

**Illuminating the Dark Universe:  
Report of The Next Generation CFHT Committee**

Harvey B. Richer, Editor

The Next Generation CFHT Committee

Claude Catala, *Observatoire Midi-Pyrénées*, Co-Chair

Ken Chambers, *University of Hawaii*

Patrick Côté, *California Institute of Technology*

Pierre-Henry Cox, *Institut d'Astrophysique Spatiale, Université Paris*

Olivier Le Fèvre, *Laboratoire D'Astronomie Spatiale, Marseille*

Simon Lilly, *University of Toronto*

Pierre Martin, *Canada-France-Hawaii Telescope Corporation*

Harvey B. Richer, *University of British Columbia*, Co-Chair

Claude Roddier, *University of Hawaii*

Christian Veillet, *Canada-France-Hawaii Telescope Corporation*

November 30, 1998

## CONTENTS

The NGC Committee.....	1
Preface.....	3
Résumé Exécutif.....	4
Executive Summary.....	6
Acronyms.....	9
Fundamental Science Goals for a Refurbished CFHT.....	10
Surveying the Current Astronomical Landscape.....	10
What New Telescope is Required to Make Progress on the Key Scientific Goals?.....	17
What Ground – Based Telescopes Will be Available in 2010?.....	22
What Space – Based Telescopes Will be Available in 2010?.....	27
The Next Generation Space Telescope.....	29
The Impact of Large Ground and Space Telescopes on the Key Science Goals.....	32
Current Developments for Very Large Ground-Based Telescopes.....	35
Proposals for the Next Generation CFHT.....	35

## 1. Preface

In January 1998 the Board of Directors of the Canada-France-Hawaii Telescope (CFHT) appointed the NGC (Next Generation CFHT) Committee to explore the question of a refurbishment or upgrading of the existing 3.6m telescope on Mauna Kea. The charge to the committee was:

- (1) to provide a prediction of the most important scientific questions that the astronomical community will want to address in the period beginning 2010 and a description of the type of facility needed to address such questions.
- (2) to project what the worldwide situation for ground- and space-based observing facilities in the year 2010 will be and how a new CFHT could fit into this overall context.
- (3) to provide one or preferably several proposed concepts for the new CFHT. For each concept the committee was asked to provide an outline of the scientific objectives which the facility could achieve together with a description of the basic performance specifications which the facility would need to meet in order to achieve these objectives.

The committee decided early on to expend its energy on the scientific questions and not to consider too deeply what could be afforded, what technological capabilities might be needed to build a particular instrument (as long as no true technological breakthroughs were required) or what the political implications of a particular choice would be.

The first committee meeting was held in Vancouver, BC at the University of British Columbia on April 23 – 24, 1998 with all committee members in attendance. A report of that meeting together with preliminary findings of the committee were presented at the CFHT users meeting in Quebec City May 18 – 20. Most committee members were present at the users meeting and discussion with the wider CFHT community ensued. A second meeting was held in Toronto at the University of Toronto July 3 – 4, 1998 with 3 committee members unable to attend. Roberto Gilmozzi, chair of the ESO OWL (Overwhelmingly Large) Telescope project was in attendance and provided scientific, technical and financial information on the ESO work. An early draft of the NGC committee's report was discussed at this meeting. A final meeting was held between the co-chairs on September 14 – 15, 1998 in Rome where the report was completed, then submitted to the Board of CFHT on September 21, 1998. Based on feedback from both the Board and the Scientific Advisory Committee (SAC) of CFHT some revisions were made to the report and it was resubmitted to CFHT in time for the December 1998 Board meeting.

In preparation for these various meetings 7 committee members attended the SPIE meeting in Kona, Hawaii in March 1998 whose theme was "Telescopes of the Future". All members presented reports at the NGC committee meetings in their scientific areas of speciality indicating where their field was likely to be in the year 2010 as well as on technical aspects related to specific telescopes and their instrumentation. The current report is a distillation of these presentations to which has been added comments and ideas from the several open discussions.

## 2. Résumé Exécutif

Nous sommes peut-être en train de vivre l'époque la plus excitante de l'histoire de l'astronomie, et ce pour de nombreuses raisons. Nous avons atteint aujourd'hui une connaissance approfondie de l'univers. Dans moins de 10 ans, nous devrions connaître ses paramètres fondamentaux (taux d'expansion, densité moyenne, constante cosmologique) à mieux que 5% près, et être finalement capables de déterminer si l'univers est ouvert (expansion éternelle), fermé (expansion, puis contraction), ou critique (expansion décélérée tendant vers zéro). Nous avons une idée de la façon dont les étoiles et les galaxies se forment et évoluent (même si nous sommes toujours à la recherche d'une description théorique complète). Nous avons découvert des systèmes planétaires en dehors de notre propre système solaire. Nous disposons de moyens prodigieux pour étudier ces problèmes, ainsi que les autres aspects de l'astrophysique moderne. Dans environ 5 ans, nous aurons un ensemble de 15 télescopes de plus de 6m au sol. L'ESA et la NASA ont développé des programmes spatiaux très prospectifs pour placer en orbite des observatoires explorant le spectre électromagnétique rayonné par les sources extra-terrestres depuis les rayons  $\gamma$  jusqu'aux micro-ondes. Un certain nombre de télescopes seront dédiés à la recherche et à l'imagerie des planètes telluriques en orbite autour d'étoiles proches. L'instrumentation qui est et qui sera disponible à la fois au sol et dans l'espace est innovante et le plus souvent limitée par la physique plutôt que par la technologie. L'instrumentation astronomique et les télescopes bénéficient de façon inédite des nouvelles technologies (e.g. optique adaptative, matériaux allégés). Les ressources humaines et informatiques dirigées vers les problèmes astrophysiques auraient été inimaginables il y a 10 ans. L'astronomie est en train de devenir un domaine beaucoup plus interdisciplinaire, avec des liens étroits vers des sciences comme l'informatique (réseaux de neurones, projet Terapix), la biologie (l'astrobiologie est enseignée dans certaines universités) et la chimie (milieu interstellaire). L'intérêt du public pour les découvertes astronomiques les plus récentes est presque insatiable. Il arrive presque chaque jour que quelque article relié à l'astronomie paraisse à la une des principaux journaux. La NASA et l'ESA dépensent des sommes considérables pour les relations publiques et les cours d'introduction à l'astronomie sont parmi les plus fréquentés à l'université. La leçon à retenir ici est que l'astronomie est en train de changer radicalement, et que le CFHT doit réagir maintenant à ce changement pour conserver la place qu'il tient depuis 20 ans au plus haut niveau de la recherche en astronomie.

Quelle science sera la plus attirante pour un CFHT nouveau ou remanié? Nous avons atteint un consensus sur cinq questions qui présenteront le défi intellectuel le plus important pour l'astronomie dans les décades à venir, et pour lesquelles on peut attendre un réel progrès pour peu que l'on puisse disposer d'une instrumentation adéquate:

1. *Sommes nous seuls dans l'univers?*
2. *De quoi se compose l'univers et quelle est sa forme?*
3. *Comment notre système solaire et les autres systèmes se sont-ils formés et ont-ils évolué?*
4. *A quoi ressemblaient les premières étoiles et comment les galaxies qui les abritent se sont-elles formées?*
5. *Existe-t-il des choses dans l'univers que nous n'avons même pas encore imaginées?*

Le point commun à tous ces thèmes est l'obscurité.

1. Toute planète tellurique susceptible d'abriter la vie ne brille dans le visible et le proche infrarouge que par la réflexion de la lumière de son étoile, et donc doit être très faible. En outre, elle se trouve à

proximité d'une étoile semblable au soleil beaucoup plus brillante qu'elle-même et doit être très difficile à observer.

2. On sait que l'essentiel de la matière dans l'univers est non-lumineuse et cette matière noire, représentant entre 90 et 99% de toute la masse de l'univers, ne semble émettre de rayonnement dans aucune des bandes de longueurs d'onde explorées jusqu'ici.
3. La formation des étoiles et des systèmes planétaires a lieu dans des régions riches en poussière desquelles peu de rayonnement optique et ultraviolet nous parvient.
4. Avant la formation des galaxies et des étoiles massives, l'univers était essentiellement sombre.
5. Tout phénomène encore inconnu dans l'univers occupera, dans l'espace des paramètres observables, un domaine très éloigné de ceux déjà observés par les instruments sol et spatiaux. Pour cette raison, de tels objets, vus depuis la Terre, seront probablement très faibles quelle que soit la gamme de longueur d'ondes considérée.

Le comité NGC a développé une stratégie visant à la réalisation d'un instrument optique qui pourra illuminer un bon nombre de ces points sombres concernant l'univers dans lequel nous vivons. En marge de cette stratégie vers ces objectifs principaux, de nombreuses opportunités se présenteront pour des projets astronomiques intéressants et plus ciblés.

Pour atteindre les objectifs scientifiques que nous avons identifiés, le comité suggère le programme suivant de remaniement et reconstruction du CFHT, qui devra probablement être étendu à de nouveaux partenaires. Une possibilité réaliste identifiée par le comité consisterait en une fusion des communautés ESO et Gemini pour mener à bien ce projet.

**(1) Le CFHT existant devrait être exploité avec la plupart de ses instruments actuels (avec l'ajout possible d'un imageur grand champ infrarouge) pour encore une dizaine d'années.** Avec MEGACAM, OASIS, la bonnette d'optique adaptative, un spectrographe à haute résolution et un imageur grand champ infrarouge, le CFHT continuera à être compétitif dans de nombreux domaines de recherche (relevés de galaxies, faibles distorsions gravitationnelles, études de populations stellaires, observations détaillées de noyaux actifs de galaxies, spectroscopie stellaire).

**(2) Le Conseil d'Administration du CFHT devrait dès à présent initier une étude de Phase-A pour un très grand télescope optique, dans la gamme de 25m+. Cet instrument, optimisé pour l'imagerie et la spectroscopie, pourrait être construit sur le site actuel du CFHT.** Ce site reste le meilleur du Mauna Kea de tous les points de vue, et la communauté va vouloir continuer à l'exploiter. Ce télescope sera capable d'atteindre les objectifs scientifiques principaux cités plus haut et de plus, avec ses 25m de diamètre ou plus, il sera capable d'exploiter les nouvelles découvertes du Next Generation Space Telescope (NGST), actuellement prévu pour un lancement en 2007.

**(3) Si la construction de ce grand télescope est décidée, elle pourrait démarrer vers 2008, date à laquelle le CFHT actuel devrait cesser ses opérations.**

**(4) Si, à la fin des études de Phase-A en 2002 - 2003 ou pour toute autre raison que technique, il est décidé de ne pas poursuivre dans la voie de ce grand télescope, un plan de construction plus modeste pour le futur CFHT devrait être mis en oeuvre.** Un tel remaniement pourrait impliquer la construction d'un télescope moderne de 8m optimisé pour l'imagerie grand champ et la spectroscopie haute résolution. Une autre option, encore plus modeste, consisterait simplement à améliorer et

ré-instrumenter le télescope existant. Ceci aurait inmanquablement pour effet de faire passer le CFHT d'un observatoire de classe mondiale en télescope de soutien et de suivi pour les grands télescopes.

### 3. Executive Summary

The current era is perhaps the most exciting time in the history of astronomical research. The reasons for this are many. The depth of knowledge that astronomers now possess is deep and profound. Within less than a decade we should know the fundamental parameters of the Universe (its expansion rate, the mass density, whether there is a cosmological constant) to better than 5% and finally be in a position to state whether the Universe is open (will expand forever), closed (collapse back on itself) or critical (decelerate to zero expansion velocity after an infinite amount of time). We have some ideas (although not a complete theoretical picture) of how stars and galaxies formed and evolved. We have discovered planetary systems outside our own solar system. The resources available to attack these and other problems is prodigious. Within about 5 years there will be a suite of 15 6m+ telescopes on the ground. Both ESA and NASA have developed very ambitious space programs that will place observatories above the Earth to explore the electromagnetic spectrum from  $\gamma$ -rays to microwaves radiated by extraterrestrial sources. A number of telescopes are planned that will be devoted to locating and imaging terrestrial planets orbiting around nearby stars. The instrumentation that is and will be available to use with both the space and ground-based telescopes is innovative and in many cases is limited by physics not engineering. New technology is being applied to astronomical instrumentation and telescopes in a manner never seen before (e.g. adaptive optics, lightweight materials). Astronomy is becoming an increasingly interdisciplinary subject with close links to such subjects as computing technologies (e.g. neural networks, terapixel initiative), biology (astrobiology has become a discipline that can be studied at some universities) and chemistry (the interstellar medium). Public interest is almost insatiable for news of the most recent astronomical discovery. It is almost a daily occurrence that some story related to astronomy appears on the front page of major newspapers. NASA and ESA are both expending a major effort on public outreach and introductory astronomy courses are amongst the most popular at universities. The important lesson here is that astronomy is in the process of enormous change and CFHT must now react to this change and position itself to remain at the forefront of astronomical research as it has been for the past 20 years.

