VII.

SUMMARY AND DISCUSSION

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The session started with a summary of the conference by N. Woolf. Then each panel member presented his views for about 5 minutes and the discussion was open to all participants.

The panel members were G. Burbidge (Chairman), D. Dravins, P. Léna, J.P. Swings, H. van der Laan and J. Wampler.

The main topics discussed were sites, costs, remote operations, choice of telescope configuration, and interferometry.

1 - Summary (N. Woolf)

At first, when I was asked to give a summary of the meeting, I felt delighted and complimented, but then I started to realize that usually the person who gives a summary at the end of a meeting is someone who is so old that they don't understand what was said. And I started to have second thoughts. I was concerned that, if I attempted to tell you what went on in each paper, I would expose my ignorance and demonstrate that I, too, had reached that stage - so I decided not to do that. But I had to rationalize it - I thought - well, after all - it is not fair to talk about these papers - they haven't been written yet and people still have a chance to get them right. So, I then started asking what had indeed happened at the meeting. There seemed to be a couple of questions that were so big that they hadn't even been addressed to the panel, questions such as "are large telescopes important?". Of course, first you look around you and you think of all the people you have talked to at this meeting who seem to think that large telescopes are important until you realize that, of course, people who don't think that large telescopes are important didn't come to this meeting. So you have to ask whether there is any other reason for believing that large telescopes are indeed important and I think we have seen it at this meeting, particularly in the session this morning. We saw four national and international projects discussed this morning, but those might well have been produced by the countries for reasons of chauvinism and might have nothing to do with the value of large telescopes, and so that didn't mean anything. But there were also a group of, you might almost say, anarchists, people struggling to get in from some other direction into large telescopes, and I think that does say something: it means that our people, who could put their effort in a variety of directions, have instead decided that large telescopes are the way to go because these really are going to bring more interesting work than almost anything else that could be done.

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Well, then, if indeed the science does justify large telescopes, how ought we to make them? That was indeed quite a large part of the discussion at this meeting. Some of the thoughts that I've had, really never seemed to surface at all, such as the thought that we've spent the last thirty years scaling down the 200 inch and we have reasons to be modest about our success. Dr. Oke showed us some CCD pictures, with 7/10th of an arcsecond resolution and most of our telescopes rarely exceed, or sometimes even reach that level of performance. We might think that we should be optimistic because, indeed, for three facilities, there has been an attempt to understand and improve internal seeing and there have been dramatic improvements in the image size. Alternatively there are reasons for being pessimistic because currently at most of the facilities the astronomers wouldn't even know the difference if the entire atmosphere were removed from 10 meters above the dome.

Now, I started off to discuss how ought we to make large telescopes. When the 200 inch project started, George Ellery Hale wrote an article which was provocative, and was meant to be provocative, in which he said that the 200 inch telescope was not only going to be the largest telescope that was made up till that time, but was going to be the largest telescope that would ever be made. And he had some reasons behind him that were sufficient to justify that at that time. We have, indeed, been having a lot of difficulty getting back up to that level. But it did stimulate some people into thinking, and in 1930, E.H. Synge in a most stimulating article in Phil Mag[a] suggested that one way of getting to even larger telescopes was a multiple use of co-aligned, co-phased telescopes, each with its own axis, in other words what we now call an MMT. And so, one of the approaches is to somehow combine the use of separate telescopes. We have seen a number of efforts that have pointed in that direction by way of creating single mirrors to be used that way. For example, there has been the thin mirror approach of Texas, there has been an aluminum mirror approach of ESO, and a borosilicate honeycomb mirror approach of Angel. Five years after Synge, Horn-d'Arturo[b] from Italy suggested alternately that there was an interesting approach of segmented mirrors around a single optical axis, in a kind of Arecibo mount, and we have seen from Penn State that indeed that is still a very attractive proposition. We have seen a very elegant variation on it developed by the University of California at Berkeley and it has indeed stimulated a large part of the effort on the US NNTT project. So, we have some way of getting towards these giant telescopes. Joe Wampler began this meeting by discussing the three kinds of new technology that we might get into: essentially, where we merely borrowed technology from somewhere else is one, where we borrowed

Ref: (a) Phil. Mag., 1930, 10, p.353, (b) Sky and Telescope, 1978, 55, p.100

it with minimal augmentation is another, and where we had to get into something pretty new as the third. And, unfortunately, it seems that the approach towards very large telescopes almost guarantees that we have to get into this third and most difficult kind of technology and that is perhaps the reason why we are being somewhat delayed in this process of creating the telescopes. We've also been delayed because we haven't quite understood why we were building these telescopes, what they had to do, and we have along the route discovered that certain kinds of work almost create the nature of the telescope. For example, the considerations of optical spectroscopy only have led Penn State to its particular route, a very sensible one. Adaptive imaging forces people to put a lot of aperture rather close together. Interferometric imaging at high resolution persuades people to move their apertures apart. Those are some of the things that have a very strong influence on the device and there are others that have a weaker influence, such as the decision to do multiple object spectroscopy which seems to have created a great deal of enthusiasm in the community, but most facilities seem to be able to cope with that so that it hasn't been a very great driver. The particular choice of adaptive imaging on one hand, that I mentioned, and interferometric imaging on the other, seems to have been related to something else. Now, when we talk about a versatile system, we're asking whether four different people can use it for different things at one time and then sometime later somebody else can come along and use it all together. And indeed the choices that we made about the kinds of highest resolution imaging reflect very strongly on that. If all the aperture is jammed very closely together, then we are liable to end up with a device that can only be used by one astronomer at a time. On the other hand, if we want that interferometric imaging instead, we push the telescopes apart and it becomes relatively easy to consider them used separately.

Each of the groups that has to create its facility has two problems: it's got a group of customers or users and it's got a different group of people that funds the project, and somehow each group has to weave its way between those two processes to come up with a telescope - because the ultimate thing is that it doesn't matter whether you've got the right concept if it doesn't end up with a telescope. It is more important to have <u>a</u> telescope than none, and therefore you have to somehow weave between the community and the financial support. And we have to accept that that may mean different things to the different groups, and it may fortunately mean that there is going to be a great variety in the kind of VLTs that we will end up with, and so they will do many different things and they will each tend to be attracted to the things they do separately and well. Of course, there are these questions,

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such as what kind of angular resolution imaging is going to end up with the most exciting results, and there it's very hard to have anything much but bias, and my bias is towards at least a factor of ten increase in angular resolution. But I recognize it's bias and bias brings me to one of the key words that I would like to talk about: bias and fear and leadership. First of all we've seen a lot of bias at the meeting. Some of it is bias towards conservatism, some of it is bias towards radicalism. The main thing I want to say about bias is that it is no substitute for calculation. A calculation is crucially important, and we, who are used to astrophysical calculations, have to certainly wake up to the fact that we are often now asked to make a calculation to a precision of a factor of two or one and a half, and sometimes even to precisions of 5 or 10 percent!

On fear: it's a good servant and a poor master. If I'm afraid of operating 1000 CCDs at the same time, and Jerry Nelson isn't, there is a strong prospect that I'll get left behind and he will make the interesting scientific discoveries. On the other hand, if I'm afraid that 1000 CCD's may be unmanagable, and I start off by calculating whether I can get hold of enough computer power to handle them, whether I can get hold of enough money to handle them, to deal with the mass production processes that will be involved in using them, and so on, then fear has become a servant, and, when it can be done, then, I will be able to be in the front doing it. Equally, that kind of fear should drive me to take a most careful look at the photographic plate. Its effective use will probably outlast me, and it may even outlast our youngest participants at this meeting.

I now come on to the topic of leadership: both of the biggest projects, the ESO VLT and the US NNTT, have for some time been in dead center. In the US, NNTT has for a number of years been caught like Buridan's ass midway between two equally attractive bales of hay - you may remember that Buridan's ass died of starvation. Further, both bales of hay have improved with maturity and even more from an influx of funds. Fortunately now, both concepts for NNTT are receiving a clean bill of health for both their technology and their scientific potential. The various residual factors needed to be weighed up - differences in the science they can do, residual technological doubts, costs, any political considerations that might make it easier to fund one than the other - must be resolved rapidly, and leadership used to set the US community moving in one direction. It will take time for the community to jell behind one concept Leadership has to make a decision, even if it comes to tossing a coin: it will be more important to have a decision than no decision(1).

I have one further comment about NNTT and that is that NNTT has become a project because it was someone's baby - it was Geoffrey Burbidge's baby. He has nurtured it; he is now sitting in the both uncomfortable and proud position of watching it stand on its feet and take its first strides away from him. We have to recognize that without his effort we wouldn't have a decent chance of getting it at all. And if I had a medal to give, the person I'd give it to would be Geoff.

Now for ESO, the merits of an array for serving its varied constituency are obvious, but the reasons for a particular array concept don't yet appear to have been properly calculated. There is quite a lot of work to be done in making that decision. I noticed at this meeting, that there were some very significant steps towards that decision, with Pierre Léna presenting a flow chart and an analysis of interferometry in the infrared. ESO's reasons for needing a decision are as acute as the USA's, but they're different. The rolling mountain chains of Chile provide very few isolated sites, and the precise site is likely to be defined by the particular acreage needs of the array. Unless the concept is defined soon, bias about sites will dominate and the site bias would choose the array and science to be done, rather than having the science and technology choose it. It is therefore very important to move rapidly to a conclusion as to what is the optimum array.

