USE OF LIGHT SCATTERING PHENOMENA IN ATMOSPHERIC AEROSOL MONITORING--
A SURVEY

D. Deirmendjian

A Rand Note
prepared for the
NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
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The work presented in this Note was sponsored by the National Aeronautics and Space Administration (NASA) under contract No. NAS6-2697. The objective was to undertake a critical survey of currently used and planned remote detection systems for monitoring the total amount, distribution, and nature of atmospheric particulates (aerosols) from natural and man-made sources. The survey was intended as the initial task of a larger effort to analyze and compare the various monitoring methods and to recommend ones that would be most effective and economical for operational use within such agencies as NASA and the National Oceanic and Atmospheric Administration.

The author and Principal Investigator has suspended further work on this project, because the sponsoring agency has been unable to continue support for the second and third tasks initially proposed, owing to a shortage of funds. Since the survey turned out to involve a larger effort than anticipated, and since the findings could be useful to the sponsor as well as to other agencies concerned with atmospheric particulate turbidity, its global distribution, and its short- and long-term evolution, it was thought advisable to present the findings in a Rand publication suitable for outside distribution. The contents of the present Note have been accepted for publication in Reviews of Geophysics and Space Physics, where it is anticipated that they will appear in somewhat condensed form.

Related work by the same author may be found in the following Rand publications:


R-1718-PR Far Infrared and Submillimeter Scattering: II. Attenuation by Clouds and Rain (1975)
SUMMARY

A critical survey of the literature on the use of light scattering mechanisms in the remote monitoring of atmospheric aerosols, their geographical and spatial distribution, and temporal variations was undertaken to aid in the choice of future operational systems, both ground based and air or space borne. An evaluation, mainly qualitative and subjective, of various techniques and systems is carried out. No single system is found to be adequate for operational purposes. A combination of earth surface and space borne systems, based mainly on passive techniques involving solar radiation, with active (lidar) systems to provide auxiliary or backup information is tentatively recommended.

Certain deficiencies in the published literature and weaknesses in journal editorial policies, organization of professional meetings and symposia, national science policies, and the support and administration of science and scientists are pointed out.
ACKNOWLEDGMENTS

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1. INTRODUCTION

The subject of particulate turbidity and its effects on the earth's atmosphere has been receiving considerable attention recently, judging from the volume of material published in the pertinent journals. This renewed interest in a subject hitherto considered of marginal importance in atmospheric processes may be attributed to several factors. A major impetus must have been supplied by concern for the potential hazard of interference with the natural stratosphere by the operation of large fleets of commercial supersonic (SST) aircraft, as assessed in the report [1] * of an ad hoc workshop convened in the summer of 1970.

In response to this concern and its manifestation, the Congress ordered the Department of Transportation to undertake a study of and prepare a report on the possible environmental effects of large scale SST operations. This in turn led to the creation in 1970 of a Climatic Impact Assessment Program (CIAP) and its report [2], part of which was concerned with aerosols, their monitoring and effects.

Goal oriented workshops, and related crash programs such as the CIAP, regardless of their merits, may become more common in the future. They may have certain advantages in focusing attention on particular problems but they also have certain drawbacks. Since some of the work reviewed here was performed under CIAP sponsorship and constraints it would be interesting to assess their influence, if any, on the

* Numbered references and footnotes are listed (roughly in order of appearance) at the end of this document.
quantity and quality of the published results. Although such ques-
tions are, strictly speaking, outside the purview of this survey, the
effects of science policy and management, journal editorial practices,
etc., and the pertinent literature such as Ref. [3] will be commented
on in Sect. 5.

A second impetus for the renewed interest in particulate tur-
bidity may have been supplied by certain claims, based mainly on con-
jecture and uncorroborated hypotheses, that such turbidity (possibly
man-made) may have played—and still plays—a significant regional
and global climatological role, for example, in the creation and main-
tenance of large desert areas [4]. The difficult question of the mag-
nitude of direct and indirect effects of aerosol layers on weather and
climate, mainly through radiative processes and their interaction with
and feedback through other mechanisms, has not received a clear answer
to this day despite considerable efforts in modelling and computations
(see, for example, papers by Toon and Pollack [5] and Fiocco et al. [6]).

Regardless of present uncertainties in the meteorological and envi-
ronmental significance of particulate turbidity, the implementation of
a reliable operational program for the detection, measurement, and
global and temporal monitoring of this permanent feature of our atmos-
phere may still be justified on various other grounds. For example, as
the number and importance of earth-surface monitoring satellites in-
crease, an adequate knowledge of atmospheric transmissivity to ultra-
violet, visible, and infrared radiation, all critically dependent on
turbidity as well as gaseous absorbers, becomes increasingly important.
The same applies to possible future earth-to-satellite communications
systems within the visible or near infrared spectrum.
In response to the above mentioned pressures, a number of monitoring programs have been proposed and may be under consideration for implementation. Some of these may be costly, for example, if they involve active or passive spacecraft borne sensing and data storage and transmission systems. A comparative and critical review of the pertinent literature, as attempted here, may therefore be useful in the ultimate selection of these programs and systems most likely to do an effective job at reasonable cost.

The present survey is not intended as a comprehensive history of the detection and measurement of atmospheric particulates by optical methods. Nor, for that matter, does it constitute a state-of-the-art review on their optical properties and composition, as attempted e.g. by Cadle and Grams [7] for the stratosphere. Rather, our survey will be essentially limited to monitoring methods based on light scattering and absorption by stratospheric and—to a lesser extent—tropospheric aerosols, and to the considerable recent western literature roughly covering the decade 1967 to 1976. Soviet Bloc literature on the subject is also considerable and significant, as represented, for example, by the published output of G. V. Rozenberg [8] and other well known Soviet scientists such as K. S. Shifrin, K. Ya. Kondratiev, V. S. Zuev, their associates and many others. This material deserves a separate survey which cannot be included here except for occasional references to it.

The limitation to optical scattering methods, passive or active, may be justified by our interest in large scale or global remote sensing and monitoring techniques. Other techniques, based on e.g. local sampling and changes in electrical conductivity [9], will be excluded although they should certainly serve as checks to validate the former.
Even with these restrictions, the volume of pertinent recent material on the subject turns out to be too great for all of it to be reviewed adequately here. Therefore we further restrict ourselves to a reasonable number of significant and/or representative papers for comment. We will give preference to papers published in the regular archival journals, covering the atmosphere and geophysical sciences, over project reports, conference proceedings, and preprints.

For our purposes we shall define particulate turbidity as that small component of the earth's atmosphere made up of aerosol particles larger than air molecules and atoms but generally smaller than cloud droplets, of linear dimensions between say 0.01 and 10 μm, of moderate optical thickness in the visible, say of the order of 1 or less. (For a definition of optical thickness and other terminology used here the reader is referred to standard monographs [10,11].

Light scattering techniques for the remote detection and measurement of particulate turbidity may be broadly classified into two categories: passive and active. Passive techniques employ natural sources, mainly the sun (and sometimes other stars and planets) to measure the extinction of source brightness as well as the intensity and polarization of indirectly transmitted (or reflected) light through various orders of scattering. Active techniques depend on controlled artificial sources in the form of narrow beams of incoherent, broad-band radiation (the "searchlight" method); or continuous or pulsed, very narrow, and intense beams of monochromatic energy or laser light (the "lidar" and related systems). Here one measures the intensity and/or polarization of the energy scattered in the backward or some other fixed direction.
Both techniques have certain advantages and suffer from a number of deficiencies. The older and classical passive techniques generally rely on the sun as a steady, continuous, and very intense source at infinity of incoherent, broadband radiation of known spectral energy distribution. By various filtering techniques one can isolate more-or-less narrow bands of the solar spectrum to study variations in the extinction and diffuse transmission (or reflection) of the e.m. energy as a function of wave-length, sun's zenith distance and other significant variables. Most of these techniques are limited to daylight and twilight hours and to cloudless conditions. In the diffuse case, there are other disadvantages, such as the number of variables that must be measured, the complexity of the theoretical analysis, and the extensive modelling and simplifying assumptions needed to recover the data sought by inference and comparison with a catalogue of solutions. The effect of higher (than the first) orders of scattering must often be included and mathematical inversion may be impractical. Still the sun as a source is ubiquitous, perennial and economical.

The more recently developed active techniques may be used during the night hours and, in the case of laser-radars or "lidars," have the very useful capability of more-or-less accurate ranging of the scattering particles. On the other hand they are somewhat limited by the intense sunlit background during daylight hours and also by the presence of thick clouds. Most importantly, due to the greater limitation on the number and range of measurable parameters, the recovery of aerosol characteristics with these techniques is even more uncertain than with passive techniques, since they must rely more heavily on models
and assumptions. However, one great advantage of active techniques is that single scattering theory is often sufficient for analysis and mathematical inversion.

Early users [12] of pulsed lasers rightly concentrated on the detection and ranging of noctilucent cloud particles and micrometeorites found in the mesosphere where the molecular or Rayleigh scattering background is relatively low. The usefulness of lidars for particulate detection and measurement in the lower stratosphere and within the troposphere has inherent limitations due to the nature of polydisperse back-scattering and the high signal from the molecular component [13,11]. At any rate, since its introduction the lidar method quickly gained favor as an easy-to-operate and practical technique for the detection and sounding of atmospheric particulates. Claims for its capabilities have tended to be somewhat overdrawn [14] however, and only the very recent literature is beginning to show a note of caution in this respect [15].

The monitoring of particulate turbidity may be classified in other ways, for example into those suitable for tropospheric and stratospheric aerosols, and into ground based, airborne or space borne systems. In this review, although we include some material on space borne systems, designed to probe the atmosphere from a considerable distance, we will stick to the above mentioned broad classification.

In general, for reasons mentioned earlier, our analysis and any conclusions reached should be considered as tentative and mainly subjective. A more quantitative and detailed study would require more time and effort than was available for the present study.
2. PASSIVE TECHNIQUES

Since our subject is the natural free atmosphere, where ensembles of identical aerosol particles are practically never found, we have to deal with polydisperse aerosols and scattering. This means that for each type of aerosol, the number density \( n(r) \) varies with the size \( r \) of the particles. In the case of a homogeneous and spherical particle, the usual scattering parameters, such as extinction and scattering cross sections, and differential scattering characteristics, may be approximated analytically in certain idealized cases or calculated exactly from electromagnetic theory [10] in terms of the radius \( r \) and particle composition given by the complex index of refraction, \( m = \nu - i\kappa \) with respect to the medium [16].

For a polydispersion of such particles the same parameters per unit volume of air may be obtained by their integration over the size distribution \( n(r) \) [11]. In principle one should be able, by inversion or other means, to retrieve the unknown characteristics of interest, such as the population, sizes, shape and composition of the particles from observation, by measuring key scattering parameters in all possible directions and for as many wavelengths as available depending on the instrumentation and source. But this would mean in situ measurements, in which case one might as well actually sample and analyze the aerosol particles by means of various physical and chemical techniques.

In remote sensing from within or from outside the atmosphere there are several limitations, e.g. in the number of measurable parameters, in the number and range of wavelength intervals and in the scattering angle \( \theta \) for which scattering parameters \( P_4(\theta) \) may be deduced. Thus we may be
able to deduce some of the desired parameters by making educated guesses about the remaining ones.

Of course, if the particles are neither spherical nor homogeneous, as is likely for example with dry aerosols; if $n(r)$ varies over the volume which contributes to the measurement; and if in addition the particle composition, represented by $m$, varies with particle size in an unknown way, as is quite likely in the humid atmosphere, then the difficulties multiply and a reliable inversion may become all but impossible.

