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Subject: gelegenheid voor weerwoord op MMA refereerapporten
To: ewine@strwchem.strw.LeidenUniv.nl
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Aan: Ewine van Dishoeck
Frank Israel
Harm Habing
George Miley
Harvey Butcher
Ed van den Heuvel

Van: Wilfried Boland

22 december 1995

NWO heeft ASTRON gevraagd een wetenschappelijk protocol samen te stellen met betrekking het voorstel inzake een Nederlandse deelname in de MMA. Dit protocol dient
het volgende te omvatten:

= Een aantal refereerapporten
= Een weerwoord van de aanvragers op de refereerapporten
= Een advies van ASTRON

Dit protocol dient in de 2e week van januari a.s. bij NWO ingediend te worden.

Het lijkt mij gewenst de namen van de referees niet bekend te maken. Om de commentaren goed te kunnen interpreteren het volgende: de 1e referee is een Amerikaan;
de 2e referee is een Duitser; de 3e referee is een Fransman. Ik heb ook een Brit en een
Japanner gevraagd een refereerapport op te stellen. Zij hebben nog niet geantwoord en
ik zal ze vragen naar de reden.

Hierbij verzoek Ewine, Frank, Harm, George en Harvey om als aanvragers samen een reactie samen te stellen op de refereerapporten. Ik wil Ed van den Heuvel vragen in dit
proces afzijdig te blijven zodat ik samen met hem als voorzitter van ASTRON Bestuur
een advies van ASTRON aan NWO kan opstellen.

De verdere input van het CvB van de RUL kan worden ingebracht bij het weerwoord op de refereerapporten. Ik heb van Harm begrepen dat hij hierop zal letten.

Graag ontvang ik het weerwoord op de refereerapporten op maandag 8 januari a.s..

Prettige feestdagen toegewenst,

Wilfried Boland

****
1st referee:

Review of proposal for Dutch participation in a large millimeter array

Overall scientific rating: excellent

Impact on Dutch astronomy: excellent
Overall urgency: urgent
Political judgement: excellent

1. Scientific Merits
a. Does the proposal have a clear scientific goal ?

The scientific goals are clear and clearly expressed.

b. Is this an important goal ?

The goals are important in astronomy and cosmology.

c. Can the goal be achieved through the proposed participation ?

Substantially.

d. Can the goal better be achieved by another way ?

No.

2. Significance to astronomy as a whole
How does the result from the proposed participation impact on Dutch astronomy ?

The impact on Dutch astronomy will be major and should ensure its leadership role.

3. Competence of the team of proposers
Can you judge the track record of proposers, in all fields of astronomy ?

Yes--all internationally respected.

4. Budget
Is the requested budget justified in the proposal ?

The budget is simply 10% of the total for 10% of the time. The overall budget seems
to have been worked out carefully.

5. Timing
Is there a clear description how the goals of the proposal can be met ?
Is the schedule realistic ?

The proposal is specific in describing the steps in which the goals can be achieved.
The schedule is realistic, bearing in mind that the United States is entering a period
of considerable science funding uncertainty.

6. International partnership
Is the proposed participation justified in the context of other international developments
with respect to large millimeter arrays ?

This is the best choice amongst the possible international proposals.

7. US MMA proposal
Could you give your personal judgement of the US MMA proposal independent of the proposed Netherlands' participation. Will we MMA make a major scientific impact ?
Which fields in astronomy will benefit significantly ?

The MMA will have an enormous impact on fuundamental questions ranging from the study of star and planetary formation to the early evolution of the Universe and
the formation of galaxies.

8. Other comments you hay have

Participation in this project will serve to maintain the distinguished role of Dutch
astronomers internationally.

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2nd referee

The following is a somewhat hurried report on the proposed Netherlands participation
in the MMA. I am sorry but it came at a bad time, I just have tried to answer your
questions one by one. I certainly consider the "overall scientific rating" to be excellent
and it is clearly "very urgent". If one does not jump on the train, it will leave the
station. As far as the "political judgement" is concerned, I am not the person to ask !!

1) Scientific Merits.

The proposal has a clear goal and that goal can be achieved through the proposed participation. There is no doubt in my mind that the proposed MMA will be (as the VLA
has been) one of the worlds leading astronomical instruments. There are probably no
other ways to achieve such goals that are financially feasible.

2) Significance to Astronomy as a whole.

Entering as a junior partner in a project like the MMA has the disadvantage that the
important instrumental and technical development is likely to take place in the USA
rather than in Europe. This limits the possibilities for innovation by astronomers who
work in the Netherlands. On the other hand, the advantages of having guaranteed access
to the MMA for Dutch Astronomers is enormous. One in my opinion has to compare the pros and cons of these two effects when considering entering as a partner in the
MMA. I take it that the proposers have decided that the interest of the "pure Astronomers" (as opposed to more technically oriented people) should be paramount.

3) The competence of the team of proposers is beyond question.

4) and 5) Budget and Timing.

Sorry. I cant really make useful comments.

6) International Partnership.