In looking at the national and multinational projects, and comparing them with the anarchists' projects - the separate little things that are all over the United States - we must be aware that these kinds of projects face very different dangers: the national and international projects face the danger that their telescopes will be underambitious and overfinanced, making up in grandiosity for what they lack in capability. On the other hand, the anarchists are overambitious and underfinanced and they may achieve very successful very large telescopes or, at the last moment, the chewing gum that holds them together may give way. So, both sides have to be aware that they face those problems, try to recognize their own limitations and, within the range of possibilities, cope with them. So, if they do, we may end up with a much better generation of large telescopes.

Our talks here have been very wide ranging; we've had talks from speakers from at least ten countries and comments from speakers from half a dozen more. Our hosts are to be congratulated for having brought us together in

(1) This decision was made in July 1984 (Ed.)

this timely way. The meetings have run extraordinarily smoothly. Our program was very well planned: it moved from our technological options through the scientific potential that technology created, to the attempts that we are making to use that technology now. We'll have a lot to reflect on when we return home.

2 - Brief Comments by Panel Members

D. Dravins

I think, to summarize what I find the most important thing, that a future very large telescope must be an order of magnitude improvement and not just a step of fifty or seventy percent. There are two points, as a consequence of that, that I ought to take up in these minutes. The first one is a choice of focus configuration which has been discussed somewhat at this meeting, and the other point is the type of site to be chosen which has not been discussed very much at this meeting. The first point which has been raised is that unique instrumentation usually comes in one piece (that was mentioned in some earlier comment). I would say that it doesn't even come in one piece, it usually comes in many small pieces and doesn't even work very well. Let us imagine a future when there are two very large telescopes operated by different groups, telescope A and telescope B. Telescope A has one single focus, while telescope B has ten different foci. There is some ingenious astronomer who has built a small machine for, for example, measuring the scattering of light in a 5 milli-arcsecond slit at the inner part of a dust shell condensing around a super giant and he has this special resolution to do this. So he applies for observing time. At observatory A he is granted ten hours; at observatory B he is informed that his application cannot be considered until he has produced 6 or 10 identical units of his machine. Now, obviously, he will choose observatory A and that is where he will make his discovery. Even if he himself would be of the opinion that it would be better to have a series of similar instruments, his funding authorities will never do that. At observatory B, instead, there will be a successful ticking away, night after night, of the ten identical photometers doing the grand statistical survey of objects of whatever type they will be. But they will be very different types of discovery from those that can be made if one has the possibility of explorative spearheading possible at one focus position.

The other point is the choice of the observatory site. Some sites have been discussed here: there was mention of Mount Graham, of Paranal, of various other sites in North America and elsewhere. These sites are somewhat better than existing ones - they are somewhat darker, they are somewhat higher,

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they are somewhat drier - but they're not really all that different, they are just slightly better. They do not represent, in particular, any order of magnitude improvement over existing sites. Now, is it possible to get such an improvement? Well, one site, of course, can give you that: if you go to space. Now that is not realistic in this context, but maybe there can be some other alternatives that give radically improved conditions. I'm aware of one site which could be an example of such classes of sites: it is one of those that is being considered by the ESO team as a conceivable VLT site. It has not been continuously monitored, I understand that it has been visited about once a month during the last year or so (could I have the first slide, please). This is in northern central Chile. From the costal city of Antofagasta you go east, you reach the oasis city of Calama and the township of San Pedro de Atacama. This is looking east towards the Bolivian border. To the right is the beautiful, conically shaped mountain of Licancabur which is defining the border between Chile and Bolivia. You can see some white things on the ground there: that is not snow, that is salt, because this is the driest desert on the surface of Earth. As the photographer stands, behind you it extends for a few hundred kilometers all the way to the cold ocean with the cool Humboldt current in the Pacific. We're now at about 2500 or maybe 2800 meter altitude which is the level of the Atacama plane. To the north (next slide) are a number of peaks, such as this one, that have been investigated. The altitude up here in the high Atacama is about 5600-5800 meters. Up there, one is standing 1.5 kilometer higher than the peak of Mauna Kea, one is three kilometers above sites such as La Silla or Tololo. I understand that the photometric quality of these sites is at least as good as on the present Chilean sites. There had been water vapour measurements made there and at least on some occasions the amount of precipitable water vapour has been only a few tenths of a millimeter, or about an order of magnitude better than it is at the presently best sites, such as Hawaii. (Can I have the previous slide back, please.) Of course, that is not very surprising to have such dry sites when you consider the general geographical environment. Hawaii, for comparison, is a rather low island situated in the warm tropical ocean, where there are rain forests and beautiful waterfalls, whereas here it is extremely dry. To the right is Licancabur, an even higher peak (6100 meters). It may be sobering to think that at the very summit of that peak an Inca shrine was built about 500 years ago. Well, they, in those days did not have access to those kinds of instruments and helpers that can run a big instrumentation. I'm thinking of a staff for such a site of maybe thirty individuals, of which maybe two or three can be humans and the rest can be mobile industrial robots, the way a modern factory would be designed

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today. Of course, nobody knows exactly what the quality of these sites are, and it is not known how practical operation could be at these altitudes but I think these should be given serious consideration.

H. Van der Laan

As most of you know I'm an astronomer and an astronomy manager and I have listened with delight to the fantastic activity in technical physics and engineering that is going on in the area of telescope building. It is clear that after the launch of ST there is a certain mismatch between what we do on the ground and what we can do in space, in the sense that the 4m class telescopes which are now the best that we have at our disposal have really lagged behind. I suspect strongly that partly due to the oil crisis of the early seventies we now threaten to make the jump from 4m class telescopes to 16m class telescopes, and that we haven't had, say, any 8m class telescopes in the interim period. And I must say that I'm rather worried about the risks inherent in such a great leap forward. I would have much preferred to have seen develop a class of 8m telescopes, well engineered, with a lot of experience from which the next big jump, which will determine the astronomy facilities on the ground for a long time, were to be made. But what threatens to happen now, and I think because of the public relations and the public funding that may be inevitable, is that in the United States and in Europe we will have the more or less simulatenous development of 8m class facilities and 16m class facilities. I don't think that's an optimum, in many ways, but perhaps it's now inevitable. I mean you can see in the U.S. the California and Texas projects proceeding right along with the 16m class national telescope. You can imagine in Europe a La Palma consortium for an 8m telescope right along with ESO to build the 16m telescope. But that's just my point of discussion, that may not be the best possible configuration, but perhaps that's now inevitable, given in a certain sense a standstill in aperture size for the last 15 or 20 years.

My next remark concerns observing modes and management of observing times and their allocation. It's a very important topic for the next generation telescope; it has not really been addressed at this meeting, but I just want to remind you of the fact that it will become inevitable that optical astronomy allocate time completely differently than it has done hitherto. The fatalism of weather has to be eliminated completely and the folkloristic tales about beautiful seeing are rather like tales of fishermen about the big fish which always get away. Good seeing has to be used and not just talked about and one has to manage one's telescope in such a way that it can be exploited to optimum advantage. That is one reason, why I think that

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absentee observing will become inevitable, and that, in the future, these very large expensive facilities will have at least two kinds of time allocations: priority 1 and priority 2. Priority 1 allocations will be guaranteed to be executed, barring disasters, under all circumstances. Priority 2 allocations will be executed if possible and feasible, and if not they'll be removed from the menu and have to be reapplied for. Then you can make, quarterly schedules for your instruments, for your telescopes, which are weather dependent. You can then optimize the schedule by writing some fairly complex computer programs with possibilities of on-site human intervention to utilize your telescope to best advantage. There is no point for astronomers then to travel from Europe to La Silla or for people, I think, to travel from the Continental United States to Mauna Kea, and, I might add, from Europe to Mauna Kea and from the U.S. to the European Chilean observatory, because I strongly believe that these facilities must be open to the worldwide community on the basis of scientific merit. A good example has now for a long time been given by U.S. national observatories and I am a strong advocate of having that sort of openness and generosity at all international or multinational facilities. So, of course, people talk a lot about remote observing nowadays, and I think we have to realize that remote observing has a whole spectrum. One extreme is absentee observing, the other one is total telescope control remote from the telescope itself. And you can think of many situations in between: one simple one being just voice contacts by telephone, the other one being the complete simulated control room. In my own view, the complete simulation of the control room is not advisable. It is much better, in my opinion, to think much more close to the other end of the scale of remote observing by means of absentee observing with fairly straightforward simple interaction between the astronomer and the team on the mountain that takes the decision and that optimizes the telescope depending on local prevailing conditions. Perhaps, that's a topic we could also briefly address.

