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Kleitman and Combinatorics: A Celebration

G.W. Peck[1]

Abstract

A discussion of the history, the mathematics, and the charm of Daniel J. Kleitman.

Our venerable friend here named Danny
Was "only in physics". A plan he
    Devised: sent his proofs
    Uncle Paul said, "No goofs!
His knack for the math is uncanny."

So he left his position at Brandei(s)
And came to teach courses at M.I.(T.).
    He conquered the region;
    His students are legion.
He characterized posets: the LY(M).

Though some know him only as Kleitman,
They always are sure to invite him.
    His lecturing style
    Entertains all the while.
We hope that this meeting delights him.

—Douglas B. West

So began a tribute to Daniel J. Kleitman at the banquet of the meeting in honor of his 65th birthday, held at M.I.T. from August 16–18, 1999. More than a hundred colleagues, former students, friends, and interested researchers from mathematics, computer science, and operations research gathered to celebrate his work and share stories of his exploits.

[1] G.W. Peck is on leave from his usual residence.

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1. The early days

Kleitman was born October 4, 1934, in Brooklyn, New York. The family moved to New Jersey in 1942, and he graduated from Morristown High School in 1950. He attended Cornell University and graduated in 1954. He married Sharon Ruth Alexander on July 26, 1964. They have three children: Jesse and Tobias Kleitman and Lea Claudia Le Mieux, and a grandson, Elijah Le Mieux.

Kleitman majored in physics but enjoyed mathematics much more and wound up taking much more mathematics than needed for a degree in it. Nevertheless, he continued in physics and became an assistant professor of physics at Brandeis. The story of his transition to mathematics is part of the lore of combinatorics. Danny has graciously provided us with recollections of his journey into mathematics from his early days at Cornell to the famous question from Paul Erdős: "Why are you only a physicist?"

> In those days students learned little or no calculus in high school, and something less than what we teach as first year calculus was taught as a three term sequence. There were two versions of this sequence; one started with geometry and the other with calculus. I took the latter, while my roommate at the time took the former. He had a lot of trouble with it, and at one stage we formulated a plan that I should take an exam for him. We were never able to summon the lack of scruples necessary to go through with this plan, but in anticipation I spent a weekend trying to learn the content of his course. I actually took the exam, and did quite well on it. I exempted the final in my course and decided to take the final in the second term course instead, and gave another weekend to study for it. I was surprised to discover that I did well and got credit for that course.
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> I recall being impressed as a freshman at how provincial Morristown was compared to the level of people I met out in the real world at Cornell. Steve Weinberg was my roommate part of my first year, and Sheldon Glashow was an apartment-mate later on. It was only after they won Nobel Prizes that I realized that Morristown was a bit more like the real world than they were.
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> The physics course I took as a freshman was quite remarkable. In those days at Cornell a physics student took a year long general physics course as a freshman, then another that went over the same material again as a sophomore, then individual courses in mechanics, electricity and magnetism, atomic and nuclear physics, etc. as upperclassmen, and then repeated once again in graduate school.
>
> At any rate, the freshman course covered in its entirety a text that claimed to be used for a three term course for sophomores at Yale. In the third week we were asked to compute moments of inertia, which required us to learn to do volume integrals on our own, no calculus course having reached integration at all. The class started with about 70 students and soon dwindled to under 20, most of whom became physics majors.
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> I got an undeserved good reputation in my section because in the second week we were asked to solve a problem about the best angle to place a sail in order to move against the wind. I had no idea what it was even about but asked my older

brother, who explained it and came up with a clever solution that made use only of trigonometry. When I went to class with it, the instructor announced that this problem could not be done without results we had not yet studied, and so he was very impressed with my solution.

My most vivid memory is of the final examination for the first term of this course. I recall staying up all night, almost, to study for it; it began at 9 AM. At 6 AM, being more or less satisfied with my knowledge of the regular course material, I decided to read over my lab reports as a final review. I decided to do this in the prone position, which was a mistake. My next memory is a knock at my door from the maid (we had maid service in our dorms in those days) and the information that there was a phone call for me. It seems I had been missed at the exam, and someone had taken the trouble of calling to find out what had happened to me. I made what I believed to be a new world record in getting up the hill to the exam room, and managed to make it for the last half of it. Amazingly enough I did reasonably well. I offer my belated thanks to whoever it was who made that call; I was too flustered to remember who it was even for a minute.

As a sophomore I took a course with Marc Kac in Whittaker and Watson type mathematics. The tripos type problems were very challenging. Of course they were so challenging that the whole (quite large) course split into informal groups who cooperated (against the rules) in solving them. Working out problems from Whittaker and Watson is an excellent way to exercise one's mind, and I heartily recommend it to students, with or without help from your friends.

As a junior, I took a course, which actually was two courses, given by Pierre Samuel, who visited Cornell from Paris that year. They were the first-year graduate courses in Algebra and Analysis. He gave beautiful perfectly organized lectures in which mathematics was developed from scratch in a very elegant though formal way, along the lines of Bourbaki, which I think he was connected with somehow. This was very esthetic, though I cannot say that I had any idea of motivations for it, or how it fitted into the mundane world.

At any rate, he gave very challenging problems, which required reasoning using techniques of the style used in the edifice he was constructing, in other contexts. I remember thinking about one of these while walking along one day, and suddenly getting an insight which allowed me to solve it. It was an exciting experience, and was my first real experience in "seeing" with my internal eye mathematical ideas which were neither recollections of what I had learned or straightforward deductions from them. I am very grateful to Samuel for challenging us to do such things.

As a junior I also took a course in electricity and magnetism, from Dale Corson, who later became head of the physics department and later president of the university. He had heard of the rather ill-behaved bunch of students, Weinberg, Glashow, and (I think) David Lubell and myself, among others, who had signed up for his course. We were apparently known for our poor attendance records, our inattention and reading of newspapers in class when we did appear, and our annoying ability to do well in our courses despite all this.

He offered us an alternative; we could attend his course regularly and behave, or never attend but instead do a final problem set and a final exam instead. It was impossible for me to accept attending his course in the regular way under these circumstances. Some of our group faintheartedly gave in and attended, but the four of us took the second alternative. I never did find out what this course was supposed to be about. I was not surprised though that Corson managed to disgrace Cornell by some of his decisions as its president.

Professor Salpeter taught the graduate course in electricity and magnetism, which was another example of a formal presentation of meticulously prepared notes, perfectly organized down to the equation numbering system, leading to elegant notes and a minimum of understanding on my part. Though I studied many things as a senior, my time was largely taken up with distractions: bridge, the opposite sex, poker, and ping pong, so I learned next to nothing.

I attended graduate school in physics at Harvard and received an MA (awarded to anyone who asked for it) after one year, and a PhD in 1958. Having supposedly taken the first year graduate curriculum as a senior I started with the second year courses. Ivar Stakgold taught an excellent course in applied mathematics, in which Harvey Greenspan was the teaching assistant.

But it was Julian Schwinger's course in advanced quantum mechanics and subsequently in field theory that were real highlights. Schwinger would arrive a minute or so late to class, having driven up in his flashy sports car. He would deliver mesmerizing lectures, perfectly organized, and beautifully written on the blackboard, ending at the door at the end of the period, and he would duck out immediately, usually chased by his throng of graduate students. He started with the algebra of measurement and derived all of physics from an action principle (which he wrote in rather condensed form as $dA = d(A)$).

Hearing these lectures was a wonderful esthetic experience, akin to listening to chamber music. The edifice he constructed was a marvel, though most of us had no idea of how it related to the rest of reality until much later in our lives, if ever. His treatment of the rotation group and its representatives was particularly beautiful. The only jarring note in the lectures came from a fellow student who had some unfortunate stomach ailment which caused him to burp every 90 seconds or so during the lecture. He was and is a very nice person and one could not hold it against him, but it was a distraction.

Unfortunately, my formal education did not lead me to develop a good sense of what the significant open problems of theoretical physics were, or of how to find them. Colloquia by theoreticians were usually quite interesting, conveying their claims to have created explanations for various phenomena often recently observed. Experimentalists on the other hand tended to give extremely boring lectures, concentrating on details of the building of their apparatus, and giving results only as minor add-ons long after their audience was asleep.

Eventually I realized that to find relevant useful research problems one was best advised to cultivate experimentalists, and learn what phenomena they were finding that might require new ideas to explain. Alas, I had no talent and little interest in doing this. I wasted a year or so doing essentially nothing. Finding this awful, I wrote two small theses (one under the direction of Roy Glauber in implications of the impulse approximation, and one under the direction of Schwinger on implications of his current model—since abandoned) and graduated.

After receiving my degree I spent two years as an NSF postdoctoral fellow, the first year at the Universitetets Institut for Teoretisk Fysik in Copenhagen and the second at Harvard. I made many friends during this period, including Torlief Ericsson, Lovro Picman, Vladimir Wataghin, Jeffrey Goldstone, Vaclav Czycz, Steve Berko, Nick Burgoyne, Richard Prange, Frank Tangherlini, Gunnar Kallen, John Toll, and Howard Volkin, among others. Saul Barshay, a classmate from Cornell, and Sheldon Glashow, who was my classmate at both Cornell and Harvard, were also at Copenhagen in 1959. I did not find any research area that really turned me on or in which I was able to make a significant contribution. I did desultory minor things and tried various areas of research, with little to show for it.

