As DISCRETE AND COMBINATORIAL GEOMETRY (abbreviated to DCG in the sequel) turns 20, due to the fortunate effect of my longevity I can contemplate the development of Discrete Geometry over the last 50 years, and the role DCG played in that development.

My good fortune was strengthened by the acquaintance – and in several cases friendship – with many great mathematicians of the third quarter of the twentieth century, that had a more than passing interest in Discrete Geometry. This list would include H. Buseman, H. S. M. Coxeter, L. Danzer, A. Dvoretzky, P. Erdös, L. Fejes Toth, W. Fenchel, H. Hadwiger, V. Klee, L. Moser, T. S. Motzkin, H. Rademacher, G. Ringel, I. J. Schoenberg, G. C. Shephard, and others, as well as many younger people that are still actively producing research mathematics.

In the 1950's there were no journals devoted to Discrete Mathematics. In fact, most journals were of a general character. The acceptable (and published) papers in this field were, on the whole, at a much lower level of technical complication and conceptual sophistication than has become the rule in later years.

As a side-effect of the increasing specialization of publications (and of mathematicians) several new journals were started, devoted mainly to Discrete Mathematics and some more particularly to Discrete Geometry. Unfortunately, this was accompanied by takeover of Enseignement Mathématique and of Geometriae Dedicata by editorial boards or publishers that were ignoring the original aims of the journals. The same is true for many conferences, such as the "Coxeter Legacy" where more than a half of the papers and presentations were well beyond what Coxeter would have appreciated.

The early development of Discrete Geometry, and of Discrete Mathematics in general, was fueled by the many easily stated unsolved problems that were circulated in the 1950's and 1960's. Prominent among these were the problems that Hugo Hadwiger regularly published in Elemente der Mathematik, as well as the many papers and talks by Paul Erdös that challenged the imagination of a generation of young mathematicians. A collection of such problems was privately circulated by Leo Moser (in 1963); since 1984 it was repeatedly expanded by W. Moser and by J. Pach, and distributed to many mathematicians. Finally, after additional updates, the expanded collection was published recently in book form by Brass, Moser and Pach [4]. Another collection of unsolved problems was widely circulated in the 1960's in mimeographed form by Vic Klee [20] and led to a number of papers; it was meant to form part of a joint project with P. Erdös, L. Fejes Tóth and H. Hadwiger, but this never materialized. Instead, Hadwiger collected and expanded his problems proposed earlier in a booklet, coauthored with Hans Debrunner [17]; English and Russian translations, both including additional material, were prepared by V. Klee, and by S. S. Ryškov and I. M. Yaglom, respectively.

Over the fairly recent years, Discrete Geometry — that originally consisted mainly of the theory of packing, covering and tiling, — expanded vastly to enclose many other geometric topics, such as configurations of points, lines, planes, etc., Helly-type results, the structure of polytopes, Erdös-type distance problems, tessellations, geometric graphs, and many others. As mentioned by W. Kuperberg [22] in his review of [4] the lines separating discrete geometry from the theory of convex polytopes, combinatorics and graph theory became blurred.

A long-running department in the American Mathematical Monthly (xxxx-xxxx) promoted unsolved problems, many of a Discrete Geometry nature. Similarly, some of the problems in a section of the journal Discrete Mathematics (xxxx-xxxx) were of a Discrete Geometry nature. Other collections of Discrete Geometry problems that circulated for many years was that of Harald Croft. It was expanded into a well-received book [6] coauthored with K. J. Falconer and R. K. Guy. Vic Klee and Stan Wagon published an interesting collection of solved and unsolved problems [21].

The availability of great computing power and computer graphics has had an invigorating effect on many topics in Discrete Geometry, and has been wholeheartedly embraced by most practitioners. As with all new tools, new questions arose concerning the computational difficulty of various questions. This led to many of the advances featured in DCG.

Several developments can serve to illustrate the changed status of Discrete Geometry and Discrete Mathematics in general. One, already mentioned, is the almost unimaginable deepening of the mathematics involved. Whereas earlier publications can be said to present the easy pickings in the fields they cover, the tendency of the more recent works is to tease out the finer and harder results. Many of the latter require very careful estimates and ingenious constructions.