My third point is that the choice of array versus multi mirror telescope, I find it very difficult to make. I would want to choose on the basis of some very detailed operational models involving realistic menus of programs to see all the different aspects that are involved in this choice. I am convinced, however, that optical interferometry is so far from a substantial utilization of the big apertures that it cannot determine that choice. Moreover, given the inevitable sensitivity limitations of optical interferometry on the ground, I doubt whether it is justified to spend even a fraction of the VLT investments on optical interferometry on the ground, and I have not seen the astrophysical case made for its importance given that apparent magnitude limitation. I think it is urgent that the people who advocate that optical interferometry be the driving force of a VLT structure make the astrophysical case and not only the technical one. It may be very different for infrared, although, say, at 350 microns you can do infrared interferometry far more cheaply with specialized cheap dishes than you can do with a high precision optical telescope that we have talked about here. So, I'm not clear in my own mind on the choice between an MMT and, say, four 8m dishes in an array configuration. I can see it go one way in the U.S., possibly the other in Europe, but, as I say, I'm not prepared to make that choice now.

Finally a remark on costs - I can't really say anything new on costs - I mean, the investment seems to be somewhere between, say, 80 and 100 million dollars, depending in large part also on the current state of the site to be chosen. The operating costs seem to me very roughly to require a home base of about 100 man-years per year and a site manpower which will vary from about 50 to 100 depending on the site, its closeness and accessibility with respect to the homebase, and depending on the instrumental configuration and the operating mode, in terms of remote and absentee observing finally chosen. This means that the annual costs will be somewhere between 15 and about 30 million guilders, that's between 5 and 10 million dollars, depending on these uncertain factors that I have mentioned. In view of that situation, and the large operating costs that it could entail, we all have to be rather careful about the structure of finances and the way such an operating budget relates to operating budgets of existing observatories and might threaten the well-being of still good existing observatories. We have to be aware of that. No doubt, we'll have to throw out a lot of hobbies: I think we'll have to start in the 1990s closing down lots of mediocre telescopes in order to make manpower and funds available to operate state-of-the-art facilities. But each country, each consortium of countries, or the U.S. in the case of the one large national facility, have to carefully look at the structure of their astronomy budgets in order not to get one good thing at the expense of many other good things. At least, that has to be done in a carefully orchestrated fashion.

J.P. Swings

What I would like to say is that during the last three years some of the topics, or arguments, that were mentioned this morning have been discussed inside some of the ESO working groups and I would just like to remind you of some of the drivers of the ESO project. The idea, to begin with, was to have a, say, 16m diameter telescope to do the following things: low resolution spectroscopy of very faint objects; high resolution spectroscopy of intermediate magnitude objects; and also one of the main drivers was imaging in the infrared. This sets the lower limit of 8m for an individual dish to be able to get to the diffraction limit in the infrared. And, as a bonus, interferometry, especially interferometry in the infrared, was mentioned but it has, at least in 1982 and 1983, not been one of the main drivers of the whole project. So what was actually concluded after several committee meetings - it was not a decision but a recommendation by the scientific and technical committee of ESO at the end of 1983 - was that an array of several 8-10m telescopes was a reasonable, or good, choice, at least as seen in 1983. But we also wanted to remain fairly conservative, i.e. we chose 8-10m monoliths and not segmented mirrors. And one of the main things I should insist on is that one would want one of these 8m or 10m telescopes as soon as possible. I think this time argument is quite important. Now, of course, there has been quite a bit of discussion this morning. Daniel Enard has presented an ESO project for the first time in these conferences so it's like throwing a ball to the dogs: everybody jumps on it and barks, and tries to argue whether it is good or not, tries to eat it, and hopefully use it. I think that's all I can say at this moment. I'll come back to the upper limit and cost arguments later on.

J. Wampler

I think that astronomers in general, or optical astronomers in particular, are rather, how should I say, retiring in their requests for money. For instance, high energy physics has no practical applications in the real world, it's only pursued for pure knowledge, and yet high energy physicists at CERN and Fermi Lab, and places like that, spend about ten times as much money as ground-based astronomers. And I suspect that in the case of astronomy from space, which is very expensive, we get our money there for reasons other than pure science. Governments like to do spectacular space things, and so we get space astronomy money for reasons other than astronomy. Now, according to a former president of the University of California, astronomy was the only exciting physical science in which it was possible to make major advances, build major instruments, build the largest instrument on the Earth, with sums of money that were within the budget of a single university. This is another way of saying, I think, that ground-based astronomy is not expensive. It looks expensive to us because our salaries could not pay for such a large telescope, the way Galileo's salary paid for his telescope. But in the world of politics, the cost of one of these large telescopes is only a few jet fighter planes, and that's not very much money.

So we shouldn't be embarrassed about asking for money. I think that we must move ahead rather quickly or events will overtake us and we will have lost the opportunity for making the discoveries now. For instance, I talked to some cost managment people who asked me, how did I reckon the value of the science that a telescope produced; and I said, well, how can you set a figure on that? And they replied, well, it's very easy: you take the capital cost of the instrument, and then you take the operating cost and you ask how many papers a year that observatory produces. That gives you the value of every paper. If you don't build the telescope, you lose that money because you don't write those papers. Well, if you build it now, rather than later, then you have the advantage of not letting - I'm not quite sure what the economic term is - the money depreciate later on. So I think we should build things now. Sure you can argue that a hundred years from now the technology will be so far forward that we could do things much cheaper, but we would have lost a hundred years of science. And I think that the science is worthwhile and that we should go after it now.

I was asked to speak briefly about how the aperture diameter ought to be chosen. I decided in my own mind that apertures should be around ten meters, and by around ten meters I mean something between 6 meters and 20 meters. Around 10 meters is a reasonable match to the atmosphere. For low dispersion work one very quickly, with these types of apertures, runs into the atmospheric background. And then it's fairly easy to convince yourself that, if the cost of the telescope escalates as the cube of the aperture (or, perhaps, some other power of the aperture, but certainly more than the square of the aperture) and that if you are pushing very hard against the atmosphere, you would be far better off with a much smaller telescope in space, where the atmosphere is much quieter and the images are much better. If you're doing high resolution spectroscopy - a space telescope being, of course, diffraction limited - you can use smaller slits and your gratings are not so big. So the actual number that I got out assuming half arcsecond seeing was, I think, about 8 meters but, of course, if the atmospheric seeing is a little better, then you go to a little larger aperture; if it'a little worse, you go to a little smaller aperture. But essentially that's how I would determine the aperture. Another consideration, of course, is the infrared diffraction images: if the seeing is quite good in the optical it would be even better in the infrared. Telescopes with apertures of approximately 10 meters might be expected to be diffraction limited. Now it is very important that the telescope perform well, that it be very stable, that the optics be aligned, that they stay aligned, that the drives track well, that the wind doesn't blow the telescope around. You might just as well have a big image, as a small image wandering all over the place. I think it really is important to have very good seeing and a lot of compromises should be made in order to achieve good seeing. You should go to the best site you can find, you should design the telescope so that the optics will produce images that match the very best seeing. Take the sort of direct images that Dr. Oke showed here yesterday of these faint clusters of galaxies: one night of this sub-arcsecond seeing is probably worth a thousand photometric nights of 2 arcsecond seeing, because with 2 arcsecond seeing you wouldn't see anything on those CCD frames. And even if you integrated for a thousand nights it's unlikely you would be able to identify these clusters.

The last thing I want to briefly touch on are the infrared requirements. There has been some talk about observing in the daytime. I'm not an infrared specialist, I think one should talk to someone like Alan Moorwood - but it has generally been my impression that infrared astronomy is not done in the daytime very much any more. The sensitivities that one is able to achieve in the night are so good now that one just doesn't want the warm daytime sky, the scattered sunlight from the sky. Daytime background sky light is objectionable with the modern detectors. Finally, if one is doing infrared field work, I think that it is very difficult to properly mask the aperture, the pupil, of the telescope, because, of course, the pupil moves around as one moves around in the field, that is if you re-image this pupil. So if you're doing array work in the infrared it will be very, very hard to mask the pupil, and then it becomes very important that the telescope be designed in such a way that the pupil of the telescope be fairly uniform and that there is not a lot of warm metal in the beam of the telescope. It may happen, and I haven't seen any discussion of this, that in fact a segmented mirror telescope, despite the fact that it has a complicated structure between the mirrors that you might think would be difficult to mask, might be the best way to do wide field infrared work, because in principle you could chop the primary (you just wiggle all these little segments). That of course is the best way to do the chopping - it's not secondary chopping, but primary chopping. It would be interesting, at least to me, to have somebody study the possibility of a chopping primary.

