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An Assessment of Ground-Based Techniques for Detecting Other Planetary Systems
Volume I: An Overview

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An Assessment of Ground-Based Techniques for Detecting Other Planetary Systems
Volume I: An Overview

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FOREWORD

When I first heard of the present Workshop, I experienced mixed feelings. On the one hand, the prospect of detecting with certainty even a single planet beyond the Solar System is intrinsically an exciting one. On the other hand, there has in the past been a tendency to seek to justify a search for extrasolar planets largely through heuristic arguments, in the absence of a sound technical foundation.

This volume of the Workshop's proceedings should prove decisive in dispelling such misgivings. The participants include outstanding instrumentalists and observers, and the resulting report is clearly reasoned, with a wealth of careful thought and study. I believe that the recommendations forwarded here--that efforts to detect extrasolar planets be continued and strengthened--should receive the prompt and serious attention of the scientific community.

Few astronomers would be likely to take issue with the idea that <u>some</u> effort be expended in this direction. However, in view of the many competing claims on the research funds available, the questions of <u>how much</u> effort should be expended and <u>when</u> become critical ones. The answers depend on one's assessment of the chances of success, of the significance of the findings (whether positive or negative), and of the long-term prospects for more detailed observations of any planetary bodies that are detected. Personally, I feel that discovery of planets beyond the Solar System would indeed, as argued in this volume, open up new vistas in astronomy, offering the possibility of checks on our theories of stellar and planetary formation and carrying implications for studies of the likelihood of life elsewhere. In view of the greatly increased precision of measurement envisioned for the program, even negative results would appear to be of interest.

We should be grateful to David Black and William Brunk for organizing the Workshop and for editing the proceedings, an effort certain to stimulate further activity in this field.

George B. Field
Director, Center for Astrophysics

Cambridge, MA
November 1979

PREFACE

The Workshop on Ground-Based Techniques for Detecting Other Planetary Systems was established in the autumn of 1978 at the request of the Office of Space Science of the National Aeronautics and Space Administration. The purpose of the Workshop was to assess the feasibility of a program to detect and study other planetary systems, and to identify and assess various ground-based observational techniques that could have application to such a program. Two meetings were held, the first from October 24-27, 1978, in California and the second from January 29-31, 1979, at the University of Maryland. The three-month interval between the Workshop meetings was established to allow time for Workshop members to prepare position papers on selected topics. The Workshop involved individuals with expertise in the various candidate observational techniques; instrumentation; and factors that could limit accuracy, such as the structure and astronomical effects of Earth's atmosphere. The number of Workshop participants was large enough to represent diverse points of view while remaining small enough for uninhibited dialogue.

The current funding situation for basic science research in general, and astronomy in particular, is not conducive to the initiation of major, expensive new endeavors. An interesting outcome of the Workshop is that, although the discussions were not restricted as to the dollar level that might be available for supporting a planetary detection program, the high-priority program recommended here is relatively inexpensive.

We express our gratitude to our fellow Workshop participants without whose sincere and dedicated efforts this report would not exist. We acknowledge particularly Bob Howard and Mike Mumma, who served as study group leaders and thereby helped to motivate and focus critical discussion on the principal issues.

One conclusion that became apparent early in this study was the uniformly great enthusiasm of the participants for a planetary detection program. Although some of the Workshop members had expressed interest in this activity prior to the Workshop, that was not used in the selection of Workshop members. It is thus significant that everyone originally invited to participate accepted enthusiastically. At the end of the Workshop, each member was asked to write down his own feelings regarding the significance of a planetary detection program. The responses are enlightening and some representative examples are given in appendix B.

Finally, some personal thoughts--there are certain endeavors in which man's ability to obtain knowledge of the universe in which he lives and to gain a better perception of his relationship to that universe matches his need for such knowledge and perception. A program to search for other planetary systems seems to be such an endeavor. The advances in scientific understanding on a variety of fronts which would result from this program are immense. The results of the search itself, be they positive or negative, will have an impact on man's perception both of himself and of his relationship to his environment that will persist well beyond the foreseeable future. To the extent that the activities and thinking chronicled here play even a small role in this endeavor, we are both proud and eager to continue.

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CHAPTER 1 -- INTRODUCTION

The study of planets has played a central role in the history of astronomy. The large apparent motion of planets within the solar system relative to the "fixed stars" made them particularly interesting to both early astronomers and religious leaders. The study of those bodies continued to comprise a major part of astronomy until large telescopes and modern technology made it possible to explore the heavens to much greater distances. This new capability introduced a trend in which progressively less observational emphasis was given to planets. This trend continued until *in situ* studies of other celestial bodies became a scientific reality and not solely a plaything of science fiction.

The intense focus of national pride and resources in the space program led to significantly increased research in many existing, but often unrelated areas of study, such as meteoritics and star formation. It also led to an enormous increase in the number of young people that chose astronomy and related fields as their vocation. This infusion of money and manpower (the number of Ph.D. degrees awarded in astronomy in 1969 was nearly 10 times the number awarded in 1959) has shown, among other things, that the formation and early evolution of the solar system involve phenomena of general astronomical interest. The growing appreciation by astrophysicists that studies of the solar system may yield valuable insight into other astrophysical problems has led to renewed interest in the formation and evolution of planetary systems.

There is currently more interest in and study of planetary objects than there was two decades ago. However, until recently, there was very little examination of how other planetary systems could be detected and studied. The first step toward such an examination was a scientific workshop on a Search for Extraterrestrial Intelligence (SETI) sponsored by Ames Research Center. The consideration given by that workshop to other planetary systems was limited primarily to reviewing the current observational evidence. As an adjunct to the SETI workshops, a separate workshop was convened in 1976 under the direction of Jesse L. Greenstein and David C. Black. The Greenstein-Black workshop considered a range of possible techniques for detecting other planetary systems, as well as their potential difficulties. That workshop considered both ground-based and space-based techniques and was concerned with a general overview of the problem. A third study, Project Orion, was conducted during the summer of 1976. That study, sponsored by Stanford University and Ames Research Center, was a systems design analysis of a ground-based astrometric telescope that has a *theoretical* accuracy some 30-50 times better than is currently obtained.

A concerted effort to detect other planetary systems is a logical extension of the current exploration and study of the solar system. A major goal of the National Aeronautics and Space Administration, and of many scientists, has been and is to understand the *origin* of the solar system. This goal is likely to remain unachieved if we do not obtain quantitative information regarding characteristics (e.g., frequency of occurrence, dependence on stellar type) of planetary systems as a general phenomenon. In the absence of information against which we can compare theoretical constructs, we can only speculate. The situation is like trying to understand the evolution of the

Sun without data concerning other stars. Present understanding of the general features of stellar evolution is on a firm basis only because it derives from observational data on many stars. An effort to detect other planetary systems could provide a breakthrough in the understanding of such systems comparable to that afforded by the H-R diagram in the understanding of stellar evolution.

There is no firm evidence regarding the formation and evolution of planetary systems. Most current hypotheses assume that the formation of planets around young stars is a natural consequence of star formation. Observations to test this concept or to provide the foundations for alternatives would result from a search for other planetary systems.

Detection of other planetary systems is difficult. The barycenter (center of mass) of the Sun-Jupiter system is displaced from the photocenter of the Sun by $\sim 5 \times 10^{-3}$ AU, which, viewed from a distance of 10 parsecs, corresponds to an angular displacement of 5×10^{-4} arcsec. Current astrometric accuracy is $\geq 20\text{-}30 \times 10^{-4}$ arcsec. Similarly, the orbital speed of the Sun about the Sun-Jupiter barycenter is ~ 13 m/sec. Current radial velocity observations typically have accuracies ≥ 200 m/sec.

Other important observations would be possible with the unique and powerful instrumentation developed for a planetary detection program. For example, second-order tests of general relativity and other gravitational theories could be performed using astrometric observations accurate to the 10^{-6} arcsec level; radial velocities with 10-m/sec accuracy would yield significant advances in knowledge concerning the frequency and nature of binary systems, as well as other stellar pulsations (and hence stellar structure).

The study reported here concentrated on ground-based techniques for detecting other planetary systems, with emphasis on their feasibility and limitations, the level of accuracy at which these limitations would occur, and the extent to which they can be overcome by new technology/instrumentation.

The principal conclusions and recommendations of the Workshop are summarized in chapter 2. A detailed discussion and evaluation of possible techniques, which forms the basis for these conclusions and recommendations, is set forward in chapter 3 as a proposed high-priority program. The report concludes in chapter 4 with some thoughts on the possible implementation of this program. Several position papers by Workshop members comprise Volume II of this report.

There is one theme common to the Greenstein-Black Workshop, Project Orion, and this Workshop: The technology exists to undertake a search for nearby planetary systems. Our ability to answer the question of whether other stars have planetary systems is limited only by our willingness to invest time, thought, and money.

We thank Doug Currie, his secretarial staff, and the Center for Continuing Education at the University of Maryland for their hospitality during the second Workshop meeting. We are especially grateful to Vera Buescher whose meticulous administrative attention allowed the Workshop members to concentrate on the task at hand. Many colleagues who did not participate directly in the Workshop have made valuable suggestions and criticisms of our study.

CHAPTER 2 -- PRINCIPAL CONCLUSIONS AND RECOMMENDATIONS OF THE WORKSHOP

This Workshop evaluated ground-based techniques for detecting other planetary systems, with specific emphasis on:

(a) Physical and technological limits to the accuracy of detection techniques.

(b) Use of existing and planned telescopes to detect other planetary systems, as well as possible improvements in such telescopes.

(c) Studies to provide critical information, currently either incomplete or unavailable, which is required to assess ground-based search techniques.

This chapter presents the principal conclusions and recommendations of the Workshop. More detailed technical discussions are given in chapter 3 and in the position papers in volume II. Individuals with diverse backgrounds and interests normally generate equally diverse conclusions and recommendations. However, the participants in this Workshop reached a rather clear consensus on the conclusions and recommendations set forward here.

PRINCIPAL CONCLUSIONS

Among the conclusions reached during the Workshop, six are particularly significant.

1. <u>A scientifically valuable program to search for other planetary systems can be conducted with ground-based instrumentation</u>.