In many branches of mathematics the past few decades have seen the solution of old problems that have stymied researchers for decades or longer. In the theory of convex polytopes, some 35 years ago came the almost simultaneous proofs of the upper bound conjecture by Peter McMullen [26], and of the lower bound conjecture by David Barnette [2], [3]. These advances served as the starting point for the deep and detailed study of various aspects of convex polytopes, many in DCG. But these developments have been in a certain sense very simple compared to other advances in Discrete Geometry and related fields. I have in mind the proof of the four-color theorem by Appel and Haken [1], and of the Kepler conjecture by Hales [18]. In both cases, the degree of complexity was such that reliance on a very extensive and sophisticated computational component was unavoidable; as a consequence, checking the proofs has become a very major undertaking, with only few people having the resources and the inclination to verify all details.

However, it should also be noted that in some of the widely publicized advances in other fields (Andrew Wiles' solution of the Fermat problem, Grigory Perelman's work on Poincaré conjecture) the verification has become entangled in difficulties due not to the use of computers but because of extremely advanced and specialized results from a variety of other fields – to such an extent that even collectives of referees have been stumped.

Let me turn now to other important – even though less spectacular – advances in Discrete Geometry concerning topics with which I am personally more involved. It will be noted that this explicitly excludes a large part of the works published in DCG and devoted to various other aspect of Discrete Geometry, and to all of Computational Geometry, with which I am not sufficiently familiar.

The investigation of Venn diagrams was once considered as ending in the three circles made popular in very basic math courses. It has since blossomed into a very sophisticated geometric discipline, with connections to group theory, lattice theory and other branches. I flatter myself that this development started with my papers [10] and [11], see Figures 1 and 2. It is amusing to note that [10] was rejected by both the American Mathematical Monthly and the Mathematical Gazette, before being accepted by the Mathematics Magazine and then earning the Allendoerfer award of the Mathematical Association of America. Recent years have brought spectacular advances in the understanding of Venn diagrams, while still leaving many unsolved problems that are easy to formulate and understand. The extent of the development and changed status of the topic is best seen in detailed survey given by Frank Ruskey [29] and the recent paper [28], the lead article in the December 2006 issue of the Notices of the AMS. But the outstanding question in the topic – whether simple symmetric Venn diagrams with 11 of more sets exist – is still open.

The theory of configurations of points and lines in the plane was somnolent for almost a century, despite the book by Levi [23], the chapter on configurations in the popular book by Hilbert and Cohn-Vossen [19], and several papers by Coxeter (in particular, [5]). More recently, the study of configurations took off due to several developments. On the one hand, the first ever diagrams of (n_4) configurations were produced [15], see Figure 3 (see also Figure 4). On the other hand, T. Pisanski and M. Boben recently found serious errors in basic results concerning the enumeration and construction of (n_3) configurations; these results were supposed to have been established long ago – in the nineteenth century – by V. Martinetti [24] and Steinitz [30]. Also, applications of computer algebra yielded the fact that for $n \leq 12$ all configurations possible in the real Euclidean plane are possible in the rational plane as well. Each of these directions led to many new investigations and unexpected results, as well as lots of open questions; a recent survey with detailed description of results and problems is [13].

The theory of arrangements of lines in the plane, and more generally of hyperplanes in higher dimensions, went far beyond the simple questions considered since the times of Steiner nearly two centuries ago. Many extremal and other problems have been considered, and relations to algebraic geometry and other fields investigated. Among other open questions is the problem of determining all simplicial arrangements, still unsolved even in the plane; see Figure 5. Several recent surveys of arrangements (in the plane, and in higher dimensions) are available, together with indications of their use in various fields and many open problems. In particular, we should mention [7], Chapter 5 of [8], and parts of [25] and [4].