What science will be the most compelling for a new or refurbished CFHT to attack? Our consensus is that there are 5 questions that will provide the strongest intellectual challenge for astronomy over the next few decades and for which real progress can be made given the right mix of instrumentation.

1. *Are we alone in the Universe?*
2. *What is the Universe made of and what is its overall geometry?*
3. *How did our and other solar systems form and evolve?*
4. *What were the first sources of light in the Universe and how did galaxies like our own come into being?*
5. *Are there things in the Universe that we haven't as yet dreamed of?*

All of these themes are linked by the common thread of darkness.

1. Any Earth-like planets capable of supporting life on them will shine solely by reflected star light in the optical/IR and hence be very faint. They will also be in close proximity to a very much brighter sun-like star and thus be most difficult to observe.

2. It is known that most of the matter in the Universe is non-luminous and this “dark matter”, comprising between 90 and 99% of all the mass in the Universe, does not appear to be emitting radiation at any wavelength that has thus far been explored.
3. The formation of stars and planetary systems takes place in dusty regions from which little optical or ultraviolet radiation escapes.
4. Before the galaxies and the massive stars in them formed the Universe was largely dark.
5. Any as yet undiscovered phenomena in the Universe will occupy a part of observable parameter space that is far to one side of the regions that have thus far been explored with ground- and spaced-based instruments. For this reason, as seen from the Earth, such objects will likely be extremely dim at whatever wavelength they may radiate.

What the NGC committee has developed is a ground-based strategy for an optical instrument that can illuminate many of these currently dark issues concerning the Universe in which we live. Along the path to achieving these goals there will be the opportunity for numerous side-trips to other interesting and more detailed astronomical projects.

To achieve the scientific goals that we have recognized, the committee suggests the following replacement/refurbishment program for the CFHT.

**(1) The existing CFHT should be operated with most of its current suite of instruments (except for the possible addition of a wide field IR-imager) for about another decade.** With MEGACAM, OASIS, the Adaptive Optics Bonnette, a high resolution spectrograph and the wide field IR-imager CFHT will continue to be competitive in many areas of research (galaxy surveys, weak lensing, stellar population studies, detailed observations of active galactic nuclei, stellar spectroscopy).

**(2) Beginning almost immediately the CFHT Board should initiate a “Phase A” study of a large optical telescope, in the range of 25m+. This instrument, optimized for imaging and spectroscopy, could be built on the current CFHT site.** This site, by all accounts, remains the premier locale on Mauna Kea and the community will want to maintain its use of it. This telescope will be able to accomplish the prime scientific goals outlined above and in addition, at 25m+ it will have the minimum size capable of exploiting new discoveries made by the Next Generation Space Telescope (NGST) currently scheduled for launch by NASA in 2007.

**(3) If it is decided to build this large telescope, construction could begin in about 2008 at which time the current CFHT operations would cease.**

**(4) If, at the completion of the “Phase A” study in 2002 – 2003 or if, for other reasons, the decision is made not to go forward with the large telescope, then a more modest building plan for a future CFHT should be developed.** Such refurbishment might conceivably involve constructing a modern 8m instrument optimized for wide field imaging and high resolution spectroscopy. Another, even more modest option, would be to simply refurbish the existing telescope. This would definitely have the effect of reducing the profile of CFHT from a world-class observatory to one which operates mainly in a support role for larger telescopes.

## ACRONYMS

<b>2dF</b>	Two Degree Field
<b>2MASS</b>	Two Micron All Sky Survey
<b>AAT</b>	Anglo-Australian Telescope
<b>ACS</b>	Advanced Camera for Surveys
<b>ASO</b>	Astronomical Search for Origins
<b>AU</b>	Astronomical Unit
<b>BIMA</b>	Berkeley Illinois Maryland Association
<b>CBI</b>	Cosmic Background Interferometer
<b>CCD</b>	Charged Coupled Device
<b>CDM</b>	Cold Dark Matter
<b>CFHT</b>	Canada France Hawaii Telescope
<b>CFRS</b>	Canada France Redshift Survey
<b>CMB</b>	Cosmic Microwave Background
<b>CNOC</b>	Canadian Network for Observational Cosmology
<b>COB</b>	Cryogenic Optical Bench
<b>COBE</b>	Cosmic Background Explorer
<b>COROT</b>	Convection and Rotation
<b>DENIS</b>	Deep Near Infrared Survey
<b>ELT</b>	Extremely Large Telescope
<b>ESA</b>	European Space Agency
<b>ESO</b>	European Southern Observatory
<b>FIRST</b>	Far Infrared Submillimeter Telescope
<b>GTC</b>	Gran Telescopio Canarias
<b>HET</b>	Hobby-Eberly Telescope
<b>HST</b>	Hubble Space Telescope
<b>IR</b>	Infrared
<b>IRAS</b>	Infrared Astronomical Satellite
<b>ISO</b>	Infrared Space Observatory
<b>JCMT</b>	James Clerk Maxwell Telescope
<b>LBT</b>	Large Binocular Telescope
<b>LMC</b>	Large Magellanic Cloud
<b>LSA</b>	Large Southern Array
<b>MACHO</b>	Massive Compact Halo Object
<b>MAP</b>	Microwave Anisotropy Probe
<b>MOST</b>	Microvariability and Oscillations of Stars
<b>MOS/SIS</b>	Multi Object Spectrograph/Stabilized Imaging System
<b>MMA</b>	Millimeter Array
<b>MMT</b>	Multi-Mirror Telescope
<b>NASA</b>	National Aeronautics and Space Administration
<b>NGC</b>	Next Generation CFHT
<b>NGST</b>	Next Generation Space Telescope
<b>NL</b>	Netherlands
<b>OASIS</b>	Optically Adaptive System for Imaging Spectroscopy
<b>OGLE</b>	Optical Gravitational Lensing Experiment
<b>OVRO</b>	Owens Valley Radio Observatory
<b>OWL</b>	Overwhelmingly Large (Telescope)
<b>SALT</b>	South African Large Telescope

**ACRONYMS Con't**

<b>SCUBA</b>	Submillimeter Common User Bolometer Array
<b>SDSS</b>	Sloan Digital Sky Survey
<b>SETI</b>	Search for Extraterrestrial Intelligence
<b>SIRTF</b>	Space Infrared Telescope Facility
<b>SIM</b>	Space Interferometry Mission
<b>SKA</b>	Square Kilometer Array
<b>S/N</b>	Signal to Noise
<b>SOAR</b>	Southern Observatory for Astronomical Research
<b>SPIE</b>	Society of Photo-Optical Instrumentation Engineers
<b>TPF</b>	Terrestrial Planet Finder
<b>UH</b>	University of Hawaii
<b>UK</b>	United Kingdom
<b>UKIRT</b>	United Kingdom Infrared Telescope
<b>VIRMOS</b>	Visual and Infrared Multi-Object Spectrograph
<b>VLT</b>	Very Large Telescope

#### 4. Illuminating the Dark Universe: The Fundamental Science Goals for a Refurbished CFHT

Human curiosity about the Universe probably began when in prehistoric times an early human raised its eyes to the sky and wondered about the nature of the bright sources visible there. We have advanced from this level mainly due to the enormous support that the public has provided to astronomical research. At first sight this is surprising as astronomical research does not directly contribute to the betterment of life for most citizens. Such benefits do accrue, however, incidentally as astronomy often forces new technological developments. Astronomy’s appeal to the public exists because people are curious and want to understand their relationship with things that they see around them. The questions that people are interested in are the fundamental ones, “Are we alone in the Universe?”, “What is the Universe made of?”, “How did our Solar System form and evolve?”, “How did the galaxies form?”, “Do things exist in the Universe that we haven’t even dreamed of as yet?”.

It is not only the public that is interested in such questions, most astronomers are also. It is the meshing of these interests that has resulted in a science vision for a new CFHT and a definition of a telescope that is capable of making enormous progress toward answering these fundamental questions. Framed more directly as scientific queries, the basic questions that the new CFHT will have to address are as follows.

1. *Are we alone in the Universe?*
2. *What is the Universe made of and what is its overall geometry?*
3. *How did our and other solar systems form and evolve?*
4. *What were the first sources of light in the Universe and how did galaxies like our own come into being?*
5. *Are there things in the Universe that we haven’t as yet dreamed of?*

#### 5. Surveying the Current Astronomical Landscape

##### When the Universe was Young

The current basic model of the Universe is the Big Bang in which the Universe had an initial extremely hot phase and has expanded and cooled since its initial “Bang”. This paradigm has had remarkable success in predicting the existence of the Cosmic Microwave Background (CMB, the ubiquitous radiation field of the Universe), the abundances of the light elements in the Universe and the expansion. The physical nature of the observable Universe is usually described by a number of parameters from which its past and future history may be determined. Among these are the present average density of the Universe, the Hubble Constant (which measures the rate of expansion of the Universe) the age of the Universe, and the smoothness of the CMB. Unfortunately, at the current time most of these parameters are not very well known. By 2010, the second generation CMB satellites will have completed their missions and large ground-based millimeter array surveys of the CMB will be in progress. These programs will either determine most of the cosmological parameters or discover a major flaw in our ideas about the Universe.

In the earliest moments of the Big Bang, the Universe is predicted to have been remarkably isotropic and homogeneous. One of the great attractions of the inflationary model (which predicts that the Universe went through an epoch of extremely rapid expansion very early on in its evolution) is that such homogeneity arises naturally from a wide range of initial conditions. And yet the Universe that we see today is very

inhomogeneous on all except very large scales. This inhomogeneity is of course profoundly important, it led directly or indirectly to the emergence of complexity in the Universe on different scales and to the existence of essentially all phenomena that are studied in astrophysics and science generally, including life.

The generally successful theoretical paradigm for the development of density inhomogeneities in the expanding Universe is that small fluctuations in the density initially present grew through the process of gravitational instability. These initial density inhomogeneities may have been the result of quantum fluctuations that became imprinted on the Universe during the inflationary phase, although other possibilities have also been investigated. All structure in the Universe on galactic and larger scales is thought to have arisen through the processes outlined above. The nature of this structure can be studied from a variety of approaches. Locally this can be done from studies of our own and other nearby galaxies which give information on the distribution of masses of galactic systems while studies of the fluctuations on the last scattering surface of the CMB provides information on the largest scales.

The 1990's have seen great progress in observational cosmology, leading in many areas to a first overview of the observational situation and to a resulting sharpening of the observational questions. The major unanswered questions that are the focus of current research in this area are as follows.

1. *What is the origin of the fluctuations in the early Universe?*
2. *Do we get a consistent picture for these primordial fluctuations from the different approaches available?*
3. *Does the development of large-scale structure with epoch match expectations from the global cosmological model defined by the cosmological parameters?*
4. *What is the dark matter, and is the dominant component a function of scale?*

On smaller scales astronomers are concerned with how these fluctuations actually produced the galaxies we see around us. Some of the unanswered questions in this field are enumerated below.

1. *What were the first sources of light in the Universe?*
2. *How were galaxies assembled? Did this proceed hierarchically by merging? What was the role of dark matter?*
3. *How did the galaxies obtain their current shapes?*
4. *Regarding the Universe globally, what is its history of star-formation, metal enrichment and gas consumption?*
5. *When did massive black holes first appear in the Universe?*
6. *What are the connections between the formation of galaxies and the development of structures on larger scales?*

The scientific questions in observational cosmology outlined above represent the focus of activity for a substantial segment of the astronomical community and, correspondingly, are the motivation for much of the present investment in new observing facilities in space and on the ground. Therefore it is reasonable to expect substantial progress in addressing them over the next ten years and the state of the field in 2008 is thus quite hard to predict.

The study of large scale structure is primarily statistical, since it is thought to be the product of random processes occurring in the early Universe. Several new facilities will come on line in the next ten years that will extract essentially all of the available information from large scale structure and our knowledge will be to a certain degree limited by cosmic sampling – we have only one Universe available for study!