2.1 Spectral extinction method: Among passive techniques this is one of the oldest still in use. It was introduced in the work of the Smithsonian Astrophysical Observatory (SAO) by Langley and its later director, C. G. Abbot [17] in attempting to deduce the solar constant from measurements of the solar flux $F(\lambda)$ at narrow wavelength ($\lambda$) intervals and varying solar zenith distance $\zeta_o$, obtained from preferably high altitude observatories. This so-called long method yields, as a by-product, the narrow band transmissivity $q(\lambda)$ through a vertical slice of the atmosphere, assuming no forward scattered light entered the spectrophotometer. In simplest terms, with the use of the zero-th approximation to the transmission of sunlight, one sets

$$q(\lambda) = \frac{F(\lambda)}{F_o(\lambda)} = \exp \left\{ -\tau(\lambda) \sec \zeta_o \right\}$$

(1)

where $F_o(\lambda)$ is the narrow band extraterrestrial solar flux, and the atmosphere is considered simply a dark filter with normal optical thickness $\tau(\lambda)$. Since $q(\lambda)$ is obtainable from the data essentially by the graphical method, i.e. by plotting the observations on semi-log paper versus the "air mass" or $\sec \zeta_o$, the aerosol component $\tau_M(\lambda)$ may be
obtained by subtracting the known Rayleigh component $\tau_R(\lambda)$ from the "observed" value $\tau(\lambda)$, given by $-\ln[q(\lambda)]$ for $\zeta_0 = 0$. With sufficient wavelength resolution one may fit a curve of $\tau_M(\lambda)$ versus $\lambda$ and try to interpret it in terms of various models.

In the actual application of this method certain errors—usually small—are introduced by neglecting molecular absorption and forward scattering effects which depend on the angular aperture of the photometer and the nature and amount of the scatterers [18]. These difficulties may generally be minimized, for example by proper selection of wavelengths and reduction of the aperture.

In the retrieval of the total number, the size distribution, and the index of refraction of the aerosols involved, and the vertical distribution of these parameters, major difficulties arise which are not easily resolved. One has to make many a priori assumptions in order to deduce one or two such parameters. Without going into the details let us mention the systematic efforts at inversion initiated by Shifrin and Perelman [19] in a series of papers dating from 1963. In this, as in subsequent attempts, the form of the distribution function $n(r)$, and the real or imaginary parts of $m$ or both have to be assumed known [20,21,22]. In addition a homogeneous aerosol layer, at least as to the size distribution if not the number concentration, has to be postulated or tacitly assumed.

The task of deducing the distribution $n(r)$ from the variation of $\tau_M(\lambda)$ as a function of the wavelength $\lambda$ is facilitated if $n(r)$ may be represented by simple functions of $r$ requiring a minimum of parameters. For this reason the so-called Junge or inverse power law distribution,
\( n \propto r^{-\alpha} \), is favored by many workers [20, 21]. Its simplicity is illusory, however, since in the case of visible and near visible sunlight, the upper and lower size limits \( r_1 \) and \( r_2 \) strongly influence the shape of the \( \tau_M(\lambda) \) curve. Other analytical density functions such as the log-normal and gamma distributions are also determined by at least three parameters. As an illustration, consider the modified gamma distribution which we have favored [11] because of its versatility in the realistic modelling of both haze and cloud particles, and which has the general form

\[
    n(r) = ar^{\alpha} \exp\{-br^\gamma\}. \tag{2}
\]

Here the parameters \( a, \alpha, b, \) and \( \gamma \) are related to such measurable quantities as the total concentration, the mode radius \( r_c \) where \( n(r) \) is at a maximum, and the radius where the rate of decrease of \( n(r) \) exceeds a certain value. In order to obtain the specific distribution corresponding to a particular set of \( \tau_M(\lambda) \) values derived by the spectral extinction method, measurements of at least two additional scattering parameters related to the aerosol are needed, even if the distribution is assumed to be of the form (2). Kuriyan and Sekera [23] attempted to eliminate the parameter \( \gamma \) by incorporating its effects in the parameter \( b \), thus facilitating the inversion of spectral attenuation data to deduce the complex refractive index [22]. Other authors [24, 25], however, find that the parameter \( \gamma \) cannot be dispensed with in modelling size spectra for haze and cloud particles.

Regardless of the possibility for formal inversion of spectral attenuation data and the merits of various models, there is no doubt that such data, especially when recorded over long periods of time, are valuable
as indicators of seasonal and longer term variations of atmospheric background turbidity. Anomalies, such as produced by major volcanic eruptions, may be detected and evaluated (see for example Volz [26], Deirmendjian [27,28] and others). Of particular interest in this respect are the solar spectrophotometric data collected and analyzed by Shaw [29,63] at Mauna Loa Observatory. The careful instrument calibration and error analysis conducted by that author [63] make his measurements particularly valuable as reliable indicators of background turbidity levels in mid-Pacific in subtropical latitudes. Retention of some of the best SAO mountain stations and continuation of its solar spectrophotometric work precisely for such a purpose was recommended to its then newly appointed Director, Fred L. Whipple, by Z. Sekera and this author more than twenty years ago [30]. Had that recommendation been heeded we would now possess an invaluable, continuous, 70-year record of turbidity and its variations over several sites in both hemispheres (see also Roosen et al. [77]).

We note that in Japan, the Bouguer-Langley or long method for reducing solar spectrophotometric data obtained with modern instrumentation has been brought up to date by Murai [45,46], who obtained some interesting $\tau_M(\lambda)$ versus $\lambda$ curves with good resolution in the near ultraviolet to the near infrared region.

Under this heading of passive techniques, we might also mention the possibility of performing solar spectrophotometry by means of satellite-borne sensors to be operated near local sunrise or sunset in order to monitor stratospheric aerosols. It appears that such experiments have already been proposed and approved for flight under the code names of "SAM II" and "SAGE" [31]. Any apparent similarity of
these experiments with earth based techniques, however, is superficial, since, for one, the latter uses platforms fixed to the ground within the lowest and often most turbid layer with measurements through a relatively small local volume of atmosphere; whereas in the satellite case, the sensors and the probed layers are constantly shifting and the measurements are taken along tangents to spherical-shell layers of unknown degree of homogeneity and stratification over many kilometers, so that their analysis and interpretation may be subject to even greater uncertainty and may require even more assumptions than in the earth bound case. At any rate, in view of the promise of wide global coverage over long periods, offered by such satellite techniques, the apparent lack of theoretical analysis, verification, and discussion in the open scientific literature prior to a decision to "fly" such experiments is regrettable. The extensive "ground truth" program [120] to check out and validate the SAM II and SAGE results proposed by the experimenters may be an indication of anticipated uncertainties in the reduction of the space data. The possibility to invert simulated data from the projected SAGE experiment, assuming a priori that the aerosol particles to be investigated are distributed according to the Junge law, was discussed by Chu [64] in a recent workshop on inversion methods.

2.2 Aureole method: This technique is somewhat more recent than that of spectral extinction. It is based on the commonly observed phenomenon of a "white" circular region or aureole around the sun, with increasing sky brightness toward the solar limb under cloudless conditions. Without going into a detailed history, we may recall that the SAO [17] staff did use a so-called pyranometer to monitor the sky's brightness within a specified ring around the sun--itself obscured by a simple occulting
ring—as a rough indicator of observing quality or degree of turbidity related to the deduced solar constant. Van de Hulst [32] also mentions early (circa 1920) and quite accurate measurements of aureole brightness and its gradient up to 4 minutes of arc from the solar limb.

It was not until several decades later that the featureless (no colored coronas or other bright rings) aureole could be interpreted in terms of polydisperse, Mie-type, scattering of sunlight on atmospheric aerosol particles whose concentration follows a realistic, size-dependent density function [33,34]. In the cited work, it was also shown that the aureole brightness and its gradient, particularly in the region from the limb out to about 6°, is rather sensitive not only to the overall number density but also to changes in the size distribution of the aerosol particles. Hence properly performed aureole measurements should contain information about aerosol properties and perhaps also their vertical distribution [34].

A few years ago this author, as an initial step in an examination of the use of scattering techniques in cloud (and aerosol) research (since discontinued due to lack of funds) re-examined his earlier aureole work. In particular, by using the more realistic gamma type distributions as in (2), rather than the Junge type ones used earlier [33,34], and precise polydisperse Mie scattering results [11], he showed that the entire region 1° to 10° from the sun is highly sensitive to changes in particle size distribution and that, for moderate turbidity, the contrast between background and aureole brightness increases with the wavelength [35].

The "beauty" of the aureole technique in aerosol monitoring derives mainly from two properties: (a) its sensitivity to low turbidity situations so that even very few, relatively small aerosol
particles in clean molecular air, as found e.g. in the unperturbed
lower stratosphere, measurably affect the near-forward scattered field
and hence the aureole (as shown, e.g., by Newkirk and Eddy's [36] bal-
loon measurements; see also Ref. [11], pp. 102, 103), and (b) the rela-
tive simplicity of the theoretical interpretation—and hence the poten-
tial to extract reliable information on the nature and magnitude of the
responsible aerosol particles—essentially due to the predominance of
the singly-scattered field in the aureole region. In particular, as
shown by this author [33,35], following an original idea of the late
Z. Sekera's [37], the observed clear-sky brightness (and polarization)
field in the aureole region, say within a ring of radius 30° to 40°
of arc around the sun, may be very well approximated by considering
the effect of primary scattering of attenuated direct sunlight on the
aerosol particles as a first-order perturbation superposed on the back-
ground skylight field produced by Rayleigh-type multiple scattering,
suitably adjusted to the optical thickness of the turbid atmosphere.
(What appears to be essentially this same—otherwise unacknowledged—
approach, referred to as "modified single scattering," has recently
been advocated by A. E. S. Green and his collaborators [38,39,40] in
the analysis of aureole measurements.)

Despite its power and potential in aerosol monitoring and re-
search, there seem to be rather few systematic and careful recent
measurements of the solar aureole over long periods, at least in the
western world. The above mentioned very careful earlier work by
Newkirk and Eddy [36] mainly concerned the use of the aureole tech-
nique to assess the influx of meteoric material into the stratosphere.
(Because of their affiliation with the High Altitude Observatory a
parallel concern may have been an assessment of the magnitude and nature of the aureole, considered as background noise during observations of the Sun's outer corona and the zodiacal light.)

As far as terrestrial atmospheric aerosol research is concerned, in this country we have, besides the aforementioned measurements of Green and co-workers [38,39,40], those of Shaw and Deehr [41,42], Twitty [43], and Twitty and co-authors [44]. Abroad there seems to be little interest in this area with the notable exception of Murai's work [45,46] in Japan, and that of Eiden [47] and Bullrich et al. [48] in Germany. One should also mention some Swedish work [49] performed on the island of Anacapri. There is, of course, considerable and significant aureole work, both experimental and theoretical, reported by Soviet scientists (see for example Ref. [8] and subsequent papers) whose discussion is here omitted for reasons outlined in our introduction.

The work of Green et al. [38] is interesting because they attempt to fit their own photoelectric and photographic aureole data by various types of size distribution function. (Their paper is marred by incomplete reference to other authors' prior work, use of some imprecise concepts, definitions, and terminology, unclear presentation and overindulgence in acronyms.) Similar comments apply to the paper by Ward et al. [39] in which the authors (unconvincingly) attempt to derive the index of refraction from "bistatic laser scattering" measurements. A third paper in this group [40] discusses--certainly not for the first time--the validity of the single scattering approximation in simulating sky brightness in the aureole region. An
innovation of the last two cited papers is the introduction of a "regularized oversize distribution" (7) which approaches the Junge type for relatively large radii and has the desirable features of a single maximum at the modal radius and of vanishing as \( r \to 0 \).

Rydgren's [49] earlier work consists of measurements limited to the innermost part of the aureole, between 4' and 66' of arc from the sun's limb, which represents only a small portion of the aureole perturbation. In his reduction and analysis of the data, such as they are, there is no mention of particle size distribution, the property mainly responsible for the observed gradients [33,34]. The qualitative connections of aureole gradients to types of meteorological "air mass" is interesting. One wishes that this line of work were pursued with additional observations from different locations, and with further analysis and interpretation of the data.