Although the Workshop members felt that an optimum detection program must eventually involve space-based instrumentation (see conclusion 5), they also felt that a detection program involving solely ground-based instrumentation would yield scientifically valuable results. This assumes that the ground-based techniques that appear most promising at present, those involving astrometric and radial velocity observations, can be developed to achieve the maximum possible accuracy[1] (see conclusion 2) which, in some instances, is more than an order of magnitude better than the accuracy of current observations.

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[1]It is important to clarify what is meant by the terms "accuracy" and "precision." Precision is a measure of the internal error in a measurement--errors inherent in the measurement process. The standard deviation of the mean value for many measurements of <u>identical</u> systems is one estimate of precision. Accuracy is a measure of the <u>correctness</u> of a measurement, the extent to which the measured value departs from the true value. Generally, accuracy is determined by measuring a quantity (a standard) whose true value is known <u>a priori</u>. When a true value is not known <u>a priori</u>, accuracy is usually determined by comparing a measured value with the value determined by measurements with a variety of <u>independent</u> instruments or techniques.

2. Significant gains in the obtainable accuracy of existing ground-based detection techniques can be obtained with modest application of current or near-term state-of-the-art technology.

Concerning astrometric systems, the Workshop concluded that photographic plates are a major factor limiting astrometric accuracy. Replacement of the photographic plate by some photoelectric system would yield a significant improvement in accuracy. The extent to which Earth's atmosphere will limit the accuracy of astrometric systems depends on the particular type of system employed (e.g., a single clear-aperture system or an interferometer). A lower limit of $\sim 10^{-4}$ arcsec per yearly normal point, due to the atmospheric effects, seems obtainable. This limit is lower by a factor of 30 to 50 than the errors of most current astrometric studies.

Concerning radial velocity systems, the Workshop concluded that there exist few, if any, technological barriers to the attainment of observations accurate to a few meters/second. The recent development of high-quality light pipes makes it possible to obtain a reproducible uniform stellar image (a limitation to the accuracy of previous systems) and to minimize errors due to flexure (using light pipes, a radial velocity system can be positioned on the observatory floor). It was concluded that Earth's atmosphere does not pose any significant limitation to the accuracy of radial velocity systems.

3. Existing telescopes, although not ideal for various technique-dependent reasons, are not currently a limiting factor for ground-based techniques: The availability of observing time on existing telescopes is inadequate.

The Workshop concluded that present accuracy levels are not limited by existing telescopes. However, the Workshop also concluded that if astrometry and radial velocity observations are to achieve their ultimate accuracy, new telescopes must be constructed. The need for new telescopes is particularly acute for astrometry. The Workshop estimated that the need for these new facilities would occur within 2 to 4 years.

4. None of the currently planned spaced-based systems is adequate for a comprehensive detection program.

Although this Workshop's primary concern was to assess ground-based techniques for detecting other planetary systems, it was felt that this assessment should be cast against the broader backdrop provided by currently planned space-based systems. The two space-based systems discussed in this context were the space telescope (ST), both for use as an astrometric system and as a direct imaging system, and the European Space Agency's proposed "Hipparchos" astrometric satellite. At the time of this writing, ESA's system was not an approved mission, but was under serious consideration. The space telescope was judged highly unlikely to image planets in other systems directly, even with any reasonable modification to the system. The astrometric capability of the ST was judged to be $\sim 10^{-3}$ arcsec. Comparable accuracy is attainable using existing ground-based telescopes with photoelectric detectors. The ESA system is nominally intended for parallax work on many stars, and is thus not as accurate for relative positional work as it might be. Its accuracy is estimated to be $\sim 10^{-3}$ arcsec, comparable to that of the ST. Also, its mission lifetime would be short ($\sim$2 years).

5. A comprehensive program to detect other planetary systems must utilize a multiplicity of techniques and of instrumentation for each technique.

The Workshop members agreed unanimously that, because of the inherent difficulty of the observations, and because of the great importance of any result from a program to detect other planetary systems, that program must use multiple instruments and techniques. It was felt that detection by one technique, let alone one instrument, must be corroborated and supplemented by other instruments/techniques. An additional strong reason for this conclusion is to optimize the scientific return from the observations. Different detection techniques can, in principle, tell different things about any discovered systems (e.g., astrometric studies yield a planet's mass; infrared studies yield a planet's temperature). The Workshop concluded that this type of scientific optimization is invaluable.

6. A comprehensive effort to detect other planetary systems will yield invaluable scientific results.

A major conclusion of the Workshop was that a comprehensive effort to detect other planetary systems would yield invaluable scientific data. Once a search for other planetary systems has been carried out with the best instrumentation possible, we will be able to state categorically whether some other stars have planetary companions similar to those in the solar system.[2] Also, the instrumentation that must be developed for a comprehensive detection effort will be 1 to 2 orders of magnitude more accurate than current instrumentation. That there will be significant scientific "spinoff" from such a quantum increase in instrumental capability is a certainty; only the nature of that "spinoff" remains unknown.

PRINCIPAL RECOMMENDATIONS

The Workshop identified a number of activities/studies that are essential to a comprehensive effort to detect other planetary systems. We present here only those recommendations that involve significant instrumentation and/or manpower and that are high priority. A more complete discussion of the Workshop recommendations is given in chapter 3. The four principal recommendations are given below in the order of their relative priorities. Recommendations 2 and 3 were judged of equal priority.

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[2]An astrometric system with an accuracy of 10^{-4} arcsec would detect (signal/noise ratio $\sim$ unity) Jupiter's effect on the Sun at a distance of 50 parsecs, or Saturn's effect at a distance of 30 parsecs. A radial velocity system with an accuracy of 3 m/sec would detect the effect of Saturn on the Sun.

1. <u>High-accuracy radial velocity studies of solar-type stars (i.e., main sequence spectral class F-K) should be carried out with existing telescopes.</u>

Until recently, the accuracy of typical stellar radial velocity studies was a few hundred meters/second, inadequate for detecting other planetary systems. However, as noted above (conclusion 2), new technology (e.g., optical fibers for image scrambling) and radial velocity systems capable of an accuracy of 10 m/sec may make possible an observational program potentially capable of detecting other planetary systems. Although the Workshop felt that development and testing of other radial velocity systems should also have high priority (see recommendation 4), it felt that an observational program initiated now could produce valuable scientific information for a planetary system detection program, specifically, (a) a better characterization of the frequency of binaries among solar-type stars with emphasis on the mass distribution of secondaries and (b) an assessment of the magnitude and frequency of intrinsic stellar variations that could masquerade as planetary systems (see also recommendation 2). Item (a) is needed because it is unclear whether planetary and binary systems are two different outcomes of a similar set of physical processes or whether the evolutionary path of a planetary system is fundamentally different from that of a binary system. The recommended high-accuracy studies should provide a much better empirical basis for comparing the respective evolutionary paths. Item (b) is important because it is unknown whether solar-type stars show periodic or quasi-periodic intrinsic atmospheric variations in radial velocity of several tens of meters/second. Such variations, if long period, could pose major interpretation problems for attempts to detect planetary systems by means of radial velocity studies. The recommended observing programs would reveal such intrinsic variability with periods comparable to or less than the duration of the observing program.

2. <u>Observational studies of the radial velocity variations of disk integrated sunlight should be carried out.</u>

As noted above, a major uncertainty in the interpretation of radial velocity searches for other planetary systems is the extent to which stellar atmospheres are intrinsically variable. The Workshop felt that this issue was of sufficient importance that (in addition to the stellar studies set forward in recommendation 1) a study of the radial velocity behavior of disk integrated sunlight should be pursued. The Sun provides an obvious benchmark and an abundance of photons. Although there have been many studies of the motion of regions of the solar surface, there have only been limited studies[3] of this type made in integrated sunlight (analogous to observations of other stars). As it is relatively easy to measure the radial velocity of integrated sunlight to an accuracy of 5-10 m/sec with existing equipment, this valuable observing program was strongly endorsed by the Workshop.

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[3]These studies have been conducted primarily by Brooks and coworkers. The limitations arise primarily from a lack of observations over a complete solar cycle and from the use of only one spectral line.

3. A program employing speckle interferometric techniques for astrometric observations should be initiated to search for planetary companions to binary stars.

McAlister suggested in 1973 that speckle interferometric techniques may permit the separation between binaries to be measured with high precision ($\sim10^{-3}$ arcsec). The Workshop concluded that the potential precision of such observations may be much greater, approaching 10^{-5} arcsec. An optimum program would involve an on-line, real-time, speckle system, which could monitor some 60 to 100 nearby binary systems. (Present speckle systems suffer from analog reduction of photographic data, which strongly limits the number of systems that can be studied.) This proven observational technique has both extremely high precision and the ability to study objects that are difficult to study with classical astrometric techniques. The Workshop strongly recommended this type of program for the immediate future.

4. The development and testing of new instrumentation for detecting other planetary systems should be carried out as soon as possible.

As is evident, there is a need for new instrumentation and facilities (telescopes) if a ground-based detection program is to be carried out at the limits of observational accuracy. This need is most acute in astrometry. The Workshop strongly recommends that alternative detector and telescope concepts be developed and tested sufficiently to provide both a basis for decisions regarding construction of new telescopes and a reliable data base for analyzing suspected planetary systems. If a ground-based program is to proceed in a timely fashion, these developmental studies should begin as soon as funding is available. A detailed discussion of the various alternative detector and telescope concepts considered by the Workshop is given in chapter 3 and in the position papers (vol. II).

The Workshop members felt that the above recommendations form the core of a sound program to detect other planetary systems. Two other recommendations were judged to merit special mention:

- To undertake a rigorous examination of sources of error in data reduction
- To define the site characteristics required for new telescopes and then to initiate a site survey

CHAPTER 3 -- A HIGH-PRIORITY PROGRAM

This chapter contains an expanded discussion of a program based on the Workshop recommendations. The program could be accomplished in four years with $0.5 million/yr. The goal of this four-year program is to provide both scientific and technological details needed before the next phase of a detection program, particularly for new instrumentation. As much of the money is for instrumental development and testing, a major reduction in funding early in the program would extend it beyond four years.

The material below summarizes the working group discussions, as well as the position papers prepared by various Workshop members on specific topics. Those position papers that can stand alone are found in volume II. The program outlined here is organized by observational technique (discussed in alphabetical order). A summary of this program is given at the end of this chapter.