I think the proposers arguments for avoiding getting involved with the European
Partnership as it is presently conceived are justified. Of course, it would be better still
if it was possible to persuade other European countries to make a joint approach to
NRAO aimed at (say) having a 50-50 partnership between the USA and Europe. This would avoid the disadvantages of "Junior Partner Status" alluded to above. However, I realise that it may be impossible either to get agreement between European
countries or to get agreement from NRAO for anything of the sort.

7) US MMA Proposal

As I think I made clear above, the MMA proposal seems to me to be likely to be

extremely successful.

****
3rd referee

Referee report on a proposal for Dutch Participation in a Large Millimeter Array

Overall scientific rating: good

Impact on Dutch Astronomy: strong

Overall urgency: urgent if approved

Political judgement: poor

The purpose of this proposal is to participate for 1/10 (20 M$ investment and later operation costs which are usually 6-10% of the investment every year) in the american project of a Millimeter Array (MMA), and to get in return guaranteed telescope time on the MMA and on the combined CalTech-BIMA millimeter interferometers, presumably moved to a high-altitude site in California. The status of the MMA is presently uncertain, but there is hope that the Congress might decide to finance a phase 1 design in 1996 for an amount of 25 M$ and a possible start in 1997. However there is a requirement that the MMA becomes an international project with foreign shares totalling 25% (a recent rumor is that these shares should be close to 50%). A positive decision on a Dutch participation will certainly help to start the MMA itself, although it will not be sufficient by itself. In view of this situation, I recommend that a decision (positive or negative) on the principles of the Dutch participation is taken soon, before mid-1996, although the participation to investment can start in 1997 or perhaps somewhat later. I now give my opinion on the different aspects of the proposal.

1. Scientific Merits

a. The project has a clear GENERAL goal: to obtain telescope time on a big facility in the millimeter-submillimeter domain. There is no doubt that after the giant optical telescopes and the indian Giant Meter Radio Telescope (GMRT) the next generation of ground-based astronomical instruments will be in short-wavelength radioastronomy; however there are two different types of possibilities: either giant instruments in the centimeter-decimeter range, or giant instruments in the millimeter-submillimeter domain.
These instruments will undoubtly be interferometers. However it is hard to decide which of a cm-dm giant interferometer or of a mm-submm giant interferometer is the most interesting. This is to some extent a question of taste and education. In any case both instruments have a very high potential for discoveries and it would not be a mistake for Netherlands to engage on any of these tracks. I do not consider it useful to go into details of the science made possible by a large mm-submm interferometer: the proposal is quite good in this respect, and the discussions at the workshop on "Science with Large Millimeter Interferometers" held in Garching last 11-13 December have confirmed this, while emphasizing a few other points not discussed in the report.

b. It is certainly an important goal to participate in a large mm-submm array; however whether the MMA can be called a giant array is problematic: it has only twice the collecting area of the IRAM interferometer (with 6 antennas) and is not a major breakthrough: I will elaborate on this later.

c. see above

d. The same goal can also be achieved through participations in other instruments, through in a different way. As a possible alternative which I will discuss below, Netherlands could participate first in an European mm-submm interferometer larger than the MMA.

2. Significance to astronomy as a whole

While the proposers are first-class astronomers, some of them being active in millimeter-submillimeter radioastronomy, they cannot pretend to represent the
Dutch astronomical community as a whole. This being said, it is the obvious interest of this community to strongly develop mm-submm astronomy as a particularly promising area of research which is very complementary to

some well-developed other areas of excellence of Netherlands (e.g. infrared astronomy).
One of the problems with the present proposal is that there is no clear desire from the proposers and consequently no guarantee for any technical and/or industrial return for Netherlands, in spite of strong national competences in several fields of relevance for the project (these competences are shortly described in the proposal and it is certain that Netherlands might efficiently contribute in the front-ends, correlators and on-line and off line software). If
the agreement does not contain from the start guarantees for such returns, there
is a good chance that the US partner will do every technical development even if
there are signs of opening in the Prospectus.

3. Competence of the team of proposers

All principal investigators are astronomers of considerable international reputation:

Ewine van Dishoeck is basically a theoretician-modeller with a strong background in physics and chemistry, a large experience and broad views resulting from truly international activities, and an interest in observations, in particular in the mm/submm domain.

Harm Habing is an eminent astronomer of the preceding generation, who has acknowledged competences in interstellar matter and circumstellar envelopes in general; observationally, he has experience in cm-dm radioastronomy and in infrared astronomy.

George Miley also has a very strong international reputation, essentially on distant galaxies, radiogalaxies and quasars and observational cosmology. He his an active observer in cm-dm radioastronomy as well as in other wavelength ranges.

Frank Israel is essentialy an observer with much observing experience, in particular in the near and far-IR and in radio including mm wavelengths. He is also very good at interpreting observations.

None of these persons has made extensive observations with mm interferometers, although they undoubtly understand their principles well.

4. Budget

The budget of the MMA has been well studied by very competent people and seems reasonable. For a country like Netherlands, a participation at the level of 10% also seems reasonable. This is probably an amount which would make Netherlands a serious partner for the US.

5. Timing

There is not much to say about the timing of the MMA, which depends essentially on the date of the green light. An optimistic version presented p. 79 of the document gives 2005 as the date of beginning of operation, but this might well be delayed by several years, in particular given the difficulty of pluri-annual budgetization in the US (remember the sad story of the SSC). In the meantime, Dutch astronomers might have access to the University interferometers (Caltech+BIMA) presumably moved to a better common site: but the money for this move does not exist and it is uncertain when this facility will be available. The proposal is curiously quite vague on this part of the cooperation.

