The Future of ELTs (Extremely Large Telescopes): A Very Personal View
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"*The Future for ELTs"* is an intriguing as well as daunting title. But this is not about telescopes. After all, what could top visions of telescopes ranging from a "mere" 20 meters, to 100 meters, to plastic 30-meter telescopes in space, to new telescopes for the Moon and even a "hyper-telescope" designed to fill the volcanic crater on La Palma? Instead, this is about an equally interesting subject: Advanced Telescope Builders of the Early 21st Century, which reflects on the gathering of unique individuals that Arne Ardeberg and the University of Lund have so graciously brought together at this workshop.

1. The Mirage of Immortality?

Since this group was last assembled in Bäckaskog Castle four years ago, much progress has been made. To recap, four years ago within the US system, there were rumors that the Decadal Survey wasn't even going to consider a new telescope because of the observation that "you have barely finished the current lot," which was not untrue. Recall the 1999 meeting of this group that started with a stern tutorial from the Maestro himself, Jerry Nelson, that to propose anything or contemplate anything beyond a 20 meter was not only irresponsible, it was close to heresy in the face of at least a hundred years of telescope building experience [1]. "Stick to a factor of two," he warned. In Europe, Roberto Gilmozzi's Overwhelmingly Large Telescope (OWL) was a "skunk works" project hidden within the bowels of European Southern Observatory, AURA's Maximum Aperture Telescope (MAXAT) was similarly "skunk work", and "Euro50" was simply "that Swedish thing".

Fortunately, true to character, we astronomers, in the tradition of Galileo, (or perhaps more in the tradition of 16th century Italian princes that so inspired Machiavelli's The Prince), believed that we could ignore history and the heavy hand of established, organized tradition. In the past four years, we have carried on in our inimitable individualistic fashion and fulfilled our dual calling of (a) being innovative and audacious and (b) going to war—primarily with each other.

However, we are in fact still very much creatures of our own history, prisoners of our own peculiar culture and still supremely confident that our luck (and history) will hold. And why not? Fifteen to twenty years ago, nobody expected the plethora of 6-meter to 10-meter telescopes that now dominate the skyline of the major observatories. We did not expect "the gift" of adaptive optics or the growth in solid-state detectors.

More importantly our view of the Universe is transformed. We discovered the first ripples of the Big Bang. We have, through the Wilkinson Microwave Anisotropy Probe (WMAP), given numbers to the key constants that seem to constrain the "unknown unknowns" of dark matter and dark energy, and determined the unfolding clockwork of our origins. We have used our telescopes to piece together a story of sheer audacity that connects the first objects, which we soon hope to find, to the emergence of planetary systems, which we have already found, to the emergence of life. As a profession, we firmly believe that in the words of Alan Dressler, "We, who are now alive, are that most fortunate generation, the people of the awakening."[2]

Today we are looking out over a landscape where the debate in the US is whether there should be a 20-meter or a 30-meter by 2014. In Europe, we are engaged in an even more ambitious undertaking: determining if a 50-meter or a 100-meter telescope defines leadership in ground-based Optical and Infrared (O/IR) astronomy. With the same audacity that we tell the history of our Universe, we are not debating "if" we are going to build these telescopes, but "when and how big." So confident it would seem, that we heard from Andreas Quirrenbach, who said that his "future" project was already based on OWL's heritage![3]

At intoxicating times like these, perhaps a little humility is called for to remind us of two things:

First, in the words of Martin Harwit, "astronomy is a luxury that society can afford only when it has provided people with life's primary needs— food, shelter, security."[4] Secondly, before we appeal to our inalienable place in the history of our advanced civilization, we should remember what Samuel P. Huntington wrote in his seminal book <u>The Clash of Civilizations and Remaking of World Order</u>, "As civilization's universal state emerges, its people become blinded by what Toynbee called 'the mirage of immortality'… the citizens of such universal states [and particularly their astronomers] in defiance of apparently plain facts…are prone to regard [their situation], not as a night's shelter in the wilderness, but as the Promised Land, the goal of human endeavors."[5]

What we have collectively achieved in the last four years is tremendously exciting, but as our respective stories and arguments unfold, would it not be better for us to consider our almost excessive successes of the last few decades as a "night's shelter" rather than a right of passage?

The reason for reframing our view of recent achievements is not because of the technical challenges (we are good at solving these), or because we are unable to tell an exciting story (who doesn't want to find the first objects, or earths around other stars?). Perhaps we ought to adjust our thinking because of the growing costs of doing ground-based astronomy.

