LENS DESIGN WITH LARGE COMPUTERS

Report on the International Conference
Rochester, New York
July 5-8, 1966

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Lens designers who use automatic programs and large computers are no longer considered to be pioneers but are now an integral part of the world's rapidly-expanding optical industry. Various techniques of automatic correction, though still in the developmental stage, are finding daily application. A designer armed with a powerful lens design program and a capable computer may now design, in a matter of hours, a lens system which would have taken months to perfect only a few years ago; the results are consistently better than those obtained by classical methods, using log tables and desk calculator.

Realizing that much has been learned about automatic design in the past decade, and realizing that the time was ripe for this knowledge to be shared, the Institute of Optics (University of Rochester) sponsored an international conference on Lens Design with Large Computers. Following is an agenda and list of speakers at this conference, held July 5-8, 1966, in Rochester, New York.

July 5
1. Welcoming remarks: W. Lewis Hyde, Institute of Optics, University of Rochester
2. Eight years of lens designing with large computers: C. G. Wynne, Imperial College, London

July 6
5. Odds and ends from a gray box: R. M. Walters, American Optical Co., Pittsburgh

7. The design of double Gauss lenses: M. J. Kidger, Imperial College, London


9. Optical design using tolerances: Koichi Yuta, Institute of Optics, University of Rochester, and Olympus Optical Co., Ltd., Tokyo


11. The CERCO program for an automatic lens design: Edgar Hugues, Centre de Recherches et de Calculs Optiques, Neuilly (Seine), France

12. Semi-automatic lens design on a large computer: Josef Meiron, Perkin-Elmer Corp., Norwalk, Conn.

13. Construction of the merit function in automatic optical design: Jan Hoogland, Perkin-Elmer Corp., Norwalk, Conn.

July 7

14. A comparison among three modern optical design programs: Robert E. Hopkins, Institute of Optics, University of Rochester

15. Experience with the LEAD program: R. Kingslake, Eastman-Kodak Co., Rochester, N.Y.


19. Optical design using the ORDEALS program: M. J. Buzawa, Tropel, Inc., Fairport, N.Y.

20. The orthonormalization of aberrations: H. A. Unvala, Institute of Optics, University of Rochester

21. Modern matrix techniques for the design of optical systems: Sandor Majoros, Hungarian State Optical Works, Budapest
22. Wide angle, long eye-relief eyepiece: Seymour Rosin, Reuben Gelles, and Judah Eichenthal, Kollsman Instrument Co., Elmhurst, N.Y.

23. Production tolerances for diffraction-limited optics: Janusz Wilczynski, International Business Machines, Yorktown Heights, N.Y.

24. Treatment of singularities which occur in the lens design problem: Charles A. Lehman, Los Alamos Scientific Laboratory, Los Alamos, New Mexico

25. Experiences using the SPADE program: B. J. Howell, Sperry Gyroscope Co., NASA/Goddard Space Flight Center

26. Simple presentation of the optical transfer function: Kazuo Sayanagi, Institute of Optics, University of Rochester

27. The Optik V lens design program: James P. Wheeler and Lura C. Dean, Texas Instruments, Inc., Dallas

July 8

28. Adaptive automatic correction: E. Glatzel, Carl Zeiss, Oberkochen, W. Germany

29. Comparison of the damped least-squares method and the adaptive method of Glatzel: R. Wilson, Carl Zeiss, Oberkochen, W. Germany


35. A comparison of computers for optical design: E. J. Radkowski, Institute of Optics, University of Rochester

36. Some examples of lens design with smaller computers: M. Isshiki and H. Ikeda, Nippon Kogaku K. K., Tokyo

37. On the merit function for automatic rough design based on the aberration theory: K. P. Miyake, Kyoiku University, Tokyo

39. Lens design program "ALSIE": Tatsuro Suzuki, Osaka University, Osaka, Japan

Now that automatic lens design is no longer in its infancy, several very basic questions are beginning to form in the minds of designers. When the conference was set up, it was suggested that these basic questions be used as guidelines for the exchange of information at the conference. The questions, with answers brought out in the conference, are as follows:

**How many different "optimum solutions" can be found for a given set of specifications, and how can the best one be located?**

Wynne (2) implied that only one optimum exists for a specified set of boundary conditions, variables, weights, etc. However, he mentioned that considerable skill is required to supply the program with the proper tools with which to locate an "optimum" which is not only a solution to a set of equations, or a least-squares minimum, but is representative of a good optical system.