Several new CMB observational experiments, including Boomerang and CBI in the near future and the satellite observatories MAP and ultimately Planck (2007) will determine the CMB temperature fluctuations on all angular scales down to where the finite thickness of the last scattering surface erases information. While there may well be surprises, it is also possible that the spectrum will be a perfect match to the expectations of the currently popular model (cold dark matter, CDM) and a particular set of cosmological parameters. The latter would be a stunning achievement and mean that much of observational cosmology on the largest scales had been correctly understood.

The Sloan Digital Sky Survey (SDSS) project will measure the redshifts for the  $10^6$  brightest galaxies in the northern Galactic Cap, determining the 3-dimensional location of all the more massive galaxies out to a redshift of about  $z \sim 0.1$ , or a tenth of the horizon scale. At greater distance, evolutionary effects would in any case become important, and so SDSS will again be essentially limited by cosmic sampling. The SDSS will be preceded by a smaller but still very impressive survey ( $10^5$  galaxies) carried out on the AAT with the 2dF facility. The SDSS and other local surveys will be supplemented by very large surveys of order  $10^5$  galaxies at redshifts  $z \sim 1$  (e.g. the VIRMOS survey on the VLT). These will clearly show the development of large scale structure in the Universe.

Major progress in the field of weak gravitational lensing which is used to map the large scale mass distribution in the Universe, will come about through the implementation of wide field imagers (e.g. CFHT12k and MegaCam) with exquisite image quality on telescopes such as the CFHT.

The hectic progress in the area of high redshift galaxies during the last few years has been driven primarily by three new observational facilities that came on-line in the 1990's; the first years of operation of the Keck 10m telescope with its superior light gathering power, the introduction of the highly multiplexed MOS/SIS spectrograph on the CFHT, and the repair of the Hubble Space Telescope (HST) yielding kpc-scale resolution on high redshift galaxies. It should be noted that these facilities all operate in the optical waveband (from 0.3 to  $1 \mu$ ). Future progress is likely to be dominated by progress at longer wavelengths.

The near-infrared ( $1 - 5 \mu$ ) will be critical for two reasons. First, the most distant galaxies presently known have  $z \sim 6$ , and the regime of the earliest stars and galaxies in the Universe is almost certainly at  $z > 7$ . Galaxies at these redshifts are completely invisible at wavelengths less than  $1 \mu$  because of neutral Hydrogen absorption in the intervening Universe. Second, the  $1 < z < 5$  regime is likely where most of the stars seen today in the Universe were formed and where the different morphological components of galaxies that produce the characteristic Hubble sequence of galaxies today were put in place. The spectral range between 0.3 and  $0.7 \mu$  is rich in diagnostic features for stellar population ages, metallicities and kinematics and the spectrum at  $> 0.4 \mu$  is also essential for studying the older components of galaxies. This rich spectral range is redshifted into the  $1 - 5 \mu$  waveband in this crucial  $1 < z < 5$  redshift range.

The far-infrared,  $> 10 \mu$ , will also be critical. Locally, about 35% of stellar luminosity emitted in the optical and ultraviolet is absorbed by dust and is reradiated in the far-IR at a temperature of 30 – 60 K. Globally, the far-IR/sub-mm background that has been recently detected by the COBE satellite has the same or larger energy content as the optical background that is obtained by adding up the light from detected galaxies, indicating that dust absorption continues to be important at earlier epochs. Indeed the picture of galaxy evolution that has emerged over the last few years from optical studies is, literally, only half the story,

and very little has been known until now about the nature and redshifts of the sources producing the cosmic far-IR/sub-mm background.

While the requirements are demanding, several developments in the next ten years will almost certainly combine to produce great progress in this area. A much wider community will have had access to 8 – 10m class telescopes. By 2008 there will have been approximately 30,000 clear nights of time on such telescopes, accessed by astronomers in every major astronomical community. This can be compared to about 1000 nights available hitherto to a very restricted user community (Caltech and University of California/Hawaii). The sub-mm waveband around 1 mm is for the first time being opened up with instruments such as SCUBA on the 15m UK/Canada/NL JCMT on Mauna Kea and at IRAM where the potential of these instruments was recently demonstrated with the spectacular detection of *CO* in a galaxy at redshift 4.7. The sub-mm will continue to be developed with first and second generation interferometers such as OVRO, BIMA and in the future LSA/MMA. In space, SIRTf and FIRST will build on the pioneering ISO mission in the 10 – 100  $\mu$  waveband.

Multi-object spectrographs on 8 – 10m telescopes will dramatically extend the first exploratory redshift surveys carried out on CFHT – the  $10^3 - 10^4$  galaxies of the CFRS and CNOC surveys carried out with MOS on CFHT will be extended to samples of  $10^5$  galaxies in the VIRMOS project on VLT. Many of the new generation of 8 – 10m telescopes coming on line in the next five years are optimized for the near-IR (e.g. Gemini) and are achieving image quality (recently spectacularly demonstrated with the VLT) that is comparable to that achieved by HST in space. Scheduled for launch in 2007, NGST will be an 8m class telescope placed far from Earth where it will passively cool to about 30 K. With diffraction-limited optics, the sensitivity will be limited by the natural zodiacal light. The science mission of the \$1B NGST is squarely targeted at solving the science areas of galaxy formation and evolution outlined above.

### **Stars, Star Formation and Stellar Clusters**

We are currently witnessing a resurgence in the field of stellar astronomy. Many of the processes which occur in stellar interiors and play a major role in the way stars function and evolve, are extremely complex and essentially are not understood. These phenomena include convection, turbulence, mixing, transport of angular momentum, the origin of the stellar dynamo and are either totally ignored or grossly parameterized in current modelling of stellar evolution. Other processes occurring at the surface of stars or in their immediate environment also play a decisive role in several stages of stellar evolution, and are basically unknown. Among these are disk accretion, jet collimation, driving of winds from stars and from accretion disks, magnetic phenomena in stellar winds and circumstellar disks.

The attention of the stellar astrophysics community in the coming decade will be directed toward the detailed understanding of these processes. This will require a substantial theoretical effort which must be accompanied by serious observational progress in order to provide the adequate constraints that our models are lacking today.

The physical processes at work in stellar interiors are usually studied by the observation of their manifestation at the stellar surface, most often by high resolution, high S/N spectroscopy. Convection controls the transfer of energy in a significant fraction of stellar interiors. Granulation is one of its surface manifestations, and can be studied by observing the shapes of spectral lines in various types of stars. Turbulence in stellar interiors results in mixing of chemicals, and affects the surface abundances. The measurement of these abundances provides us with an indirect tool for studying turbulence inside stars. Transfer of angular momentum in the internal layers is also an important pending problem. Measurements of surface rotation and surface differential rotation constitute the only conventional tool to study this problem.

This can be achieved either statistically on ensembles of stars by measuring the projected rotation velocity, or more directly by measuring the rotational modulation of stellar light (photometric monitoring) and of stellar line profiles (spectroscopic monitoring). Dynamo generation of magnetic fields in stellar interiors is another unsolved major problem in stellar physics. Observations of surface magnetic field geometries are used to constrain dynamo theories, and can be obtained using the methods of Doppler and Zeeman-Doppler imaging. These methods require high resolution ( $R > 30,000$ ) spectroscopy and spectropolarimetry at very high S/N ratios, from several hundreds for Doppler imaging to several thousands for Zeeman-Doppler imaging, but multi-line techniques reduce considerably these requirements. Monitoring of the stars during several rotational cycles is necessary to apply these imaging methods.

All of these observational tools have been applied to bright stars in the past few years, and have yielded important results concerning the study of the processes listed above. The next important step will be to study how these processes evolve during stellar evolution, and therefore to apply the above tools to ensembles of stars offering a complete age sequence, including pre-main sequence stars and members of stellar clusters.

In addition to these indirect tools for studying the processes at work in stellar interiors, asteroseismology is developing quickly, and is likely to provide us in the near future with direct probes of stellar interiors. Asteroseismology consists of the detection of stellar oscillation modes, and the measurement of their frequencies, amplitudes, and lifetimes. These measurements require very high precision photometry or spectroscopy as well as monitoring for a long enough period of time to yield sufficient frequency precision, typically two weeks or more.

While we have basic ideas on the structure and evolution of stars, their formation remains a key unsolved problem in astrophysics. Without a good theory of the star formation process, no complete understanding of the origin of planetary systems or galaxy evolution can be achieved. Tremendous progress has been made in this field in the last 20 years following the development of infrared astronomy, both from space – (e.g. IRAS, HST) and ground-based facilities. For instance, the circumstellar environment around protostars are better known, important results have been established on the global star formation activity in galaxies, and some knowledge on the level of star formation in high-redshift objects has been gathered. However, many crucial questions remain to be answered.

1. *What triggers star formation ?*
2. *What are the main parameters (e.g. mass, gas fraction, morphological type, bar, environment) controlling the global star formation rate in galaxies?*
3. *How are the observed bursts of star formation triggered in the nuclei of some galaxies? Is there a relationship between the presence of a starburst and the properties of the host galaxy?*
4. *What was the star forming activity in protogalaxies ?*

It is actually rare to have a star form in isolation. They tend to form in pairs, triple or higher order systems and very often in clusters. Star clusters are gravitationally bound groups of stars that all formed at the same time, likely from the same gas cloud. These objects are the laboratories for studying the ages of the stars, how stars evolve, the effects of differing metal abundances on this evolution, and the dynamical evolution of hundreds or thousands of gravitationally bound stars. One example of the great utility of star clusters is that the most precise determination of the age of the Universe comes from old globular star clusters in our Galaxy.

The study of star clusters and large samples of individual stars in the Milky Way Galaxy are fields in which explosive growth is expected during the decade ahead. Enormous progress is anticipated on several fronts, as a wide variety of new telescopes, both large and small, are poised to begin operation. All-sky, near-IR surveys such as DENIS and 2MASS will provide magnitudes and colors for several hundred million Galactic stars. The SDSS will revolutionize studies of Galactic structure by measuring magnitudes for  $\geq 50$  million stars; low-resolution spectroscopy for several million of these will be obtained during the second, spectroscopic, portion of the survey. Meanwhile, 15 telescopes having apertures in the range 6 – 10m are set to begin operation before 2003 (see Tables 3 and 4) including seven in the south, a hemisphere in which only one telescope larger than 4m is currently in operation. Yet this hemisphere contains the best and brightest Galactic globular clusters, the Galactic bulge and several of the nearest Local Group galaxies, including the Sagittarius Dwarf Galaxy (which is currently being absorbed into our own Galaxy) and the Magellanic Clouds.

Important questions which may remain unanswered by 2010 include the following.

1. *What is the number and distribution in mass of the oldest stars?*

Microlensing surveys such as MACHO and OGLE have provided evidence that  $\sim 50\%$  of the mass of the halo of our Galaxy is in the form of dark, 0.5 solar-mass objects (presumably white dwarfs). Since globular clusters are prototypical old objects and should contain large numbers of white dwarfs, it should be possible to detect the dynamical signature of such white dwarfs in these old clusters. At present, such studies are possible with relatively nearby globular clusters, but a thorough understanding of their contribution to the outer halo and bulge of our Galaxy is probably beyond the capabilities of planned 8m-class telescopes.

2. *What is the age of the Galaxy from the cooling of white dwarfs*

As white dwarfs age they cool, so that the temperature of the white dwarf can be viewed as a cosmic clock. The search for very cool (and hence very old) white dwarfs in the halo of our Galaxy is certain to be a high-priority goal during the decade ahead. In principal, this is a feasible, though challenging, project for a 4m-class telescope equipped with a wide-field, mosaic camera and a suitable set of narrow-band filters. Nevertheless, sample contamination by white dwarfs in the disk of our Galaxy is expected to be a concern and, ideally, the halo sample should not be drawn locally but should instead be selected *in situ* (at distances greater than  $\sim 3$  kpc above the Galactic plane). Identifying such a sample would require not only a very large telescope, but also the ability to perform accurate astrometry on very faint stars ( $V \sim 30$ ).

3. *What are the element ratios among stars in the halo and the bulge of our Galaxy?*

The chemical composition of old, metal-poor halo and bulge stars provides information on the early chemical homogeneity and star formation history of the Milky Way Galaxy as well as constraining nucleosynthesis in the Big Bang. High-resolution spectroscopy from forthcoming 8m-class telescopes will provide much new information on the element ratios in bulge and halo stars (perhaps telling us if they shared a common origin), yet high-resolution, high-S/N spectroscopy of many of the most interesting targets (*i.e.*, distant halo dwarfs and highly reddened bulge dwarfs) will be impossible even with the new large telescopes.

