ANONYMOUS

REVIEWS

OF

THE MILLIMETER ARRAY

PROPOSAL

In my opinion the MMA Project, as described in the NRAO proposal, is an outstanding one with tremendous scientific potential. The combination, at mm-wavelengths, of unprecedented high sensitivity and high resolution offered by the MMA would provide a uniquely powerful instrument for addressing a host of fundamentally important scientific topics, and should result in major breakthroughs and discoveries in many astronomical disciplines. I found the scientific case presented in the proposal to be excellent, and I believe this instrument would be the premier mm-wavelength facility in the world during at least the first decade of the 21st century.

Among the attractive features of the facility are its versatility and the extremely broad range of astronomical questions which can be addressed. To achieve this, the emphasis on multiple configurations, broad frequency coverage, up to 366 GHz, excellent u-v coverage for snapshot observations, finding a dry site, and provision of total power capability for all antennas to permit independent operation are examples of important concepts which are addressed in the proposal, and steps are planned or underway to ensure these needs are met. The proposal convinces me that the MMA will be able to address the full range of scientific interests within the mm-wavelength community, provided that the intention, stated in the proposal, to provide sufficient computational resources and powerful, user-friendly software to permit real time data monitoring is carried out. The requirement for real time data display would be especially important for efficient use of the array in programs such as line searches and chemical abundance mapping, but I am satisifed that the proposal acknowledges this need.

I have no doubt that the MMA is a feasible project, although there are technical problems and difficulties to overcome, such as the requirement for 1 arc second pointing, the need for tunerless mixers and local oscillators which don't require whiskered multipliers. The proposal identifies these obstacles and, although definite solutions are not presented at this early stage, reasonable directions, approaches and extrapolations of technical progress are given which ensure that adequate answers can be found. In my opinion, there are no insurmountable technical difficulties which would jeopardize the realization of the MMA.

The NRAO is a uniquely well-qualified organization to design, construct and operate this facility. Experience acquired in operating the VLA will be directly applicable, and the traditional strength of NRAO in furnishing top-rate, innovative receivers for radio astronomy will be a strong basis from which to develop receivers for the 40 antennas.

The estimate of total capital costs is reasonable, but I am doubtful that the project schedule, which calls for full operation by 1998, can be achieved. There is a danger that demands imposed by the Green Bank Telescope and the VLBA projects will detract from resources available for the early planning and design phases of this project. However, because of its multi-element nature, a delay of a few years in bringing the array to full operation would not be too serious, as useful observations could be done with a partial array, provided the software and computer support is available.

In summary, I enthusiastically endorse this project. The proposal is well-written and complete. Potential problem areas are identified and reasonable solutions or directions for investigation are presented. If completed, the MMA will be the most powerful mm-wavelength facility in the world, and it would restore American preeminence in this important astronomical discipline.

COMMENTS ON THE MMA PROPOSAL

The construction of the MMA should be a top national priority. As outlined in this proposal, this instrument will impact every major field in astrophysics, from Solar System studies to cosmology. The millimeter-wave spectrum is ripe for the opening of large portions of new parameter space; the MMA will push the limits of sensitivity and angular resolution by orders of magnitude over present instruments. By pushing the upper operating frequency beyond 1 mm, this facility will push the upper frequency limits of ground based observations.

The mm region is fundamentally interesting for astronomy since this is where the cold matter in the universe, the gas and dust from which stars, planets, and galaxies form, can be studied. Furthermore, the microwave background, and the dust and fine-structure cooling line radiation emitted by highly red-shifted objects is in principle observable in this wavelength range. This instument will be able to investigate a very broad category of objects in the universe.

Most interesting problems in astronomy require high angular resolution. The MMA will provide resolution comparable to that attainable only from space with the Hubble Space Telescope in the optical (when it is fully repaired) and by large 8-m class near-IR instruments of the future which might be equipped with sophisticated adaptive optics to compensate for atmospheric turbulence. Yet the MMA will be far cheaper than any possible comparable resolution space-based instrument at any wavelength, and far cheaper than the total amount still to be spent before adaptive optics can be made to work with comparable resolution.

This is not to say that the MMA will do all interesting mm-wave science of the future. There will still be a need for dedicated moderate sized instruments with which to make large scale low angular resolution maps of the distribution of gas and dust in the sky. Eventually, most-of-the-sky surveys, analogous to the Palomar Sky Survey, should be performed. Coverage of substantial portions of the sky with the MMA is not feasible. Furthermore, there is a very important need for a large single dish, which can (1) survey efficiently the very faint signals from distant galaxies, especially in the continuum where incoherent ultra-high bandwidth bolometers will have a large advantage, (2) survey large bandwidths efficiently in search for new molecules, or (3) study emission (including polarization) from dust grains.

Nevertheless, the MMA concept is so spectacular in its potential impact on science, that it is the best instrument to build in the 1990s. The effective use of any modern single dish requires the development of large focal plane arrays of receivers

Construction of the MMA will force the technology to ripen, making multiple receivers for such telecopes possible. Furthermore, the investment in the MMA will provide greater improvements in angular resolution and sensitivity than a comparable capital outlay is likely to produce in a large single dish. For these and other reasons, the MMA is the most exciting astronomical instrument to contemplate building at the present time.

I will now comment on several specific aspects of the MMA proposal. I find that the estimated cost of receiver development to be too low. Using to cost of 160K per receiver pair given on page 154, I estimate that the 320 SIS mixers for the MMA will cost about 26M, which represents the lions share of the 38M budgeted for all electronics on page 152. To reach a degree of reliability required for the maintenance free operation of 300 or more systems will require a tremendous improvement in systems and component quality. I worry that this proposal does not budget sufficient resources to reach this level of quality.

The development of 4 K refrigerators is also a serious problem. Although many companies make cryostats of the sort needed for the MMA, I'm not convinced that any of them have sufficient reliability to satisfy the needs of the MMA. With 40 antennas, with several refrigerators per antenna, the mean time between failure must be extremely low. Most groups who have used the present generation of refrigerators have spent lots of time debugging problems.

Consideration of future capabilities and needs should be a key element in the design of any major instrument whose expected lifetime is many decades. The most likely future direction I see for the MMA is the implementation of multiple feeds in the focal plane which would speed up the rate at which large areas of the sky can be mosaiced. Although the fabrication of even more receivers is hard to contemplate at present, we should anticipate that over the operational lifetime of the MMA, focal plane array technology might mature to the point where such arrays would be usable on the MMA. This might impact two considerations in the present design: (1) The antennas should have a wide usable field of view. Optical designs such as the Schwartzchild or Ritchey-Cretien should be considered over the classical Cassegrain configuration. These changes represent mild departures from the standard curves on the primary and secondary, and could be implemented without additional cost to the dish fabrication. The receiver room should be have lots of space for future expansion. (2) Consideration should be given to the ease with which the IF system could be expanded in the future. Cable tunnels should be easily accessible.

The history of computation in the last 1 or 2 decades should provide some important lessons about how to implement a computing plan at the MMA. A static hardware base is not suitable, since there is evidence that computing speed and power will continue to get less expensive. With modern languages, most code can ported within a family of machines. Therefore, the MMA should budget for the continuing renewal of its computing hardware base. At the present, it appears to be cost effective to replace old hardware with new every 3 to 5 years. The MMA management plan should absorb some of the costs required for such upgrades. The VLA has also demonstrated that new algorithms can dramatically improve the the imaging quality of a synthesis array. Sometimes, these algorithms are expensive in computer time. Since there is no reason to believe that we are at the end of developing innovative new software, it is imperative to maintain flexibility with respect to the hardware platforms on which the software runs.

Furthermore, since the MMA will be primarily used in spectral line mode, I'm not convinced by the expectation that the computing load will be less with the MMA than with the VLA. On the contrary, I expect that with the need to mosaic, the load will be much greater.

I also worry about the relationship between the MMA proposal, and the other interferometer projects in this country (OVRO, BIMA, and the Smithsonian SMA). I have heard many recent suggestions that the MMA project staff should get together with both OVRO and BIMA, at least to make use of the expertise developed in California, and possibly to share technology development costs. We all have a common goal, the production of the best mm-array possible. In the present financial situation, the nation can't afford to dilute the effort in this field. I urge the NSF to encourage any and all cooperative efforts between NRAO, BIMA, OVRO, and the CFA. The most direct encouragement would be the provision of some funds to encourage such cooperation. Clearly, a shared electronics development program would benefit all. The NSF, in conjunction with these other institutions, should develop a far-sighted 10 year plan for the future of mm-wave interferometry, in order to maximally utilize available resources, and to minimize duplication of capital development.

Given NRAO's experience with the VLA and the VLBA, it it most experienced group in the world in the construction and operation of large synthesis arrays. Despite some suggestions I have heard to the contrary, no other group, in my opinion, has comparable competence to manage a project as large as the MMA. On the other hand, the smaller interferometry groups are at present more technically advanced and experienced in mm-wave interferometry. Therefore NRAO must take steps to take maximal advantage of this technology base. In addition to the above suggestions, NRAO and MMA staff should be encouraged to make visits to the other institutions for both technical exchanges, and to observe so as to improve NRAO's first-hand experience in this field. The NSF could play a vital role by providing funds for this type of exchange.

The Millimeter Array: Review of Proposal

I shall review NRAO's proposal for the Millimeter Array (henceforth MMA) under several headings: (1) the scientific case for the MMA; (2) the ability of NRAO to run the MMA; (3) the timeliness and feasibility of the MMA; (4) the schedule for construction and operation of the MMA; (5) the budget for the MMA; (6) the national and international significance of the MMA; and (7) should the project proceed?

1. Scientific Case

The MMA is conceived as an instrument that can make high-sensitivity, high-resolution maps of small regions of the sky in the atmospheric windows between 65 and 370 GHz. The principal strong astronomical signals that lie in these windows are emission lines from molecules such as CO, and thermal emission from dust at various temperatures. The best justification for the telescope is then that it will make excellent images of dust and line emission from stars and star-forming regions in our own galaxy and in other, relatively nearby, galaxies. In this respect the scientific case is clear and strong. There are many problems in the structures of star-forming regions that cannot be addressed with less-sensitive and lower-resolution telescopes (such as the 13-m telescope and the existing mm-wave arrays), although these telescopes have been highly productive in opening up the field of mm-wave astronomy. Much of section III of the proposal deals with the science that can be done with better data on gas and dust in and near star-forming regions in galaxies. The MMA would be an unsurpassed tool for obtaining such data.

Some of the other science contained in Section III of the MMA proposal is tentative or highly contentious. This is hardly surprising, since the quality of much of the mm-wave data on the objects discussed is poor, and therefore our existing knowledge of their physics is very incomplete. The lack of a high-quality mm-wave telescope in the US has severely hampered the development of all the fields in astronomy that rely on high-resolution molecular-line data. Some of this deficiency is being cured by the OVRO and BIMA arrays, on whose work the MMA would build, but there is a continuing demand for higher sensitivity and resolution. No existing telescope in the US has close to the sensitivity that the MMA would have. And if the MMA is developed as a highly flexible instrument, it will provide capabilities for mm-wave observing that will vastly improve the state of our knowledge on astrochemistry, star formation and evolution, and the state of the interstellar medium. Most of the reservations I have about the project center on the issue of whether the MMA will be a sufficiently flexible tool for the varied projects for which it should be used.

The MMA is a compromise design: if it can be operated in the many modes that are necessary, it will still be incapable of some projects that might be desirable. Some projects require much larger bandwidths at the smaller baseline (continuum detection experiments) — the MMA will not provide bandwidths > 1 GHz. Some projects need higher resolution (e.g., mapping stellar disks) — the longest MMA baseline, 3 km, is not sufficient for these purposes. And some projects would certainly like more collecting area (for which larger antennas would be superior). However, if the MMA is adequately flexible, the compromise that has been selected will provide extensive information that could not have been gained in any other way, and will clarify the need for, and the technology required for, the other projects that might be imagined.

In summary, the MMA would certainly be of interest to a wide range of astrophysicists — particularly those concerned with stellar winds and the inter-relation of stars and the galactic environment. I am convinced that there is a strong case for *an enhanced mm-wave capability for US astronomy.* In the following sections I attempt to answer the question whether the MMA is the *best* instrument to provide this capability.

2. NRAO and the MMA

NRAO is the only organisation in the US that has attempted to run a large interferometer array as a common-user facility. It therefore has considerable experience that would help it in running an array of this complexity. NRAO has also a well-respected receiver laboratory, where the development of state-of-the-art receivers for cm-wave and mm-wave receivers has proceeded for a number of years. If any organisation is able to operate a telescope like the MMA, it is probably NRAO. No University, or University consortium, could conceivably provide the same amount of effort, or bring a similar amount of experience to the operation of such a telescope.

This said, NRAO has some organisational drawbacks. The experience of operating a mm-wave telescope is confined to the group based in Tucson, and their experience is confined to a single dish instrument. No experience of operating a mm-wave array is currently available at NRAO. Few of the NRAO staff in the major installations (at Socorro and Charlottesville) have mm-wave observing experience, or experience with the types of data that the MMA will obtain. NRAO must, therefore, undertake a significant hiring program to build up its expertise in these fields. Since the pool of available experts is small (largely confined to personnel of the OVRO and BIMA arrays), I anticipate that it will be difficult to accumulate enough experienced personnel to construct the array on the short (seven year) timescale that NRAO is proposing.

The issue of the operation of the telescope brings up another worry. In order for the MMA to operate, it requires $40 \times 5 \times 2$ receivers in 40×3 packages. Progress in the design and development of receivers for mm wavelengths is still rapid, so that any one of these receiver systems will become obsolete in less than five years. This means that NRAO will have to continuously supply receivers for the array at the rate of about 2 receivers per week. The associated development and construction jobs are difficult and time-consuming, and I cannot imagine the current staff of NRAO as being nearly sufficient to maintain the system at the level at which it will be required to operate.

Operating the telescope will also require the development of powerful computer software tools that represent a radical departure from NRAO's previous software designs. In order to conduct the scientific program, the computer operating system must be highly flexible and capable of a wide variety of different modes (for example, imaging at different frequencies in different sub-arrays, as may be useful for observing solar transient phenomena; or, perhaps, imaging with the bandwidth split to cover several lines). I anticipate that the observer would be faced with a formidably difficult telescope control task unless NRAO develops intelligent software tools for setting up the observing program (far better than the primitive tasks currently available for the VLA). Also the control of the telescope during observation will be difficult, and the real-time data previewing system required to allow the observer to interact with the incoming data (essential to achieve the highest observing efficiency) will require considerable computing power. Finally, the routine analysis of imaging spectral line data will be catastrophically time-consuming without the development of much improved analysis tools (AIPS would be hopeless). No such capabilities have been provided for the VLA after a decade of software development. I anticipate that NRAO will need to invest considerable effort in the design of expert systems to schedule and run the MMA.

3. Timeliness; feasibility

The scientific timeliness of a major new mm-wave facility in the US is not in question: indeed, such a facility is long overdue, and the US has allowed much of its original advantage in mm-wave astronomy to be lost to the European and Japanese groups.

A special emphasis on the timeliness of a major mm-wave facility is provided by the IRAS data-base: the extensive information on the dust properties of stars, galaxies, and the interstellar medium available from that highly-successful satellite is currently leading to a number of investigations that promise to improve our knowledge of a many astronomical objects. These investigations are considerably handicapped by the small collecting areas or low resolutions of the mm-wave telescopes that are available.

As an instrument, the MMA is marginally feasible at present — which probably means that it will be entirely feasible by the time that much of the major construction is under way. The MMA represents a major jump in mm-wave observing capabilities over any telescope that has been constructed before (though borrowing design features from several) — in complexity of operation, by all measures (correlator, number of antennas, operating modes) the change in the mm-wave capability of the US provided by the MMA is far greater than the change in the cm-wave capability that was provided by the VLA. As an instrument of high sophistication, the MMA represents an appreciable risk, particularly if its construction and design are rushed or if untested features are permitted. I am skeptical of the schedule which has NRAO constructing the telescope within 7 years: and I am even somewhat skeptical of NRAO's ability to construct the MMA as anything other than a mm-wave VLA. The MMA, to be successful, must be far more than a higher-frequency VLA.

It is my judgment that a homogeneous array design as advocated by NRAO (and both praised and vilified by eMail in a continuing exchange of views in the mm-wave community) is the correct decision for the MMA. Little or no improvement in the imaging performance of the MMA is achieved if a larger element is added to the array (unless it is added as a distant outrider, to provide extra resolution — this may be a desirably later development of the array, when the initial system has been completed).

The major criterion on which this array will stand or fall is, therefore, the ability of an astronomer to use it flexibly in complicated modes. Some data previewing is also desirable, and real-time reconfiguring of the telescope (for example to take advantage of the rare periods of good weather at the highest frequencies) is essential to its efficient operation.

The data-reduction facilities required to use the MMA are considerably more complicated than those required for the VLA. Line-mode observing with the VLA produces vast amounts of data, which are then difficult and slow to reduce using the AIPS software: my institution is equipped with excellent vectorized computing facilities, but even a small number of line projects at the VLA by my colleagues or myself are capable of swamping those computer facilities for months at a time. The MMA, with its larger number of baselines, spectral channels, and modes, will cause an even larger data-analysis problem: not only at NRAO, but also in the Universities, so that the NSF must anticipate a large hidden cost in the MMA through the need to provide enchanced computing facilities to the many investigators who will use it. If mosaicing becomes a major part of the observing strategy with the MMA (as is almost certain), then non-linear mosaic reduction will dominate the computing budget even if only a few percent of the observing time is dedicated to mosaic observing. The possibility of line-mode snapshot observing of many molecular clouds (an obvious proposal for the MMA!) is particularly daunting: the major part of such a project becomes the data-analysis rather than the data-taking.

