the next ten-to-twenty years as the continuation of your work?

Vu: As I mentioned earlier, combinatorics has started to produce methods and tools which are of interest to researchers from many other areas. I would really like to see this trend continue. Not so long ago, most people viewed combinatorics as a collection of clever, but ad-hoc, problems, and ideas. It should no longer be the case. We must build more theories, methods, and tools, which can have influence across the boundaries of mathematics.

Compared to other areas of mathematics, combinatorics has a big advantage in that it is quite close to real-life applications. When a practitioner explains his/her problem to a mathematician, my bet is that a combinatorialist would have a better chance of understanding or even proposing a solution than a researcher from a more abstract area. I would like to see more penetrating applications of combinatorics in areas outside mathematics. The application of the de Brujin graph on genome ensembles is a wonderful example.

Mansour: Do you think that there are core or mainstream areas in mathematics? Are some topics more important than others?

Vu: It is a matter of taste, I guess, and every branch has its own merit. From the applications' point of view, however, I think that probability and statistics have become more and more important. With respect to general science and industry, they probably play the role of analysis in Newton's time.

Mansour: What do you think about the distinction between pure and applied mathematics that some people focus on? Is it meaningful at all in your case? How do you see the relationship between so-called "pure" and "applied" mathematics?

Vu: No, I am not even sure how one defines the distinction. Personally, I do appreciate the kind of mathematics which can be explained and makes sense (at least the motivation and the result) to graduate students, regardless of the area or the label of "pure" or "applied". When mathematics makes sense, it is intriguing and beautiful. Unfortunately, quite a few branches have become more and more abstract, and we now have far too many talks where most of the audience get lost after the first 3 minutes. As DGS of Yale's mathematics department, I need to address this as it is a serious concern for our students.

Mansour: What advice would you give to young people thinking about pursuing a re-

[10]M. Ajtai, J. Komlós and E. Szemerédi, *A note on Ramsey numbers*, J. Combin. Theory Ser. A 29:3 (1980), 354–360.
[11]L. Guth and N. H. Katz, *On the Erdős distinct distances problem in the plane*, Ann. Math. 181 (2015), 155–190.

search career in mathematics?

Vu: Well, most mathematicians love what they do, because they can do what they love. This is our most valuable reward. If one is in it for the glory, then there is a high chance that one will be disappointed.

Some time ago, Terry Tao wrote an interesting essay "Does one has to be a genius to do Math." Coming from him, the title sounds a bit funny. (The answer is No, but it would be way more convincing if it came from, say, me.) But I totally agree with his points and highly recommend young researchers to take a look.

Mansour: Would you tell us about your interests besides mathematics?

Vu: I like to travel and I am a fan of sports and movies. About ten years ago, I started to write a blog (in Vietnamese). I have found it to be quite relaxing and entertaining.

Mansour: Before we close this interview with one of the foremost experts in combinatorics, we would like to ask some more specific mathematical questions. What does *universality* mean in the context of random matrix theory? What are the major results in this regard? Are there still challenging open questions in this direction?

Vu: Actually, the story around this notion is interesting. There are two different interpretations. In the beginning, random matrix theory was studied mostly by physicists, who were particularly interested in the interaction between nearby eigenvalues. This interaction can be expressed through a parameter called the correlation function, which they could compute precisely for matrices with Gaussian entries, thanks to special properties of the Gaussian distribution. The original universality conjecture says that the correlation function does not depend on the distribution of the entries. For instance, if we replace the Gaussian with a different distribution, we should obtain (asymptotically) the same correlation function. This was the main conjecture from Mehta's book "Random Matrices"[12], which was considered, for several decades, the key reference in the area.

Terry Tao and I started to work on spectral properties of random matrices about 15 years ago. We actually did not know much about the mathematical physics literature. Our viewpoint was purely linear algebraic. In general, we thought that all limiting distributions concerning spectral parameters of random matrices (eigenvalues, eigenvectors, determinant etc) are universal, namely, they do not depend on the distribution of the entries. This more general "universality" belief has turned out to be true in most cases considered so far, including the special case of the correlation function.