Of considerably greater interest is the carefully designed and executed observational work of Murai [45,46] in Japan. The initial work [45] covered the visible range of \( 0.35 \leq \lambda \leq 0.8 \) \( \mu \text{m} \) by means of a double monochromator and photomultiplier, scanning along the sun's vertical at angular distances of 1° to 15° of arc from the sun. This range covers the steepest and most significant part of the aureole brightness gradient. Measurements were carried out at sea level and at 1000 m altitude. The brightness values are conveniently expressed in terms of solar brightness at the same wavelength deduced by the Bouguer-Langley method, which also yields the aerosol optical thickness \( \tau_{\lambda}(\lambda) \) by subtracting the Rayleigh component, \( \tau_{R} \), and the \( O_{3} \) absorption from the total. Thus the absolute spectrum and energy distribution of
sky brightness in the aureole region at several angular distances from the sun are nicely displayed, perhaps for the first time in such detail.

In a continuation of this work [46], the measurements were extended into the near infrared up to $\lambda 1.6 \, \mu m$ using improved instrumentation as to resolution, calibration and scan speed. The analysis indicates that aureole brightnesses increase (relative to the Rayleigh equivalent) with increasing $\lambda$ in the close-in aureole region within an angular distance of $5^\circ$ from the sun, and also with increasing aerosol optical thickness. Some size distribution fitting was attempted using the Junge, log-normal and modified gamma models. Although the results of this research seem to be incomplete, we believe that it is the careful reduction, analysis, and interpretation of this caliber of aureole measurements, together with equivalent solar spectral transmission work, that will ultimately provide the best tool in aerosol monitoring, at least by passive techniques.

Within this same category of high standards of execution we must classify the extensive measurements reported by Eiden [47] and by Bullrich et al. [48] carried out in Germany, Switzerland, and Hawaii. This work follows the good tradition in skylight and aureole studies already established in the early fifties by K. Bullrich and other German scientists such as F. Volz, E. de Barry, and F. Möller and his students and associates (for a bibliography of this earlier work, see literature cited in Refs. [47] and [48]). Both of these studies [47,48], parts of which seem to incorporate Eiden's doctoral thesis, are of particular interest here in that the same instrumentation and observing technique were used to obtain data from different sites typical of sea level, urban, continental, maritime and mountain conditions, showing
corresponding variations in aureole brightness and gradient. However, one may question the efficacy of the exclusive use of the Junge power law in the interpretation of the data (see below).

Another interesting recent study in this area is that of Shaw and Deehr [41,42] which, unfortunately, has not been published in the open literature, presumably pending a more complete analysis and utilization of the data [50]. This work is of particular interest to us because the aureole measurements have been carried out along the sun's vertical plane (rather than along the almucantar) and a perturbation method is used in the analysis as advocated by this author [33,35]. We believe—though it has not been reliably demonstrated—that such measurements, when properly analyzed, should also yield some information on the vertical stratification of particulate turbidity. Shaw and Deehr's [41,42] data analysis relies heavily on Junge type models to derive the particle size distribution. Continuation of this line of work utilizing the excellent instrumentation described by these authors [42] should be highly desirable and useful in developing a refined aerosol monitoring system.

The aureole related work of Twitty and co-authors [43,44] also deserves mention. In the first of this pair of papers [43] there is a serious attempt at formal inversion of aureole data to deduce the particle size distribution. The method proposed is tested on simulated measurements, using a "piecewise linear spline function" to reconstruct the predetermined distribution function, usually of the Junge type. This is limited to a size range $0.25 \leq r \leq 8.00 \mu m$, which precludes consideration of the sizable effects of particles outside this range on the aureole and other parts of the phase function. Tests on reconstructing
other types of distribution, such as hazes L and M [11], are fairly successful but fail in the case of haze H.

Parenthetically, a rather interesting paper of Weinman's [51], which is worth mentioning here, considers multiple scattering in a water-droplet cloud medium whose phase function is roughly approximated by a forward peak and another, smoothly varying function of the scattering angle $\theta$, both of which are analytical in $\theta$. Weinman's approach considers only axially symmetric solutions of the radiative transfer equation. It is eminently suitable for investigating the effects of multiple scattering on the skylight intensity and its gradient in the aureole region as the optical thickness $\tau$ is increased, keeping $n(r)$ constant or conversely, varying the form of $n(r)$ while keeping $\tau$ constant. One particularly interesting result is his finding that a qualitatively arrived at estimate of $\tau = 16$ for the cloud optical thickness corresponding to the "filtered sun effect," i.e. when the sun's disc, barely visible through the cloud layer, appears to have the same color and brightness as the cloud background (see Ref. [11], pp. 113-114) is nicely corroborated in Weinman's paper (Ref. [51], Fig. 3b) which appeared while the book [11] was in press. (A good experimental verification of this phenomenon in a closely related case is provided by Kabanov and Savyel'yev [52].)

The second paper of Twitty and co-authors [44] is interesting in that it describes what may have been the first successful aureole measurements taken from an airplane, though certainly not the first airborne ones. Priority for the latter belongs to Newkirk and Eddy [36] for their important earlier work with a balloon borne coronameter. The airplane measurements [44] seem to have met with difficulties in refinement and accuracy of the aureole data, especially at small $\theta$, due to the large
field of view of the optical system needed to compensate for aircraft roll. Also the choice of a single band-pass filter is unfortunate for, despite their justification in terms of the insensitivity to wavelength of their inversion method, other studies [35] show that both the phase function and the aureole imbedded in the Rayleigh type skylight background may be measurably wavelength dependent.

Discounting these and some other drawbacks this work [44] demonstrates the value of airplane-borne aureole measurements, especially their potential to provide data on the variation of aerosol concentration—and possibly also of size distributions and composition—with height above the ground. Their inversion relies on the above mentioned technique of Twitty’s [43] and that of Weinman and co-authors [53]. However, the authors of Ref. [44] seem to have been somewhat carried away in claiming that their inversion method assumes no a priori functional form for the size distribution, when in fact the previous work [43,53] they cite nowhere demonstrates this but is applied only to a Junge type example.

In this connection, one cannot help noticing that in the published work of a number of authors cited here [20,21,41,43,44,47,48,53,78,81,83] there is a persistent and often exclusive use of the Junge-type or power-law size distribution in modelling aerosol particles for the direct evaluation, as well as the inversion, of their scattering effects. This situation almost amounts to a mystique (similar to that surrounding the use of the so-called Marshall-Palmer law to represent raindrop spectra), and is quite puzzling vis-à-vis actual size distributions as found by particle capture and sizing (see for example Refs. [54,55, and 56]) and the obvious inadequacy of a single power law
to fit the data over the entire size spectrum. In light scattering applications to polydispersions, using Mie theory or equivalent approximations, it was demonstrated some time ago [57] that the results of model calculations are too sensitive to the choice of lower and upper bounds, \( r_1 \) and \( r_2 \), in particle size that one must choose when integrating a power law distribution to be useful in the analysis or inversion of scattering data. (Despite this limitation, in a recent discussion on the inversion of projected satellite data (Ref. [64], p. 525) the Junge distribution is referred to as a "two parameter" model.) Nevertheless, what we have called "Model C" (for continental aerosol) in earlier work [59,60] to simulate the Junge distribution was later adopted by several authors despite our caveats. (Such difficulties in interpretation are also implicit in a recent analysis by Fraser [58].) In an attempt to adhere to the Junge model, some authors have used the term even after their results have indicated that the "exponent of the (power law) aerosol size distribution...has to be considered a function of the particle size" [47](1).

To be sure power law models have been very useful as an initial approximation to the size distributions observed in the early measurements of H. Dessens (1946) and C. Junge (1952) cited in Ref. [34], particularly in the significant large-particle end of the spectrum. However, they have no place in the more sophisticated and detailed analyses and applications of scattering and radiative transfer theory required for current air pollution and climatic studies, where the use of analytical, well defined and more versatile density functions, such as mentioned earlier, would be more productive.
Before closing this section it is worth mentioning—even if unable to properly review it here—that a few years ago the well known Russian scholar G. V. Rozenberg [61] had examined the possibilities of twilight aureole observations from a low-orbit spacecraft. In a collaboration with Nikolaeva-Tereshkova [62], he analyzed the first such data from the manned satellite Vostok-6, to obtain information on the vertical structure of stratospheric aerosols. The technique, though closely related to the aureole, should be treated under twilight phenomena, in which the sphericity of the atmosphere must be considered, whereas in aureole observations from the earth's surface, when the sun is relatively high in the sky, the sphericity effects may generally be neglected. Also, the vertical stratification of the aerosol must be considered, in both the direct and the inverse problem, thus introducing additional unknowns; whereas in earth-bound observations this may be neglected in a first approximation and the actual vertical distribution replaced by an equivalent, homogeneously mixed turbid atmosphere, as shown earlier [34,35]. Hence it may be considerably more difficult to interpret—in terms of scattering and radiative transfer in inhomogeneous media—twilight observations from space than to interpret those from the earth's surface.

We have devoted considerable space to the aureole technique because we feel that, among passive techniques, systematic and continuous measurements of the solar aureole, in combination with simultaneous Bouguer-Langley type spectrophotometry of direct sunlight, represent one of the most reliable and sensitive turbidity monitoring tools, which also inherently contain much valuable information about the nature and distribution of the responsible aerosol particles.
2.3 Other passive techniques: In addition to those relying on sunlight extinction and aureole brightness, one may suggest other passive monitoring techniques based on radiation parameters measured from inside or outside the sunlit atmosphere. Their interpretation, however, is generally more difficult as it must often depend on complete solutions of the appropriate radiative transfer equations for a mixed, vertically and horizontally inhomogeneous spherical atmosphere with all relevant orders of scattering properly accounted for. [In contrast, the techniques discussed above under 2.1 and 2.2 depend on the zeroth (no scattering effects other than extinction) and first order (primary scattering only) approximations, respectively.] Since there are apt to be as many solutions to these equations as there are optical weather situations to be simulated, the recovery of aerosol characteristics, for example, by comparison of observed parameters with a catalogue of those obtained from model solutions seems hopeless. Equally—if not more—hopeless is the mathematical inversion of a set of data for arbitrary weather situations and observing conditions. Only under special conditions, such as stably stratified cloudless air overlying a smooth water surface, may the model of the local atmosphere be simple enough to yield tractable solutions and possibly inversions.

After the appearance of S. Chandrasekhar's well known monograph on radiative transfer in sunlit planetary atmospheres [65] the late Z. Sekera was among the first to realize its value in investigations of atmospheric particulate turbidity. While concentrating his efforts on studies of the degree and plane of polarization, and their distribution over the sunlit sky, he was also among the first to point out, in two original and scholarly papers [37,66], the great difficulties involved
in the recovery of particle size distributions from measurements of these parameters. The obvious and much easier aureole method, in which—contrary to the previous instance—higher order scattering effects on brightness and polarization may be neglected, he left to one of his students [33]. Sekera, who belonged to that now rare and disappearing breed of workers who attach equal importance to theory and observation, believed in gradual progress based on a thorough understanding of a step taken before advancing to the next. Hence his reluctance to let himself and his associates undertake "complete" numerical "solutions" to a given problem.

Without doubt there has been considerable progress in the solution of the problem of radiative transfer in planetary atmospheres since the appearance of Chandrasekhar's treatise [65] almost three decades ago. Evidence of this is the rather extensive literature on various methods of evaluation and numerical examples of the effect of aerosols and clouds on the inward and outward, multiply scattered, fluxes from the boundaries of sunlit atmospheres. A review of this literature is outside the purview of the present survey but as relevant examples we might mention the work of Hansen [67,68] and of Hovenier [69] on the outward fluxes from planetary atmospheres, and that of Braslau and Dave [70] on inward fluxes from our own atmosphere. Also, a long list of papers by Plass and co-authors (see e.g. Ref. [71]) have been published on various applications of Monte Carlo techniques originated by Collins and Wells [72]. For an excellent review of the problem of sunlight reflection on planetary atmospheres see the paper by Hansen and Travis [73] and for a lucid discussion of various methods of evaluation of multiple scattering effects see that by Irvine [74].
Whereas most of the recent literature concerns the general problem of radiative transfer in planetary atmospheres, a number of papers address specific aspects related to subjects often of ephemeral public or government interest such as, for example, the effects of turbidity on weather and climate [70], mentioned in our introductory Sect. 1; or the remote probing of the Venusian atmosphere by photopolarimetric scanning of its sunlit disc [69,73]. A few of these are specifically concerned with the monitoring of the amount, nature, and spatial and temporal distribution of particulate turbidity.