I also had many weird and curious adventures in traveling about Europe, whose description would take up far too much space to be inserted here.

At the end of this time, without making any application, I received two job offers, one an instructorship at Carnegie Institute (now Carnegie Mellon University) and the other an assistant professorship at Brandeis University. In those days you did not apply for certain levels of academic jobs in this country. Your advisor advertised your availability to those who might be interested and solicited an offer on your behalf.

I accepted the offer from Brandeis and taught there for the next six years, (apart from a leave taken at Harvard). During my second or third year there I met a friend from Cornell, David Lubell, who was a Benjamin Pierce Instructor at Harvard, who invited me to his home for dinner with his new wife. Just before leaving his home he told me about a result he had obtained (the LYM inequality) and a problem he hoped it would help solve.

The problem was to find a good upper bound on the size of a collection of subsets of an n element set that has no three members such that one is the union of the other two. I thought about it a bit when I got home, and suddenly I saw a simple solution to the problem, off from the true answer by at most a constant factor.

Dave informed me that he had found the question in a book of mathematical problems gathered by Stan Ulam. He lent me the book, and I pored over it, finding two others that I could solve. One was a sort of bridge problem, but the other was in the same chapter of the book as the first and had been suggested by Littlewood and Offord some years before.

These two results, thin as they were, seemed to me to be far more satisfying than the work I had done in physics, and far more elegant.

I wrote to Ulam after this to find out where these problems had come from and what he thought of the results. He informed me that he had received other solutions to the bridge problem, but that the other results were new to him. He wrote that he had gotten the problems in that chapter from his friend Paul Erdős and suggested that I contact him. He also, however, gave no clue as to how this could be done, since Erdős, he said, had no fixed address, rather wandering from place to place more or less at random.

The problem of locating Erdős baffled me, and so, after perhaps a year, I decided to write the results up for publication. I did so and submitted them to a mathematical journal edited by Paul Halmos. In only a short time I received my manuscripts back with a summary rejection and a damning referees report.

To make it self-contained, I had begun the first paper with a very short proof of Sperner's theorem: the largest size of a collection of subsets of an n-element set with no member containing another is the binomial coefficient $\binom{n}{n/2}$. The referee claimed that this result was obviously incorrect so that he did not bother to read further or examine the other paper. And furthermore, in the odd n case I had neglected the greatest integer notation on my $n/2$.

I found this so stupid a response that I was dumbfounded. But I learned the lesson that I had written the papers up in a manner that a physicist would write, and that this was apparently not acceptable to mathematicians, though I could not see why.

After leaving the manuscripts in a drawer for a few months, I decided to bring them to a mathematician at Brandeis, to ask him for help in rendering them in mathematically acceptable form. I asked Edgar Brown for this favor. He gave me the assistance needed and suggested appropriate journals for the papers. So I submitted them again and hoped for the best.

Several months later I was surprised to receive a blue air-letter from Hungary, from Paul Erdős. He informed me that he had been sent the papers to referee and loved them. There is normally room on one of these air-letters for two paragraphs. In the first he made his comments on the papers, and the second paragraph consisted of another mathematical problem.

Naturally this letter pleased me. That afternoon, I was expected to attend a meeting of the physics department faculty to discuss some matter or other. Attending this meeting gave me some uninterruptible time that I devoted to thinking about the problem in the air-letter, and to my amazement I was able to see a solution to it.

I wrote up a solution and sent it to Paul, only to find that it and another air-letter from him containing another problem crossed in the mail. This time I worked furiously at it and was able to solve it as well. This strange correspondence continued again and still again; twice more my solution and another air-letter with still another problem crossed in the mail.

At some stage the personal part of Paul's letter contained the question "Why are you only a physicist? You should be a mathematician." I informed him that I would be delighted to be one but that I was alas, but a physicist by education and position, and not a very good one at that.

Paul then wrote to a number of his friends about me, and amazingly enough, just when I needed it, I received two job offers: one from City University in New York, and one from M.I.T., both as Associate Professors of Mathematics. I accepted the position at M.I.T. beginning in the Fall of 1966, and I have been here ever since.

2. Kleitman's work

Kleitman has proved fundamental results in many areas of discrete mathematics, including extremal hypergraph theory, asymptotic enumeration, graph theory, and discrete geometry. His work is characterized by keen combinatorial insight and intuition, coupled with mastery of mathematical tools such as linear programming, counting arguments, and probability.

Including the papers of G.W. Peck (more on that later), Kleitman has authored or coauthored more than 200 papers so far. He has more than 130 coauthors, most frequent among them being Noga Alon (13 papers) and Zoltán Füredi (10 papers). He has eight joint papers with each of Curtis Greene and Jerrold Griggs, and seven with Paul Erdős. Others with whom he has at least five joint papers are Susan Assman, Wayne Goddard, Ron Holzmann, Bruce Rothschild, Michael Saks, James Shearer, Douglas West, and Ken Winston. He has had 30 PhD students and altogether has more than 110 academic descendants already. His coauthors and descendants that we know of so far are listed in appendices to this paper. He also served as the Managing Editor of the *SIAM Journal of Algebraic and Discrete Methods* at its founding (it later split into two SIAM journals).

The early portion of Kleitman's work, through about 1980, is described in the survey by Michael Saks following this paper. During this period Kleitman made great advances in extremal set theory (finding the largest or smallest family of sets satisfying special conditions) and asymptotic enumeration (studying for example the asymptotics of the number of posets with n elements, the number of antichains of subsets of an n-element set, etc.), and with Curtis Greene he proved the celebrated Greene–Kleitman Theorem generalizing Dilworth's Theorem to a min-max relation for unions of k antichains in a poset. We leave further discussion of these results to the survey by Saks.

To gain an impression of his more recent work, we asked Kleitman to provide us with a list of his publications. We were somewhat perplexed to discover that, unlike most mathematicians, he had no publication list. The explanation is vintage Kleitman:

> Years ago I had an excellent secretary who kept all my records of papers and everything else in his files. Unfortunately for me he retired, and I have never been able to find the files. I then had another excellent secretary, and she kept records of my letters and papers on her computer. When she left, my access to these disappeared as well. I kept some records myself on my trusty Commodore computer. Unfortunately I no longer remember how to access them, and I doubt if the disk drives still work. Other records are on similarly inaccessible obsolete machines.
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> Fortunately, one of the joys of old age is that you do not need to keep track of what you have done in detail if you are fortunate enough not to have to beg for money any more.
>
> Thus I have to confess I have no records of what I have written, no cv, and no lists of invited talks and honors to give you. Not even a list of papers.

Nevertheless, Kleitman described for us a few of his recent interests and mathematical accomplishments of the past 20 years. He has been interested in geometric combinatorics, extremal graph theory, applications of mathematics, and mathematical education. We describe a few of his results in order to indicate the range and flavor of his interests outside the earlier areas of extremal set theory and asymptotic enumeration. The first three problems in discrete geometry mentioned below are described more fully in Section 4 of Saks' survey. We state them here because they come from the more recent period of Kleitman's work, because he is fond of them, and because the results settled important and long-standing open problems.

The Prison Yard Problem asks the following: given an n-sided polygonal prison, guards must be placed on the vertices so as to have unimpeded views of everything inside and outside of the polygon. A convex prison requires $\lceil n/2 \rceil$ guards; how many suffice in general? The problem was posed independently by D. Wood and J. Malkevitch, and J. O'Rourke conjectured that $\lfloor n/2 \rfloor$ guards suffice for non-convex polygons. Füredi and Kleitman [21] proved the conjecture, by what Kleitman calls "a rather complicated argument". In reviewing the paper, Źivaljević commented, "This problem has occupied one of the central places among the art gallery type problems."

Consider the graph-theoretic diameter of a convex d-dimensional polytope defined by n linear inequalities. A famous conjecture of Hirsh in 1957 is that the maximum diameter of such a polytope is at most $n-d$ when $n \geqslant d$. (The bound fails for unbounded polyhedra; Klee and Kleinschmidt [29] constructed such examples with diameter at least $n-d+\lceil d/5 \rceil$.) The problem is relevant to linear programming; the diameter of the polytope of feasible solutions is a lower bound on the worst-case number of iterations of the simplex algorithm. Kalai and Kleitman [27] showed by a nonconstructive recursive argument that the diameter is always at most $n^{\log d+2}$. With typical modesty, Kleitman writes, "Gil Kalai had some interesting ideas on this problem, and somewhat by accident I was able to supply one more idea which improved his argument for a new bound on this very old problem." Their paper is only two pages long and remains the best known upper bound.

The Helly Theorem says that if every set of $d+1$ members of a family of convex set in d dimensions have nonempty intersection, then there is a point common to all of them. A family of sets has the (p,q)-*property* if among every p members, some q have nonempty intersection. The Hadwiger–Debrunner Problem, posed around the same time as the Hirsh Conjecture, asks whether there exists an integer c, depending only on p, q, and d, such that every family of convex sets in $\mathbb{R}^d$ having the (p,q)-property can be "pierced" by c points, meaning that each member of the family intersects that set of c points. Alon and Kleitman [3,4] proved that the answer is "yes" using a fractional version of Helly's Theorem, linear programming duality, and a result on weak ε-nets of convex sets. (The second paper gives a simpler, purely combinatorial proof, but its bound on $c(p,q,d)$ is looser). The smallest such c has not been found. For the case $(p,q,d)=(4,3,2)$, Kleitman, Gyárfás, and Tóth [36] showed that 13 points are always enough to meet all the sets. Kleitman writes, "It seems that three or four is the correct answer here, but 13, alas, was the best we could do."