6. International partnership

It is necessary to put the MMA project in context and to examine the other facilities. There is a description in the proposal but it is not completely up-to-date and contains errors: thus I decided to write the following summary.

- The most powerful present mm interferometers are those of IRAM and of Caltech.
A submillimeter interferometer on Mauna Kea (not mentioned in the proposal) is mentioned on p. will
also be a powerful instrument.

The IRAM one will soon have 5 antennas with a total collecting area of 884 sq. meters; the adjunction of a 6th antenna for which a financing is becoming likely will provide 1060 sq. meters and allow good images to be obtained

with a single configuration. There are dual receiver boxes at 3 and 1.3 mm. Contrary to what is said in the proposal, the site of plateau de Bure allows baselines up to 1.7 km and is sufficiently good for routine operation at 1.3 mm from November to March and occasionally outside this period.

The Caltech interferometer has 6 10-m antennas with a collecting area of 471 sq. meters, allowing good imaging in a single configuration. It also has dual receiver boxes at 3 and 1.3 mm. The site is probably somewhat inferior to that of IRAM, but there are plans to move it to a better location.

On Mauna Kea, a submillimeter interferometer will consist of the 15-m JCMT (to which Netherlands has access), the 10-m CSO (Caltech submillimeter observatory) and the 6-antenna SMA, a submm interferometer developed by the CfA. This will be
a pionneering instrument at the highest possible frequencies (350 GHz and possibly 800 GHz), and the Dutch astronomers should have a share on it through the JCMT if they continue their participation.

- There are 3 main projects for large mm interferometers. All three will have many antennas allowing to obtain snapshot images.

The MMA consists of 40 8-m antennas with a total collecting area of 2011 sq. meters. The present plans are to have the highest frequency as 360 GHz but the excellent site in northern Chile where it is likely to be located might allow observations at higher frequencies and there are plans to push the operation up to 800 GHz. However this site is at 5000 m elevation where life is really difficult even for natives.

The Japanese LAMA will have 50 8-m or 10-m antennas (collecting area 2513 or 3927 sq. meters) and will presumably also be installed in Northern Chile although at a somewhat lesser elevation. There is no financing yet and I don't know much detail on the time scale of this project.

Europe is planning a large interferometer with 50 16-m antennas for a total collecting area of 10 000 sq. meters. The highest frequency is 360 GHz. This instrument would also be installed in Chile at a manageable elevation because of its larger extent. There is no financing yet.

Three remarks are in order at this point:

1. The maximum angular resolution for a given sensitivity in brightness depends strongly on collecting area A resolution of say 0.1" will barely be achieved on thermal line sources (say molecular clouds) with an area of 2000 sq. meters (MMA) while this would be possible with an area of 10000 sq. meters (the European project). Note that there is probably no other limitation to baseline length that sensitivity, as the atmospheric phase correction schemes presently under development will very likely allow to correct phase for any baseline.

2. The optimum size of antennas is a multi-parameter problem, and no clear answer exists even when surveys of large areas of the sky are considered, although a clear advantage of large antennas is the ease of calibration of atmospheric fluctuations on reference sources. If a larger collecting area is desired large individual antennas are necessary in order to keep their number hence the number of baselines and the size of the correlator within reasonable limits: this is the concept of the European interferometer project. The smaller field of view is not a drawback given the progress of mosaicing techniques (which actually require a good signal-to-noise ratio on individual fields hence large antennas), and of mosaics of receivers in the focal plane.

3. It is not obvious that in the considered area of Northern Chile there is a strong gain in atmospheric conditions with elevation, thus a site at moer than 5000 m elevation as proposed for the MMA may not be necessary.

The idea of participation of Netherlands to any of those projects is good, but the choice made in the proposal is disputable for reasons which will be given in Section 8 below, after discussing the US MMA proposal.

7. The US MMA Proposal

This proposal is undoubtly the result of much reflection, but nevertheless it is already a 10-year old project with a number of weak points.
The main point is that it is not ambitious enough, in that its total collecting area is too small. It is only twice that of the IRAM interferometer, which can provide images of discrete sources not much inferior to those of the MMA. For surveys of cosmological importance which are perhaps the main driver of large mm arrays, going to higher frequencies (the MMA vs. the
European project) is an advantage due to the very steep spectrum

of the continuum of galaxies on the long-wavelength side of the maximum, but this advantage is balanced by the larger area of the European array at longer wavelengths; when the redshift is such that the maximum is at 1 mm (z=5-6) collecting area becomes the dominant factor. For surveys in the CO lines, which are redshifted at relatively long wavelengths, only area counts. In general, e.g. for study of the very small structure of thermal emission (e.g. protostellar disks in lines or continuum), the only important thing is sensitivity and the European interferometer will be considerably better than the MMA. Also, the antennas of the MMA are probably of too small diameter: the best sizes are probably in the range of 10-16 m, but this remains to be studied in details. These antennas are probably too small for reaching the relatively high signal-to-noise ratio required for mosaicing over a large field, and this is a severe drawback for surveys at high angular resolution. Finally, the site provisionally retained for the MMA is at a very high elevation, making life really difficult for the personnel.