The theory of tilings, in particular in the plane, has roots going to antiquity. More recently it has become quite popular, in part because of its relation to aperiodic and quasiperiodic tilings. Starting with [16], this has engendered many books and articles – several in DCG. Many of the publications are related to physical aspects.

The theory of not-necessarily convex polyhedra in the Euclidean 3-space has also had significant advances. This topic stagnated since early in the twentieth century, and was revived towards its end. The renewed interest led to the consideration of several specific classes of such polyhedra, but more importantly it underlined the need for a consistent theory of polyhedra more general than the convex ones. This has now been developed (see [12], and Figure 6). It turned out to essentially coincide with the 3-dimensional case of the "abstract polytopes" of McMullen and Schulte [27]. However, the geometric reach of this work on abstract polytopes is diminished by the insistence on what the authors term "faithful representations".

One additional new phenomenon is the widespread collaboration by multiple authors. While joint publications by two authors have long been an accepted feature in journals (as well as for books), the recent years have seen a surge in papers with three, four, or more authors. This is in part attributable to the ease of communication made possible by email and other electronic means. The possibility of quick interchange led to much faster spread of ideas. In turn, this led to the many new approaches evident in the papers published by DCG and elsewhere.

Another factor that may contribute to the spread of joint authorship is the regrettable fact that the electronic availability of a great portion of the literature is very uneven. In many large research institutions (such as my home university) people enjoy almost unlimited access, free to the individual, to the digital publications and repositories. In contrast, many of the workers at smaller institutions are not as fortunate. It is a sad fact that even the pricing of Mathematical Reviews (or MatSciNet) is imposing a heavy burden on people in such institutions.

On the debit side of this proliferation of joint authorships and of papers in general one has to keep in mind the tremendous pressure on young researchers to come up with a long list of publications at the time of promotion and/or tenure, and even of primary employment.

After surveying some of the directions of Discrete Geometry, a question that arises naturally is: Where is Discrete Geometry going? The only honest answer I can give is that I do not know. It is extremely hard to handicap the many emerging directions of investigation. For me, this uncertainty is increased by the very reason that led to the writing of this article. The longevity that gives perspective on the past implies, as a corollary, reaching old age. This, in turn, means a poor understanding of novel ideas and a regrettable tendency to see the future as a continuation in the tracks made in the past.

Finally, what about the future of DCG?

Excellent as the record of this journal has proved itself over the last two decades, I would venture to make three suggestions.

One is the active recruitment and solicitation of surveys of the different directions in which Discrete Geometry is actively developing. These should not be surveys written to popular consumption – readers unfamiliar with discrete geometry could hardly be expected to read them. Instead, the surveys should be authoritative accounts meant for generally knowledgeable people not specializing in a particular field.

Another is motivated by the availability of online versions of the published papers; this is certainly a step in the right direction. But the utilization of web-based possibilities could be enhanced by having a parallel online repository of detailed accounts of which only short reports would appear in the printed journal. This could be used for extensive tables or collections of diagrams, of accounts of proofs the length of which makes them unsuitable for the printed version. It could also be used for the surveys mentioned above, which in this mode could be kept up-to-date much more easily that in print.

Lastly, it is a fact that besides the academically oriented activities reflected in journals and meetings, there is a "parallel universe" of people communicating through the web, at a variety of levels of knowledge, but with a very high degree of enthusiasm. Many parts of the communications happening there are best left alone – because they reflect ignorance of well-known facts. However, the enthusiasm and energy invested in these web pages often contain genuinely new knowledge and interesting ideas and problems. It would be worthwhile to try to establish a connection with this universe, and make the interesting parts available to the academic community in the pages of DISCRETE AND COMBINATORIAL GEOMETRY.