2. What is the Cost of Credibility?

In the last decade, we have collectively spent ~$1.7 billion building the current generation of 6-meter to 10-meter telescopes. We are currently spending globally over $120 million per year operating and instrumenting these facilities.

Against this backdrop of success, we have a new story to tell. Uniquely in our 400-year history of telescope building, we have become far more technically confident in what we think we can build. This is primarily because of our new ability to model, in some considerable detail, the performance of our new telescopes before we actually cut glass or steel. It was Pierre Bely, formally of the Space Telescope Science Institute (STScI), who first pointed this out to me why the National Aeronautics and Space Administration, NASA, was now confident it could go from a 2.4-meter Hubble Space Telescope (HST) to a 6-meter class Next Generation Space Telescope, now the James Webb Space Telescope (JWST) by using extensive, proven analytical and modeling techniques to lower risk (hence cost). Our engineering models can today be almost entirely "virtual" as they were in the case of Gemini's 8-meter Telescopes. As we put the Gemini telescopes together and closed each successive loop, the performance improved (when the systems actually worked) exactly (to better than 0.1 arcsecond) as predicted.

Each of us now contemplating ELTs is setting up rafts of analytical tools, groups of engineering analysts and accreting more and more computer power. Neither Industry nor other Big Science Projects would be surprised by this move. It is what they have been doing for years.

If we want to detect First Light objects and hunt for planets around other stars, we know that the only way to increase the grasp of telescopes is to first increase its collecting area. So in the US, we are looking at single telescopes that have collecting areas that are comparable to the total collecting area of all the 6-meter to 10-meter telescopes built this last decade (40% for a 20-meter, and 90% for a 30-meter) [6]. In Europe, the debate is raging between a single telescope, which represents almost 2-1/2 times the collecting area of all 6-meter to10-meter telescopes ever built, to a machine with almost 10 times this cumulative collecting area, which represents a collecting area of almost seven times the total collecting area of every telescope ever built [6].

No one can say that we lack ambition!

We also realize that raw area isn't everything, and that sensitivity comes from the ratio of the diameter compared to the delivered image width. Thus, to ensure we get the gains that we need from the ground, adaptive optics (AO) becomes a key enabling technology. We now have working systems at the 8-meter to 10-meter scale, but to get to a 20-meter to 100-meter scale, requires the surmounting of some quite formidable technological challenges. The complexity of AO scales from D^3 to perhaps D^6 for implementing full closed loop control [7]. Let's put numbers on the jumps required in adaptive optics technology. Going from an 8-meter to a 30-meter telescope requires the technology to be scaled by factors between 50 and almost 28,000. To go from an 8-meter to a 100-meter requires the technology to be scaled by between 2,000 and 3,800,000.

Given these scientific and technological ambitions, there are two points we must accept:

1. We must recognize that, for the first time, these not insubstantial technological challenges may have to be done by us alone. The Department of Defense (DOD), at least in the US, may not come to our rescue anymore. This is a unique position for us. To realize ELTs we will need high-power sodium lasers, large deformable mirrors, Microelectromechanical Systems (MEMS) and huge quantities of detector real estate.

2. We must have a credible story on cost. Today that credibility has to be defendable to a wider audience; such as an agency, private foundation, or skeptical Big Science colleagues who understand the true costs of buying-down risks. That is, we have to avoid "drinking our own bath water" as a respected colleague and ex-Clerk to the US House Appropriations Committee likes to say [8].

What we find is that our respective cost estimates have shown considerable growth, both with respect to inflation and when compared to our ambitions of only a decade earlier. We are like healthcare providers in that our requests show substantial and real cost growth with respect to our previous (already funded) obligations.

To calibrate us, let's look at what happens if we inflate the original 1930's Palomar donation of $6 million to today (references exist on the Web that give the cumulative inflation figures going back to 1913)[9]. That original sum becomes $66 million in 2003 dollars. The cost of the Kitt Peak National Observatory (KPNO) 4-meter telescope becomes roughly $47 million, the Keck Foundation donation becomes ~$170 million, and the full cost of Gemini ~$200 million (spread around 6 countries). In the same 2003 dollars, we are asking for between $300 million and $1 billion for each of our 20-meter to 100-meter facilities—a cost that far exceeds the single largest donation or Agency contribution ever.

And though we may grumble about rising healthcare costs, there is little doubt about what the public at large will be most prepared to pay.