Several elements of the design of the MMA deserve comment:

(a) The maximum bandwidth that is likely to be available from the current design is 1 GHz. This puts a strong limit on the continuum sensitivity of the instrument: the receivers are intrinsically much broader-band, and a better design should provide all this bandwidth to continuum observers.

(b) The correlator must be able to provide several sub-bands within the overall bandpass of the receivers being used: three or four lines could then be observed simultaneously, with a considerable improvement in observing efficiency. This facility may also be essential to allow proper calibration of the instrument: one sub-band containing weak lines could be calibrated with the help of a different sub-band containing a maser line.

(c) Several modes of observing (single-dish like, mosaicing, split line arrays, phased array for VLBI) should be supported simultaneously. Since I anticipate that much receiver work will be necessary, I expect that some of the antennas will be periodically used to test new receivers or fix older receivers, and this activity must be able to go on (with full astronomical testing) at the same time as common-user observing.

(d) The antenna design must be carefully done: better than one arcsecond pointing is needed if the pointing accuracy is not to have a significant effect on the mosaicing error budget. This will be difficult, but since errors in the antenna design will limit the MMA permanently, the antennas are the most *permanently critical* part of the design.

(e) The arguments for 9-mm operation of the MMA seem rather thin: this band is probably the least justified of any of the MMA bands. Yet it is possible that a large component of the cost of the antennas will be driven by the need to support 9-mm operation: for example, the secondary and tertiary mirrors must be much larger for 9-mm work than for 3-mm work, and this reverberates into the size of the motors used to nutate the subreflector, etc.

(f) The issue of the site cannot be sufficiently emphasized. At the target top frequency of operation, 345 GHz, the weather will permit the telescope to be used less than 15 per cent of the time (assuming that the site test's 79 per cent efficiency reflects a random sample, and not the best 79 per cent, of the weather). This forces the array to work through contingency scheduling: observations that require the best weather must be able to commandeer the telescope when the weather is good, which implies, once again, that frequency agility and flexibility of operation and scheduling will be essential features of the MMA.

(g) The technical question of LO power supply needs study urgently: a full solution to this problem is needed before the receiver design can be selected and the receivers constructed.

(h) The reliability of the receivers must be unusually high. Unlike room-temeperature receivers, in which repair work can often be done quickly, these receivers will be embedded in cryogenic systems, so that a receiver with a problem will be unavailable for days rather than hours. With 400 receivers to maintain, a single receiver's MTBF as high as 3 months would still imply that three receivers will fail per day, and hence that (for plausible repair schedules) that about ten receivers will be undergoing repair at any one time. A significant skilled staff will be needed for receiver maintenance.

4. Schedule

Several major systems studies are required before the schedule for the MMA can be definitively set: studies on the LO power, the receiver design and reliability, the antenna design and the effects of weather on the antennas, the correlator architecture, and the software. If the telescope is to be contructed in the next decade, these studies must be started now: I anticipate that three years of preliminary work are needed before orders for any of the major components of the array can be prepared. Site preparation and construction might therefore start in 1995 if funding began immediately. I would be very surprised to see much work becoming possible earlier.

NRAO's schedule for constructing the MMA involves building the 400 receivers for the array (and testing them, and readying them for full operation) in three years. I feel that this is highly optimistic. If the 9-mm system is retained in the design, it could certainly be prepared in this time, since 9-mm systems are now readily constructed. The other two systems require considerable skill, and individual attention, in the assembly, and that two reliable receivers per week could be constructed, tested, and installed seems unlikely. I predict that it will be the receivers that limit the time taken for the telescope to become fully operational.

5. Budget

Just as with the schedule, I believe the budget for construction is too low (at $ 120M), since the array requires extensive innovative technology. The antenna pointing requirements are difficult, and I'm not convinced that the mount costs are adequate if 1 arcsec pointing is to be achieved. The receivers and the LO system need to be highly reliable, and there should be an adequate complement of spare receivers for rapid repair. Much the same can be said for the correlator system, which also requires an unusual degree of flexibility to allow for good calibration of the array and its efficient use. The computing operation of the array and the data-reduction will also be expensive: the figures used in Table X-1 seem generous for the hardware and parsimonious for the software and algorithm development efforts, which will have to be unusually intensive. In summary, I would not be surprised to see a final cost greater than $ 150M.

However, the construction costs are far more plausible than the operating costs. The recurring equipment cost for the array is likely to exceed $ 6M, allowing for continuous improvements to the receivers, the LO system, the correlator, and the computing. Accepting the other costs in Table IX-2, I estimate the annual operating cost of the array to be in excess of $ 11M *if the array is to be adequately maintained and improved with time.* If no allowance is made for improving the technology on the telescope as receiver technology improves, and as the scientific emphasis of work on the telescope changes with the new results that it will produce, then the array should not be built: modern projects *must* undergo continuous development if they are to remain at the forefront of research.

Let me emphasize, once again, the magnitude of the software development effort that the MMA requires. The existing VLA software (both on the telescope, and the software provided as tools to the observer) would be hopelessly inadequate for the MMA. Based on my experiences with the VLA system, I would expect that no observer could hope to set up an observing schedule and reduce a plasibly (fairly-complicated) line observation with the MMA in less than a year of real time. With this sort of overhead, few observers would be willing to use the telescope. The software effort required can be compared with that needed to build the operating system for a modern supercomputer (which the MMA and the VLA resemble in many characteristics): the effort is measured in person-centuries.

A major hidden cost in the proposal is the cost to the NSF of equipping observers at Universities and research Institutes with the computing power that they would need to reduce data from the MMA. The proposal leads me to the estimate that a minimum of 10 CPU hours on a mini-supercomputer will be needed to reduce one hour of data from the MMA. About ten or twenty per cent of this data-reduction will be done with the facilities built up by NRAO for the MMA — the remaining eighty per cent must be done at the observers' home institutions. Based on the cost table, Table X-1, this will cost the NSF at least another \$ 25M to \$ 30M, spread across the country.

A related cost will be that of facilities for the real-time examination of data taken by the MMA at distant observers' home institutions. Such facilities are needed if the MMA is to make the best use of the varying observing conditions, and if the observers are to make the best use of their observing time. I anticipate that the system adopted will resemble the systems used for remote optical observing, in providing direct access to the telescope control computer and a real-time display of images calculated by that computer. Of course, such facilities are useless unless the data can be reduced (in a preliminary form) in real time (which is why such a facility would not be useful for the VLA, for example). Based on the costs of the facilities used (or not used) by optical astronomers, and the extensive geographical dispersion of observers, I would expect the NSF to incur an additional cost in excess of \$ 10M to provide these communications facilities.

6. National and international significance

The MMA would be a unique telescope. In collecting area, resolution, and frequency range it is clearly superior to any of the arrays (BIMA, OVRO, Nobeyama) or single dishes (JCMT, CSO, Kitt Peak, IRAM) that currently exist. This implies not only that it can expect to make discoveries that would otherwise be closed to study, but also that it can improve dramatically our present state of knowledge of many enigmatic objects. There is some overlap in its capabilities with existing instruments; but this is not a negative feature, since single-dish telescopes are easier to use for long-term large-scale survey type work, and much easier to use for trying out innovative new receivers (for new frequency bands or using new physical principles). Some overlap with the Smithsonian's SMA will also arise, but only at the shortest wavelengths of the MMA, where it operates least often: the SMA, on a better site, is intended to observe mostly at shorter wavelengths, providing a complementary imaging capability for studying many of the same objects.

However, the MMA, as an instrument, may also act to restrict further opportunities for the development of US mm-wave science. The presence of a large array, which is capable of many interesting projects, may lead to the dangerous frame of mind that *any* important projects can be done on the MMA. This is incorrect. As I have noted above, the MMA has an intrinsic bandwidth limitation (set by the electronics). Wide-bandwidth observations, such as might achieve the highest continuum sensitivity, cannot be performed with the MMA — a single dish equipped with a bolometer array is a better instrument for such work, and the construction of the MMA should not imply that single dish systems will become obsolete.

A more pernicious possibility is that the existence of the MMA will remove the opportunity for University departments or other organisations to develop new receiver technology. Without a telescope platform to try out a new receiver, further improvements in receiver technology could be stifled. It is important, therefore, that the MMA be flexible enough to support the use of one or several of its antennas as single dish systems for the development or testing of new receivers — in the same way that NRAO's single-dish systems have allowed user instrumentation to be mounted and used. Such a use of individual antennas, or groups of antennas, on the MMA should be available by open proposal in the same way that the use of the overall instrument should be available.

Finally, if the US is to remain competitive in mm-wave astronomy during the construction of the MMA (which will take the training lives of two generations of research students), it is vital that the Kitt Peak telescope be maintained (and its receivers continuously improved) as a research tool. It would be highly desirable to improve and maintain operation with the Kitt Peak telescope even after completion of the MMA — if mm-wave VLBI is to develop, it is necessary that there be several sensitive mm-wave antennas available in the continental US.

7. Conclusion

My main conclusions about this proposal can be expressed as interest in the completed instrument, but skepticism that the instrument as advertised can be built within the next seven years. In the framework of the 'additional' questions in Pankonin's letter:

(a) Is the scientific case established?

Yes. The MMA would be an outstanding instrument for many topics of research. It is not the ideal instrument for *all* purposes in mm-wave research, but it is a good compromise between the various design requirements imposed by the major research subjects.

(b) Does the design meet all scientific applications?

No. The MMA is a compromise instrument. More bandwidth is needed in some applications, more spectral resolution is needed in others, longer baselines in a third. But it is a good compromise, meeting most requirements, and some of the handicaps imposed by the design can be fixed by later improvements.

(c) Is the management approach adequate?

There is no indication of the management approach in the proposal, so the answer to this question is indeterminate. Several major hurdles to the construction of the MMA remain (see section 3, above) — if the project is to proceed, then close *internal* management attention at NRAO and close *community* attention through a consultative committee are essential.

(d) Are the cost estimates reliable?

No. The construction estimate is fairly uncertain because of the new technology that is required for the MMA to operate. There is also an appreciable collateral cost to the NSF in providing adequate data-access and data-analysis facilities for the community, so that the telescope can be used efficiently. The operating budget seems to me to be wildly low: the maintenance and improvement of the receivers and other sensitive equipment are very under-budgeted.

(e) Are there substantial risks in the project?

Yes. The most obvious risk is that the telescope will be so complicated to use that it will never become a common-user instrument at all. If the telescope operations can be made flexible and transparent to an outside user, then the benefits that the MMA will have for mm-wave science will be tremendous. There are also risks in the reliability of the receivers and the LO system, in the pointing accuracy of the telescopes, and in the complexity of the correlator.

In summary, the MMA is a project whose successful realization would give the US a substantial lead in mm-wave astronomy (and in mm-wave technology as a whole). It is an expensive project, and somewhat risky, since the technology is not entirely proven, and the telescope must be much more flexible than any other interferometer array (or single dish telescope). The successful completion of the project demands full NRAO support, including a program of hiring mm-wave experts (most likely from the existing mm-wave arrays) and control systems specialists (most likely from industry or defense programs). With adequate NRAO, NSF, and community support, I believe that these hurdles can be vaulted, and that the MMA would be a major scientific resource for the US in the first quarter of the twenty-first century. My overall conclusion is, therefore, that *the project should proceed, subject to careful community and NSF supervision.*

There is no question in my mind that the science which can be done with an instrument like the MMA is great. It will undoubtedly open up new areas of planetary research, as e.g. cometary and asteroid research, and the search for extra-solar planetary systems. It will also improve dramatically the planetary science done with both the BIMA and OVRO arrays.

However, I do have some critique on the schedule and development of both hardware and software. Usually an array like this will be built with matured technology, which is not the case with the MMA. In addition, I have the impression that NRAO, largely driven by the community, is still debating whether or not to add a large single dish to the array. So what I suggest to do is to split the proposal into two phases: Phase A, where the telescope gets designed, and all questions are properly addressed, and phase B, where the array gets actually built. So before phase B gets into effect, NRAO should re-submit a proposal based upon their findings from phase A.

Phase A, which should be funded immediately, should entail the following:

a) Improve on technical development; e.g. is it really possible to built 80 345 GHz recivers; that can only be done with well-proven technology.
You can likely built these receivers only once, so as long at the technique is not proven there likely will be upgrades in the receivers. It would be too bad if NRAO would start building them before they are optimized.

b) Software development: Mosaicing forms an important integral part in the design of the MMA. Many computer simulations have been done, which show that mosaicing is quite feasible. However, computer simulations and actual observations are quite different, I think. For example, I would like to see a mosaiced image of a large uniform disk, as a planet or the moon, before I trust mosaicing to better than 5-10 %. The NRAO staff should be able to demonstrate this with help of VLA data.

c) Site testing. I think it is very important to pursue further site testing before a decision is made as to where to build the array. If indeed the atmospheric phase stability does not improve with increasing altitude, one is left with only the improvement in opacity. Even though I think that should be the main driving force behind the site selection, the phase stability of the atmosphere cannot be ignored.

d) The single dish issue. There is still a big debate between various members of the millimeter community whether or not a large single dish should be part of the array. If so, the cost of the array will undoubtedly go up, and the budget should be re-assessed. My personal recommendation is to build a large millimeter interferometer, and forget about a single dish. Most, though not all, single dish projects can be done with the MMA in the single dish mode (see e). Obviously, a single dish is preferred if one likes to do much receiver development, but that is more appropriate to conduct in a university environment than at a national facility.

e) The single dish mode of the MMA. My impression is that one will be able to obtain data in the single dish mode while mapping a source. This is certainly a very interesting idea. However, my impression from the single dish community is that they are not convinced it will work. Using the VLA as a testbed, the NRAO staff should be able to develop the same idea for the VLA. Once done, they can demostrate that it works, e.g. by comparison of data taken in the most compact array configuration, D or preferrably E, with data obtained with a 100 m sing' dish telescope. In addition to testing this idea, the VLA would also greatly benefit from this additional technique. I think herewith in particular about cometary observations: the OH coma is large, and largely resolved out in the D-array. Measurements simultaneously in a single dish mode would be advantageous. The simultanuity in this particular case is very important because these objects are highly variable in time.

The most impressive aspect of this proposal is the overall strength of the science. Essentially every field of astronomy will benefit tremendously from this project. It represents an order of magnitude enhancement in sensitivity and resolution over existing instruments and will be the premier instrument of its kind in the world – these are the standards which should be met for projects of this magnitude and the MMA meets them. I will concentrate my comments on the science on the areas of molecular clouds and star formation. Before doing so, I pause to note several capabilities in other areas that I found particularly impressive: combined with AXAF, the MMA can give an independent measure of the Hubble constant to about 10 percent, using the Sunyaev-Zel'dovich effect; the MMA will be able to image starburst galaxies to a redshift of at least 10, allowing study of galaxy formation; the radii of many stars can be determined and compared to those determined by indirect means, providing a vital check on basic data; and the MMA may be able to detect directly proto-Jupiters around other stars.

In the area of research on molecular clouds and star formation, the MMA is the instrument that is really needed most to make progress. While this field requires a broad spectrum of observational and theoretical progress, the MMA would clearly be the centerpiece. The reason for this preeminence is the order of magnitude improvement in sensitivity and resolution at wavelengths which are most critical for this field. The two basic probes of the material in star-forming clouds, and in planet-forming disks, are continuum emission from dust and spectral line emission from molecules. The dust emission increases as ν^3 or ν^4 in this spectral region, making the MMA operational capability at 350 GHz extremely valuable. This capability is also crucial for the molecular line emission, because the lines available at higher frequencies provide probes of warmer and denser gas; together with the lower frequency transitions of the same molecules, relatively complete pictures of the density and temperature structure of the star forming region can be constructed. Because of this need for multiple transitions, the broad frequency coverage and versatile receivers planned for the MMA are essential. The most exciting capability of the MMA in this area is the imaging, in both continuum and spectral lines, of disks around forming stars; the resolution of the MMA will allow us to examine the physical, dynamical, and chemical conditions in these disks down to scales comparable to that of giant planet formation. Together with the capability to detect forming giant planets in somewhat more evolved systems, these observations will finally allow a comparative study of solar system formation, providing us for the first time with the possibility of a real understanding of this essential step in our own origins.

In view of these comments, it is clear that I believe that the scientific case for the MMA is very well established. Let us turn to the question of whether the MMA, as proposed, can achieve its scientific objectives.

The MMA satisfies another criterion for projects of this magnitude; it pushes the state of the art in several technologies, providing a stimulus to national development. At the same time, the very ambitious technical goals seem to be within reach. The scientific goals require the following characteristics: a large collecting area, capable of being distributed in a flexible way to trade off resolution and field of view; extremely sensitive receivers over the whole range from 30-370 GHz, excluding only regions of high atmospheric opacity; the hardware and software which will allow imaging with high resolution over large areas, "single-dish" sensitive spectroscopy, and extremely sensitive imaging of single fields. The solution to these problems in the MMA proposal is the right one.

Consider first the need for large collecting area; one could imagine a large single dish instead of the MMA. While a single dish of equal collecting area might be cheaper to build and operate, it will be extremely hard to achieve the kind of pointing accuracy needed (a 50 m dish, operating at 350 GHz, would have a beam of about 4″, requiring pointing accuracy of better than 0.4″). In addition, it will be much less flexible; there would be no way to achieve the sub-arcsecond resolution promised by the MMA. Finally, it is clear that the only way we could ever achieve *truly* large collecting areas is by combining many smaller dishes; one could never build a 1000 m single dish, but one could expand the technique of the MMA to achieve arbitrarily large collecting areas. While this expansion is not proposed here, the MMA is a step in a direction that can be expanded in the next century, rather than a dead-end. It has been pointed out that some projects, notably sensitive spectral line searches, do not require the array and might be better done on a single large telescope outfitted with optimized receivers which would probably out-perform the MMA receivers, which must be simpler in order to operate so many of them. While this may be true, it neglects the fact that receivers at these frequencies are approaching the point where their noise will be dominated by atmospheric and spillover noise; if the MMA receivers approach this goal, as is proposed, there is no advantage in further optimizing receivers. On scientific grounds, I would also claim that the MMA is a better instrument for these spectral surveys because it can provide information on the spatial location and distribution of the rare molecules being searched for. With the clear evidence that different molecular species have different spatial distributions, it will be essential to determine the distribution of the new molecules to understand their role in the chemistry.