ASTROMETRY

The technique that appears to be least susceptible to ambiguous data interpretation is astrometry. There is very little sensitivity to the orientation of a planetary orbit relative to the line of sight so long as the orbit is not highly eccentric. However, astrometry appears to be the detection technique for which the current accuracy is farthest from its potential accuracy (single-aperture interferometry is an exception). This limitation is due primarily to the technology used. Thus, the Workshop recommends the development and testing of new instrumentation for astrometry. The limiting accuracy (determined, for example, by Earth's atmosphere, field of view, and photon noise) of astrometric techniques varies with technique, but 10^{-4} arcsec per yearly normal point can be obtained.

Single-Aperture Interferometric Astrometry

One of the principal recommendations of the Workshop is the immediate use of single-aperture interferometry (SAI) (see vol. II, P1[4] for details) to search binary stars for evidence of dark companions. Such evidence would take the form of periodic variations in the relative separation of the components. Two forms of SAI, namely, speckle interferometry and amplitude interferometry, are proven techniques. Instrumentation to begin a pilot program currently exists, thereby avoiding significant instrumental development. In the next few years these methods should be used to demonstrate the accuracy possible. However, an intermediate-sized telescope (~2 m in diameter) is needed to study a suitably large sample of target objects. Yearly accuracies of 10^{-4} arcsec should be attainable for relative binary positions, with precision of 10^{-5} arcsec in a few cases (10^{-5}-arcsec accuracy would make it possible to detect Sun-Jupiter systems at a distance of a few hundred parsecs). There are more than 100 suitable binary systems within 20 parsecs of the Sun.

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[4]The position papers in volume II are referred to as P1, P2, etc.

The major uncertainty concerning planets within binary systems is whether planets can form in such systems. Harrington has shown that there exist stable planetary orbits if the binary separation is larger than the planetary orbit by at least a factor of 4. Moreover, studies of triple star systems seem to show that the ratio of orbit sizes is consistent with the stability criteria. Binary stars are certainly possible sites for planets and should therefore be studied. Indeed, SAI is the best method for astrometric studies of close binaries (separation $\lesssim$0.5 arcsec). The method is therefore desirable for statistical completeness. An important astrophysical result from an SAI study of binary stars would be a large increase in our knowledge of binary mass functions.

The SAI techniques discussed here use very short exposure samples of starlight passing through the atmosphere. In this short exposure regime, atmospheric turbulence is "frozen," helping to circumvent to a great extent atmospheric limitations to the achievable accuracy, and any conventional telescope behaves like a multiple-aperture interferometer. From such data, information on spatial scales comparable to the theoretical diffraction limit of the telescope can be extracted. Consequently, high-precision measurements are possible. The method is limited by the fact that light from a program object and a comparison star must pass through the same column of turbulent atmosphere, requiring that stars be separated by $\lesssim$5-10 arcsec, a limitation met generally only by binary stars. This limitation arises because the apparent motion of stars (due to atmospheric effects) is highly correlated only if their angular separation is small. Any decorrelation significantly degrades the achievable accuracy.

The SAI techniques have already produced observations of binary star separations precise to a few milliarcseconds and stellar angular diameter observations repeatable to a similar level. Recent advances make it possible to obtain considerably more data with better accuracy. Multiple detectors for amplitude interferometry and fast digital detectors for speckle interferometry make it possible to observe faint stars, perhaps as faint as +16th magnitude. These instruments are currently available and may be mated to virtually any large telescope.

The recommended SAI pilot program would demonstrate whether the accuracy suggested by theoretical estimates can be attained. The pilot program should focus on 10 or more selected binary systems. The results would be compared with other astrometric and radial velocity data. This program requires several nights per month on a telescope in the 1.5- to 2-m class. Although somewhat small, the Cloudcroft, NM, 1.25-m telescope may be available for this program.

Following this pilot program, it would be desirable to have a telescope at least partially dedicated to SAI. As shown in P1, a modest quality telescope in the 2-m class would be ideal. To obtain the desired accuracy requires about 50 five-min observations per year on each star. Such a telescope would also prove useful for other aspects of a planetary detection program, such as the radial velocity search. As many of the same stars as possible should also be observed with radial-velocity methods for comparison. The SAI studies would require about 40% of the available time of the facility. Due to possible errors arising from photometric effects, it would be desirable to have simultaneous photometric monitoring of the program stars.

One other desirable instrumental development in terms of SAI is an on-line, real-time digital system for speckle interferometry. Such a system is within the capability of current array detector and computer technology and is essential to expand the speckle program to include several hundred stars. On-line digital systems currently exist for amplitude interferometry.

Both speckle interferometry and amplitude interferometry have desirable features. The two methods are independent and complement one another. Speckle interferometry is cheaper and may work better in bad seeing. Amplitude interferometry has a better precision/unit aperture size and requires simpler data-reduction procedures. We recommend that both methods be pursued, as comparison between the two provides independent checks on the results.

SAI methods are probably less expensive and easier to implement than the other methods of astrometric study considered here. However, they can be used to study only binary star systems, a class of objects that may not have planetary systems. Moreover, there is at present no way to determine which component in the binary has the planets if they are detected. Nevertheless, they would provide data on an enormous sample of target stars not readily studied by other astrometric methods.

The scientific return from the proposed SAI observations is noteworthy. Binary star orbits are astrophysically important for several reasons. Their orbital parameters can be combined with spectroscopic and photometric results to provide stellar mass-luminosity relations. These data are fundamental for all stellar physics. Moreover, even if planets do not exist in binary star systems, complete binary mass function results are essential for star-system formation theory. Any theory correctly explaining planetary system formation must also explain binary mass functions and distributions, if planetary and stellar systems are both by-products of the same process.

Classical (Single-Aperture) Narrow-Field Astrometry

This technique has been the backbone of astrometric studies. Unlike the SAI techniques which use a pair of closely spaced (in angular separation), physically associated stars to define a measurement metric, narrow-field work uses a background of stars physically unrelated to the star of interest (target star) to define the measurement metric. As the angular separation between these fiducial stars and the program star generally greatly exceeds 10 arcsec, atmospheric turbulence decorrelates their apparent motions. The observer must model the effects of Earth's atmosphere on the apparent position which, in turn, requires the use of enough reference stars to determine the model parameters. Until recently the general practice in astrometry has been to use a small number of reference stars (3 or 4). This practice is adequate for accuracy at the 10^{-2}-arcsec level, but is insufficient for studies at the 10^{-3}-10^{-4}-arcsec level. A detailed discussion of the limitations on classical astrometry due to Earth's atmosphere is given in P2. To summarize the findings of that discussion, we note that an accuracy of

10^{-4} arcsec per yearly normal point does seem achievable using classical techniques and roughly 30 reference stars. The achievement of that significant gain in accuracy, however, is likely to be realized only through the use of new technology, in both detectors and telescopes.

Detectors--The Workshop identified five characteristics that should be embodied in an ideal narrow-field astrometric detector, namely,

(D1) High spatial resolution.

(D2) High quantum efficiency.

(D3) Freedom from sensible systematic error.

(D4) High precision.

(D5) Ability to provide concurrent records of a sufficient number of reference stars to allow optimum use of the optical system.

The tried and true "detector" of astrometry is the photographic plate. This detector has desirable archival properties, but recent studies indicate that it is a major error source at the level of accuracy required for the detection and study of other planetary systems. The deleterious effects on accuracy stem not only from the intrinsic properties (e.g., emulsion shifts and nonlinearity) of the photographic plate, but also from the need for an additional manipulation involving measuring machines. A measure of the accuracy attainable on a high-quality telescope, such as the U.S. Naval Observatory's 1.55-m telescope at Flagstaff, is that a good quality plate with two exposures yields positions accurate to 0.010 arcsec (s.e.). This error includes contributions from measuring errors and from emulsion shifts. The use of finer grained emulsions (e.g., III aJ) and improved measuring machines may increase the attainable accuracy by a factor of 2 to 3, but seem unlikely to yield the gains of a factor of 10 to 30 that appear to be realizable with photoelectric detectors. We do not suggest that further attempts to improve the accuracy of photographic astrometry be ignored; the photographic plate may still have a role to play. Rather we suggest that emphasis be placed on the development and testing of photoelectric detectors that could provide a quantum increase in accuracy.

Recent advances in photoelectric detection and inexpensive computers have fostered several proposals for new detectors. These fall into two groups--those that measure the position of each reference star and those that seek to reduce the so-called "cosmic error" (errors introduced, for example, by motion of the reference frame stars) by sensing the average position of a very large number of faint reference stars. The first of the latter was developed and tested by van Altena with moderate success in obtaining precisions similar to those obtained with the photographic technique. More recently, Connes has proposed a more sensitive instrument (P3), with provisions for seeing and guiding, that can sense the average position of hundreds of 17th, 18th, and 19th magnitude stars for comparison with the photocenter of the program star. This system would be mounted on a specially designed telescope that would be free of sensible systematic errors.

The first operational detector in the second group was the so-called AMAS developed by Fredrick and coworkers. The AMAS employs a single photometer in conjunction with a rotating Ronchi ruling. Unfortunately, the need to deconvolve the position of the individual reference stars from the complex signal obtained by viewing the entire field with a single photometer through a rotating Ronchi ruling limits the system to a determination of the positions of the brightest stars in the field. Thus, compliance with condition (D5) is marginal. The most recent proposal in this group is the multichannel astrometric photometer (MAP) developed by Gatewood and coworkers (P4). With this system, the number of reference stars is one less than the number of independent photoelectric channels. While this is a more costly approach than the other systems, it is conceptually simpler and it retains all the dimensional information available for each star. It also makes it possible to model and remove the sources of the "cosmic error" by reducing the motions of each star to a mean defined by the group.

The photoelectric detectors discussed above involve one or more photomultipliers suitably located in the optical system. The increasing development and use of array systems for many astronomical applications raises the question of whether such systems might have a role in ground-based astrometric studies.

The charge coupled device (CCD) and charge injection device (CID) are photo-engraved arrays of pixels (picture elements) on a silicon chip (or other semiconductor). Typical CCD's now available have between 10^4 and 10^5 pixels on an area smaller than 1 cm^2, but special units are under development with as many as 6.4×10^5 pixels (800 x 800) in a 12 x 12 mm area, in which case the pixels are 15 x 15 μm square. Photo-response is fairly linear, the peak quantum efficiency is high (~50%), the spectral response is broad (0.4 to 1.0 μm), and each pixel is capable of accumulating up to 10^5 charges without serious spillover. To read out the image, these stored charges are transferred, pixel by pixel and line by line, to an output electrode by means of a sequential voltage pattern applied to a grid of conductors on the chip.

