Adaptive Optics Development: A 30-Year Personal Perspective

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Foreword

I've been involved with the development of adaptive optics almost from the beginning, and because of that I've been asked to talk about what happened in the development of that field, as I perceived it. In response to that request I have prepared the following—which is an un researched and necessarily somewhat subjective account of what I recall about the development of adaptive optics over the last 30+ years, as seen by an active and deeply involved participant.

The work I've been involved with was almost entirely funded by the DoD so it is naturally to be expected that most all of what I'll have to say will have military interests underlying the work—but in fact it couldn't be very different as almost all of the work done to establish the field of adaptive optics was funded with military objectives in mind.

Well before there was a field called adaptive optics I published several papers that addressed the matter of what the effects of having to propagate through atmospheric turbulence would be on the performance of various types of optical systems. As a consequence I got drawn into the field of adaptive optics development quite early, both as an analyst and as an advisor to several of the military organizations that were pushing the development of that field. I got to regularly attend government reviews of hardware development programs at various contractors, and then got to go home and analyze technical/phenomenology questions that came up during the reviews. As a consequence I've had a great seat from which to watch the story unfold, and now I'm going to try to put words to a bit of that story.

I think I was involved in or at least had an overview look at most all of the work that led to the development of the field of adaptive optics, but I'm sure not all of it is still in my memory banks—which is my way of saying that just because I don't mention it doesn't mean it didn't happen. Some of what is missing in this presentation I just don't recall any more, and other parts are missing because they were not, in my opinion, part of the main stream (*i.e.* the critical path) in the development of the field.

In what follows I try to trace the field's development up to about a decade ago.

The Pre History

What I call the "pre history" of adaptive optics is that period when all the concepts that were being considered required that the reference source be a point source (or at least that it be too small to be resolved by the full aperture of the adaptive optics system) and in some cases required that the reference source be monochromatic.

My first encounter with what I only recently have come to recognize was a form of AO was at a 1967 (or 1968) summer workshop at Williams College sponsored by Henry Plotkin of NASA Goddard. At this workshop the interest was in laser communications from a satellite (presumably a deep space scientific satellite) to a ground station. At this workshop I heard a concept put forward for

Laser Weapons Technology II, William E. Thompson, Paul H. Merritt, Editors,
Proceedings of SPIE Vol. 4376 (2001) © 2001 SPIE · 0277-786X/01/$15.00

a laser communications receiver that was based on the use of many separate optical heterodyne receivers; the concept utilized rf adaptive array techniques to coherently combine the signals from the multiplicity of small receivers. The adaptive coherent combining capability was to be capable of rapidly varying changes, capable of changing quickly enough to be able to keep up with the time varying effects of optical turbulence.

I don't recall much of the combining details for this concept other than that it seemed quite straight forward to the rf-types who were there. To the best of my knowledge this multiple optical heterodyne receiver concept never was seriously pursued.

This multiplicity of receivers with coherent combining of the signals they received, I now recognize as a multiple "sub aperture" adaptive optics system.

In the late sixties I was aware of some work in the group under Richard Gudmundsen at North American Aviation, Autonetics Division (which company I worked for at the time) to develop an adaptive optics system which was to transmit 10.6 μm laser beams from ten small (sub)apertures [each sufficiently small that atmospheric turbulence would not distort the beam from such a (sub)aperture—in effect having a diameter significantly smaller than what I had called in a 1966 publication "the effective coherence diameter" and denoted by r_0], all ten of the beams directed at the same target position, with each of the ten beams phase shifted with respect to each other in such a way that they would combine coherently at the target position. A 10.6 μm laser point source (or possibly a glint point or corner reflector to return some of the incident laser radiation) was assumed to be located at the target position and the heterodyne-detection receipt of this signal at each of the ten transmitter apertures would provide the required information to control the phase shifts placed on each of the ten transmitted laser beams. As I (somewhat vaguely) recall it, the phase shifts were imparted to the transmitted beams by an ultrasonic Bragg diffraction cell—with the phase shift being introduced via the electronic signal driving the Bragg cell. It is my recollection that such a device was assembled and demonstrated, but I don't believe that anything much beyond that ever came of the concept.

At the same time, *i.e.* the late sixties, there was a great deal of work going on at the Hughes Research Laboratories on the multi dither concept for adaptive optics, which I and many others consider to be the first steps in the modern vein of adaptive optics—the deformable mirror based adaptive optics system. I have heard Tom O'Meara, who was the lead for a great deal of that work, referred to as the "father of adaptive optics" —a sobriquet I think he deserves. I believe that it was the multi dither adaptive optics work of Tom and his associates that got serious interest and government funding into this area.