Among these, the previously cited paper by Weinman et al. [53], notwithstanding its somewhat misleading title implying that entire phase functions are derived from "multiply scattered" sunlight, essentially boils down to showing that only a limited portion of the phase function, at forward scattering angles $0 \leq \theta \leq 20^\circ$, may be derived with any confidence by considering single scattering only, and this provided the particles behave themselves and follow a particular Junge-type size distribution. In this sense, their solution amounts to just a special case of the aureole problem discussed in Sect. 2.1 above.

Another paper in this category is that by Herman and co-authors [75] in which they claim an ability to determine an (otherwise undefined) "effective complex part," (i.e., the imaginary part $\kappa$) of the refractive index $m = \nu - i\kappa$ of the aerosol substance (assuming the real part $\nu$ is known or unimportant). Their proposed method consists of comparing the ratio of $H/S$ of the diffusely transmitted global skylight $H$, received at a horizontal surface and the reduced direct sunlight $S = F \cos \xi_0$, where $F$ is given by equation (1), with a catalogue of theoretical curves of this ratio plotted as a function of $\kappa$ (keeping $\nu$ constant) for various
solar zenith angles, dust optical thicknesses, and ground reflectivities. The idea is attractive but, in the form presented by its authors, it would provide some information (of low reliability) on the nature and none on the quantity of aerosol particles. Furthermore, the potential use of H and S data in this fashion is demonstrated only in principle, relying on a hypothetical set of measurements under significant restrictions such as a knowledge of the nature of the surface reflectivity and of the real part \( n \) of the refractive index. Moreover the authors have considered only a particular Junge type distribution (with no vertical variation) albeit admitting that departures from it may vitiate their results.

A more interesting scheme for using H, the total global diffuse skylight received from the entire hemisphere, involves the ratio \( S/H \) or the reciprocal of Herman and co-authors' ratio (where both S and H are integrated over the same \( \lambda \) range). Its use was suggested more than twenty years ago [76] as a rather sensitive measure of overhead degree of turbidity, at least for very clear up to moderately turbid days. This sensitivity derives from the fact that as the turbidity increases the numerator, \( S \), decreases while the denominator, \( H \), increases, so that the drop in their ratio is quite fast. For constant turbidity, the ratio \( S/H \) should also drop somewhat as the ground reflectivity increases. This results from a moderate increase in \( H \) due to back-scattering of the reflected component, whereas \( S \) remains constant, as shown theoretically for both a Rayleigh [76] and a turbid [75] atmosphere. Furthermore the ratio \( S/H \) drops quasiexponentially also with the air mass or sec \( \tau_0 \), both in the molecular or Rayleigh atmosphere and in the turbid atmosphere (see Fig. 4, Ref. [76]). Thus the S/H
technique is attractive since both quantities $S$ and $H$, whose sum $G$
is sometimes called the *global radiation*, can be routinely monitored
by relatively simple, well tested instrumentation, as was done during
the operation of some SAO stations (Ref. [17], vol. 5; see also [168,169]).

In fact we believe that the above $S/H$ technique deserves to be
more fully explored for its potential as an aerosol monitoring and
investigating tool to be used in conjunction with modern sophisti-
cated instrumentation and realistic model calculations. For example
wavelength resolved ratios $S(\lambda)/H(\lambda)$ plotted against $\lambda$ for various
zenith angles $\zeta_0$ of the sun may contain information, not only on the
amount but also on the size range and distribution of the responsible
aerosol particles. Note that the ratio $S/H$ is dimensionless since
the illuminating solar flux $F_0$, over the wavelength interval used for
the measurement, enters both numerator and denominator as a factor
[76]. It may thus serve as a quantitative measure of degree of tur-
bidity since it tends to vanish with increasing aerosol optical thickness $\tau_M$, in which case $S \to 0$ while $H$ remains finite. The degree of
turbidity $T$ (from the radiation point of view) may then be formally
defined by the expression

$$
T \equiv 1 - \frac{S/H}{(S/H)_R}, \quad 0 \leq T < 1
$$

(3)

where the subscript $R$ refers to the Rayleigh case and $S$ and $H$ are meas-
ured (or estimated) for $\zeta_0 = 0$ or some other fixed position of the sun.
Note that (3) properly vanishes (zero turbidity) when no aerosols are
present in a purely gaseous atmosphere, and it closely approaches unity
for a sky completely overcast by a layer of opaque cloud.

As far as we know these notions have not been broached before now.
We intend to expand on them elsewhere as soon as possible if and when support for an extension and followup on the present survey can be secured.

Under special conditions it may be possible to infer the dust loading of the atmosphere from satellite measurements of outward fluxes of sunlight reflected from the earth-atmosphere system. The method may be called the diffuse reflection technique. Here the theoretical matching by modelling is considerably more complicated (than the skylight case) by the fact that under cloudless conditions the outgoing radiation is critically dependent on the nature of the underlying ground (or sea) surface, as demonstrated some time ago by R. S. Fraser [78]. Furthermore, as clearly indicated, for example, by the measurements of Coulson and co-authors [79,80], and of others, not only is there considerable variability in the reflectivity of natural surfaces for various types of radiation but also it depends significantly on their angle of incidence and emergence. It is therefore not surprising to find that any estimates of dust amounts arrived at by the above technique are subject to considerable uncertainty since they may require important a priori assumptions on the nature of the underlying surface, the dust particle size distribution law, dust composition, shape, etc.

One example of this technique is Fraser's [81] attempt to deduce the mass of wind-blown Sahara dust over the adjacent ocean from "Landsat-I" satellite data obtained by means of the on-board "multispectral scanner subsystem." These consisted of nadir radiances measured within four bands centered in the visible and near infrared spectrum. The analysis essentially involves (a) erecting a single,
turbid atmosphere model of fixed optical thickness, particle size
distribution, and solar zenith distance; plus some additional assump-
tions needed to assign values to otherwise undetermined parameters
(such as sea-surface reflectivity and bulk density of the particles),
guided in part by simultaneous surface observations of unknown accu-
ry; and (b) comparing the computed radiances with measured radiances
over the ocean. Finding that this model results in values which are
within about 10 percent of the measured "reflectivities" the author
then arrives at a value of 1.6 g m\(^{-2}\) (column\(^{-1}\)) for the mass of the
overlying Sahara dust. (This, by the way, is about four times our own
estimate [27] of about 0.39 for the Krakatoa dust expressed in the
same units, and adjusted to the same density of 2.6 g cm\(^{-3}\), assumed
by Fraser for the particle substance. Bearing in mind that our esti-
mate is based on an optical thickness anomaly of \(\tau_D \approx 0.55\) for the
Krakatoa dust, very near the value of 0.5 assumed for the Sahara dust,
the sizable difference in the mass estimate must be mainly due to dif-
fences in the size distributions assumed in each case.)

There are error estimates given [81] for various changes in
parameters, mainly through a linear (seemingly empirical but not other-
wise justified) relation between dust mass and nadir radiance. Some
other sources of error, uncertainty, and unreliability are also pointed
out by the author [81]. Not mentioned are significant uncertainties that
we believe may be introduced by the adoption of an \(r^{-3}\) type size dis-
tribution model as the sole criterion to assess the validity and re-
liability of the results obtained by this method. (We note that, in
his review paper on marine aerosols [82], Junge indicates that the
Sahara dust component may be represented by a \textit{continuous} size distri-
bution function in the range $3 \leq r \leq 20 \, \mu m$ superposed on the background marine aerosol, itself represented by a continuous distribution with a definite mode radius). At any rate Fraser's [81] is a serious attempt to use satellite based radiance data for this purpose, with quite plausible results obtained despite the above mentioned difficulties.

A somewhat earlier serious attempt in this direction which deserves mention is that by Thompson and Wells [94] who, using two of our models [11], called haze $M$ and $L$, respectively, for maritime and continental aerosols, arrive at equally plausible results with good accuracy. Neither of these numerical experiments constitute proof, however, that this technique recommends itself for use in a global aerosol monitoring program. Considerable further competent theoretical analysis, study of observed conditions, and sophistication in the choice of instrumentation and most useful measurable parameters, etc., is needed before basing an operational aerosol monitoring program on earth radiances obtained from circumterrestrial space vehicles.

Another example of such techniques is the so-called "simple method" described and used by Mekler and co-authors [83] and by Koepke and Quenzel [84]. In the first of these papers, in an analysis similar to Fraser's [81], satellite (ERTS) measurements of upward radiances over a desert lake are used to deduce a "relative aerosol content" by comparison with model calculations. Here again the credibility of the results essentially hinges on the assumption of a known surface reflectivity—and hence of sea-state which depends on certain wave producing parameters—and a size distribution conforming to $n(r) \propto r^{-6}$. Thus, as in the previous case [81], the claimed high accuracies may be somewhat illusory.
In the second paper [84] of this pair—at this writing available only in conference summary form—the authors underscore the advantage of a geostationary space platform for measuring diffuse reflection over a fixed area (of the ocean, in this case) and its expected variation as a function of \( \zeta_o \), the local sun's zenith angle, to determine the overlying aerosol optical thickness \( \tau_M \). The details as to how this may be accomplished may be spelled out in a future paper according to a private communication from H. Quenzel (27 Nov. 1978).

In the recent past, techniques based on the brightness, polarization and color distribution of the twilight sky, and their variation with the sun's depression angle have been proposed and used in studies of the aerosol content of the lower and upper stratosphere. Examples of these are the studies initiated by Volz and Goody [85] in the U.S., and in the USSR the work described in Rozenberg's excellent monograph [86]. Interest in this area is not new, dating at least from the post Krakatoa period [27], for good reasons: Terrestrially observed twilight phenomena are especially sensitive to even small amounts of stratospheric aerosols and their variations. For equally good reasons, however, the twilight technique does not lend itself as an aerosol monitoring tool in the quantitative sense because, as mentioned in the previous section, the interpretation of twilight observables is fraught with difficulties and uncertainties. For example, due to the nature of the corresponding scattering problem, in order to reproduce observations fairly accurately, the modelling and theoretical treatment of the direct problem must include at least secondary scattering [86]. This, combined with the spherical geometry involved, renders the solution of the pertinent integro-differential equations much more
difficult than in the flat atmosphere case. Furthermore, the observed twilight involves the passage of sunlight through a sizable portion of the atmosphere overlying a geographical area of several $10^4 \text{ km}^2$ over which the turbidity may vary significantly. Thus the number of skylight parameters needed to deduce the aerosol properties in a twilight problem may be double that needed, say, in an aureole problem, and a meaningful mathematical inversion becomes practically impossible. It is not surprising therefore that, except mainly as a qualitative tool in the early diagnosis and monitoring of the duration of anomalous stratospheric aerosol incursions, such as those from major volcanic events, the twilight method can not be used as a standard aerosol monitoring technique.

Finally we mention another passive technique, first proposed a few years ago [87], using satellite-borne sensors to detect the occasional appearance of tenuous particulate layers, such as nacreous and noctilucent clouds, found at high latitudes in the upper stratosphere and mesosphere respectively. This consists of the photometry of the earth's terminator from a low, polar orbit satellite to detect its sunlit extension against the dark background of the shadowed surface and lower atmosphere. Subsequently Rozenberg [88] examined this and other similar schemes for the same purpose and demonstrated the feasibility of daytime satellite observation of such particulates. Precisely this kind of observation was successfully carried out aboard the U.S. OGO-6 polar orbit satellite in 1969, revealing a continuous veil of a "dense scattering layer" over the geographical pole, as reported by Donahue and co-authors [89] (who, incidentally, seem to be unaware of the earlier suggestions [87,88] along these lines). They conclude
that noctilucent clouds are a persistent summer phenomenon over the poles, with an average optical thickness (in yellow light) of $10^{-4}$ and a particle radius of 0.13 μm, both of which are very close to our own earlier estimates [87]. (The same order of magnitude for the optical thickness may be deduced from older surface observations and rocket sampling data [90].)