The Six Lonely Runners Problem can be phrased geometrically or number-theoretically. Six runners start at the same point and run around a circular track at constant but distinct speeds. The slowest runner becomes lonely if he is unable to see another runner within 1/6th of the track length in either direction. Are there speeds such that he never becomes lonely? Bohman, Holzman, and Kleitman [10] proved that the answer is no.

Given n points in general position in the plane, how many of the $\binom{n}{2}$ open segments determined by the points can be chosen so that they pairwise intersect? Kleitman and six coauthors [8] showed that at least $\sqrt{n/12}$ can be chosen.

Consider a rectangle partitioned into subrectangles so that every line parallel to a side intersects the interiors of at most n subrectangles. Erdős asked how many subrectangles could be used in such a decomposition; let $f(n)$ be this value. After splitting the rectangle into a 2-by-2 grid of rectangles, we can decompose two rectangles in opposite corners into $f(n-1)$ subrectangles each to obtain $f(n) \geqslant 2f(n-1)+2$ and hence $f(n) \geqslant 3 \cdot 2^n - 2$. Kleitman [33] proved that $f(n) \leqslant c \cdot 2.2413^n$ for some constant c. When the cutting lines can lie in any direction, the construction above is no longer available, but he still gave $3^{n/6}$ as an exponential lower bound.

Kleitman has also solved many hard problems in extremal graph theory. Classical extremal graph theory asks for the maximum size of an n-vertex graph or digraph that avoids a particular configuration. One such result that Kleitman describes as "one of the hardest results I remember" concerns the number of edges in an n-vertex strong digraph that are needed to force a cycle of even length to appear. Chung, Goddard, and Kleitman [13] proved that $\lfloor (n+1)^2/4 \rfloor$ edges are necessary and sufficient.

A problem with a similar flavor, posed independently by Gallai and by Zelinka, asks for the maximum number of triangles in an n-vertex graph where all the subgraphs induced by vertex neighborhoods are forests. An optimum of $n^2/8$ was conjectured, achieved by adding a matching to one partite set in $K_{n/2,n/2}$. Zhou [45] showed that this can be exceeded slightly when $n \equiv 7 \bmod 8$. Füredi, Goemans, and Kleitman [17] improved the lower bound to $n^2/7.4 + n/15$ when $15 \mid n$. They improved Zhou's upper bound to $n^2/7.02 + 5n$ for all graphs. They also proved that the desired bound of $n^2/8$ holds for all graphs with $n \geqslant 100$ and minimum degree more than $(2n+16)/5$, and they conjectured that it holds for all regular graphs.

Some of these results have beautifully easy proofs that display cleverness rather than tenacity. For example, Graham and Kleitman [23] proved that in every linear ordering on the edges of K_n there is an increasing trail of length at least $n-1$. They grow the graph by adding the edges *in order*, with a weight at each vertex recording the length of the longest trail so far that ends there. Each added edge increases the total weight of its endpoints by at least 2, so the final total is at least $n(n-1)$, and the pigeonhole principle completes the proof.

Another very short argument concerns maximal triangle-free graphs (adding any edge creates a triangle, so these are the triangle-free graphs of diameter 2). Goddard and Kleitman [22] proved that every maximal triangle-free graph with minimum degree δ has an independent set of $3\delta - n$ vertices with identical neighborhoods. This is sharp for $K_{m,m}$, the expansion of a 5-cycle by independent sets of the same size, and the complement of the $r-1$st power of C_{3r-1}. They actually prove the existence of a vertex v such that at least $\delta + 2d(v) - n$ vertices have the same neighborhood as v, by studying a nonadjacent pair with distinct neighborhoods having greatest overlap of neighborhoods. The result yields a much simpler proof of Woodall's conjecture that binding number at least $\frac{3}{2}$ guarantees the existence of a triangle (and cycles of all lengths), which was first proved by Shi [44]. The binding number of a graph G is the minimum of the ratio $N(S)/|S|$ over all S with $\emptyset \neq S \subseteq V(G)$ such that $N(S) \neq V(G)$.

Trees in various forms have appeared often in Kleitmans' work. He has studied optimal spanning trees of various types [14,25,42], existence and enumeration of trees with special properties [12,20,30,35,41], optimal binary search trees [26,39], and other aspects. Among these, the paper by Chaiken and Kleitman [12] deserves special mention. Their Matrix Aborescence Theorem is a vast generating function generalization of Kirkhoff's well-known Matrix Tree Theorem and Tutte's Directed Matrix Tree Theorem. The proof is entirely combinatorial and inductive, using properties of polynomials. They prove that a particular generating function for the arborescences with roots at a specified vertex set S is equal to the determinant of the matrix obtained by deleting the rows and columns for S from a particular weighted form of the adjacency matrix.

Kleitman also attacks purely combinatorial problems. Consider card shuffling: How many times must a deck of n cards be shuffled to make it possible to deal out every possible deal of j hands of n/j cards each? Alon, Berman, and Kleitman [2] established bounds for the cases $j=n$ and $j=2$.

Given nonnegative integers $a_1,\ldots,a_n$, does there exist a matching in the n-dimensional hypercube that has for each i exactly a_i edges whose endpoints differ in coordinate i? Felzenbaum, Holzman, and Kleitman [16] characterized when the answer is yes. Obviously $\sum a_i \leqslant 2^{n-1}$ is necessary. For $n \geqslant 2$ this is also sufficient, except that when equality holds also all a_i must be even.

Kleitman also continues to work in extremal set theory. Good examples of his later work in this area include [1,5,6,11,15,18,19,24,32,40]. He has also delved into applications, including computer science [37,38] and computational biology [9].

In recent years, Kleitman has turned his interest to the use of computers in mathematical education. The best way to describe his philosophy on this is to let him do it himself:

> In education I have long believed that we should make use of computers in teaching in ways that I have not seen done. I have had the good fortune to have Jean-Michel Claus help me attempt to develop materials to help students visualize mathematical concepts and to empower them to use them. This seems to require developing of new curricula as well, for the following peculiar reason.
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> One would think, naively, that labor saving devices simplify life and make it possible to get through life with less and less effort and activity. The evidence of our senses is just the opposite. Labor saving devices allow us to avoid some drudgery, but we adjust to this by trying to accomplish more. Life gets busier and more complicated for most of us the more use we have of them. And so it should be with education. If we can indeed make it easier for students to comprehend and learn to use abstract concepts, we should not stop there. Otherwise students will learn the same amount in less time and devote more of their lives to other things. The net result will be that they learn less mathematics than before. Instead we must use educational tools to make students get more (deeper understanding, better able to use concepts, less drudgery, more interesting and exciting content) from the same amount of or even more effort.

Another peculiar problem is the timelessness of material available on a computer. Naively you might think that having a canned lecture by a great lecturer is better than having a live lecture by a novice, and having a demonstration is far better than watching a talking head and a pair of hands write slowly on the blackboard, and empowering students to actually do themselves what the lecturer talks about is still better. However, this ignores the true value of a lecture. It lies in its evanescence: if you don't attend it you miss it and it is gone forever. And the grotesquely slow pace evidenced if you ever watch a taped lecture serves a purpose: the listener can think about things without losing track of the lecture, up to a point. Students unconsciously employ the maxim (or perhaps minim): Never do today what you can put off until tomorrow! Fast paced material on line leaves little time for thought and, being always available, need never be looked at! To give it value it is necessary to create new kinds of exercises and requirements that require students to use them, play with them, and think about them.

At any rate, we are about halfway up the path to creating what is needed for a calculus course (we have created materials and tested them on students, more or less, and are trying to improve them and develop appropriate exercises, and we are thinking about other courses as well. Any suggestions from you will be appreciated.

What we have so far is available and visible on the web at (math.mit.edu/djk) if you click on 18.013A. If you ever look at this stuff we would greatly appreciate any comments (send them to (djk@math.mit.edu))

3. Kleitman the man

No tribute to Danny Kleitman is complete without some of the many anecdotes about his mathematical habits, his playful and kind nature, his encouragement of students, and the state of his office. He seems to want everyone to enjoy mathematics and life as much as he does.

Debbie Franzblau speculates that Danny's approach to mathematics (or at least puzzles) is inherited. "I was once in his office when Danny's mother was visiting. She was playing with one of the puzzles on his desk. After a while, she turned to me and said, 'first you play around—until you get stuck—*then* you think'."

Kleitman's brother David tells us when he realized that his brother would probably become a mathematician.

> When Danny was 3 years old, we were playing in a sandbox. The parent of another child in the park asked us how old we were. I said I was 6, and my little brother was 3. "You are twice as old as he is," the parent replied. Danny apparently thought about that, because he said, "You know, after my birthday next month, that won't be true any more. It will never be true again."