For multi dither adaptive optics laser beam transmission the original version of the concept assumed that there was a glint point or similar feature on the target, a feature which would cause the strength of the laser radiation that was scattered from the target back towards the transmitter to be proportional to the laser power density incident on the target at this feature. By dithering each actuator of the deformable mirror at some high frequency—a different frequency for each actuator—the strength of the backscattered signal was caused to fluctuate at each dither frequency, with the amplitude and phase of each of the frequency components of the signal fluctuations indicating what adjustment of the average position of the corresponding actuator needed to be made to maximize the laser power density at the target feature. In this type of system the strength of the back scattered signal would be monitored by a separate ("photon bucket" class) receiver positioned along side the adaptive optics laser transmitter's aperture.

In a variation of this concept, where a bright point source (not necessarily monochromatic, but necessarily of a different wave length than the laser) is assumed to exist at the target position, the image of that point source—as formed by the multi dither adaptive optics system—would be passed through a pin-hole and the fluctuations of the strength of the part of the signal that got through the pin-hole would be monitored to develop the control information for each of the actuators.

Both of these forms of multi dither adaptive optics systems were experimentally demonstrated.

These multi dither systems had to do with a single point in the system's field-of-view and so had no general relevance to the problem of achieving diffraction limited imaging through atmospheric turbulence of an extended object pattern. Their potential utility was restricted to transmission of a diffraction limited laser beam—which while quite interesting to DoD sponsors, did not address imaging.

In a very ingenious departure from the above described approach to the development of the information needed to control the displacement of the mid point of each actuator's dither cycle in a multi dither system Muller and Buffington in the early seventies demonstrated a method of developing the needed information using what I call the "integral-I-squared" signal. They showed that the integral over the image plane of the square of the intensity of the image formed by the adaptive optics imaging system would be maximized if the deformable mirror's shape was such as to exactly cancel the path length variations introduced by atmospheric turbulence.[1] Thus by observing how the integral-I-squared signal's strength varied at each of the dither frequencies they could determine the corrections to be applied to each actuator. (What Muller and Buffington actually demonstrated was directly related to but a slight departure from this concept.)

All these multi dither approaches to adaptive optics system design (except that based on the integral-I-squared value) suffered from the need to assume a point feature—a glint point type backscatter source or a point source light source—on the target in order for the actuator control signals to be developed. All of the approaches, including the last, suffered from a limitation in the number of actuators that could be controlled due to practical limitations on the maximum dither frequency that the hardware could accommodate. And all of them suffered from a potential paucity of signal strength—in effect a signal-to-noise ratio problem—because not only did the amplitude of the dithers have to be rather small in order for the system to impart proper wave front correction to the transmitted beam, but also for each actuator the portion of the signal affected was only a small fraction of the total signal. There were other problems with (or limitations to) the performance of a multi dither adaptive optics system, and it was with some relief (as indicated by the clean sweep of the field that more modern adaptive optics systems have made) that the work in adaptive optics switched to what I call "integration of difference-measurements" (IoDM) based systems.

Integration of Difference-Measurements Based Systems

The integration of difference-measurements (IoDM) concept is the key to modern adaptive optics! This concept is based on the measurement/estimation of the value of the difference between the turbulence induced phase perturbation at pairs of adjacent positions—positions in a set of positions that are close to each other, the set of positions providing a fairly dense coverage of the aperture of the adaptive optics system—and the combining of these difference measurements to form an estimate of the phase at each of the positions.

The measurements are only nominally of the difference of the phases at pairs of adjacent positions. Actually the measurements are either

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[1] **Based on what they told me I believe that the proof of this property of the integral-I-squared value was originally developed by Freeman Dyson during a JASON summer study.**

In the case of a shearing interferometer —— of the average over a sub aperture sized region, of the signal associated with the interference between the optical fields at pairs of points, pairs for which the separation is small—with the average of the interference signals being attributed, after suitable scaling, to the difference of the phase at a pair of positions located at the mid-points of opposite edges of the sub aperture, or

In the case of a Hartmann sensor —— of the gradient of the phase (actually a measurement of the wave front tilt) averaged over a sub aperture sized region—with the average gradient being attributed, after suitable scaling, to a difference of average phases; the average of the phase at two adjacent corners positions of the sub aperture minus the average of the phase at the other two corner positions of the sub aperture.

Using what has come to be called a "reconstructor" these phase-difference related measurement values are combined (pieced together ??) to form an estimate of the turbulence distorted phase pattern over the entire aperture of the adaptive optics system.

