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Interview with Einar Steingrímsson

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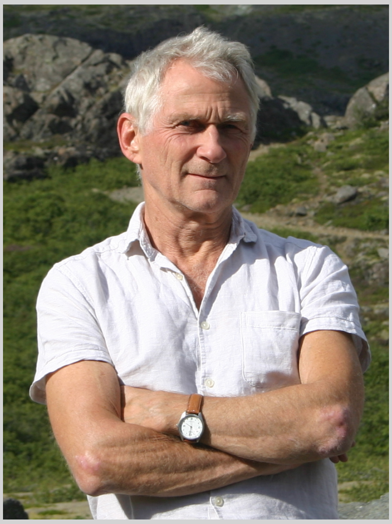

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Einar Steingrímsson completed his undergraduate studies at the University of Pennsylvania in 1987. He obtained a Ph.D. from the Massachusetts Institute of Technology (MIT) in 1992 under the supervision of Richard Stanley. He is currently a research professor of mathematics at the University of Strathclyde. Professor Steingrímsson has held visiting positions at Institut Mittag-Leffler; University of Florence; University of California, Berkeley; and Université de Bordeaux 1. Some of his research-related activities include Head of the Strathclyde Combinatorics Group (2011-2021), Head of the Reykjavik Combinatorics Group (2005-2010), Head of the Combinatorics Group at CTH/GU in Gothenburg (1998-2005), Member of the Editorial Board of Enumerative Combinatorics and Applications (since 2020), and Pure Mathematics and Applications (since 2015).

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

Steingrímsson: I will not say that defining combinatorics is as hard as defining what art is, but combinatorics has become quite a wide discipline in the last few decades. Of course, enumeration of families of discrete objects decidedly belongs to combinatorics, as do bijections between different such families that preserve structural properties and statistics. But then you have topological properties that, for discretely defined structures, turn out to depend only on data that are clearly combinatorial in nature, such as the Euler characteristic or Discrete Morse Theory. A question this raises is whether more highly developed continuous math can be "reduced" to combinatorics. And although continuous analysis will never lose its value, an intriguing question I allow myself to ask, in my total ignorance of modern physics, is whether not just energy but also time and space are quantized, so that discrete models would be the "true" picture of the universe.

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

Steingrímsson: It is great to see how combinatorics has been mixing with many other disciplines in the last few decades, such as probability and various physics models. And this is a cliché, or maybe just prejudice, but when a field of mathematics shows strong links to other branches that certainly bolsters how interesting we find it, since that is a hint that it is dealing with some "deep" mathematical properties.

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

Steingrímsson: To the extent that I have had any particular goals in my research, they have mostly arisen after the fact, namely, from minor discoveries that pointed to something more general lurking beneath the surface. It may

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look like a goal of mine has been to translate everything I can into properties and statistics of permutations, but that is rather a consequence of permutations being my "hammer" and my thinking everything I see is nails.

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?

Steingrímsson: My father, who was a very good mathematician, although he never pursued research in that direction, would sometimes give me interesting problems to work on when I was a kid. I have often thought that the school system should be doing more of this, rather than just drilling students in using standard methods to solve standard problems. Hopefully, that is getting better, but even though I found math easy in school I was never very interested (nor in any school work in general) until I went back to finish high school when I was 26, having dropped out to do different kinds of work for many years.

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

Steingrímsson: I do not remember any specific such problems, but I do remember thinking about simple combinatorial problems at a young age, such as counting the number of "words" one can form from a given multiset of letters. What made me fascinated with combinatorics, as an undergraduate, was when I found out about the algebraic combinatorics that was flourishing at the time.

Mansour: What was the reason you chose MIT for your Ph.D. and your advisor Richard Stanley?

Steingrímsson: I somehow knew early on as an undergraduate that I wanted to do combinatorics, and when I heard about the algebraic combinatorics being done at MIT, pioneered by Stanley and Rota, that sounded very attractive. Incidentally, I always wanted to take the combinatorics course at Penn taught by the late and great Herb Wilf, but it always clashed with other courses I had to take. So I went to MIT to do combinatorics, without ever having taken a course on the subject, but I quickly realized that my instinct was right; this was exactly what I found most attractive of all the math I had seen.