The last 20 years saw an enormous amount of work on random matrices, with several breakthroughs leading to solutions of long-standing, famous, problems. In my opinion, most major questions about spectral limit distributions, such as the Circular Law conjecture[13] (the non–hermitian analog of Wigner semi-circle law[14,15]) or Mehta's conjecture[12] (mentioned above), has been settled (for the most studied models of random matrices, at least). There are still very important questions left open, such as the Localization conjecture[16]. But for this, one needs to understand an entirely different model of random matrices, which, I would say, appeals more to a physicist than to a combinatorialist.

Mansour: The study of random polynomials, initiated by Marc Kac, has also motivated many research programs and currently rich literature accumulated on the topic. Would you tell us briefly about the milestone results in the historical context of this topic? What are the major open problems?

Vu: Actually, Kac did not initiate the study of random polynomials. The notion of random polynomials was considered earlier by Waring and Sylvester[17]. The first official result

[12] M. L. Mehta, *Random Matrices*, 3rd edition, Academic Press, New York, 2004.

[13] T. Tao and V. Vu, *Random Matrices: Universality of ESDs and the circular law*, Ann. Prob. 38:5 (2010), 2023–2065.

[14] E. Wigner, *Characteristic vectors of bordered matrices with infinite dimensions*, Ann. Math. 62 (1955), 548–564.

[15] E. Wigner, *On the distribution of the roots of certain symmetric matrices*, Ann. Math. 67 (1958), 325–328.

[16] G. Stolz, An introduction to the mathematics of Anderson localization, arXiv:1104.2317v1 [math-ph], 2011.

[17] I. Todhunter, *A history of the mathematical theory of probability*, Stechert, New York, 1931.

[18] A. Bloch and G. Pólya, *On the roots of certain algebraic equations*, Proc. London Math. Soc. 33 (1932), 102–114.

[19] M. Kac, *On the average number of real roots of a random algebraic equation*, Bull. Amer. Math. Soc. 49, (1943), 314–320. Erratum: Bull. Amer. Math. Soc. 49, (1943), 938.

[20] M. Kac, *On the average number of real roots of a random algebraic equation. II*, Proc. London Math. Soc. 50 (1949), 390–408.

was proved by Bloch and Polya[18] about 10 years before Kac[19,20] entered the game. It was true, however, that the results of Littlewood-Offord[21,22,23] and Kac (all in the 1940s) drew the attention of the math community to the subject and made it a focus of both analysis and probability.

Briefly speaking, a random polynomial is of the form $P_n(x) = \sum_{i=1}^{n} c_i \xi_i x^i$, where c_i are deterministic parameters which may depend on both i and n, and ξ are iid random variables. The most natural problem is to study the number and distribution of the real roots. This was the subject in all the papers mentioned above, restricted to the special case when $c_i = 1$ for all i. In this case, the polynomial is referred to as Kac polynomials. There are other ensembles, such as the Weyl polynomial ($c_i = 1/\sqrt{i!}$), which have also been studied intensively. Different ensembles usually lead to totally different behaviors (for instance, the number of real roots is of different orders of magnitude).

Similar to the situation with random matrices, precise formulae can be obtained for the Gaussian case (when all ξ_i are iid Gaussian). Again, one would make a universality conjecture. This has been settled in various cases but very basic questions remain open. For instance, is it true that the number of real roots follows a central limit theorem? The answer is Yes for Kac polynomials, as a result of Maslova[24,25] from the 1970s, almost 50 years ago. But we still do not know the answer for the Weyl ensemble and many other ensembles.

Mansour: In one of your influential papers, coauthored with Terence Tao, *Inverse Littlewood-Offord theorems and the condition number of random discrete matrices*[26], published at Annals of Mathematics, you developed *the Inverse Littlewood-Offord theory*. What is this theory about? Why were these results important?

Vu: In the series of papers on random polynomials from the 1940s (mentioned above), Littlewood and Offord[21,22,23] developed their famous anti-concentration result. In the simplest form, it says that if a_i are non-zero integers, and ξ_i are iid ± 1 random variables, then the sum $S = \sum_i a_i \xi_i$ does not have too much mass on any point. Technically speaking,

$$\rho := \max_a P(S = a) = O(\log n/\sqrt{n}).$$

This result generated a whole area of research in combinatorics, with many influential results from leading researchers such as Erdős (who reproved and sharpened the above result with lovely use of Sperner lemma), Kleitman[27], Sárközy-Szemerédi[28], Stanley[29], Halasz[30] etc. The general direction is to make more assumptions on the a_i, and prove sharper bounds. For instance, a famous result of Sárközy-Szemerédi and Stanley showed that the bound can be improved to $O(1/n^{3/2})$ if we assume that a_i are different.