Because the optical measurements are only of phase differences over a separation no greater than the length of a side of a sub aperture, the angular size of the reference source can be quite large, generally much greater than the size allowed by a criteria associated with the resolution (λ/D) that goes with the size of the full aperture of the adaptive optics system. Certainly the reference source need not be a point source (which the previous/prehistoric concepts had required it to be). With the integration of difference-measurements approach there is essentially no penalty for reference source size so long as that size is less than the diffraction limit associated with the sub aperture (λ/d), and the penalty for the size being greater than that is only in terms of achievable measurement precision (and possibly also in terms of the various dynamic range and measurement accuracy considerations introduced into the design of the wave front distortion sensor). With modern wave front distortion sensors, such as might be used with a solar telescope, the reference source does not even have to be of finite size; it only has to have a pattern with sufficient high-spatial-frequency content.

I first became acquainted with this IoDM approach to wave front distortion sensing in about 1973, when I started working with Don Hanson and Ray Urtz at RADC. I had a small contract with RADC to provide technical support to them in their management of a program to develop an adaptive optics imaging system. The application for this adaptive optics imaging system was to obtain high resolution images of a satellite using a ground based telescope. This effort was funded by ARPA, with Jim Justice as the point of contact at ARPA. This program apparently had its origin in a concept for an IoDM adaptive optics system proposed by John Hardy at the Itek Corp. This concept was presented to ARPA a bit before the time of my involvement with the program so I only know about this through hearsay. It is my impression that the wave front sensor concept proposed by Itek, which was a shearing interferometer—one which passed the light through a series of three diffraction gratings (the first and last stationary and the middle one oscillating) thereby achieving the required shear—was a concept that was originated by Jim Wyant. (By the time I had a chance to meet with the Itek people Jim had left Itek for the University of Arizona and I never got to talk to him about this.)

The initial contract with Itek produced a small scale, 21 element, adaptive optics system called RTAC (Real Time Adaptive Correction), that used a shearing interferometer, an analog wave front reconstructor, and a deformable mirror. RTAC was able to correct for simulated atmospheric turbulence in the laboratory. The RTAC was taken to an RADC optical test site where correction of real atmospheric turbulence over a 300 m slant path for a 0.5 m diameter telescope was demonstrated. This provided the confidence for DARPA to proceed with a program called Compensated Imaging, to put an adaptive optics system on a 1.6 m aperture diameter telescope on Mt. Haleakala, HI.

The program started with three one-year contracts, to Itek, Perkin Elmer, and Hughes—each of which proposed a different approach to the wave front distortion sensing concept. The three contracts constituted a competitive effort to define/refine/analyses and establish the engineering feasibility of the proposed concept—with the intention being to letting a single much larger contract to one of these three companies at the end of this effort, a contract for the fabrication of the adaptive optics system to go to the observatory on Mt. Haleakala.

The Itek shearing interferometer concept used three diffraction gratings, one of which was oscillating at a high speed, to overlay, with some misregistration (*i.e.* with some shear), two versions of the pupil function—with one version of the pupil function being frequency shifted with respect to the other version. The frequency shift was induced by the movement of the oscillating grating. At each point in the resulting overlay of the two versions of the pupil function the intensity would be oscillating at this frequency difference, with the phase of the oscillation at each point indicating the wave front distortion phase difference between a corresponding pair of pupil points—a pair of points that were plus and minus one-half of the shear distance from the point where the oscillation of the interference was. The measurement was of the average, over the extent of a sub aperture, of this interference. Each sub aperture's signal was detected by a photomultiplier.

The Perkin Elmer concept was that of what we now call a Hartmann wave front distortion sensor. This was before the days of lenslett arrays and the Perkin Elmer concept used a multi faceted reflective prism to divide the pupil into a set of widely separated beams (one for each sub aperture). Each beam was imaged onto an intensified silicon quadrant detector, where the wave front tilt for that beam/sub aperture was measured—with the phase difference from one side of the sub aperture to the other being inferred from the measured tilt.

The Hughes concept was based on the multi dither approach with (as I recall it) an assumption being made that the signal (reflected sun light) from the space object would be dominated by a glint point, so measurement of the amount of light passing through a λ/D sized pin-hole in the image plane could be used to generate the multi dither control signals.

As the work progressed on this first (competitive) phase of the program it became apparent that a first consideration in selecting between these different approaches had to be system sensitivity—how faint an object could the system use as its reference source and still provide an accurate estimate of the distorted wave front.[2] As a result of studying their system's sensitivity the Itek and the Hughes systems were changed very significantly.