4. *How dark are dwarf spheroidal galaxies?*

The large number of faint dwarf galaxies and their apparently high mass-to-light ratios indicate that they may be a major contributor to the mass budget of the Universe. At present, however, the evidence for dark matter in these faint galaxies is limited to the anomalously high central velocity dispersions of the Draco, Ursa Minor and Sagittarius dwarfs: the three nearest galaxies, and three dwarfs which are most susceptible to Galactic tidal effects. Several additional dwarfs will soon become available in the south (*e.g.* Phoenix,

Tucanae) but even with 10m-class telescopes, measuring internal velocity dispersions for these extremely low-surface-brightness galaxies becomes very difficult at the distance of the Andromeda Galaxy, the nearest galaxy to us that is similar to the Milky Way. Thus, the question of the dark matter content of dwarf spheroidal galaxies is likely to remain an open one until a much larger sample of dwarfs, spanning a wide range in environment, have had their internal kinematics studied by larger telescopes.

After 2010, NGST will completely dominate research on star clusters and Galactic structure *in the near IR*. Nevertheless, NGST's modest aperture, small field-of-view, low spectral resolution and limited (and, possibly, non-existent) performance in the optical regime suggest that a large optical telescope (*e.g.* diameter  $\geq 25\text{m}$ ) having superb image quality over a large field-of-view and the capacity for high-resolution, multi-object spectroscopy would be an extremely competitive instrument for studies of Galactic structure, Local Group globular cluster systems, nearby dwarf galaxies and stellar structure.

### **The Search for Life on Extrasolar Planets**

In a mini-survey of members of the committee in which they were asked what single observation they would like to do in the next few decades, the almost unanimous answer was the detection of a terrestrial planet around a nearby star and confirmation that life could be present on that planet. Such an observation would galvanize the general public, alter opinion about our purpose in the Universe and have a profound effect on political, philosophical and religious thought. There is of course no guarantee that such objects exist, but surely we must search for them.

There are several routes to a firm detection of a life-bearing terrestrial planet in orbit around a solar-type star. But, to be convincing, the planet must be imaged and identified from its gross photometric properties as a likely terrestrial planet. Eventually, a spectrum must be obtained of the planet and shown to contain evidence of life-supporting gases ( $\text{H}_2\text{O}$ ,  $\text{CO}_2$ ,  $\text{O}_2$ ,  $\text{O}_3$ ).

A first step toward such detection might be indirect. Radial velocity variations of the star in the planetary system could be evidence of the presence of planets. Such work is currently being actively pursued at a number of observatories including CFHT, Keck and ESO. A Jovian planet will cause velocity variations in a solar type star on the order of 10 - 100 m/s. Such measurements are well within current technology and periodic velocity variations have been found in about a dozen systems to date. By 2010 surveys for in excess of several thousand stars will have been completed and there will likely be a sample of more than 100 known systems containing giant planets. A terrestrial planet will produce orbital motion in the central star amounting to  $< 1$  - 10 m/s. The low end of this range is not currently achievable and in any case may be dominated by velocity noise from the surface of the star itself and be difficult to detect. The interpretation of all such observations suffers from uncertainty in the inclination of the planet's orbit which makes the mass determination of the planet ambiguous. This technique also naturally selects out those orbits which are the smallest. Further, real situations may be very complex with a number of Jovian and terrestrial planets co-existing in the same system causing difficulty in the interpretation of the velocity variations.

Other possible indirect approaches are astrometry, occultations and microlensing. In astrometric observations of a solar-type star a Jovian planet would produce an orbit of the star with an amplitude amounting to 10 - 100  $\mu\text{arcsec}$  and terrestrials about one-tenth these values. Such measurements will have to await the interferometers current being planned for space. The presence of an orbiting planet could also be inferred from observed periodic occultations of its parent star. The brightness change is caused by the planet blocking out a small fraction of the star light and can yield the planetary period, distance from the star and the inclination of the orbit. This is one of the few methods capable of detecting a terrestrial planet in the habitable zone (the temperature of the planet being such that water is largely in the liquid state) of their

parent star. These observations require photometric accuracy to about 1 part in  $10^5$  and is a good project for a small dedicated telescope in space. COROT, a 27cm telescope funded largely by the French Space Agency and scheduled for launch in May 2002 will attempt such observations but they are very challenging. As regards microlensing, the presence of a planet orbiting a star that is being gravitationally lensed produces a lensing signature that is unique and distinct. While a planet may be found with this technique, it is pure serendipity and will be difficult to follow up.

The direct approach to terrestrial planet detection will be the most convincing: obtain an image of the planet and a spectrum to illustrate its atmospheric composition. This project is extremely difficult, the main problem being that the planet is quite faint and close to a star that is much brighter. This brightness ratio is about a factor of  $10^6$  at  $10 \mu$  increasing to  $10^9$  at  $1 \mu$ . The main limitation in detecting an object with such a large contrast ratio is scattered light caused by diffraction of the telescope aperture, the Earth's atmosphere, and the quality of the telescope optics. The Earth's atmosphere is the overwhelming problem here causing image spread, speckles and absorption bands in just the wrong places. The scattered light from each of these can be significantly reduced with, respectively, a coronagraph, adaptive optics and super smooth mirror optics. A planet in the habitable zone of a star 10 pc away from Earth appears separated from it by 0.1 arcsec. The diffraction limit of a 25m telescope at  $1 \mu$  is 0.01 arcsec.

## 6. What New Telescope is Required to Make Progress on the Key Scientific Goals?

The five key science goals for astronomy in the coming decades were identified by the committee as follows:

1. *Are we alone in the Universe?*
2. *What is the Universe made of and what is its overall geometry?*
3. *How did our and other solar systems form and evolve?*
4. *What were the first sources of light in the Universe, and how did galaxies like our own come into being?*
5. *Are there things in the Universe that we haven't as yet dreamed of?*

In assessing the kind of observations that would be most effective in addressing these questions, the committee identified the following as key ones.

1. *Are we alone in the Universe?*

Obviously the single most important observation would be the detection of signals of an unambiguously artificial nature that could only have originated from intelligent life. The SETI searches in the radio waveband are the best hope for this. This is outside the purview of the NGC study. Likewise, exploration of the Solar System (e.g. Mars or the moons of the outer planets) may yet turn up evidence that life started independently elsewhere. Failing these dramatic developments (and rather more plausibly), we envisage a step-by-step approach involving the following.

- a. **Characterization of extra-solar planetary systems using indirect search techniques** (i.e. the reflex motions of the primary). At present, the emerging view of extra-solar planetary systems is driven by the selection bias in favour of massive planets near to their parent stars. Future improvements can

be imagined in **astrometry** (e.g. with the Space Interferometer Mission) and in higher **precision radial velocity measurements** that will lower the planetary masses that can be detected through these indirect means. However, there is likely to be a limit imposed here by the velocity noise of the star itself so that more than just light gathering power by itself will be required.

b. **Direct detection of terrestrial planets** through the isolation of their light. Terrestrial planets are not outstandingly faint (typical visual magnitude 26 around nearby stars), but their direct detection requires exquisite suppression of the light from the much brighter star (e.g.  $10^9$  times brighter at  $1 \mu$ , 0.2 arcsec away). Detection of the planet’s own thermal radiation (e.g. at 10 micron for a planet in the “habitable zone”) reduces the required contrast relative to the star to  $10^6$  at the expense of poorer resolution. Thus direct detection requires a combination of **very high resolution, super-smooth optics and advanced suppression mechanisms** (i.e. coronagraphic or null-interferometric techniques).

As a concrete example of what will be required for such a detection consider a terrestrial planet orbiting the nearby star  $\tau$  Ceti at 1 AU. The separation from the star is 0.3 arcsec and the planet’s H-magnitude is about 26. Assuming that we can largely correct for the speckle noise and have a Strehl ratio of 0.8 in the image, this planet could be detected with a signal-to-noise ratio of 3 in about 12 hours of observing time with a 25m ground-based telescope. This estimate neglects any contribution from possible zodiacal light in the  $\tau$  Ceti system and it also assumes that such a high Strehl ratio could be obtained with a large, likely segmented mirror. This has yet to be shown and **requires a real technological advance before it can be achieved.**

In any case, to achieve the resolution required to adequately suppress the stellar light in the IR/optical and detect the planet’s reflected radiation, interferometric baselines or apertures of at least 25m are needed.

c. **Detection of life gases.** A spectrum of a terrestrial planet orbiting a nearby star would take about a week of time with a 25m telescope and be restricted to finding  $O_2$  in the optical/near infrared region as the remaining “life” gases only have spectral signatures in the 3 - 20  $\mu$  region. This is a part of the spectrum much better accessed from space and will be the prime goals of DARWIN and TPF. However, this 25m telescope could provide candidates for further spectroscopy with NGST and other space observatories.

## 2. *What is the Universe made of and what is its overall geometry?*

The nature of the “dark matter”, believed to be the dominant form of matter in the Universe, is unknown and it is, by nature, difficult to detect with telescopes. It is possible that direct detection by particle detectors on Earth or a breakthrough in theoretical particle physics may solve this question, but this is by no means certain. The role of astronomers is to characterize the average density of the dark matter, the power spectrum of density perturbations in the Universe, to determine the relationship between the distribution of luminous material and to determine how this changes with cosmic epoch. There are three ways to detect accumulations of dark matter - through observable scattering effects at the last scattering surface of the CMB, through gravitational lensing effects on background sources of light and through large-scale dynamical effects acting on luminous test-particles (i.e. galaxies).

The overall geometry and dynamics of the Universe is described by the classic cosmological parameters and will reflect the dark matter and other components to the mass density. At present, there is renewed speculation in the possible existence of a false vacuum term which would produce an acceleration of the Universe as opposed to the more conventional deceleration, and much work over the next decades will be focussed on pinning this down. Demonstration of the existence of this term would represent a profound contribution to physics to rival the discovery of gravity in the 1700s and of the expansion of the Universe in the 1920’s.

The primary observational capabilities that will be required to tackle these problems are enumerated below.

(a) Measurements of the CMB anisotropies on all angular scales greater than 2 arcmin (e.g. with MAP and Planck). If there are no surprises, this has the potential for determining the cosmological parameters to exquisite precision (though with some degeneracies) and for determining the spectrum of fluctuations of the dark matter.

(b) Undertaking the classical cosmological tests to yield estimates of the age of the Universe and the deceleration parameter  $q_0$  can break the degeneracies present in (a) and provide a fundamental consistency check. Much of this work, for instance the study of supernovae at high redshifts, is well suited to wide field 4m class telescopes (e.g. CFHT) with follow-up from 10m class telescopes such as Gemini. NGST should extend this program to  $z > 1$  and a very large ground-based telescope would be a powerful complement to NGST, especially in the **area of spectroscopy**.

(c) Mapping the large scale structure of the Universe will be best undertaken by 2 – 4m class dedicated telescopes like the SDSS which will map the distribution of  $10^6$  galaxies over a scale of 1/10 of the horizon. Dynamical studies of bulk flows and large collapsed structures such as clusters will also be carried out on 4m class telescope. At high redshifts, 10m class telescopes on the ground will pursue these programs to  $z \sim 1$  and likely beyond. **The highest redshifts will demand levels of sensitivity that could be attained only with NGST or a very large ground-based telescope.**

(d) Mapping the dark matter distribution through gravitational lensing effects is a key goal of CFHT's wide field imaging surveys - the requirement is for the highest possible image quality sustained over the largest possible fields. Again, **NGST or a very large ground-based telescope will extend these studies in a more specialized regime at  $z > 1$ .**

### *3. How did our and other solar systems form and evolve?*

Developing a better understanding of the star formation process (and at the same time how planets might form) means investigating phenomena on very different scales. The key word thus describing any facility for this work is probably **versatility**. Superb image quality with both **adaptive optics** systems and **wide field imagers** appear necessary as well as an **integral field spectrograph** (or a Fabry-Perot interferometer) and a high resolution spectrograph. Two options thus appear suitable for the next CFHT facility.

(a) **A large ( $\geq 25\text{m}$ ) telescope with superb image quality** (adaptive optics system) equipped with an integral field spectrograph and/or a high resolution spectrograph. This facility should be optimized for the visible and the near-infrared and could study the star formation rate in galaxies at high redshift and even view individual high mass stars being formed at redshifts beyond  $z \sim 1$ . It would also open up the possibility of measuring oscillation frequencies of stars in the nearest open clusters, thus providing decisive tests of stellar evolution theory.