P. Léna

It seems to me that it is the first conference where we can think that infrared astronomy has reached its age of maturity. The reason is that the IRAS survey is now completed: it has revealed a completely new sky; it has revealed a number of objects we can hardly dream of today; probably a large fraction of them deserves follow-up from the ground; some of this follow-up will bring up new phenomena that we hardly understand today, as the extraordinary emission of certain galaxies or fragmentation in clouds. This age of maturity has probably also consequences on the way the very large telescope will be operated in the future, on its sharing of time between visible and infrared - and I'd like to call optical the combination of those two wavelength ranges. I think at this point that future committees will have - I'm not pleading for chauvinism - really to make a strong scientific assessment of what is the relative fraction of time to be given to this new adult, the infrared field. A number of interesting things have come out in this conference with respect to the infrared. I would just like to remind you of a few of them. One point which struck me is the adaptive optics progress in terms of thinking and of likely configurations of telescopes. The infrared is clearly an ideal field to which to apply that, because of the extended atmospheric coherence cells at this wavelength, the larger time coherence in the atmopshere, and the larger isoplanetic field. So we can expect great progress in this direction. It has also been said that the limiting magnitude obtained in speckle work improves by a large power, about the fourth power, of the atmospheric stability of seeing and this is one reason, among others, to plead for a very flexible scheduling of the telescope. If such scheduling is achieved it may be that one hour of good seeing devoted to a particular high angular resolution single pupil program is worth one, two or three nights of average seeing, which could still be very good for spectroscopy.

Another satisfactory point was to see the very rapid progress of detector arrays. Maybe I'm getting a bit old now, but from the beginning of my career as an astronomer until today I have seen about 8 orders of magnitude improvement in detector sensitivity in the infrared. This is quite something. Hearing that array detectors are now only about an order of magnitude away from their fundamental thermodynamic limit is a great pleasure.

Finally, there comes this question of daytime, which is an important issue for a next generation telescope, because, of course, we are dealing here with a factor of two in total observing time. Three micron is about the wavelength where day or night becomes irrelevant with respect to sky emission. So there are easy questions there, like tracking and pointing stability. There are not so easy questions, like compatibility with active optics, if a bright star is needed to servo the surface of the primary and how to operate it in daytime. And, then, there are even more difficult questions, such as thermal mirror stability, foolproof sun avoidance, thermal control. Maybe the suggestion made at this conference to encircle the tube of the telescope with the floor of the dome and make a shroud which is thermally controlled, is a very interesting avenue to explore in order to solve in a positive way this problem of daytime observing.

Second, a few comments on interferometry. I don't like too much the word interferometry, because, after all, the process of making an image is per se an interferometric process. But it seems to become the commonly accepted name for a mode of operation of diluted aperture as soon as it is more diluted than some meters, so let's stick to it, at least for the time being. Interferometry is an appealing challenge and it's difficult. If one is an optimist, one will look at the first aspect of it and do it, and if one is pessimistic, one will look at the second aspect and try to leave it for the next generation of astronomers. I would rather follow the first approach, as it seems to me many people in this conference do. There is a significant change in general attitude to be noticed from conferences held, say, five years ago. The work of some pioneers in the field is indeed no stranger to this change. Interferometry may be the way through which the first proto planetary system will be observed through its near infrared emission. Even if this were the single goal of it, I think it would still deserve the amount of effort it requires. It's very much seeing dependent, but it calls not only for the usual seeing knowledge we have with a single telescope, but it calls for more. It calls for knowledge about the external scale of turbulence, which is a completely new factor, and its dependence on time and also on sites, if any. This is something which will have to be measured. It will require imagination to do it and systematic work. It will need the joining of efforts of our American colleagues and a few Europeans, both theoreticians and experimentalists, plus probably contributions from atmospheric physics people, to tackle this difficult problem. There is the question of wavelength. Interferometry is good from x microns to 30 microns and then, maybe to a lesser extent, to 350 microns. What is the value of x? What's the optimum value? Well, we have one scientific answer which is that for infrared per se x must be between 2 and 3 microns. Should we consider that, short of this, it's a fringe benefit - to use Antoine Labeyrie's humouristic, but very profound remark - or is it a must which is driven also by astrophysical considerations. This is a point which requires more thinking. Another point is the frequency coverage. How much of this can we compromise for cost or site reason? I would say, as little as possible, maybe none. That has also to be looked into very carefully, because it will set a standard for at least a generation of astronomers.

Finally, the costs. We heard from Dr. Humphries this very good formula which, if I recall it; is the cost for economic solution which is obviously what we are all after: $.3\ D^2$ in dollars - million, excuse me. And to that we have to add a certain function of the diameter, so it becomes $.3\ D^2$ plus $f(D)$, where f is the interferometric function. What is f? I would guess that it probably is a linear function of D, and no more than that. But what is the coefficient of this linear function, and what does it add as a constant term? We don't know - we have to find out. This will also have to be done in the near future. There is also this little nasty question which is hidden in the bottom of the cabinet, that is: what is the thermal modulation effects in terms of noise, due to interferometry with far apart telescopes and a number of mirrors in the way. We can't escape this question, it has to be looked into also.

Finally, there is a last conclusion I would like to make which is rather a personal wish. I watched that at this conference there were about 16 countries involved. We are at an IAU meeting, 16 countries is a large part of the world, it's not all the world. I would wish that this next generation of telescopes in this IAU context be also the generation of telescopes which will allow such a meeting to be held in 30 years with a larger representation of the world than the one we have today.

3 - General Discussion

(G. Burbidge, opening the discussion to the floor) Well, Dr. Dravins made an interesting proposal, I think. He suggested that a very high remote site was a very good site. Now, what are the trade-offs? Do you all agree with the idea that one should go to 20 000 feet and try to operate a very complicated machine in the wilds of South America? Is that a good idea? I personally would argue that it is probably not a very good idea because it would be logistically an extremely difficult place to operate and one really has to worry about the boundary conditions. And the boundary conditions, as far as I can see, are very severe if you go to those extreme sites. But do the other members of the panel agree with what he said?

(J. Wampler) I think it depends on what the conditions are like up there. If they are really good, then I think you do it. If you can get 0.1 or 0.2 of an arcsecond fairly frequently then, in my opinion, it is worth doing. I think, incidentally, just because the Incas were able to build stone structures on the mountain doesn't necessarily mean that American or European astronomers can work there. Remember that the descendants of those Incas are quite happy playing football at 5000 meters, but if you ever try that, I think you're in for a big shock.

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(G. Weigelt) I liked very much what Dr. Dravins said, because if you look at measurements of the atmosphere you see that the atmosphere consists of about ten layers, and nearly all of them are concentrated in altitudes of up to 5 kilometers and in heights between 10 and 15 kilometers. Therefore, the probability of getting extremely good seeing is very high. And seeing, as we know, is a most important point of the whole project. But we have to find out whether this is true, as he said. We have now to start measuring these things and we must buy very soon transportable large telescopes and do measurements. By large telescopes I mean 1m telescopes or, for example, 1.5m telescopes which we can get from Antoine Labeyrie. And they will cost us nothing because we'll use these telescopes first for site testing, and, in a few years, we'll use them for the interferometer, as was shown today in one of the slides. So, I would really like to start very soon with site testing. And as I said, it will cost nearly nothing because we'll need these telescopes for later interferometric work.

(G. Burbidge) How much of your life would you spend in South America working on this project?

(G. Weigelt) I'd like to do it.

(G. Burbidge) I'm mindful of the problem that we have currently with Cerro Tolo where it is very hard to keep scientists and technical staff even in populated parts of South America. So, I still have to say that, personally, I think it is impractical. In fact, I believe, at the end of the last century a Harvard group was operating in southern Peru and they finally gave up for logistical reasons.

(A participant) It would seem to me that we should not be afraid of operating a 10 or 15m telescope at an altitude of 5 kilometers, when we are not afraid of operating a 2m telescope at an altitude of 600 kilometers.

(G. Burbidge) If we had the same operating budget, I would have no problem. That brings us into the funding problems.

(J.P. Swings) Actually, in this context, I think a bit of realism is required, because given a certain budget, are you going to spend 50% of that budget developing a site and logistics instead of more percent for your telescope or your array, or whatever? I mean, that has to be balanced as well. I agree with Gerd Weigelt that sites at 5000-6000 meters are probably exquisite. But has anybody ever made a study of the costs of developing such a site? Maybe the Mauna Kea and CFHT people would know. Maybe René Racine or some others would like to comment on that? I would be interested to know for a given project, a 4m telescope, how much do you spend for the telescope and for developing the site?

(J. Wampler) Let me interrupt just one more time on this particular subject. I think that if the cost of the telescope scales as the second power of the aperture, then it probably is worthwhile to spend half of the total project cost on developing a site, if you can get a factor of two in seeing. Now I don't think it is necessarily true that this Andese Ridge Line has good astronomical sites on it. The coast of California and the western coast of South America are rather similar in geography, except that the mountains in California are not quite as high. There is a cold offshore current. The winds during periods of good weather in California come from the west and blow against these mountains. Along the costal mountains the seeing is very good. But if you go to the Sierras, which are considerably higher than these costal mountains, then the seeing is very bad, and the reason for that is that the wind hits this mountain chain and goes up and forms these lenticular clouds along the tops of these mountain peaks. Now, observing at Tololo I have often seen the same sorts of lenticular clouds on the high Andes that I have seen in California. And we have looked at these inland sites in California, and they're just terrible. The seeing is 20-40 arcseconds.