It appears that the Soviet space program also has paid considerable attention to the detection and study of noctilucent clouds as indicated in discussions of their observation aboard their manned Orbital Scientific Station Salyut-4 by Villman [91] and Avaste [92]. Interest in noctilucent clouds is not new to Northern European—particularly to Estonian—scientists, as may be seen, for example, from papers presented at the 1966 Tallinn International Symposium on Noctilucent Clouds [93].

Experiments such as those mentioned above demonstrate the value of space-borne observations in the detection and monitoring of various particulates found in the upper stratosphere and mesosphere, say between 20 and 80 km above the surface, by means of passive techniques, should this be deemed important in the future.
3. ACTIVE TECHNIQUES

For the purposes of this survey, we shall define active techniques as those involving the use of artificial sources of controlled spectral composition, brightness, polarization, and modulation to detect and investigate aerosol particles by measuring the transmission and scattering characteristics of turbid air. These techniques are, of course, best suited to laboratory studies where the type, origin, volume, etc. of the air sample may also be controlled. However, there are other applications, involving for example searchlight and laser beams, where portions of the natural atmosphere may be the subject of observation.

Measurements of the direct transmission of collimated light through varying horizontal paths of air can yield the volume extinction coefficient \( \beta_e(\lambda) \), by a method similar to the SAO's long method described in 2.1 and by equation (1). Assuming homogeneous air in the path, one simply replaces the exponent \( \tau(\lambda) \) in (1) by \( \beta_e(\lambda)L \), where \( L \) is the (variable) path length, to obtain a graphical solution for \( \beta_e(\lambda) \) by appropriate plotting of the data. There are difficulties, however, due to the presence of "air light" or diffuse light scattered toward the receiver, ground reflection, etc. as pointed out by Middleton [95]. These may be minimized by a proper choice of experimental techniques and conditions, as shown by the still unexcelled data obtained by this technique more than twenty years ago by Arnulf and collaborators [96]. However, this particular active technique, involving as it does short paths, of the order of \( 10^{-2} \) m, is essentially an \textit{in situ} rather than a remote sensing one, and it will not be further discussed here.
3.1 The searchlight or "white light" method: Before the advent of laser radar and in the immediate post World War II years, the searchlight was favored, principally by Soviet scientists as the main tool for remote probing. They seem to have extracted considerable information on the aerosol content of the lower and middle stratosphere up to 50 km, as may be inferred from references and data in Rozenberg's twilight monograph [86], and his two excellent reviews on light scattering and aerosols [8,97]. (Except for a review by Kondratyev [98], the original 1960 monograph Projector Beams in the Atmosphere, by Yu. S. Georgievski and five co-authors, including its editor, G. V. Rozenberg, was not available to us, either in Russian or in translation.) According to Rozenberg [97], I. A. Khvostikov in the Soviet Union was among the first (1944) to realize the value of searchlights in aerosol studies. In this country the principal exponent of the searchlight method has been Louis Elterman [99] who cites E. O. Hulburt as the pioneer (1946), although Rozenberg [97] mentions "Synge" as the first (1930) to propose the rather obvious idea of using projector beams as atmospheric probes.

We need not overly dwell on the searchlight technique here since it seems to have been supplanted by the more recent and sophisticated monostatic and bistatic laser radar systems. Suffice it to say that the technique essentially consists of the photometric scanning of a more-or-less vertical searchlight beam, usually composed of "white" light as produced by a carbon arc, to record brightness variations along its extent. Assuming that the illuminated and sensed volume is of the right size, primary scattering theory may be invoked to
deduce some information on the vertical density distribution and parts of the scattering phase function of the aerosols. There are a number of uncertainties and difficulties of interpretation stemming mainly from the geometry and other observational constraints involved. Examples are the changes in illuminated and observed volume with altitude, the inseparability of changes in brightness and polarization due to altitude and scattering angle, unknown horizontal inhomogeneities in aerosol content and properties between the source and receiver, and others. Nevertheless some quite credible results on the vertical aerosol stratification have been obtained, both in the Soviet Union [97] and in this country [99], by this method, which has been and may still be used to advantage to detect and gauge the relative magnitude of turbidity anomalies produced by volcanic dust incursions [100] and to independently check laser radar results (see below).

3.2 Laser or monochromatic light techniques: It was not long after the invention of the marvellously promising optical maser or laser in the early sixties that it was used in the optical probing of the higher atmosphere [12,101]. Thanks to the introduction of "Q-switching" or "giant" pulse generation it eventually became possible to detect and range lower atmospheric particulate layers as well. (According to Collis [14] the awkward acronym lidar, which is often used in lieu of optical or laser radar, was introduced by the late Myron Ligda after an original (1953) suggestion by Middleton and Spilhaus.) Once the technology became available, the optical radar was quickly adopted by many workers in the United States, West
Indies, England, Australia, and Japan [14; 102 to 106] as the optical aerosol sounding method in preference to older and well-tested techniques. This may have been somewhat premature, as discussed above and below (see also an unpublished paper by Holland [107]). In fact, despite its obvious advantages in compactness, simplicity of operation, speed, and mobility of the system, in certain cases the lidar technique may not be the best tool for the remote probing of the atmosphere for aerosol content and composition. Nevertheless, judging from the increasing number and frequency of specialized conferences on the subject [108] (the ninth one in the series is scheduled for Munich summer 1979), laser probing of the atmosphere seems to have mushroomed since the initial successes.

Besides the great advantages of monochromaticity and extreme narrowness of beam, the chief attraction of optical radar stems from the shortness of the emitted pulses, of the order of $10^{-8}$ second, resulting in pulse lengths of tens of meters or, depending on distance from the source, illuminated volumes of say less than $10^3 \text{ m}^3$. Hence, except possibly in optically very dense media such as cumulus congestus, multiple scattering may be quite negligible and the analysis and interpretation of data much simplified. The question of coherent versus incoherent (or independent) scattering of laser light was raised and quickly disposed of in favor of independent scattering under most atmospheric conditions, experimentally by Sherman et al. [109] and theoretically by Ivanov and Khairullina [110].

As indicated in the introduction, the laser radar, with all its obvious advantages and versatility, suffers from certain drawbacks
and limitations. Chief among the latter, and one which has not been overcome yet, is an ambiguity pointed out some time ago [13] which stems from the nature of the technique. Briefly, in terms of the relevant physics of particulate optical turbidity, the so called lidar equation boils down to writing [13; 11, p. 124]

$$R_s(h) \equiv \frac{I(h)}{I_R(h)} = 1 + \frac{\beta_{SM} P_M(\pi, h)}{\beta_{SR} P_R(\pi, h)} > 1 \quad (4)$$

where $R_s$ is the dimensionless ratio of intensities (or fluxes), with $I(h)$ being proportional to the observed lidar return at height $h$, and $I_R(h)$ to that expected from the molecular or Rayleigh atmosphere (assumed known) in the absence of aerosols. On the right hand side of (4), $\beta_s(h)$ and $P(\pi, h)$ stand for the volume scattering and normalized back scattering phase function, respectively, with subscripts $M$ and $R$ denoting Mie (simulating the natural aerosol) and Rayleigh (molecular and/or atomic) scatterers. Both $\beta_M$ and $P_M$ of course are given by summations or integrations over the particle populations or continuous size distributions, n(r,h), and on the nature or composition of the aerosols, which may also be height dependent.

Note that, assuming the Rayleigh quantities in (4) are known or may be estimated from a knowledge of the atmospheric molecular density at height $h$, lidar observation is capable of supplying values only of the product $\beta_{SM} P_M$ with no way of discriminating between the individual factors, except by independent measurements of either one.

In a well written paper covering work under the D.o.T.'s CIAP program [2] Russell et al. [15] analyzed their Menlo Park lidar data,
covering the 1972-1974 period, by means of a "scattering ratio \( R(h) \)" given by an expression exactly equivalent to (4). To properly reduce the data by this method it is necessary to know the absolute molecular densities at each datum level at the time of the lidar sounding. These being unavailable the SRI authors used a "clean air calibration method," originally suggested by Grams and Fiocco [111]. This consists of normalizing the data so that \( R(h_o) = 1 \) at a reference level \( h_o \), where the ratio of the actual signal to a theoretically computed one, \( I_R(h_o) \), is at a minimum. It is then assumed, without sufficient justification, that at the said reference level \( h_o \), the particulate content is nil or negligible. Such an assumption could lead to underestimates of the particulate loading (see below).

The results obtained by the SRI group [15] appear to be quite plausible, so far as they go, considering the methodology and assumptions spelled out in their paper. They manage to resolve the problem of discriminating between the factors in the product \( \beta_{sM}^p \) by simply adopting a well defined aerosol model for the local stratosphere, including both composition and size distribution (the so called Haze-H model [11]). This allows them to estimate the vertical distribution and integrated scattering optical thickness \( \tau_s \) of the stratospheric aerosol between 10 and 50 km. Their value of about 0.005 for \( \tau_s \) at \( \lambda 550 \) mm may be representative of background or intervolcanic turbidity levels. For comparison we note that the aerosol optical thickness above Mt. Wilson for the clearest, pre-Katmai eruption (June 1912) days was estimated from the SAO data [27] to be about 0.05, or ten times the above SRI value—perhaps a reasonable difference
considering the low elevation (1.7 km) of Mt. Wilson Observatory. This exercise, of course, is no proof of the lidar’s ability, by itself, to monitor accurately the amount and nature of stratospheric particulate turbidity.

Both in the preliminary report and in the published version [15] some of the SRI data are shown to yield values of $R(h) < 1$, somewhere between 10 and 15 km above sea level, contrary to the implication of (4), where the aerosol contribution is assumed to be always positive. This may be explained either by experimental error or by the actual molecular densities being lower than the standard atmosphere values adopted by the SRI authors [15]. There may, however, be another explanation which admits the observed $R_S < 1$ values and molecular densities equal to or less than the adopted standard ones, that is to say, the presence of aerosols may yield weaker lidar returns than those from an equivalent purely gaseous layer.

This may be easily demonstrated if we remember that (4) was based on single scattering theory for diffuse reflection, assuming an optical thickness $\tau_s$ of the illuminated volume such that $\tau_s = \tau_R + \tau_M << 1$. In that case we have [11, p. 97]

$$I_R(\tau_s, h) = \tau_s \frac{P_R(\pi, h)}{4\pi} F(h)$$  \hspace{1cm} (5)

where $F$ is the illuminating laser radiation reaching the level $h$, and we have assumed the volume element, characterized by $\tau_s$, to be devoid of aerosols. If we now write an equivalent expression for the same volume containing turbid air of the same optical thickness, $\tau_s$, we
have to introduce mixing ratios for the scattering function [11, p. 99], which means writing

$$I(\tau_s, h) = \frac{\tau_s}{4\pi} F \left[ \frac{\tau_R}{\tau_s} P_R(\pi) + \frac{\tau_M}{\tau_s} P_M(\pi) \right]. \quad (6)$$

As an illustration, assume $\tau_R = 0.9 \tau_s$, and use our haze H model, so that

$$\frac{P_M(\pi)}{4\pi} = 0.011 \text{ at } \lambda 0.7 \mu m \ [11, \text{ p. 178}], \text{ and } \frac{P_R(\pi)}{4\pi} = 0.12,$$

whence, taking the ratio of (6) to (5), we get

$$R_s(h) = \frac{\tau_R}{\tau_s} P_R + \frac{\tau_M}{\tau_s} P_M \quad (7)$$

or in this case $R_s(h) = 0.909 < 1$, contradicting Eq. (4). We arrive at an identical result by applying the perturbation technique used in interpretations of the aureole [33,34]. For example, by using this technique it may be easily shown that the ratio $R_s(h)$ may be expressed in the alternate form

$$R_s(h) = 1 + \frac{\tau_M(h)}{\tau_s(h)} \frac{P_D(\pi, h)}{P_R(\pi, h)} \quad (8)$$

where $P_D(\pi) \equiv P_M(\pi) - P_R(\pi)$. In the above mentioned example $P_D$ is clearly negative and hence (8) again gives a value of $R_s = 0.909 < 1$.