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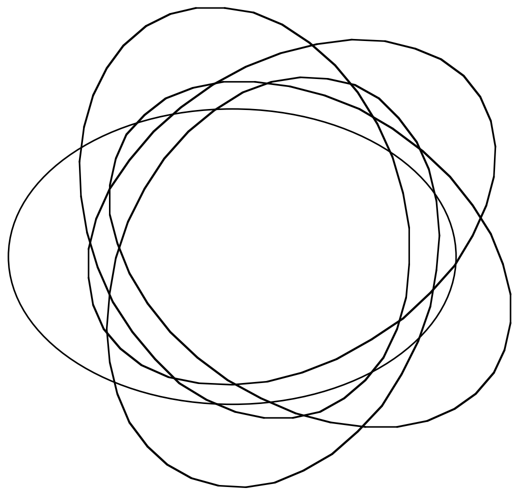


Figure 1. A simple symmetric Venn diagram of five ellipses (from [10]). "Simple" means that no three of the curves have a common point, and "symmetric" means that the curves are all congruent under rotations about a single point.

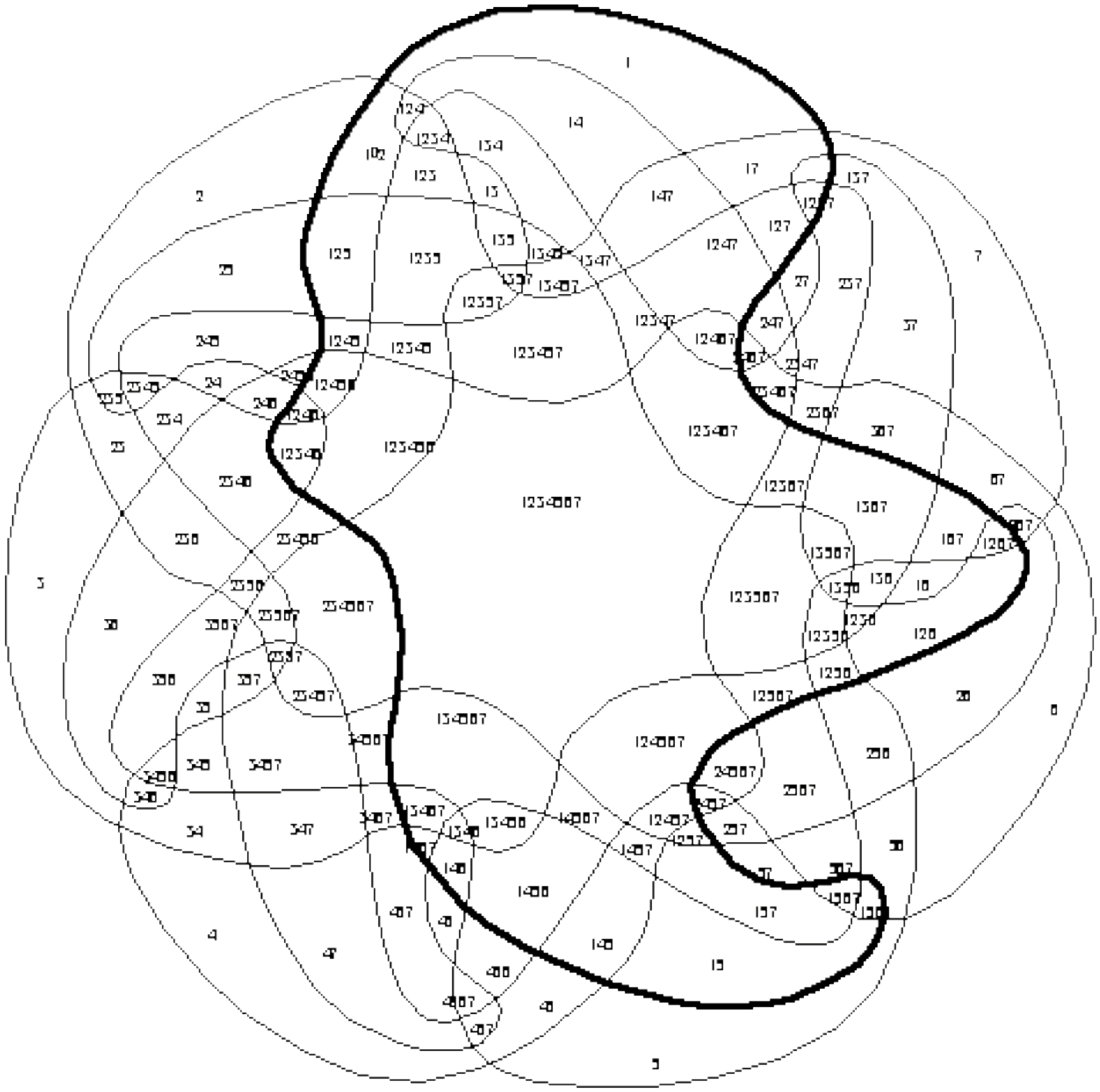


Figure 2. The first simple symmetric diagram of seven sets (from [11]).