(D. Dravins) Of course, the quality, in detail, of these sites is not known, and my suggestion is that a serious effort be made to examine those. There is, however, a very strong gradient in weather conditions along the high Andes as you go from, say, central Chile, where Tololo and La Silla are located, further north. Down in those areas there are notorious winds and conditions are very unpleasant. As to how easy it is to operate something at such altitude, I think the reaction may be different whether one asks a person who is used to running a large ground-based observatory the way it is being done or whether one would be asking some space astronomy person. And I think one should recognize that there may be a large community, significant both in terms of people and financial effort, that conceivably could join a very large telescope effort, if it could be at an extremely dry site, because it could be directly competitive with the quality of observing that is at present obtained from airplanes and maybe even balloons.

(G. Burbidge) Well, I mean, I agree with what you say from a scientific standpoint. But I have to reiterate what I said just now, which is that I'm sure one is obsessed by the cost problems: but they are real, they don't go away, they don't change and over a time they seem to more or less stabilize. And I just don't believe that you could argue that a ground-based remote site will get the measure of financial support that you get from a space based operation.

(D. Dravins) That's correct. However, in the long term one of the major sources of cost will be the running costs. If one is to design an observatory at 6000m altitude then that has to be designed in a very different manner than the small workshop type. I mean, you'd come with a screwdriver and adjust the mirror here, a bolt there, the way it is done today. That has to be designed from the ground up the way a modern manufacturing industry would be made, with maybe 100 industrial robots and maybe a small staff of two or three humans at the site supervising the operation. There is no way to have a large number of humans working at such altitudes.

(A. Labeyrie) I believe that this deserves some study. In fact, it is a very important point. We should first try to look at the image quality on top of these big mountains and then do that sort of industrial study. I think that is a very worthwhile thing to do.

(H. van der Laan) Isn't it true that the cost of operating a site is not only dependent on its height, but also on the question of whether there is an urban area near enough, that you can persuade competent Europeans, technicians, to work there, and spend good fractions of their lives there. That's the real problem. I don't think it makes that much difference whether we're talking about 4500m or 3200m height. Rather we are talking about a) developing the site in the first place, and that is rather dependent on the proximity and accessibility of an urban area in that particular region of Chile. And b) it is indeed, as we have seen at La Silla, extremely difficult for people, more difficult peculiarly for Europeans than for Americans, to be persuaded to spend good fractions of their lives under those circumstances.

(A. Labeyrie) Well, I believe if you get good communication links then you will find a few candidates who will like to spend their life up there.

(G. Burbidge) Our experience is that people will go for a period, but it's very hard to get people to really be transplanted into those parts of the world. And I think, however many robots you put it you're going to end up with some people ... It seems to me that we had some volunteers in this audience...? I hope the ESO people are making note of this.

(G. Weigelt) I think we cannot decide this questions of whether it's worthwhile to go or not to go. My point is, we have to measure how good the site is and maybe the image quality will be 0.1 arcsecond, and then, I guess, you will get problems getting people to come to Kitt Peak, because everybody will want to go to this new observatory. Who wants to go to an observatory where you have 1 arcsecond seeing when you can have 0.1 arcsecond seeing?

(G. Burbidge) I'm getting more discussion than I anticipated!

(A participant) This is perhaps a related question, since it is also on sites, but there is also a scientific reason why we have to go to the southern hemisphere anyway. If you hear the projects discussed here - I would say there are two major projects, the VLT of ESO and then the NNTT, and a large number of smaller sized projects - but apparently all these people are planning to put their observatories in the northern hemisphere. I don't know, perhaps, about the British? I suppose they also want to go to the La Palma observatory or perhaps to Australia? But in the southern hemisphere you have the Magellanic Clouds, you have the Carina Complex and a lot of things you have to observe. Even the sites discussed by ESO at 24° southern latitude, there the Magellanic Clouds are already at 45° from the zenith at the highest point. I think it's urgent to have some sites there anyway.

(G. Burbidge) I agree. I mean, in a facetious sense, even I would hope that the Universe is symmetrical if we go faint enough ... So it doesn't matter. On the other hand, one of the powerful arguments you have not made is for observations which complement space where indeed there is no bias. Therefore, I agree: we do need major facilities in the south. Are we still talking about sites?

(H. van der Laan) Yes, because this is a crucial issue and I feel that the situation for the United States is very different than for Western Europe. In the United States it seems that you either go to Mauna Kea or you go, say, to Mount Graham if that turns out to live up to its promises or to its expectations. For Europe, it seems to me - and that might be heretical -

that it would be much better for ESO to decide to build one 8m telescope for La Silla, finish it by 1992 or 1993, utilize that time to do a real good site testing campaign with 1m class telescopes and not make an irreversible decision on the VLT until the early 1990s.

(M.H. Demoulin-Ulrich) I would like to ask people like René Racine and Jacques Beckers what is the cost of putting a large telescope on a very remote site, like the first telescope on Mauna Kea, or the difference of cost between the same NNTT on Mount Graham and on Mauna Kea?

(R. Racine) The contractors who bid on the construction of our dome assumed that their workers and the production would be at about a 60% efficiency. They made a bundle of money. We ran at about 85-90%. So, it cost us a lot of money to build the telescope. The construction cost of the CFHT is well known: it was 30 million dollars in 1976. So you can compare that to what it is today. The construction costs would probably be about 15 to 20% higher on Mauna Kea. I have no idea what it would be on the Chilean sites. Our operating budget is 3.6 million dollars a year. We're completely self-sufficient, we do everything: we do not have any agencies, we do not have any universities or government labs to support any of our activities; we do the management, the scientific, the technical, building maintenance, etc. We are a staff of 41 and a budget of 3.7 right now.

(G. Burbidge) Let's move on to a related question. Harry, in the course of his discourse, said something about remote observing. Remote observing is, for many, the only way to operate large telescopes in the future. I take it that everyone agrees with this point-of-view? What worries me a little about remote observing is that you're taking the scientists further and further away from the equipment that they use, and very soon we're going to have a generation of scientists who really don't know how the equipment works at all. They're not even encouraged to find out. The next stage, of course, is to take the equipment away and they won't know the difference....! But there is a serious question here, I think, which the high energy physicists have also faced when they do the very big experiments. Their solution essentially is to send the graduate students, at least in the States, to the big labs for extended periods. I don't know of any equivalent way of handling things if we operate large telescopes remotely in remote sites. But I take it that everyone is agreed that it is a good idea to remotely observe and that it will be done for all of the major 10m class telescopes on these exquisite sites? Is that a fair statement?

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(J. Wampler) Geoff, I think there is a problem here in comparing high energy physics with astronomy. In high energy physics you spend four, five, six years making the equipment work. It works once and you tear it apart. In astronomy you spend however long it takes to build the equipment, you test it in the lab, and after the hundredth time that it works you're confident that in fact it will work and then you take it to the telescope. I think that probably it's more important to know the science that you want to do and to interpret the data than know how exactly 8th order diffraction theory works.

(G. Burbidge) How will you train students in the future?

(J. Wampler) Well, I'm not particularly worried about how to design optics: I get Harlan Epps to do that. And I'm not particularly worried about how to interface computers with instruments: we have computer engineers who can know that far better than I could.

(M.H. Demoulin-Ulrich) I think that people who are operating the Space Telescope don't have worries about this, i.e. the fact that their observers will be far apart from the observatory. What is important is to understand the characteristics of the data one acquires, the noise for example, rather than being close to where the photons come, really. So, I believe that optical telescopes can very well be operated from a very large distance. But that depends also on the voice contact and whatever telecommunication systems there are.

(A. Boksenberg) I think this whole business of remote observing is complicated. I'm personally committed to it, in the sense that I believe in it. I don't think, though, that it's a black and white activity. You still have to have the other sort of observing, because an observatory is not a sort of power station where you just sew it all up and then run it, but it's continually evolving. And instrumentation, of course, continually evolves; in fact, it's that particular component of an observatory that makes it work. I think I'm probably right in saying that the 5m telescope 20 years ago had only a hundredth of its capability today. It's the same telescope. It's the instrumentation that is different and it is that that requires the attention. So, yes, remote observing for those instruments which have been run in and shaken down, etc. and, maybe, that's the sort of instrument that takes most of the time: the spectrographs, the CCD cameras or whatever. But

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there will always be instruments that need attention; there is always, therefore, the requirement to have staff on the mountain and innovative work going on. I don't really believe robots, by the way, can take over because of that particularly evolutionary requirement. But let me say that when I last went to the AAT, some years ago - I haven't got any time since - I went to the control room and observed and went home again and realized I hadn't gone onto the observing floor and stroked the telescope, and I'm sure this is perfectly alright. I don't think one has to see the thing, in fact the instrumentation nowadays in common user or a national sort of observatories is so complex that you don't actually allow the observer to touch the instrument. It's the on-site experts that set it up for you, but you know the instrument through the control desk, etc. And that control desk may as well be back home. So, I'm sure it works. But one hasn't to take a black and white attitude to it. There still has to be the other sort of observing as well.

(J. Beckers) I don't think this is a problem, even if one decides that the NNTT or the VLT would be operated totally remotely there are plenty of other telescopes around which will not be operated that way, on which instrumentation development and new techniques can be worked on. So I don't think that is really where the problem is in astronomy. The problem is with robots. I think they are going to develop to be sexual beings and the problem is going to be to keep their wives and children happy on this mountain top.