The approach of the MMA proposal on receivers is also correct; it is essential to have nearly complete spectral coverage and flexibility in choice of bands and tuning within a band. It would also be very valuable to allow observations with several receivers simultaneously, using beam splitters, and I encourage serious consideration of this option. The only question I have heard about the receiver plans is whether NRAO can actually achieve the ambitious goals of the project. This is not my field of expertise, but I note that the NRAO staff have developed some of the best receivers in the world. I support their intention to begin work on the receivers for the MMA as soon as possible.

The need for high frequency operation and long baselines have greatly restricted the possible sites. My own tastes would be to emphasize the high frequency capability and to place the array on a site like Mauna Kea. However, I recognize the need for the long baselines for some projects, as well as the added cost of construction and operation on Mauna Kea. Fortunately, some acceptable alternatives seem to exist, relatively near the existing array operations center for the VLA. The Magdalena mountains site seems preferable for the high frequency performance, but the other sites also look acceptable. Because of the well-known problems with site acquistion, this aspect should proceed expeditiously, but with extreme attention to environmental groups, in order to avoid crippling delays. In this area, the Magdalena mountains site would also seem to have the advantage of being a scientific reserve and (based on my visit to the site several years ago) already rather trashed from an environmentalist's point of view. However, I leave this issue to be determined; the NRAO plans in this area seem reasonable.

The goal of $1''$ pointing for the array antennas may be the hardest to meet. Experience with existing antennas would suggest that this goal can be met only with extreme and ongoing attention – probably impractical for an array of 40 antennas. However, the MMA has a chance to engineer the dishes from the start toward this goal and creative techniques may be found. However, it is important to note that the great majority of the science to be done with the MMA will not require pointing at this level. The main problem caused by mispointing is in the dynamic range of mosaicked images, but reference to figure IV-9 of the proposal indicates that an rms pointing error of $4''$ (surely achievable) still can provide a dynamic range of 200, better than the signal to noise will allow for most experiments that I can think of. While strong efforts should be made to achieve the best possible pointing, I do not see the difficulty of meeting the $1''$ goal as a problem with the proposal.

The need to mosaic large fields does raise the issue of the single dish again, though in a different context. The total power mode will solve the "zero-spacing" problem, and the compact configuration will solve many of the missing flux problems usually associated with interferometers. Indeed, the coverage of the u-v plane presented in figures IV-4 and IV-5 for the compact configuration looks very good. These figures support the claims that this instrument can be used very effectively like a single dish of 70 m diameter and modest aperture efficiency. The only serious flaw in the uv coverage is the necessary dip at spacings between that of the dish diameter and the shortest distance between antennas. The traditional solution to this problem is to add data from a single dish with about twice the diameter of the array dishes, hence an argument for a "central element" of about 16 m diameter. This element, present in earlier MMA designs, adds flexibility and would please some single-dish enthusiasts. However the current design has only a modest dip in the u-v coverage; and various techniques, such as more uniform illumination of the dishs and sub-aperture illumination, could be employed to alleviate this problem. NRAO has not foreclosed the possibility of adding a single dish, and careful studies of the advantages and disadvantages should be made, under the watchful eye of an oversight committee (see below) before a final decision is made. I would like to note however that there is no particular reason why the "central element" need be at the same site; in general, placement at a higher site would make more sense. Also, it might be more cost-effective to upgrade existing facilities, like the 12-m or even university telescopes, to perform this function than to build a new antenna. My own guess is that no central element is needed, but I think that the study should be done with an open mind.

The cost estimate is very high; as far as I know, this would be the most expensive ground-based astronomy project to date. Is the cost estimate accurate? This is outside my area of expertise, but I note that the driving cost is that of the antennas. There are many approaches to this problem and it is not clear which is the best. It is important to provide enough money soon for much more detailed design studies of the antennas, preferably pursuing several alternatives (for example, carbon-fiber versus aluminum) and then assess the costs more definitively.

This leads me to consider the management approach. NRAO has the best record I know of for bringing very large projects to completion within budget and with capabilities equal to or greater than those that were proposed. I have great confidence in the technical abilities of the NRAO personnel. My only area of concern is that there is very little expertise in millimeter-wave astronomy in the group that seems to be dominating the MMA planning and there is no expertise within NRAO in millimeter-wave interferometry. I believe that these deficiencies have contributed to a certain unease in the millimeter-wave user community about this project; this unease has been compounded by the perception that the user community is not being listened to.

Fortunately, these problems can be easily overcome. There are several millimeter-wave observers on the NRAO staff, and they should become more involved in the planning. Because of the NSF and other investment in the OVRO and BIMA arrays, there is now a great deal of experience in millimeter-wave interferometry available in this country. NRAO should tap this experience in two ways: NRAO personel involved in planning the MMA should spend extended periods on leave to the the existing arrays, observing and collaborating on software (and possibly hardware) development; and NRAO should hire several veterans of the existing interferometers to help design and build the MMA.

Finally, I believe that a strong oversight committee should be established; probably there should be two of these committees, with some overlapping membership. One committee would advise on technical issues, especially in the process of evaluating antenna alternatives, but they should also consider receiver plans and site selection criteria. The other committee should be concerned with keeping the science goals updated and checking that the more detailed designs are consistent with these goals; this committee should also ensure that the correlater/computer/software development meets the flexibility requirements that will ensure the efficient use of the MMA, not only as an imager, but also in its "single-dish" mode. It should also address the issue of refereeing, time allocation, and the consequences of the dynamic scheduling that will be necessary to make optimum use of the higher frequencies. Part of the concern of the "single-dish millimeter-wave" community is that they will never get time for their projects on the MMA. In part, this perception stems from experience with the VLA; in part, it stems from the rather peculiar method used by NRAO for refereeing proposals, in which the referee's identity is kept secret and all reviews are done by mail. This method has its advantages, but it can lead to perceptions of bias against certain kinds of science. The MMA may require a more conventional system, in which referees are known to the community and meet to discuss proposals; in this method, claims of bias could be examined in an objective way.

In considering these kinds of questions, I think that these oversight committees could be very valuable to this project. How to select the members is a vexing question. I think that NRAO should not choose all the members of the committees, especially for the second, more policy-oriented one, but that some should be chosen by the community through some kind of democratic process.

In conclusion, I would like to return to my earlier point; this is a very exciting project that will have great impact on all fields of astronomy and will contribute to the future scientific and technological strength of the United States. If I were given a choice of all the astronomical projects completed in the last decade, including the Hubble Space Telescope (without its flaws), and those underway or proposed for the current decade, I would have no hesitation in choosing the MMA as the most exciting, most important, and most likely to succeed. We must do it.

F

The Millimeter Array (MMA) as envisioned in the proposal represents a major instrument, with major impact for the field of astronomy. Beyond this, the MMA will be capable of "Big" science that will have a significant impact on other areas of science and technology, as well as affecting how we see the universe and our place in it. As a national facility, however, its uses for "small" science is also prodigious, enabling researchers from all types of institutions to pursue their own research interests.

In response to the particular points I have been asked to consider:

1. Is the scientific case for the MMA clearly established?

The answer is a resounding yes! From the inception of the idea for the MMA, the scientific community has been asked to think about what science could be done with the instrument, and what design considerations are most important to enable that science to be done. NRAO has done an exemplary job in condensing those ideas into a cogent digest of science, much of it revolutionary, that will be done with the MMA. It is important to recognize that for every foreseeable result of the MMA, many unforeseen results are likely to come as well.

continued -

2. Does the proposed design meet the needs of the scientific applications?

Here I cannot answer for all possible science to be done, and so will restrict myself to my own area of expertise -- solar and stellar physics.

Both the Sun and stars push the design in the area of required time resolution, since solar and stellar outbursts can have variations as fast as a few milliseconds. The only reference I found in the proposal to this issue was related to data rates, where the design capability was for 1024 channels in 10 s. For continuum observations (two channels) this corresponds to 20 ms, but of course there is more to high time resolution that data rates. I would like the upcoming "site" review to raise this issue and ensure that receiver and back-end design allows for at least 10-20 ms time resolution in continuum mode.

Another requirement for the Sun, one that has received some attention in the proposal, is that the dishes be low in specular reflection, as in the manufacture of the BIMA dishes. This is necessary for pointing at the Sun without warping or otherwise damaging the secondary reflecting element.

A final point is that extremely high spatial resolution would pay off in allowing the disks of nearby stars to be imaged, so that photospheric features could be seen directly. This would require an additional dish some tens of kilometers from the main site; a possibility mentioned in the proposal in connection with the Magdalena Mt. site. This possibility should continue to receive some attention.

3. Is the proposed management okay?

I strongly believe that NRAO should manage the construction and operation of the MMA, as proposed. The NRAO has a proven track record with the VLA and VLBA (the latter having funding related delays through no fault of NRAO). NRAO is responsive to input from the scientific community, and any other management scheme would, in my opinion, only waste money and reduce efficiency.

4. Are the costs and construction schedule reasonable?

There is always a certain amount of "creativity" involved in cost estimates involving future or leading-edge technology. I would especially describe the computing estimates as "visionary," yet the extrapolations into the future are backed by cogent arguments which appear reasonable, and I see no reason to disbelieve them.

One way to see that NRAO is trying to cut costs where frills are concerned is to note that the original straw-man design included one large dish, but the discussion of mosaicing with a densely packed array configuration convince me that such a large, costly dish will be unnecessary.

The other costs seem reasonable and well justified, including nothing that is not necessary to get the job done, although I am not qualified to judge this aspect of the proposal too closely.

As for the schedule, it looks possible if the full funding level is available when the schedule requires it. The only question I have is in the site selection, which is scheduled for this year. I was confused as to whether the site candidates had been reduced to three or not. Some parts of the discussion seemed to imply that other sites were still under consideration.

In conclusion, the scientific justification for building the MMA is overwhelming, and the instrument as proposed will be capable of delivering on the expectations. It will be necessary to keep the scientific community involved all through the designing stage, to ensure that some compromise does not adversely impact the science. The MMA should be built, and the proposed design is the way to do it.

With regard to the 4 questions posed to me:

I. Is the scientific case for the MMA clearly established?

I think that there is a very powerful case for further development of our capabilities to perform millimeter astronomy. Therefore, I strongly endorse the basic thrust of this proposal which makes compelling arguments that a 40 antenna array will be very powerful and a great improvement over current facilities. This proposal does not demonstrate that this configuration is the optimal way to proceed at this time; I'm not sure what are the budget and other constraints that are relevant.

Even though the scientific case that is presented is excellent, for the record let me note a few comments:

1. Figure II-5 is misleading: The caption describes a range in possible observing time from one minute to eight hours, or a factor of 480 increase in observing time. If the sensitivity improves as the (integration time)$^{1/2}$ (see equation 3 of the Proposal), then the sensitivity should improve by a factor of 22. The large vertical shaded areas imply improvements by a factor of 100.

2. The statement (p. 24) that confusion will be important in every MMA beam for continuum sources of 1 mJy may mean that driving the system to reach sensitivities of 0.01 mJy (Figure II-5) is not sensible. There is very little discussion about confusion in this proposal; I assume it is not important. However, this statement about confusing continuum sources should receive greater attention.

3. I don't think that the imaging quality of the Hubble Space Telescope should be used as the goal to match for the MMA. (Is the statement on p. 27 that the MMA will have better angular resolution than the Hubble Space Telescope really correct?) Scientifically, it would make more sense to match the angular resolution with what might be available with large format IR arrays on large ground-based telescopes at good sites such as Keck. Rather surprisingly, little connection is made in this proposal between radio and infrared work, and there seems to be little thought addressed to the impact that infrared arrays may have in astronomy.

Some details:

1. The CO emission from the cooling flow galaxy, NGC 1275, could be studied to determine the relationship between the hot X-ray emitting gas that is being accreted and the cold, molecular gas. (p. 27) This is at least one elliptical galaxy where we think we know something about the origin (if not the fate) of the molecular gas.
2. (p. 28) Some S0 galaxies may have on-going star formation (Gregg, 1989, *ApJ*, **337**, 45).
3. I really doubt that anyone is going to measure the Hubble constant from the tidal radii

of GMC's (p. 29).
4. To use the Yale Bright Star Catalogue to estimate the number of stars whose photospheres might be detected with the MMA is slightly misleading since it assumes that all the southern stars will be observable. There are plenty of M stars to study, but there are so few main sequence stars in Figure III-9, that it is a matter of some interest whether the candidate stars are northern or southern. As an added point, if star positions are going to be dermined accurately enough to reveal low-mass (planetary) companions, it will be possible to measure accurate parallaxes (p. 37). Such a capability would be very valuable.
5. The mm emission from disks (p. 52) around main sequence stars such as Vega have been detected (Chini, Krugel, Kreysa 1990, *Astr. Ap.*, **227**, L5). This could develop into a very exciting application of the MMA.
6. Section III.6 (pp. 62-74) is poorly prepared. For example, some OH/IR stars such as NML Cyg and VY CMa have luminosities close to 10^6 $L_\odot$, much more than "a few tens of thousands solar luminosities" (p. 62). That is, there is a confusion in the proposal between red supergiants and AGB stars. Also, it is by no means clear that the AGB stars dominate the total mass ever returned by stellar evolution processes (p. 63). The Figure caption for III-19 refers to OH 127.8-0.0; the data are apparently for OH26.5+0.6. There is no evidence to support the use of the Reimers' relationship for AGB stars (equation 8). No AGB superwind of 10^{-3} $M_\odot$ yr^{-1} has ever been identified (p. 64); the highest mass loss rate for such a star is probably no more than 10^{-4} $M_\odot$ yr^{-1}. The statement that inner envelope regions closer than 10^{16} cm to the star (p. 69) have not been probed ignores an enormous amount of VLBI studies of masers and infrared sutdies. There are plenty of other problems as well in this Section.

II. Does the design meet the needs of the scientific applications?

As far as I can tell, the answer is yes.

III. Is the management approach appropriate?

I believe that there are some unanswered questions with regard to this point. In particular, Section IX, "Operations" (pp. 151-152) is very thin, and does not address at least three important issues.

1. This will be a facility for the national community: how many proposals and users do they expect to support? Is the support staff sufficient (or too much) to meet this anticipated demand?
2. As stated in the Introduction (p. 1), much of the thrust in millimeter astronomy now and in the future will be foreign. In the past, NRAO has had a philosophy of accepting proposals from all nationalities. This has been relatively easy to do when most proposals are from Americans. Will this be true ten years from now? If not, what will be NRAO's policy?
3. I would guess that a very large fraction of the MMA time will be devoted to millimeter spectroscopy. The NRAO does not have a large scientific staff involved with these topics. Last year, the NRAO even considered closing down the 12m telescope. Will the NRAO have

the institutional desire to increase its scientific participation in millimeter spectroscopy? This will be relatively easy if its budget increase in real terms by a large amount, but what if the budget does not increase? Will there be a redirection of institutional priorities?

IV. Are the cost estimates reasonable?

I believe that there is at least two areas where indirectly related costs are not addressed in this document.

1. The MMA will be a user facility; it will be necessary to support the users financially. It is no secret that NSF support for the individual investigators is becoming more and more difficult to obtain. However, even if the money is not going to come directly out of NRAO's pockets, it will come from the NSF's budget or there will not be a large, viable community to use the MMA. Without any trouble, I think that this sum could be in excess of $1 million a year in expenses directly required to support the operations. Assume: 20 advanced graduate students doing work with the MMA ($25,000 a student for salary, overhead and benefits); 200 published papers a year (publication costs $1,000 a paper) and 500 people-trips a year to NRAO and/or MMA to acquire and reduce the data ($600 trip). Maybe these numbers are wrong, but I suspect, if anything, they are very conservative.

2. There is an indirect cost as well as a benefit to the MMA. An underlying question behind the entire MMA proposal that is not addressed is the future of Hat Creek and Owens Valley. Is the NSF going to support both University and national facilities? Has it thought about this issue?

This proposal is for a really fabulous instrument by an organization which has an unbroken record of accomplishment and success and which has provided state of the art instrumentation which is flexible, unprecedentedly powerful and which is used by the entire astronomical communities of the US and the rest of the world.

It makes sense to deal with this proposal by answering the questions posed by the NSF's covering letter.

1. Is the scientific case clearly established? Most emphatically yes. The richness of the science discussed in the first part of this proposal is absolutely unprecedented in my experience. The topics addressed by this instrument cover the whole range of present day astronomy and astrophysics. This instrument **really** gets at questions ranging from galaxy evolution across the whole redshift range and galaxy formation itself (worth building the instrument for all on their own) to the detection of planetary and protoplanetary systems. These topics are, somehow, perennially appealing and loom large in any proposal for new instruments. This is the first that will actually deliver. It is striking that this instrument addresses both the 'big picture', i.e. large scale structure and galaxy formation, and the 'small details', such as star formation and molecular cloud chemistry, which make the whole wonderful thing work. At the beginning of the science section the performance of this array is compared with that claimed by propagandists for the late unlamented HST. There's simply no comparison; this array is enormously more versatile, powerful and effective. I was tremendously struck at the science workshops by how all this lovely science bubbled effortlessly forth from the participants. If one can think of all this stuff now, how much more wonderful work will it actually do? This is far and away the most exciting astronomical instrument proposed in the last 30 years. It should receive the very highest priority for funding.