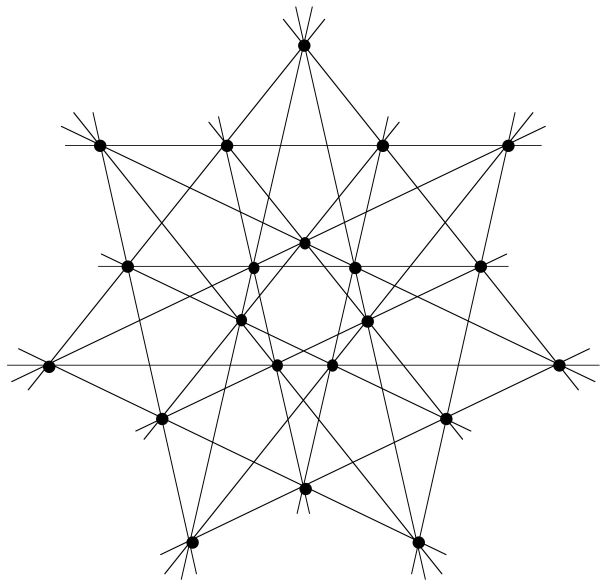


Figure 3. A configuration (21_4), from [15]. For a long time this was the smallest configuration (n_4) known, and has been conjectured to be the smallest possible. However, this is not the case, see Figure 4.