(H. van der Laan) I think it is important to realize that the term remote observing does not have the same meaning for everyone. It's important to define it. As I said, there is a whole spectrum, and one extreme is simply to have a menu of observations for which time has been allocated by a critically evaluating program committee, and then the on-site management makes the menu and in fact adapts it to current situations, continually changes it. That's absentee observing where you get sent home a data set with calibration data and all the specifications you need in order to analyze it. That's one extreme perhaps. The other extreme, as I said, of remote observing is to totally simulate your control room at a remote distance. And there are many intermediate situations. I would like to hear from the audience whether people think that it is necessary and desirable to have the very great expense of completely simulating the local control room remotely, say at your home institute or at national or regional institutes, and also to try to pay for the very high data rate links using satellites that you require and have to pay for in order to have a true simulation of

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the complete control room. I will say that, from my point of view, it is preferable to be much closer to the simplest end of the remote observing spectrum which means to have competent staff on site that optimize the decision making. Then I'll be happy to wait for superb data for a week.

(G. Burbidge) I might add that when you do things this way you do need as you just said very good people on site and you don't really reduce significantly the complement of people required even as compared with those when you have your astronomers going to the mountain directly. So, as far as the operation is concerned we go back again to the thing that was worrying me when one was talking about high remote sites. You need a very significant complement of highly talented people and I really don't see that changing.

(H. van der Laan) I think to first order, remote observing does not save money and does not decrease the size of the crew on the mountain. It changes their duty somewhat. What it does is that it optimizes currently prevailing conditions, it optimizes the seeing and takes it out of folkore into real exploitation.

(S. Isobe) I think we need all wavelength ranges astronomy. So we need to consider the relation between other wavelengths of observations, such as the X-ray and radio ranges. This is the reason we decided to introduce a remote operation in Japan. We would like to make a tight connection with X-ray and radio people and then we have the same control desks in the same places. Then one can make quite nice collaborative work.

(A participant) I would like to relate my own experience with remote observing. Remote observing was offered to me twice: once on a 24 inch at Las Campanas, and I did accept because it was a secondary program; and it was offered to me a second time at CFHT and I did not accept it. The reason why I did not accept it was because I felt that the hands-on experience, immediate experience, you get by actually handling the instrumentation and doing things was too valuable for next time when I would design another observing program. There are two other things also that are of consideration. One is that when you go to an observatory you meet a lot of people, so you have that kind of stimulation. The other thing is that if you live on the East Coast it's nice to go to Hawaii in February or March.

(G. Burbidge) I thought I would mention that aspect at some stage, but I'm glad that you did. We have people who really don't want to go into remote

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observing, because to come to Arizona in the winter, to get out of classes and so on, is a marvelous experience.

(J.P. Swings) Remote control or remote operations also implies, I guess, special designs of the instruments once you start thinking of your telescope and your equipment. You can't hang an instrument and then decide it's going to be remotely controlled. It has to be thought of very carefully when you design your telescope and the instrument. Therefore, the implications might be quite severe for the definitions of the telescopes and the sort of instruments which will be attached to them; whether they will be made available, say, for long periods of time or whether they remain fixed or can be interchanged very rapidly, and what are the computer connections, and so on. There are a lot of implications as far as the definition of the future projects go.

(G. Burbidge) I don't know ... I've got the impression that people would usually do what has to be done to get remote observing. Alec I think agrees with me. He has a flying circus that goes around the world and they would deprive the airlines of a great deal of money if they went into this business.

(A. Boksenberg) Well, actually, yes, that particular equipment you're referring to is certainly not remotely operable. I was just going to say something to that point about designing instrumentation. As I understood it, most modern instrumentation is, in fact, designed for remote operations (certainly all on the AAT and on La Palma is). It's remotely operated from 50 feet away. You don't actually go and touch it and see it; everything is totally remotable. So, it's just a matter of extending that link to the station back home. I don't think, by the way, Harry, it's a very costly exercise - it's not cheap but it's not very costly. The sort of equipment one's talking about has already been designed because it's there already and one has to build a copy of it. It's not that costly. The cost of the lines or the satellite links, etc., generally speaking, can be offset by not using airfare, say. This sort of thing has been looked at and if it had been totally out of the question, I don't think we would be talking about it now. In the U.K. at least, when we looked at this - and I still believe it's true, we could say, look, as a boundary condition we'll just have the same cost working remotely or not and that includes trading airfares for communication links.

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(B. Oke) I agree completely with what Alec says. There is, however, one real difference, I think, in remote instruments and that is that you have to be able to verify that they in fact are working properly. You see that, I think, with the instruments which are being designed for the Space Telescope. An enormous part of the costs of those instruments, the functioning of those instruments, is in fact going to be the testing which goes on to verify that on any particular occasion they're doing what they're supposed to be doing. I think if one does this on the ground one is going to have to provide that kind of link as well. If the observers, and particularly observers who've designed the instruments, are there from time to time they, in fact, can do this much easier. If it's remote, however, it will be more expensive.

(D. Enard) I have to say something concerning this question and also the fact of, or constraint in, operating a remote site with a very small staff. I think if one compares the cost of a VLT with, say, the Space Telescope, it has been said that the cost ratio was a factor of 10. The question then arises where this difference comes from, because, after all, a VLT is not a simpler machine than a Space Telescope. It's even more complex in many respects. I think the difference in cost, if you don't consider the political and technical constraints of putting something in space, is simply reliability. If one is going to operate a remote observatory in I would not say the same way but a similar way, we'll have to pay for this reliability. And that implies that instead of building one prototype, which is after all what we do in astronomy - all our instruments are prototypes, we'll have to build several models and that will cost money. It's difficult to tell, of course, right now what the difference will be, but I guess that it will be a factor of 3 or 4. I think that is something to consider very seriously if one wants to set up an observatory at 6000m to be operated with a crew of 2, 3 or 4 people.

(A participant) Certainly, the remote control is a very useful thing. However, it also requires that a large number of instruments be permanently ready to be activated on the telescope. Something like 10 or 20 or perhaps even more instruments would be just waiting there on the telescope ready to be activated in minutes or in seconds. This means that the optical solutions for that will have to be studied. How do you send light to a choice of any among 30 instruments within a few seconds? This is a very important thing.

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(G. Burbidge) Several members of the panel said something about costs. I calculated this morning that we must have spent close to a million dollars attending these meetings over the last few years. In the States we've probably by now spent enough money to construct a rather cheap 4m telescope in the work that has been done both at the national level and at universities in the NNTT program, or that will be the situation at the end of this 3-year period. It seems to me that we do have real problems here which are not only present in the United States but also in the United Kingdom and, as far as I can tell, in the ESO countries: We haven't yet really found a way of persuading our political masters that we should get funding in these areas appropriate to the projects of the magnitude we're talking about now, which are basically on a scale ten times those that we've had in the past. The one bright glimmer on the horizon to me has been the likelihood, since the last meeting of one of these groups, that California is going to get maybe half of the cost of a 10m telescope from a private source. That is a landmark, really, because I think you have to go back as far as Hale's raising money for the 200 inch telescope to find that kind of money being raised in that way. I think that project will come off. Otherwise we do have a problem. And the problem, as I see it, is that we're just getting about enough money to operate the facilities we have and upgrade them, but new money of significant amounts is not coming forth. Thus the trade-offs are very difficult, because they are 'operational support' against 'new projects'. And it's no good arguing that you can save a significant amount of money by closing down many small facilities. You will not save that kind of money. The real trade-offs are associated with closing down major, first-class operating facilities in order to build different kinds of facilities and those are questions that none of us want to face. I, perhaps, am too pessimistic, but I think we're all trying very hard but we haven't been terribly successful. Why doesn't someone else tell me that I'm wrong?

(D. Dravins) In selling a large telescope project, I think a very important thing should be not that it is attractive to astronomers but that it is attractive to the physics community, because it will, in one way or the other, be competing with other grand physics projects, such as new accelerators or plasma physics experiments. Therefore, I think it is important that the facility be something so unique and so different from what has previously been built that it cannot be dismissed by people in the physics community as just another of those telescopes that are littered all over the world.

(G. Burbidge) Kurt, do you have any comments? Kurt Riegel is from the NSF and I warned him I might start talking budgets... He knows very well that he's not being put on the spot.

(K. Riegel) We've had the view expressed by Joe Wampler that astronomers, in some sense, have been too modest in their requests for funds from funding agencies. I agree with that as compared with many other points of calibration. On the other hand, the US National Science Foundation has just had the pleasant experience of going through two successive years of 20% budget increases and we find that the astronomical community has in their proposals consumed that increment many times over. I'm happy to say that I don't think as we contemplate major new facilities it is only the single choice of whether you will operate or build. I think that, in fact, there is at least some chance of achieving a natural increment above base budgets. When you're talking about NNTT scale projects at a 100 million dollar level or so, I think it is impossible to conceive the funding there out of base budgets by simple redirection. You must in fact seek and obtain a major increment and in my opinion astronomers do have a very good and very competitive story to tell with respect to the other sciences. In the U.S. I think that that case for funding should be made and that it can be successful if three conditions are satisfied. First of all, there must be some measure of broad support, the kind of support that was achieved for the VLBA, the number one priority within the US astronomical community. And that support has to be evident not only to the astronomy division of the National Science Foundation but to the Foundation at large, to the Office of Science and Technology Policy in the U.S., to the Office of Management and Budget, a creature of the White House, and so forth. Furthermore, I think that NASA, which represents a very large component of space astronomical funding, has to somehow be persuaded that ground-based instruments of the kind we have been talking about are very useful in their own terms, so that NASA becomes a porter of support for major ground-based astronomical facilities. There has to be a reiteration of the feeling - if there is such a feeling - that the consensus of the astronomical community at some point - 3 or 4 years in the future - is that the number one priority will be a 15m or so class astronomical instrument. And then, finally, there has to be engendered a genuine feeling that such a telescope can be built to work, that it will work; and it's the best investment that can be made in science relative not only to other astronomical projects but to other scientific projects.