Itek found that their three grating shearing interferometer had only a 15% optical through-put, at best—which put them at a significant disadvantage compared to the Hartmann sensor based concept. To get past this difficulty Richard Hutchin[3] invented a very simple wave front sensor which had as good a potential through put as the Hartmann sensor. In this concept the collected light from the full aperture was brought to a focus and a rotating grating chopper was placed in the focal plane—placed at a position such that the grating period was of the order of the size of the image of the reference source. The light that was not blocked by the chopper passed through and expanded—at which point each region of the expanded beam corresponded to a region of the pupil so this light could be divided into sub aperture portions. A separate photomultiplier was used to collect/detect the light for each sub aperture. (To maximize utilization of the collected photons in the reference signal the rotating grating was reflective, so the light blocked could be directed to a second set of photo multipliers.) The photomultiplier signals were all chopped at the same

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[2] **When the program first started there had been some hope that a near by bright star could be used as the reference source, but when I developed anisoplanatism results—theory and estimates of the size of the isoplanatic patch size, θ_0 —it became clear that the adaptive optics system's reference source would have to be the object being imaged.**

[3] **At that time he spelled his name Hudgin, though now he uses the spelling Hutchin.**

frequency. Using arguments developed from diffraction theory (an argument in which the grating not only diffracted the light at an angle dependent on the grating's period, but also Doppler shifted the diffracted light) Hutchin showed that the photomultiplier signals would be phase shifted by an amount corresponding to the difference of the phase of the reference source at points in the sub aperture that were separated (*i.e.* sheared) by an amount determined by the gratings diffraction angle. With this revised shearing interferometer design Itek was a viable contender.

The Hughes analysis of their system's sensitivity convinced them that they could not, as I would put it, squeeze all the control system information that they needed through a single detector—the single pin-hole/single detector that their original multi dither concept utilized. To get around this limitation they developed a multiple pin-hole/multiple detector arrangement. The light reflected from each deformable mirror actuator was, in part, combined with the light from two adjacent actuators, and then focused and passed through a pin-hole that was unique to that set of three actuators. Some of the light from each of these three actuators also was combined with light from other, also adjacent but different actuators, and then focused and passed through a different pin-hole and detector. In this arrangement all of the actuators contributed light to several combinations of three actuators, with a separate pin-hole and detector for each such combination of three actuators. Now only (at most) three dither frequencies were required to allow unambiguous interpretation of the detector signals. Each component of the detector signal gave information about the required displacement of one of the three actuators relative to the other two. Then since each actuator contributed to several combinations of three, a linked network of information was established about how all the actuators had to be displaced relative to each other. A reconstructor was formulated for combining all of these relative displacements bits of information—to relate all of the detectors' two dither frequency components' phase shifts to the required actuator displacements.[4]

With this arrangement the signal measurement precision was greatly improved because the effect of the dither of any one actuator, as manifested in a detector's output signal, was not "competing" with the shot noise associated with the light from the many remote actuators—only with the shot noise associated with the light from three actuators. Also, with the image placed on the pin-hole being from only three actuators, the diffraction limit was of the order of λ/d rather than λ/D (d denoting the actuator region's size while D denotes the system's aperture's diameter), so now the reference source could be correspondingly larger. The glint region could be quite large; it didn't have to be so small as to be un resolvable by the adaptive optics system.

By the end of this contract effort it was decided that it looked as though a system could have sufficient sensitivity to be able to work with the reflected/scattered sunlight from a satellite. (One of the things that was learned/recognized in this work was that simply being larger and thus supplying more light did not make a satellite easier to work with. The relevant criteria were whether the satellite's angular size was at least as large as the sub aperture's diffraction angle, λ/d, and what was the relevant value of the BRDF.) However it was also concluded that hardware feasibility, though it seemed possible/plausible, had not been demonstrated even at a fairly fundamental level, and that it should be demonstrated in a next phase—before proceeding with a commitment to a full system. It was decided to continue the competition with contracts to Itek and Perkin-Elmer, dropping Hughes. For Itek the objectives of this second phase effort were to

- demonstrate that the rotating grating shearing interferometer did indeed measure wave front distortion, and

[4] **It is interesting to note that in a certain sense this multiple pin-hole/detector version of multi dither could be considered to be an arrangement in which each set of three actuators was tracking its image of the reference source, measuring the associated tilt—just as in a Hartmann sensor. The multi dither actuator displacements moved the image relative to the pin-hole, with the detector signal indicating which actuator displacement increased/decreased the centering of the image on the pin-hole.**

- demonstrate their approach to the deformable mirror wave front distortion corrector.

For Perkin Elmer the objectives of this second phase effort were to

- demonstrate that the Hartmann sensor in the form they conceptualized it was mechanically stable—that the null positions of the various sub apertures would not shift with respect to each other when the sensor was rotated by 90°, as it would be when mounted on the telescope [*n.b.* It had to be mounted on the telescope since the target telescope did not provide a hollow axis capability, which (if it had had such) would have allowed the sensor to be operated off the telescope.], and

- demonstrate their approach to the deformable mirror wave front distortion corrector.