Hoogland (13) emphasized that our present knowledge of the intricacies of merit function construction is perhaps a bit shaky; he presented several examples of cases in which the pupil aberration plots (or even spot diagrams) were misleading and difficult to interpret when attempting to determine, for example, the plane of best focus or the image with highest contrast. He demonstrated quite clearly that systems designed by semi-automatic programs, using classical aberration theory as a basis for operation, require a great deal of skill to manipulate to best advantage. He also demonstrated that the correlation between spot size, aberration theory, ray fan plots, and MTF is not always obvious.
As Kingslake (15) pointed out, one of the biggest problems encountered in designing with computers is the choice of a proper merit function. That is, if the user gives the computer improper weighting or demands, the optimum which the machine reaches for these specifications may be found to be rather far from the required system. One could say that there is much to be learned about "optimizing merit functions."

In regard to the second part of the question, "how is the best optimum located?," ninety per cent of the designers answered--their own programs, of course!

Wilkerson has observed that, at least in the case of programs using exact (as opposed to paraxial) ray tracing in the optimization process, a better solution may be obtained for a given set of weights once apparent optimum for these weights has been reached. This can be done by changing the given set of weights as soon as the program appears to have reached optimum (when the merit function ceases to improve) for the initial set of weights. When the merit function appears to be at a standstill for the new set of weights, the weights should be changed back to their original values and a new run made. The merit function will probably go down during this run with the original weights and will be noticeably better than the "minimum" that occurred the first time with these weights.

If the starting point and specifications are the same, do different programs give substantially the same answer?

Lytle's experience with the Perkin-Elmer lens design program would seem to indicate that, for a given set of demands, boundary conditions, and variables, a unique solution will be found, no matter what the starting point. Wynne (2) implied that a classically derived initial design was
always a reliable takeoff point, but that it would invariably be improved by the computer (except in the simplest cases).

It was Lytle's observation that, though many of the designs discussed at the conference never strayed far from their initial prototypes, others evolved into configurations bearing no resemblance to the prototypes. This behavior may be linked to both the programming intricacies and the location (in parameter space) of the starting design. No one, however, spoke of conducting a concerted study in order to examine how the initialization process constrains a program with respect to location of the final solution domain(s).

Pegis (6) described an interesting program which eliminates the necessity of calculating a first-order prototype, or using one from patent literature. His program automatically generates a first-order configuration consistent with a specified set of physical and optical constraints. This starting system is a degenerate one with the chief and marginal ray heights on all surfaces equal to zero. The ray heights and angles of incidence are the variables, and, except to establish boundaries, the designer has virtually no control over the indices, curvatures, spacings, or, for that matter, the number of elements used to satisfy the assigned criteria.

Kidger (7) discussed a study of the optimization of a family of double Gauss lenses. He stated that two highly asymmetric solutions were obtained for infinite conjugates, and that these were reached from several different starting points. (He did not mention how much different the starting points were, nor whether both lenses were obtained from the same automatic correction prescription.) He noted that in the cases in which the lens was designed for finite conjugates, a symmetric starting design evolved into a substantially symmetric final design. The inescapable conclusion seems to

-6-
be that, in the case of the particular program employed in this work, the starting point certainly did influence the configuration of the final design. In addition, it appears as if more than one acceptable solution to the problem did exist, though possibly obtained through dissimilar merit function construction.

Worth mentioning here is a program entitled "the adaptive method," written by Glatzel (28). It operates as follows: Target values are set for the various classical aberrations according to the designer's requirements. The automatic correction process begins by reducing the one aberration which is farthest from its target value. This is done at the expense of all the other aberrations. When that aberration becomes better controlled than some other one, whichever aberration is then farthest from the target value is operated upon. The optimization proceeds thus until an optimum is reached. This occurs when even a minute correction of one aberration transfers control to some other aberration. It seems that such a program could never diverge, but more investigation of this technique will be needed before the picture is complete.

Also worth mentioning is a comment, in private discussion, by John Buzawa of Tropel, Inc. Like the writers of this report, he also has found the RYDEV correction in ORDEALS to be of limited use, and he has noted the same oscillatory behavior observed at Steward Observatory.

Unfortunately, most of the better programs aren't widely available, as they are proprietary in industry or belong to the Government. Gordon Spencer (18), for example, knows of no program containing diffraction transfer function which is available to all industry.
What types of glass are the programs choosing, and what types of glass are never chosen? How early in the optimization can the glass choice be dropped as a variable and real glasses be designated?