(G. Burbidge) I don't know how the climate looks in Europe with respect to this question. Harry?

(H. van der Laan) Again, the European situation is very different from the North American one, or from the United States one, because one is dealing, say in the ESO context, with 8 different countries, 8 different ministries, all of which have by and large different budgetary structures. In some countries the ESO contribution competes with the national program, in others it does not very directly. It's interesting to see how CERN has handled these problems. CERN has only very recently gone through a long and complex procedure to finance LEP which is their next major accelerator. There were three scenarios proposed to Council and then council members took all those scenarios home. There were three different financial levels. The highest one of course meaning a very substantial and virtually a long-term increase to the national contributions. The other one being very nearly level financing with extremely painful measures to close down facilities that were still very good. But within CERN one had to reach a consensus unanimity and that was in fact achieved. The lowest scenario was decided upon and LEP is now under construction. I think for ESO and the next generation telescope such a low scenario, which is almost within the current envelope, is in fact simply not feasible, because on La Silla there are an insufficient number of things that could conceivably be closed down and save operation's money in order to make a wedge for new initiatives. So in the ESO case there has to be a permanent increase in the operating and investment budget. Therefore, all countries in fact have to agree to permanently pay this. As you know, the ESO contribution is based upon the GNP of all the countries, so everyone pays his own fair increase in share. That does mean that all the countries, and astronomers in all the countries, have to make a concerted effort to persuade the national authorities that this is worthwhile. Perhaps we should some time have a small European workshop of 2 or 3 people from each country to work out a common scenario in order to establish that European consensus.

(G. Burbidge) The discussion of budgets seems to quiet this audience down. Let's go back to the question of the size of the telescope. How large should a large telescope be? Is this really to be governed by cost? (At some level it is governed by cost.) Obviously there is a kind of a band-waggon developing for 7-8m class telescopes based on the idea, I think, largely that Roger Angel is likely to be able to make such mirror blanks in such sizes. Is that where we really should be going next, rather than going somewhat further? I believe there is a political case in some quarters for arguing that biggest

has some special significance - it doesn't obviously to the scientist, but it does often to the person who reasons in a rather chauvinistic way: well we've got to go one better than the others. Is it reasonable to argue that we should - let me be heretical for a moment - that we should back away from the VLT and the NNTT and say that our real next step should be a 7-8m class telescope, given the ridiculously low price of 20 million dollars which you heard this morning? (I personally don't believe this, but ...)
Nick, what do you think about this?

(N. Woolf) I think that we do indeed want larger telescopes, but we have to think about what we mean by "larger" telescopes. Back in Hershel's time, when he talked about his 20 or 40 foot telescope, he was not talking about the diameter, but about the length of the tube. And we, when we start talking about our big telescopes, are still only talking about the diameters of the mirrors or the collecting power and not, for example, about the angular resolution power. And therefore, indeed, there are a variety of big telescopes that now become available, so that the concept of what is a big telescope is something that is adjustable. The radio astronomers have tried to deal with this and they have faced up to the fact that very large single dishes start getting extraordinarily expensive so that, even though the Jodrell Bank Mark I was made back in 1956, it's only relatively recently that we've gone up to 100 meters. And there seems to be no great demand to go bigger. Instead the effort has gone in the direction of arrays, either on a modest scale with great filling, and when I say modest scale I mean only 20 miles or so, or over a trans-continental base line with very little filling. We too have those options. We have certain peculiar differences that arise simply because of the efficiency of optical infrared instruments that we gain by in fact going to our biggest aperture, whatever it is, and trying to mass-produce it, whereas the radio astronomers tend to gain more by going to a somewhat smaller aperture and make it many more times over. But nonetheless this does push us into a particular direction. We see that this ridiculously low price for the 7 or 8 meter class telescope is not impossible to attain, and that, indeed, as a standard unit it may be the appropriate way to go to these big facilities.

(R. Wilson) I should like to comment on the point raised by Harry van der Laan regarding the optimum size of the unit telescopes of a VLT. It seems to me there are three powerful arguments in favour of unit telescopes of about 8m:

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1. The extrapolation factor from existing telescopes: Although 5m and 6m telescopes exist, we are mainly extrapolating from the 4m class as most recent contemporary telescopes and 8m represents a factor of 2. Now a factor of 2 is a lot if you look at the history of the telescope. The 200 inch was an extraordinary achievement and this was only a factor of 2 relative to the 100 inch. If Hale had followed Pease's dream of a 300 inch or bigger, I wonder whether the resulting telescope would have worked anything like as well as the 200 inch? I think experience has shown that one learns more and finally advances more rapidly if the size jump is reasonable and not exaggerated.

2. Monolithic blank technology: While the segmented approach is pointing the way for larger things in the future, going to 8m would permit extension of existing monolithic blank technology which falls, I believe, mainly into Joe Wampler's first category of New Technology, that with low risks. Beyond 8m, monolithic blank production and working may become a major technical problem or may, anyway, become uneconomic compared with a segmented approach.

3. Monolithic blank handling: Handling, even more than production, suggests a monolithic limit of the order of 8m. Even if it is possible beyond this, the risks and inconvenience are going to grow rapidly, even if aluminizing were done in situ in the telescope.

If one accepts these arguments, they would point to the logic of a super MMT as the choice for the NNTT, also from the point of view of the existence of a tried and proven model of 4.2m. This may be a provocative remark ignoring the constraints of the American scene, but it is how it looks to me from the European end, particularly as a 10m SMT project is already underway.

(G. Burbidge) I was reflecting as you were talking that Hale, one man, in 25 years managed to conceive of and get built the largest refractor, and conceive of the 200 inch telescope, all between 1890 something and 1928: 30 years. Hale went to a factor of 5 in his own lifetime before he became as old as I am or you are. Well, why could he do it so much faster than we can? That's right, this is a favourite theme of mine: there was absolutely no democracy. That was one point. He simply went to Andrew Carnegie and smoked a cigar and talked to his banker and got a cheque.

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(J. Wampler) Geoff, there is another aspect to this, too, and that is what the atmosphere is going to do to you. Because, you know, he started the 16 inch at Yerkes and then moved it to Mount Wilson. And on Mount Wilson the seeing is really quite excellent - it's a bad site now because people like to live there and they built a big town around the place which gives nice access but it ruins the astronomy. But for people who have tried to build instrumentation for 3-5m telescopes it is very difficult to build fast optics, and by fast optics I mean optics that uses few elements to match seeing disks of say 1 arcsecond or greater. Bev could comment about this. I think that if you have 1 arcsecond seeing, the largest telescope you want to build is something like 4 or 5 meters, because if you build a telescope bigger than that, then the optics for the ancillary instruments becomes so difficult and the number of optical elements are so many that you're beginning to lose light in your spectrographs or photometers that the primary mirror collected. Now, if the seeing is half an arcsecond then you talk about 8 meters, and if it's a quarter of an arcsecond you talk about 16 meters. So, there is a natural limit that we now are facing in the atmosphere that Hale didn't have to face.

(J. Beckers) I'd like to go a little bit back. I think the jump from a 5m telescope to a 7.5m telescope is a very small one. In fact, I would think that the type of science that you could do with a 7.5m telescope compared to a 5m telescope is not all that overwhelmingly better. Unless you put it on a very superb site. And I think it's going to be very hard to sell on the national level a telescope, unless it can really do substantially qualitatively different science. I don't think the jump from 5m forty years ago to a 15m is all that unlikely a step that we cannot make that reality. We started thinking a number of years ago about a 25m telescope, and we went back to a 15m telescope mostly due to cost configuration. But I think it can be done. The case that has to be made has to be made on the basis of new science and that lies mainly in things like infrared imaging, interferometry of some kind, detector background limited detection of high dispersion spectra of quasars and of other distant faint objects. There are a number of good scientific drivers for the 15m telescope. Whether you want to do it in an array or in a compact type telescope like the SMT or the MMT - this is a kind of an array - is something that I'm looking at very carefully at the moment. I think if it is in an array one adds to the complexity substantially and one doubles maybe the amount of problems we shall encounter. So I'm trying to make a comparison there. But I think it can be done and I think we should go for the larger telescope. We are much too conservative if we're just arguing for 7.5m telescopes.