One clear advantage of such a telescope on the ground compared to NGST will be its superior spectroscopic capabilities. For example, for a protostellar system in our Galaxy, a 25m NGC could trace the gas falling down from the outer edge of the protostellar envelope in to the stellar seed (i.e. resolve protostellar disks on scales of 2 - 5 AU). A spectral resolution of 100,000 coupled with the large collecting area would allow for measurements of velocities to a few km/s of faint gas clumps and permit us to estimate the mass infall rate. If planets are formed during the accretion phase, high spectral resolution is necessary to observe the gaps in disks via the shapes of the absorption lines. The spectroscopic capabilities of the 8m-class telescopes will just barely allow this kind of observations but the integration times will be very long ( $\sim 10$  hrs). By contrast a 25m

NGC will not only allow investigation of very specific targets but also permit surveys of protostellar systems to be carried out searching for planetary systems in formation.

The star formation activity in galaxies of different morphologies, environment and epoch varies by many orders of magnitude. The origin of these differences is still unclear but a large aperture telescope will allow one to study star formation in faint protogalactic systems (a supporting role for NGST) and study the distribution of star forming regions in individual objects at high  $z$ . Equipped with **integral field units** and a spectral resolution of a few thousand, imaging spectroscopy of faint irregular galaxies up to  $z \sim 1 - 2$  will allow us to derive their star formation properties (i.e rates, distribution, abundances). Stellar populations of nearby galaxies ( $< 25$  Mpc) could be studied spectroscopically for individual objects to derive the star formation history of these systems, including their chemical evolution which is a long-standing problem in galaxy evolution studies.

(b) Since a detailed understanding of the physical processes at work inside a star is required before complex models of its formation and evolution can be developed, a future CFHT can be optimized for high spectral resolution studies including asteroseismology. For this purpose a telescope of the 4 – 8m class equipped, at a minimum, with an efficient echelle spectrograph/spectropolarimeter will be needed for most of the stellar physics programs foreseen for the next decade or more. Such a facility will indeed give access to the study of the physical processes in both individual nearby stars and members of young open clusters and pre-main sequence objects. Also, based on existing asteroseismological results on 2m class telescopes, it can be expected that solar-like oscillations will be detected down to about 7th magnitude with an 8m telescope. This will give access to a robust stellar sample.

Another important observational aspect of stellar physics in the future will be the need for continuous spectroscopic and spectropolarimetric monitoring. This need implies that if the new CFHT is in the 4 – 8m class range it must be operated in such a way that long observing runs can be attributed to specific stellar physics programs, and that CFHT observations can be part of world-wide multi-site campaigns involving other telescopes of the same class.

4. *What were the first sources of light in the Universe, and how did galaxies like our own come into being?*

This science goal requires **extreme sensitivity and good angular resolution** because the interesting objects are extremely distant as they must be observed in the early Universe. Because the light from the earliest objects is highly redshifted, observations in the **near-infrared** are extremely important, both for detecting older stars at high redshifts ( $z \gg 1$ ) and, crucially, for detecting the first stars formed at  $z > 6$ . We also know that about 50% of the light from star-formation at high redshift is obscured by dust and reradiated in the far-IR and observations in the **sub-mm waveband** are required to follow this component. Thus many facilities will be required to yield a comprehensive picture of how galaxies such as our own formed and evolved and how the Universe first “lit up”, ending the darkness which followed the Big Bang.

(a) **The first stars and galaxies and the assembly of mass:** NGST will have exceptional sensitivity in the near-IR and will make major contributions to this area. With its spatial resolution of a fraction of 0.1 arcsec, corresponding to kpc scales at cosmological distances, it will allow the separation of different morphological components of galaxies and the determination of gross physical properties (such as chemical abundances) across the galaxies. Thus NGST should enable us to follow the assembly of material into galaxies by detecting sub-galactic fragments even to very high redshifts. A very large aperture ground-based telescope (25m+) would act as a powerful complement to NGST in the optical and near-infrared (even without full diffraction limited-performance) by having enough aperture to perform the required spectroscopy on the galaxy fragments.

(b) **The hidden phases of galaxy evolution:** FIRST and the future millimeter-wave arrays (LSA/MMA) will also be very important as detectors of dust emission and line studies of the gas. The latter will provide images with comparable (kpc-scale) resolution as will be obtained with NGST.

(c) **Breaking open galaxies for observations:** Finally, a very large aperture diffraction limited ground-based telescope would offer a whole new capability – especially at visible wavelengths where the gain relative to NGST would be maximized. First, the exquisite resolution and sensitivity would be sufficient to study individual objects (such as star-forming regions, even the brightest individual stars) within galaxies at redshifts as high as  $z \sim 1$ , i.e. when the Universe was about 1/3 its present age. This would transform our view of galaxy evolution – galaxies would be observed as composed of individual interacting objects rather than as overall “systems”. Likewise the gain in visible sensitivity and resolution would enable detailed studies to be made of the stellar content of local galaxies – a 25m telescope would detect moderate mass stars in the Virgo cluster, vastly increasing the range of galaxies for which their past history could be determined.

While many questions posed above will be tackled by the NGST, this telescope will have its own limitations as it will be a modest sized instrument (with limited capabilities in the optical waveband, spectroscopy and wide field imaging). It appears necessary to complement NGST (and the future LSA/MMA) with a large aperture ground-based telescope, in much the same way HST and the 8 – 10m telescopes are complementing each other today. To access LMC-type building bricks of galaxies at  $z \sim 3 - 10$  will require a facility capable of reaching magnitudes in the range of 28 – 29 in spectroscopic mode. A ground-based telescope ( $> 25\text{m}$ ), with full adaptive optics, multi-object and integral field spectroscopy will be required for this task.

*5. Are there things in the Universe that we haven't as yet dreamed of?*

By the very nature of this question, it is impossible to predict what observational facilities will be required to discover completely new types of objects or phenomena. The history of astronomy suggests that performance gains of several orders of magnitude are required to discover previously unsuspected phenomena or objects. Smaller gains in performance, while invaluable in the progress of the discipline, lead to evolutionary gains rather than revolutionary gains. A recent example of such revolutionary gains might be the BeppoSax satellite which, with its wide spectral coverage and extraordinary positional accuracy, has revolutionized the study of gamma-ray bursters. Among the range of facilities now being built or planned, we identify the following as instruments which have the potential for major surprises.

- 1) NGST and FIRST in the 10 – 200 micron range where they will achieve 1000 – fold gains in sensitivity.
- 2) Kilometer-scale interferometric arrays on the ground or in space, operating at visible to mid-IR wavelengths and offering the potential for 1000 fold gains in spatial resolution.
- 3) The sub-millimeter/millimeter array, offering enormous gains in resolution and sensitivity.
- 4) Extremely large centimeter arrays on the ground such as the SKA.
- 5) The MAP and Planck microwave satellites which will undertake the mapping of CMB anisotropies over all angular scales greater than 10 arcminutes, to a level of accuracy set by astrophysical limits,
- 6) A ground-based optical-IR telescope having a minimum diameter of 25m. Such an instrument would represent a factor of 6 increase in the collecting area of the primary mirror over planned or existing facilities. This increase is identical to that achieved by the Keck telescope when it began operation in 1993. Many of the scientific gains realized by Keck are better classified as revolutionary rather than evolutionary, particularly in the fields of high-redshift galaxies, quasar absorption line systems and gamma-ray bursters. A 25m+ telescope will certainly play a similar role and enjoy comparable gains. It is also worth noting that such an instrument

would result in a factor of 5 gain in spectroscopy over NGST (at  $1\ \mu$ ) — itself a revolutionary instrument — and potentially much larger gains in the optical, where NGST’s performance is expected to be limited or non-existent.

## 7. What Ground – Based Telescopes Will be Available in 2010?

At the time of the NGC meeting in April 1998, only two optical/IR telescopes having diameters greater than 6.5 meters were in routine operation. Yet it is expected that more than a dozen such facilities will be operating just five years from now. Commensurate advances in wide-field imaging capabilities, adaptive optics and detector technology (particularly in the infrared) all point to a period of explosive growth in the field of ground-based, optical/IR astronomy.

### Intermediate Telescopes

It is widely believed that, in order to achieve maximum productivity, the new generation of large telescopes (*i.e.*, having diameters greater than 6.5m) should be “fed” by smaller facilities. A large number of such facilities are now in routine operation and several new 4m-class telescopes are in various stages of design and/or fabrication (*e.g.* Galileo and SOAR). Tables 1 and 2 list all 4m-class optical/IR telescopes which are likely to be in operation during the upcoming decade. With the exception of UKIRT, most (but by no means all) of the instruments available for scheduling on these telescopes are designed for use in the optical regime — a reflection of the predominantly optical background of their user communities. Nevertheless, since many of the new 8m-class telescopes will be optimized for IR observations, most of these smaller facilities are heavily committed to the development of infrared instruments. Similarly, most of the existing and planned 4m-class telescopes are, or soon will be, equipped with wide-field CCD cameras.

### Large Telescopes

Tables 3 and 4 provide some rudimentary information on the new generation of 8m-class optical/IR telescopes. The columns record the telescope, the properties of the primary mirror, the telescope design, the anticipated “first light”, the partner institutions and a summary of the first-light instruments. For the Keck telescopes, second generation instruments are also included.

Fifteen telescopes having diameters greater than or equal to 6.5m are now either completed, under construction or fully funded. Although final funding for the SALT (South African Large Telescope) has yet to be obtained, this is likely to occur within the year and will raise of the total number of large telescopes to sixteen. The southern hemisphere will experience particularly heavily growth since the number of large telescopes there will rise from zero to at least seven.

One interesting aspect of these new facilities is the diversity of their designs and budgets. By utilizing a segmented mirror design, and by sacrificing full sky access and imaging capabilities, the Hobby-Eberly telescope (built by a consortium of American and German universities) is nearing completion for a total budget of \$15M US, not including instruments. By contrast, ESO’s VLT program is likely to cost upwards of \$0.5B US. Versatility seems to be of importance for the CFHT community. There are associated costs with this choice.

## 1. 4m-Class Optical-IR Telescopes (Northern Hemisphere)

Telescope	Primary	Design/Focii	1st Light	Instruments
Palomar	5.0m	Cassegrain f3.3,f9,f70,coudé	1950	OS (LR, MR, HR coudé, HR echelle, multi-fiber, multi-slit, fabry-pérot) IRS (LR, MR), polarimetry, OI, IRI (NIR, MIR+AO)
KPNO	4.0m	Ritchey-Chrétien f3.1,f8,f15	1974	OS (LS, MR, HR echelle, multi-slit) IRS (NIR, MIR, LR, MR, HR, multi-slit) OI (WFMC), IRI (NIR)
CFHT	3.6m	Cassegrain f4.2,f,8,f35,coudé	1979	OS (LR, MR, HR coudé, fabry-pérot) IRS (NIR, IFS, FTS, multi-slit), OS (WFMC), IRI (NIR), AO
UKIRT	3.8m	Cassegrain f35	1979	IRS (NIR, MIR) IRI (NIR), NIR polarimetry
Calar Alto	3.5m	Ritchey-Chrétien f3.93,f10,f45	c1985	OS (LR, MR, multi-slit, multi-fiber, fabry-pérot), polarimetry, OI (AO), IRI (NIR+AO)
WHT	4.2m	Ritchey-Chrétien f2.8,f11 (Nasmyth) f11 (Cassgrain)	1987	OS (LR, MR, HR echelle, fabry-pérot multi-fiber, multi-slit) OI, IRI (NIR+TT)
WIYN	3.5m	Ritchey-Chrétien f6.5 (Nasmyth) Cassegrain	1994	OS (LR, MR, HR echelle, all multi-fiber), OI (TT)
ARC	3.5m	Ritchey-Chrétien f5,f10,f20	1994	OS (LR, MR, HR echelle) IRS (NIR, multi-slit), OI (WFMC), IRI (NIR)
Galileo	3.6m	Ritchey-Chrétien	1998	OS (LR, multi-slit, HR echelle) IRS (NIR, LR, MR, HR), OI (TT), IRI (NIR, AO)

### Abbreviations in Tables 1 and 2:

OS = Optical Spectrographs

IRS = Infrared Spectrographs

LR = Low Resolution ( $\lambda/\Delta\lambda \leq 1,000$ )

MR = Medium Resolution ( $1,000 \leq \lambda/\Delta\lambda \leq 10,000$ )