When David was dating his future wife, Danny helped save her sister's academic life by intensively tutoring her in trigonometry—she was self-avowedly hopeless, but

Danny's efforts allowed her to barely pass the course and stay in Douglass College. His niece Naomi Kleitman describes him as a fabulously fun uncle who was incredibly interesting and well read:

> When we were kids, he entertained us with card and magic tricks and sent postcards from (then) Tanganika. When I was in high school, I would call him cross-country to discuss ideas for history term papers (who else had ever heard of The Mountain Meadows Massacre or could supply 1903 newspapers to illustrate a paper on the Pure Food and Drug Act?).
>
> At a cousin's Bar Mitzvah, Danny decided to illustrate for us what "burning the candle at both ends" meant. He proceeded to dismantle one of the centerpieces and demonstrate; my mother almost killed him! At Danny's wedding to Sharon, the most popular pastime was trying to pop a champagne cork from the backyard over the house to the front; something about physics...

Danny's communication style in mathematics is somewhat unusual. He discusses mathematics on an intuitive level, laying out the broad outlines of a proof and leaving the details to be added later. When pressed for details, Danny will begin to develop them on the spot, as if he had them in his mind all along. Indeed, he thinks and talks at the same time, holding the attention of the listener while he thinks of the next idea.

When discussing a problem with a colleague or student, he will draw a picture on a page or blackboard, and while discussing the idea he will repeatedly draw the same picture, with appropriate variations, in approximately the same place, until the whole mass becomes undecipherable. At the Third International Conference in Combinatorics in New York in 1985, he went directly to the end of this process, beginning his lecture [32] by putting all his slides on the projector at once.

Danny has a unique lecturing style. He lectures to an audience as if he is talking one-on-one with each individual as he would in his office. He cajoles the audience, persuading listeners that they see the problem as he does.

Tom Trotter describes a memorable lecture:

> Danny was invited to give a colloquium talk at the University of South Carolina and then learned that he had a conflict with a charity event. Not wanting to disappoint his youthful colleagues at SC, he hosted the charity event and then hopped a red-eye flight to Columbia, arriving for breakfast and then a 9:00 am presentation.
>
> Despite the travel ordeal, Danny was his usual exuberant self during his talk, and the audience warmed to the occasion. At a key point, Danny wanted to stress the significance of a number being projected on the screen, so he took the indelible marker and circled it boldly—on the screen and not on his transparencies! Clearly, the dramatic gesture served its purpose well.
>
> For many years afterward, South Carolina faculty referred to the screen with its very permanent bright red oval as the DJK Memorial Projector Screen.

One is never quite sure what to expect from a lecture by Kleitman. Jeff Kahn tells a story from when he was running the MIT combinatorics seminar:

> One time, sitting at the back of the room with the Kleitman in question, I asked him if he'd give the next talk. He declined. So when, as was customary before the day's talk, I announced the upcoming schedule, I said we had nobody for the following week because Danny refused to speak. This apparently humiliated him to such an extent that at some point during the hour he leaned over and said, "Okay, I'll talk." Do you have a title? "No, I have to prove something first." So do you want me to wait to announce it? "No, announce it and I'll HAVE to prove something." So I did.
>
> As it happened, I was living on one of the upper Kleitman floors at the time, so got to talk math with Danny in the basement on evenings when he had nothing better to do. We'd had some idea that seemed likely to prove a conjecture of Sergei Yuzvinsky, and it turned out that this was what he'd had in mind for a talk. But, sadly, the idea quickly met the fate of some of our others.
>
> So, Danny set to work to rectify this situation, mostly around the 1:00 AMs as far as I could tell, but seemingly without much success. At breakfast on the morning of talk day he told me he'd found a great proof the night before, which unfortunately had also proved some even better false theorems; that for a little while he'd thought perhaps he'd found a contradiction in mathematics, but that on reflection this seemed an unlikely place for one to show up; and that his plan for the talk was to describe the problem, give this nice false proof, and see if anyone could see what was wrong with it. So, off to school.
>
> During the day I saw Danny off and on, always seemingly deep in thought. I think the seminar was for 4:30. I was teaching at 3:00, and for most of that hour I could see Danny pacing the hall outside the classroom. When I emerged he came running up with "I've got it, I've got it," or words to that effect, and quickly sketched a proof which did, in fact, seem to make sense. So, off to seminar.
>
> The talk went well for a while, but the speaker may have been a little tired at this point, and as things progressed the circles on the board grew ever larger and the argumentation more emphatic, ending, in my recollection, with both board and audience fairly beaten into submission, but neither, perhaps, entirely persuaded. And that was the last I heard of it for a long time.
>
> It was several years later that either Noga or Gil (Alon or Kalai), both of whom had been in the audience that day, reminded me of the talk and asked if Danny had really solved the problem. To which I could only say that the proof he'd sketched to me had sounded plausible, but that I'd never thought or heard about it since. But by amazing coincidence, it was just a day or two after this that I found, on the new journals shelf at Rutgers, Danny's [31], containing, indeed, a proof recognizable as the one he had told me in the hallway.

Danny's listening style is as famous as his lecturing style. He is well known for napping in a lecture and then asking perceptive questions at the end. He has said, "My wife always accuses me of sleeping while I'm working, because to the outside world they're indistinguishable." Tom Trotter recounts a story that makes this point even clearer:

> Once a mathematician visiting MIT was giving a talk to DJK's combinatorics seminar. The speaker is well known for giving energetic (some might say frenetic) presentations, but this didn't stop Danny from taking his usual seat at the back of the small seminar room and promptly going to sleep.
>
> About two-thirds of the way through the talk, the speaker—in an effort to drive home a key point—asked a question which could readily be answered IF you had been following the presentation closely. Of course, DJK being quite asleep had not. But most of the audience were graduate students and were reluctant to voice an answer—perhaps being just a tad overwhelmed by the speaker's manner.
>
> Frustrated that no one volunteered an answer, the speaker then raised the stakes by stating emphatically that he would pay 25 cents for the first right answer. At that point, DJK awoke from his slumber just enough to blurt out the answer (only God knows where he got it from). The speaker reached into his pocket and tossed him the promised reward. DJK is well known for being exceptionally clever, but as this story proves, he is pretty darned good even when he's asleep!

Mike Albertson recalls a lecture that he gave at MIT, at which Danny showed up 40 minutes late. He listened for a couple of minutes and then said, "What about this? ..." His insight and intuition was so good that he had immediately identified the crucial difficulty, which Mike was trying to avoid discussing the details of.

On the other hand, a comment in a lecture can send Danny into furious mathematical activity. Peter Winkler explains:

> At a meeting on ordered sets in Oberwolfach in 1984, Danny sat in the front row of my audience for a talk on random orders. During the talk, while illustrating the power of the calculus for proving theorems in this model, I displayed a complex expression which I claimed was "weakly non-integrable".
>
> As I'd hoped, someone asked what that meant; I said "it means I can't integrate it."
>
> Amidst some mild laughter, I saw Danny bum a piece of paper off his neighbor and begin scribbling. He scribbled through the rest of my talk and the next two, and at the break he handed the paper to me. "I've integrated your weakly non-integrable function for you," he said.
>
> I examined the sheet; it was covered with writing, some upside down or sideways, on both sides. Everywhere were sums, products, gamma functions, Bessel Functions... I searched in vain for something boxed or circled. "Danny," I said, "Where's the answer?"
>
> "What do you mean?" he replied, gesturing to indicate the entire sheet. "*That's* the answer!"

Danny's sense of humor and unique perspective lend spice to his conversation. He has been quoted in at least two books for articulating the most basic result in Ramsey theory: “Among any three ordinary people, at least two have the same sex.”

When Debbie Franzblau visited MIT to decide whether to attend as a grad student, she had lunch with the combinatorics group. She asked whether there were oral exams, and Danny said “they’re actually rather anal ...”. Danny told a student he had enough material for a thesis by saying, “Well, I think you’ve been here long enough.” Asked whether a student should accept a particular job offer, Danny shrugged and said, his voice trailing off into space, “Well, there are worse places.” Asked by a dean at another university whether a world-renowned combinatorialist would be a good candidate, Danny replied, “Well, that depends... Do you want to be on the map?”

Doug West offers a few more such examples:

> When confronted with a disaster out of his control (such as when his car died in the Callahan Tunnel as I was driving him to the airport), Danny’s response would be “This is too horrible to contemplate”.
>
> When I wanted to further improve some results we had, Danny said, “You should never solve a problem completely. If you solve it completely, then there won’t be anything left for anyone else to do, and so no one will want to read the paper.”
>
> When Don Knuth called to tell Danny about a job opening at Stanford, Danny listened and replied, “Well, for the job you are describing, there are four people who would do: Gessel, Griggs, Odlyzko, and West. The thing is, the first three already have jobs.” A bit later, I returned from a vacation and found myself in the TA office when Danny called looking for Mike Saks. When he recognized my voice, he said, “Oh, it’s you. You exist? You want a job? It’s in control theory. Don’t tell them you don’t know anything about control theory; it’s all just linear programming.” So, thanks to Danny’s unique way of operating, I spent two months consulting in Colorado and continued on to Stanford.

Despite (or perhaps because of) his joking around, Danny has a knack for effective communication. Jerry Griggs remembers that Danny agreed to take the department chairmanship at a time of great friction between pure and applied math. He was able to smooth things over, and the two sides remain united to this day.

On the other hand, Danny’s brand of commentary is not always successful. Mike Saks describes an unusual lecture at MIT:

> Once, there was a talk from a music composer, who discussed the use of combinatorial designs in modern musical composition. There was a serious communication gap between the speaker, who was using mathematical ideas rather loosely to accomplish his artistic ends, and the audience of mathematicians, who insisted on precise definitions and propositions. As the audience grew more insistent, the seminar became even more chaotic than usual.
>
> From the back of the room, Danny’s voice rose above the din, “It’s like olives.”
>
> The room was suddenly quiet. Everyone turned around to face Danny.
>
> “It’s like olives”, he repeated. Everyone waited silently.