Itek demonstrated a deformable mirror, what they called an MPM (a monolithic piezoelectric mirror) the device concept having been formulated and the device developed by Julius Feinlieb. This device is based on a single relatively thick piezoelectric disk with a pattern of electrodes deposited on one face (the "front face") of the disk—the pattern of electrodes corresponding to the pattern of actuators—and with a single electrode deposited over all of the other face (the "back face") of the disk. At the center of each of the actuator electrodes a hole was drilled through the disk with an insulated wire run through each hole, the wire making contact with the actuator electrode segment. A thin glass cover was placed over the front face of the disk, and was given a reflective coating. With the back face electrode grounded different voltages applied to the different wires would result in increases or decreases of the thickness of the disk under each actuator region, raising and lowering the surface of the front face mirror—providing the deformable mirror action. The deformable mirror had the defects of requiring in excess of ± 1 kV to achieve the desired mirror surface displacements (which resulted in the connector assembly being very much larger than the piezoelectric element itself, and which made the driver circuitry quite substantial—almost 200, kV-swing, kHz-bandwidth drivers), and of being subject to a certain degree of actuator "print-through" —the inability to uniformly raise or lower the entire mirror surface with the same displacement between the actuator centers as at the actuator centers. But the MPM deformable mirror worked well enough.

Itek demonstrated that their shearing interferometer wave front sensor was actually able to measure wave front distortion by working with a version of the wave front sensor that consisted of a linear strip of sub apertures. An almost collimated beam was input to this wave front sensor and the out put data was used to calculate the curvature of the input wave front. It was shown that the wave front sensor could correctly determine the curvature. It was not considered necessary to test the invariance under 90° rotation of the alignment of the different sub apertures of the wave front sensor since the design inherently insured alignment through the common chopping of the focused light from all the sub apertures.

The Perkin Elmer approach to the deformable mirror was based on electrostatic deflection of a membrane mirror. The membrane was given a metallic coating to make it reflective and to make it an electrode. The membrane was mounted on an optically flat ring, attached to the ring in a process that resulted in the membrane being highly tensioned. By careful selection of material for the membrane the resonant frequency of the membrane was made to be as high as possible. Immediately below the membrane, with a small gap between it and the membrane was a surface on which an electrode pattern was deposited—the electrode pattern being the desired actuator pattern. By application of relatively small voltages (small compared to those that would be required by the MPM) the surface of the membrane would be deformed. In tests the membrane mirror proved to have significant residual distortion, but the basic principle of the membrane mirror as a deformable mirror for an adaptive optics system was established.

At the completion of this phase of the work it was decided to go ahead with the fabrication of an adaptive optics system with of the order of 200 actuators to be mounted on the 1.6 m diameter telescope on Mt. Haleakala, HI. The contract for this was awarded to Itek, with John Hardy as the Project Engineer. This instrument was built and installed on the telescope where it was used for adaptive optics space object imaging for almost two decades.

At the time of the completion of this instrument a number of important technology advances, beyond the instrument itself, were to be scored to the effort. The first was the establishment of the principle of adaptive optics without a point source (monochromatic) reference source. It was now clear that there was no magic involved. In fact we all felt ourselves to be on rather solid ground regarding the basic IoDM technology and the associated concept of a wave front reconstructor. The second was that we felt comfortable with both the shearing interferometer concept and the Hartmann sensor concept (even though the Hartmann sensor concept had lost the competition). The third was the development of a set of analytic tools for the analysis of the expected performance and for the selection of an adaptive optics system's design parameters. We understood the isoplanatic limits. We knew how to analyze/evaluate what we call fitting error—the failure of the deformable mirror in "interpolating" between actuators to exactly match the pattern of the wave front distortion. We knew how to analyze/evaluate the effect of system servo bandwidth—the servo lag error. We knew how to relate signal strength to the rms error in the measured phase difference values, and understood (and could evaluate) the noise gain in the reconstruction process—the noise gain being the factor that relates the mean square phase difference measurement error to the mean square error in the reconstructed wave front.

With the completion of this effort a number of studies were undertaken to develop component technology. Amongst these was an effort to develop a different form of deformable mirror, one with lower required voltages, for which there was an absence of the hysteresis of a piezoelectric material, and which could accommodate a very large number of actuators. Itek received a contract for this work based on a concept of developing small diameter stacks of many layers of electrostrictive material, with conductive electrodes between the layers and with the wiring to these conductive layers being "staggered" so that there was no net voltage difference from one end of the stack to the other end but there was the same voltage across each electrostrictive layer. This technology was successful and with the latter demise of Itek provided the foundation for Xienetics, where we have been going in recent years for deformable mirrors.