HR = High Resolution ( $\lambda/\Delta\lambda \geq 10,000$ )

OI = Optical Imagers

WFMC = Wide Field Mosaic Camera

IFS = Integral Field Spectroscopy

AO = Adaptive Optics

TT = Tip Tilt

NIR = Near Infrared ( $1-5\mu\text{m}$ )

MIR = Mid Infrared ( $5-20\mu\text{m}$ )

IRI = Infrared Imagers

## 2. 4m-Class Optical-IR Telescopes (Southern Hemisphere)

Telescope	Primary	Design/Focii	1st Light	Instruments
CTIO	4.0m	Ritchey-Chrétien f2.87,f8,f30 f15(TT)	1975	OS (LR, MR, HR echelle, multi-fiber, fabry-pérot), IRS (NIR, MID, TT), OI (WFMC), IRI (NIR, MIR)
AAT	3.9m	Ritchey-Chrétien f7,9,f15,f36 (IR) f36 coudé	1975	OS (LR, MR, HR echelle, HR coudé, multi-slit, multi-fiber), IRS (NIR, IFS) OI, IRI, polarimetry
ESO	3.6m	Ritchey-Chrétien f8,f35,coudé	1977	OS (LR, MR, HR coudé, HR echelle) IRI (MIR + AO), polarimetry
NTT	3.5m	Ritchey-Chrétien f11	1989	OS(LR, MR, HR echelle, multi-slit) IRS (NIR), polarimetry, OI, IRI (NIR)
SOAR	4.0m	Ritchey-Chrétien f10,f15	2001	OS (LR, MR, fabry-pérot) IRS (NIR, MR, HR) OI, IRI (NIR), AO

### Abbreviations in Tables 1 and 2:

OS = Optical Spectrographs

IRS = Infrared Spectrographs

LR = Low Resolution ( $\lambda/\Delta\lambda \leq 1,000$ )

MR = Medium Resolution ( $1,000 \leq \lambda/\Delta\lambda \leq 10,000$ )

HR = High Resolution ( $\lambda/\Delta\lambda \geq 10,000$ )

OI = Optical Imagers

WFMC = Wide Field Mosaic Camera

IFS = Integral Field Spectroscopy

AO = Adaptive Optics

TT = Tip Tilt

NIR = Near Infrared (1-5 $\mu$ m)

MIR = Mid Infrared (5-20 $\mu$ m)

### 3. 8m-class Optical-IR Telescopes in 2007 (Northern Hemisphere)

Telescope	Primary	Design/Focii	1st Light	Partners	Instruments
Keck I <sup>1</sup>	10.0m	Ritchey Chrétien f15,f25,f40	1993	CalTech, Univ. California, NASA	HIRES, NIRC NIRSPEC
Keck II <sup>1</sup>	10.0m	Ritchey Chrétien f15,f25,f40	1996	CalTech, Univ. California, NASA	LRIS, DEIMOS
Subaru	8.0m	Ritchey Chrétien Cass/Nasmyth	1998	NAO Japan	IRCS, FOCAS, CIAO OHS, HDS, SUPRIME-CAM
MMT	6.5m	Cassegrain f5.25,f9,(2)f15	1999	SAO, Arizona	Hectospec, Hectochelle, Megacam, Binospec, IRCam
Gemini N. <sup>2</sup>	8.0m	Ritchey Chrétien f16,f19.6	1999	USA, UK, Canada, Brazil, Argentina, Chile, Australia	GMOS, NIR/MIR Imagers NIR Spectrograph Michelle (shared with UKIRT)
HET	9.2m	Gregorian f/1.8	1998	Texas, Penn State, Stanford, Göttingen, Ludwig-Maximilians	LRS, MRS, HRS UFOE
LBT <sup>1</sup>	2×8.4m	Gregorian (2)f4,(2)f15,f15	2003	Arcetri, Arizona, Ohio St, Beteiligungsgesellschaft	Imaging Interferometer, IR imager, MOS, HRS, LRS
GTC	10.0m	Ritchey Chrétien	2003	Spain + ?	Opt/IR Imager Opt/IR Spectrograph

1 = interferometer

2 = data archive

#### 4. 8m-class Optical-IR Telescopes in 2007 (Southern Hemisphere)

Telescope	Primary	Design	1st Light	Partners	Instruments
Magellan I <sup>1</sup>	6.5m	Gregorian f11,f15	1999	OCIW, Harvard, MIT, Michigan	NIRS, MIR Imager, Big Throughput Camera
Magellan II <sup>1</sup>	6.5m	Gregorian f11,f15	2001	OCIW, Harvard, MIT, Michigan	Echelle Spectrograph, WF Imaging Spectrograph
Gemini S. <sup>2</sup>	8.0m	Ritchey Chrétien f16,f19.6	2000	USA, UK, Canada, Brazil, Argentina, Chile, Australia	GMOS, HROS, MIR Imager, Phoenix (shared with CTIO), COB (shared with CTIO)
ESO-VLT <sup>1,2</sup>	4×8.2m	Ritchey Chrétien Cass/Nas/Coudé		Belgium, Denmark, France, Germany, Italy, Sweden, Switzerland, Netherlands)	
VLT1			1998		LGS, CONICA, FORS1, ISAAC, SINFONI
VLT2			1999		UVES, FORS2, FUEGOS
VLT3			2000		VIMOS, VISIR
VLT4			2001		NIRMOS, FORS1, CRIRES
SALT <sup>3</sup>	11.0m			South Africa + ?	
ELT <sup>3</sup>	25-40m	4-Mirror/f50-1.2		Sweden, France, Denmark	
OWL <sup>3</sup>	100m/	4-Mirror/f6.4	2015	ESO	

1 - interferometer

2 - data archive

3 - unfunded as of April 1998

## 8. What Space – Based Telescopes Will be Available in 2010?

### NASA's program

NASA has an ambitious program of missions planned and either funded or budgeted through to 2010. Most of these, and certainly the ones that are most relevant to the NGC, are carried out under the “Astronomical Search for Origins” (ASO) program. The stated goals of the ASO program are to address the following scientific themes:

1. *How did the first galaxies form?*
2. *How do stars and planetary systems form?*
3. *Are there any planets outside the solar system that are capable of sustaining life?*
4. *How did life originate on Earth?*
5. *Is their life (however primitive) outside our solar system?*

The major first-generation missions in this program are the Next Generation Space Telescope (NGST) and the Space Interferometer Mission (SIM), to be followed by the Terrestrial Planet Finder (TPF) and to be preceded by a number of more minor, or pre-cursor, missions, many of which have been in existence for some time.

### Pre-cursor Missions

#### *HST*

The HST is projected to continue through to 2010, albeit in low-cost non-refurbishment mode. The main instruments during this phase will be ACS (Advanced Camera for Surveys) operating in the visible and near-uv and an ultraviolet spectrograph.

#### *FUSE*

FUSE is a modest Explorer class mission that will be launched in late 1998 and which will carry out spectroscopy in the far-ultraviolet, with particular emphasis on determining the Deuterium content of the Universe.

#### *WIRE*

This is a fairly modest Explorer class mission to be launched in mid-1999. It will map of order 50  $1 \times 1 \text{ deg}^2$  patches of sky in the 12 and 25  $\mu$  wavebands with typical sensitivities of order 0.1 mJy. The primary aim is a survey for dusty objects at high redshift, not least as targets for SIRTf.

#### *SOFIA*

SOFIA is an infrared 3-m passively cooled telescope mounted in a modified Boeing 747 aircraft.

#### *SIRTf*

This is a 0.85m cryogenically cooled telescope that is currently in Phase C/D with launch planned in December 2001. It will cover the 3.5-180  $\mu$  waveband with broad-band imaging and low resolution spectroscopy ( $R =$

50 – 600). SIRTf will be operated as a facility-class observatory, with an emphasis on early execution of a number of major “Legacy” science programs.

### **Astronomical Search for Origins Missions**

#### *NGST*

Without doubt, NGST is the space mission of most relevance to the NGC question, and therefore more details are provided below. Broadly speaking NGST is planned as a cooled filled-aperture 8-m telescope operating in the 1-5  $\mu$  waveband with possible extensions to 0.5  $\mu$  and to about 30  $\mu$ . NGST will have a range of imaging and spectroscopic instrumentation, including likely multi-object and integral field spectrographs. Launch is planned for 2007 and the telescope will likely be at the Earth-Sun L2 point where refurbishment will not be possible.

#### *SIM*

SIM will be an interferometer operating in the visible waveband, with launch planned for 2004. Six 0.3m apertures on a 10m baseline will yield impressive performance for astrometry (4  $\mu$ arcsec astrometry at  $V = 20$  in  $10^5$  sec), high resolution imaging (10  $\mu$ arcsec images of the core of M87 in  $10^5$  sec) and planet detection (with a nulling interferometer operating at  $10^4$  rejection).

#### *TPF*

This second generation ASO instrument, intended to be launched in 2010+, combines technologies from NGST and SIM. TPF is planned as a set of large (4m?) cooled telescopes acting as an interferometer on a 100m baseline in the 7 – 17  $\mu$  waveband. This will give 0.025 arcsec resolution,  $10^6$  null rejection for isolating the light from extra-solar Earth-like planets. The few hundred stars within 13 parsec will be surveyed. Earth could be detected broadband in 1 hr and an  $R=20$  spectrum (sufficient in principle to give evidence of  $CO_2$ ,  $H_2O$  and  $O_3$ ) obtained in 1 week of observing.

One point should be made concerning this program. It is directed squarely at the search and study of extra-solar planets with an emphasis on interferometric techniques – the ASO program is also funding the Palomar Prototype Interferometer and the Keck Interferometer on the ground. NASA’s ultimate goal is the Planetary Imager (PI), which would consist of multiple sets of 8m class telescopes acting as an interferometer on a 6000 km baseline, the technology for which is not even “on the horizon”. At least some of the enthusiasm at NASA for NGST, the most traditional “astronomical” mission, arises from the perception that the technology required for NGST will be applicable to the later very ambitious interferometers.

### **ESA’s Program**

ESA’s current approved space astronomy program is both spectrally broader and generally shorter term than the NASA program outlined above. The most relevant missions to the NGC question are the FIRST and Planck missions with XMM and INTEGRAL being somewhat peripheral.

#### *FIRST*

Now planned for launch in 2007, FIRST will be a cryogenically cooled 3.5m telescope operating in the 80 – 670  $\mu$  waveband, with imagers and spectrographs with  $R=2000 - 10^7$ . This is aimed primarily at studying dust-enshrouded phases of galaxy evolution at high redshift as well as local star-forming regions.

### *Planck*

As currently envisaged, Planck will share a 2007 launcher with FIRST. Planck will map the Cosmic Microwave Background (CMB) with 4 arcmin resolution and definitive sensitivity. Planck should extract essentially all of the available cosmological information available from the CMB.

### *XMM*

This is an observatory in the soft X-ray region (200 eV to 17 KeV) to be launched in January 2000. XMM will host 3 scientific instruments: an imaging X-ray camera, an X-ray grating spectrometer, and an optical monitor. XMM will be targeted to the study of stellar coronae, cataclysmic variables, supernova remnants, normal galaxies, active galactic nuclei, and galaxy clusters.

### *INTEGRAL*

INTEGRAL is a gamma-ray observatory, to be launched in 2001, dedicated to fine spectroscopy ( $E/\Delta E = 500$ ) and fine imaging in the energy range 15 keV - 10 MeV.

In the longer term, ESA is planning on participating in NGST, and in undertaking some or all of the following “Horizon 2000+” missions currently under study. Launch dates for these would be in the 2008, 2012, 2016 timeframe.

### *GAIA*

A follow-on to Hipparcos, GAIA is a filled-aperture telescope with similar astrometric performance to SIM. The emphasis however would be on an all-sky survey of all  $5 \times 10^7$  objects with  $V < 15$ .

### *IRSI (also known as DARWIN)*

This is an IR interferometer with similar capabilities and science objectives to TPF.

### *XEUS*

A large X-ray telescope aimed at spectroscopy of early Universe X-ray sources.