For astrometry, there is an optimum relationship between pixel size and the width of the point-spread function of a star image, so there is an optimum focal length for a telescope feeding a CCD (or CID). A star image profile that is too small in comparison with pixels will not be adequately sampled for precise centroid determination, whereas a magnification that is too large will undesirably reduce the number of comparison stars falling within the field of the CCD. For CCD's with 15-μm pixels, the appropriate focal length of a ground-based astrometric telescope would be about 10 m. The centroid error should then approach the theoretical limit, $1.77a/\sqrt{N}$, where a is the core radius of the point-spread function (tremor disk) and N is the total photoelectron charge in the star image. With a 2-m telescope, an 18th-magnitude star will produce 10^5 charges in a 1000-sec exposure, and the centroid accuracy should approach 3×10^{-3} arcsec (0.01 pixel) in good "seeing."

Although astrometric program stars will typically be brighter than 18th magnitude, the small field covered by the CCD (4 x 4 arcmin in the foregoing example), coupled with the need for many reference stars, requires the use of reference stars down to 18th or 20th magnitude. The greater quantum efficiency of photoelectric detection implies that fainter stars can be used for reference. For example, the error due to photon statistics in observations of an 18th magnitude star with a 1-m aperture is less than that due to seeing. Thus, a photoelectric survey could employ reference stars 4 to 5 magnitudes fainter than those that would normally be employed in a photographic program using the same aperture. This means that a smaller field can be used, thus reducing instrumental error; the reference stars will generally be farther away, thus reducing the likelihood of undetected sources of "cosmic error."

An additional strong argument for the capability to observe to faint magnitudes accurately is the magnitude distribution of candidate stars (see fig. 1, provided by W. Baum). The figure indicates those known candidates for which the half-amplitude of positional wobble due to a planet of Jovian mass and orbital size would exceed 0.003 arcsec. Nearly all fall between 8th and 18th magnitude, with a median at about 13th magnitude. These are the candidates to choose if planet detectability is the sole consideration, without regard to the time required. To include brighter stars in a planetary search, one needs to settle for less amplitude of wobble, but shorter periods can be expected. The point here is that photoelectric detectors offer an efficient way to study faint stars, and that may ultimately be required.

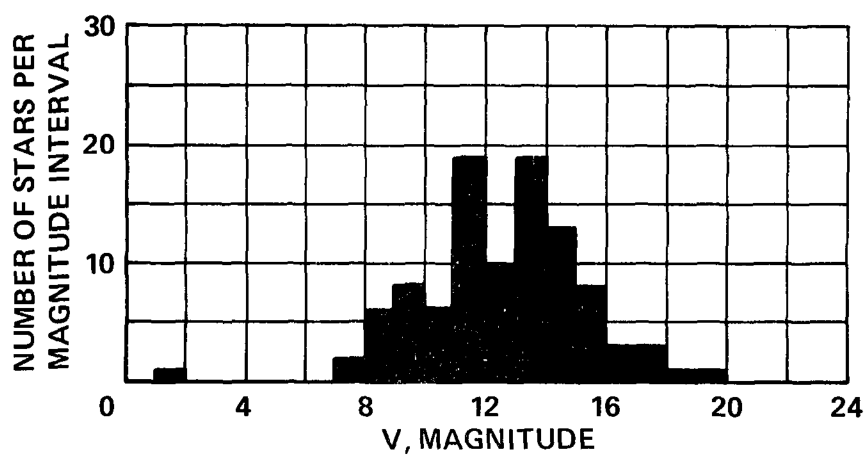

Figure 1. Magnitude distribution of Gliese stars for which the half-amplitude of positional wobble due to a hypothetical "Jupiter" would exceed 0.003 arcsec (provided by W. Baum).

Telescopes--The greatest part of narrow-field astrometric work has been done with old refractors, few of which were designed for that purpose. Some work has also been done with Cassegrain and Ritchey-Chretien reflectors, but again they were not designed for astrometry. Only in recent years have reflectors been built for astrometric studies (the most active of which is the Strand reflector at Flagstaff).

An _ideal_ astrometric telescope should have:

(T1) Symmetrical images (i.e., no coma).

(T2) Complete uniformity of images over the field (i.e., no axis or no field center).

(T3) No drifts due to temperature, flexure, and aging effects, _and_ a means for checking this result.

(T4) Zero chromatic aberrations, even when the atmosphere is considered (i.e., the telescope-atmosphere system should be achromatic).

(T5) All optical components located at the system's pupil. Existing refractors have some residual coma, large chromatic aberration, and flexure of the long tube (at f/15 to f/20). Flexure and temperature effects within the lens have also been noted. Either slow unchecked aging or abrupt changes (after dismounting and remounting the lens for instance) are serious defects for very long-term programs.

The Strand-design reflector has relatively large but highly stable coma. However, the effect of coma can be minimized with a narrow field of view and with linear detectors. Accurate means for checking optical axis position are provided in this design. Although the telescope length is about five times the aperture, the compensating truss structure significantly reduces flexure.

In no case so far has compensation been provided for atmospheric dispersion; even with reflectors the spectral range is much reduced (~1000 Å) by filter-plate combinations. This means that the aperture is inefficiently used.

From recent results it appears that the Strand reflector has been found to be free of systematic errors down to the present accuracy limit for photographic results (e.g., a mean 0.001-arcsec error in parallax studies). It is also clear that one cannot speak of telescope errors independently of plate reduction techniques. Modern error-modeling procedures are able to accommodate field effects that used to be disastrous.

Nevertheless, even such major improvements do not allow these error sources to be ignored because, as discussed above, we expect that photoelectric detection will be considerably more precise than photographic. If this ability is to be translated into an actual increase in accuracy, attention must be given to smaller telescope defects. It would be most unlikely for systematic errors (of present designs) to remain negligible at the 10^{-4}-arcsec level. Because it is not possible to predict which particular defect(s) may be the limiting one, it is important to approximate as well as possible the ideal design characteristics listed above. Lastly, another consideration is the relative complexity (compared to a simple photographic plate) of photoelectric systems: it is highly desirable to have them operating at constant temperatures and gravity (i.e., at a _fixed_ focus). This fixed focus requirement is important enough to deserve being called point T6.

Two promising designs have been so far proposed (by Connes and by Gatewood; see P3 and P4 in volume II). They are similar concepts in that both telescopes would be fixed and use just one moving plane mirror; hence both fully satisfy points T3 and T6. An essential feature of both systems is the ability to run extensive internal tests on the overall optical system performance by autocollimation on the plane mirror. No such checking ability has ever been available in astrometry. As there is no turbulence internal to the telescopes, the sensitivity and accuracy of these tests will far exceed those that could be made using external sources.

As can be seen from P3 and P4, the two designs have somewhat different optical systems. In the Gatewood design, an astronomical type doublet lens is used; points T1 and T2 are approximately satisfied. All stars in the field use exactly the same optical surfaces; hence, while the system coma is not zero, the effect of mirror/lens imperfections is minimized because they produce zero distortion. The telescope optics are in vacuum with the lens acting as a window. No correction for atmospheric dispersion is attempted; thus the spectral range must be reduced.

The Connes telescope is similar to a plateless, long-focus Schmidt; the main mirror is spherical and larger than the pupil. Points T1 and T2 are, in principle, exactly satisfied. Spherical aberration is not zero, but is small compared to the seeing disk. The fact that different portions of the main mirror would image different stars may make it necessary to rotate the mirror to average out effects due to mirror imperfections. Nearly exact compensation for dispersion is realized by filling the telescope with helium gas. The use of helium gas and a tiltable, thin, plane parallel window permits the use of a broad spectral range (3500 to 9000 Å).

Hybrid designs are also conceivable. For instance, the Gatewood system might be immersed in helium behind a plane window; the Connes telescope, while originally proposed in vertical form with two flats, might be aligned along the polar axis.

Finally, the size of the telescope should not at present be considered as fixed in any way. Admittedly, a 1-m-class telescope would be preferable and is technically feasible, but even a 0.5-m aperture would be highly worthwhile because the dual advantages of photoelectric detection and increase in spectral bandpass would yield a large increase in efficiency compared to present instruments.

Two-Aperture Interferometric Astrometry

The technique of SAI circumvents to a large degree the deleterious effects of Earth's atmosphere on the accuracy of astrometric studies by confining observations to stars with small angular separations. These effects are minimized in classical narrow-field astrometry by the use of many reference stars.

A third approach is that of two-aperture interferometric (TAI) astrometry. TAI techniques would work in the same field-size domain as classical methods. Several forms of TAI have been suggested. They may be characterized in terms of whether one or two colors (wavelengths) are observed and/or whether fringe amplitude or phase is measured. The theoretical accuracy advantage of TAI over its classical narrow-field counterpart lies in its ability to minimize, perhaps eliminate, atmospheric sources of error. Light from two stars separated by an angle θ will generally traverse different portions of Earth's atmosphere before arriving at a telescope. The atmosphere generally causes a different amount of refraction for each star. The difference in path length traversed by light from the stars leads to an error, $\delta\theta$, in specifying the angle θ. The error can be related to the rms path difference, $\langle \delta L^2 \rangle^{1/2}$, and the baseline (aperture), B, of the telescope by

$$\delta\theta \propto \frac{\langle L^2 \rangle^{1/2}}{B} \tag{1}$$

It is known on both observational and theoretical grounds that, if observations are made at one wavelength, then $\langle L^2 \rangle^{1/2} \propto B^{5/6}$ (see P2 in volume II), so long as B is less than the outer scale length L_o of thermal atmosphere turbulence (~50-100 m). Once $B \gtrsim L_o$, $\langle L^2 \rangle^{1/2} \sim$ const. Currie (P5, volume II) has suggested a one-color TAI system with a baseline ~1 km. At such a large B value, thermal atmospheric turbulence would lead to negligible errors. However, little is known as to the errors that might arise due to mechanical (i.e., pressure waves) atmospheric disturbances. Currie's system would measure the amplitude of the visibility fringes produced by the interferometer.