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

Steingrímsson: The problem that led to the work in my thesis was rather trivial, and I needed to solve it because of something else I was working on, which never worked out. Namely, I needed to generalize the definition of excedance in permutations to the signed permutations of the octahedral group, aka the type B Coxeter groups, and this definition had to have certain properties echoing properties of the already defined descent. Once I figured out how to do this I saw that there was an obvious way to extend both these definitions to what are now called colored permutations, which are simply permutations where each letter has any one of k colors for some fixed k. And then I realized that one could ask which of the myriad results for ordinary permutations would generalize in a nice way to these colored permutations, and I managed to do several of these[1].

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

Steingrímsson: I have not often set out to work on a known hard problem (one exception being my paper[2] with Claesson and Jelínek on avoiders of 1324), but mostly started from some simple observations that have begged the turning of some stones under which nice things happened to be found.

One rather innocent observation that led to some interesting results was when I mentioned to Eric Babson that most permutation statistics are based on inversions of various kinds. For example, an occurrence of the pattern 132 is a subsequence with an inversion only between the last two of the letters. Eric asked me to explain what I meant, and when I wrote down some examples of this, such as how the major index or the statistic MAK defined by

[1] E. Steingrímsson, *Permutation Statistics of Indexed and Poset Permutations*, thesis, MIT, 1992, see `https://dspace.mit.edu/bitstream/handle/1721.1/35952/26391022-MIT.pdf?sequence=2`.

[2] A. Claesson, V. Jelínek, and E. Steingrímsson, *Upper bounds for the Stanley-Wilf limit of 1324 and other layered patterns*, J. Combin. Theory Ser. A 119(8) (2012), 1680–1691.

[3] D. Foata and D. Zeilberger, *Denert's permutation statistic is indeed Euler-Mahonian*, Studies in Appl. Math. 83 (1990), 31–59.

Foata and Zeilberger[3] could be expressed in this way, we noticed that they (and a few more Mahonian statistics) all had the same form when written as combinations of what are now called vincular patterns. This very simple "theoretical observation" about inversions then led to our paper *Generalized permutation patterns and classification of the Mahonian statistics*[4].

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

Steingrímsson: This is absolutely the case for many enumerative problems on "regular" structures like permutations. For example, if you have two statistics on permutations that are defined in a sufficiently simple way, and you have verified that they have the same distribution on permutations of length n for each n up to 12 (or whatever is computationally feasible to check by brute force), then you often "know" they must be equidistributed for all n because whatever could fail would fail already for smaller n. I am convinced there is a nice meta-theorem to this effect, but I do not know how to formulate it so that it could be proved.

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

Steingrímsson: I do not have a good answer to this question. I suppose we could agree on some such results as being seminal, but it is also easy to under- and over-estimate the long-term effects of what has been done in the near past.

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

Steingrímsson: The number of permutations avoiding the pattern 1324. I do not think the answer will be terribly exciting *per se*, but it looks like we will have to come up with some novel way to think about this if we are to crack it. And those methods I hope will provide tools for solving much more. I also hope this gets solved soon, at least for $n = 1,000$, because I stand to lose 170 euros to Doron Zeilberger if that does not happen by 2030.

The conjecture Natasha Blitvić and I have, that the enumerating sequence of permutations avoiding any given classical pattern is a moment sequence. This conjecture was first made by Andrew Elvey Price for the pattern 1324, and Clisby, Conway, Guttmann and Inoue[5] have conjectured this for all such patterns of length 5, based on extensive numerical analysis. We had arrived at this conjecture independently, not so much numerically at first, but by looking at some of the combinatorial constructions that show up in noncommutative probability and thinking of ways in which these could naturally generalize. Should something like this indeed be true, it would give us an entirely new angle on pattern avoidance and open up new tools with which to study these very difficult enumerative problems.

Finding a combinatorial proof of the symmetry of the distributions of area and bounce on Dyck paths[6], that is, showing that (area, bounce) has the same distribution as (bounce, area). I am not sure such a proof would have significant consequences, but this is irresistibly attractive because of its simplicity and how hard it seems to be.

Finally (you did not really think I would just give three, did you?), the meta theorem mentioned above.