## **Next Generation Space Telescope**

NGST is currently in a pre-phase A Study Phase with Phase A starting in mid-1999 and launch planned for 2007. The current design specifications for NGST are:

1. *A filled-aperture primary mirror of 8m diameter, likely composed of deployable segments.*
2. *Location far from Earth, e.g. at L2, behind a large sunshield, leading to passive cooling of the telescope to around 30K, which yields a performance limited by the natural zodiacal background at all wavelengths out to 10  $\mu$ .*
3. *Science instrument complement will be decided in 1999 but will include wide-field imagers and spectrographs with multi-object capability.*
4. *Point source sensitivities exceeding 1 nJy ( $AB = 31$  magnitudes) in the near-infrared in reasonably long integrations (e.g. 5- $\sigma$  detection in  $10^4$  sec).*

The sensitivity gains of NGST relative to ground-based 8m class telescopes in the near-IR and mid-IR arise from: (a) the reduced background relative to that produced by the atmosphere and by ambient temperature optics and (b) the attainment of diffraction limited images over a wide field ( $\sim 4 \times 4$  arcmin).

NGST achieves its remarkable performance by virtue of being above the Earth’s atmosphere (which has strong OH emission lines between 0.8 and 2.0  $\mu$ , strong continuum thermal emission at  $>2 \mu$ , and absorption which limits ground-based observations to atmospheric windows which are reasonably open shortward of 2  $\mu$  but narrow and only partly open at longer wavelengths). For NGC, the reduction in background varies between about a factor of ten in the near infrared to a factor of  $10^6$  at 10  $\mu$ , giving gains in sensitivity from this reduced background alone of factors of 3 to  $10^3$ , depending on wavelength. NGST also gains by the attainment of diffraction-limited performance without the need for Adaptive Optics, which is limiting in terms of field of view and has difficulties in the optical waveband.

The implication of the fact that NGST is essentially as large as any ground-based telescope is that, although the ground-based telescopes will have more up-to-date instrumentation (since NGST will not be astronaut-serviceable), NGST will be able to do any observation at wavelengths longer than 1  $\mu$  better than any present ground-based telescope, except for very high spectral resolution spectroscopy,  $R \gg 10^4$ , where the performance is limited by the detectors and only a limited wavelength range is usually required.

We look first at the aperture required to match NGST’s performance at wavelengths longer than 1  $\mu$ . The sensitivity gain in the diffraction-limited case is simply  $D^2b^{-0.5}$  ( $D$  is the relative diameters of the telescopes and  $b$  the relative backgrounds), or in the case where the image quality is independent of aperture,  $Db^{-0.5}$ . Thus, if a ground-based telescope is fully-diffraction limited with AO, the aperture required to match NGST is only  $1.7 \times 8\text{m}$  at 1  $\mu$ , but increases past 2  $\mu$  to  $10 \times 8\text{m}$  at 3  $\mu$  and  $30 \times 8\text{m}$  at 10  $\mu$ . **Clearly observations at  $>2 \mu$  are unattractive from the ground, since very large telescopes are required to even match NGST’s performance.**

At wavelengths less than 2  $\mu$ , a diffraction-limited 10m with efficient AO is, in principle, already comparable to the performance of NGST (although NGST still avoids the highly featured OH background, the effects of the atmospheric transmission windows and isoplanetic effects associated with AO). Thus at these wavelengths, ground-based astronomy is still competitive with NGST.

The “uniqueness space”, thus, for a very large “next generation” ground-based telescope is in observations at wavelengths  $<2 \mu$ . Gains in the speed to reach a fixed point-source sensitivity in the background-limited case (i.e. imaging and low resolution spectroscopy) depend on both the aperture (as  $D^2$ ) and the continued shrinking of the diffraction-limited image (again as  $D^2$ ). Thus a diffraction-limited 25m represents a gain in speed of a factor of 40, or a gain in sensitivity of a factor of 6. In the non-background limited case (e.g. in very high resolution spectroscopy) the gain in speed is simply  $D^2$ . Even without the gains of AO diffraction-limited imaging, a 25m would represent as large a gain over the Keck as Keck itself was over previous generations of 4m class telescopes.

The most obvious need for a ground-based 25m telescope to exploit the gains of NGST would be in spectroscopy of very faint high redshift sources in the 1 to 2  $\mu$  regime. The Lyman- $\alpha$  line is still shortward of 2  $\mu$  for all  $z < 15$ . Although NGST may be able to detect forming star-clusters at this redshift (it can detect a system forming one solar mass per year for  $10^6$  years to  $z = 20$  in a pessimistic cosmology, i.e.  $\Omega = 0.2$ ), it will not be able to spectroscopically confirm these redshifts – such objects have  $K \sim 30.5$  ( $AB = 32.5$ ). Very deep spectroscopy with NGST itself could reach to  $K \sim 28$  with heroic efforts, i.e. to within about 3 magnitudes of the imaging limit, but to probe still deeper would require a larger collecting area (and still heroic efforts). In this sense the 25m would be to NGST as the Keck is to HST – in both cases the aperture increase is about a factor of four.

In the Figure below we summarize the above discussion by plotting the relative sensitivities of a 25m ground-based instrument compared to the NGST. This is done for both imaging and spectroscopy of point sources in the wavelength region 1 – 20  $\mu$ . The calculations adopt the parameters specified in the paper by Gillett and Mountain (in *Science With the Next Generation Space Telescope*, eds E.P. Smith & A. Koratkar, ASP Conf. Proc 133, in press) which are reasonable ones for the respective instruments. What is important here is that both telescopes are likely to have segmented mirrors and so are given equivalent Strehl ratios in the calculations.

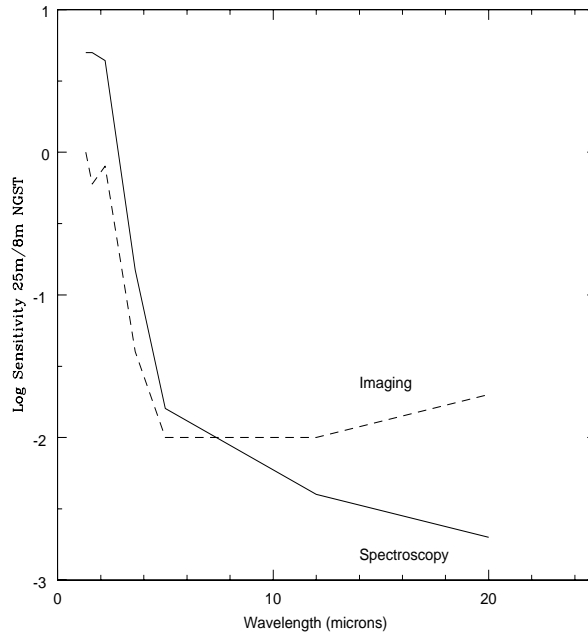


Fig. 1.— Point source sensitivity of a ground-based 25m telescope compared with an 8m NGST for both imaging and spectroscopy. Relative exposure times for a given signal/noise ratio will scale as the square of these sensitivity ratios.

For imaging point sources, NGST and a ground-based 25m telescope will have equivalent sensitivities at 1  $\mu$  with NGST superior by about a factor of 100 at 10  $\mu$ . For wavelengths shorter than 1  $\mu$ , the 25m ground-based telescope has superior performance. The gains of the 25m relative to NGST for spectroscopic applications are quite spectacular since these may be detector limited for NGST. For point sources observed with a spectroscopic resolution  $R = 1000$  at 10  $\mu$ , NGST is more sensitive by approximately a factor of 100, the sensitivities of the two telescopes are equal at about 2.3  $\mu$  and by 1  $\mu$  the 25m ground-based instrument wins by more than a factor of 5. Current design does not push the NGST sensitivity below 1  $\mu$  but even if it does extend to this short a wavelength, **the 25m instrument will have absolutely no competition in the optical for either imaging or spectroscopy.**

NGST will be operated as an facility-class observatory for a 5-10 year mission lifetime. The main science areas, as defined in the Design Reference Mission, encompass

- (a) the detection of the very first stars and galaxies at redshifts  $z \gg 5$

- (b) elucidating the physical origins of the Hubble sequence of galaxy types that is believed to be set up during the  $1 < z < 5$  range
- (c) the study of the distribution of dark matter in the Universe through weak lensing effects
- (d) the study of stellar populations in nearby galaxies
- (e) the study of star-formation regions in our own and other galaxies
- (f) the detection of low mass bodies in star-forming regions
- (g) study of low mass bodies in the Solar System (especially Kuiper belt objects).

NGST is budgeted through to launch in the ASO program and enjoys strong political support in the U.S. It is therefore very likely to proceed as planned. ESA and CSA are planning to participate at the \$200M and \$50M levels respectively.

### **Implications for the NGC Study**

There are three main implications for an NGC in the timeframe of 2010 of the space missions outlined, particularly as a telescope to complement NGST.

In terms of sensitivity, NGST will likely surpass any ground-based facility of less than 25m aperture longward of  $2 \mu$ . However, the field of NGST will be limited, and the instrumentation will be unchangeable and will represent 2003 technology. Furthermore, NGST's life will be limited to 5-10 years. In general terms ground-based facilities will be most competitive for

- (a) the optical waveband, where the background reduction in space is minimised.
- (b) high resolution spectroscopy, where the background is less important than the detector performance.
- (c) wide-field imaging and spectroscopy, especially at high image quality.

## **9. The Impact of Large Ground and Space Telescopes on the Key Science Goals**

In order to provide a summary of the discussion in the previous sections, the committee established a rating system of how existing and future telescopes might impact on the principle science goals. These are arranged in the following two tables. The intent here is to indicate what impact the various facilities would likely have had on the main science goals 25 years from now. So the way to interpret the ratings are to imagine the year 2023, look back on the previous 25 years and rate how each telescope has contributed to the key scientific issues. The committee made every effort to be objective and the ratings were arrived at by consensus although the dispersions in each category were generally remarkably small. For reasons of space and clarity the tables are broken up into ground and space telescopes but the real desire here is to have an intercomparison of how space and ground facilities will contribute to the key astronomical questions of the future. Hence the tables should be read together.

The main point to glean from these tables is that almost every instrument will address particular problems quite well, but have little impact on other fields. A prime example of this is the various interferometers planned by ESA and NASA as well as the ground-based ones. These will be key instruments in the science goal of searching for extrasolar terrestrial planets but have almost no impact on the dark matter searches or the quest for the first stars in the Universe. By contrast with every instrument currently planned or even envisioned, a large ground-based optical telescope will impact all the key areas significantly.

## 5. Impact on Key Science Goals: Ground-Based Telescopes

Science Goal	CFHT <sup>α</sup>	VLT/Gemini <sup>β</sup>	VLTI/KeckI <sup>γ</sup>	IRVLA/OVLA <sup>δ</sup>	LSA/MMA/SKA	NGC <sup>ε</sup>
1	D	C	B	A	E	B
2	B	B	E	D	E	A
3	D	B	B	B	A	B
4	D	B	E	E	A	A
5	C	C	B	A	A	A

### Science Goals

- 1 - Are we alone in the Universe?
- 2 - What is the Universe made of and what is its overall geometry?
- 3 - How did our and other solar systems form and evolve?
- 4 - First sources of light and how did the galaxies like our own form?
- 5 - Are there things in the Universe that we haven't as yet dreamed of?

### Ratings

A = must have, B = extremely useful, C = useful, D = marginal, E = not useful

### Notes on Ratings

$\alpha$  - includes all the 15 4 – 6m and the 2 – 3m specialized telescopes, high rating in goal 2 for SN searches, MegaCam and lensing, high rating in goal 5 for SDSS and MegaCam.

$\beta$  - includes all the 15 8m class telescopes

$\gamma$  - interferometers only

$\delta$  - IR and optical interferometers, unfunded

$\epsilon$  - aperture larger than 25m visible and near-IR telescope - high rating in goal 5 for aperture  $\geq$  50m only

## 6. Impact on Key Science Goals: Space-Based Telescopes

Science Goal	HST/ISO/SIRTF/AXAF/Compton <sup><math>\alpha</math></sup>	NGST/FIRST/Planck <sup><math>\beta</math></sup>	Space Interferometer/TPF/Darwin
1	E	B	A
2	C	A	D
3	B	A	A
4	C	A	D
5	C	A	A

### Science Goals

- 1 - Are we alone in the Universe?
- 2 - What is the Universe made of and what is its overall geometry?
- 3 - How did our and other solar systems form and evolve?
- 4 - First sources of light and how did the galaxies like our own form?
- 5 - Are there things in the Universe that we haven't as yet dreamed of?

### Ratings

A = must have, B = extremely useful, C = useful, D = marginal, E = not useful

### Notes on ratings

$\alpha$  - the C rating for goal 2 is for AXAF, the other telescopes are less useful

for this goal, the B rating for goal 3 is due to SIRTF.