A second approach to TAI involves the observation of stellar position at two colors (or wavelengths). This approach is under study by Shao and Staelin (P6, volume II) and by Currie and his coworkers (P7, volume II), the former involving measurement of fringe phase and the latter involving measurement of fringe amplitude. The idea of two-color TAI systems is to remove entirely errors arising from Earth's atmosphere. By measuring at two different wavelengths, the apparent angular separation of two stars can be corrected for atmospheric refraction by use of the following:

$$P_0 = P_1 - (P_1 - P_2)\left(\frac{1 - n_1}{n_2 - n_1}\right) \tag{2}$$

where P_0 is the true position (i.e., the apparent position if there were no atmosphere), $P_{1,2}$ is the apparent position at wavelength $\lambda_{1,2}$, and $n_{1,2}$ is the index of refraction of air at $\lambda_{1,2}$. The angular displacement of the stellar image caused by the atmospheric wedge depends on the index of refraction of air. The measurement of the apparent position at two colors, hence at different indices of refraction, can be used to extrapolate to the position of the star in the zero-atmosphere condition. The use of the two-color technique will, however, yield an accidental error 30 to 50 times larger than a corresponding measurement made at one wavelength.

The two-color scheme requires a large number of photons; hence it will be limited to relatively bright objects. This limitation may be severe in terms of the detection statistics indicated by the data in figure 1.

In concluding this discussion of TAI techniques, we mention a one-color imaging stellar interferometer concept discussed in detail elsewhere (Project Orion: A Design Study of a System for Detecting Extrasolar Planets, NASA SP-436). That concept involves the simultaneous measurement of relative azimuth separations of up to 20 stars. The use of only azimuth separation greatly minimizes effects due to atmospheric refraction, but necessitates measurements at two different hour angles to obtain the total angular separation between the observed stars. Although the Project Orion study considered a 50-m baseline, there is little increase in error from the atmosphere by reducing the system to a baseline of 10 m (for one-color systems, the atmospheric effect for a 10-m system is $(50/10)^{1/6} \simeq 1.3$ times larger than that for a 50-m system).

One concern expressed by the Workshop regarding TAI methods is whether TAI systems will be able to successfully locate and track the fringes that the optical system produces. Two-color amplitude systems (Currie) have been successfully operated at a baseline of 5 m, and two-color phase systems (Shao) have been successfully operated at a baseline of 1 m. Also, it is essential that simultaneous observations be made of several stars. Currently, none of the two-color systems operate in this mode; the Project Orion system concept does. These concerns are not intended to be an indictment of TAI techniques, but rather a clear demonstration of the need for further development and testing of these novel, and perhaps highly accurate, systems.

PHOTOMETRY

The Workshop considered the role of photometric studies in an effort to detect other planetary systems and upheld the conclusions of earlier studies, namely, that photometric studies are not practical as a primary detection technique, but they serve as a valuable adjunct to both astrometric and radial velocity studies. Simultaneous photometric observations of program stars in either an astrometric or a radial-velocity survey are desirable to detect variable stars and eliminate or correct data affected by intrinsic stellar variability due, for example, to flares and spots. Greater photometric accuracy in such supplementary checks will give greater reliability to the final results. Finally, it should be noted that the photometric program could be carried out using the same telescopes (perhaps simultaneously) used for the astrometry/radial-velocity programs.

At present, there is the prospect of doing general photometry to an accuracy of a few thousandths of a magnitude. This accuracy would, in principle, allow detection of the transit of Jupiter-sized objects ($\Delta m \sim 0.010$ magnitude for the Sun, more for smaller stars). Although the chances of discovering a planet by photometric means are remote, accurate photometry of planetary transits is the only likely way to determine the sizes of detected planetary companions and hence their densities (if masses come from orbital measurements).

RADIAL VELOCITY

The technique that appears to be most able to yield significant gains in knowledge relating to the detection of other planetary systems on a short time scale is that of radial-velocity studies. The short-term strength of this technique derives from the availability of high-accuracy systems and from the fact that radial-velocity studies are less limited by a need for extensive new technology and telescopes than, for example, is astrometry. However, the technique is unable to provide unambiguous evidence for a planetary system around a given star.

The ambiguity derives from the proportionality between orbital radial velocity and the product of companion mass with the sine of the inclination angle, i, between the companion's orbit and the observer's line of sight. Thus, a small measured value may be due to small sin(i) rather than to a small companion mass (i.e., planet). However, given a large enough sample size, radial-velocity observations will yield the statistical information required for a detection program. This technique is, to a large degree, immune to atmospheric effects.

Traditionally, radial velocities have been found from line-by-line measurements on photographic spectra. In recent years, new techniques have been introduced: correlation measurements (many lines measured simultaneously by photoelectric receivers); Fabry-Perot spectrometers, such as PEPSIOS; Fourier-transform spectrometers; and heterodyne spectrometers. Some of these techniques, involving scanning the spectrum one resolution element at a time, are limited to one line (or at most a few lines); Fourier-transform measurements are equivalent in speed to scanning in the photon-noise-limited (optical) region. These methods are inefficient, but their speed can be increased by combining them with a grating spectrometer and multiple detectors. Although it has been reported that the newer methods yield higher accuracy than classical photographic methods, there are only a few convincing demonstrations for an accuracy of better than 100 m/sec on measurements of stars (Fourier, Fabry-Perot, and heterodyne spectroscopy results have shown an accuracy of a few meters/second on planets). To obtain maximum precision and accuracy (e.g., freedom from systematic errors due to telluric line blending), it is necessary to use as many lines as possible; this requires the wide wavelength coverage of a grating spectrometer. Consideration should be given to the question of the optimum wavelengths for radial-velocity studies, particularly as the optimum regions will be a function of stellar spectral type. Some of the key factors that influence this consideration are:

(1) Avoidance of telluric features that might blend with stellar lines, thereby leading to a bias in the velocity determination.

(2) Avoidance of regions of the spectrum dominated by intrinsically broad lines and/or regions likely to contain variable emission components.

(3) Bandwidth of available detectors.

The Workshop felt that it would be useful to assemble a suitable spectral "atlas" for bright objects of differing spectral type (such compendia exist for the Sun, Arcturus, and Procyon). Using these spectra as a base, simulations of cross-correlation measurements could be conducted to estimate the obtainable precision as a function of exposure time, wavelength region, and other key observational variables.

Photoelectric detection provides high efficiency. The best results using conventional systems for stellar work (~100 m/sec) have therefore come from photoelectric correlation spectrometers. Higher precisions (~10 m/sec) have been reported for studies of the rotation of the Venusian atmosphere; however, the various results yield nominal values that differ by as much as 100 m/sec. Whether this spread in results is real, it demonstrates the necessity of using different methods to check each other. At present these disagreements are not resolved. One may hope that efforts to clear up this confusion may help to reduce systematic errors in the future and may even show that some present techniques have 10-m/sec accuracy. The extension of similar techniques to studies at the magnitudes required for a planetary survey appears feasible from the photon-noise viewpoint. However, suitable Michelson-Fourier and Fabry-Perot spectrometers then tend to become complex systems--they require a post-disperser, an image detector, and a large amount of data processing. So far one Fabry-Perot spectrometer has been built by Serkowski specifically for extrasolar planetary detection (see P8, volume II); adaptable Michelson-Fourier spectrometers exist, but none has been available for this type of work. Grating spectrometers operated as correlation radial-velocity meters have recently been improved and have realized satisfactory photon-limited performance on faint stars. Such systems are simple, they contain few elements, and real-time results are given with very little data processing. They have not yet matched interferometers so far as low systematic errors are concerned, but none has been built with this purpose in mind. A system designed to meet the planetary detection requirements has been proposed by Connes (see P9, volume II).

Although the technological and instrumental state of radial-velocity systems is more developed than is that of astrometric systems, there is a need for additional work in this area. The Workshop felt that support should be given to test Serkowski's system adequately, as well as to commence development and testing of a correlation spectrometer (such as that proposed by Connes). Further, support should be given to the use of optical fiber image scramblers on existing coudé systems operating at maximum dispersion (2 Å/mm). Coudé systems equipped with scramblers could reach 50-100 m/sec and thereby provide invaluable knowledge of binary frequency and, more importantly, the mass distribution for secondary bodies in binary systems.

As noted in chapter 2, the two strongest Workshop recommendations involved the initiation of radial-velocity observational programs, one aimed at observations of many solar-type stars and the second aimed at the radial-velocity variation of integrated sunlight. The latter study could also be conducted using Serkowski's system (and/or one similar to that proposed by Connes), as well as with some of the existing solar telescope systems. With the exception of correlation spectrometer observations, the limitations to undertaking these important radial-velocity observational programs are chiefly those of available telescope time and support for the observations.

While existing telescopes are adequate to test existing and proposed radial-velocity instruments, a thorough search for planetary systems requires a dedicated telescope. Also, a dedicated telescope is highly desirable to obtain long-term instrumental stability on the program. We note that about 30 new binaries were found within a year by an instrument not optimized for small radial-velocity changes and with an accuracy of about 200 m/sec. However, a 1-m telescope was available nearly full time. Clearly, looking at a large number of stars and at a wide range of possible periods requires nothing less.

As the number of stars that can be studied depends strongly on light-gathering power, a large aperture ($\geq$ 1 m) is important. (A large-aperture telescope is needed to survey the faint nearby M dwarfs that will be part of an astrometric survey.) If no existing telescope can be diverted to this program (as seems likely), a suitable telescope can be built for this purpose at a fraction of the cost of a conventional telescope of the same aperture.

There are no special technical requirements for such a telescope--neither very good seeing nor a very dark sky are needed and any telescope design is usable (a Coudé focus is not necessary). Accuracy for a given object will be a function of the number of photons collected (i.e., observing time for a given result will be proportional to $(\text{flux})^{-1}$ within a wide range of flux). It is thus reasonable to operate in the 1- to 2-m-diameter range. The advantages of using an optical fiber image scrambler to feed the spectrograph are: (1) optical tolerances are reduced (modest quality images are sufficient); (2) no secondary mirror is needed if the fiber input is at the prime focus; and (3) the spectrograph may be removed from the telescope, thereby improving instrumental stability. Based on others' experience in building a low-accuracy infrared collector, we have no doubt the cost would be only a fraction of that for a conventional instrument. The undertaking would be relatively simple because a classical primary mirror would be used and no relay optics are needed.

Regular full-time observations could be started after one or more radial-velocity meters are set up at the fiber-focus of the selected telescope. One cannot guarantee which type of meter would prove best at this stage and it is entirely possible that both interferometers and grating spectrometers should be developed. Their theoretical performances are similar, but the interferometers might prove more useful for bright stars because of lower systematic errors, while the grating correlation spectrometer may be best for fainter stars because of its greater efficiency. Using an optical fiber system, there would be no difficulty in setting up two systems at the same telescope and switching from one to the other.