This being said, the MMA in absolute terms will allow very significant progresses and will presumable have a major impact especially in the fields of cosmology and star formation: but so will its competitors, and in a better way if they have more collecting area.

8. Final comments and conclusions

There are several possible strategies for developing mm-submm astronomy in Netherlands.

First one should remember that there is presently a non-negligible access for the Dutch astronomers to mm-submm facilities: the SEST, the JCMT, and more interestingly in a near future the Mauna Kea submm interferometer consisting of the JCMT, the CSO and the SMA. Withdrawing from participation in the JCMT as suggested in the proposal would also mean losing access to the latter facility which holds very interesting promises, probably more exciting due to its pionneering nature than those offered by the combined Caltech-BIMA interferometer which will remain less sensitive than the IRAM interferometer.

The strategy proposed in the report is to join the MMA while withdrawing progressively from the JCMT. The first results will come in 2005 or later, if the MMA is done. Before that Netherlands would have access to the combined Caltech-BIMA array (but not to the IRAM interferometer which is better, and to the Mauna Kea submm array which is also of very high interest). If the European mm-submm materializes, the MMA will become a second-rate facility unless it specializes in the submm domain, or unless both projects eventually merge into a world project (which would be the best solution, but is at present difficult politically). Technical/industrial return to Netherlands of participation in the MMA would be doubtful. Finally, the Dutch community would cut itself from European mm-submm astronomy which is presently very well developed and may well have the best potential worldwhile. In this respect the proposal is surprising as several Dutch astronomers are active in studies of the European project.

An alternative strategy would be to join the European array which reperesent an increase in sensitivity by a factor 10 with respect to existing facilities, against a factor of only 2 for the MMA. A Dutch input would obviously help very much the European project to start. The first results would arise only in 2010, but this time with the best facility in the world. The problem then for the Dutch community is to keep busy in the meantime. This would come naturally from its share of the JCMT and of the Mauna Kea submm interferometer. Netherlands can also consider a participation to the IRAM (adding antennas). This would give it access to the best mm facility after the MMA (if the MMA is ever built!), as well as to the 30-m IRAM telescope, and help building a truly integrated European mm community. Technical/industrial return can be negociated from the start with participation in the European projects. There is of course the risk that the European mm-submm array does not materialize, but the risk is small as everyone is convinced that the next big projects for astronomy in Europe will be in radioastronomy. Sooner or later there will be a European interferometer and also presumably a large cm-dm interferometer, and it would be regrettable that Netherlands stays aside of even a single one of these two projects.

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Status: R

Aan: Ewine van Dishoeck
Frank Israel
Harm Habing
George Miley
Harvey Butcher
Ed van den Heuvel

Van: Wilfried Boland

2 januari 1996

In aanvuling op mijn mail van 22 december j.l. stuur ik jullie hierbij nog twee refereerapporten toe met betrekking het voorstel inzake een Nederlandse deelname in de MMA. Hiermee hebben alle referees geantwoord.

Graag wil ik het tijdschema realiseren zoals uiteengezet in mijn mail van 22 december j.l.

De beste wesnen voor 1996,

Wilfried Boland

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Review of proposal for Dutch participation in a large millimeter array

reviewer: xxxxxxxxxxxx

Overall scientific rating: excellent

Impact on Dutch astronomy: excellent

Overall urgency: very urgent

Political judgement: excellent

1. Scientific Merits
a. Does the proposal has a clear goal?
Yes.

b. Is this an important goal?
Yes.

c. Can the goal be achieved through the proposed participation?
Yes.

d. Can the goal better be achieved another way?
No.

2. Significance to astronomy as a whole
How does the result from the proposed participation impact on Dutch astronomy?

The Netherlands has a long histry in radio astronomy and has made outstanding contributions to many fields of astronomy. Dutch astronomy made step from the 25-m Dwingeloo radio telescope to the WSRT, and then extended to participations in JCMT submillimeter telescope and space astronomy projects like ISO. The very large collecting area and very high spatial resolution at millimeter wavelengths would be the natural next step to be challenged. It is very timely
opportuinity for Dutch astronomers to participate in one of the large millimeter
array projects from the very beginning of its construction.

The scientific potential of the MMA is very high in that it address urgent questions in modern astrophysics, such as the galaxy formations, the structure and evolution of galaxies, and the formation of stars and proto-planetary systems.
The ability of large arrays in millimeter and submillimeter wavelengths is widely
recognized to make significant contributions in addressing these questions. It
should be noted that molecular gas and thermal dust emission in proto-galaxies at
very high redshift can only be seen in the millimeter and submillimeter windows.
Subarcsec imaging is also essential, for example, in order to observe the protoplanetary disks in neaby star forming regions with a spatial resolution comparable to our solar system.

The technology required for the MMA is familier to the Dutch astronomy community,
and the expertise in WSRT and JCMT would enable Dutch astronomers to make outstanding contributions to many fields of technical development. The Netherlnads
has the reputation of world-leading instrumentation developments especially in low
noise receivers, high-speed digital correlation techniques and image processing techniques.