2. Yes, as far as I can tell, the design meets the needs of the scientific applications very well. I'd put in a plea here for ensuring if at all possible (i.e. so long as the high frequency performance isn't compromised) that the site be such that the baselines can be expanded if this seems possible and desirable. I'm also a little disappointed that the spectacular South American sites seem prohibitively difficult.

3. Management. Unfortunately this is hardly addressed in this proposal. Yes, NRAO has a really wonderful record, but this project will be underway while it is operating the VLA, the 12 meter, the GBT, the VLBA and the 43 meter. These facilities all need running and maintainance, and it is abundantly clear that NRAO is struggling right now, thanks in no small part of course to lack of funding. This array will be the flagship of NRAO and probably of US astronomy. I'd like to see much more attention to management issues by both NRAO and the NSF. A major worry stems from the possibility that there is no management structure in place at all; at recent meetings the project has seemed somewhat headless. Surely if we've learned anything at all from the ghastly ST fiasco it's that these big projects not be undertaken without a strong chain of responsibility, i.e. someone in charge.

4. Budget. I have an unquiet feeling about this, too, in the sense that it seems to be too low. This sort of project is almost impossible to cost accurately, to be sure, since

some of the most vital components, such as the correlators and the computers, are likely to decrease quite drastically in cost. However, the receiver budget in particular seems far too low - at present day costs, the receivers themselves would cost more than $ 120 M. Something that NRAO and the NSF should do now is to fund receiver, correlator and software research groups devoted to this project, to the tune of a few $ M a year to begin research and development in preparation for building the array. It would be good to see some sort of management structure and contingency planning.

It seems vital to me to get on with this project right now. The funding should be supplied in large enough doses to finish the project on budget and on time. I can't think of any project more likely than this one to inject some much needed enthusiasm, drive, honesty and hope into US astronomy.

The Millimeter Array - AUI/NRAO

This proposal, to build a state-of-the-art millimeter wavelength telescope, is the outgrowth of discussions within the radio astronomy community over the last decade. The proposal and the instrument are, by their very nature, complex and I will address each of the various aspects of the proposal separately.

Astronomical Needs: There can be no doubt about the need for a first class, national facility for millimeter wavelength astronomy in the US. The scientific case outlined in the proposal is a summary of the scientific case put forward by the astronomical community during NRAO/MMA workshops. NRAO has done a good job in involving the community in this aspect of the proposal. As a result, the proposal clearly outlines the scientific need for a major new, millimeter wavelength telescope. There is naturally a bias in the scientific discussion toward the current research interests of the astronomical community. However, I believe that the authors also realize the potential of the instrument for future, unknown astronomical projects. It is important to make sure that the design of the telescope allows these future prospects to be easily realized.

I have only one negative comment regarding the scientific discussion in the proposal. Although, the various areas of potential research are well outlined, there isn't any discussion as to how various research areas impact the overall design of the telescope. In several areas, there are tradeoffs between the scientific capabilities and the design (and cost) of the instrument. These topics should be explored explicitly and discussed within the millimeter wavelength astronomical community. For example, the desire to do solar observations places severe design constraints on the antenna and reflector surface designs. The desire for a high resolution array configuration severely restricts the site possibilities. A study and analysis of the tradeoffs and the compromises on such areas would be extremely valuable in the conceptual design of the telescope. Although some areas of research could suffer from such compromises, others would benefit.

In general, the scientific case is clearly established. The millimeter astronomy community's desire for a major, first class facility is well documented.

Wavelength Range: The wavelength range of the MMA is specified as 0.9 to 9 mm. There is a vague discussion of extending the range to 0.6 mm, as the antenna design would allow this, however, receivers are not planned for that in the beginning. The frequency range appears to be set by the current research interests of the authors of the proposal rather than based on the projected broad interests of the community in the coming decades. This has been true throughout the evolution of the project. The major interest 5-10 years ago was 3 mm wavelength, especially CO work. The emphasis of the current proposal is on the 1.2 mm atmospheric window. As more astronomical observations are done at 300-500

GHz in the coming years with the new submillimeter telescopes available to the community, I expect a change of interest to even higher frequencies. Just two examples: (1) Observation of the CI line at 490 GHz will certainly become a standard tool in the coming decade. The MMA proposal recognizes this and discusses such observations for high red shift galaxies, but there isn't any provision planned for observations above 370 GHz. (2) The flux density of thermal dust emission rises sharply with frequency at millimeter and submillimeter wavelengths. Continuum observations of many objects will be enhanced in both resolution and intensity by observing at higher frequencies.

The decision on the wavelength range of the telescope should be guided by the atmospheric physics constraints on ground based observations, not current research interests or perceived competition with other projects. The atmospheric windows would dictate a range of frequency range of 70-500 GHz for the design of the MMA. There are broad bands where the atmosphere is opaque on either end of this range.

The frequency range below 70 GHz should be dropped from this project. Much of the 9 mm wavelength astronomy which is proposed, especially the compact configuration work, can be done with appropriate instrumentation on the new Green Bank Telescope. The 9 mm capability for the MMA was included in the design before the GBT was conceived and is no longer appropriate. In addition, inclusion of the long wavelength capabilities compromises the optics design of the antenna elements. The money would be better spent enhancing the short wavelength capabilities of the MMA.

Site Selection: The work on site selection is disappointing. This aspect of the project got off to a good start, with a thorough search of potential sites and on-site measurements of the water vapor. The intent of their program was good and NRAO should be praised for this aspect. However, the execution of the site studies is another matter. It is clear from the presentations of the site testing group led by Owen that there is a "hidden agenda" in their work: they want to justify putting a new telescope in their backyard, on South Baldy. They began tipping meter tests of the VLA and South Baldy sites early in the project, but it is only recently that they have even begun testing other sites. Although they claim to have seriously considered other locations, especially Southern Hemisphere sites, I don't see any evidence that these possibilities have received anything more than a brief, cursory examination.

I am especially concerned about how the final site selection will be made. If the process is a continuation of the current effort, the result will be extremely controversial. The NRAO should stimulate outside studies and assemble a panel of experts from within the community to deal with final recommendations on this topic.

One of the arguments for locating the center of operations near Socorro or Tucson is the potential cost savings of combined operations. I am not convinced of the value of this argument. I understand that the joint VLA and VLBA operation at the AOC in Socorro has not been as smooth as anticipated. Unless the sharing of manpower between projects is well defined beforehand, all the parties involved will feel that they are getting a poor deal when conflicts arise. Construction of a separate facility to handle the operations of the MMA is a far more satisfactory solution. Of the locations mentioned, Tucson is a more appealing since the current millimeter effort of NRAO is already located there, the potential for attracting technical employees is greater, and access for the user is more convenient.

Antenna Design: The conceptional design of the antenna elements has evolved slowly during recent years. The next step is to examine in detail the tradeoffs of various design details. Several technical issues which need to be addressed include:

(1) What is the real cost difference between CFRP and an all metal reflector construction.
(2) How will Solar observations be handled.
(3) How will environmental protection of the reflector surface be guaranteed.
(4) Is such a large secondary mirror necessary. This will make any chopping secondary system more difficult if it is also provided for.
(5) Is the Coude focus optimal. The large number of mirrors is disadvantageous, especially at higher frequencies. Can much of the same function be accomplished with a Nasmyth focus?

The conceptional work on the antenna design has been good up to the present. A high priority should now be placed on addressing the difficult technical issues and design details.

Configuration Design: The conceptional design of the configuration has not changed significantly over the past 8 years, namely 40 antennas usable in several different configurations. There are two important issues which were not addressed adequately in the proposal.

(1) What is the expected phase coherence size of the atmosphere at millimeter wavelengths. Is the long baseline configuration of 3 km really justified? How often can one expect to be able to successfully use such baselines? This topic has considerable impact on the other aspects of the MMA project. If a smaller maximum configuration size was found to be acceptable, then there would be several other site possibilities. The final cost of the project could be significantly impacted by the answer to these questions.

(2) How is the shadowing of close antenna elements handled in the compact (70 m) configuration? What is the impact of the shadowing on the imaging capabilities of this configuration? How difficult is to going to be to fully realize this configuration in practice?

Receivers and Electronics: The task of equipping the MMA with the specified large number of state-of-the-art millimeter wavelength receivers is a formidable one indeed. To put the task in perspective, one should realize that the number of receivers proposed at some of the wavebands probably exceeds the current total worldwide inventory of sensitive receivers at that wavelength. The receiver frontends will probably account for the greatest operational expense of the MMA because of both energy costs (for the cryogenics) and maintenance costs. The proposal identifies several key areas of technological development which are necessary for the realization of the receiver plan. NRAO should be urged to begin addressing these areas immediately.

Computers: I expect that the off-line reduction of data from the MMA will be handled adequately, just as it is for the VLA. However, the MMA proposes several operational modes which are unique for a synthesis instrument, such as preview and "single dish" modes. The software development necessary for this to succeed is not adequately addressed in the proposal.

Single Dish Modes: A major design requirement of the MMA is to provide complete imaging capabilities on all size scales, including "zero spacing" data. This topic, more than any other feature of the MMA, has generated disparate discussion within the millimeter wavelength community. The authors of the MMA proposal feel that all of the requirements for zero spacing and other single dish type observations can be handled by the array. The opposing viewpoint advocates the inclusion of a large single dish as a central element to the MMA design. Unfortunately, the NRAO has handled the discussion of this issue very poorly. There has been little interaction with the millimeter community in designing this aspect of the proposed array. The design is principally the product of the centimeter wavelength imaging group at the VLA.

The proposal envisions 4 operating modes for the MMA: (1) Interferometric imaging in various configurations. (2) Coherent phasing of all antennas for use with VLBI. (3) Single dish observations by rastering all antenna elements in synchronization. (4) Single dish observations by each antenna element on a different pointing. The controversial point is whether or not the last two modes are best done with the MMA.

I don't believe that the technical feasibility of the proposed single dish modes is in question. The various simulations which have been presented indicate that the zero spacing data can indeed be provided by the compact configuration and appropriate observing techniques. However, I don't believe that this is the most cost effective solution. The capital investment of the MMA is considerable and the user demand will be very high. It would be wasteful to use such a sophisticated instrument on single dish observations when a far simpler, less expensive telescope would do the job. In addition, there are several expensive features which must be built into the MMA to accomplish these modes. The most obvious is a wobbling secondary mirror assembly on each antenna element. The price of $4M for this item could just as well buy most of a single dish as the central element.

The success of the proposed single dish modes will depend on how well the on-line software is developed. One of the strengths of a single dish telescope is the system flexibility and the ability to do on-line, interactive observing. The demands on the MMA control software are well beyond the capabilities of any existing array. A large software development effort is necessary for this capability to be realized.

The comparison between bolometers on a single dish and continuum capabilities of the MMA are exaggerated in favor of the MMA. The sensitivities used for bolometers are existing instruments and actual measurements (often several years old). The sensitivity used for the MMA is a projected value. It would be far more useful to compare projected technology in 5 years time, or to compare the ultimate sensitivity limits under background limited conditions. Such a comparison would cast the MMA is a less favorable light. In addition, these comparisons are only for a point source detection. For low-resolution extended-structure observations, a bolometer system on a single dish will be far more sensitive.

Enabling Technologies: Several enabling technologies have been identified for the successful realization of the MMA. NRAO should be encouraged to actively pursue a development program in these areas even before funding for a MMA becomes available.

Costs and Operations: The cost of operation and staffing of the MMA has not been addressed in the proposal. How is this project going to compete against the VLA, VLBA and GBT? What are the priorities within NRAO?

Management and Politics: I am seriously concerned about NRAO's commitment to millimeter wavelength astronomy. At present, most of their facilities and operations are at centimeter wavelengths. Only the small Tucson operation is dedicated to the millimeter field. Yet when budget pressures have become critical in recent years, the Tucson operation is the first to be threatened with closure. The NRAO User's Committee has consistently expressed concern about the low level of support for the millimeter wavelength operation over recent years. NRAO needs to show a real commitment to millimeter wavelength astronomy in order to be a credible leader of this project.

I must seriously question whether or not NRAO is the right organization to lead the MMA project. The MMA is clearly an excellent concept and the astronomy community needs, and wants such a telescope. Perhaps the NSF should encourage a new consortium to form a new national millimeter wavelength observatory to promote this project, just as optical astronomy and centimeter wavelength astronomy are better served by separate organizations.

If NRAO is to continue as the lead organization in the MMA, I would recommend the following:

(1) NRAO should hire dedicated staff to work on this project full time. Individuals with experience and expertise in millimeter astronomy should be solicited. At present, this project is a part time affair for many different staff members. Few of the principals in this project have much experience with millimeter observations, and there are significant differences between millimeter and centimeter observations.

(2) NRAO should involve the outside community more fully in the technical decisions. This project once had a Technical Advisory Panel, but is has not been used for years now.

(3) Commit more of its current operational resources toward addressing the critical technological issues which are of direct relevance to the MMA.

Conclusions: The need for a MMA to address the problems of millimeter wavelength astronomy during the coming decades is well justified and well documented. The astronomical community needs and wants such an instrument. However, I am disappointed in the proposal which NRAO has put together for this project. The current proposal is only average in quality. With the proper attention to detail which this project deserves, an excellent proposal could be put together in the future.

Overall grade: Excellent

This is an outstanding proposal for a major new astronomical facility. The scientific justification and technical merits of the instrument are thoroughly and carefully described. The design is well matched to the scientific questions being addressed, which span a wide range of disciplines from active galactic nuclei to star formation and planetary science. The performance of the array is comparable to that of the VLA in sensitivity and angular resolution, but spanning a decade of frequencies above the VLA's range. I fully expect that the scientific impact of the MMA will be at least as great as the VLA, which is regarded by many astronomers as the most productive ground-based telescope ever built. In the following, I will briefly discuss several issues which have been discussed in the U.S. astronomical community with respect to a new millimeter facility.

o Array or Large Dish?

The relative merits of arrays versus large single dishes for millimeter wave astronomy have been hotly debated since the before the ill-fated 25 meter telescope proposal almost a decade ago. Historically, the advantages of a single dish design are ease of use and better sensitivity to low surface brightness emission. Both of these issues are moot with the present design, which allows several 'single dish' observing modes (Appendix B and pp.98-100), so that the astronomer can think of the phased-array instrument as equivalent to a single 25 meter dish (also done at the VLA). However the most compact array has a 50% filling factor (cf. 2% for the VLA) which allows excellent low-brightness sensitivity. In addition, mosaicing of large fields avoids the loss of low spatial frequencies caused by the primary beam. The proposal makes the advantages of an array design abundantly clear; anyone who still prefers a single dish design must rebut the detailed arguments in the proposal and show what scientific goals could not be accomplished by the MMA but could be with a single dish of comparable cost.

o University based or NRAO based?

The development of millimeter-wave astronomy in the U.S. has been almost entirely University based, with the arrays at Caltech and Berkeley, the front-end lab at the University of Virginia and the Caltech submillimeter telescope being good examples. However, both the scope of the project and the degree to which its success depends on sophisticated interferometry hardware and software dictate that the only institution with adequate resources is N.R.A.O. The NRAO has probably the world's best experts in many of the requisite fields, including receiver design (Weinreb's frontend group), correlator hardware (building on VLA and VLBA experience) and imaging software (AIPS group, especially Cornwell). I fully expect the NRAO will continue to tap the experienced groups at Caltech, BIMA, and elsewhere in the University community help with detailed design.

o Does the U.S. really need another large radio telescope project?

A frequently heard question regarding the MMA is whether the U.S. can afford to build an expensive new millimeter instrument given the large number of recently-built, mostly non-U.S. millimeter and sub-millimeter instruments such as Nobeyama, IRAM, and CSO, and SEST. I sympathize with the general concept that the U.S. cannot lead the world in every sub-discipline of astronomy. However, this proposal provides strong evidence that the increased speed and mapping capabilities of the MMA can significantly advance our understanding to many of the relevant scientific issues. The situation reminds me of the pre-VLA era when the Westerbork and Cambridge arrays were producing excellent first maps of extragalactic radio sources. The dramatic improvement in sensitivity and dynamic range of the VLA not only improved extragalactic radio astronomy, but opened entirely new disciplines such as stellar radio studies and maser and extended spectral line maps of star formation regions.

o Site Selection

Chapter VI addresses three suitable southwestern U.S. sites for the MMA. The best documented is the South Baldy site near the VLA, which appears to have (barely) adequate room for the longest baselines and reasonalby good opacity (excluding summer). The site has the further advantage that it is only about 1 hour drive from the AOC in Socorro and has a good dirt access road. Although a site on Mauna Kea or perhaps the Andes would certainly be better in terms of water vapor and sky coverage, the authors wisely reject these alternatives as being too costly to build and operate. The u-v coverage of the proposed array geometries at the three sites (p.116-117) looks impressive and should allow both high dynaminic range full synthesis maps as well as good quality 'snap-shot' mode observing over a wide range of declinations.

As I faced the task of reviewing a project as large and as complex as the MMA, I decided to concentrate on certain sections of the proposal and on certain of the broad questions I was asked to consider. For instance, I am not able to say much about cost estimates and assumptions about the schedule (and little about Chap. VIII, §4). Equally, my involvement in some of the early discussions about the MMA and its scientific missions makes it inappropriate to say much about the scientific case and some aspects of the management approach to date.

Let me begin, however, with some general remarks about the science planned for the MMA. The millimeter wave windows provide astronomers the opportunity to study two general classes of emission not readily accessible at other wavelengths: molecular lines and the thermal emission from warm dusty objects, especially those at high redshift. In the latter connection, fig. III.1 of the proposal is particularly revealing. Study of molecular lines is crucial to understanding the physics and chemistry of low temperature gas throughout the cosmos. Thermal dust emission is intimately connected with the birth of stars and galaxies. For these reasons and others, I expect millimeter wave interferometry to yield at least as rich a scientific harvest as we are reaping from the VLA. Thus I consider the scientific case for millimeter wave astronomy very strong.