$\beta$  - the B rating for goal 1 is for NGST, the A rating for goal 2 is for Planck,

the A rating for goal 3 is due to FIRST and NGST, the A rating for goal 4 is for NGST

while FIRST would rate a B here on its own, the A rating for goal 5 is for NGST and

FIRST.

## 10. Current Developments for Very Large Ground-Based Telescopes

Tables 5 and 6 provide the roadmap to the next generation facility with which the CFH community should be involved. It is clear from these that the one ground-based telescope that will most impact the science goals that we have recognized is a very large instrument optimized for imaging and spectroscopy. The NGC committee is of course not alone in thinking along these lines. One group which is well advanced with such a project is ESO whose OWL initiative envisions a telescope in the 25 – 100m range. NOAO very recently commissioned a study of future telescopes for its community (MAX-AT), the conclusion being that the US astronomers will require a telescope with an aperture in the range of 30 – 50m.

A large telescope such as a 25m one would have to operate with full adaptive optics correction, for which several solutions have been proposed, including tomography. This technique, widely used in medical imaging, is still in its infancy in its application to adaptive optics. It uses the different beam illumination from a few natural stars to reconstruct the wavefront. In principle, this technique works best for larger diameters (i.e. larger air column over the telescope for better modelling of the atmosphere) but at the moment, it is not clear what the minimum diameter of the primary mirror should be to be able to apply the tomography technique. This could be a key driver in the choice of aperture for the NGC and can only be decided upon after a detailed study.

## 11. Proposals for the Next Generation CFHT

The following plan follows naturally from the discussion in the previous sections. It is clear that the telescope which will have the strongest impact on the key science goals in the next few generations is an instrument with an aperture in the range of 25m or more. A 25m ground-based telescope would have a resolution of .005 arcseconds in V, and would reach  $V = 35$  in 10 hours integration for point sources. A ground-based telescope with a diameter of 25m would be, at  $1 \mu$ , equivalent or superior to NGST, with better spatial resolution, and higher collecting efficiency. For shorter wavelengths the ground-based instrument completely dominates for both imaging and spectroscopy. Above  $\sim 2 \mu$  the thermal background of the ground telescope starts to dominate and in this region NGST will have unsurpassed efficiency. It is therefore important to seek a complementary approach between NGST and the next generation of ground-based telescopes, as has been done successfully between HST and the 8 – 10m instruments on the ground.

Below we provide three options for a Next Generation CFHT. The committee was close to unanimous in the choice for a large instrument but each proposal suggested in what follows had its champion. The first option is an instrument that will be capable of impacting strongly the scientific goals outlined in the preceding sections. The second option will have less of an impact but will be a more conservative project providing a telescope which is an important contributor to certain restricted areas of the science program that has been developed. The third option will produce an instrument which will serve mainly as a support instrument for the larger telescopes.

### OPTION 1: A LARGE OPTICAL/IR TELESCOPE

The first option of the committee, and the one which it *overwhelmingly* recommends, is composed of 3 parts.

(1) **The existing CFHT should be operated with most of its current suite of instruments (except for the addition of a wide field IR-imager) for about another decade.** With MEGACAM, OASIS, the Adaptive Optics Bonnette, a high resolution spectrograph and a wide field IR-imager CFHT will continue to be competitive in many areas of research (galaxy surveys, weak lensing, stellar population studies, detailed observations of active galactic nuclei, stellar spectroscopy).

(2) **Beginning almost immediately the CFHT Board should initiate a “Phase A” study of a large optical/IR telescope, in the range of 25m.** The CFHT staff should be directly and actively involved in this exercise. This telescope should be optimized for imaging and spectroscopy, have an adaptive optics system and a suite of instruments that includes wide field imagers in the visible/IR, and integral field, multi-object and high resolution spectrographs. Such a telescope could be built on the current CFHT site. This site, by all accounts, remains the premier locale on Mauna Kea and the community will want to maintain its use of it. This telescope will be able to accomplish the prime scientific goals outlined in the previous sections and in addition, at 25m, will have the minimum size capable of exploiting new discoveries made by the NGST currently scheduled for launch by NASA in 2007.

(3) **If it is decided to build this large telescope, construction could begin in about 2008 at which time the current CFHT operations would cease.**

The committee is well aware that this telescope requires some technological developments before it can be realized. In particular, for it to have its optimal impact, the telescope will have to operate with a full adaptive optics system. This point and other related issues are discussed more completely in the following section.

#### **OPTION II: A SUPER-SMOOTH 8m TELESCOPE OPTIMIZED FOR ADAPTIVE OPTICS**

A second possible option for a Next Generation CFHT, and one that had only modest support from committee members, is construction of a modern, moderate-sized (8m) replacement for the existing CFHT. This telescope could likely fit in the existing CFHT dome and be much less costly than the large instrument proposed above. However, most committee members have clear concerns that such an instrument will not have the scientific impact of the larger instrument. However, building a very large (25m+) telescope on Mauna Kea is not without its risks. Some of these are detailed below in making the case for a more modest refurbishment of the CFHT.

It is almost certain that telescopes of the future that will be 10m or more in diameter will have segmented primary mirrors. This will likely limit the quality of the images that will be obtained with such instruments due to scattered light at segment boundaries and misalignment of individual segments. In a short technical note M. Espiard, former director of REOSC, claims that an inexpensive refurbishment option for CFHT would be an 8m with the best 8m mirror constructed to date. Such a mirror could have a Strehl ratio as high as 0.92 and an RMS wavefront error at 550nm of about 20nm. Effectively this translates into a mirror that is diffraction limited in the V band. A further claim is that a telescope with this mirror could be installed in the existing CFHT dome in 3 years at a cost of about 300M French francs.

Assuming that none of the above has been overstated, we can ask what the impact of such an instrument will be. Allowing for detailed “Phase A” studies and financial arrangements means that this telescope will likely not be available for observations before about 2006, 7 years from now. By this time 8 northern 8m class instruments will be in operation as well as 7 in the south. But this telescope will be somewhat unique with its superb optics. Even so it will not impact strongly on our science goal #4 (First Stars and Galaxy

Formation) which requires either long wavelength sensitivity ( $> 10\mu$ ) to see forming galaxies at high redshift or a very large aperture to carry out spectroscopy of extremely faint objects. Similarly, science goal #2 (What is the Universe Made of and Geometry of the Universe) which explores the dominant source of mass in the Universe demands a telescope capable of investigating the kinematics of the visible matter in distant galaxies. The dominant spectral features that will be used for such a study will all be significantly redshifted so that a modest (8m should now be considered modest!) ground-based telescope will find it difficult to compete with NGST. Although the images with this proposed new CFHT will surpass that of any other ground-based telescope currently planned, it will still not place the telescope into a regime of parameter space that entirely new phenomena are likely to be discovered. Its step forward from the existing and planned 8m instruments will be only incremental. Hence it will have little impact on science goal #5 (Things Not Yet Dreamed Of).

Where this new CFHT could have its most important influence is on goals #1 (Are We Alone?) and #3 (Formation of Solar Systems). Adaptive optics imaging with a coronagraph of nearby stars searching for zodiacal light-type disks as indicators of planetary systems would be a prime observational program for such a telescope. It is not at all clear that infrared imaging (at  $\sim 10\mu$ ) would be more advantageous for such a project as at least the one system that has been studied in most detail (our Solar System) does not appear to have a large infrared excess. The smooth optics of this NGC telescope, with its minimum of scattered light, would also be useful in microlensing studies of large samples of stars in the direction of the Galactic centre. This program could eventually provide statistics of the number of planetary systems as the presence of a planet (in the proper position) yields a microlensing signature which is unique and which can be produced even by terrestrial-type planets. An 8m telescope will not be able to directly image a terrestrial planet. This feat will be reserved for large ground-based instruments ( $\geq 25$ )m or sophisticated space interferometers.

The smooth mirror of this NGC will also be important in other studies not directly defined by the stated science goals. Wide field imaging, both in the optical and near IR, will greatly benefit from the superb images of this telescope. Such imaging studies could be important in large scale structure investigations, in searches for distant supernovae as well as in trying to locate the Galactic microlensing candidates.

It is only reasonable to evaluate the impact of this telescope on the primary science goals in the same manner that we have done for the others. If we add a column to Table 5 for this 8m “Super Smooth” instrument, the evaluations would be B B B B C.

This particular choice for the next generation of CFHT is likely to present many fewer technological challenges than the 25m option. The current new generation of 8 to 10m telescopes has been made possible by a technical breakthrough: active optical control. No equivalent breakthrough has been made today that can ease the construction of a much larger telescope. The use of a thin meniscus mirror for a 25m telescope is out of the question. Only segmented mirrors can be used and the likely price to pay is a loss of optimal optical quality. This is clearly demonstrated by the first VLT images, the quality of which already surpasses that of the Keck images at the same wavelength. At its current level of development, adaptive optics does not properly correct for discontinuities in a segmented mirror surface. This is an area where technological advances will be required in order to make the 25m+ feasible.

It can be argued that the mere collecting power of a 25m telescope amply justifies the investment. Unfortunately, under seeing limited conditions, instrument size grows in proportion to telescope size which makes instrumentation as problematic as the telescope to construct. Worse, detector pixel size must also grow in the same proportion to the detriment of detector performance. The problem is already severe on current

8 to 10m telescopes; it may be insurmountable on a 25m telescope. The only way out is the use of adaptive optics. The committee's primary recommendation assumes that adaptive optics can be implemented on a segmented-mirror 25m telescope; it is indeed the case that this is a large extrapolation from current knowledge.

The first VLT results have demonstrated that actively supported thin meniscus mirrors can deliver images with an optical quality which surpasses that of any other ground-based telescope. Development costs for such mirrors have all been paid for, and companies such as REOSC can now routinely produce top quality 8m mirrors at bargain prices. Moreover, the CFHT dome is so oversized that a modern 8m telescope would fit inside it. The argument that 15 other similar telescopes will already be operating must be viewed in context of the history of CFHT. When the CFHT saw first light, a number of 3.6m or larger telescopes were already in operation, including the Palomar 5m and Soviet 6m telescope. It did not prevent the CFHT from being considered, 20 years later, as one of the most productive telescopes in the world. This is because of the telescope's unique performance and highly advanced instrumentation. Today the CFHT is still unique for both wide-field imaging and adaptive optics. There is no fundamental reason why an 8m CFHT would not remain competitive.

There are a number of other issues that make the 8m proposal attractive. Canada currently has about 155 nights on the CFHT. Her share of time on a 25m telescope is unlikely to be as large (other partners will probably be required to financing the building of such an instrument). With closure of the present CFHT to build the large instrument, Canada's access to ground-based telescopes will be very minimal. France is in a somewhat better position with its connection to ESO, but even here her access to the northern sky will be curtailed. One benefit that will accrue from moving quickly to an 8m option is a sharing of instruments and instrumentation development costs with other 8m telescopes on Mauna Kea. There is also the potential for considerable savings in operational costs if appropriate arrangements with the Gemini consortium can be developed.

### **OPTION III: A REFURBISHMENT OF THE EXISTING CFHT**

We offer one other option for a refurbished CFHT, and that is a simple upgrade of the existing telescope coupled with a *strong* investment in a next generation of instrumentation. Such a plan would be low cost relative to the other options mentioned above and could keep CFHT in a competitive position for the foreseeable future *in the limited areas of research in which it has decided to specialize*.

Such a refurbishment might involve overhauling the telescope mechanically and electronically, improving the dome environment and possibly replacing the mirror. Simultaneously, CFHT must develop a strong and unique instrumentation plan that will continue to exploit the superb CFHT site.

If this option is adopted, CFHT should start to define and implement, as soon as possible, a new operating mode for the telescope. The operating mode should be dedicated to vast key programs: wide-field imaging with Megacam (it is unlikely we will need a next generation of this instrument): place the wide field IR camera high on the priority list of instrumentation and plan eventually for an even wider field IR camera, possibly to be located at the prime focus, and which will also have spectroscopic capability: high resolution cross-dispersed spectroscopy/polarimetry for asteroseismology and other long term spectroscopic monitoring programs (a second generation instrument after Espadons could be planned with even higher resolution).

This new mode of operation of CFHT could start to be phased in immediately and be fully operational around 2003-2004 at which time some of the next generation instruments (larger IR camera, next generation

spectrographs) could undergo serious development. A CFHT operated in such a mode will, to a great extent, be a support telescope for the larger instruments. It would be useful for the construction of databases of multicolour photometry that would be helpful in identifying suitable candidates for spectroscopy or higher resolution imaging with the bigger telescopes.