> "Nobody likes olives the first time they eat them. But if you keep eating them, you realize 'Hey, these are not bad'." More silence in response..
>
> "And that's like this music–the first time you hear it, it doesn't sound so good, but if you keep trying it's not so bad."
>
> Not surprisingly, this speech did little to restore the decorum of the lecture, which was brought to an unsteady conclusion soon afterwards.

Perhaps most important about Danny's ways of communication is his encouragement of students and colleagues. Griggs explains:

> As his student, I spent many hours in his office because he had so many people after him for guidance or a look at their problems. His problem-solving genius is world-recognized, plus he has an exceptional amount of good humor and common sense.
>
> Going to Danny's office was a unique experience. After an initial greeting and a moment's attention, he would be interrupted by a phone call or a new visitor, and he would try to help them. He always tried to help everybody. I finally realized that it was not a queue of people in his office—it was a push-down stack! Once you got set aside, the new arrivals would get his attention first. Sometimes I waited for two hours or longer to pin him down, only to learn that he had to leave immediately. Some of his best advice came while I walked him to his car!
>
> This sounds bad, except I learned so much from the other people who were there. My main thesis problem actually came from Gyula Katona, who showed it to Danny, and I was lucky enough to be there to hear it (and to figure out later how to solve it).
>
> The other big point is that, busy as he always was, Danny came through when it was really important. He was able to give us excellent problems to work on and to give us good advice on dealing with problems, finding jobs, and so on. He was always supportive and encouraging. That is why we were so happy to be able to put together a conference in his honor and why it was so easy to attract a first-rate slate of speakers for it.

Another perspective on what it is like to be Danny's student comes from Ken Winston:

> Shortly after I became one of his PhD students, he described a problem to me and proposed that we both think about it. I went off and thought about it and got nowhere. When I next saw him a few days later, he announced with great glee "we solved it!" He then proceeded to sketch out a method of attack on the problem that was so unlikely that I thought he was nuts. It was like a quarterback calling a play saying "I'll take the hike, then trip over my shoelaces and lose control of the ball, which will carom into number 22's hands. He will then do a triple somersault and land upright 10 feet from the nearest defender, at which point he will easily walk across the goal line."

Kleitman told me to go check this out. I did, the quarterback tripped, the ball caromed, number 22 caught it, the touchdown was scored. It came down to a very tight inequality—that he couldn't possibly have done the calculations on while coming up with his sketch of a proof—just barely squeaking by. This became the basis of the first paper we wrote together. I was incredibly impressed.

So when "we" worked on the next problem and Kleitman again announced a totally unlikely sketch of a proof and I went off to check it out, I was dismayed when I couldn't make the proof work. In fact I came up with a counterexample to one of the interim steps that showed the whole line of attack would never work. Since I was completely convinced that Kleitman was the magic man, I figured I must have it wrong. I tried to find something wrong with my counterexample for a long time. Finally, I crept into his office and tentatively said "I must be missing something, but ..." and gave him the counterexample. He looked at it for about two seconds, said "Oh, yeah, that'll never work. Well, how about this ..." and was off and running with another, completely different approach.

Eventually I figured out that his hit rate was something like one out of five—he just spewed out innovative ideas, most of which didn't work. But he could churn out 10 innovations a day, and two would be incredibly clever and solve something no one else could. The other eight would also be incredibly clever but would not actually work, which was probably more an indictment of the real world's dreariness than of Kleitman's cleverness. But the only cost of the ones that didn't work was some graduate student's time, which is to say no cost at all.

I worked on my PhD thesis for the next two years. During that time, Kleitman and I wrote (as I recall) five papers together. I worked on the asymptotic number of tournament score vectors and made good progress. I got a lot of partial results and set up a huge mechanism for proving the big prize—the actual asymptotic number. This mechanism chopped the problem down to one ridiculously simple statement in elementary combinatorics.

I then worked for about four months straight doing nothing but trying to prove this ridiculously simple statement, which eventually became known as the "Kleitman–Winston Conjecture." I figured if I didn't prove this last piece and solve the entire problem, I couldn't get a PhD. Finally I was willing to admit defeat. I walked into Kleitman's office and said "I can't prove this ridiculously simple piece. I want to get my PhD and graduate. So I'm ready to give up and start on a new thesis topic."

Kleitman gave me a rather odd look and said "It would be great if you could solve this piece, but what you've already done is probably about two or three PhD theses already. Just write it up and you're done." I was ecstatic and walked out of his office on Cloud Nine.

It was only later that it occurred to me to wonder why, if I had enough material for two or three theses, he hadn't informed me of this one or two theses ago. My best guess is that he thought I was so thrilled by the chase that I had risen above petty earthly concerns like getting a piece of parchment. To my shame I hadn't, and crassly insisted on getting a PhD. After that I went on to a happy life far removed from combinatorics.

Every time I visited Kleitman for the next 16 years or so, he greeted me with a new approach to the frustratingly simple little problem, but nothing worked. A number of other mathematicians worked on this problem over those 16 years as well, no doubt egged on by Kleitman. In 1999, it was finally solved by Jeong Han Kim and Boris Pittel [28].

Some people are generous enough to give you the shirt off their back. Kleitman takes this one step farther. Tom Trotter explains:

A well known Native American saying is that you can't know a person until you've "walked in their moccasins." The DJK update is that you don't really understand someone until you've "worn their underwear." Here's how it all happened.

One winter, I visited MIT for a few days and was graciously invited to stay with the Kleitmans. It would be another story to write about the many and very curious treasures of their Newton home ... but on with this tale.

A bitter cold snap swept through New England and the water pipes froze, leaving the house without heat. To make it worse, I was headed on to New Hampshire where the cold was even more severe. Since I was totally unprepared for this kind of weather, Danny lent me a pair of his long underwear, which I used to survive the next several days. The underwear were subsequently washed and mailed back to Newton, with lasting appreciation.

Tom also reports that at a one-week meeting in Oberwolfach, Danny wore the same maroon V-neck sweater on four consecutive days. He wore it in all four possible orientations, one each day.

Danny has a unique approach to transportation. Richard Stanley explains:

Danny is an expert in determining the optimal method for driving between two places. Once when his car was being repaired I drove him from MIT to his home in Newton. I was given constant advice on the exact route to take, the precise speed to drive, etc.

As we approached a busy intersection where the light was red, Danny said to get into the extreme right-hand lane (which was empty) and not to slow down, because the light would change to green exactly when I reached the intersection. I told him that this was too risky because a driver might run the light in the other direction just after it turned red. He scoffed at this idea and continued to tell me not to slow down. Nevertheless, I stopped at the light just as it turned green. A second later a car ran the red light in the other direction going about 60 mph! That was the end of Danny's driving advice for that trip.

When Griggs and West visited Danny in Maine, he took them out to a lobster dinner near the shore. Jerry had quite some difficulty keeping Danny's car in sight as he tried to follow him on the half-hour drive to the restaurant.

Tom Trotter reports that Danny claims that there is a special zone surrounding Boston in which the rules of driving are different from elsewhere. For example, drivers are allowed to park anywhere, and the right-of-way is transitive. The cardinal rule of Boston driving, however, is "Never show fear". Tom explains:

> Danny was driving me to the airport. As we neared the tunnel and many lanes merged into two, Danny squeezed out a bus. The bus wheel went up on the curb, and the bus was leaning over us. Danny refused to look at the bus, saying that if he did so, then the bus driver would know he 'had' us.

Several stories about traveling come from Danny's year in Denmark. He planned to meet a tour group in Munich to tour Yugoslavia, because afterward he wanted to continue on to Greece instead of returning to Denmark. When he arrived in Munich, they were nowhere to be found; it turned out that the tour had been canceled for lack of interest. Danny was left to make his way through the Slavic countries on his own. Not speaking any of the languages, he communicated in sign language, bumped along on rickety buses, shared a room with a Turk (without knowing it in advance) who complained about his snoring, and had many other unusual experiences. He discovered that the way to secure a bench to lie down on a European night train is to sleep in your clothes and look as disheveled as possible, to frighten away other potential occupants of your compartment from asking you to sit up so they could share your bench.

The institute in Denmark arranged housing for post-docs, and Danny's was at the end of a long trolley line with many stops. He figured it would be more efficient to buy a bicycle. This he expected to be easy, since everyone there rode bicycles and bicycle shops were everywhere. Unfortunately, he didn't speak Danish and didn't know how to ride a bicycle, but he had a phrasebook and a belief that everyone in Denmark spoke English.

He entered a bicycle store and asked in his best broken Danish for a used bicycle. The shopkeeper had no idea what he was talking about. The same thing happened at the next bicycle shop. He realized that the problem was that although Danish looked like English, everything was pronounced completely differently. At the third bicycle shop, he decided to try English and asked whether they had used bicycles. "Oh, yes", said the shopkeeper, "come with me." Mystified, Danny followed the man out the front door and down the block. Finally, he pointed to the curb and said, "Here it is". It was the bus stop!

Danny felt obligated to take the bus and found another bicycle store. This time he succeeded in buying a bicycle. The shopkeeper encouraged him to try it out. Somewhat sheepish about not knowing how to ride it, Danny walked the bike around the corner before beginning his experiments in bicycle riding.