A more pointed question is the strength of the scientific case <u>for the MMA</u>; is its combination of resolution, light-gathering power, and analysis software crucial to these scientific goals?

The MMA offers a wider range of angular resolution--a big advance beyond BIMA and OVRO. The advantage of higher resolution (to $0\overset{''}{.}1$ at 1 mm) is obvious. Equally important, I feel, is the sensitivity to larger angular scales than are readily measured at BIMA or OVRO--mapping extended objects at high dynamic range is becoming more and more important. It offers far greater sensitivity, especially at 1.3 and 0.9 mm--crucial for both molecular line work and studies of dust emission. The ability to make broad band continuum observations is a plus; indeed some of the extragalactic programs suggested (like the searches for primordial galaxies on the Sunyaev-Zel'dovich effect require the order of magnitude increase in sensitivity the MMA will provide.

Let me now turn to some detailed questions. The first set of questions revolves around design choices for the individual antennas and for the array.

Antenna size. The 8 m diameter is presented as a compromise. Smaller dishes would be less expensive, easier to make to required tolerance and would have larger primary beams. On the other hand, with smaller dishes, the geometrical blocking factor by the secondary grows proportionally larger and the number of dishes (and hence of receivers and correlators) goes up.* The cost trade-off seems to me to favor smaller dishes, but 8 m is not an unreasonable figure IF the desired surface figure and pointing accuracy can be achieved. Both seem to me to be crucial questions which deserve careful examination before the full-scale array is funded. Given the novelty of CFRP technology in the U.S., and the need for simplicity and mobility, can 8 m dishes with 25μ rms surface accuracy be made? Can that accuracy be maintained in the open without active thermal regulation? Even more important is pointing accuracy. The remarks about the cumbersome method used at IRAM were not encouraging (but of course 8 m dishes may be easier to point than the much more massive IRAM dish). Under this same heading, I worry about shadowing in the 70 m "D" configuration, especially for study of the Galactic center. I recognize that atmospheric emission will favor observations at high elevations, limiting the shadowing problem. But the Galactic center is at elevations $\lesssim 30°$ for any of the favored sites. An elongated N-S baseline for the densest configuration (70 m) would help.

Beam switching. For "single dish" mode operations, beam switching is the key. The optical and mechanical problems arising from beam switching need to be looked at in detail (e.g., limitations and asymmetry in the optical path, especially at 9 mm; vibration; pointing accuracy of the two beam positions; etc).

Site selection. To me, the ability to exploit the 1.3 mm and the 0.9 mm windows are a key new feature of the MMA. Therefore, the highest reasonable site is to be preferred, even at the cost of some complexity in the 3 Km "A" configuration. I note that the initial assumption was that the A configuration of the VLA would be heavily subscribed; in fact, lower resolution measurements dominate now.

This last point raises another question. Could the MMA be built without an "A" (3 Km) configuration at first? Obviously, one would want to select a site with a potential for expansion to 3 Km.

*A problem not only in dollar terms, but in terms of time needed to re-configure the array. The latter may already be substantial for 40 dishes, and needs a little study.

Finally, I think NRAO is right to exclude sites north of ~ 36°—many interesting regions of the Galactic plane are at $\delta \lesssim -20°$ (or elevations $\lesssim 35°$) for a site at 36° N.

Computing. The opening paragraphs of Chap. V are only too true. The VLA software is becoming less rather than more user friendly and transparent. Astronomers, especially those not used to interferometry, will need much simpler software and much greater "protection" from the complexities of the data analysis. It is important that the software development keep up with the hardware and that it be done with the average, not the expert, user in mind.

Since the MMA aims at both scheduling flexibility to make use of excellent weather, and rapid low resolution survey mapping, essentially real time imaging of some sort should be a priority aim (impossible at the VLA).

I'm heartened to see the careful tests of mosaicking ideas in the proposal and in other MMA reprints; these studies need to be continued and checked by actual observations where possible.

Operations. As most radio astronomers will tell you, NRAO is starved for funds, and will be in even worse shape when the VLBA is fully operational. Funding for the MMA must not be allowed to cut into funding for the Green Bank telescope, the VLBA, or the VLA. Without an adequate, new, infusion of funds and staff, the present programs at NRAO may grind to a halt.

Frankly, Chap. IX (only 2 pages long!) does not come close to addressing the problems I see in operating a project as complex as the MMA. At some point, a much more detailed management and operations plan will be needed. In particular, the resources devoted to software development and maintenance look awfully thin to me.

Risk. To summarize, my major technical concerns are pointing accuracy and the demands of beam switching. The major financial risks seem to me to lie in the areas of staffing and operations. There is no point building the MMA unless it is properly staffed and effectively managed.

Effect on BIMA and OVRO. Valuable as these two university programs are and have been, we obviously can't fund three millimeter arrays for the next decade. If the MMA is supported, some plan must be devised for phasing out NSF support for both BIMA and OVRO. In this connection, you may find it useful to see a review of BIMA and OVRO proposals I prepared for Ken Turner 6 months ago (copy attached).

The concerns I have expressed and the questions I have raised should be read against a background of strong support for millimeter wave interferometry. The MMA (unlike a single large mm dish or even the VLBA) would present U.S. astronomy with a chance to make a notable technological breakthrough with many expected (and surely many unexpected) scientific benefits. As an NRAO facility, it would have the further virtue of being open to <u>all</u> astronomers in the community, as university facilities really are not. But we must not allow the MMA to bleed dry present NRAO programs and facilities.

L

General Comments:

The Millimeter Array will be a fabulous addition to our nation's astronomical capabilities. Like all great astronomical observatories, I expect that the MMA will make significant contributions to virtually all areas of astronomy and that it will have an important impact on the development of astrophysics as a whole. I strongly support the NRAO's efforts to build this instrument and their leadership in getting the job done so far. The scientific case and national need for the MMA is very strong, and I encourage the NSF to fund the project.

Specific Comments:

1. Scientific Case

The MMA proposal does a good job at laying out the broad scientific goals of the project. It should, since the NRAO took great care to consult with astronomers with a wide range of interests before it wrote the proposal. NRAO should be commended for this, and I think that they did a fine job of putting it all together. What is more difficult to assess is the question of national priorities. Is the scientific case for this millimeter-wave instrument stronger than that for other instruments that might be imagined? A special strength of the MMA, is that it touches a wide range of areas and that it will be recognized as an important instrument by many communities. In some areas, including my own, it is clear that some other instrument, like a big ir-optimized telescope for example, might be preferred over the MMA. However, this fact does not change the ability of the MMA to do exciting and unique studies that will complement what is done at other wavelengths. Thus, the main question to answer is whether this is the best millimeter-wave instrument for our national needs. I argue that the ability of the MMA to conduct a wide range of investigations, as documented in the proposal, make it precisely the right millimeter-wave instrument to build and the scale of the project makes it an appropriate project for the NRAO to undertake.

2. Design

It would be impossible to design a telescope that does everything well, even in a limited part of the electromagnetic spectrum. The MMA does a good job of covering a wide range of observing options without compromising its primary purpose: high resolution imaging. If anything, I would argue in favor of limiting the range of capabilities of the MMA to emphasize high resolution imaging and not worry about such things as making the MMA look like just another big antenna. The NRAO should take care not to degrade this capability when budget compromises need to be made.

3. Management Approach

The NRAO does a great job at running these projects and works hard to keep the community involved. One aspect of their approach that does deserve comment, though, is in the area of software development. The design of the MMA will make the instrument extremely versatile. At the same time, the MMA will be correspondingly complicated to use. This will make it essential that the observers get a lot of support from on-site personnel and from the data collection and reduction software. In my opinion, the level of software support is inadequate at all NRAO sites, NOT because of lack of hard work but rather because the amount of work that needs to be done is not fully appreciated by those allocating resources. The MMA project would do well to learn from the past rather than simply reproduce it, as stated in the budget explanation. I think, for example, that it would not be unreasonable to have comparable numbers of software and hardware engineers working on the project, rather than the 3/1 ratio in the proposal.

4. Cost

If the cost and schedule were realistic, then it would be the first time I've ever seen that happen in an astronomical instrumentation project. In general, I think that projects like this one are pretty optimistic about how little it will cost to build and operate the instrument. In the Green Bank Telescope project, for example $75 million sounded like a lot until the NRAO actually had to build it. Now $120 million sounds like a lot, but the MMA is bound to either cost more, or more likely, take longer. There are many aspects of the hardware that need "development" to achieve the MMA goals, and although recent efforts have produced results in a number of areas, I'm unconvinced that this will proceed rapidly. Finally, I am particularly worried about operations, budgeted at 6% of the construction costs. As I mentioned above, I believe that the operation of such a complex system will either require a larger support and software budget or diminished expectations from the user community.

Millimeter-wave Array Proposal

The millimeter-wave array (MWA) has been in the talking, thinking, planning and proposing stages for the better part of a decade. This review of the Millimeter-wave Array proposal (AST-9024403) is, like the proposal itself, by necessity, incomplete. Opinions in four specific areas are requested: (1) the scientific case, (2) the design concept, (3) the management approach, (4) the cost estimates and "risk". I see several sub-issues to these areas which I will either discuss or pose questions regarding. I, unfortunately, do not have much wisdom regarding management, cost and risk other than a general opinion about NRAO.

1. The Scientific Case

The scientific case for the MWA stands or falls on the importance of and its potential impact on three areas of astrophysics: the structure and evolution of (nearby) galaxies, the microstructure of molecular clouds, and circumstellar disks. Other topics, listed in gory detail in the proposal are, in my opinion, sideshows. These are the areas, plus galactic structure, which have primarily occupied millimeter-wave astronomy for 20 years. The need for a MWA was motivated by these problems and its design is "tuned" to attacking them. Are these areas important enough to justify the MWA? If we were to attempt to independently list the most important <u>current</u> problems in astrophysics, I suspect that the list would include topics like the evolution of the very early universe, galaxy formation, large scale structures and ordering of galaxies, missing mass (?), planets and planetary formation, end points of stellar evolution, details of nuclear processes, etc. In other words, the overlap with the MWA's strongest scientific motivations is good but not perfect. The MWA will clearly have enormous impact on circumstellar processes and, thus, our understanding of planetary formation through both molecular line and continuum measurements. It's impact on early stellar evolution will also be huge. How important the morphology of galaxies is to understanding their formation and evolution is, surprisingly, an open question. Similarly, studying the microscopic structure of molecular clouds is connected with galactic evolution (and, obviously with star formation) but in an indirect manner. The success of the MWA in attacking these problems, which, I think require a statistical approach, will depend upon its speed and the way it is operated.

Question: Will the MWA have an impact on problems of large scale structure in astrophysics?

Question: Will the MWA's speed, observing complexity and management approach be suitable for carrying out statistical experiments?

Many of the discussions about MWA capabilities and scientific impact revolve around its unique features. Its most unique capability will be the ability to make high resolution and sensitivity continuum observations. In general, the higher the frequency of continuum operation, the better although being able to cover roughly 90-350GHz is also important. Continuum measurements allow dust to be measured and thus a relatively new (for radio astronomers) component of the universe to be observed. The fact that this is the main thing that far-infrared astronomy measures adds a degree of synergy to continuum studies.

2. Design Concept

The MWA design concept was driven by the history of millimeter-wave astronomy. It seeks to find a compromise between high resolution and sensitivity on the one hand, and the ability to map large fields on the other. There has been, for example, a great deal of discussion regarding the role of or need for large single elements to aid in mosaicing images together. Clearly, there is no need for such an element for two reasons. First (and most important) it has been conclusively shown that large fields can be mapped with high fidelity using arrays of small antennas. Second, at least in my opinion, problems requiring large fields of view are not the major task of the MWA. Having a large field mapping capability is a nice feature but not a vital one.

Sensitivity, on the other hand, is of paramount concern. The MWA's design has been driven by estimates of the expected continuum and spectral line intensity of typical objects. For example, its continuum sensitivity of ≈1mJy/min is about right for detecting stellar chromospheres.

Site selection is an intimate part of the overall design concept since it can set both the resolution and sensitivity limit. Unfortunately, site selection appears to be the most opaque part of both the proposal and actual activities to date. In almost all of the MWA proposal preliminaries, it was emphasized that the higher the "typical" operating frequency, the better.

Question: What is the most important criterion for a MWA site? Atmospheric transparency? Suitability for a ≈3km array? Proximity to other VLA sites?

3. Management

The management question is simply put: Is NRAO the best institution to design, build and operate the MWA? Alternate possibilities might be for a university consortium to do the job or for to expand one of the existing millimeter-wave interferometers. The scope of the MWA project is clearly appropriate to the national observatory and its experience with the VLA should be an appropriate model for construction and operations. On the other hand, it is clear that NRAO has not been in the forefront of millimeter-wave interferometry. Also, its commitment to millimeter-wave astronomy appears to occasionally waiver. With some notable exceptions, its scientific and technical staff is not in the forefront of this field of astronomy. These issues are, to me, secondary to the appropriateness of NRAO's role. The MWA proposal itself is a demonstration of the NRAO's ability to synthesize complex scientific requirements and technical limitations. The proposed array is the best that the community not just NRAO can come up with.

Operation of the MWA in a manner which maximizes its scientific productivity will be an interesting challenge. NRAO has shown, in the past, the ability to develop strategies for running observatories which get the most from the investment. Unfortunately, NRAO also has a tendency to initially underestimate operations costs.

Estimation of the computer resources required for the MWA is likely to be a problem. If past experience is a guide, NRAO tends to both underestimate the computational resources needed for observatory operations and routine data reduction and to overestimate the rate of improvement of computer performance/cost. Also, astronomy seems to consume more and more computer resources as the individual astronomer is farther and farther from the telescope. Unfortunately, the section of the proposal devoted to computing is sketchy.

Construction of the MWA is probably the point where the NRAO is weakest. It is not clear that it always gets the most for its investment in construction of instruments. Sometimes, as is probably emerging as the case with the VLBA, the "design by the community with NRAO providing the details" approach can lead to foolish results.

4. Cost and Risks

It is impossible for the layman to evaluate the MWA cost estimates. One area stands out: computing. The computation cost estimate is probably too low. Also, as the proposal admits, the operating cost estimate is unrealistic with a very small allocation for equipment. Obviously the cost of running the MWA is based upon little system evolution and a finite lifetime (when the MWA wears out it will be closed down). Both assumptions are typical of proposals.

Question: What will be the real yearly costs of upgrading and properly maintaining the MWA for a projected 50 year lifetime?

Conclusions

Overall, my evaluation of the MWA proposal can be summed up as follows.

The scientific justifications are based upon 20 years experience with millimeter-wave radio astronomy. The really key justifications overlap with many of the most important current problems in astrophysics.

The MWA concept is driven by sensitivity and resolution considerations from the most important scientific justifications. Some consideration has also been given to observing large fields of view.

The concept does not require a large single element to accomplish the most important science. Such an element probably isn't required for any of the MWA's possible scientific jobs.

Site selection is still not clear. I'm afraid that non-scientific considerations may be given too much weight.

NRAO is the most appropriate institution to design, build and operate the MWA. NRAO will probably do the best job although, historically, it probably will underestimate the computing requirements for operations and data reduction.

Estimates of the cost of the MWA are difficult to gauge but I suspect that both the computing and operations estimates are too low.

N

This proposal for a National Millimeter Wave Aperture Synthesis Array presents a very strong scientific case for such an instrument within the next decade. Based on the science presented in this proposal, the instrument would be clearly as powerful and more widely applicable than the VLA for studying a broad range of astrophysical phenomena--ranging from planetary atmospheres to detailed studies of nearby star-forming regions, galactic structure in nearby galaxies and distant protogalaxies.

Although the scientific imperative for the instrument is clear, many design issues, technical trade-offs, and managerial questions are inadequately treated in the present proposal. In this sense the proposal must be viewed as a conceptual document rather than a proposal for actual funding. Below I discuss some of the issues which need to be addressed.

1. Operational bands: As proposed the instrument would operate between λ=7mm and 0.8mm with virtually continuous frequency coverage. The four principle observing bands are at 7, 3, 1.2 and 0.8mm. The scientific case and rational for the central 2 bands is clear since they contain the first two CO lines and are technically easily achievable. Although the scientific case for the long wavelength band is not clearly stated in the proposal its cost is relatively insignificant. On the other hand, the justification for the short wavelength limit (λ=0.8mm) is not adequately justified. This is a particularly serious problem in as much as the short wavelength limit will drive the cost of the instrument--determining the required antenna surface accuracy, pointing, and the site. It is my belief (without seeing evidence to the contrary) that the instrument should be limited to wavelengths λ>1mm and the higher frequency bands left to the submillimeter array planned by the Smithsonian Astrophysical Observatory on Mauna Kea. Such a division of responsibility between the two instruments might reduce the overall cost of the NRAO array and provide a firmer focus for the SAO array.

2. Sites: Three sites (all relatively near to the VLA) were considered in the present proposal. It is unclear why such a limited geography has been surveyed in view of the fact that the technical staff for the millimeter array would be largely independent of that for the VLA and the data from the millimeter array will be transmitted easily on wide band data links within the continental United States. All three sites considered in the proposal have serious problems--in particular the Mount Baldy site is known to have particularly severe thunderstorm activity; in addition the geometry of the site is awkward (to say the least) for large telescope configurations. I believe that if the larger radius were surveyed, a better site (at higher altitude and with a larger area of level terrain) could be found.