The Dutch participation in the MMA will definitely give a strong impact on the Dutch astronomy in the coming decade by providing Dutch astronomers with an almost
perfect complement to the large-scale mapping capabilities with the JCMT, and the
FIR spectroscopic observations to be obtained with the ISO, and the more distant
future with FIRST.

3. Competence of the team of proposers
Can you judge the track record of proposers, in all fields of astronomy?

The proposers have outstanding achievements in various fields of astronomy and will
be capable of leading the very successful project.

4. Budget
Is the requested budget justified in the proposal?

In view of the complexities of the MMA project and complimentarity with other important facilities, the 10 % participation in the MMA seems to me a very reasonable
trade off between the scientific gain and the required investment.

5. Timing
Is there a clear description how the goals of the proposal can be met? Is the schedule realistic?

Yes. It is very timely opportuinity for Dutch astronomers to participate in the US
MMA project from the very beginning of its construction. I recommend very strongly
that funds be made available for the Dutch participation in the MMA to maintain the
Dutch astronomy at the forefront of radio astronomy in the next century.

Considering that the field of millimeter astronomy is very competitive, Dutch participation in the MMA should be promoted with the greatest urgency.

6. International partnership
Is the proposed participation justified in the context of other international developments withrespect to large millimeter arrays?

A long term plan for large millimeter array (MMI10000) with a very large collecting area is being considered in Europe. However, because of the very large scale of the planned facility, it is unclear whether the construction funds can be found within Europe. Even if ESO choose the MMI10000 as the first priority for the next European large scale astronomy facility, the completion would be after 2010. In view of these difficulties in the European option, it would be very reasonable for Dutch astronomers to consider seriously the participation in the US MMA project. Furthermore, it would be very good opportunity for Dutch astronomers to facilitate the early access to the observational and student tranining program provided by California merged array (CMA).

In Japan, there is a similar plan to construct the Large Millimeter and Submillimeter Array (LMSA) proposed by Nobeyama Radio Observatory (NRO) which will consist of 50 10-m antennas distributed in a 2 km area. Considering the similarity in the proposed facility and in the anticipated timelines for both projects, NRO and NRAO have been discussing various possibilities of bi-lateral collaborations between MMA and LMSA since 1994. If the Netherlnads decides to participate in the MMA, although the Dutch participation in the MMA is under the umbrella of NRAO, multi-national aspects in the international partnership should be addressed. Dutch astronomers are expected to join the discussion to establish more productive international collaborations.

7. US MMA proposal
Could you give your personal judgement of the US MMA proposal independent of the proposed Netherlands' participation. Will we MMA make a major scientific impact? Which field inastronomy will benefit significantly?

I recognize that the MMA project is ambitious and represent the natural successor to the present generation of millimeter and submillimeter facilities. The MMA will make a strong scientific impact on various fields of astronomy. The main fields which will benefit significantly from the MMA would be studies of early universe with large sample of high-z galaxies and the systematic studies of the formation and evolution of stars from cloud cores to proto-planetary systems.

8. Other comments you may have

It would be worthwhile to consider the Dutch involvement in the technical developments such as large digital correlator and high precision antenna design. I would also suggest the Dutch group develop the focal-plane array receiver for one of the MMA antenna to be used for the survey single-dish telescope. These technical activities promoted by Dutch astronomers would make the Dutch originality much more stronger.

wb0101 Fri Jan 5 12:03:07 1996 11

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Review of proposal for Dutch participation in a large millimetre array

from: xxxxxxxxxxxx

Overall scientific rating: Excellent

Impact on Dutch astronomy: Excellent (slight reservation here about size of the community involved - see below)

Overall urgency: Urgent (although probably not as urgent as indicated in the proposal)

Political judgement: I find it impossible to rate this as the situation is complex. In terms of the narrow context of Dutch astronomy, it may be a good choice: in the broader context of the development of European science it looks pretty bad. See 6. below.

1 Scientific Merits

a. Yes, the proposal does have clear scientific goals. These are very broad.

b. These goals are important. Large millimetre-wave arrays will, I believe, have a major impact on a wide range of very interesting astronomical topics. The proposal does an excellent job of setting out the current state of knowledge in these fields and indicating the glittering prospects that a large array will offer. In fact the nature of the field is such that the array will certainly open up areas which we are not yet able to imagine.

c. The goals can be achieved by the proposed collaboration in the US MMA project, provided of course that it is funded and construction goes ahead roughly as planned. As you are well aware, you will not be in control of those events, although your decision may have a considerable influence on the progress of the MMA and the European project as well as the merged California array. If you decide to go ahead with the US collaboration, it may be wise to consider also some contigency plans in case the full funding of the MMA is not forth-coming or is delayed for a long time.

11 contingencies

d. Realistically the goals can only be achieved by participation in a large international collaboration. As the proposal explains there are three projects presently under discussion. Although collaboration in the US project is the most obvious choice (in that NRAO have set out clear terms for participation which look attractive for the Dutch situation) the position is perhaps not quite as clear-cut as the proposal suggests. For example it does appear that the Japanese project may be quite close to obtaining approval and in principle their funding could start rather soon because the Subaru spending will start to decline in about 2 years from now. I also feel that the European efforts are dismissed too lightly as discussed more below.