A contract was given to Perkin Elmer for further work on the membrane mirror. In the coarse of this work Bob Hufnagel showed that because of the non linearities in the displacement equation for the membrane mirror and the relation of tension, the number of actuators, and the desired resonant frequency lower limit, the maximum possible displacement was limited. For more than a certain amount of displacement the nominally required voltage was too large and contact between the membrane and the underlying electrode plate would occur. He showed that membrane mirrors capable of the desired frequency, number of actuators, and displacement could not be designed.[5]

A contract was given to Adaptive Optics Associates (AOA) for work on development of an alternative to the hundreds of photo multipliers that the Itek approach had needed and for a different approach to the implementation of a Hartmann sensor. For the photo detectors AOA had Reticon develop a very high read out speed, 64-by-64 detector array, CID detector. It was very noisy but it was fast. To get around the noise problem AOA developed a stacked image intensifier unit with fiber optic coupling between the units and to the CID detector array. To provide high sensor speed the

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[5] **In reviewing this paper Don Hanson has told me that he has no recollection of such a contract (and if there was such a contract it almost certainly would have gone through him), but I remember the work quite distinctly. Very possibly there is some intermediate explanation of where this work fits into the whole set of DoD funded efforts conducted at that time.**

intensifier units utilized very fast, and therefore rather low gain, phosphors. To accommodate the low phosphor gain the intensifier stack had three or four intensifier units. This sort of unit was successful in the lab but I don't believe it was ever used in a system in the field.

In AOA's work to demonstrate the Hartmann sensor they developed a technique for production of what we know as lenslett arrays. This, in conjunction with the use of two-dimensional detector arrays made the implementation of a Hartmann sensor a straight forward matter. Julius Feinlieb was, I believe, responsible for the development of the technique for the production of the lenslett arrays.

In the late 70's work was started at Lincoln Laboratory, under Daryl Greenwood and Chuck Primmerman, to field an adaptive optics system to be operated on Mt. Haleakala, HI with an about 0.5 m diameter aperture, to project a diffraction limited laser beam onto a scoring board mounted on a sounding rocket that would be launched from elsewhere in Hawaii (from Barking Sands). The adaptive optics system used as its reference a point source located very near the front end of the rocket. A major objective of the experiment was to test our understanding of the matter of anisoplanatism. For this purpose the scoring board was mounted far enough back of the beacon source that if the laser beam was directed right towards the apparent direction to the reference source the laser beam would strike a position on the scoring board—but in addition the scoring board was long enough that the laser beam could be directed significantly behind the apparent direction to beacon source, far enough behind so that one would expect to encounter anisoplanatism problems, with the laser beam none the less striking the scoring board. The experiment was successful in demonstrating that the anticipated anisoplanatism effect was essentially as expected—and this had implications leading to the research effort discussed in the next section.

For this experiment the Lincoln Laboratory adaptive optics system was mounted on an optical table, not on the telescope, and it was seen what a tremendous advantage this provided.

In the course of the development of this system Lincoln Laboratory developed a high read out speed, very low noise CCD detector array. The noise level was low enough that no intensifiers were required, which is where we are today—with Lincoln Laboratory as the non vendor of choice.

Also in this work Lincoln Laboratory decided that since the reconstructor process was a linear process the reconstructor could be implemented as a matrix multiply—and that with the then available semiconductor components, particularly DSP's, the matrix multiplication could be accomplished in parallel by specially designed hardware.

The reconstructor implemented by Itek for the hardware they had delivered had used an analog computer to accomplish the reconstruction. (That analog computer reconstructor utilized a square array of resistors—a lattice whose lattice elements were resistors—and constant current amplifiers, one supplying current to each node of the lattice, *i.e.* each point of the lattice where four resistors came together.) The constant current amplifiers were driven by values calculated from the "divergence" of measured phase difference values, and the resulting voltage at each lattice node was taken as the reconstructed phase at that point.

The development of a digital reconstructor by Lincoln Laboratory represented a great improvement over the analog computer approach utilized by Itek.

Laser Guide Star

By the end of the 70's the basics of adaptive optics were considered to be well in hand and there was interest in DoD in the possibility of using adaptive optics in transmitting a laser beam from the ground to a satellite. However it was recognized that there was a point ahead problem, that the laser beam would have to be transmitted in a direction that leads the apparent position of the satellite by $2v/c$, where v is the component of the satellite's velocity that is perpendicular to the line of sight and c is the speed of light. For a near earth orbit satellite this point ahead angle is of the order of 50 μrad. This is such a large angle, so much bigger than the isoplanatic angle, θ_0, that light from the satellite did not seem to be usable as the adaptive optics reference source. A scrambling to find some way to get past this barrier was initiated. At first we could find no satisfactory approach. I studied the possibility of using extrapolation of wave front distortion data using the fact that turbulence at different altitudes moved across the beam at speeds proportional to the altitudes. But I was able to show that that wouldn't provide the needed adaptive optics control information for the point ahead direction.