3. Seeing: Recent studies by Masson on Mauna Kea (a site considerably better than those considered here) have indicated a maximum angular resolution of 0.5-1 arcsecond under all but extraordinary conditions. If these preliminary studies are correct, it is then unclear how one can justify much larger configurations of the MMA with baselines of several kilometers. This clearly needs study before the instrument can be finalized or a site selected. The proposal must not overstate that the angular resolution which could ultimately be achieved.

4. Telescopes: The basic technical specification determining the overall sensitivity for the array is the collecting area. No real justification is given in the proposal for this particular parameter ($2000m^2$) other than reference to an outdated report (the Barrett committee) written approximately a decade ago before any millimeter wave interferometry had been done. This is a serious problem with the present proposal in as much as the total collecting area determines the desired number of telescopes and the size of each.

   Even given the desired $2000m^2$ collecting area, no justification is provided for the telescope size and number which are proposed. Such a choice should be predicated on the basis of cost effectiveness (total construction costs plus operations costs) or on the basis of imaging capability. It is thus unclear whether the same sensitivity could be achieved more economically with a smaller number of large telescopes or a larger number of small telescopes. I suspect that the former is the case.

   Another basic issue with respect to the telescopes is the quoted design specifications (a surface accuracy of $\lambda/40$ and a pointing accuracy of 1 arcsecond). These specifications are real cost drivers. The quoted specs are a factor of 2 tighter than would normally be justified (by conventional thinking). Reducing them by a factor of two might well lower the telescope costs by 25%. This is also an area where the stated intention for routine observations $\alpha\tau$ λ=0.8mm will drive the overall costs.

5. Budgets: The budget presented in this proposal is entirely conceptual with no detailed justification. Given the lack of detail in the proposed budget I suspect that a considerable margin for error has been inserted. In the present financial climate this is not acceptable.

   The funding schedule outlined in Table X-2 implies that the total costs, correcting for 5% inflation, will be $150M. This budget seems high given the costs of the 6m BIMA and 10m OVRO telescopes (approximately $1M and $2M respectively including electronics). (To provide the same collecting area as the MMA, would require $71 and $51M respectively on the basis of those cost estimates).

6. Management: The NRAO is nearing completion of the VLBA project and is just starting the GBT. Both are $70 - $100M projects which will take many years to be in full, routine operation. How the NRAO can take on another even bigger project simultaneously with these (in addition to continued operation of the VLA) is a serious question. (Sandy Weinreb was a major player in past NRAO instruments but is no longer at the NRAO; he was a unique engineering talent capable of a <u>complete</u> understanding of all aspects of the system. Is there such a person now at NRAO?)

Many of the technical and managerial issues noted above might be addressed were the NRAO to propose this instrument in collaboration with the existing universities involved in millimeter wave research. The primary university groups here are BIMA, OVRO, and FCRAO, all of which have considerable expertise in millimeter wave and interferometric instrument development and have expressed some willingness to participate in the MMA project with the NRAO. Such an approach was suggested at the recent MMA workshop in Tucson and met with considerable enthusiasm within the community. Such collaborations, between a national facility and university research groups has worked extremely well in high energy physics. In the case of the MMA this might be particularly beneficial since the university arrays could form the basis for the MMA in its early years. It would also resolve the issue of the future of these university groups in the era of the National MMA.

Rating: EXCELLENT

The MMA proposal receives my highest rating. NRAO should be commended for a superb proposal. In terms of scientific impact the MMA is in a class with any proposed telescope in any field. It will vastly exceed the scientific diversity and impact of the VLBA, and will dwarf even the VLA in importance to astronomy. Much like the VLA opened new parts of the universe to radio study, so too will the MMA to at least as great an extent. Much of the proposed science is fundamental and unique -- no other instrument will be able to do much of the science.

I think the current work with millimeter interferometers offers only hints of what will be possible with the MMA. To date mm interferometry has had limited sensitivity, dynamic range, and short-spacing coverage; although the expansions of OVRO and BIMA will lead to order of magnitude improvements in capabilities, the MMA will go well beyond the university telescopes.

I was essentially neutral on the question of a heterogeneous vs a homogeneous array, having heard primarily from those who favored the large single dish. But NRAO has convinced me that a homogeneous is the way to go. I am confident that the homogeneous array will offer superior image quality.
Some people are vehemently opposed to a homogeneous array, largely I think because they simply won't get nearly as much time to do their line detection experiments on a homogeneous system. There is so much fantastic science to be done with the MMA that typical projects will only receive a few hours of time, not enough for most detection experiments, not the weeks of time chemists have become accustomed to getting. It would indeed be preferable to use an experimental receiver, such as a focal plane array,
on one 20 m dish than on 40 8 m dishes; however, as discussed below

I think even the current NRAO plan puts too much of the future of US radio astronomy in the hands of NRAO. I think experimental work should take place in the universities, not at NRAO.

I do have reservations about making this entirely an NRAO effort.

The great technical innovations in radio astronomy in the US have been fueled to a large extent by physics and engineering students who have been attracted into the university radio astronomy laboratories. In all its years of existence, to my knowledge NRAO has not produced these students.

NRAO's position is unique within the US astronomy community.
In no field other than radio astronomy does a national organization even remotely approach NRAO's likely dominance of the technical aspects of the field. This is a VERY dangerous scenario that will inevitably threaten the health of US radio astronomy in the long run. Some key parts of the construction MUST be farmed out to the university laboratories.
The opportunity to use a non-standard receiver in the 'spare' instrument bays will not suffice in this regard.
I point out that NRAO has never developed new technology on a large scale. The VLA is essentially a scaled-up Westerbork, with hardly anything in the way of new technology; similar remarks can be made about the VLBA. The MMA, on the other hand, involves development of technologies such as the carbon fiber antennas which are still in the development stage. I think the Hubble fiasco has taught us that diversity is essential to the health of big projects like this. We must not risk the health of our university laboratories.

Also, I think the management of the construction of the MMA and decisions about its design should be in the hands of a group that includes substantial reprsentation outside of NRAO. Although I agree with NRAO's conclusions regarding the homogenous array, I am somewhat disturbed by the manner in which the decision was made.

Despite these reservations, let me state categorically that the proposed MMA is a superb instrument and will have a major impact on many of the most fundamental problems in astronomy.
^Z

This project should be supported. The science is of high importance for the entire astronomical community. The NRAO has shown that it can carry out large projects with the construction of the VLA and the VLBA. I cannot judge whether the cost estimates are realistic. Before full scale construction starts, a solution for the antenna design, the SIS front ends, the LO system and the user-friendly software must be found. This might take 2 years, but this would allow the MMA to be built in a more efficient and perhaps improved manner. Assuming that these conditions are accepted, I am happy to give the MMA my highest rating in all categories. However, the project is NOT ready to go as of 1991.

I am impressed with the general descriptions of the scientific goals of the MMA, although I think that some of the claims, particularly in regard to finding protostars, are overstated. I am more impressed with the description of the science which can be done using broadband mapping. This instrument will accomplish more than the BIMA or OVRO facilities, and will allow much first rate work. I believe that the astronomy community needs this instrument, and I am of the opinion that only the NRAO can make this very complex concept work. This project is much more tricky than the VLA because of the greater influence of weather, and the greater accuracy needed in regard to antenna pointing and surface accuracy. These considerations lead to a number of misgivings about the project as it is described. First, although I understand the need for facilities close to the VLA site, and the aversion to Mona Kea, because of the costs of building on that site. However, the conclusions about the in Chapter VI ignore the summer months. What will be done then, when the phases vary much more than in the winter, when the testing was done? Some thought is needed here, or at least the comment that nothing can be done. Second, the antenna design is unclear. I would strongly urge building a prototype antenna to see how this behaves in the typical weather expected at the sites selected and to see if moving the antenna causes any damage to CFRP materials. The LO problems need to be solved at least in principle. I do not see a solution in the very near future, and I would hesitate starting a project without at least ONE solution in hand. The same goes for the SIS development. Third, I would urge the NRAO to develop the software to make observing simple NOW, rather than waiting until the MMA is funded. I am suspicious of the statement that the MMA software will make this instrument as easy to use as a single dish radio telescope. There are single radio telescopes with lousy software. Also, the software could be tested to improve the accesability of the VLA or VLBA to non-specialists in the meantime.

My overall assessment is that this is an excellent proposal and should be funded. Even so, I believe there are a number of important concerns raised by the proposal, some related directly to the construction and operation of the array, and some related to the direction of astronomical research in the US.

The scientific case for the MMA has been considered now by a wide range of astronomers whose ideas are embodied in chapter III. The MMA will provide an excellent complement to the VLA and to IR and optical imaging telescopes for studies of a very wide range of problems. Imaging of thermal dust continuum and of a host of molecular transitions is clearly an area in which the MMA will excel, and so will fill a large gap in observational parameter space. I expect that the proposed instrument will rival the VLA in scientific throughput, possibly exceeding the VLA's rate of 200 published papers a year. This instrument will place US mm-wavelength observational facilities once again in the forefront of astronomy.

The design presented seems to be well-suited to meet the scientific criteria laid out. The question of whether a large single element is needed or desirable has been convincingly answered in the negative. As proposed the homogenous array can meet all the observational needs discussed in the scientific program, and do so better than a large single-dish telescope.

The NRAO has demonstrated a high degree of competence in designing, building, and operating large synthesis arrays, and there is no question that this project is within their capability. However, I am concerned specifically about a lack of experience in mm-wavelength interferometry among the NRAO staff. As far as I am aware, only 3 NRAO staff members have ever used the existing mm interferometers. The practical problems of mm-wavelength interferometry are NOT identical with those at longer wavelengths. The scope of the proposed instrument is so large that it would seem prudent for NRAO to (1) increase its in-house expertise and (2) do a better job than in the past at interacting with the BIMA and OVRO groups on a wide range of scientific applications of mm interferometry, before getting too deeply into design details and specifications. (I comment below on the proposed Joint Development Group.)

The management approach is rather ill-defined at present. This lack of definition is probably not surprising or alarming at this stage, since the selection of a site will obviously affect the project management in a substantial way. I see some cause for concern in the possibility that the site selection is being unduly influenced by the proximity of the Magdelena site to Socorro. (I find it remarkable that of all 50-odd sites considered in the Southwest, the one toward which at least some NRAO staff seem to be leaning most strongly just happens to be the one nearest the VLA!) At this point, I believe it would be a mistake to focus on one site too early, before fully comparable data on the site quality are available at least for the top 3 under consideration. In this connection, the quality of atmospheric phase noise is almost certainly of more importance than the statistics of zenith opacity. It is imperative that as long a time baseline as possible, of at least the ΔT(rms), be obtained for doing a fair statistical comparison between prospective sites. Initial investigations on legal and environmental questions could proceed in parallel for all 3 sites while such data are being accumulated.

The Joint Development Group recently announced seems a good idea on the face of it. There are several obvious areas where NRAO can benefit from the efforts and experience of the existing university-based mm-astronomy groups, especially OVRO and BIMA. The budgetary and manpower constraints under which the MMA would be built mean the NRAO must avoid the "not invented here" syndrome. This comment applies especially to software, a subject notably absent from the written proposal. Other technical areas would also be served by outside input.

However, it was unclear to me how the JDG will function in practice as a vehicle for interchange between NRAO and the university groups. What is the quid pro quo for them? With both BIMA and OVRO now undertaking ambitious expansion programs of their own, what incentive do they have to divert their own very limited resources to assist NRAO in building an instrument which will almost certainly lead to their own demise as competitive observatories? This strikes me as a delicate issue which has so far not been seriously addressed (at least that I am aware of).

The cost estimate and schedule for construction seem to be reasonable, as well as I can judge. The operating budget, however, seems to be seriously underestimated. The item for equipment maintenance and repair should probably be increased by at least $2 million, or the MMA will be in the same situation as the VLA is now, with large accumulated needs for repairs. This issue should be faced squarely at the outset in the proposed budget, rather than trying to sell the project too cheaply.

At the same time, a realistic number for operating costs implies a significantly larger long-term commitment by the NSF to provide continuing support. A serious question is whether such a commitment will have a large negative impact on support for university-based research (the area identified in the Bahcall Committee report as the single most critical need for the coming decade). This question of the wider impact of MMA funding at the expense of other astronomy programs must be weighed carefully by the NSF.

As for risk items in the proposal, I see two areas of concern. First, the selection of the best site requires, I would argue, more thorough testing of the crucial atmospheric phase stability issue. All of the three favored sites could use a longer time base for statistical evaluation before one is selected. Second, a continuing source of SIS junctions is clearly essential to the success of the project. Industrial sources seem unlikely, and the UVa laboratory appears to be hanging by a thread. Steps to assure a reliable supply of junctions should be taken as early as possible. Again, this is an area where NSF support might make a significant difference.

The MMA will be the most powerful and exciting millimeter wave astronomy project ever seriously considered for construction in the US or anywhere else. Its potential scientific returns are enormous. Millimeter wave astronomy is extremely rich in molecular and atomic spectral lines that can probe a wide range of objects and physical conditions in our galaxy and other galaxies. Nor is this region of the electromagnetic spectrum of importance only to spectroscopy, the continuous emission at millimeter waves can arise from several processes of high importance in astronomy: solar system objects such as the Moon, the planets and their satellites, and asteroids emit strongly at millimeterwaves by thermal blackbody radiation; radio free-free emission is produced by ionized plasmas including HII regions, planetary nebulae, stellar winds of massive stars, etc; nonthermal emission via synchrotron and gyrosynchrotron processes are known to be produced in a wide range of objects such as quasars, galactic nuclei, galactic halos, stellar winds, etc.; the thermal cosmic background radiation peaks at millimeter waves; and interstellar dust associated with many types of objects emits strong thermal blackbody radiation at millimeter and submillimeter wavelenghts. There will be no lack of interesting and important objects that this facility will be able to observe in greater depth than any radio telescope yet conceived. Perhaps of even greater importance, is this facility's potential to revitalize US millimeterwave radio astronomy by attracting many of our most promising young scientists into this field. I strongly believe that the MMA will do for millimeterwave astronomy, what the VLA has done for microwave astronomy. One can easily foresee several important areas where the MMA is almost certain to make major contributions, but of course, the most important new discoveries cannot be predicted nor imagined from our present point in time.

As a national facility, the MMA will be used by a large fraction of the entire astronomical community to attack a wide range of problems, most of which we cannot today predict. This is an instrument that will not be completed and available to astronomers before the year 2000, at best. It is therefore of paramount importance that this project be adequately funded to permit it to achieve its sensitivity and resolution design goals. The design goals are driven by sensible scientific goals. I am, therefore, convinced that the number and size of the antennae for the MMA are correct and necessary to achieve the stated scientific goals. The combination of 40 antennae, each with 8m diameter, also represents the maximum collecting area that we can buy within the budget, presuming that the cost equation used is approximately accurate.

Establishment of the Joint Development Group is a very positive step. Although it is not yet clear how this will function in practice, it at least establishes a mechanism that will permit experts in the community to contribute to the project in concrete ways. NSF should insist that this be spelled out more fully and that it be more than a façade. There is real concern in the community that NRAO should have more millimeter astronomers on its staff. In particular, there is concern about "in house" expertise on the NRAO staff and the issue of who gives scientific advice on the MMA project.

There are several issues that give me concern about this project, the solutions of which, will ultimately determine my final evaluation of the proposal. Since there is a reasonably long lead time before the MMA project can begin construction, there should be ample time to resolve these issues before irreversible decisions are made. Let me discuss these issues in order.

1) **The 70m or the Stretch 70m Configuration**: There seems to be a serious problem here with shadowing. We were told that this had been carefully considered and that there is no serious problem with shadowing in the most compact array. I checked in the original science document and found that, yes, there was considerable discussion about this and several configurations had been considered to avoid shadowing. It seems that in the end all the early ideas were thrown out and the shadowing problem has simply been ignored. I am now convinced more than ever that this is far more serious than it might at first appear. For galactic astronomy, most radio sources lie within about 40° of

the galactic center which means that they are for the most part at negative declinations. In the most compact configuration as it is presently conceived, it will be impossible to integrate on low declination sources for more than a few minutes before shadowing becomes a problem. We should not invest 120 million dollars in an instrument that cannot integrate for more than a few minutes on low declination sources (which includes most of the interesting galactic sources) when it is in the compact configuration. **NRAO must demonstrate an acceptable solution to this problem.**

2) **The 3 km Configuration:** Here the issue is the maximum coherence time at the shorter wavelengths. In order to self-cal on a source, it is necessary to phase lock on it long enough to achieve good signal-to-noise. Thus, the maximum coherence time will determine the minimum flux that a source must have to be observable in the largest configuration at the shorter wavelength range of the array. One does not want to build a facility for which there are only 5 or 6 sources in the entire sky that it can observe in the largest configuration. **NRAO must determine the average coherence times at 1mm on the potential sites and estimate the flux limits that this will impose on observations in this configuration.**

3) **On-Line Data Display and Reduction Software:** NRAO has advertised the MMA as a facility that can be operated essentially in the same mode as a single dish telescope. It is essential that NRAO demonstrate its ability to produce a user-friendly, on-line control package that will permit users to display data in various forms and to make on-line decisions. I suggest that such a system be implemented on the VLA and the VLBA before construction of the MMA begins. It is equally important that an off-line reduction software package be developed that will permit quick, efficient reduction of spectral line synthesis data. Such a system should be portable to universities that use moderate sized work stations. The VLA has never reached its full hardware potential because of limitations on computing power, a major part of which is due to software that was never designed to deal with line synthesis data. **The same problem must not happen with the MMA.**

4) **Site Selection:** It is curious that of all the mountains in the southwest above 9000 ft, the only three that meet NRAO criteria are within a half-day's drive of Socorro. It appears that an additional criterium is that the site should be close to Socorro. It is important that the sites be intensely tested for meteorological conditions (wind, snow, rain, cloud cover, precipital water vapor, etc), environmental impact, topology suitable for the array, access, and coherence at 1mm over baselines of 3 km. **Although some testing has been done, it appears that much more needs to be done to make an informed choice between the sites.**

5) **Receivers:** The MMA requires 320, broadband, cooled, SIS receivers to be routinely operating on the system. So far, no receivers with the envisioned bandwidth and noise characteristics has been built. It would be prudent for NRAO to push forward with development of a prototype receiver as quickly as possible to demonstrate that it can be done and to test robustness, since the latter will be a primary requirement for the MMA operation.