2. Significance for Dutch astronomy as a whole

I can't say much here as I do not know the Dutch situation in detail, but generally I would have thought it is positive. Clearly the proposal only makes sense if more people move into the areas for which the millimetre array will have an impact and this presumably means less people working in other areas, but from my perspective the prospects are sufficiently exciting that this shift is a good thing.

11 need shift

3. Competence of team

This is a very strong team. I know them all and regard them very highly. For the main participants there is an excellent match between their interests and knowledge and the scientific content of the proposal.

I am more skeptical about the possible technical involvements that are outlined in the proposal. Although Dwingeloo could certainly play a major role in the correlator development and SRON could be involved in receivers, I note that: a) taking on a substantial part of the system would entail a major commitment of the very best people available stretching well into the operational phase of the project; b) it is not clear that NRAO will be willing to hand over substantial parts of the project to "Associates"; and c) I have not sensed great enthusiasm for working on the technical aspects of this project from those who I would imagine would be the ones involved.

4. Budget

I believe that the overall estimate of US$200M is fairly realistic and $20M for a 10% share therefore follows. NRAO have a good record of bringing projects in on budget.

What is unclear is how the proposers hope to get both a significant role in the combined California array and be Associate members of the MMA for $20M. The existing investment in the California telescopes is large so they can be expected to want a considerable contribution in exchange for access. At present NRAO have set 10% as the minimum share for Associates. The proposal suggests that this can be solved by negotiation, but I am concerned that this may prove optimistic. They should explain what they have in mind.

not so too much detail

5. Timing

As usual the timeline in the proposal is the fastest conceivable under ideal circumstances. I would be surprised if things happenned as quickly as this. Given the tight budgets in the US, delays seem almost inevitable, especially at the key decision point which will be approval for full construction (hoped for in 1998 or 1999). Access to land and other resources in Chile cannot be taken for granted either. This is however not a major criticism, more a comment on the way these things usually go these days. I would anticipate that some funding for development work will be forthcoming, especially if Dutch involvement has been agreed, and this will be vital for building the momentum of the project.

The prospects for progress on the combined California array are unclear to me. In particular I do not know how committed the Berkeley group are to this as they are still heavily involved in further development of the Hat Creek array.

!!

6. International partnership

The current situation, with three competing proposals, is frankly a mess. My belief is that it would be hard to justify building three such arrays, although there might just be a case for having two - one in each hemisphere. I feel, however, that the best solution would be for all the available resources to be combined to build a single really outstanding instrument. The proposal pays lip service to this possibility in section 4.4 by talking about the possibility of expanding the MMA in the more distant future. The problem with this is that the MMA as presently defined would be very difficult to expand - adding more dishes to an array of 40 is not an efficient way of increasing collecting area and it seems unrealistic to imagine starting again with bigger antennas.

!! yes

If this proposal for Dutch participation as an individual Associate in the MMA is approved, it will of course strengthen the propects of the MMA going ahead in its present form. I suspect that this will eventually kill off any significant independent European effort in this area, which will be a great pity given the strong position that we have established with the combination of the IRAM 30 m and interferometer and the JCMT. (The proposal is, by the way, quite unfairly negative about the capabilities and prospects of the Plateau de Bures array). I would therefore have preferred a more unified European approach - perhaps with a group of countries working together as a partner for the US, say at the 30% level that the NSF requires initially, but with the goal of ensuring that further expansion is feasible if and when resources become available and the science proves to be as exciting as I expect. I believe it is not to late to set up such an arrangement and I know that there are many in the European community who would support it. I would urge you and your colleagues to examine whether there are ways in which the very impressive resources and influence that you have available might be used towards that sort of an outcome.

?

?

7. The MMA proposal

I have no doubt that the MMA as presently proposed would be an extremely powerful instrument and will have a very large impact in many areas of astronomy, exactly as described in the Dutch proposal. With 40 antennas its imaging capabilities will be particularly impressive.

I do however have concerns about the MMA's sensitivity. It is misleading to compare it to the present day performance of the existing arrays because most

of the gain is due to predicted enhancements in receiver performance and bandwidth which will presumably be applied to the other arrays too. Because the antennas are rather small the increase in collecting area is actually rather modest and it is doubtful that it represent a large enough advance given the high cost. I also question the strong emphasis now being given to the submillimetre performance at the expense of the range 100 to 300 GHz, where I believe the science will be most interesting, and am concerned about the very high altitude of the Chilean site now being favoured (which is in fact somewhat higher and also smaller than stated in the Dutch proposal).

but site + quality also

As I am sure you are aware, these concerns are shared by a number of colleagues, especially in Europe. We have passed them on to the MMA team (attached is a copy of a note I sent to the recent meeting in Tucson). I understand that these points were discussed very openly at that meeting and the participants generally felt that they were satisfactorily answered, but unfortunately I was not able to attend and have not yet heard enough of the detailed arguments to be fully convinced myself. No doubt a lot of the differences arise from the relative emphasis one puts on galactic versus extra-galactic work and on imaging very large fields versus working at the very highest resolution possible.

give overview

8. Other comments

Naturally I am somewhat concerned about the impact of this on Dutch participation in JCMT, but my general view is that we (JCMT) will have to come to terms with the new instruments which are developed for these wavebands in any case.