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

Steingrímsson: I do not have any particular such wishes or hopes. I am always happy to see my work leading to work by others, this is very gratifying, but it is hard to predict what others may find interesting, let alone want to work on. If I had to mention one problem I would really like to see some kind of solution to it would again be the meta theorem mentioned above, but such a result need not be a continuation of my work in any way, even though the idea came to me because of the results in my paper with Babson[4], and speculations connected to that.

Mansour: Do you think that there are core or mainstream areas in mathematics? Are some

[4] E. Babson and E. Steingrímsson, *Generalized Permutation Patterns and a Classification of the Mahonian Statistics*, Sém. Lothar. de Combin. 44 (2000), Article B44b.

[5] N. Clisby, A. R. Conway, A. J. Guttmann, and Y. Inoue, *Classical length-5 pattern-avoiding permutations*, 2021, arXiv:2109.13485.

[6] J. Haglund, *The q,t-Catalan numbers and the space of diagonal harmonics*, Volume 41 of University Lecture Series, American Mathematical Society, Providence, RI, 2008. With an appendix on the combinatorics of Macdonald polynomials.

topics more important than others?

Steingrímsson: At any given time there are areas with more exciting activities than others. And some topics eventually "die", because they get exhausted instead of leading to more and more good questions. But it is hard to guess what will flourish and what not, and it's a bad idea to try to talk down other areas since such prejudice can easily have a detrimental effect on people's willingness to work in a field that later might prove fruitful.

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?

Steingrímsson: We who are attracted to pure math, that is, the kind of math driven by sheer curiosity that is not motivated by any real-world applications, should be grateful for all the successful applied mathematics because it is probably the main reason societies put so much money into pure math research. Of course, most applied math is built on what once was pure math, and often striking applications of enormous financial or other societal worth come from discoveries in pure math, which is a good reason for supporting pure research. This also sometimes works the other way, namely that math driven by applications develops its own pure research programs when solutions to real-world problems expose things that are interesting in their own right without necessarily having real-world applications. In discrete math, I guess the discovery of error-correcting codes is one of the prime examples, with its connections to sphere packing and other problems that have a very "pure" appeal.

Mansour: What advice would you give to young people thinking about pursuing a research career in mathematics?

Steingrímsson: I used to say that this should be approached as thinking about becoming an artist, namely that you should only do it if it was a burning desire and you felt it was the only thing you could imagine doing since you need to invest a lot of time before knowing if you will succeed. But for a long time now, pursuing pure math research has entailed no risk. Namely, people with strong math skills, not to mention if they can also program, will probably always have lots of good and interesting jobs to pick from should they give up on an academic career. And many such jobs today include a significant research-like component.

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

Steingrímsson: I do not think this is interesting for others to hear, since I do not have any exotic interests. I like hiking and camping in the wilderness, preferably rather barren such with wide, open spaces. And I like climbing, but although I have never been a good climber, I was lucky enough to start doing this in Iceland when you did not have to be good to be able to do first ascents all the time.

Mansour: One of your research positions was at *Decode Genetics*. This was during 2001-2003. We read on their page that since the founding of Decode Genetics in 1996, they have been focused on discovering genetic risk factors for common diseases, and work to face this challenge by using the latest technology for analyzing DNA to assemble as much data as possible across a large and well-defined group of people. Would you tell us about the role of combinatorics, specifically enumerative combinatorics, in such research?

Steingrímsson: When I started working at Decode they were basically hiring any mathematicians they could get their hands on, and I was told that I could just pick some problem they were having a hard time dealing with in a computationally effective way and try my hand at this. One problem I worked on was how to split large family trees into chunks small enough to do analysis on common genetic markers while minimizing the loss of genealogical information resulting from this cutting into pieces. Another was to adapt known algorithms for haplotyping a set of individuals, in order to use information about their kinship (which is the idea Decode was built around, to exploit the near-total knowledge of how any two Icelanders are related to each other going centuries back). I do not think anything came of my work there, and I do not know much about the state of the art today.

Mansour: One of your very recent projects is on connecting physics models via permutations. Would you tell a little bit more about this project and such inter-connecting models?