It should be stressed that, in contrast to the astrometric case, a specialized telescope is not required and will not produce better results. What is required is a telescope available full time, and the larger the better. The choice between use of an existing or a new dedicated telescope is a matter of economics and observational continuity. Again, a long-term commitment is essential.

DATA REDUCTION AND ANALYSIS

Generally speaking, the detection and study of other planetary systems will involve measurements one or more orders of magnitude more accurate than any previously obtained and the extraction of signals in the low signal/noise domain. Consequently, an integral and essential aspect of a successful detection program is a full understanding of the sources of error in the techniques used to reduce and analyze the data. There is little point in developing instrumentation with ultrahigh precision if the accuracy is degraded significantly because of inadequate data-reduction techniques or incomplete data. It should be emphasized that considerations of this type should not simply apply *post facto* to the data; they should be considered as a *prerequisite* to the design of any new observational facility.

We will not discuss data-reduction techniques in detail, but rather list below some of the factors the Workshop identified as being crucial.

1. Noise in reference frames: In all types of relative measurements, observable parameters of a target object are compared to a set of similar observable parameters for the reference objects. There is an uncertainty associated with any reference frame because its defining quantities are themselves determined by a statistical adjustment of a set of observations. Of particular interest is the "noise" in the reference frame in which a large fraction (>50-90%) of the reference stars are unknown binary stars (or have planetary systems) which could introduce evidence for a spurious "wobble" in the motion of a target star.

2. Choice of a reduction technique: The Workshop felt that a detailed analysis should be made to identify the most appropriate reduction technique(s) for the observational programs suggested here. This analysis would involve both the selection of a reduction model and the selection of a suitable adjustment algorithm(s). The reduction model, which relates estimated parameters to observed parameters, should be as realistic and accurate as possible. Only then can one, for example, arrive at a meaningful decision as to whether observed harmonic motion is Keplerian. The choice of adjustment algorithms may seem to be a rather trivial aspect of the activity discussed here. However, it has been demonstrated that algorithms which do not consider all available constraints in as rigorous a fashion as possible leads to errors that could be significant at the level of accuracy required for the detection of other planetary systems.

3. Tradeoff studies on measurement parameters: Items (1) and (2) can be studied independently, but the Workshop emphasized the need to undertake tradeoff studies concerning the attainable accuracy as a function of reference frame characteristics versus reduction technique complexity. It is obvious that a reference frame which is insufficient for any reason, as well as a complex reduction model that requires a large number of reduction parameters for which estimates must be found, both have a detrimental influence on the accuracy of the result. One will thus have to find an optimum

balance--compatible with the possible expenditure of labor and funds--between the magnitude of the reference frame and the complexity of a reduction model to achieve the maximum possible accuracy in the final results. Such tradeoff studies are of great practical importance and they should be made an integral part of design considerations for future detectors and telescopes.

OTHER DETECTION TECHNIQUES

Prior to summarizing the major elements of the recommended program, it is worthwhile to note that there are other approaches to the detection problem. The fact that these alternatives are not given equal footing with the techniques of astrometry and radial-velocity studies is more a statement regarding the relative infancy of these techniques than a statement of inadequacy. Two other general techniques which the Workshop felt deserved special mention are discussed below.

Notable among alternative approaches are infrared spectroscopy and infrared spatial interferometry. Infrared spatial interferometry is particularly well suited for studying early or late stages of stellar evolution, stages in which visual observations are usually inadequate because the principal energy output of such systems lies in the infrared, not the visual. To exclude these objects is to guarantee that quantitiative data on the origin and fate of planetary systems will be precluded.

A second powerful argument can be made in support of infrared interferometry. The results of radial and transverse-velocity measurements on solar-type stars may be considered to be a "selection" exercise, i.e., a method for identification of those stellar systems that may have associated planetary systems. The interpretation of the observed periodicities in terms of planetary perturbations may be less than definitive because other known phenomena (photospheric turbulance, stellar cycles, star spots, etc.) could also be invoked to explain the observations. In order to unambiguously show that extrasolar planets exist, it will be desirable in the long term to "image" them. This is likely to be possible only from space, perhaps only with a large interferometer, although other techniques still in their infancy may emerge. In the case of coherent interferometers, it is not even necessary to combine the collected stellar light directly; this can be achieved electronically. Current ground-based coherent infrared interferometers can achieve a precision and positional accuracy in the milliseconds-of-arc range with a baseline of ~5.5 m and with 1.5-m collectors. Several hundred known stars and several thousand other potential sources can be studied. The basic technology to do this already exists. Using larger optics increases the sensitivity in proportion to the area of the collectors. Incoherent infrared interferometry can examine substantially more sources, but with a considerable increase in engineering and optical difficulties resulting from the need to precisely maintain the optical path differences and the need for direct recombination of the stellar optical beams. A final point is that both spatial interferometry and radial-velocity studies may be easily achieved simultaneously with a coherent interferometer if suitable stellar spectral lines exist near a local oscillator frequency. This is so for the Sun and is expected to be so for most cool stars as well. Infrared coherent and incoherent interferometry is now being done in the United States and in France.

Measurements of stars are now being obtained and a body of knowledge and experience will be accumulated in the next few years, leading to possible exploitation of these techniques for the detection of planetary systems using a space-based system.

Another alternative involving infrared studies relates more to the detection of planetary systems in their infancy, perhaps at their birth. Over the past decade, the advent of sensitive IR and millimeter wavelength detectors have permitted astronomers to begin to survey the birthplaces of stars. From studies made with these new techniques, our knowledge of the early history of solar-type stars has increased significantly. We have found evidence for extensive circumstellar envelopes, some of which may be disklike in form. Current beliefs regarding the formation of planetary systems suggest that disks of gas and dust are the precursors of planetary systems. Further hints of the existence of circumstellar disks (and hence possible protoplanetary systems) come from the observation of biconical nebulae--light from the star at the center of a thin, optically thick disk escapes from its polar regions, illuminating nearby interstellar dust. Our understanding of the frequency with which disks occur, their physical properties, and their evolution with time is, however, quite primitive.

Observations aimed at the detection of disks and studies of their properties bear directly on the detection of planets. They may provide valuable insights into the frequency of planetary system formation and into the conditions that existed during the early history of the solar system. It would therefore be of significant value to focus additional observational attention on known disk systems and to attempt, from new surveys, to estimate the frequency of such systems among solar-type stars. The principal thrust of observations in this area should be on (1) studies of biconical nebulae and (2) statistical studies of the occurrence of disk systems in young stellar clusters.

ADJUNCT ACTIVITY

A decision on the requirements for new observational facilities, if needed, would not be necessary during this stage of the program. However, the Workshop felt that preliminary consideration concerning potential telescope sites requires that a study be initiated concurrent with the other activities. The motivation for early site consideration stems primarily from (1) the rapidly diminishing number of good sites and the lengthy development times required and (2) the possibility that some of the new telescope concepts may have unconventional site requirements. A discussion of site considerations, including a review of available sites and their characteristics, is given in P10 (volume II).

An important type of study which should be pursued in the context of a program to detect and study other planetary systems is that of theoretical studies on the formation of planetary systems and related items (e.g., the formation of planets, and the stability of orbits in a multistar system).

SUMMARY

As noted in the Introduction, a comprehensive program aimed at the detection and study of other planetary systems is technically feasible and has great scientific potential. Implementation is constrained only by our willingness to invest time, thought, and money. This Workshop report considers those factors that might reasonably influence and/or constitute a first step toward a comprehensive program. The Workshop recommendations define the broad outlines of a modest-cost, four-year pilot program directed at providing both high scientific return and the technological basis for decisions on the ground-based phase of a comprehensive program.

A summary of this program is presented in table I. We stress that the items in table I are listed in order of their current status, progressing from activities that need to be begun, to those which need to be continued, to those which should be completed. This order is _not_ a priority ranking by the Workshop.

Table I: A High-Priority Program

I IMMEDIATE ACTIVITY (0-2 YEARS):

A. INSTRUMENTAL STUDIES

1. Astrometry

1.1. Begin development/testing of a Connes-type detector

1.2. Begin studies of array detectors (e.g., CCD's)

1.3. Continue testing of two-color interferometer system concept

1.4. Continue studies of infrared heterodyne systems

1.5. Continue studies of single-aperture amplitude interferometry systems

1.6. Complete development/testing of Gatewood-type detector

2. Radial Velocity

2.1. Begin development/testing of grating correlation spectrometer

2.2. Complete development/testing of Serkowski's system

B. OBSERVATIONAL STUDIES

1. Astrometry

1.1. Initiate pilot SAI program with existing systems (~10 binaries)

1.2. Initiate observations with Gatewood's system

2.. Radial Velocity

2.1. Conduct high-accuracy (50-100 m/sec) studies of solar-type stars using Coudé facilities

2.2. Conduct high-accuracy (~10 m/sec) studies of solar-type stars using Serkowski's Fabry-Perot spectrometer

2.3. Conduct studies of the variations in integrated sunlight using both existing solar telescopes and Serkowski's instrument

2.4. Conduct studies of solar system objects (e.g., Venus) to provide calibration of radial-velocity systems

3.. Others

3.1. Conduct studies on the frequency of occurrence and distribution with spectral-type circumstellar disks in young stellar clusters

3.2. Conduct studies of biconical nebulae for evidence of giant protoplanets

C. THEORETICAL STUDIES

1. Conduct studies on sources of error (e.g., reference frame noise, inadequate reduction models) in data-reduction techniques

2. Conduct studies on formation and stability of planetary systems

II. <u>NEAR-TERM ACTIVITY (2-4 YEARS)</u>:

A. INSTRUMENTAL STUDIES

1. Astrometry

1.1. Pending results from I.A.1.3, construct and test a long-baseline, two-color interferometer

1.2. Develop and test an online, real-time speckle data system

1.3. Conduct a paper study of a narrow-field telescope using inputs from I.A.1.1 and I.A.1.6

2. Radial Velocity

2.1. Conduct a paper study of a radial velocity telescope using inputs from I.A.2

B. OBSERVATIONAL STUDIES

1. Astrometry

1.1. Continue SAI pilot program (I.B.1.1)

1.2. Initiate/continue studies using any or all of the detectors (I.A.1.1, 1.2, and 1.6) on existing narrow-field telescopes

2. Radial Velocity

   2.1. Continue high-accuracy stellar radial velocity study (I.B.2.2)

   2.2. Continue solar studies (I.B.2.3)

   2.3. Continue studies of solar system objects

3. Others

   3.1. Continue I.B.3

   3.2. Initiate site survey activity for possible new dedicated telescopes

C. THEORETICAL STUDIES

1. Complete I.C.1

2. Continue I.C.2 and related work

CHAPTER 4 -- PROGRAM IMPLEMENTATION

The preceding chapters have reviewed the background and motivation of this Workshop, outlined its principal conclusions and recommendations, and presented the basic elements of a four-year, high-priority program. The major goal of the program, outlined in chapter 3, is to provide the essential first steps toward the development of a ground-based program to detect and study other planetary systems. The Workshop participants felt that they would be remiss if they did not also offer some thoughts on general philosophy and direction, arising from their experience, which might be helpful in implementing this program.