Danny tells us a bit more about that bicycle:

> Danish bicycles all had combinations; of course there were only 32 or 64 possible combinations, so it was not a very strong security measure. One day I went to the bike rack at the institute to get my bike, and several friends and I rode to a restaurant where we ate. When I came out, I noticed that I had taken the wrong bike, though it had my combination. It was slightly different from mine. I rode back to the rack and found my bike still there.

I tried to find out whose bike I had taken. After some detective work, I found somebody who told me I had stolen *my own bike*. That bike had been left by a previous institute visitor for the next American visitor, who was me. Of course, nobody had bothered to tell me of its existence before this. The bike disappeared again a few weeks later.

I never was able to tell whether this strange incident was a practical joke created by some mischievous Danish students (who did similar odd things) or happened the way it seemed.

Sometime during the 1970s, Kleitman began a famous seminar course that continues to this day. In the early days, he would bring in a pile of papers at the start of the semester and toss them at the various students (and post-docs), saying, “Some of these are very bad papers. But, it’s *good* to read bad papers.” The students were required to read and present the papers to the class. Griggs explains Kleitman’s method:

Danny taught us to set aside the paper after looking at what it found and try to solve the problem or prove the main theorem ourselves. If Danny can’t prove it in half an hour, it means it is a good paper (nontrivial). I think a half hour for Danny equals a day for the rest of us. Anyway, very often we came up with original proofs or more general results that were worthy of publication themselves. We learned to work on all sorts of problems, so that the seminar broadened our experience tremendously. Refereeing many papers was also valuable. Danny does not just isolate students to work on single, narrow problems.

Doug West recalls “One student tried twice to present a paper, and everyone remained confused. Danny grabbed the paper, raised his arm, and looked around the room. His eyes settled on me, and he leveled his arm to point directly at me. ‘You!’ he said. ‘Fix it!’ It turned out that this was a problem he had worked on some years earlier with David Daykin. They had both forgotten most of it, and the work remained unfinished. I filled in the rest of the details and took it a bit farther, and it became a chapter of my thesis.”

Years later, Danny’s volume of editorial work had increased. Debbie Franzblau reports: “He would clean off his desk, put everything in a box, and then have the students choose something to present. It turned out that the paper I picked was something he was supposed to referee. I found an error, ended up fixing it, and that was my first publication.”

Some think that when Kleitman is not doing mathematics, he spends his time acting in the movies. He was an extra in the movie “Good Will Hunting”, which purported to show the discovery of a mathematical genius at MIT. Danny also was a consultant on the movie; he and then-post-doc Tom Bohman had a mathematical conversation in the common room to show the moviemakers what mathematics was like. Jerry Griggs reports “The movie professor has a giant office—it is thought that the movie people (Damon and Affleck) must have thought that the MIT applied math commons room where Danny and Tom met with them was actually Danny’s office.” The amusement in the mathematical community was such that Danny penned an essay in the *AMS*

Notices [34] entitled “My Career in the Movies”. Interested readers can find further details there. Jerry Griggs prepared a special version of the poster for the movie; it resides at (http://www.math.sc.edu/ griggs/Djk/gwh.gif).

In fact, Danny’s avocation outside mathematics is antiques. It is common when a professor goes off to a conference that a TA gets an opportunity to teach a class. With Danny, this happens when there is a particularly important antique auction.

Danny and his wife Sharon have an antique shop on their farm in Maine called *Sharon’s Shed*, and they have expanded by adding a store in Newton. The shop in Maine is filled with all manner of antique Americana. Every five years or so Doug West would show up with a new fianceé, and Sharon sold them antique clothing, old buttons, or African sculpture.

The farm itself fuels one of Danny’s endearing habits. As Griggs puts it, “In the fall he would greet you with ‘How ya doin? You want an apple? Go ahead, take an apple!’ He would always have a basket full of tasty apples from the farm up in Maine.” Indeed, he would bring the basket to his undergraduate class in discrete mathematics and toss the apples out at the students.

Once when West came to visit, the Kleitmans arranged to meet him at an antique auction. During the viewing period, Danny carefully examined the items to be auctioned. During the auction, he and Sharon sat in the middle of the front row. They had a unique approach: He made the bids, but as the bid rose he would look at Sharon for authorization to bid higher. Somehow this gave them the psychological effect of having an extra bidder in the fray.

Danny and Sharon have a wonderful relationship, with his clowning balanced by her down-to-earth good nature. She also is wonderfully protective of his students and interested in their progress in life. West provides an example:

> I was sitting in Danny’s office just before my thesis defense when he picked up the phone and dialed home. “Hey Boog, is everything ready for the party?” ... “Whaddya mean, ‘What party?’ The party to celebrate Doug West failing his thesis defense” ... “Yeah, he’s right here.” Danny gave me the phone, saying Sharon wanted to talk to me. She said, “Now, don’t you listen to a single word he says.”

Danny and Sharon’s huge rambling old house in Newton serves partly as a display case for antiques. Sharon keeps it immaculately arranged except for the one room, somewhere up on the nth floor, that is designated as “Danny’s room”. Inside that room are tottering piles of books topped with antique typewriters and the like. In that room he is allowed to keep whatever he wants.

“Danny’s room” is an extension of his office at MIT, or maybe his office is an extension of his room at home. In any case, that brings us to a description of his famous office. Griggs provides a few details:

> He had an amazing assortment of antique tools and curiosities. I loved the stuffed owl. He asked people to guess what they were used for. My favorites included an amazing apple peeler, an eel spear, and a “rooter” which was used to pinch the

> snout of a pig to prevent it from rooting, or digging up, vegetables (or truffles, I expect).
>
> There was an MIT library book sale when I was in school, in the mid-70s. I picked up one book for fun, called Hazell's Annual. I picked through them and decided that the 1899 edition was the nicest. Later I discovered in Danny's office that he had purchased the other dozen or so years available, and he seemed annoyed that I had bought even one of them! He also purchased the large bound volumes of the *New York Times* from the 1940s. They remained stacked in his office for years, serving as overflow sofas.

The volumes of the *New York Times* were from 1944, with plenty of war reporting. It was one day in the summer of 1975 that Danny rounded up eight graduate students and brought them to the basement of the next building, under the library. Together they ferried the books to his office and placed them in library fashion at the base of the wall, under the chalk tray of the long blackboard, extending for about 15 feet. The books gradually became buried under paper. Griggs continues:

> He had a large bicycle wheel that had been converted to a wheel-of-fortune, probably from a travelling carnival, with numbers from 1 to 100 on it. He would ask a visitor to spin the wheel and guess what number would come up. If I happened to be there, he would ask me to guess as well. The visitors would be very impressed usually, since it usually landed on the same number, 79, or just missed it!
>
> For a lesson on probability, he might bring in the wheel [to class]. Students expect such a wheel to land on each number with equal probability. Someone would spin the wheel. When it stopped on a number, Danny would reach into his pocket and pull out a slip of paper with the number written on it! Of course, since it usually landed on 79, this often worked. But the wheel was often off by one or two from 79, and I wondered how he dealt with that. Well, Danny reached into another jacket pocket and showed me 78; a slip in a pants pocket had 80; another pants pocket had a slip numbered 81; and so on! He was prepared for a whole range of numbers!

Other memorable items that have adorned Danny's office for decades include a wooden duck (a hunting decoy) and a large winged red Pegasus from the first era in which Mobil used the symbol. One of his favorites is a small machine patented in 1855 that purports to cure all diseases, which he encourages visitors to try. The patient turns a crank, and the machine builds up static and administers a small shock.

Listing the unusual and unexpected items in Danny's office does not paint an adequate picture of its condition. There is also a layer of papers several inches think over every surface, including the floor. The reprints of a single paper of Danny's could be found scattered in every corner of the room.

During the summer of 1976, Danny entrusted West with the keys to his office in order to collect and sort the mail while he was away. Left to his own devices, it seemed perfectly natural to the poor student to put the reprints of each paper together, to organize the issues of particular journals in series, to file correspondence, etc. Danny never complained, but he must have been aghast at the order that temporarily reigned when he returned (probably it was "too horrible to contemplate"). Danny reports that a similar disaster recently befell him when a corner of the room needed to be cleared for access to wireless local area network. A student was assigned to box up the contents of that corner and proceeded to put the contents of most of the room into boxes!

Griggs continues:

> Another story about his office concerns the many people who signed their names on his chalkboard with the date they signed. People at the conference wondered how this came about, so I told them at the banquet. Danny was my assigned advisor in grad school at MIT. When I was just leaving for the summer of 1974, after my first year there, I thought I should leave my contact info for him in case he needed to reach me. So I wrote my phone number and address in California on his blackboard. (This was long before email!)
>
> The staff at MIT is good about NOT cleaning boards where there is writing. What I wrote was still there when I returned in the fall, and stayed there all fall. It stayed there all year, and the next year, and was still there when I graduated in 1977. It was still there when I visited a couple of years later.
>
> To my disappointment, Danny changed offices in the early 80s (I forget exactly when) and moved across the hall. To start something going in the new office, I wrote in the corner of the blackboard my name and the date I had been there. I thought it would be fun to see how long it would stay up on his board. When I next visited some 5 or 6 years later, I was astonished to see, not just my name, but names of many other visitors to his office with dates of their visits! It is an impressive list, and it should still be there as I write this.