Steingrímsson: There is a series of papers by Sylvie Corteel and Lauren Williams[7] where they show how the so-called permutation tableaux (special cases of the Le-tableaux introduced by Postnikov[8]) capture the steady-state distribution of the Asymmetric Exclusion Process[7] (ASEP), a model in statistical mechanics. In papers by Dukes, Selig, Smith, and myself[9,10,11] we show how another statistical mechanics model, the Abelian Sandpile Model (ASM), on certain graphs, is in bijective correspondence with another type of tableaux, which we call EW-tableaux (after Ehrenborg and van Willigenburg[12], who first defined them). We found a bijection between these two kinds of tableaux, which thus connects the ASEP and the ASM. What I would like to see is if this connection can be exploited to obtain more information about each model, and to generalize the ASEP, which is essentially defined on a path graph.

Mansour: In joint work with Antonio Bernini, Matteo Cervetti, and Luca Ferrari, *Enumerative combinatorics of intervals in the Dyck pattern poset*[13], you have initiated the study of the enumerative combinatorics of the intervals in the Dyck pattern poset. What are the main results of this line of research? Would you point out some future research directions?

Steingrímsson: The results here are very scant so far. We gave results on the size, and rank function, of some classes of intervals in this poset, and formulas for the Möbius function on one small class. It is a little surprising how hard it seems to get a grip on these intervals because this poset seems well behaved in some respects. In particular, it seems to have alternating Möbius function — so these intervals might all be shellable — in contrast to the poset of permutations ordered by pattern containment, which is surprisingly messy. On the other hand, intervals here can have enormously large Möbius function, relative to their rank. It took a while to get significant results on the Möbius function of the permutation pattern poset, but many such have been obtained, as well as some understanding of the topology of that poset. So it may simply take some hard work to get good results on the Möbius function and topology of the Dyck pattern poset, and there is a good reason to believe that it is much better behaved and thus allows for much more general results.

Mansour: One of your papers, co-authored with Peter R. W. McNamara, *On the topology of the permutation pattern poset*[14], contains the first explicit major result on the topology of intervals. Would you tell us more about this result and recent other results motivated by this paper?

Steingrímsson: The Möbius function of a poset is just one invariant of its topology. So an obvious question after getting some intelligible results on the Möbius function is always whether it is possible to understand the topology in a more general way. In particular, one wants to know whether intervals are shellable, which has strong implications for the topology. This is what Peter and I set out to do, and we did get some results in this direction. There have been a few more papers on this, by Jason P. Smith[15], and by Elizalde and McNamara[16] on the related poset of consecutive patterns, but this is wide open for more general cases, which I am sure can be solved.

Mansour: You have published a series of papers on *the Möbius function*. Would you elaborate more on these works?

[7] S. Corteel and L. K. Williams, *Tableaux combinatorics for the asymmetric exclusion process and Askey-Wilson polynomials*, Duke Math. J. 159:3 (2011), 385–415.

[8] A. Postnikov, *Total positivity, Grassmannians, and networks*, 2006, arXiv:math/0609764.

[9] T. Selig, J. P. Smith, and E. Steingrímsson, *EW-tableaux, Le-tableaux, tree-like tableaux and the abelian sandpile model*, Electron. J. Combin. 25(3) (2018), Article 3.14.

[10] M. Dukes, T. Selig, J. P. Smith, and E. Steingrímsson, *Permutation graphs and the Abelian sandpile model, tiered trees and non-ambiguous binary trees*, Electron. J. Combin. 26(3) (2019), Article 3.29.

[11] M. Dukes, T. Selig, J. P. Smith, and E. Steingrímsson, *Permutation graphs and the Abelian sandpile model, tiered trees and non-ambiguous binary trees*, Electron. J. Combin. 26(3) (2019), P3.29.

[12] R. Ehrenborg and S. van Willigenburg, *Enumerative properties of Ferrers graphs*, Discrete Comput. Geom. 32(4) (2004), 481–492.

[13] A. Bernini, M. Cervetti, L. Ferrari, and E. Steingrimsson, *Enumerative combinatorics of intervals in the Dyck pattern poset*, Order 38 (2021), 473–487.

[14] P. R. W. McNamara and E. Steingrímsson, *On the topology of the permutation pattern poset*, J. Combin. Theory Ser. A 134 (2015), 1–35.