6) **Antennae:** The proposal calls for a pointing accuracy of 1" for all the antennae. This is substantially more accurate pointing than any operating telescope today. We need to know how much <u>more</u> the antennae are costing to achieve this accuracy (if it can be done) and what the consequences will be if it isn't achieved. We were told that there is a gradual decline in image quality as pointing accuracy deviates from the goal of 1". It is important to give a quantitative indication of how much decline in image quality should be expected with decreasing pointing accuracy .

There is plenty of time to resolve these issues before the MMA project gets underway. NSF and the community should insist on well studied solutions to these issues. Assuming that they are satisfactorily resolved, my evaluation of the project is **excellent.**

The following comments are based on the NRAO proposal "The Millimeter Array" and attendance at the site visit which was held at the NRAO in New Mexico in April 1991.

1. Science Case

The science case for the MMA with the performance characteristics specified in the proposal is overwhelming. This uniquely powerful instrument will have significant scientific impact in most areas of astrophysics. Both the formation and death of stellar systems often involve dense, molecular gas, which the MMA will sensitively observe through both dust continuum and molecular line emissions. Thus the MMA will be especially powerful in studying the evolution of astrophysical systems.

While the proposal clearly establishes the science case, it will be useful to continue to monitor the growth of the field as the MMA develops. This is necessary to ensure that final instrumental specifications will optimally fulfill scientific needs for millimeter astronomy. I also noticed that while much of the scientific case rests on line observations, the proposal often seems slanted towards the continuum when discussing technical requirements.

2. Design Concepts

The written proposal gives only a basic summary of the design considerations. During presentations at the site visit these were expanded to include clear explanations of the choice of number of dishes versus size, updates on receivers, etc. It is my impression that NRAO has a sharp vision of the proposed MMA based mainly on considerations of snap-shot mode UV plane coverage and an estimate of maximum feasible baselines for high resolution observations.

There are, however, a few outstanding issues which must be resolved before the MMA has a final design:

1. Since line sources often have low brightness temperatures, they inevitably push the sensitivity limits and require long integration times. It is not obvious that this requirement has been folded into the dish-size/number selection criterion or the geometry of the compact array. Is the compact configuration able to observe sources for several hours during times of good transparency without shadowing? Has the speed of the array been optimized for observations of smaller fields at the sensitivity limits (e.g. individual protostars, extragalactic sources, etc.) or for large angular coverage? It is my personal bias that the choice of array

parameters in the NRAO proposal tilts towards wide angular coverage while the science case may be more oriented towards deep observations of smaller fields. I hope that NRAO will reexamine these issues from a scientific perspective.

2. Site selection is strongly driven by the desire to achieve high angular resolution. The science case for this capability is firmly established, but the actual performance of the 3 km array configuration is less well defined. Will self-calibration techniques normally be required, which might limit the applications of the 3-km array? How does the real atmosphere behave in terms of coherence? For example, might a 2 km array provide much better performance at only a modest cost in angular resolution?

3. The MMA project lacks credibility in the absence of a powerful software package to handle interferometer line observations. This capability must be an integral part of the project.

3. Management Issues

NRAO's approach to management of this project has been very good. The community has been involved from the beginning through a series of workshops to define instrumental specifications based on the science that the community would like to do with the MMA. The recent creation of the "Joint Development Group for the Millimeter Array" is a positive step toward sharing the expertise resident in the operating U.S. OVRO and BIMA millimeter interferometers. This kind of interchange with the community lowers risk for the MMA, and should be supported as part of the project. Community involvement should continue as the project progresses.

One area, however, stands out as requiring action. NRAO has only a small internal millimeter astronomy science group, and it is not even clear that this group is fully engaged in the project. NRAO should add experienced scientific staff in this area to provide stronger internal scientific advisory capabilities for the MMA. In addition, it would be good for NRAO to build a pipeline to get students involved with the MMA project as soon as possible.

There is also a need (which seems to be understood by NRAO) to develop a more complete set of operational concepts. The proposed sites are all at high altitudes where strong winds and heavy snow are common. More thought might be useful in considering how to build the MMA so that it will operate reliably for low cost under severe conditions. For example, one suspects that conventional telescope movers could present big problems, as would the ground-level stations that would be quickly buried by snow.

I also picked up a feeling that the Tucson site may have become somewhat of a loose end with regard to the MMA. Perhaps this is not correct, as it would be unfortunate when much of NRAO's millimeter expertise resides in Tucson.

4. Cost and Schedule

The key point here is whether the MMA is technically feasible and whether NRAO can manage the project. The answer to both questions appears to be yes; so this is a real project!

It is too early to fully judge the reality of the proposed cost and schedule. They are probably the best that can be done without (1) selecting a site, (2) finalizing a specific design, (3) settling choices for first generation receivers and computers, (4) revisiting software requirements, and (5) accounting for central development lab activities.

Operational costs, however, are less well-determined. These can be decided only once a site has been selected and a realistic operational model built. NRAO should be encouraged to emphasize lifetime costs over construction costs as criteria for choosing a site.

5. Summary

The MMA will be a leadership U.S. astronomical research instrument for the next century. It is scientifically well-motivated and a logical addition to the complement of U.S. ground-based national observatories. Construction of the MMA should be a high priority for the NSF in the coming decade.

As a national facility, it would be a grievous error to underscope the MMA project before it even begins. I strongly feel that NRAO has gotten the big picture about right, and now needs to fine-tune a final design. To this end, I would urge the NSF to provide NRAO with funds to carry out a thorough design study that could then be subject to technical and scientific review before embarking on a construction project.

Overall rating: EXCELLENT

T

This proposal will add an excellent and needed addition to world class instruments used to study sources of celestial emission. The Millimeter Array will open the high radio frequency spectral window to intense scientific investigations by allowing highly sensitive arc second spatial resolution measurements at frequencies of 30 - 300 GHz. This instrument greatly complements the capabilities of other ground based astronomy observations at optical and radio wavelengths, which typically have a spatial resolution of order an arc second.

The design goals of the Millimeter Array i.e. one arc second resolution, collecting area of 1000-2000 m^2 and good imaging capability at a wavelength as short as one millimeter is well founded and as already stated, these design goals make the Millimeter Array a world class instrument. The National Radio Astronomy Observatory is well qualified to fabricate and operate this instrument. The present concept of the instrument is very well developed. A large array of 40 elements of 7.5m diameter appears to be the best array design strategy. However, more work must be done in the following areas. The effect of the atmosphere on high frequency operations needs to be developed in a more quantitative manner. The low frequency (~30 GHz) observational program needs to be developed. The exact array configuration and site selection also has to be made.

I give the instrument an excellent rating and urge NSF to go ahead with its development.

The NRAO Millimeter Array proposal represents a truly major step forward for astronomy. It provides for an instrument whose science achievements will rival or exceed those of SIRTF, HST, AXAF and the VLA. There is no question in my mind that every effort should be made by the community to ensure that the project goes forward in the near future with its full capability. As a result of excellent work in single dish astronomy and pioneering interferometer studies at universities the field of millimeter astronomy is now ideally placed to construct a major instrument. The U.S. has the opportunity, for reasons which may not exist in other countries. The U.S. science community is strong in numbers and leads in astronomical understanding, the technology has been developed in the U.S. and finally the U.S. has several suitable sites for the facility.

The most important reason to build the instrument now is that we have recently understood that the millimeter and submillimeter bands hold the key to the problem of star-formation and probably to galaxy formation also. In addition they may well tell us about the nature of the environments of quasars. The field has been slower to develop as compared say to optical or radio astronomy, because the techniques are difficult and emission strengths of the objects are low. Nevertheless, these problems will be overcome by the MMA. It has to be an NRAO project, because only the National Observatory has the infrastructure to manage such a large-scale program. The experience obtained by the VLA staff will be invaluable.

In this review I will just deal with what I feel to be the major issues for the next phase of the project.

1) Site Selection and Atmospheric Effects

The NRAO staff has apparently done a good job in locating and describing several suitable sites with adequate transparency. These have been narrowed down to two for a variety of reasons. I see no basis to object to the selection process, however continued work is required in the area of characterization of the two sites in terms of "seeing", or more specifically, phase coherence time as a function of baseline and frequency. To me this seems to be a critical factor in the science plan. The reason is that long integrations will be required to detect the most distant objects or to obtain sufficient signal to noise at the very high spectral and spatial resolutions needed for various star-formation studies. Such integrations can be obtained either if the phase is stable for long periods (as will be the case on short baselines) or if a calibration source is detectable in the field of view within whatever coherence time is available (presumably the most likely operating mode at long baselines). A knowledge of the statistical behavior of the atmosphere for the sites would help in providing confidence about which observations could be performed and aid in the overall design of the instrument, particularly the decisions about the sizes of the various configurations. Work carried out to date at NRAO by Owen with the temperature fluctuation sensor is very valuable. Actual coherence time measurements as a function of baseline and frequency would be even better, but analysis of VLA data, as initiated by Sramek and touched on briefly in the proposal, could be used to establish a model which could be scaled to the other sites and, using the Kolmogorov scaling with frequency, to the actual frequencies of the MMA.

In summary what is needed during the next phase of development of the project is a clear description of the behavior of the coherence time on the sites, plus a discussion of the number of calibration sources that would be available for the various configurations under those conditions of seeing. It would make sense to confine this study to the two sites already singled out.

2) Instrument Design

Certain observations can be used to define the specifications of the instrument. Chief among these is probably the analysis of disks around forming low mass stars, which will require spectral resolutions of better than .05 km/sec and spatial resolutions of 0.1 arc sec. In the continuum this type of observation argues for the use of the 350 GHz band to gain high sensitivity to dust. On the other hand, distant galaxy detection requires very high sensitivity, down to 1 mJy or less and requires excellent low frequency capability.

a) High Spectral Resolution

High spectral resolution is needed to achieve the thermal limit (.05 km/sec for CO) for all useful star-formation lines, probably down to CS (1–0). This implies a minimum channel width of about 8 kHz for CO or 4 kHz for CS for proper sampling. One should not be misled by the wide lines seen by current low spatial resolution instruments. This requirement could impact the correlator design and the sensitivity calculations.

b) Low Frequency Capability

Low frequency capability is a crucial factor, because of the very heavy emphasis on detection and analysis of dynamics and chemistry of high z or primeval galaxies. A few interesting numbers are:

CO (1–0) at	z = 1	57 GHz
	z = 2	38 GHz
CO (2–1) at	z = 2	77 GHz
	z = 3	57 GHz
	z = 4	46 GHz
	z = 5	38 GHz

This may impact the arguments on sensitivity versus imaging capability in favor of sensitivity and push the low frequency detector technology.

A further argument is that the seeing will be best at the lowest frequencies if the Kolmogorov spectrum holds for atmospheric fluctuations, as is current wisdom.

c) High Frequency Capability

High frequency capability is also needed, because of the requirement to be sensitive to the dust continuum emission. However, the caveat here is that one needs to see into the dust disk or whatever it may be and would like to pick a frequency where $\tau \sim 1$. This may be a strong argument for 350 GHz capability, but some more modelling of dust in disks might be useful.

d) Map Quality

Map quality is a very important area and this has been addressed in excellent detail in the proposal. The plan to obtain small spacing data through total-power measurements of the array dishes appears a fine way to avoid the complications of a large single dish. Because of the small field of view of the antennas, mosaicing of fields will be essential and this has been discussed in a most satisfactory way in the proposal. The analysis has led to valuable information on antenna pointing requirements, for example.

e) Size of the Instrument

At the present this is being analyzed from two points of view, sensitivity requirements and imaging speed. While the present analysis leading to about 40 antennas of 8 meter diameter seems reasonable, I feel that the instrument should certainly not be scaled down, but scaled up if anything. My reasons are that we want to make this the very best possible national instrument and it is unlikely that the opportunity to expand it or rebuild it will subsequently occur, so we must aim for the most sensitive instrument right at the beginning. I concur with the cost estimates throughout so I think that a larger array would inevitably increase the overall cost. Nevertheless, increased sensitivity would relieve the problem of calibration sources and allow deeper probing of both the galaxy and star-formation problems. I think that sensitivity is the more critical of the two issues, because the field is basically photon starved, as compared with the radio or optical and the most interesting of the science aspects are near the sensitivity limits.

f) Receivers

It is clear that the most cost effective route to better sensitivity is to develop lower noise receivers. Progress with niobium SIS junctions is very good at labs such as Caltech and NRAO at Charlottesville. This is another area (like the seeing studies) where a strong near-term effort will pay off. However, for the reasons stated above, this should not be used as an argument for reducing the size of the array.

Two related areas which require a significant near-term effort are cryogenics and local oscillators.

Both of these have existing solutions which are technically viable, but a lot of effort and money will be saved in the final project if a more efficient cooler could be developed (such as the Er_3Ni concept) and if efficient wideband, easily tuned local oscillators are developed.

g) The Single Dish Issue

I see no strong argument for a single dish to be part of the array. In the first place the NRAO solution to the small spacings problem is well thought out and provides the necessary information within the context of a homogeneous array, so reducing technical problems including maintenance. In the second place I haven't heard any strong scientific arguments for single dish science with the type of dish which would be needed as part of the array. It is hard to think of anything which could be done by such a dish which would not be done very much better by the array. Finally, I doubt whether it makes sense to commit a large fraction of the observing time of a single dish, which really does have its own scientific rationale, to the subservient task of providing small spacing fluxes for other observations.

h) Computer Hardware and Software

What we have learned about interferometry in the time of the VLA is that computer hardware and software can be used to provide amazing capabilities for the instruments, but if its role is not anticipated it may well be the limitation in the use of the instrument. Clearly NRAO will make use of existing concepts such as self-cal and mosaicing, but it also seems really important to develop a system which allows calibration in real time. NRAO must plan for rapid data reduction of observations involving the full array, all baselines, polarizations, velocity channels etc.. Again, it would be appropriate to start the planning now.

3) Staffing

The project is a grand national endeavor, but if NRAO is to carry it out it has to build up a staff of experts in the field of millimeterwave interferometry. While there may be no money to do th right now, something has to be done, because the detailed planning stages are critical to developing an excellent project. A lot of developments specific to millimeterwave interferometry have come about in the universities in recent times and there is much expertise in millimeterwave interferometry. I believe it is essential to get at least one such expert on board right now.

4) Management

It is obvious that a management plan is needed and it is entirely up to NRAO as to how it would go about it, i.e. whether to centralize the VLA, VLBA and MMA efforts or possibly keep the MMA on a separate site. Presumably the decision is strongly coupled to the choice of site for the array itself.

5) Relations with the Universities

Rather like the single dish issue, this is one that has caused concern in the general community. The reason is the same. An operational MMA will completely change the range and quality of millimeterwave astronomy that can be done and will severely impact the role of existing instruments. It is hard for people to see their own roles in the distant future. However, the MMA will provide such tremendous observing capabilities that I am sure when the time comes single dish and current millimeterwave interferometer scientists will find appropriate opportunities for themselves. In the meantime the NRAO plan to consult with the university groups who have expertise in the field is reasonable and should prove useful to all.

Some areas where NRAO would benefit and by their support could help the universities keep their student training activities alive and strong, might be in receiver technologies, correlator designs and chip development, software for data reduction, overall instrument planning and science planning. Although this is an NRAO project it needs to use the "national" expertise.

Summary

I congratulate NRAO on a superb project. Such a tremendous step forward needs aggressive planning and substantial funding in the planning stages. I urge NSF to provide NRAO with an adequate budget to begin staffing and accelerate software and technology development. I would recommend that NRAO pursue the site selection and seeing problems. Work should continue on the instrument design with either in-house hardware efforts or university collaborations to improve receivers, cryostats, local oscillators and correlator chips. The lower frequencies should not be forgotten, because of the great importance of detection of high z CO lines. Very high bandwidth front-ends, IFs and correlator chips are needed for continuum and high frequency work, but possibly even more important, very high spectral resolution is needed for analyzing the extremely small volumes of gas which will be detected at high spatial resolution.

Review comments -

Scientific Merits:

The only negative charge that can be realistically leveled at the scientific case for the MMA is that it did not go far enough. In other words, the scientific case is stronger that the proposal indicates. Whether or not the latter is true, the demand for such an instrument among the world's radio astronomers will undoubtedly be very high and should continue to be high for decades after it is finished. However, the scientific *limitations* of the instrument caused by the relocation of antennas, the reliability of components and the atmospheric turbulence are very important. If, for instance, the atmospheric phase stability were to be very limiting at the longest spacings, it might make the total power mode much more important or lead to a different geometric configuration. Site selection may be affected by these unknown factors. Such uncertainties are normal at this stage of development, but should be clarified in a detailed study before any of the major design decisions are fixed.

Technical:

The effort to build the array is large, but technically well within the current state-of-the-art and the capabilities of the NRAO organization. Little improvement in the current technology is needed beyond the expected cost improvements in solid state hardware. There appear to be no "show stoppers" in the way of completing the system as envisioned, with the possible exception of the site acquisition. Site acquisition is a thorny area that may have become even more thorny with the recent activities at a nearby site. Access to the site should be well in hand before funding is committed to other major subsystems.

The imaging analyses are particularly well conceived. The case for a homogeneous array seems to be quite convincing in the proposal. The controversy surrounding the large single dish antenna is likely to be a short term phenomenon which will soon clarify in favor of the NRAO design.