THOUGHTS ON PLANNING

Examination of table I shows that the recommended program involves a balance primarily between instrumental studies and observational studies, with a small, but important, theoretical component. This balance should be an integral aspect of the planning for the suggested program or any alternate program. This balance does not imply equality of financial resources--the bulk of the expenditure must go to instrumental studies. Rather it is a balance of information--information that is <u>required</u> if a sound judgment is to be made at the end of the program regarding the direction and magnitude of the next phase in a planetary detection program. It should be stressed that the recommended instrumental studies will, on the average, require more time than will the recommended observational studies. Thus, if the time scale for this program is not to be significantly lengthened, it is essential to initiate as many of the instrumental studies as possible early in the program.

The National Aeronautics and Space Administration has recently played the lead role in investigating the detection and study of other planetary systems. Much of the impetus for the NASA's involvement has come from the scientific interests of its own researchers. The Workshop strongly endorsed continued leadership by the NASA. The principal reasons for this endorsement are:

(1) This program offers a unique approach to one of the major goals of the agency, viz., to understand the origin of the solar system.

(2) A fully comprehensive program to detect and study other planetary systems must ultimately involve space-based systems as well as ground-based systems.

As the ground-based activity recommended here is necessary for both (1) and (2), it is proper for the NASA to be involved in a concerted ground-based program. However, the recommended observational studies will interest and involve both other funding agencies (notably the NSF) and institutions that control and allocate observing time on existing telescopes. The Workshop felt that a strong effort should be made by the NASA to identify and involve these complementary funding agencies in the program planning at an early stage. The NASA should emphasize the importance of support in providing telescope time for sound observing proposals to those bodies that control the allocation of observing time.

A subject that was not formally considered by the Workshop, but one which arose frequently in working group discussions, was the need for some form of continuing planetary detection working group. The function of this group, which would include representatives from the NASA and other participating agencies as well as members of the university community, would be to assist NASA management in evaluating the progress during the program and to provide a nucleus that could further stimulate other researchers to think about this far-reaching program.

Finally, the observational studies should attempt to focus, as much as possible, on a common set of target objects. Such focusing should not be overly restrictive and/or artificial; the very nature of the various techniques makes complete overlap impossible. However, the strong need for corroborative or complementary studies (see conclusion 5 in ch. 2) suggests that an effort in this direction is well merited, particularly for the observations recommended in the near term (see table I). The Workshop felt that a yearly meeting or workshop, involving the prospective observers, would be an effective way to define a common target list as well as to share and evaluate preliminary results. An informal exchange of lists of candidate target stars by those involved in observations could precede the suggested meeting.

THOUGHTS ON PRIORITIES

It would be premature and presumptuous of this Workshop to define detailed priorities for the many elements contained in the recommended program. However, the Workshop felt that some general program priorities might be beneficial.

Maintaining the aforementioned balance between instrumental, observational, and theoretical studies should be given high priority. However, priority should generally be given to instrumental studies early in the program.

The technique that would benefit the most from instrumental studies is astrometry. Also, it is likely that, whatever form a new astrometric telescope might take, it will be significantly more expensive and require longer development time than its radial-velocity counterpart. Finally, there are presently diverse astrometric concepts that merit instrumental study. These factors led the Workshop to recommend somewhat higher priority for astrometric instrumentation.

In a more general vein, the Workshop noted that the coming two decades will undoubtedly yield exciting scientific advances, but that there was a need for identifying and pursuing new frontiers. These should be frontiers of great scientific value and of philosophical and spiritual value to all of mankind. There are very few scientifically meaningful endeavors to which the lay public can easily relate, endeavors that restore mankind's pride in his ability and that lift his eyes above the day-to-day affairs toward a greater awareness of his place in the Universe. The Workshop felt that the detection and study of other planetary systems, particularly if they exist around nearby stars, would provide such a frontier. Consequently, it is the recommendation of the Workshop that an overall effort to detect and study other planetary systems be made a priority item for the NASA and the astronomical community in general.

APPENDIX A: WORKSHOP CHRONICLE

The Workshop held two meetings. The first meeting was held at the Asilomar Conference Grounds in Pacific Grove, California, from October 24-27, 1978. The second meeting was held at the University of Maryland from January 29-31, 1979. Listed below are the meeting agendas for the two meetings.

The Workshop members were:

D. C. Black, NASA-Ames Research Center, Co-Chairman

W. E Brunk, NASA Headquarters, Co-Chairman

H. A. Abt, Kitt Peak National Observatory

W. A. Baum, Lowell Observatory

P. Connes, Centre National de la Recherche Scientifique, France

D. G. Currie, University of Maryland

H. K. Eichhorn, University of South Florida

G. D. Gatewood, University of Pittsburgh

J. L. Greenstein, California Institute of Technology & Hale Observatories

W. Heacox, University of Arizona

R. F. Howard, Hale Observatories

C. E. KenKnight, University of Arizona

M. J. Mumma, NASA-Goddard Space Flight Center

K. M. Serkowski, University of Arizona

M. Shao, Massachusetts Institute of Technology

K. A. Strand, U.S. Naval Observatory

S. E. Strom, Kitt Peak National Observatory

M. F. Walker, Lick Observatory

R. Walker, NASA-Ames Research Center

S. P. Worden, Sacramento Peak Observatory

A. T. Young, Texas A and M University

AGENDA FOR FIRST PLANETARY DETECTION WORKSHOP
Held at Asilomar Conference Grounds, Pacific Grove, CA

October 24, 1978:

9:00 a.m.	Welcome: C. A. Syvertson, Director, Ames Research Center
9:10 a.m.	Opening Remarks by Chairmen D. C. Black and W. E. Brunk
9:20 a.m.	Review of Planetary Detection Workshop and Project Orion Findings D. C. Black
10:00 a.m.	Open Discussion
10:30 a.m.	------------------------------Break------------------------------
10:45 a.m.	The Capability of Planned or Existing Space-Based Telescopes to Undertake a Search W. Baum
11:25 a.m.	Open Discussion
11:45 a.m.	------------------------------Lunch------------------------------
1:00 p.m.	Physical Limitations to Ground-Based Astrometry; Atmospheric Effects C. KenKnight
1:40 p.m.	Open Discussion
2:00 p.m.	Instrumental-Technological Limitations to Ground-Based Astrometry D. Currie
2:40 p.m.	Open Discussion
3:00 p.m.	Data Requirements for Ultra-High Precision Astrometry H. Eichhorn
3:40 p.m.	Open Discussion
4:00 p.m.	----------------------Break Until Evening----------------------
7:30 p.m.	Summary of First Day Discussion and Additional Thoughts G. Gatewood
8:00 p.m.	Open Discussion
8:30 p.m.	To Be Determined W. E. Brunk

October 25, 1978:

9:30 a.m.	Physical Limitations to Ground-Based Radial Velocity Studies; Astrophysical Sources of Noise B. Howard
10:10 a.m.	Open Discussion
10:30 a.m.	-------------------------------Break------------------------------
10:45 a.m.	Instrumental-Technological Limitations to Radial Velocity Studies K. Serkowski
11:25 a.m.	Open Discussion
11:45 a.m.	-------------------------------Lunch-------------------------------
1:00 p.m.	Other Potential Ground-Based Detection Techniques S. Strom
1:40 p.m.	Open Discussion
2:00 p.m.	Frequency of Binaries; Implications for Planetary Detection H. Abt
2:40 p.m.	Open Discussion
3:00 p.m.	----------------------Break Until Evening------------------------
7:30 p.m.	Summary of Second Day and Additional Thoughts J. Greenstein
8:00 p.m.	Open Discussion
8:30 p.m.	Recent Relevant Research Results G. Gatewood, K. Serkowski, J. Greenstein, and M. Shao

October 26, 1978:

9:00 a.m.	Organize Subpanels for Detailed Discussion D. C. Black and W. E. Brunk

SP-1: Physical Limitations (Leader: TBD)
SP-2: Technological Limitations (Leader: TBD)
SP-3: Information Content, Program Scope, Detection Probability (Leader: TBD)

Meet as Subpanels for Remainder of Day (no evening session)
(All participants involved)

October 27, 1978:

8:30 a.m.	Report of Subpanels to Workshop
10:30 a.m.	-------------------------------Break--------------------------------
10:45 a.m.	Action Items for Second Workshop Meeting D. C. Black and W. E. Brunk
11:15 a.m.	Closing Remarks D. C. Black

AGENDA FOR SECOND PLANETARY DETECTION WORKSHOP

<u>January 28, 1979</u>:

7:30 p.m. Welcome Social Hour

<u>January 29, 1979</u>:

9:15 a.m.	Welcoming Remarks D. Currie
9:25 a.m.	Organizational Remarks D. Black and W. E. Brunk
9:40 a.m.	A Comparison of Two Potential Space-Based Detection Systems L. Bandermann
10:10 a.m.	Open Discussion
10:25 a.m.	Application of Infrared Array Detectors to the Detection Problem R. G. Walker
10:45 a.m.	Open Discussion
11:00 a.m.	------------------------------Break------------------------------
11:10 a.m.	Final Report: Outline and Writing Assignments D. Black and W. E. Brunk
11:40 a.m.	------------------------------Lunch------------------------------
1:00 p.m.	All Participants Meet as Working Groups Group 1: Physical Limitations (R. Howard, Chairman) Group 2: Instrumental Limitations (M. Mumma, Chairman)
5:00 p.m.	End of First Day's Activity

<u>January 30, 1979</u>:

9:15 - 12:00	All Participants Meet as Working Groups
12:00 - 1:00	Lunch
1:00 - 5:00	All Participants Meet as Working Groups

January 31, 1979:

9:15 - 12:00	All Participants Meet as Working Groups
12:00 - 1:00	Lunch Break
1:00 - 3:00	All Participants Meet as Working Groups
3:00 p.m.	Break
3:15 - 5:00	Discussion of written material for post-workshop assignments

APPENDIX B: RATIONALE FOR A PLANETARY DETECTION PROGRAM--PERSONAL THOUGHTS BY WORKSHOP MEMBERS

Each Workshop member was requested to indicate his own answer to the question "Why search for other planetary systems?" Several themes were common to many of the responses, and the responses varied in detail and length. In an attempt to convey to the readers some feeling for the factors that motivated the members of this Workshop to participate, we present below representative samples of the answers.