[15] J. P. Smith, *A formula for the Möbius function of the permutation poset based on a topological decomposition*, Adv. in Appl. Math. 91 (2017), 98–114.

[16] S. Elizalde and P. R. W. McNamara, *The Structure of the Consecutive Pattern Poset*, International Mathematics Research Notices 2018:7 (2018), 2099–2134.

Steingrímsson: My work on this, with various coauthors, has mostly been on the Möbius function of the poset of permutations ordered by pattern containment (which captures all information about containment of patterns in permutations). It is interesting how this all started. In a 2002 survey paper on permutation patterns, Herb Wilf mentioned the Möbius function of this poset, saying essentially that this "should be done". A year later, at the first conference on permutation patterns in Otago, he said he had looked at this, together with his student Aaron Jaggard, and had come to the conclusion that this was a horrible mess, which I have sometimes taken the liberty to quote as "It is a mess, do not touch it!". I had started looking at this around the same time and was also baffled and depressed that I was unable to come up with any interesting results.

The first results on this were given by Sagan and Vatter[17] in 2006, and then Bridget Tenner and I[18] managed to get some results on small classes of intervals in 2010. Then in 2011, with Burstein, Jelínek, and Jelínková[19], we came up with results on some more substantial classes. Since then, this has been solved for many more interesting cases, by several authors, but there are still no very general results. I am not sure there will ever be any effective master formulas, but I am fairly optimistic there will be significant progress compared to where we are today on this.

Mansour: One of your recent interesting results, co-authored with Natasha Blitvić, is *Permutations, moments, measures*[20]. Therein, you study which combinatorial sequences correspond to the moments of probability measures on the real line. Would you tell us about the main ideas behind this result?

Steingrímsson: The main idea here comes from two observations. On the one hand, surprisingly many of the classical sequences in combinatorics are moment sequences of well known (and some less known) probability distributions. On the other hand, many of the fundamental constructions in probability and mathematical physics have some sort of combinatorial "underpinning". We already talked about the combinatorics of the ASEP and the ASM, but there are also many, many examples of combinatorics enabling various forms of diagrammatic calculus with which to tackle problems in probability, analysis, physics, etc.

So one of the things we want to understand is what types of combinatorics could play such a structural role and, conversely, whether having this kind of additional probabilistic/analytic structure may shed a new light on hard problems in combinatorics. I should say that I am a novice on the probability side, so those ideas are entirely Natasha's, whereas she also came up with some of the crucial ideas on the combinatorial side to make this work.

The central object of our paper is a continued fraction $\mathcal{C}$ with 14 parameters, whose specializations give counting sequences for lots of combinatorial objects, which, by properties of $\mathcal{C}$, are all moment sequences. In addition to the classical objects involved — permutations, set partitions etc. — this also led naturally to what we call the *k-arrangements*, permutations with k-colored fixed points, which do not seem to have been studied much, but clearly have lots of interesting properties.

Mansour: You have advised several graduates and postdocs. How important is working with Ph.D. and post-doc students and passing knowledge to them? Do you keep working with them?

Steingrímsson: I have only had four Ph.D. students, but I was extremely lucky; they are all very active and strong researchers. I have also had many postdocs, most of whom have also gone on to do great work. I have worked with many of them after being their advisor, and still do, which is a great pleasure. I can safely say that my own work would be immensely poorer were it not for working with these talented people, both because I have learned a lot from them and been driven to do interesting things through collaborations with them, which is probably easier, given the greater common background I have with them than with combinatorialists in general.

Mansour: In your work, you have extensively used combinatorial reasoning to address im-

[17] B. E. Sagan and V. Vatter, *The Möbius function of a composition poset*, J. Algebraic Combin. 24 (2006), 117–136.

[18] E. Steingrímsson and B. E. Tenner, *The Möbius function of the permutation pattern poset*, J. Combin. 1:1 (2010), 39–52.

[19] A. Burstein, V. Jelínek, E. Jelínková, and E. Steingrímsson, *The Möbius function of separable and decomposable permutations*, J. Combin. Theory Ser. A 118 (2011), 2346–2364.

[20] N. Blitvić and E. Steingrímsson, *Permutations, moments, measures*, Tran. Amer. Math. Soc. 374(8) (2021), 5473–5508.

portant problems. How do enumerative techniques engage in your research?