(A. Boksenberg) I'd just like to echo that. I do think that a small step is clearly achievable, and it may not in fact give any more information about making the step you really want to make, because one is really talking about roughly the same technology. But having said that, I don't think it's difficult to make a big telescope. The telescope is mainly steel. That's easy, as Hale himself said. In fact, as he said, it's a much more difficult job making a ship than a telescope. When I looked at the 4.2m telescope on the floor at NEI Parsons it was the smallest thing they were making. I really don't think it's a problem. The problem is the mirror and what you arrive at as its concept. I think that is mainly the uncertainty we're discussing. But not the structure.

(J. Wampler) I think there is a difference between a telescope and a ship. One time when I visited the Royal Greenwich Observatory, I was told by a shipwright that a machinist works to the nearest thousandth of an inch, a carpenter works to the nearest tenth of an inch, and a shipwright works to the nearest ship. I think that the precision which a telescope requires is what drives the cost up. It's not just putting steel together.

(L. Barr) I can't let Alec's remark go by without saying something about that. Telescopes are different than ships. For one thing, a telescope would sink immediately if you put it in the water. But beyond that, it's been my experience that there are very few engineering disciplines associated with telescopes that aren't generally pushed right to the edge of some capability or other. You mention structure as being straightforward in telescopes. They're really not. Telescopes are right behind airplanes in terms of being the most analyzed structures around. The precision with which we have to maintain optical alignment in telescopes far transcends any ordinary structure that you find being built. The business of fabricating optics, we all accept, is being a critical job. But consider even the electronics that goes into telescopes. We're forever trying to get the noise levels down well below anything that would be ordinarily acceptable anywhere else. The list goes on and on. I really believe that telescopes do push the engineering profession around quite a bit.

(G. Burbidge) We could go back to this question that was raised by two panel members concerning interferometry. How valuable is it? How much is it worth in terms of the tradeoff in costs? I'm not sure that that might not spark a little bit of an argument?

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This technique is a very difficult one. A large fraction of the astronomers are interested in faint objects and low resolution. And that's where the action is at the moment. It will change. But now there is a feeling that there really aren't that many important problems to be looked at once you go in the direction of interferometry.

(D. Dravins) I think one driving force for interferometry will come from the Very Large Telescope itself: there will be a threshold passed in angular resolution, which at the present time is just barely reached, namely the resolution of stars which at the present time are only marginally resolvable with the largest telescopes. There are maybe 3 or 5 pixels across the diameter of the largest stars. But if we go maybe to 15m, then suddenly a large number of stars, in the order of 100 or so, will become resolvable, and it will become possible to study the physics of stars, the physics of stellar mass loss, maybe the physics of star spots, to study spectra from the centre of stars, from the spots of stars, from the limbs of stars. And this will drive an interest to get higher angle resolution to do interferometry on spectral line profiles, or maybe Zeeman splitting and so on. And this kind of research is not done today. A lot of the discussions at this and similar meetings about what is to be done in the future with large telescopes is centering on what people have been doing in the past with the present telescopes, only more of the same and maybe slightly fainter. But I think there will be a whole new class of physics problems that will be opened up by the VLT itself and that will drive the interferometry requirement from there.

(J. Beckers) I'd just like to make a few comments on interferometry. I've been doing it myself. Maybe I'm one of the pessimists, Pierre, on this matter, but there are a number of items in interferometry that, at the moment, are just being pushed under the rug, but that should be addressed if you're serious about pursuing it. I asked this morning about the thermal background, which is one of them I think. Much of the application of interferometry will be in the infrared. The thermal background in those devices are going to be high. I don't know how it is going to be handled. It may require a long cryogenic tunnel and cooled mirrors to get rid of the emissivity of the mirrors. I don't know if one needs to chop between different areas in the sky and maintain interferometric imaging while one does that. But I think I have not heard that particular item really very much addressed.

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The other issue is phasing. All the interferometry that I have done, that Dr. Labeyrie and other people have done, has been done on bright stars where you see interference fringes. So you just tweek things up until you see interference fringes appear. Works beautifully at the MMT. When I try to apply the interferometry technique, say, to active galactic nuclei in the Seyfert galaxies, I'm working in the dark. I don't have enough light to look at the fringes. I can go to a bright star, tweek it up until I get the fringes and just keep my fingers crossed that I stay phased up and work in fact in an interferometric mode. And it turns out, when I go back to the bright star after my 15 minute observing run, that I'm way out. Maybe I can improve that, maybe that can be helped . I think I know, with an MMT type concept, how to lock the different mirror segments together; and with an SMT that should be done mechanically. But if I have an array, like the versatile array or the ESO array that was discussed today, I have no idea how I'm going to keep those telescopes together if I don't have enough light to look at interference fringes. That's a problem that has to be solved and it's a non-trivial problem. The problem of UV plane coverage is not as simple as it's been led to believe. The galactic center, even if you have a versatile array, is pretty low in the sky and you don't get a full UV plane coverage. It's going to be very thin. The problems of the optical beam path length adjustor and optical configuration adjustments that you need in order to keep the zero white light fringe at the center of the star image, and if you want to have some field of view, are non-trivial. There are very large mechanical problems and optical problems associated with it which undoubtedly can be solved, but it's going to take a lot of work and I think it really substantially increases the complexity of such a telescope. And there are a number of other items, I have on my list. But I think interferometry is not trivial, it's not just a matter of making the picture and say I put the light together and look at fringes. I think it requires much more than that.

(A. Labeyrie) Well, I've got to answer that. You do not want to have the extreme accuracy you are talking about. You don't need white fringes. Instead you want to disperse your spectrum as much as you can, so as to relax the coherence requirement, which does not change the limiting magnitude, it just requires more pixels. In this case you have no problems of coherence. It does not have to be accurate better than, say, one millimeter, provided there are no vibrations. All that is required is a good statistical way of getting the visibility measurements. And there are theoretical studies which have been made, particularly by Roddier, which show how to do this. We

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believe that this is not a fundamental problem. You certainly do not want to maintain white light fringes with better than one micron coherence. There is no need for that. Just 1mm is good enough. And that's why it is not that difficult.

(J. Noordam) I'd like to emphasize again that VLBI has shown that image reconstruction from the visibility amplitudes alone does not work and you make terrible mistakes in model fitting. This is especially true if the UV plane is sampled only sparsely, as it is always the case in optical interferometry. The image reconstruction from the amplitudes alone, like Fienup does, works only if you've got a fully sampled UV plane. We discussed this before. If you only have a few samples of amplitudes alone of a large UV plane, then you cannot reconstruct the image. You can maybe infer some parameters of separation and sizes of simple objects, but that's all you can do. You can never unambiguously get the image back.

(A. Labeyrie) Well, some people say the opposite.

(J. Noordam) Well, VLBI, which has a long history already, has only produced good images since phase-closure techniques have been introduced. And they're going to be very difficult in optical interferometry.

(A. Labeyrie) I'm not sure I understood very well what you said. But I understood that you said, if the aperture plane is not filled then you have trouble applying the Fienup method. Is it what you said? (Comment: Yes, exactly) Well, first, you can fill the aperture plane with aperture synthesis or super-synthesis using only four telescopes. That's the first thing. And there are other methods. I think the best approach is to try to fill the aperture plane when you can, when you have the time to do it, and then apply Fienup or equivalent algorithms. Then you get the full image with reasonable certainty. There are a few cases of very simple objects, where you may have trouble with the Fienup method, but it appears that in practical cases you don't have trouble.

(J. Noordam) But there are very few practical cases around in optical interferometry as yet, I understand, while there are many in VLBI.

(A. Labeyrie) Well, Fienup takes a lot of computing power and I think that's the main reason why it has not been applied very frequently yet but in a few years the computers will be so powerful that there will be no problem.

(J. Noordam) Now, but Fienup never works with undersampled UV planes. I've had an argument with him about this, and he just didn't understand what I was talking about because he couldn't conceive of a UV plane that didn't have 256 by 256 points. And you're going to spend years to get one field sample that thoroughly with an optical interferometer. I'm saying it is fundamentally impossible to get a unambiguous image reconstruction from a sparsely sampled UV plane, or aperture plane, as you call it. It is a very important point, that should be understood.

(P. Léna) I would certainly agree with you that filling the UV plane must be something which drives the design of an interferometer reasonably thought of right from the start, and that the better filled it is, the more accurate reconstruction of the true image is. But there is another aspect to this, which is that we have yet little indication on the stability of the phase difference between the two apertures. But the indication we begin to have seems to be that the phase excursion is smaller than predicted by the Kolmogorov law if it were extended to large distances like 100m or so. If this is indeed the case, then, probably longward of 5 microns or so, the phase can be measured just as accurately as the amplitude. And that will improve very much over simple visibility measurements. It will indeed give amplitude and phase of the Fourier components of the object. So, maybe the situation is not as bad as one might be afraid of.

(J. Noordam) But in this case I must emphasize that, since absolute phase measurements are impossible, you need phase closure so that you get a number of visibility points simultaneously. And with phase closure you get a good relative phase. That means that you have to have at least 3 elements going at the same time, and, as Merlin has shown, probably you need at least 6 to be reliable. That's going to be pushing it very hard.

END OF SCIENTIFIC DISCUSSION