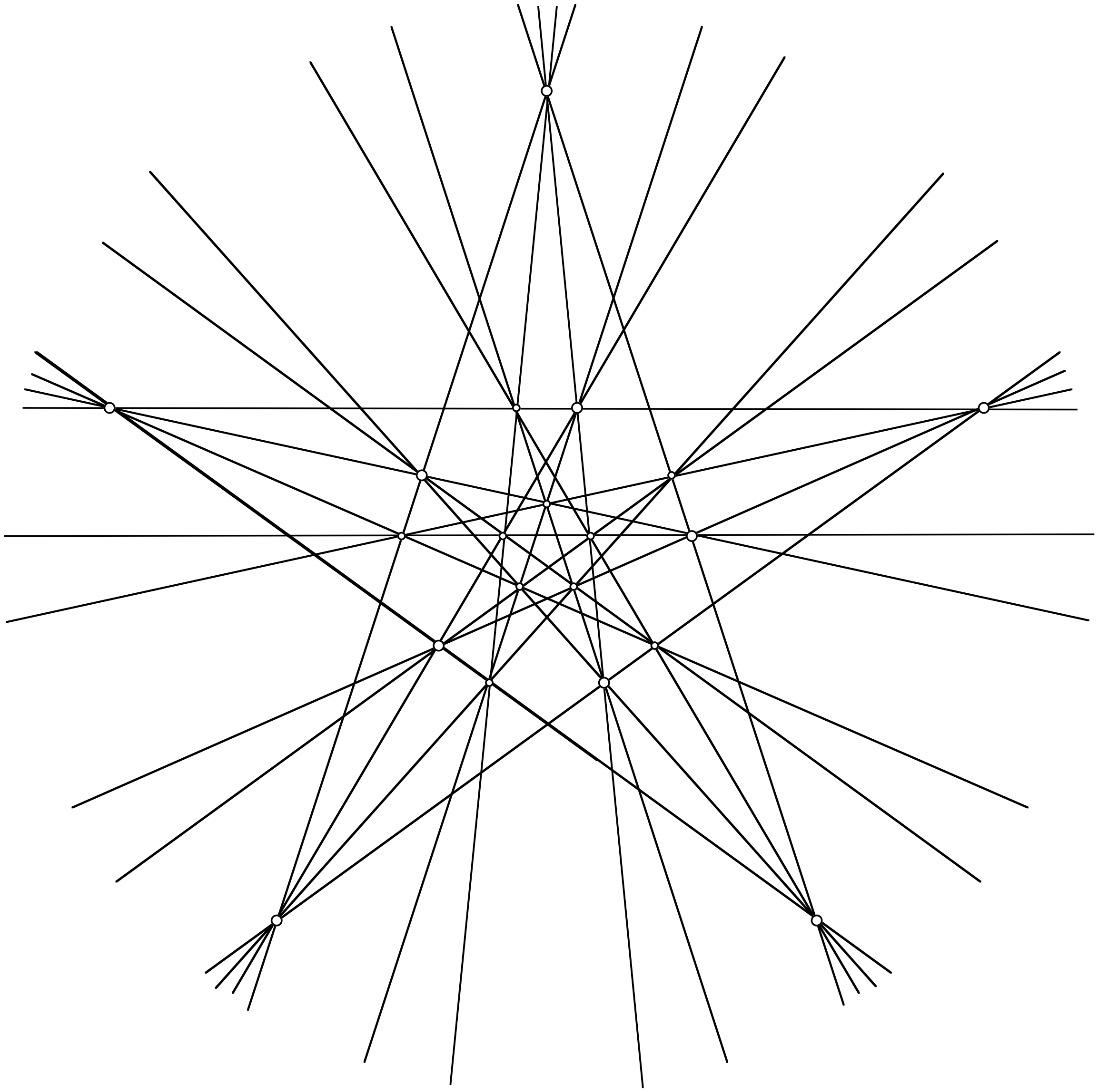


Figure 4. A recently found configuration (20_4) (from [14]).