Terry and I brought a different viewpoint to the problem[26]. We assumed that ρ is relatively large (say $\rho \geq n^{-100}$), and tried to characterize all the sets of a_i which make ρ this large. This idea is motivated by the Freiman inverse theorem[6] on sumsets, and that was the reason we named it Inverse Littlewood-Offord theory. It turned out that such a characterization is possible. We showed that the a_i must have a very strong additive structure in order to make ρ large. Later, many researchers found different characterizations. One particularly useful

[21] J. E. Littlewood and A. C. Offord, *On the number of real roots of a random algebraic equation. I*, J. London Math. Soc. 13 (1938), 288–295.

[22] J. E. Littlewood and A. C. Offord, *On the number of real roots of a random algebraic equation. II*, Proc. Cambridge Philos. Soc. 35 (1939), 133–148.

[23] J. E. Littlewood and A. C, Offord, *On the number of real roots of a random algebraic equation. III*, Rec. Math. [Mat. Sbornik] N.S. 54 (1943), 277–286.

[24] N. B. Maslova, *The variance of the number of real roots of random polynomials*, Teor. Verojatnost. i Primenen. 19 (1974), 36–51.

[25] N. B. Maslova, *The distribution of the number of real roots of random polynomials*, Theor. Probability Appl. 19 (1974), 461–473.

[26] T. Tao and V. H. Vu, *Inverse Littlewood–Offord theorems and the condition number of random discrete matrices*, Ann. Math. 169:2 (2009), 595–632.

[27] D. Kleitman, *On a lemma of Littlewood and Offord on the distributions of linear combinations of vectors*, Advances in Math. 5 (1970), 155–157.

[28] A. Sárközy and E. Szemerédi, *Über ein problem von Erdős und Moser*, Acta Arithmetica 11 (1965), 205–208.

[29] R. Stanley, *Weyl groups, the hard Lefschetz theorem, and the Sperner property*, SIAM J. Algebraic Discrete Methods 1:2 (1980), 168–184.

[30] G. Halász, *Estimates for the concentration function of combinatorial number theory and probability*, Period. Math. Hungar. 8:3-4 (1977), 197–211.

characterization is due to Rudelson and Vershynin[31] who introduced the notion of asymptotic least common divisor.

The new results have found a number of applications. First, it implies most of the classical "forward" results, such as the result of Sárközy and Szemerédi mentioned above. More importantly, it plays a decisive role in the study of random matrices and random functions. Let me single out an application in random matrix theory. Here, a parameter that appears frequently is the distance from a random vector X to a hyperplane H. If the coordinates of X are x_i and the coordinates of the normal vector of H are a_i, then this distance is exactly the absolute value of S. Usually, we know something about H that ensures that the a_i do not have any additive structure. Thus, by the Inverse theory, we can conclude that with high probability, the distance in question is not zero, or X does not belong to H. In fact, the Inverse theory also allows us to replace $P(S = a)$ by the probability that S belongs to a short interval centered at a. This way, we can bound the distance from below. This fact plays a critical role in the study of the least singular value and the circular law.

This is one of the papers I really enjoyed, as while the applications seem to belong entirely to probability, the key new element really comes from combinatorics.

Mansour: Would you briefly explain the notion of *anti-concentration*? In what type of problems is this new phenomenon observed? Would you point out some future research directions?

Vu: Let $\xi_1, \ldots, \xi_n$ be iid real random variables and $F = F(\xi_1, \ldots, \xi_n)$ a real function. A typical anti-concentration result asserts that the probability that F belongs to a short interval I is small. The Littlewood-Offord theorem above is one example when F is a linear function. The Inverse theory provides a fairly satisfactory understanding in this linear case. However, for other functions (such as higher degree polynomials), we are far from having a complete picture.