This creates a seeming paradox because according to either (7) or (8), the higher the aerosol content the smaller the ratio $R_s$, which again contradicts Eq. (4) and also is contrary to observation. This
inference would be correct if the introduction of aerosols were to result in an equivalent reduction of molecular density which governs $\tau_R$ [121]. In fact, however, the addition of aerosols can hardly alter the molecular density except in extreme cases. At 15 km for example the air density of a standard atmosphere is of the order of 194 g m$^{-3}$; the volume occupied by the haze-H model particles [11, p. 78] is $3.14 \times 10^{-6}$ cm$^3$ m$^{-3}$, hence the mass mixing ratio of aerosol to air is of the order of $10^{-8}$, even assuming 100 particles per cubic centimeter.

The example illustrates the sensitivity of the backscattering ratio to the assumed molecular density in the SRI method of reduction. Also, the technique is not very sensitive to low concentrations of stratospheric aerosols. As mentioned elsewhere the optical density (or thickness) contributed by aerosols is much larger than their contribution to the air mass density. Thus the lidar method, by itself, may be incapable of detecting their presence in small amounts. Hence, unless a technique is found to discriminate between the molecular and aerosol returns, optical radar will remain a good tool for monitoring relative, but not absolute, turbidity levels.

Remsberg and Northam [112] arrive at similar conclusions in comparing lidar and "dustsonde" data taken over the same point. They emphasize the importance of independent and direct measurements of molecular densities at the time of the lidar sounding, in which case, of course, the lidar technique would cease to be a remote sensing one.
There is no doubt that the lidar technique is a good one for detecting extraordinary increases in stratospheric turbidity, as in the case of the Volcán del Fuego event of October 1974. This was clearly evident in the SRI data taken after that event [113] from which they derive a volcanic dust optical thickness of $\tau = 0.03$ (i.e. six times the pre-Fuego value). This is close to the value derived by Elterman [114] by the searchlight technique. However, in both cases the data reduction strongly depends on model assumptions about the size distribution and composition of the volcanic dust. Interestingly, the turbidity anomalies for both the 1912 Katmai and the 1963 Agung events, derived from solar and stellar extinction measurements [27,28], of about $\tau_D = 0.30$, is ten times that of Fuego volcano. The difference may be partly explained if we consider that the Katmai and Agung data pertain to the atmosphere above the observatories which are located within the troposphere. Also since these values involve no a priori models but depend only on a comparison of pre- and post-volcanic direct extinction measurements, without change in instrumentation and site, we consider them to be more reliable than the lidar results.

Additional evidence that lidar measurements may systematically underestimate stratospheric particulate turbidities is provided by an analysis [115] of various types of non-lidar data, mostly by Soviet and some other scientists. Although the lidar data used in their analysis do not include most recent U.S. work, given the reputation of the senior author G. V. Rozenberg, their conclusions must be considered seriously. They introduce the apt term "optical stratification"--as
distinct from mass stratification—and suggest that, besides the well
documented aerosol minimum near the troposphere, there may be a second
minimum near 33 km. Of course if the aerosols are irregular particles
the lidar returns will be even smaller than for equivalent spherical
particles [107].

In any case, the presence of Volcán del Fuego dust afforded a
good opportunity to compare various measurement techniques thanks
to the availability of instrumentation and experiment teams imme-
diately after the eruption. (Such was not the case, for example,
with the Agung 1963 event, when whatever measurements we have were
made by chance rather than by design [27].) An attempt at local
sampling by means of a balloon borne "light scattering counter" [116]
after the Fuego event provides only a rough check of the lidar data.
It is difficult to judge the reliability of this local sampling tech-
nique from the literature cited [112,116].

In another instance of laser applications Schotland and Reiss
[118] examined the possibilities offered by a "bistatic" laser system.
They considered a system with a laser operating in the "normal" mode
(rather than the Q-switch mode used in lidar) i.e. pulse duration of
the order of 0.001 sec, hence more energy. Due to the long length of
the pulse (~300 km), secondary scattering effects at the receiver cannot
be neglected, rendering the interpretation of the data too difficult to
be useful. In another bistatic laser scattering system [39] a short
baseline seems to have been used to measure the phase function in the
range $8^\circ \leq \theta \leq 172^\circ$ but it is not clear whether changes in illuminated
volume were considered in the analysis of the data.
Before closing this section we should mention the possibilities of satellite borne lidars for global turbidity monitoring. For example, Elterman et al. [100] have demonstrated the feasibility of such a system used in the dark hemisphere. Of course one would have to assess the cost of operating such a system on an unmanned platform versus the value of the data. Remsberg and Northam [117] have proposed the use of lidars on the space shuttle, presumably to be operated by on board observers. Their paper, available only in short summary [117], merely considers hypothetical situations to demonstrate the feasibility of a fixed, nadir-pointing system for cloud, aerosol, and molecular densities. No compelling scientific arguments are presented in support of such measurements versus ground based ones. The problem of calibration in order to discriminate between molecular and haze components is not discussed.
4. SUMMARY AND TENTATIVE CONCLUSIONS

As originally planned, this survey was undertaken in order to present a more-or-less objective and coherent picture of the state of the art (circa 1976) of certain remote sensing techniques and to offer some cogent recommendations regarding the best techniques. In practice, however, quite apart from our inability—due to various circumstances—to devote the necessary time and effort to do a thorough job, our purpose has been frustrated by the increasing volume of recent literature on the subject, a good percentage of which is of doubtful relevance and poorly presented, containing ambiguous "results" and hastily arrived at "conclusions." Of course we have also come across a smaller number of papers, of admittedly high standards in exposition and content, representing contributions of more permanent scientific value and operational potential and these we have tried to single out for special attention. However, we have also seen certain pieces of work, consisting of carefully collected and analyzed original data, with a well defined interpretation and good potential for application (see e.g. Ref. [41]), which are eminently suited for publication in archival journals but which, for various reasons, remain unpublished except as reports or in other limited circulation formats; whereas handwaving types of pieces on fashionable subjects (such as climate control), no matter how shaky their scientific justification, have no trouble finding public vehicles, thanks to questionable and arbitrary editorial policies. Thus, even more than a generation ago, there is today an unquestionable
need for the publication of succinct and germane papers with solid data obtained from carefully executed laboratory experiments and field measurements of aerosol scattering parameters such as e.g. those of Pritchard and Elliot [160], the Moscow school of workers under Rozenberg [8], Murai [45,46] in Japan, Holland and co-authors [161,162], and Quiney and Carswell [163] in Canada. Careful measurements of as many as possible scattering parameters for irregular particles [163,164] are particularly relevant since the shape factor is very important in lidar returns but their modelling and corresponding scattering theory are practically intractable.

It is in the nature of critical survey work that the clearer and more complete the exposition and presentation of results and conclusions in a given study (see e.g. Ref. [81]), the more liable it is to critical evaluation and comment. This should not detract from the quality of the work, quite the contrary. On the other hand, if we have failed to here consider or discuss certain papers such as e.g. Refs. [123,124] (except when overlooked unintentionally), more often than not this is due to our own inability to follow a muddy or incomplete or needlessly obfuscating presentation of an otherwise straightforward proposition, rather than to intentional neglect (see also Sect. 5).

It would be convenient to summarize the results of our survey by means of some sort of rating system whereby the merits or shortcomings of each technique, its potential, reliability, etc. could be evaluated. For the reasons outlined above, however, such an exercise would be impractical in this case and whatever conclusions
we have should be considered as preliminary and tentative. Nevertheless, and fully aware of the risk of its misuse or of being quoted out of context, we shall attempt a rough evaluation scheme, bearing in mind the above caveats and reservations. We shall arbitrarily assign four highly subjective and qualitative "grades" based mainly on our own judgment and "feel" derived from the perusal of the mass of cited (and some uncited) material, as follows:

A: Excellent or very high; highly recommended.

B: Very good or fairly high; recommended with some qualifications.

C: Good; recommended with significant limitations.

D: Poor or low; not recommended.

These grades are assigned in the following four categories of evaluation in the order indicated:

(1): Efficiency in coverage: global, geographical, vertical atmospheric, and diurnal or temporal.

(2): Information content on aerosol loading and properties.

(3): Reliability on the basis of theoretical justification (or mathematical inversion) and/or experimental checkout of the technique.

(4): Overall rating or level of recommendation for implementation.

For economy's sake, the various areas or sub-categories mentioned under each of the above main categories are not graded separately but are briefly discussed in the explanatory notes under each monitoring technique considered in the sections below. We first consider passive techniques.
4.1 Solar and stellar spectral extinction method (Sec. 2.1): C; B; A; B.  
Geographical coverage depends on the number of ground stations involved; no vertical discrimination for fixed platforms; diurnal and temporal coverage depends on cloud cover. Information content is high for total (vertical) aerosol loading but only fair for properties such as refractive index and microstructure. Reliability on both theoretical and experimental grounds is high.

Regardless of the existence of any other remote monitoring schemes, there should be a number of permanent observatories using this technique, established at strategic locations over the globe in both hemispheres to keep a continuous record of background turbidities. The information gathered from such observatories could be useful for various other applications such as availability and duration of sunshine for energy conversion, cloud distribution, etc.

4.2 Solar aureole method (Sec. 2.2): B; B; A; B.  
Coverage is as in 4.1 except that there is good vertical discrimination if the aureole is scanned along the sun's vertical. Measurements should be confined to several narrow spectral bands within the visible and near infrared regions. Information content and reliability are high on microstructure or particle size distribution, particularly on the large particle content, but fair on physical properties such as shape and refractive index.

4.3 A combination of 4.1 and 4.2: B; A; A; A.  
Among passive techniques, a system similar to the SAO's Bouguer-Langley or "long" method of solar spectral extinction determination,
in conjunction with simultaneous measurements of aureole brightness and gradient at various solar elevations, is probably the most reliable and economical indicator of particulate turbidity over long periods of time. We cannot overemphasize the usefulness of this combination, especially if established over a strategically distributed global network of stations, not only for recording general background turbidity levels, but also for the monitoring of unusual events or anomalies such as volcanic "dust" incursions, their geographic migrations, and time variations.

4.4 The S/H method (Sec. 2.3 and Eq. (3)): C; C; B; C.

This method has not been sufficiently investigated despite its high potential as a turbidity indicator, particularly for narrow band measurements of global skylight. Its geographical coverage would be similar to 4.1 and 4.2 with no vertical discrimination and unknown information content. There is some theoretical justification but we need more observational checks to evaluate its sensitivity to variations in the composition, amount, and microstructure of aerosols. It may be recommended as an important adjunct to the combination 4.3 since, with the proper instrumentation, it may be easily implemented at the same observing stations as 4.3. Of course measurements of the spectral character of daylight are intrinsically important, for example in studies of photosynthesis and other phyto-biological processes [122].

What we have called the diffuse reflection technique (over water) and discussed under Sec. 2.3 will not be graded here. Its use in conjunction with measurements from orbiting satellites is attractive
but the interpretation or inversion of data is fraught with difficulties because of the unknown reflectance properties of rough water. Even if known, the need to specify sea state independently severely limits the method's usefulness for the remote sensing of turbidity. Use of this technique of course precludes measurements over land areas, which are important sources of particulates.

Earth bound twilight sky brightness and polarization measurements, also discussed under Sec. 2.3, are in the same category, that is the interpretation or inversion of data is too uncertain to be of value as a monitoring technique, except for the detection of anomalies from volcanic eruptions. Satellite photometry (and polarimetry) of the earth's terminator may be a good detection and monitoring tool for mesospheric particulates such as found in noctilucent clouds. It is limited to these high level aerosols, however, and the significance or need of such information needs to be demonstrated.

We next consider active techniques.

4.5 Ground based laser radar (Sec. 3.2): B; C; B; B.

Geographical coverage similar to other ground based techniques, i.e., it depends on number of stations and of cloudless days. Vertical coverage excellent if only the stratification and relative magnitude of turbidity is of interest. (In fact in their excellent review on lasers, Collis and Russell [119] do not claim any further capability for single wavelength lidars.) Diurnal and temporal coverage is limited somewhat. Information content on size distribution and particle shape may be improved by use of multiwavelength and
polarimetric systems provided the necessary theoretical ground work is more fully developed than at present. We have graded the reliability and overall rating with a "B" or as very good mainly because the system is already in use even though a reliable inversion of the data has not been demonstrated.