"We are unlikely ever to understand the way in which the solar system formed until we have discovered other planetary systems to learn which characteristics are basic and which are accidental, in the same way that psychologists must study many people to learn which characteristics are widespread or unbiquitious and which are individual or peculiar. If our discovery of other planets should eventually lead to the discovery of other civilizations, we will learn whether life is unique and, if not, what other forms it may take. If our search reveals other advanced civilizations, we are likely to learn the solutions--or alternate solutions--to our major problems."

"The real world is so complex and nonlinear that the expected factor of about 100 increase in the accuracy of fundamental measurements is certain to uncover many new phenomena. Thus, while the search for planets of other stars is itself a sound scientific problem, I expect that the superior techniques required for such a search will lead to other discoveries, presently unforseeable, that will ultimately prove to be more important than the discovery of other planets. (For example, the discovery of asteroids led Gauss to develop the method of least squares to determine their orbits; but least squares has been of enormously greater value, in all branches of science, than any information we have ever obtained from the asteroids.)"

"Knowledge concerning the existence (or nonexistence) of planets around other stars will have important effects on some fundamental areas of astronomy. The nature of the distribution of planets around other stars will serve as fundamental data for theories of star formation. This field is badly in need of more parameters to fit, and having the distribution of mass and angular momentum of the leftover material as a function of stellar mass, for example, will provide theoreticians with a very good set of basic data. When a search for extraterrestrial intelligence is begun, a firm knowledge of the frequency of planetary systems will probably make such a program enormously more efficient. Sources of astrophysical noise in a search for planets are themselves very interesting stellar phenomena. We are just beginning to learn about large-scale circulation and oscillations in the Sun. When we begin to get data about these phenomena in other stars, we will gain enormously in our understanding of stellar structure and convection and of solar-activity-type dynamos. Some of these areas have some very practical terrestrial applications on the solar side (such as weather and communications). There is also some general cultural significance in results about other planets. To a large extent, people today show the attitudes and behavior that they do because of their knowledge of the universe around them, much of which has been learned in the last century or so. Clearly, further knowledge concerning such matters as planets and life upon them will further affect our attitudes and beliefs. For example, can you imagine the reaction that would ensue if we could prove today without a doubt that the Sun possessed the only planetary system in the universe?"

"It is generally acknowledged that publication of the first photographs of Earth from the Moon had an immediate and profound effect on man's thinking. No more dramatic visional statement could be made to emphasize the finiteness, beauty, and isolation of man's home planet. It is still not clear how much these photographs will influence man's appreciation of his environment and his subconscious understanding of the need for all men to preserve our unique gifts. Knowledge of the frequency with which other solar systems occur may have a similarly dramatic effect on man's thinking. The discovery of many solar systems will almost certainly spur efforts to seek direct evidence of other civilizations. There is, however, a non-negligible possibility that the formation of solar systems are relatively rare events. If our quest suggests that Earth is a relatively lonely planet and, consequently, that life elswhere is rare, perhaps man will develop a deeper appreciation of the value of life."

"How did Earth form? This is one of those fundamental questions asked by children and philosophers since time immemorial. Our present-day science has no answer to this question--only numerous contradictory hypotheses. One cannot choose among them because we know only one example of a planetary system: our own. Therefore, we cannot exclude the hypotheses that explain the origin of our planetary system as a unique, extremely unlikely event. Finding which types of stars have planetary systems and what are typical masses and orbital periods of the largest planets would be a great step toward explaining the origin of planetary systems and the origin of Earth. Since at least one planet, Earth, is an abode of life and of civilization, the planets are, from a philosophical point of view, incomparably more important astronomical objects than lifeless stars or nebulae. But astronomers have made no effort to detect these most spectacular objects outside our own planetary system. The present NASA Workshops are the first attempt to remedy this despicable situation."

"A search for extrasolar planets is important from both a scientific and philosophical standpoint. I must confess that it was the opportunity to participate in this type of project which was a strong influence in my choice of astronomy as a profession. Finding other Earths, or even other solar systems, holds more potential excitement than quasars, black holes, and other such "pop" science. Unlike other areas of "sexy" astronomy, a search for other planetary systems has guaranteed scientific payoffs. To start with the most concrete benefits, stellar masses, radii, and distance from even an unsuccessful search will provide fundamental distance scale and mass luminosity calibrations at least an order of magnitude better than the present ones. Another inevitable result will be a complete list of binary frequencies and parameters for a representative sample of stars, the solar neighborhood stars. These numbers are essential inputs for any model of star system formation, even without positive detection of planets. Moreover, the results of a systematic search of the nearby stars will reveal how much mass there is in "large" objects. The frequent observation of these objects may also show fundamental properties of the stars themselves. We should get a good idea of such questions as stellar variability and flare properties. If the accuracies in velocity and position are good enough to detect planets, we will almost surely obtain information about large-scale velocity fields on the stellar surface. Periodicities in the range of minutes to days may serve as probes of the stellar interior, just as they are beginning to do on the Sun. We could also observe surface structure, such as spots and stellar cycles. To detect planets, it is clear that the techniques proposed must be pushed to their limits. This effort will invariably advance instrumental technology and produce new methods. This item alone justifies putting considerable effort in the program. A successful search for planetary systems is the real payoff. As mentioned previously, information on how planets may form and in what numbers is a basic input for star and stellar system formation theory. However, public interest generated by the discovery of even one planet would be enormous. Most nonscientists I have talked to are sincerely interested and excited by the possibility of finding other solar systems. This program is hard science and has none of the science fiction aspects of SETI. It is an excellent example of good science that has real public appeal. I feel the NASA should begin a strong public relations program to implement this program. This aspect may be all that is needed to translate this search into reality."

"The observable Universe appears to consist of a hierarchy of structures from the largest (clusters of galaxies) through the smallest (subatomic particles). It is a characteristic of modern western thought that such a hierarchy be sought to be understood in terms of a coherent, unified, interdependent body of theory, a sort of master plan as formulated by nature or by God. While philosophers in this century, especially Godel and Tarski, have cast doubts on the ability of the human mind ever to comprehend such a grand design, understanding the overall structure of the Universe remains the goal of modern science. In my opinion, it is the unstated hope of many, if not most, scientists that their work, by advancing understanding of the structure of the Universe will thereby assist mankind in gaining perspective on its role in the Universe and lead ultimately to a better understanding of the human condition. Nowhere in science is this more true today than in astronomy, and nowhere in astronomy than at the two extremes of the astronomical hierarchy: the large scale structure of the Universe (cosmology) and the formation of planetary systems (cosmogony). With the possible exception of some aspects of biology, no currently viable scientific disciplines have more prospects for strongly affecting our philosophical makeup than do those two areas of astronomical research. The value of cosmology in this regard has long been appreciated; that of cosmogony has been confined until late to relatively few scientists and philosophers. Apparently, it has taken the impact of spaceflight to turn the attention of astronomy at large to the structure of our tiny corner of the Universe and to the possibility of its replication elsewhere. Ultimately, the fascination with the prospect of discovering extrasolar planetary systems is derived from the hope of discovering other forms of life, thereby casting valuable perspective on the definition of life and on the meaning of our existence in the context of the Universe as a whole. The issue is illustrated by considering two possible extreme outcomes of such a search: first, that nearly all stars have revolving about them planets similar (in some ill-defined form) to Earth and, second, that no (or nearly no) stars possess such planets. In either case, the implications for the human perspective are profound. In a somewhat less abstract vein, the question of planetary system formation appears to be inextricably linked to that of stellar formation and probably to the structure and evolution of galaxies. Our ignorance of the mechanisms of planetary system formation represents a serious gap in our understanding of the hierarchy of the Universe, and this ignorance reflects on our understanding of adjacent steps in the hierarchy for a considerable distance in both directions."

"The really great discoveries of science are those that have changed human horizons. If planets are found to be as abundant in the Universe as some of us suspect, the impact on human philosophy may be greater than that of any other astronomical discovery of our time, and the challenge to send spacecraft to visit these newly discovered other worlds could well become the driving force of space sciences. Studies indicate that the use of both space instruments and ground-based telescopes is needed for a balanced program. The search will take time, the facts will emerge gradually, and we should dedicate ourselves to the task without delay."

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16. Abstract

One of the oldest unanswered questions in astronomy is "how did the solar system form?" Recent studies of the solar system using telescopes and spacecraft have provided exciting new discoveries as well as valuable information on the present state and evolution of planets in the solar system. However, the processes involved in planetary evolution tend to obscure vital clues as to how the planets formed, and even if we could unravel the natural record to reveal how Earth and her sister planets formed, it remains unclear whether we would understand how the solar system itself formed. One way to obtain valuable and probably essential data for an understanding of the origin of the solar system is to discover and study planetary systems revolving around stars other than the Sun: there is currently no unequivocal observational evidence for other planetary systems.

In an attempt to examine whether it is feasible to use ground-based astronomical techniques to search for other planetary systems, two scientific workshops were held during the interval October 1978 to January 1979. The workshop participants, experts in various branches of astronomy and instrumentation, addressed the questions of whether the accuracy of existing techniques (e.g., astrometry and spectroscopy) could be improved to the level required for such a demanding observational effort, what the factors are that limit the achievable accuracy, and whether the achievable accuracies were sufficiently good to permit strong scientific interferences in the presence of negative results from a search. Volume I of this report is an overview of the workshop findings; volume II contains technical position papers authored by workshop members on major aspects of the workshop deliberations.

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