Steingrímsson: Although I like being able to enumerate new families of interesting objects, my curiosity has always been more about the structure of such families, and in particular the understanding of structural similarities between equinumerous families. But I have often used brute force enumeration — constructing all objects of a given family by computer to compute various statistics on them — to get some ideas about such structure. For example, if you have a family of objects equinumerous to permutations, and you see that a certain statistic on them has the same distribution as the number of descents on permutations, then that is evidence of a structural similarity that may make it easier to find a structure-preserving bijection.

Mansour: Would you tell us about your thought process for the proof of one of your favorite results? How did you become interested in that problem? How long did it take you to figure out a proof? Did you have a "eureka moment"?

Steingrímsson: I have mentioned before how Eric Babson and I came up with the core idea for our paper on generalized (vincular) patterns. I was interested in this very trivial observation that most permutation statistics are about inversions in some way, and when we wrote down what I was trying to say about various Mahonian statistics there was certainly a eureka moment in observing the regularity of their expressions as pattern combinations, and Eric came up with a proof of why that was inevitable in a few hours. What I like most about this paper is that the proofs are very simple; one of the core proofs is about showing that a 4×4 matrix of small binomial coefficients is invertible. In fact, this is the only paper of mine that I can read through and understand without ever getting bogged down in details that seem very complicated so long after I had them fresh in my mind.

Incidentally, this paper contains one claim that is not proved, and which turns out not to have the simple proof that I thought it would. This was pointed out by Bruce Sagan, and if we cannot soon find a proof we will have to post an erratum, rather than a corrigendum. Fortunately, this does not kill any of the main results, only restricts them, to the cases that are considered explicitly in the paper. But this may be an interesting question to study, so look out for that erratum!

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

Steingrímsson: I have occasionally worked for a while on trying to find a bijective proof of the symmetry of the distribution of area and bounce on Dyck paths, as I mentioned before. I have made no real progress towards that goal, just found a couple of minor results (unpublished) that do not help in solving the problem. I have said for many years that this will be my retirement research because it is so easy to look at this from many different angles, and so to try new things all the time, rather than having to build a big "theory". But even though I am now officially retired from Strathclyde (while retaining my connection to the university as a research professor) I still have much too much research I want to work on in the near future to switch to retirement mode yet.

Mansour: You have a minor degree in Philosophy so we would be happy to ask you some philosophical questions. A central claim of existentialism is that *existence precedes essence*. Would you explore this claim as a mathematician?

Steingrímsson: I should first say that I am of course no more than a layman in philosophy. Despite that (or maybe because of that) I am happy to respond `:)`. What I concentrated on in my philosophy studies was the philosophy of mind, since I have always been fascinated with the (poor but fast-improving) understanding of how our minds work. But this is, I think, a stellar example of how philosophy works at its best; the research has more or less left the realm of philosophy because the natural sciences (including computer science) have developed methods for answering the questions posed by philosophers.

As for the claim you mentioned, and much of what I have seen of the philosophy of the last century, I find these questions less and less interesting, and although I know this may be due to my ignorance I do not hesitate to say, if I want to provoke philosophers to convince me otherwise, that they are pointless. A good example of such questions is the one about what it means for something to be true, which I have a hard time seeing as meaningful. If I wanted to give this (admittedly arrogant novice's) stance a name, I would call it *isism*: Most things just are what they are, as we see them superficially, and if you try to understand them by picking them apart you will be left with meaningless parts, not any deeper understanding of the whole thing. This is just like in math, where most questions are unanswerable in any interesting way. In particular, I think Sartre got this dead wrong `;)`. Of course, even though almost all questions have no good answers, that still leaves plenty of good ones.

Mansour: The first sentence of Tolstoy's novel Anna Karenina is: "Happy families are all alike; every unhappy family is unhappy in its own way." Is it true, what do you think?

Steingrímsson: I have often quoted this because I think there is an interesting truth to it. Happiness, in some sense, means no problems. And it is problems that are varied and interesting, not non-problems, just as in math. Of course, although we want people to find the math we work on to constitute interesting problems, most of us would rather not have an "interesting" family life.

Mansour: Have you figured out why we are here?

Steingrímsson: That is obvious to an isist: There is no reason, we just are.

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