The searchlight or white light method, discussed in Sec. 3.1, need not be rated as it seems to have been superseded by the lidar technique.

4.6 Satellite borne systems both passive (Secs. 2.1, 2.3) and active (Sec. 3.2): These systems, to be carried on unmanned satellites or manned space vehicles (e.g. shuttles), cannot yet be fully evaluated and they remain to be tested. Once this is accomplished, however, there is no doubt that space borne systems, bearing in mind certain limitations discussed earlier, may offer the best means for the global monitoring of particulates, at least if only their optical thickness or relative loading is of interest, but not their nature and total mass.

4.7 General conclusions and recommendations: Naturally none of the above techniques, by itself, is capable of yielding complete and reliable information on the microstructure of the aerosol population and the shape and composition of the particles. Moreover, independent checks, preferably by measuring properties other than light scattering, are needed to check the efficiency of each technique. This applies especially to the assessment of background levels away from urban centers and other known large sources and over the oceans.
An example is the measurement of air conductivity [9] and its relation to particulate loading. *In situ* sampling as a check, of course, is always welcome provided the method does not involve scattering.

Thus, it is evident that only some combination of passive and active techniques, ground based and satellite borne, will provide the most reliable and cost effective system for the remote monitoring of atmospheric particulates. Choosing the right combination of the various techniques requires considerably more detailed analysis and intercomparison than was possible within the scope of the present survey. Regardless of such a choice, however, we believe that at present it would be difficult to justify an overly sophisticated and expensive particulate monitoring system, in the absence of more solid evidence than currently available on their climatological significance. On the other hand, there is no doubt that efforts to obtain reliable information on and to monitor the ubiquitous aerosol particles *can* be justified on their own merits regardless of climatological considerations. Such information could be valuable, for example, for the verification of and further progress in our understanding of the physics and chemistry of aerosols, which already seem to be in an advanced stage, according to two recent monographs by Twomey [165] and Friedlander [166] respectively. If particulate turbidity is at all worth monitoring, it is worth looking for the most reliable and cost effective system possible within budgetary limitations.

At this stage, and without further justification than our own subjective criteria, we are inclined to favor a judicious combination of direct solar and aureole narrow band photometry, as in Sec. 4.3,
obtained at selected observation sites, in conjunction with some ground based lidar system, as in 4.5, as well as a satellite based system preferably employing passive techniques. Such a combination, in addition to information on general trends in particulate turbidity, will surely provide some information on the size distribution and physical and chemical nature of the particulates. (An unpublished piece by Shaw [159] seems to confirm our conjecture about the potential of spectral extinction combined with aureole data in recovering the size distribution.)

Finally if our view of the situation proves to be correct, there is a need for inputs by several qualified workers from contiguous fields and it may be advisable to organize a workshop (similar to ARWG [147]) on the subject of the present survey, particularly on the most efficient uses of existing and future space platforms. The problem is how to find sponsors who can guarantee the independence of the deliberations and their report from the direct or indirect influence or encroachment by the sponsors. (Unsponsored workshops and their reports, unless they are endorsed by professional societies or government agencies, tend to be generally ignored in support or otherwise of future work, as e.g. in the case of the ARWG Report [147] despite the reputation and competence of its members.)

As mentioned earlier, whatever conclusions we have reached here must be considered as tentative and subjective rather than definitive and the product of "objective" analysis and intercomparison of various techniques. One could, of course, argue about the meaning of "objective"—in the present and similar contexts—that there can be no such
thing as a truly objective inference even in the hardest of "hard" physical sciences, unless one is willing to accept the "objectivity" of computer produced results addressed to, or for the consumption of other computer systems—not humans.
In a critical survey such as this, matters being what they are, it is almost impossible not to touch upon questions which, at first sight, would appear to be somewhat unrelated to our main theme. These have to do mainly with (a) the quality and content of published papers, (b) the support and administration of science and scientists, and (c) science policy in general. According to current practice and fashion these questions should properly be discussed only by recognized establishment scientific authorities, economists, other administrators, and policy specialists, respectively. However, being of an older, less pliable generation than some of our colleagues we strive not to be swayed by fashion or self appointed arbiters in what one may or may not express in print as a scientist and human being. Furthermore, we are convinced that a discussion of questions (a) to (c) is very relevant to our subject and whatever conclusions we managed to draw from our survey. Consequently in what follows we shall attempt to give form and substance to certain feelings and thoughts we have been living with for some time now, despite the risks involved in doing so.

The poor quality and impact (or lack of it) of some of the papers surveyed here demonstrate the truism that hurriedly conceived and force financed studies undertaken in response to government agency requirements [2] in the end may turn out to be wasteful and counter-productive for the orderly research in and understanding of natural
phenomena (e.g. stratospheric aerosols). Results thus obtained may lead to ill advised science policy decisions and even premature legislative action which may later have to be withdrawn or substantially modified in the light of newer and better substantiated findings.

Equally wasteful, counterproductive as well as confusing is the current practice of multiple presentation and/or publication of a single piece of work first announced in a conference or other meeting program. It may subsequently appear, in some form, first in a "preprint volume" (the merits of preprint volumes, prepared at considerable cost and effort, are highly debatable); then in a post-meeting Proceedings volume; and finally as an article in a regular archival journal. (We tend to agree with J. W. Huxley when he says "I sometimes wonder whether the danger [of observations influenced by unverified theory] may be increased by the numerous symposia and conferences that are held nowadays. The same story is told at each, ...until on the...principle 'what I tell you three times is true' it becomes impossible even to contemplate any alternative" [144].) Furthermore some journals and their editors are increasingly lenient in enforcing certain minimum standards on the origin and validity of material cited by authors of papers accepted for publication. For example it is not uncommon to come across material cited as "in preparation" or "submitted to...," or in the form of a preprint appearing in a preprint volume, or, worst of all as "privately communicated"; all as proof—we suppose—that the author is familiar with the very latest on the subject, as if up-to-dateness or newness guarantees the quality, relevance and reliability of the material
cited (another highly debatable premise. These observations, of course, are more-or-less confined to journals and articles concerned with the subject of this survey.) We believe professional journals' editorial policies should drastically discourage such practices.

More generally, the organization and presentation of material appearing in a number of papers published in certain journals and reviewed here are sub-standard, in our opinion, due perhaps to lax editorial (and reviewers') standards. Indeed, some papers (e.g., Refs. [38,157], as published, appear as if they have escaped the review process altogether. (Certain journals show a "date received" and none for acceptance, so that the reader is left in the dark as to the length of time used in or even the existence of a peer review process. In other papers [7] the proximity of the two dates indicates no review process.) The language used in such articles becomes increasingly "iffy," indeterminate or imprecise to a degree that the reader is often frustrated in his quest for a solid statement to sink his teeth into, either in agreement or in objection (e.g. phrases such as "reasonably" or "fairly good agreement" abound and are used without qualification as to what constitutes "reasonable" or "fair"). Nor are there any signs of a reversal in this lamentable trend toward increasing use of loose and imprecise language, muddy presentation and inappropriate style. (The following sentence did actually appear in a published paper, under "discussion and conclusion": "Thus it appears possible to extending (sic) the single-scattering approach to almost all practical ranges of interest by developing phenomenologically characterized multiplying factors based upon a selective
number of Monte Carlo multiple scattering solutions" [158].) As examples we may cite a number of papers more recent than Ref. [4], such as Refs. [125,126,127], on the supposed climatological implications of volcanic particulates (and other minor constituents). In some cases [44] even glaring misprints seem to have escaped the proofreading process; in others, editorial arbitrariness is evident in format and font size. For example we have seen the use of minuscule, almost illegible, type in mathematical expressions [100] or unnecessarily large type and long numerical tables occupying several whole pages [128].

If we have lingered somewhat on the subject of style, organization, etc. of certain papers, it is because we agree with the editorial writer's [156] assertion that, whatever else it may or may not be "science is...largely concerned with communication"; and "anyone who interrupts the channels of communication...is capable of doing immediate and serious damage...to the advancement of science." Putting a somewhat different stress on this than the editorialist's, we submit that if an author and the journal's editor and editorial policies and practices do not see to it that a published paper communicates effectively, they are both remiss in their function by impeding orderly and meaningful progress in the discipline.

This situation, that is, the general deterioration in quality and proliferation in the number of papers accepted for publication even by sometime reputable and respected archival journals, may be partly attributed to the ever increasing proliferation of society meetings, conferences, and symposia or workshops on narrowly special-
ized subjects. (The number of those covering the atmospheric sciences alone is quite overwhelming, as may be verified from the announcements appearing in the professional publications. We believe it simply impossible that there should be so much new and significant research worthy of presentation to fill the agenda of so many professional meetings.) Attendance at these meetings often has to be justified by the submission of hastily prepared (or even just planned) "papers" based on on-going work without allowing adequate time for proper analysis and extraction of meaningful new results. Moreover, "meeting reports" reviewing the material presented and subsequently published in the professional journals are almost invariably laudatory in tone, seldom finding fault with the justification for convening, program content, relevance, etc. of the meeting in question (see for example Ref. [108]).

We are not, of course, alone in our above criticism of what might be termed--borrowing from the economist's jargon--a runaway inflation in the scientific content of material presented in meetings (e.g. on the subject of remote sensing of particulate turbidity and its climatological effects). Similar criticisms, in regard to biomedical research for example, will be found in a recent editorial by Cyril Comar [129]. Although his concern is mainly with the effects of bad (biomedical) science on public health, bad (atmospheric) science also "can tend to technological fixes that do more harm than good" [129] in the area of environmental effects and their control. It is therefore a matter of considerable importance to individual scientists to find ways to reverse this trend before a curtailment
of meetings and controls on the size and quality of papers submitted to journals is imposed on us by the funding agencies and by the Congress (since most of the costs of attending meetings and publishing papers are ultimately borne by the general taxpayer). The money thus saved should somehow be reinvested in the support and job security of the productive individual scientist.

Lest it be thought that we are overly zealous in our critique of the current situation in this our survey, the reader is referred to Pittock's [130] rather thorough critical review of the state of affairs in another subject in the atmospheric sciences. Perhaps it is not surprising that that author quite independently (we had not seen his article [130] until after preparing our draft manuscript; nor is it possible that Pittock could have been aware of ours at the time he must have prepared his m.s.) arrived at conclusions remarkably similar to those expressed here. To quote from his abstract: "The state of the literature in this particularly controversial area must raise doubts as to the prevailing standards of objectivity and critical analysis in other areas of science as well." And from his conclusions: "...increasing specialization leads too many scientists to accept the conclusions of others without adequate critical thought....I would plead for a more critical and rigorous examination of the literature by all scientists,...particularly for a more critical stand by the editors and referees of reputable scientific publications" [130]. With all of which we heartily agree in regard to the area covered in the present survey.

These thoughts inevitably point to the underlying more funda-
mental question of the extent and effectiveness of financial and other--explicit and implicit--support for the individual scientist (as distinct from the support of an abstract entity under the catchall term \textit{SCIENCE}) and his product. While a detailed discussion of this subject is outside the purview of the present study, a few comments are in order insofar as the material we have surveyed is understood to be authored by \textit{scientists} working both in the quasi-basic and applied aspects of science.