General:

Enough of the cost elements are known that the total proposal value is certainly close (inflationary issues aside). One exception may be the software budget, where there is strong community pressure to exceed past performance. The time scale may be more difficult than the cost. The lack of equipment funds in the proposed operations budget makes completing the build during the project phase imperative. This may present difficult peak staffing requirements that tax even the largest resources available to NRAO. A safer approach might be to spread out some of the subsystem costs into a five year plan that overlapped the early operations at specific configurations or wavelengths.

The size of the project is good for a National MM Facility. It could not be much smaller nor much larger. A smaller project would not be worth the effort, be inappropriate for the year 2000, and not provide the user community with a sustainable advantage over other instruments. A larger project would be hard to justify at this time.

The operating budget seems thin if we are to really make use of this class of facility. A upgrade budget of 5% of capital expense per year, exclusive of operating expenses, is not uncommon for high tech facilities.

Clearly, it was the recommendation of the Barrett Committee that a national millimeter wavelength array should be built, and it was recognized at the time (1983) that the logical organization to build it was the NRAO. However, times change and while it is still true that a big, national millimeter array is badly needed, it is essential that the NRAO demonstrate that they are capable of building the best possible instrument. The scientific part of the proposal is very interesting, but one must realize that this section is almost like a "wish list" which must be separated from the question of whether or not the NRAO can actually build an instrument which will execute this proposed science. The Joint Development Group is an excellent idea, but since it was started so late in the game, it is too early to tell how well it will work. Possibly because the idea of gathering input from active millimeter interferometer users was not taken seriously earlier, there are some major areas of concern with this proposal that I wish to discuss here. The problems are:

1. A homogeneous array versus a heterogeneous array and the close spacing problem. The driver for the Millimeter Wave Array has been radio molecular spectroscopy of astronomical objects. The astronomy starts when a molecular line identification is made; then astronomical objects are mapped, usually with a single-element telescope; finally the ultimate map of each object is made via an array. One of the very most important sources in the field continues to be the galactic center region. Because of the popularity of Orion, the importance of the Sgr molecular clouds in this process may be overlooked but, in fact, many new molecular species are found first in Sgr B2 and even today quite a few are known only in that source. At the Site Visit, the problem of potential shadowing of the proposed homogeneous array in the compact configuration while observing low declination sources (such as Sgr B2) was treated too casually. A hybrid configuration was suggested as the obvious solution, but further questioning after the meeting revealed that it is very difficult to get time on the VLA in the hybrid configuration that is optimized for the galactic center region because not many other sources can be conveniently scheduled along with Sgr. This is easily seen from the attached finding chart which shows the paucity of good molecular sources around the declination of Sgr B2. It may appear that an answer to this dilemma might be for the NRAO to simply build the hybrid into their permanent repertoire of configurations, but then this would impair the UV coverage for sources at other declinations, and probably compromise the efficient scheduling of a national visitor instrument - which is very important to the NRAO. A less serious solution of putting part of the array on a hillside was suggested, but this assumes that somehow 45 ton antenna elements can be

maneuvered around on slippery slopes. The most obvious solution of using a 15-20 m class single dish for taking the close-spacing data (a heterogeneous array) in the compact configuration was dismissed by the NRAO people without any serious discussion. Why?

2. Proper costing of software needs. It sounds trivial to say that the Millimeter Array is not the same as a centimeter array, but I don't think that this point has really sunk in at the NRAO. An important component of the future work on the MMA will be the spectroscopic study of new molecular species. The people who do this work do not overlap very much with the people who do centimeter wavelength imaging. The millimeter people tend to work under more funding pressure and deadline pressure, so they can not afford to wait for months while processing interferometric data for a spectroscopic study. The computing discussion should include the use of user-friendly software to check for bad data points and to provide a quick way for the observer to extract calibrated spectral data from a data cube. The appropriate cost for developing this software has not been included in the budget for the MMA. The VLA is the obvious testbed for the NRAO to use to show that it is serious about developing the appropriate software for spectral line studies, but it has been very slow to do so. What assurance do we have that the MMA spectral line software won't end up like the VLA spectral line software?

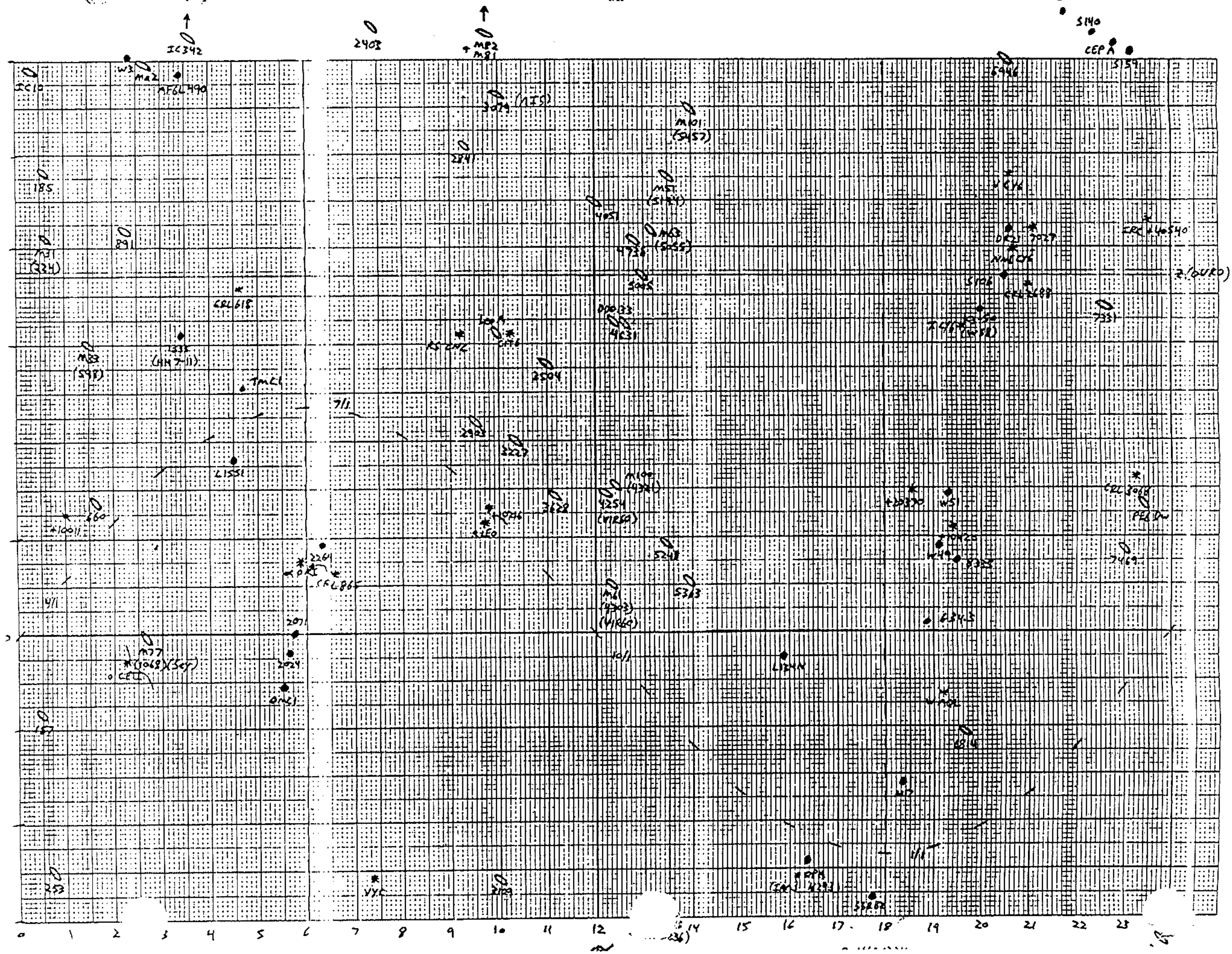
3. Millimeter experts. The NRAO does not employ anyone who does full time millimeter interferometry. So how do decisions get made on this project? Clearly, some important problems have gone unrecognized already and others have not been properly solved because nobody at the NRAO works full-time in this area. Over the short run, the best thing to do is to add postdoctoral people and senior visitors to act as local resource people. Over the long run, active millimeter experts should be added to the scientific staff and used as in-house consultants on this project.

4. Site selection. It appears that the Springerville site is superior, but the Magdalena Mountains site is being promoted because it is closer to Socorro. Another problem is that at the time of the site visit, the correlation length problem for the longest baselines available at the Springerville site had not been properly addressed.

In summary, it appears that I must agree with those who think that the current MMA proposal is written more like a concepts document than like a real proposal with a carefully considered array design and a realistic budget. I think that a lot of work should be done fairly soon to correct the problems discussed above with the array design, the software, the personnel, and the site selection before I have much confidence that the NRAO is ready to build the MMA.

K+E 10 X 10 TO THE CENTIMETER 18 X 25 CM.
KEUFFEL & ESSER CO. MADE IN U.S.A.

461510



Review of Millimeter Array Proposal

Since the early 1970s, the US has made a strong commitment to the development of high angular resolution radio astronomy. This commitment has led to the construction of two world-class facilities: the Very Large Array which is the premier instrument for interferometric studies of astronomical sources at cm wavelengths, and the Very Long Baseline Array which promises to provide the basis for ultra-high angular resolution cm-wave observations. During the 1980s, millimeter astronomers in the US initiated highly successful efforts to develop millimeter-wave interferometers. The Berkeley and Cal-Tech interferometers, while of very modest scale, have already demonstrated the enormous potential of obtaining high angular resolution imaging and kinematic data for primitive bodies in the solar-system, stars, protostars and galaxies. The NRAO proposes to build on the experience garnered from the university-based interferometers and to construct a high sensitivity imaging instrument capable of providing 0.1 images at 1 mm. This instrument, if built, will be a tool of immense power, which promises to enable galactic and extragalactic studies which should be revolutionary in their impact. Combined with new generation ground-based optical/infrared telescopes equipped with adaptive optics, and extant cm-wave interferometers, the proposed array will provide US astronomers with the tools to image the sky at 0.1 resolution over a wavelength range extending from 2μm to 20 cm. Investment in O/IR telescopes early in the 1990s and the mm-array in the mid- to late- 1990s, would represent a natural extension of a long-standing US commitment to world leadership in high angular resolution astronomy. Without such an investment, leadership in ground-based astronomy will pass to our colleagues in other countries, who are investing considerable effort and funds to explore the new frontiers of high angular resolution astronomy. The millimeter array is a key element in the astronomy strategy of the 1990s and must be given the highest priority for NSF funding following the development of the twin national 8-m telescopes.

The NRAO proposal appears to be scientifically compelling, as well as technically sound and feasible -- building as it does on the experience of the US mm-community both in single-dish mm-astronomy and in interferometry. There appear to be no "show stoppers" as far as I could judge from the discussions among technically knowledgable individuals.

One of the research areas to which the mm-wave array will contribute most is that of star and planet formation. The MMA will be able to study to structure and kinematics of proto-stellar cores, the forming star/disk systems embedded within these cores, the physics and chemistry of disks surrounding young, optically-visible pre-main sequence stars, as well as some evolved, perhaps post-planet-building structures which may surround older stars. The advantages of the mm-array for such studies result from its ability to image both molecular gas (to probe the structure, temperature, density, kinematics and chemistry of the disks and cores), and dust, which may provide the most sensitive tracer of the planet-building process. Because the nearest star-forming regions lie at a distance of 150 pc, the ability of the mm-array to make images with an angular resolution of 0.1 is critical; at this distance, 0.1 corresponds to 15 AU or ~1/3 to 1/5 the extent of a forming solar-system around a solar-type star. Even greater angular resolution would thus be highly desirable. The high sensitivity promised by the MMA is also *critical* to star and planet formation. Perhaps no other area of research drives both resolution and sensitivity to comparable limits. The strongest demand for high sensitivity derives from the need to map the kinematics of slowly rotating and/or infalling gas (< 1 km/sec) located in the outer regions of circumstellar disks or in protostellar cores.

From the tables presented in the proposal, my estimates suggest that successful, "self-calibrated" observations of the circumstellar environs of young stars will be difficult, but possible given the full complement of antennae proposed by NRAO. *Scaling back the sensitivity or the angular resolution will seriously impair observations of star- and planet- building regions*, and may render some classes of observations (kinematics in outer disk or core regions) impossible. It would therefore be unwise to reduce either the size of the array, or of its component antennae. In fact, star and planet formation studies would benefit significantly from efforts aimed at enhancing the proposed sensitivity limits. Specifically, the MMA project should make strong commitments to:

** the choice of a first rate site which a) has atmospheric transmission and stability, and b) which can accomodate the addition of out-rigger antennae to enable higher angular-resolution studies;

** the development of the most sensitive possible receivers; this effort should be coordinated closely with university groups;

** efforts to reduce the costs of individual antennae so that the individual elements can be as large as possible, given budget constraints.

It is also *critical* to the long-term health of US millimeter astronomy and to the success of the MMA project to involve university scientists and graduate students in a continuing program of innovative instrumentation development. Having all the expertise in receiver development, data analysis techniques, etc. centered at NOAO is in the long run detrimental to the advancement of mm-astronomy per se, and the development of future generations of mm-wave astronomers. The mechanism proposed by the NRAO -- the formation of a Joint Development Group, comprised of university scientists -- is a promising first step. The NRAO and the NSF will be challenged to evolve a management structure and instrumentation development plan which provides for substantive involvement by university scientists in trade-off decisions, and participation in instrumentation/receiver development programs. However, there is time to evolve a plan which provides a role and support for university scientists carrying out specific functions in support of the MMA project.

To effectively manage this project, it is imperative that NRAO not only draw on the expertise within the university community, but to enhance its internal strength in mm-wave astronomy and technology. Specifically, I would strongly urge the immediate appointment of a senior mm-wave astronomer to lead the effort within NRAO.

Overall Rating: EXCELLENT

Y

1. The proposal is technically sound and NRAO is probably the optimum organization to manage it. It does require skills and resources outside of those presently at NRAO, which is at least one reason for close cooperation with other centers now busy with millimeter-wave astronomy. Past experience is that NRAO has generally managed its efforts in a commendable way and helped in the production of outstanding research.

2. The research which might be done with the proposed millimeter array is interesting and exciting. It can be expected to lead to a better understanding of many astronomical objects, and also to be challenging technically. It should stimulate substantial engineering and applied science efforts in sensitive detection and amplification, as well as in development of other millimeter wave equipment.

3. The proposal has important scientific goals and some possibility of contributing to technology and the solution of societal problems. The technology obviously has potential importance to communication, radar, military, and space work. There's no clear and immediate demand for such technology, but no doubt it will be of value in the future.

4. I believe the primary issues which are difficult to resolve involve the effects o. such a project on the nation's scientific and engineering research, education, and human resources base. In general, concentration of research in single big instruments and in national institutions is, in my view, to be avoided as far as is practical so that many universities and other institutions can participate in research and young scientists can be involved in research in a first-hand and very personal way. However, there are certain fields which require massive instrumentation, and one can argue that the time has come for a large instrument in the field of millimeter wave interferometry.

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

Results from NRAO prior work have generally been excellent.

The several present smaller systems in which the nation has invested have, I believe, been doing a good job in applying resources and training young people. If there is to be a large centralized instrument, it would be extremely important to see that these groups which are now active plus still others are involved in its planning, design, and use, and that present installations are kept viable as far as is practical. Apparently, NRAO management has this in mind and I believe will try to do a good job in both cooperating with and calling on talent from the broader community. Hence, it seems to me logical that planning for such an enlarged system should be made.

The primary uncertainty in my own mind is the scale of the new system. Scientifically and technically, it would be logical to start with a system of perhaps 20 antennae, while allowing for a later expansion towards a larger system at some time after there has been considerable experience with a smaller one. While there are equipment uncertainties, such as sensitivity, frequency flexibility, and bandwidth of the detector-receiver system, there are not a large number of uncertainties which should make the planning of a big system impractical. Perhaps the primary open question in this respect is atmospheric fluctuations, and these could change optimum plans substantially. Furthermore, in time somewhat better designs for the microwave antennas, receivers, and data processing will very likely develop. The biggest argument against attempting a more modest-size instrument at this point and for aiming towards perhaps 40 telescopes in the array rather than half of that number seems to be political. That is, since 20 telescopes will probably turn out to be quite successful and yield very good results, once they are built the same system will seem quite viable and the science community may find it difficult to obtain money to enlarge the system. The common thought is that it would be easier to get an ultimate system by seeing that it is funded from the beginning. I regret making decisions on such a basis, and believe money is often wasted by such a philosophy. The country's scientific efforts are already somewhat distorted, in my view, by too many rather large-scale efforts and not enough smaller, more varied, and more personal scientific efforts and experience. "Main stream" science is often rather blind and we need opportunities for a richer variety than it sometimes allows. Nevertheless, the political argument mentioned certainly has some validity and I have no strong recommendation as to just how this particular matter should be decided.

Advisory Committee for Astronomical Sciences (ACAST)
Resolution on the Millimeter-wave Array (MMA)

The ACAST reviewed the MMA proposal, and is very excited about this bold and scientifically meritorious project. The committee suggests that the research and development work required to resolve the remaining technical questions involved in operating such a facility, and which is required for accurate costing of the project, proceed. In addition, the efforts needed for addressing the environmental impacts of the MMA should also be carried out, especially in view of the time scales imposed by environmental regulations. In considering the MMA project, the ACAST was excited by the potential of the MMA, but was also very concerned about how the operation and construction of the array would be funded. The ACAST suggests that very careful consideration be given to identifying where the operating costs will be found within the AST budget, and to implementing measures that will make the operations as cost effective as possible.

Marcia J. Rieke

Marcia J. Rieke
Chair of ACAST
January 6, 1992