The stories above provide the context for the other poem that was presented at the conference banquet, written by the then-ten-year-old daughter of Jerry Griggs:

Danny Kleitman
—poem by Malia Griggs

He has antiques and a house
  Got three children and a spouse
Get the hose and water the lawn
  Oh, please pass the Mexican flan
He's got an office, full from A to Z
  He hasn't even got room for me
He's got a movie, all for him
  He eats Thai food, till he's slim
He's got this inkwell, of a man eating turkey
  And he's got the biggest brain cells, that ain't no beef jerky
He has this wheel, that hits 79,
  it seems like each and every time

Sharon is his spunky wife
He's had her for part of his life
Sharon keeps him on the line
(Hey, you know this is a rhyme!)
So, we're here to celebrate his birthday today,
I'm sorry for the people who have to pay!

4. The mysterious G.W. Peck

G.W. Peck published 14 papers in discrete mathematics from 1980 to 1992, plus about 30 signed reviews in *Mathematical Reviews*. Who is G.W. Peck, and why is he the author of this paper? We let Kleitman begin the story:

> At one time Paul Erdos in his extensive travels worked on a problem with George Purdy; I have forgotten what it was about, and I am not really sure I ever knew. Anyway, when Paul visited Ron and Fan, they had some comments about the result which extended it or applied it or simplified its proof. Subsequently I had done something which simplified the proof still more. By this time the result and its proof could be stated in little more space than would be required to list the authors, if all of us were to be given credit for our contributions to it.
>
> Ron called me one day and pointed out the silliness of submitting a paper of that sort, but he thought that the idea should be published. He suggested having each contributor get one letter in a single named author. While playing with the idea, it occurred to him that the relevant letters were G(raham), P(urdy), E(rdős), C(hung) and K(leitman), and these naturally formed the combination "G. Peck". Gregory Peck being a famous movie star, this seemed like a name that had an existence of its own, which lent it cachet and verisimilitude that supported the idea.
>
> The original problem had turned out to be a special case of something that I already knew. After G. Peck had already been invented, it occurred to me that the paper would be much better if it was more than the result I already knew. The issue was, how to find a more general and new result that included the old one and said something new and nontrivial.
>
> Shortly thereafter I mentioned the problem to Doug West, who was a graduate student at the time, and he suggested an extension of the result which was a clear improvement to it, and it seemed appropriate to add his initial as well, if a paper was to be prepared on it. And so G.W. Peck submitted his first paper, which was duly published [P1].

West remembers this part of the story a bit differently and thinks that Danny is being too generous. Here is his rebuttal:

> One day Danny came in from the snow to the applied math common room and told me about a problem, using his galoshes as a pointer to various parts of the

> board. He wanted to know how many integer rectangles could be found in an m-by-n rectangle so that no two of them had the same projection in one coordinate and projections ordered by inclusion in the other. A family of subrectangles with fixed perimeter satisfies the constraint, and he said we should be able to prove that the largest legal family has this form. This would be a nice example in Sperner theory.
>
> I suggested looking at the 3-by-2 rectangle. Here there are seven subrectangles with sides of lengths 0 and 1, and this is the largest family with fixed perimeter. However, the six 0-by-0 points plus the two 1-by-1 squares form a legal family of size 8.
>
> Danny shouted, “Oh, my God, that’s it!”. “What’s it? What are you talking about?” I replied. “That’s the answer! You’ve solved the problem.”
>
> From the one small example, Danny had instantly seen the full solution, along with the proof. The maximum family in general is obtained by taking all the subrectangles that are squares. He didn’t really tell me what I had supposedly done, but he did tell me to generalize it and write it up.

Kleitman continues:

> It seemed at first that we had lost something in adding Doug’s initial, since Gregory Peck lacked it, as far as we knew, but we added it.
>
> One day a few years later, while looking through a book about prominent figures of the 1880s that came in a lot of books I had bought at an auction, I discovered that there really was a G.W. Peck, in fact George Washington Peck, and he was quite a colorful figure. He wrote a humorous column for various newspapers and subsequently a number of books about an extremely obnoxious boy who was constantly inflicting awful practical jokes on everyone around him. You may even have heard of him. His most famous book was called *Peck’s Bad Boy and His Pa* (it’s still very funny).

According to the preface of one edition of that book, G.W. Peck was born in 1840 in Henderson, New York, and was raised in Wisconsin. He had a succession of failing business enterprises before entering the Union Army in 1863. Although in the 1870s he bought a newspaper (Peck’s Sun) in Lacrosse, Wisconsin, and moved it to Milwaukee, he had little success until in 1882 he began publishing stories in this newspaper about Peck’s Bad Boy. Between 1882 and 1907 he published 10 books, mostly about Peck’s Bad Boy (he published one earlier unrelated book in 1871).

On a platform of opposition to the Bennett Bill, which prohibited the use of foreign languages in schools (the Lutherans taught in German and the Roman Catholics in Latin), in 1890 Peck was elected mayor of Milwaukee by the widest margin in the history of the city. In the same year, he was elected Governor of Wisconsin by a landslide. He was reelected Governor in 1894.

In 1979, Peck began publishing research in discrete mathematics. His first paper acknowledges the six individuals named above for “suggesting the problem, proving the theorems, and writing up the final paper”. Kleitman continues:

> Shortly after G.W. began his new mathematical career, a student came to me with a manuscript by Richard Stanley in which Richard had proven the following result. Suppose we have n distinct positive integers; how should they be chosen to maximize the number of subsets of them that have the same sum? The answer is to chose the integers from 1 to n. He had proven this after going to California on sabbatical, and in those pre-email days it was not easy to communicate with people that far away whose addresses were not known. He later told me that he had proven a related result which his landlord in California, who happened to be Jerry Griggs, said proved this statement.
>
> The student wanted to know what this implied about the corresponding statement when the n integers were distinct but not necessarily positive. This latter question had been raised by Erdős. We were able to prove the appropriate result in that case from the first, but it didn't seem right to submit a paper on this, since it seemed probable that Richard knew the answer already, if we knew how to reach him. On the other hand, it seemed appropriate that the graduate student get some credit for her efforts. So it occurred to me to let G.W. Peck submit a paper on this [P4], and to tell her to list it as a publication, as she was as entitled as anyone to be a part of G.W. Peck.

Trotter and West wrote a paper and sent it to Kleitman for comments. Back came a letter, "It seems that your proof has an error. Fortunately, G.W. Peck and Peter Shor have found a way to fix it. I suggest that you include them as coauthors." The outcome was [P9]. Another time Kleitman contributed fundamental results to a problem that Trotter and West had been discussing and on which Madeleine Paoli had worked very hard. The list of authors came out as Paoli, Peck, Trotter, and West [P13].

Such papers pose some difficulties for those who study the collaboration graph of mathematicians. Kleitman explains:

> Ron Graham popularized the notion of *Erdős number*, defined to be the distance by number of collaboration links from Uncle Paul. Since Paul was a part of the imaginary G.W. Peck, links to Peck constituted imaginary links to Paul. The logical rules for computing Erdős distance in the complex plane require a paper in itself, which paper is itself imaginary, still another complication.
>
> I never determined what Erdős number resulted from being a part of Peck, or whether it was really appropriate to give someone who collaborated with Peck an Erdős number of $1 + i$ or i to account for this. Of course, the linkage to Paul implied by being a part of a later Peck paper gives rise to a whole new dimension of potential Erdős numbers. Straightening this out is perhaps a good topic for a graduate student. Of course you can run into the same problem in assigning an Erdős number to a paper on that topic as Goedel found with assigning a number to the statement: 'this statement is false'.

The numerical aspects were never resolved. In light of the papers mentioned above, Griggs was given an Erdős number of $1 + i$, and West became viewed as one of the few mathematicians who has written a paper with himself. When he arrived at the University of Illinois, West was assigned to share an office with one N.T. Peck, a complex analyst who turned out to be a distant relative of the original G.W. Peck. For most of the seven years they shared the office, a sign on the door turned West into Peck in four steps: WEST, PEST, PERT, PERK, PECK.

Kleitman concludes:

> Over the years, for one reason or another, G.W. Peck became author of about a dozen or more papers and after a while contributed reviews to *Mathematical Reviews* as well. All reviews written by Peck were solicited in letters from *Math. Reviews* which got sent to me (at least all those that I knew about). Peck was quite diligent in responding to correspondence.
>
> I toyed with the idea of trying to get him a job, but his capacities as a speaker are limited, and I think the IRS frowns on carrying this to the point of receiving payments.
>
> All in all, while he is not a major figure of any kind, he has done some nice work, has had the concept of a "Peck Poset" named for him [43], and has bothered nobody for letters of recommendation: a fitting tribute, I think, to the historical George Washington Peck.

As Appendix A, we have provided a chronological list of the publications of G.W. Peck. We note that two of the papers by G.W. Peck, including the original 1979 paper, were reviewed in *Math. Reviews* by one Daniel J. Kleitman.

Acknowledgements

The author wishes to thank the editors of this volume for writing this paper and the colleagues and students and memory of Daniel Kleitman for sharing their recollections.

Appendix A. Publications of G.W. Peck

[P1] G.W. Peck, Maximum antichains of rectangular arrays, J. Combin. Theory Ser. A 27 (1979) 397–400 (Reviewer: D.J.Kleitman).