Then Richard Hutchin invented a truly crazy idea, a spin-off of which is the origin of the laser guide star concept. Rich suggested transmitting—after reflection from the deformable mirror—a separate short-pulse laser-beam from each sub aperture, each beam nominally directed in the point ahead direction. (The beams were to be only nominally all sent in the same direction. They were to be parallel before reflection off the deformable mirror, but after reflection from the deformable mirror they would have slightly different directions.) He argued that if the combination of the tilts imparted to the beams by the deformable mirror and then by the atmosphere were such that all the beams arrived at the top of the atmosphere heading in the same direction then the deformable mirror setting was the correct one for compensating for the atmospheric turbulence effects. I'm not sure I agree with that supposition, and raised a question about that back then, a question which has never been resolved. But that's not what mattered.

What mattered is one aspect of Rich's idea for determining the direction of the laser beams at the top of the atmosphere. He proposed that detection of the atmospheric back scatter of some of the laser radiation would allow the direction of the beams to be determined. This was to be accomplished by imaging the back scatter on a reticle. The image would be moving across the reticle as the distance to the pulse changed and the signal getting through the reticle would be chopped at a frequency that was related to the tilt of the beam. I considered this to be a preposterous idea, one which I was able to show required a nearly infinite signal-to-noise ratio for any meaningful tilt measurement precision to be achieved. But what was important was the introduction of the concept of using a pulsed laser and observing the atmospheric back scatter. Itek received a small contract to do something with this concept, but the work never went anywhere. However, in parallel with the work on this concept Rich came up with a second, considerably more reasonable idea for using atmospheric back scatter of a pulsed laser—an idea that eventually was referred to as the S-method.

The S-method (the "S" stands for "shear") required that two full aperture, nominally collimated (*i.e.* collimated except for deformable mirror induced perturbations), pulsed laser beams be transmitted almost in the direction of interest, *i.e.* in the point ahead direction. The two beams have a slight inclination relative to each other with the average of the two directions being the direction of interest. A temporally gated imaging system, using the adaptive optics system's aperture (and its deformable mirror for wave front distortion correction) would collect the back scatter from some particular range, forming an image of the interference between the two laser beams at that altitude. From the interference between the two beams at any point in the image, by development of a set of four such images, each generated with a different multiple of 90° phase shift between the two beams at transmission, it would be possible to determine the difference between the turbulence induced phase shifts to be associated with the pair of points on the aperture that contributed to the point in question on the image. With a proper choice of the inclination between the two beams and the back scatter altitude the distance between the pair of points on the aperture could be set to a convenient length, nominally of the order of r_0.

This idea directly addressed the point ahead problem and had the virtue of not looking preposterous, and Itek got a contract to study/refine the concept. There are reasons to think that this approach to wave front distortion sensing has certain fundamental limitations that limit its potential to a prohibitive extent, and to the best of my knowledge no closed loop system has operated using the S-method. But now the idea of exploiting atmospheric back scatter of a pulsed laser transmitted in the direction of interest was in the air, and another concept soon showed up.

Julius Feinlieb, Adaptive Optics Associates, proposed focusing a pulsed laser beam at some high altitude and using a gated Hartmann sensor to observe the back scatter from the region where the beam was focused. It is my impression/suspicion that the idea of using pulsed laser back scatter for wave front distortion sensing leaked from Itek to Adaptive Optics Associates, probably through the Socratic method of "teaching by questioning", though Julius has assured me that the idea occured to him when he looked at the back scatter of a visible laser beam being transmitted up through the atmosphere.

Julius's idea was initially dismissed because the rays of the focused beam, and the rays back to the wave front sensor from the focal spot, would be tilted even more than the point ahead angle—so it seemed unlikely that the measured wave front distortion would be any more relevant to the point ahead direction than would wave front distortion measurements made using the satellite as the reference source. But Julius kept coming back with his idea. Finally, to get him to stop I did an analysis of the concept and found that the effect, which has since come to be known as "focus anisoplanatism" (or as "the cone effect"), limited the useful aperture diameter to about one-half of a quantity I denoted by d_0 (for the evaluation of which I gave expressions) and that the value of d_0 was interestingly large—in the one meter range for visible wave lengths and backscatter altitudes as high as about $H = 15$ or 20 km. This changed the situation dramatically.

To test this result I, *i.e.* the Optical Sciences Company, received a small contract to work with Bob Fugate at the Air Force Weapons Laboratory to demonstrate this approach, which became known as the "A-method" (the "A" standing for "astral", the back scatter from the focused spot being an artificial guide star—an astral reference). Adaptive Optics Associates received a considerably more substantial contract to design and build a rather large wave front sensor system which would be able to operate with multiple laser guide stars simultaneously. The experiment I designed did not attempt to achieve closed loop adaptive optics operation. It settled for merely demonstrating that wave front distortion measurements made with the laser transmitted in the direction of a bright star would be comparable to the measurements made using the star itself as the reference source. (Because we didn't have a telescope on a mount available to us for the experiment Bob and I used Polaris as the reference star—since that star's motion is so slight that it could be tracked with a jury rigged tracking system. We used an off-axis parabola on an optical table and looking out through a steering flat, both of which happened to be available, as our telescope.) The experiment was a success; the mean square difference between the wave front distortion measured using the laser back scatter as the reference source and that measured using Polaris as the reference source was very nearly equal to what theory predicted.