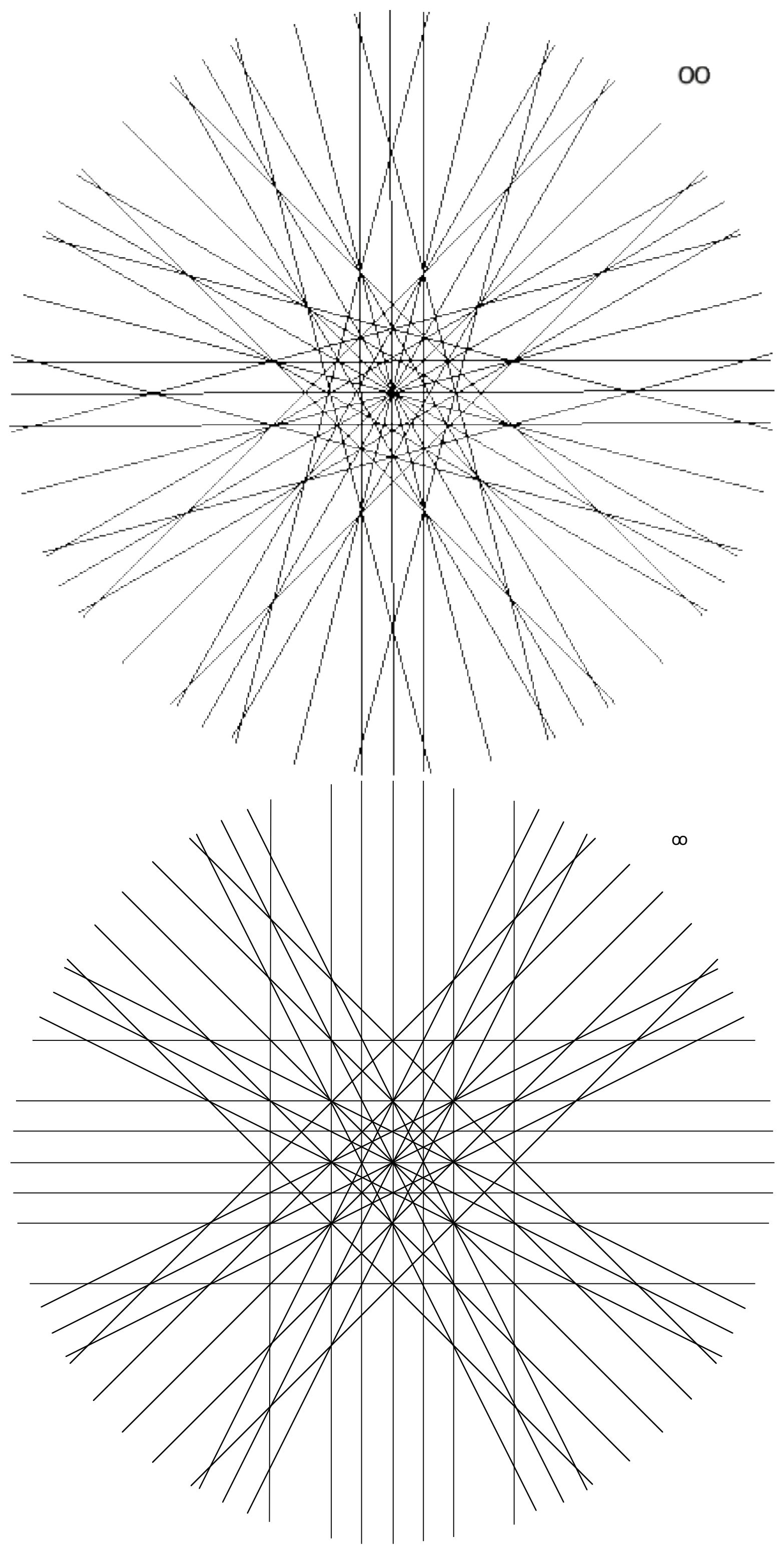
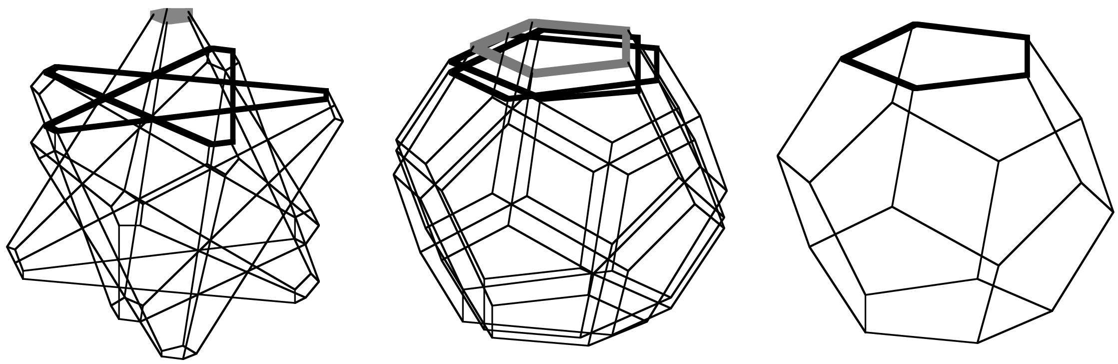


Figure 5. Two simplicial arrangements with 37 lines (including the line at infinity); from [9]. These are the largest known simplicial arrangements that do not belong to one of the three infinite families. ("Simplicial" means that all faces are triangles.)



(a) (b) (c)

Figure 6. An example of the new approach to polyhedra: the truncation of the regular polyhedron {5/2,5}, Kepler's small stellated dodecahedron. (a) shows an early stage of the truncation; one of the pentagonal faces, and one of the decagonal faces are emphasized. (b) shows an almost complete truncation, illustrating the proximity of the emphasized pentagon and decagon. (c) is the complete truncation, in which each face of the "dodecahedron" represents one pentagon {5} and one decagon {10/2}. Each point that is a dodecahedral vertex represents three distinct vertices of the uniform polyhedron (5 . 10/2 . 10/2), the complete truncation of {5/2,5}. From [12].