According to Kingslake (15), there appears to be no reliable rule of thumb for predicting the behavior of variable indices and dispersions. In the context of his experience with the LEAD program, glass types may rapidly stabilize, or they may, with great deliberation, move to the imposed boundaries and remain there. In other instances, they may change continuously. In several of the designing runs cited at the meeting, the glasses seemed to wander all over the glass table without improving the design noticeably.

The consensus of opinion in informal discussion seemed to be that too much time and money were being spent trying to make small glass alterations which would at best improve the performance of systems only slightly. It seems as if judicious choosing of glasses from a very limited list would, in the long run, save considerable time and money at little expense to lens performance. An efficient program, it seems, can optimize a complex system to a set of targets with almost any sanely-chosen group of glasses. The Perkin-Elmer program, for example, usually requires only two or three iterations to regain a previous merit function after a glass change has been made. With powerful programs and modern computers becoming more readily available, why waste effort making subtle changes to scarce, exotic glasses (which may be nonexistent)? Spencer (32) presented evidence similar to that of Kingslake, which lends support to this type of thinking.

Is calculation of the modulation transfer function worthwhile during the optimization? If so, how is it used?

Hoogland (13) seemed to be of the opinion that MTF is, most simply stated, a useful method for determining whether or not the designer has
constructed a merit function which is indeed capable of generating a good optical system. Sayanagi (26) inferred that the optical transfer function could, for a moderately well-corrected system, be expanded by the Gaussian series to yield a worthwhile design tool. Along this same line of thinking, H. H. Hopkins (10) discussed the use of the Strehl intensity ratio as a design tool. He showed that it is at the minimum variation of the wave aberration where the best aberration correction and focal plane are found (in a well-corrected system). In a system which is not yet approaching optimum performance, he suggested that a useful merit function would be the ratio of the transfer function in the presence of aberration to its theoretical maximum value, and stated that the introduction of canonical pupil coordinates enables the designer to more easily incorporate this criterion into automatic design programs.

**In complex lens systems (five or more elements), what is the role of aspherics?**

It seemed that most of the delegates to the conference had little faith in the future of aspheric surface generation technology, and they therefore overlook the fact that designs containing aspherics can contain fewer surfaces and still be unquestionably superior to designs employing only spherical surfaces.

**How can a true optimum be distinguished from "stagnation" of the program, from a "computer noise" limit, or from a "plateau" in the improvement?**

Oddly enough, there was little discussion concerning this question. The subject of solution domains and design terrain was largely neglected (or avoided), with the exception of a few remarks by Grey (4). He treated
truncation and round-off error, and explained how their effects may be minimized through proper orthonormalization of the coordinate system employed. There was some discussion of how much precision was necessary in computer arithmetic (as applied to optical design), and most contended that six figures did not provide enough accuracy. A great many, though, held that a well-conceived program could do as well using eight significant figures as using twenty.

What attributes (such as thick components, short back focal distance, defocused axis) tend to distinguish machine-optimized lenses?

Though this question was not treated specifically by any speaker, Lytle and Wilkerson have observed that, as the computer is allowed more and more freedom (a loosening of the boundary conditions), the systems assume configurations which would not ordinarily be conceived by a designer trained in classical methods. There seem to be no universal characteristics peculiar to automatically-designed lenses, and unconventional configurations appear only when the merit function construction brings them about by pure happenstance. There does, however, appear to be a point beyond which sizable increases in the thickness of lens elements yield only minute improvements in performance. The designer usually recognizes this condition; the computer usually does not. This may explain the tendency for automatically-designed lenses to have some thick components.

What mathematical or programming "tricks" have been found to speed up convergence? For example, what minimum number of rays must be traced
at each iteration, and should this number be increased as the optimum is approached?

Many mathematical and programming "tricks" were discussed, most of which were reputed to speed up the convergence process and produce superior designs. In most cases, though, the "tricks" amounted to the favorite design program being described by each speaker. Several--Feder (3), Grey (4), Pegis (6), Yuta (9), Meiron (12), Unvala (20), Glatzel (28), and others--did examine in detail (too much to be described here) the mathematical intricacies which make a program powerful (or weak?). The late Charles Lehman (24), especially, presented an elegant, if terse, paper concerning the treatment of singularities which occur occasionally in the design process. This simple technique, contended Lehman, may speed up the convergence process tenfold or more in instances where singularities appear frequently for one reason or another. Put simply, the troublesome variable is simply ignored until such time as it is no longer troublesome.