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Note sent to Bob Brown at NRAO for the Tucson meeting 5 - 7th Oct 1995

Some thoughts on the MMA Specifications

Since I can't be at the meeting I thought I might put down a few thoughts. These may be based on out-of-date information about what the current plan is and they are certainly biased in the sense that I don't know much about mosaicing. I am taking the starting point as 40 dishes of 8 metre diameter and 25 micron surface accuracy and one beam per telescope on a 5000 metre altitude site. In that case:

1) I think the COLLECTING AREA IS TOO SMALL. This can be seen:

a) in terms of the ability to detect faint objects - both at high redshift and for example in looking at protostars in the molecular ring of our galaxy. I am sure Denis Downes will hammer home the point about how small an increment in collecting the MMA represents compared to the IRAM telescopes;

b) just by noting that the basic idea of this instrument is to give an order of magnitude improvement in angular resolution over the existing arrays. Since the existing arrays are very often limited in what they can do by lack of brightness temperature sensitivity, the implication is that we need to increase the collecting area by TWO orders of magnitude (in order to maintain the same filling factor). Clearly this is not really possible - some of the improved sensitivity will have to come from better receivers, more bandwidth and better atmospheric transmission, but it does bring home the need to maximize the area.

Given that the total funding is fixed, I believe that the way to solve this is primarily by increasing the size of the individual elements. It is crucial to get this right because, unlike the receivers, correlators and even the number of dishes, the dish diameter cannot be changed once the project is underway. I think that the optimisation of the dish size needs to be done again: a) taking account of current scientific goals; b) using modern costings for antennas and other parts of the system; and c) taking into account the option of using clusters of feeds in the receivers, so that one can observe multiple fields simultaneously.

(I have't seen a detailed analysis of this but obviously the additional cost of putting 4 or even 7 feeds in the focal plane of a millimetre receiver is very small compared building complete systems - antenna plus receiver. I note that many of the projects which require large fields only need modest bandwidths so that one would not have to multiply up all of the backend or even the IF systems. I think it is also true that, for modest numbers of feeds, shaping of the antennas for high efficiency would still be possible.)

Additional advantages of reducing the number of dishes but making them substantially larger are that:

a) construction and operation of the facility are simplified - more of the cost is tied up in simple things like dish structure and panels and less in receivers, drives, encoders and correlators;

b) it will make it more realistic to expand the array if more money becomes available later, e.g. if other countries are able to join.

Apart from the reduction in mapping performance for large fields (which should be partially answered by the use of multiple feeds) the most obvious disadvantage is pointing especially on a windy site. This brings me to my other main point:

2) I think THE 5000 METRE SITE IS TOO HIGH. Despite what the people who have been to visit it say, I am sure that in reality the problems of operating at such a height will add very significantly to the cost of the project and reduce the effectiveness of the facility. I have spent a lot of time on Mauna Kea (well over a year in total by now!) trying to do difficult things and seeing the effects of that altitude on my own and other people's thought processes, efficiency and morale. It is well known that the gradient in the effects of altitude is steep in this range and there is no doubt that the problems will be more severe at 5000 metres. I would myself also be seriously concerned about sending people to work for long periods on such a site without knowing a great deal more about the possible effects on their health.

accessibility

I do understand that the atmospheric conditions are superb but I do not believe that the additional gain by comparison with a site in the same neighbourhood at say 3500 to 4000 metres can be justified unless the major objective of the project is to do astronomy at wavelengths shorter than 800 microns. Although there is no doubt that there is some tremendously exciting science to be done at the short wavelengths (and that is where I work myself) I think the regions between 1 and 3 mm is even more important and a push for the submm wavebands will inevitably compromise that. On a lower site submm observations would in any case not be ruled out.

>From what I have seen of the maps and pictures, I am confident that somewhat lower sites are available in the Atacama region and I strongly suspect that it will be possible to find one with rather more shelter so that the typical wind speeds are lower too.

I do hope the meeting goes well and I look forward to hearing the what comes out of it.

Comments on Referee's Reports on:

Dutch Millimeter Array Proposal

We are pleased that all five referees consider the scientific merits of the proposal very good, and appreciate their constructive comments. In the following we briefly address the main issues the referees raised in response to the questions from NWO. We refer to the referees as nr. 1-5, with nr. 1=American, nr. 2=German, nr. 3=French, nr. 4=Japanese, nr. 5=English.

International partnership

All referees agree that the proposed scientific goals will be reached through participation in the US MMA, but referees 3 and 5 suggest that the same goals could also be obtained in a European collaboration.

As explained in the proposal, our main motivation for favoring participation in the MMA now is the time scale. It has been clear for some time that a major European effort requires significant involvement of the European Southern Observatory (ESO). Such an involvement is feasible only when the present VLT/VLTI funding level starts to decrease. This is currently expected to occur in 2002 but it may slip to as late as 2005. In addition, it is presently not at all certain that ESO would choose a millimeter interferometer as its next big project. Thus, a major European array might become available at the earliest 5 years after the MMA, possibly 10 years later. The European millimeter interferometer will truly be a "next generation" instrument. The experience and scientific results obtained with the MMA will be essential to define the scientific goals and technical design of such a major project.