3. Who will pick up the bill?

In a talk a few years ago, Martin Harwit described the importance of understanding the cost society was prepared to afford for Big Science facilities. He states that when scientist's ambitions go beyond what can be termed the "societal cost"[10] for Big Science and ask "for more than society could easily afford progress inevitably was slow and often faltered"[4]. The Superconducting Super Collider (SSC) under Martin Harwit's model ran foul of this number. In a subtler way, I believe our collective ambitions may be approaching this number. Immediately many of you will cry foul and ask, "What about NASA"? Let's take the example of the JWST. Its total cost (construction, launch and operations) is projected to be ~$1.6B – and NASA is requiring European and Canadian partnerships to make this affordable. I would assert that the total cost of something like OWL (construction and equivalent operations) will approach this number. Today, as we predicted a number of years ago, our ground-based ambitions on either side of the Atlantic are now approaching the scale of Space Projects[11]. The key question then becomes, can the ESO or the US O/IR "community" garner the same political will to fund our ambitions as NASA has corralled to fund JWST?

To answer this question, I think we have to take a step beyond Martin Harwit's original idea, and realize (quite remarkably) and contrary to popular [US] belief, that ground-based astronomy has become a surprisingly collaborative endeavor, and that increasingly diverse partnerships play key roles in enabling large projects. For example, of the ~$1.7B spent to date on 6-meter to10-meter telescopes, 70% of this funding has come from non-US sources (and almost exclusively from a consortia of Government Agencies). To recognize this inevitable trend, I am going to take the concept of the "societal cost" of big science a step further and define a quantity called the "societal quantum," the number beyond which individual funding entities are not prepared to go (even collectively) to enable astronomers' aspirations.

Let's almost arbitrarily set this "quantum" at ~ $300 million and look at a few examples: 1) The cost of the Atacama Large Millimeter Array (ALMA) is currently ~$600 million and consists of two large partners, ESO and the National Science Foundation (NSF). The cost may go up to ~$800 million when Japan joins; 2) The cost of the Laser Interferometer Gravitational-Wave Observatory (LIGO) is roughly ~$300 million. Now that LIGO wants additional resources to enhance its detectors, the NSF and the LIGO partners are actively looking for international participation. In the private sector, the Bill and Melinda Gates Foundation is the largest private donor on the planet. However, if you look at the actual grants awarded [12,13] the largest single donation to a single entity rarely exceeds $300 million. Caltech received a donation of two of these "quanta" from the Moore Foundation and family[14]. It's possible that Caltech has several proposals from its faculty to spend all this and more. ESO spent roughly 2-3 of these "quanta" on the Very Large Telescope (VLT), but this took the collective economies of 10 countries. We heard yesterday that, although Japan spent in excess of one of these quanta on Subaru, they are not prepared to do it again.

As astronomers, we must realize that the rest of science (outside particle physics and space science) has, unlike a decade ago, awakened to the concept that large collaborative projects can potentially leverage substantial resources. Examine the US community's wish list beyond ALMA—the Advanced Technology Solar Telescope (ATST), Large Synoptic Survey Telescope (LSST) and Giant Segmented Mirror Telescope (GSMT)—together exceed by almost a factor of two the amount of money the NSF has spent on astronomy facilities to date.

Today every agency and foundation is struggling to find equitable ways to distribute not insubstantial sums to groups of very smart and highly articulate people, like us, and this is seemingly being done in Big Science "societal quanta."

If we add up our aspirations from this morning, using our own numbers, we are already exceeding $2 billion in capital costs alone, or roughly 6-7 societal quanta. Are there six or seven agencies or foundations globally that are prepared to come up with roughly $300 million each to fund our dreams?

The other problem is, of course, operations. For the large facilities, we are currently spending globally well in excess of $100 million per year. However these cost are spread across at least 14 facilities in the 6-meter to 10-meter class with 365 nights apiece. It's unlikely that we are going to see this many 20-meter to 100-meter telescopes. If we assume optimistically, we may get 3. It would not be unreasonable to estimate the operations costs for these scaling roughly in the same proportion as the capital. Even if we can squeeze these three facilities into no more than we have spent in the last 10 years (i.e., $1.7 billion), we have just increased our cost per night by 14/3 or almost a factor of five. This is the curse of only having 365 nights per year.

4. Machiavellian Princes or Global Leaders?

Individually, most of you may think, "Well, that the other guy's problem**, my** project **is** affordable." We still can't kick the competitive habit that has proved so successful in the past. We are prisoners of our own history, and our own myopic vision of astronomy. We are trapped in "the mirage of immortality" or, put more bluntly; we are "drinking our own bathwater."