According to Hopkins (14), the user should know the workings of the lens design program in minute detail in order to employ it efficiently. On the other hand, Kingslake (15) feels that one need not know all about what is going on internally to get good results, just as one need not understand the internal workings of a desk calculator to make good use of it.

Some designers (including the writers) are beginning to conclude that many more rays must be traced when cascaded polynomial aspherics are used as design variables. In such cases, field and pupil aberrations may not be smooth functions, and brute force techniques (many rays in the pupil and several field points) may be necessary to preserve satisfactory performance at all field points, from all pupil zones.
Is the use of big machines worthwhile? Are the results enough better than those obtained by a good designer on simple machines to pay the cost?

Hopkins (14) raised some skepticism: "We seem to be lacking some fundamental techniques used by old-time designers. When we decide we must go to double precision arithmetic in order to be able to solve our 40 sets of equations, I recall that I designed good lenses on desk calculators using six significant figures and never solved a set of simultaneous equations. Are we missing something obvious?" (One answer to this query may be in the LASL program's method of using a different small subset of variables at each iteration from the total array of variables.) Hopkins complained, justifiably, that the slow turnaround time connected with most large computer facilities somewhat defeats the purpose. In addition, he stated that no more than one run per day is usually possible in a large, closed-shop computer facility. This makes it hard for the designer to maintain the continuous concentration necessary to guide the development of a prototype into a well-corrected system. He prefers an accessible, in-house machine of limited capabilities (such as the IBM 7074), combined with an automatic program (such as ORDEALS), which allows the designer to put to use his experience and knowledge.

Though Hopkins and others sometimes find themselves frustrated when dealing through large, closed-shop computer installations, some designers relish the opportunity to use a large computer, no matter how adverse the conditions. Many designers maintain that a medium sized or large computer, using a powerful automatic program, can produce better systems with little guidance from the designer. The LASL program used on the IBM 7094 is a good example. (Some even expressed the opinion that having to make several runs per day would shortly drive most designers to hysteria!)
The Perkin-Elmer optical design group seems to have arrived at a good compromise: their SDS 9300 operates on a first-come, first-served, open-shop basis. The program itself, which was devised by Dr. Josef Meiron, allows the operator virtually complete control over the optimization process. In this situation, it is impossible for nonlinearity to develop, resulting in the design's blowing up, and wasting several iterations in the process. This often occurs in a closed-shop operation, where the designer sits outside, helpless to do anything. An on-line cathode ray tube display may be used at any time to evaluate the performance of the design presently in storage.

Radkowski (35), discussing the merits of various computers used in optical design, stated that the SDS 9300 is the most economical to use, in units of "lens surfaces traced per dollar." The SDS 930, though cheaper by the hour, is somewhat more costly in terms of lens surfaces traced per dollar. The IBM 7094, though much faster than either the SDS 9300 or SDS 930, is very expensive in terms of lens surfaces per dollar. If economy, then, is to be considered as important as speed, it looks as if the Perkin-Elmer designers have arrived at a good compromise. All this, of course, assumes that Radkowski's data are sound, and that the unit "lens surfaces traced per dollar" is carefully standardized, and representative of optical systems in general. Actually, it seems that such a comparison would be difficult because the various lens design programs are not all compatible with one computer, nor is any one program adaptable to many different computers. In addition, the skill of the programmer is certainly a factor where ray tracing speed is concerned. In the writers' opinion, the SDS 9300 is most useful when many lens surfaces and rays are to be traced. For simpler systems with relatively short cycle times (say, 30 seconds), much time and
money could be wasted while the designer considers a new strategy, or makes
data changes at the console. In such a case, the smaller, less costly
(timewise) machine would no doubt be the better choice.

In summary, the lens design conference was enlightening to all who
attended. A multitude of ideas was exchanged both formally and informally,
and most delegates left the conference with the feeling that something had
been learned. Unfortunately, in the writers' opinion, several of the
speakers used the conference only as an opportunity to talk about their
current pet projects, praise their own lens design programs, etc., and made
no attempt to disseminate new knowledge to the other delegates. The
conference might have been even more profitable had these few restrained
themselves and followed more closely the guideline questions.