No doubt an ideal situation for any field or sub-field would be the publication (a) of a few scholarly, high quality pieces of work per year which are the product of well conceived, slowly gestated, and well documented and executed research programs, and (b) of critical and authoritative surveys or reviews of the field, say per decade, by working authors who enjoy their peers' maximum respect, to place the "state-of-the-art" in proper perspective. This would be essential for the health of any discipline or sub-discipline and would serve as a check or restraint on the mad rush to produce and publish hastily arrived at "results" of undigested work. The question is how to encourage and foster an atmosphere conducive to the ideal situation under present conditions. Interestingly, in his recent article on science and technology, the President's science adviser F. Press [131], while mentioning a possible 5 percent real growth in the federal support of basic science, fails to define "basic" and to address the question of how much of the increased allocation will actually go toward the support of individual science workers and how much to science administration--another kind of inflation.
Obviously good basic (and even applied) science research can hardly be conducted under the prevailing mode of support by starts and spurts. This engenders a climate of sometimes savage competition for funds whereby often the individual--and not the science administrator--is forced to use a "sales talk" style in preparing his proposals with exaggerated promises of success in obtaining his objectives, which are themselves overstated for the proposed grant or contract period. This is the "name of the game" (who invented this demeaning phrase?) and those workers who cannot adapt and learn to play "the game" under the new rules are often unfunded. How can one apply the necessary concentration to produce good, credible research work under the present--almost universal--conditions of constant pressure to submit proposals, mostly for short term support of new or continuing projects, having to invent implausible objectives based on research results to be obtained under the proposal? (Not to mention the tremendous inflation [or loss] represented by the time, energy, and money spent in the preparation of numerous proposals, only a small percentage of which are funded, the rest being destined for the waste paper disposal!) These conditions clearly will result in constantly diminishing rewards for the conscientious scientist or a relaxation of personal ethical standards in the quest for financial support (i.e. one's livelihood) and in the conduct of research. (The importance of individual scientists' personal integrity has been commented on in a 1965 note [132] of ours, and in an article by Glass [133] which essentially expands on our comments. Our prediction [132] of the possible advent of a
scientific-judicial body was almost realized in the recent proposal for a Science Court to pass judgment on controversial scientific topics; its apparent demise (thank Heaven) may be attributed to the intense opposition expressed by many thinking scientists as well as cogent arguments by a jurist [134] about the inability of an adversary-judicial process to arrive at scientific "truth."

The need for more adequate, judicious, and stable funding of science (and presumably scientists) than at present has been acknowledged by no less an author than E. Q. Daddario [150], noted champion of science in the Congress and retiring AAAS president. After asserting that "...science is not now in the best of health..." he explains that "...the government's preoccupation with...immediate solutions to demanding crises...[has induced] mission oriented agencies [to move] away from basic research and toward a banner labelled 'relevancy.'...I do feel that steady and predictable funding will avoid inefficiency and waste, which are also the consequences of sudden changes, up or down, in research budgets" [150].

Another phenomenon related to the current science support situation is the ever shorter response time to funding pressures generated by federal (Congressional or Executive) preferences or mandates on the direction in which they think scientific research should proceed. For example, we are now in the midst of an obsession with climate, climate change, and its control. It is amazing to observe how many atmospheric scientists discovered themselves (by their own account) to be covert climatologists and how much of their own work has explicit or implicit relevance to climatological
phenomena. For example, in the area of the present survey, in their
eagerness to demonstrate the importance of aerosol particles on cli-
mate—via their scattering and absorption properties—some authors
put forth the notion that a very simple criterion, such as the ratio
$\beta_s/\beta_e$ of single scattering albedo to a "backscattering extinction
coefficient" [137] could have profound climatological significance
[137,127]. As is clearly evident to any knowledgeable atmospheric
scientist and as recently demonstrated by numerical modelling and
analysis (see e.g. Ref. [70] and others) an assessment of the ef-
facts of aerosols, say on near surface temperatures or radiation
budget in the real atmosphere, is inherently a difficult task and
it could not possibly be accomplished by simplistic reasoning or
back-of-envelope arithmetic. (Interestingly the author of Ref. [127]
in a later reassessment [138] seems to have become more skeptical
of his earlier conclusions on the climatological role of volcanic
dust.)

The recent preoccupation with and overemphasis on the supposed
all-pervading influence of climate on human affairs perhaps cul-
minated in the appearance of an imaginative book [135] on the sub-
ject, addressed to the lay public, by an author who certainly is no
layman. (For a cogent and critical commentary on this book by a
well known climatologist see Ref. [136]; for less critical reviews
see Refs. [142,143].) One might ask, how did the current obsession
with climatic questions come about and why with such alacrity? How
explain the current preoccupation and anxiety (at least here and in
Europe) by that most adaptable of life forms, Man, in this most
technological of ages, about the remote possibility of undesirable climatic changes, when his prehistoric forebears survived ice ages? Obviously, if there is a demonstrable danger of initiating irreversible climatic trends by irresponsible human activity, something should be done about it provided the mechanisms involved are reasonably well understood. But, in the present "state-of-the-art," are we justified in expending enormous sums and energy on climate problems (the level of spending on climate research is slated to rise by 37 percent in the projected 1979 budget [139]) and focusing special attention on them by the creation of an interagency climate research program to formulate a National Climate Program [131]? We are rather inclined to agree with the Director-General of the U.K.'s Meteorological Office in that "the atmosphere is a robust system with built in capacity to counteract any perturbation....Sensational warnings of imminent catastrophe, unsupported by firm facts or figures, not only are irresponsible but are likely to prove counterproductive" [141].

In recent years a nefarious trend toward ever increasing remote control of scientific effort through the organization of large research programs (we might call it "centrally orchestrated" science) and levels of spending is evident, at least in the atmospheric sciences. Examples are programs and coordinated international "experiments" represented by various acronyms, such as the GARP and its attendant exercises, BOMEX, TROPEX, GATE, etc. These seem to have been mainly spawned by a few select scientists (one is tempted to call them the "scientific establishment"), somehow chosen, who emit value judgments
on the state-of-the-art in a given area without the benefit of consultation or open and public discussion and consensus on the part of rank-and-file scientists about the merits of the proposed super-programs. Indeed there is evidence of a lamentable tendency, on the part of laymen and other scientists, to accept authority—or "authoritative" studies or reviews—uncritically to the detriment of sound judgment and policy decisions. (Parallel opinions on the management of basic science in another discipline may be found in the article by Lewis Thomas, such as e.g.: "In basic science...committees cannot formulate the ideas or lay out the plans; the work can only be done in the mind of the investigator himself" [140].) The assumption behind the promotion of these grand and costly international programs—an assumption whose validity is certainly not universally shared, see e.g. Refs. [145]—is that recent progress in the atmospheric sciences and our understanding of relevant mechanisms are so advanced that all we need is a more accurate and denser set of measurements than heretofore to demonstrate existing prognostic capabilities—a pious hope! (The title and text of the press announcement [146] of the launching of the latest international program Global Weather Experiment, costing half a billion dollars, confirm this feeling.) Furthermore, as so aptly expressed by Gall (according to a review [149] of his book Systematics), on tendency of large programs and systems is the collection and storage of vast amounts of data—for their own sake—which tend to control us rather than vice-versa.

Obviously, some central planning and organization of large
experimental programs involving the atmosphere and the oceans are inevitable considering the vast proportions of the medium to be studied. Certain aspects of such "experiments" may even be justified if the corresponding disciplinary branch or sub-branch is in such an advanced stage of development that all it needs is experimental or field verification. But, according to the report [147] of an informal working group (not convened or supported by any official government agency nor professional society), at least in the area of atmospheric aerosols and their effects on the radiation budget, our knowledge and understanding was nowhere near that stage in 1972 (when the BOMEX and GATE experiments must have been planned).

So also in other branches of the atmospheric sciences, despite the impression given by the proliferation of specialized professional meetings and symposia, volume of published papers, amount of money spent, etc. And if planning, organization, and remote control and direction are inevitable under present science support constraints, at least there should be a conscious effort on the part of those responsible to prevent undue stifling of independent research by the individual scientist. (A case in point is the language and content of a so-called "A.O." [148] where not only the type of instrument, parameters, and technique of measurement but even their mode of interpretation are frozen before the individual scientist is invited to submit proposals!) As a matter of fact the individual scientist's plight is seldom mentioned in general articles on the subject of science support.

Indicative of the trend toward increasing remote control and
guidance of research is the CIAP exercise [2], mentioned in the intro-
duction. It is quite possible that some research money was wasted in
supporting hurriedly conceived and overcommitted proposals. This
should be weighed against any new insights or techniques that might
have been developed through CIAP sponsorship. In either case such
questions are intimately related to government science policy and
how it is determined. In this regard, Branscomb's discussion of the
inadequacy of present mechanisms in formulating and implementing a
national science policy are interesting. To quote: "Science is
being justified as applied research. Applied research is directed
from Washington as though it were product development" [3]. In its
quest for reliable "facts" on which to base science policy, the
government often relies on the output of ad hoc committees whose
members are appointed for their seniority, authority and prestige
(seldom on the advice or recommendation of rank and file working
peers). The committee reports then carry much weight and authority
despite their passing judgment on nebulous areas (e.g., the effect
of surface pollutants on the ozone layer) where there is no sci-
entific consensus. And as pointed out by J. Schmandt, such reports
are often "not subjected to the traditional quality control system
of the scientific disciplines....Funding is mostly outside the peer
review system" [155].

It is clear that science policy decisions significantly affect
the conduct, content, and output of the scientific endeavor. The
formulation of science related policy and its implementation should
not be left to "policy experts" alone but, for the benefit of the
nation as well as the health and progress of science, should include the active and effective participation of competent, working scientists in the process. In some cases, no policy may be the best policy.

Finally the foregoing leads naturally to another fundamental question, that of the administration of science and scientists, which is intimately connected with that of science policy and its implementation. It is quite evident to any good observer that, as noted in a recent Science article, "...bureaucratic and economic constraints are strangling the freedom of research" [151]. This increased stifling of (we would rather say) the individual working scientist's freedom of research may be partly attributed directly to over-administration by the ever increasing top hamper of administrators imposed on him. It is reaching such scandalous proportions that even the research directors themselves in "a view from the top...not the vantage point of the scientist at the bench," [151] have had to take note of it in an annual report of the National Science Board. We dare say that this condition, call it administritis, is reaching the character and proportions of an epidemic disease. According to the Science reporter the corresponding document was "transmitted to the President and the Congress, where it is apt to be widely ignored" [151] (emphasis ours). It most probably was ignored since, as of this writing (early 1979), no cognizance of or improvement in the situation is noticeable. If this "administritis" continues its unchecked growth either the quality of the scientific product and its technological derivatives will further deteriorate or we may see some kind of revolt on the part of working scientists against the
growing burden of science administration at all levels, or both.

For the creative young scientist with ambitions for recognition
and some material advancement the administrative ladder becomes his
only option. The position of those who opt to continue as working
scientists is made all but untenable within a demeaning and humiliat-
ing atmosphere (reminiscent of George Orwell's Animal Farm [152])
created by a pyramidal management hierarchy busily spawning ever
more useless red tape and paperwork and constantly eroding the
worker's usable time while justifying its own existence. (This
situation is probably being duplicated elsewhere in the West or at
least in Britain according to comments attributed to B. J. Mason
[153].) In certain cases it is not unusual for employees of service
departments (accounting, publications, library) to enjoy better job
security than staff scientists who are constrained to attract outside
support for their own survival. To add insult to injury, the latter
seem to be the lowest figures on the totem pole and hence the target
of numerous memos, directives, procedural rules, etc. emanating from
the top management all the way down to service department function-
aries. (These sentiments are almost exactly echoed in a recent article
[167] on obsolescence in R&D organizations we found by chance in a
journal on industrial research management.) No doubt as the ratio
administrators/scientists keeps increasing the time will come (if it
is not already here) when universities will be conferring degrees in
Science Administration to individuals with no scientific background.

When the individual scientist's ability to freely and objec-
tively examine and criticize is abridged or squelched, as is in-
creasingly the case within both federal and outside (non-profit as
well as for profit) research departments or organizations, due partly to acute "administritis" and other factors, a vicious circle is initiated. This will inevitably affect the vitality and productivity of scientists as a group or individually. A very interesting discussion of this and other aspects of the productivity of scientists within pyramidal research organizations will be found in Argyris's 1968 article [154] whose implications, despite their relevance, alas, do not seem to have impressed the administrators of research organizations.

In the final analysis, one of the main attributes of robust health in any scientific discipline (or sub-discipline) is a vigorous and sustained autocritical activity. In the atmospheric sciences, at least in those branches related to our subject, such activity--if it exists--is not sufficiently evident to be effective. The present essay is an attempt in this direction, undertaken in the hope of improving the health of our discipline rather than as destructive criticism.
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16. For convenience, economy of space, and consistency we will generally adhere to the terminology, definitions and symbols introduced in an earlier monograph [11] on polydisperse scattering.


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