Mansour: In your paper *Finite and infinite arithmetic progressions in sumsets*[32], coauthored by Endre Szemerédi, published at Annals of Mathematics, you proved an old conjecture of Folkman, by showing that if A is a set of natural numbers of asymptotic density at least $cn^{1/2}$ for sufficiently large constant c then the collection of all subset sums of A contains an infinite arithmetic progression. What were the main new ideas behind the solution of this long-standing conjecture?

Vu: First we made really long arithmetic progressions (APs) and then merged them together. The initial approach to this, in spirit, is somewhat similar to the Green[4]-Tao[5] approach to long APs in primes. We tried to characterize all large sets whose collection of subset sums do not contain a (sufficiently) long AP and found out that the main reason is that the set itself is the sum of two original APs. In this case, the collection of subset sums has the same structure. Thus, the problem became actually a two-dimensional problem. You can think of the collection of subset sums as a rectangle that does not contain any AP longer other than its two sides. However, if c is sufficiently large, the two-dimensional object has to "wrap around" itself and create a long AP. One can achieve this via a random tiling argument, but the details are sort of complicated. It took us several months to write it down after having a fairly convincing sketch. This was the paper where I learned a good deal about additive combinatorics from Endre.

Mansour: In your work, you have extensively used combinatorial reasoning to address important problems. How do enumerative techniques engage in your research?

Vu: Yes, very frequently. However, for us it is more important to have a "soft" estimate on a parameter (like determining its order of magnitude) than to compute it exactly or to achieve a closed formula (which could be very hard to evaluate). As a matter of fact, one of the main purposes of the Inverse theory is to give us a characterization of some "bad" sets so that we can estimate their size (or probability).

Mansour: When we read your papers, we see that combinatorial and probabilistic arguments play an important role in your research. Would you comment on the interplay between

[31] M. Rudelson and R. Vershynin, *The Littlewood-Offord Problem and invertibility of random matrices*, Advances in Math. 218 (2008), 600–633.

[32] E. Szemerédi and V. H. Vu, *Finite and infinite arithmetic progressions in sumsets*, Ann. Math. 163 (2006), 1–35.

combinatorics and probability?

Vu: On one hand, the probabilistic method is one of the most efficient and powerful methods in combinatorics. On the other hand, combinatorial ideas can be used to build new probabilistic tools or lead to new ideas in probability theory.

At a deeper level, I feel that people in certain areas of analysis, combinatorics, and probability are frequently interested in essentially the same things. It is very hard to put names on these things, but usually, they are general phenomena (such as pseudo-randomness or large deviation or anti-concentration or hypercontractivity or expansion). With some experience, one can get a sense of the relevance even when the results appear in rather unfamiliar forms.

Mansour: In your works, you have several references to Paul Erdős. Have you ever met him? Which of his results fascinate you most? There is a published book about him titled as *The Man Who Loved Only Numbers*. Do you think saying *The Man Who Loved Graphs* would be a more precise description of him?

Vu: Yes. I met Erdős twice. Both times were memorable. The first time, I was a student in Budapest. Kati (Vesztergombi) introduced me to Erdős while he was visiting the Renyi institute (maybe around 1992). I did not know about the meeting in advance and did not prepare any math problems, so I blew my chance to have an Erdős number 1 (my number is still 2). We did talk a bit about the Vietnam war instead.

The second time I was a graduate student at Yale. He gave a title lecture there. Then we talked a bit about my thesis. He refused to believe our result with Noga Alon[8] on coin weighing. The problem was this: given n coins of two possible weights. How many weightings does one need to be sure that they are all the same (or not)? (As usual, at one weighting, one can weigh any k coins against another k). The answer was $\log n/\log\log n$; but he insisted it should be $\log n$, so strongly that at some point I started to panic and thought that something was really wrong.

Erdős wrote so many influential papers in so many different areas of mathematics, not only number theory and graph theory. If one looks at logic or probability or analysis, one finds fundamental results bearing his name. What about the Man who loves mathematics?

Mansour: Is there a specific problem you have been working on for many years? What progress have you made?

Vu: I am working on some basic questions concerning random polynomials and random matrices. So basic that it is embarrassing that we cannot settle them. But maybe one day...

Mansour: Professor Van Vu, I would like to thank you for this very interesting interview on behalf of the journal *Enumerative Combinatorics and Applications*.