[P2] G.W. Peck, Short proof of a general weight Burnside lemma, Stud. Appl. Math. 60 (1979) 173–176 (Reviewer: L.C. Grove).

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[P4] G.W. Peck, Erdős conjecture on sums of distinct numbers, Stud. Appl. Math. 63 (1980) 87–92 (Reviewer: G.O.H. Katona).

[P5] G.W. Peck, Hamiltonian cycles of adjacent triples, Stud. Appl. Math. 63 (1980) 275–278 (Reviewer: S.F. Kapoor).

[P6] G.W. Peck, A Helly theorem for sets, SIAM J. Algebraic Discrete Methods 2 (1981) 306–308 (Reviewer: D.J. Kleitman).
[P7] S.F. Assmann, G.W. Peck, M.M. Sysło, J. Zak, The bandwidth of caterpillars with hairs of length 1 and 2, SIAM J. Algebraic Discrete Methods 2 (1981) 387–393 (Reviewer: A.K. Dewdney).
[P8] G.W. Peck, A new proof of a theorem of Graham and Pollak, Discrete Math. 49 (1984) 327–328 (Reviewer: H.J. Straight).
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[P12] G.W. Peck, A. Shastri, Bandwidth of theta graphs with short paths, Discrete Math. 103 (1992) 177–187 (Reviewer: Y. Lin).
[P13] M. Paoli, G.W. Peck, W.T. Trotter, D.B. West, Large regular graphs with no induced $2K_2$, Graphs Combin. 8 (1992) 165–197 (Reviewer: R.A. Beezer).
[P14] D.-Z. Du, D.F. Hsu, G.W. Peck, Connectivity of consecutive-d digraphs, Discrete Appl. Math. 37/38 (1992) 169–177 (Reviewer: L.W. Beineke).
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Appendix B. Academic descendants of D.J. Kleitman

1969 Kenneth Lebensold
1973 Arnold Barnett
  at MIT:
  1985 Anthony LoFaso
  1990 Jonathan Caulkins
  1990 C.S. Venkatakrishnan
  1994 Armann Infolfsson
  1995 Robert Shunsky
  1996 Alan Rimm-Kaufmann
  1997 Michael Miller
  1997 Arni Haukkson
1973 Da-Lun (Dave) Wang
1974 Wen-Ning Hsieh
1975 David Kwiatkowski
1975 David Scheim
1975 Joseph Cheng
1977 Jerrold R Griggs
  at U. South Carolina:
  1989 Mingshen Wu
  1990 Roger K.-C. Yeh
  1991 Christine Sun
  1991 Daphne D.-F. Liu
  1993 Yan-Chyuan Lin
  1993 T. Kim Jonas
  1994 J. Ouyang
  1996 Chuanzhong Zhu
  1997 Peter Sandberg
  1998 Chih-Chang Ho
  1998 Éva Czabarka
1977 Harvey Diamond
1978 Jean (Jeanine) Tamaki
1978 Douglas B. West
  at Princeton:
  1984 Edward R. Scheinerman
    at Johns Hopkins:
    1991 Ann Trenk
    1995 Paul Tanenbaum
    1996 Karen Singer (Cohen)

        - 1998 Gregory Levin
        - 1998 Donniell Fishkind
        - 2001 Michael Capalbo (w. Kosaraju)
    - at U. Illinois (Urbana):
    - 1988 Thomas M. Kratzke
    - 1988 Margaret L. Weaver
    - 1990 André E. Kézdy
    - 1991 Hunter S. Snevily
    - 1993 Todd G.Will
    - 1993 Myung S. Chung
    - 1993 In-Jen Lin
    - 1994 Yi-Wu Chang
    - 1996 Glenn G. Chappell
    - 1997 Christopher M. Hartman
    - 1998 Dhruv Mubayi
    - 2000 Tao Jiang
    - 2001 Radhika Ramamurthi
    - 2002 Michael Pelsmajer
- 1979 Ken (Kenneth) Winston
- 1980 Seth Chaiken
    - at State U. New York (Albany):
    - 1989 Young Cheul Wee
- 1980 James Shearer
- 1980 Michael E. Saks
    - at U. California (San Diego):
    - 1993 Fotis Zaharoglu
    - 1994 William Burley
    - at Rutgers:
    - 1994 Andres Fundia
    - 1996 Shiyu Zhou
    - 2001 Clifford Smyth
    - 2001 Srikrishna Divakaran
- 1983 Susan Assman
- 1983 Fernando Lasaga
- 1984 Dean Sturtevant
- 1985 Debbie Franzblau
- 1987 Forrest Quinn
- 1987 Steven Altschul
- 1988 Dimitri Bertsimas
    - at MIT:
    - 1990 Michel Goemans
        - at MIT:
        - 1993 David Williamson
    - 1990 Daisuke Nakazato
    - 1991 Garrett van Ryzin
        - at Columbia:
        - 1996 Noah Gans
            - at Wharton:
            - 2000 Yong-Pin Zhou
            - 2001 Sergei Savin
        - 1999 Siddharth Mahajan
        - 2001 Itir Karaesman
    - 1992 Peter Vranas
    - 1992 Michael Peterson
    - 1993 Carolyn Haibt-Norton
    - 1994 Haiping Xu
    - 1994 Zhihang Chi
    - 1995 Georgia Mourtzinou
    - 1995 Jose Niño-Mora
    - 1995 Michael Ricard
    - 1995 Joe Milner
    - 1995 Andrew Luo
    - 1996 Chung-Piaw Teo
    - 1996 John Paschalidis
    - 1997 Sarah Stock (Patterson)
    - 1997 David Gamarnik
    - 1998 Thalia Chryssikou
    - 1999 Ioana Popescu
    - 1999 Leon Hsu
    - 1999 Jay Sethuraman
- 1990 Aditya Shastri
    - at Banasthali:
    - 1997 Suruchi Gaur
    - 1999 Ramprakash Somani
- 1991 Philip Hahnfeldt
- 1992 Bill Hoff (Edwin)
- 1992 Erlan Wheeler III
- 1992 Wayne Goddard
    - at U. Natal
    - 2002 Mieso Denko Kabeto
- 1993 Lenore Cowen
    - at Johns Hopkins:
    - 1999 Christine C.T. Cheng
    - 1999 Christopher G. Wagner
    - 2000 Adam H. Cannon
- 1994 Daniel Port
- 2001 Lizhao Zhang

Appendix C. Coauthors of D.J. Kleitman

Abbw-Jackson, D.
Aggarwal, A.
Aigner, M.
Alon, N.
Aronov, B.
Assman, S.F.
Bárány, I.
Barnett, A.
Batzolgou, S.
Beineke, L.W.
Berger, B.
Berman, K.
Bird, E.
Bohman, T.
Burkhard, W.A.
Burosch, G.
Chaiken, S.
Chow, Y.
Chung, F.R.K.
Claus, A.
Clapham, C.R.J.
Coleman, S.
Coppersmith, D.
Cowen, L.J.
Daykin, D.E.
Demetrovics, J.
Du, D.Z.
Duffus, D.
Edelberg, M.
Ein, L.M.H.
Entringer, R.C.
Erdős, Paul
Erdős, Peter
Fan, C.K.
Fellows, M.
Felzenbaum, A.
Finkelstein, L.
Fisk, S.
Frankl, P.
Franzblau, D.S.
Fredman, M.L.
Füredi, Z.
Furry, W.H.
Glashow, S.L.
Goddard, W.
Goemans, M.
Golden, B.
Graham, R.L.
Greene, C.
Griggs, J.R.
Gyárfás, A.
Halász, S.
Hamburger, P.
Heilmann, O.J.
Holzman, R.
Hsieh, W.N.
Hsu, D.F.
Hu, T.C.
Johnson, D.S.
Kahn, J.
Kalai, G.
Katchalski, M.
Katona, G.O.H.
Klawe, M.
Klugerman, M.
Koren, M.
Krieger, M.M.
Kwiatkowski, D.J.
Lander, E.
Lasaga, F.
Leighton, F.T.
Lemke, P.
Lepley, M.
Leung, J.Y.-T.
Lew, R.A.
Lewin, M.
Li, S.-Y.R.
Liebowitz, R.
Lieb, E.H.
Lipman, M.J.
Lipton, R.
Losonczy, J.
Lubell, D.
Ma, Y.
Martin-Löf, A.
Magnanti, T.L.
Markowsky, G.
Meshulam, R.
Meyer, A.R.
Miller, G.L.
Milner, E.C.
Ngo, H. Q.
Nussinov, R.
Pach, J.
Pachter, L.
Paoli, M.
Pieczenik, G.
Pippert, R.E.
Pomerance, C.
Purdy, G.
Rabin, M.
Richman, D.R.
Rivest, R.L.
Rosenbaum, D.M.
Rothschild, B.L.
Saks, M.E.
Sapozhenko, A.A.
Schulman, L.J.
Seymour, P.
Sha, J.C.
Shastri, A.
Shearer, J.
Sherman, S.
Shor, P.
Spencer, J.
Steiglitz, K.
Sturtevant, D.
Sussman, D.E.
Sysło, M.M.
Székely, L.A.
Tamaki, J.K.
Thomassen, C.
Toth, G.
Trotter, W.T.
Vohra, R.V.
Wang, D.L.
Weiner, P.
West, D.B.
Whinston, A.
Winklmann, K.
Winston, K.J.
Zak, J.

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