Right from the first suggestion of the use of atmospheric back scatter of a pulsed laser beam the idea of doing that was classified. Once we succeeded with our experiment the idea was given special access treatment and there were no publications. The experiment demonstrating the A-method was assembled and conducted in the second half of 1981 and early 1982, when the results were presented to the cleared community. However nothing was published in the open literature till 1991 when the concept's classification was removed, by which time Bob Fugate had long since pushed the technology along to the point where he had a closed loop adaptive optics system using a laser guide star (A-method) adaptive optics system operating on a 1.5 m aperture diameter telescope.

In the summer of 1982 the experimental results were briefed to the JASON summer study. At that time Will Happer, a member of the JASON group suggested that since a higher back scatter altitude would allow a larger diameter aperture to be properly corrected we ought to consider using a pulsed laser tuned to the sodium D line and take our back scatter from the resonant scattering by the mesospheric sodium layer at 90 km altitude. This idea was taken up with considerable interest and has been pursued ever since. The problem here is that of getting a suitable laser and it is only recently that LLNL has had such a laser working at Lick Observatory—but that's getting too close to current times, which period I do not intend to cover here. It's not history, its too current.

Declassification

I'll close with a few words about the declassification of the A-method concept. In about 1990 I was contacted by people at the National Science Foundation asking if I would participate in a review of some proposals from academia for funding to develop the laser guide star concept that had been described in the 1985 publication by Foy and Labeyrie.[6] I said that I would be willing to help with the review but that there was a person at the Air Force Weapons Laboratory that they should contact—that person being the one with oversight of laser guide star classification (though I could not so identify him to them at that time)—and tell him about NSF's plans. This seems to have led to acknowledgment by the DoD of the fact that the cat was out of the bag and that no useful purpose was being served by the classification of the laser guide star A-method. This is what allowed the 1991 publication of those results that had been generated almost a decade earlier.

This is about as far as I feel it is appropriate for me to carry this recounting of the history of the development of adaptive optics, partly because to go farther would get into things it might not be appropriate for me to present here, and partly because the activity in the field has expanded beyond what I am able to stay aware of. I would like to close with the note that not only is the development and application of adaptive optics a significant activity of the DoD at this time, but also there is a rather substantial adaptive optics development effort in the astronomical community. I would characterize that astronomical activity by noting that where once the Hubble space telescope was seen as the key to high angular resolution astronomical imaging, now with the recognition of

- the potential of adaptive optics to allow ground based diffraction limited imaging,

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[6] **In regard to the 1985 Foy and Labeyrie publication I must say that I find it curious/surprising that those two authors came up with the full panoply of laser guide star techniques as it was then understood by the people with whom I had been interacting—no more and no less, with all of the understanding and none of the initial doubts which we had worked to resolve.**

They had come up with the idea of using atmospheric back scatter of a focused pulsed laser beam as a suitable reference source for a Hartmann sensor and had felt confident enough about that idea's potential to publish it without any analysis of the so called cone (or focus anisoplanatism) effect. This seems surprising to me since it had not been obvious to us, at least not before we had completed our analysis of the matter, that that effect would allow any useful degree of wave front correction to be achieved.

Not only had they come up with that idea but in addition, though they had no assessment of how severe the cone effect might be they had none-the-less recognized the need for the use of a higher back scatter altitude, they had also come up with the idea of using resonant scatter from the mesospheric sodium layer, just as we had done; *what a coincidence.*

Being of a suspicious nature and generally unwilling to credit multiple spontaneous occurences of such insight, and having been perturbed by a certain gentleman's remarks at the time we briefed him (in 1982) on our experimental results—perturbed by his remarks about the potential importance to astronomy of our results—I can not dispel the thought that the Foy and Labeyrie paper was prompted by insights passed on by that gentleman.

- the fact that when it comes to diffraction limited imaging bigger is better, and
- the feasibility of very large segmented mirror telescopes—Keck:10 m, CELT:30 m, and possibly an ELT:100 m

the thrust in high resolution imaging is towards adaptive optics with a GSMT (Giant Segmented Mirror Telescope) on the ground. Adaptive optics has brought astronomy back to earth.

Acknowledgment

I would like to thank Don Hanson for reviewing this manuscript and for contributing the discussion of the RTAC system.