The SMA submillimeter interferometer on Mauna Kea is briefly discussed in Section 4.3 of the proposal. Its small collecting area (226 m2) could be increased to 480 m2 for part of the time by adding the JCMT and CSO. Assuming that this will be the case for ~40% of the nights and that the JCMT partners would have a ~35% share (cf. the JCMT collecting area), we would receive ~8 (8-hour) shifts per semester (at the cost of 20 shifts single dish-time) based on our 20% JCMT share. Mauna Kea weather statistics indicate that only half of this time is good enough for observations at 350 GHz or higher, where the SMA will outperform existing millimeter interferometers. Thus, Dutch scientists will have only ~4 shifts per semester available for high frequency interferometry. This will allow some Dutch astronomers to carry out a few unique projects per year, but is totally inadequate to serve the needs of the community as a whole. Furthermore, it appears that the MMA will contain a submillimeter channel from the start (see below). If so, its 4-10 times larger collecting area and better site will make it far superior to the SMA(+JCMT+CSO). Note also that we would not consider withdrawing from the JCMT before the MMA is operative, i.e. after 2006, contrary to what is stated by referee 3.

The US MMA project and its sensitivity

Referees 3 and 5 comment that the MMA project may not be ambitious enough, given the recent extension of existing interferometers, in particular IRAM.

The MMA project, defined over the period 1985-1990, has been reviewed continuously, most recently through a science workshop in October 1995 in Tucson. The twin objectives of this meeting were to update the scientific goals and compare those to the strawman design developed by the technical working groups. The workshop was attended by nearly 100 people, including several Dutch participants. A summary of the workshop and the recommendations of the Millimeter Array Advisory Committee are attached to this reply. The most salient new recommendations are that the MMA is outfitted from the start with a submillimeter band and with outrigger stations to allow resolution significantly better than 0.1". The continuum sensitivity of ~0.6 K in 6 hrs is high enough to allow observations of thermal dust emission at a resolution of 0.02" (3 AU in the nearest star-forming regions). Both features imply a major step forward compared with existing facilities or planned expansions.

The message of referee 5 to NRAO was discussed extensively at the workshop by all five panels. All groups concluded that the MMA presents a major step forward in their research area. In terms of imaging speed and quality, the MMA will be nearly two orders of magnitude better than any existing instrument.

The MMA collecting area is indeed only a factor two more than that of the IRAM 6-element interferometer, but the high surface accuracy of the dishes and the excellent site lead to a total increase by at least a factor of 3-6 at 80-260 GHz. This comparison assumes that the existing facilities will be upgraded with similarly sensitive receivers and large bandwidth backends; if not, the MMA gain will be even larger. At higher frequencies (>300 GHz), the MMA will be unrivalled. Especially for dust continuum observations, the gain compared with lower frequency observations can easily amount to an additional factor of 10. We note that there are many projects of interest to Dutch scientists which are not limited by sensitivity, and will greatly benefit from the snapshot mode and rapid imaging possible with the MMA.

We also note that historically, factors of 4 improvement in collecting area have led to significant new science. Examples are the WSRT compared to the One-Mile interferometer (factor 4), the VLA compared to the WSRT (factor 2) and the Keck 10m vs the Palomar 5m optical telescope (factor 4).

We agree with referees 2, 3 and 5 that eventual joining of the European and/or Japanese efforts with the MMA to obtain an instrument with a much larger collecting would be highly desirable. The MMA Advisory Committee has already asked NRAO to reconsider the optimization between the number and size of the dishes, including both cost, scientific factors, and the possibility for future expansion. The Dutch are committed to help develop the long term European plans through the IRAM-OSO-ESO-NFRA technical study and through our membership of ESO and indeed active participation in the ESO mm Advisory Group; we have no intention of isolating ourselves from the European effort.

The high altitude of the proposed MMA site in Chile is scientifically very attractive, but may pose practical problems, as noted by several referees. This problem is recognised also by the MMA community and medical advice is being sought. There are several alternative plateaus in Northern Chile being studied at somewhat lower elevation, which are also promising in terms of conditions. We note, however, that there have been mining operations for decades in Chile at even higher elevations.

Technical involvement

Referees 2-5 note (in response to questions 2, 2, 8 and 3, respectively) that the current proposal does not express a clear desire for technical involvement of Dwingeloo and SRON in the MMA project in spite of strong competences. Informal discussions with NRAO staff have made clear that many aspects of the technical design of the MMA have been extensively researched and planned during preparation of the proposal to NSF for financing. A role for one or more Dutch groups in the MMA technical development is therefore subject to negotiation between the parties. These discussions will be circumscribed by existing long term commitments and plans at the several Dutch technical laboratories, by the limited manufacturing capabilities of those labs (mostly one-off prototype) unsuited for the mass production required for the MMA, as well as by the degree to which additional investment funding can be obtained to optimize local infrastructure specifically for MMA development work. Nevertheless, it remains our intention to seriously explore possibilities for effective technical contributions both by Dutch technical laboratories and by Dutch industry. Such contribution has, for example, recently been organized as part of the ESO VLT instrumentation program and should similarly be possible as part of our participation in the MMA project.

January 9, 1996

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