It's time to wake up. Collectively, the combined dreams that we have on display here—to a chemist, a biologist, a particle physicist or space scientist—are now "Big Science." We have (perhaps unfortunately) come of age, and the real lessons of history are pertinent. When Martin Harwit asserted that it was the "cost" that brought the SSC down, this was not likely the whole story. If you read through the history of the SSC[14], equally important was the lack of consensus within the physics community on both the objectives of the SSC and how the SSC should be run and built. The US particle physics community was at war with itself, and the US Congress finally got weary with the whole expensive endeavor. We, that is, all of us in astronomy, must now make sure our funding agencies and foundations don't get weary of our collective endeavors, especially given the scope of our ambitions. If we want to see anything like the resources we had in the past, particularly if we expect real increases, we are going to have to work together. We are going to have to find a way to define and articulate a global and coherent vision for ground-based astronomy rather than the 5 or 6 visions on display at this workshop.

To quote from a 2001 article on the history of the SSC, "The conflicts which erupted between the high-energy physicists and engineers hailing from the military-industrial complex during the abortive construction of the Superconducting Super Collider can be understood as another episode in [the] continuing struggle and, perhaps short-lived reversion to an earlier mode of social and political organization of the scientific enterprise. At the multi-billion dollar scale of the SSC (roughly equivalent to the Manhattan Project in constant dollars), powerful forces came back into play that had not figured at the hundred-million-dollar scale of Fermilab and SLAC [Stanford Linear Accelerator Center]"[15].

Like it or not, we are now in transition. We have done the "hundred-million-dollar scale" 8-meter to 10-meter telescopes, and now have billion-dollar-scale dreams. To realize these dreams, we will have to start thinking and behaving like space scientists or post-SSC particle physicists. Like it our not, we have collectively contrived to enter their league. As the LIGO Team discovered, when they decided to build their gravitational wave detectors "like bridges"[16] rather than physics experiments, we are going to have to relinquish cherished notions of individual even institutional dominance. Big Science partnerships are about subsuming individual and institutional goals and working together. With operations costs per night (probably exceeding 4 or 5 times what we pay today), we are going to have to relinquish cherished notions of "time fractions" and "number of nights" on "our" telescope and accept concepts of experimental teams, observatory machines and pipeline-reduced data.

If we want to see an ELT on the time scale of JWST, we are going to have to collaborate. If we want anything on the scale of a Euro50 or OWL within our professional lifetimes, we are going to have to work together internationally. Otherwise, we risk what befell the particle physicists in the US —forfeiting our dreams to the next generation of (more collaborative) early 21st Century telescope builders.

As a community of astronomers, we have been remarkably successful. We have a fascinating story to tell and, as a consequence, still have remarkable public and private support. Some of the smartest people of this generation are assembled at this meeting. Here together, in this isolated spot, out of earshot of the rest of the world, this is a great place to argue and debate. However, what ought to come out of this Workshop is not another set of papers extolling how smart we are. **That's not news.** What should be coming out of this workshop is not another set "of self-licking ice cream cones" (as a senior colleague in the US put it a few days ago), but a blueprint, or at least a framework of what our collective vision from ground-based astronomy should look like in the coming decade. Because if we don't sort out what we want to do, someone else will.

Bill Smith, who is the current President of Association of Universities for Research in Astronomy (AURA) and spent his formative years working in Congress vetting and authorizing all the major NASA and NSF projects of the last decade, likes to remind me that all sustainable projects need "off ramps." That is, when you hit trouble, there has to be a controlled way of exiting your current track so you don't just hurtle into the pile up in front of you. Where are our off ramps? What happens if the technologies we need are vastly more expensive, or we find a 30-meter far exceeds our cost expectations? Do we reduce scope or find more partners? What happens if our arguments in the US over a 20-meter versus a 30-meter persist and result in self-annihilation? What happens if the US economy can no longer sustain the deficit[+] or the priorities of the nation (both public and private sector) change to healthcare, getting people like "Joe Public" into orbit or national security? What happens if, in Europe, ALMA hits cost overruns or the biologists get organized and persuade the European nations that applied science will better stimulate the economy, or if the bill for EU expansion exceeds all expectations?

The particle physicists have been organized for a decade, the radio astronomers have gotten themselves organized, the gravitational physicists are getting themselves together, even NASA and European Space Agency (ESA) try (grudgingly) to coordinate their respective programs. It's now time for ground-based optical/infrared astronomy to get its act together. As Machiavelli remarked in the conclusion of the <u>The Prince</u>, "Fortune is the arbiter of half the things we do, leaving the other half or so controlled by ourselves"[17].

+ Footnote: **"**Perhaps the most difficult problem is that a budget climate which has gone from a $250B surplus to a $400B deficit cannot sustain many science projects that cost three to four times the cost of the Gemini telescopes. I see a wall ahead where things are going to get harder in the '05 - '07 timeframe." [7]

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