Complementarity of NGST, ALMA, and far IR space observatories

John C. Mather

NASA Goddard Space Flight Center, Code 685, Greenbelt, MD 20771
john.mather@gsfc.nasa.gov

Abstract: The Next Generation Space Telescope (NGST) and the Atacama Large Millimeter Array (ALMA) will both start operations long before a new far IR observatory to follow SIRTF into space can be launched. What will be unknown even after they are operational, and what will a far IR space observatory be able to add? I will compare the telescope design concepts and capabilities and the advertised scientific programs for the projects and attempt to forecast the research topics that will be at the forefront in 2010.

1. Introduction

In a strategic planning sense, the most ambitious space observatories must follow after the most ambitious ground-based and space-based observatories of the previous generation. Almost by definition, they look impossibly difficult when they are conceived. By the time the consensus is built for such a space observatory, it must be clear that the other tools have been nearly exhausted and that there are important mysteries that can never be resolved without new tools.

When can we anticipate that other tools will be exhausted, so that a series of far infrared space observatories becomes mandatory? The crystal ball is cloudy, but we do anticipate several new observatories that have been strongly endorsed by the National Academy, and may be online within the next 10 years. The SIRTF (Space Infrared Telescope Facility) is coming within a year, pending resolution of certain flight software issues, and will provide an extraordinary leap in sensitivity, reaching the confusion limits for its size at all wavelengths. The ALMA (Atacama Large Millimeter Array) and the NGST (Next Generation Space Telescope) are the two most powerful, approaching the far infrared region from both ends. Some kind of 25 m GSMT (Giant Segmented Mirror Telescope) visible – near IR telescope will be built. By 2015, all three will have been in operation for about 5 years, and the cream of the new discoveries may already have been found. What might those be, and what will they imply for far IR observations in space?

The technical capabilities are easily summarized. NGST imaging sensitivity will be photon background limited out to about 12 μm, with an aperture of 6 or 7 meters, and there will be moderate resolution spectroscopy (R ~ 1000 out to 5 μm, R ~ 2000 from 5 to 28 μm). ALMA will have about 64 12 m aperture dishes but detector technology and atmospheric interference limit its sensitivity to several times the quantum noise limit in those submillimeter atmospheric windows where observations can be made at all. ALMA will have high spectral resolution, as high as wanted to resolve any thermal motions. The GSMT will presumably be outfitted with a full array of imaging and spectroscopic capabilities, but its longer wavelength infrared performance might not be optimized since the prime driver will be the possibility for good imaging.

The case for a far IR observatory in space has been very ably presented in this conference (Rieke, 2002), and includes many wonderful goals and objectives. There is much to be learned from SIRTF and from a future successor, e.g. the SAFIR (Single Aperture Far IR) telescope. However, I am concerned that we have not let our imaginations run sufficiently far into the future. Such a mission will not fly today or tomorrow, but more than a decade into the future. So what will be discovered by 2010 or 2015, and what will be left for far IR astronomy? This is an important exercise, because most of the arguments advanced today for the need for future missions are written from today's perspective, and many will need revision in another decade. As we explain the need for SAFIR, let us consider where our colleagues will be in the future.

2. Key Scientific Questions and Forecast

The National Academy has identified the following key scientific questions in the Decadal Survey (McKee and Taylor, 2000), many of which will be rather well addressed by the ALMA and NGST. What will be left open? What will the competition be? The UV astronomy community does not have a major mission planned after HST, but a committee is meeting shortly in Chicago to outline new strategies for UV astronomy in space. Neither far IR astronomy nor UV astronomy is ripe for commitment to a new mission, but either or both could be ready soon and I believe both should appear in the NASA strategic plan.

The Decadal Survey (p. 52) identified 5 key topics that will set the direction for astronomy and astrophysics for many years to come:

1. "How did the universe begin, how did it evolve from the soup of elementary particles into the structures seen today, and what is its destiny?
2. How do galaxies form and evolve?
3. How do stars form and evolve?
4. How do planets form and evolve?
5. Is there life elsewhere in the universe?"

They identified 5 goals for the new initiatives of this coming decade as well (p. 53):

1. "Determining large scale properties of the universe
2. Studying the dawn of the modern universe
3. Understanding black holes
4. Studying star formation and planets
5. Understanding the effects of the astronomical environment on Earth"

These goals are stated in very broad terms, and most of the new initiatives have something to say about most of them.

The NGST science working groups (SWG's) have defined a set of 23 large observing programs that might be carried out, and a subset of 7 of them that should be used to drive the observatory design. These 23 are called the Design Reference Mission, and are not meant as a substitute for the competitive process that will eventually select observers and their programs. The 7 core programs are:

1. Formation & Evolution of Galaxies - Imaging
2. Formation & Evolution of Galaxies - Spectroscopy
3. Mapping Dark Matter
4. Search for Reionization Epoch
5. Measuring Cosmological Parameters
6. Formation & Evolution of Galaxies - Obscured Stars & AGN
7. Physics of Star Formation: Protostars

These 7 programs lead to the SWG's recommendation for 3 core instruments. There would be a near IR camera covering 0.6 to 5 μm with about 48 Mpixels across about 16 square arcminutes, Nyquist sampled at 2 μm, with an R ~ 100 grism mode, suitable for studying the first light, galaxy formation, dark matter, supernovae, young stars, Kuiper Belt Objects (KBO), and stellar populations. There will be a near IR (1-5 μm) multiobject spectrometer covering about 9 square arcminutes, capable of observing > 100 spectra simultaneously, with resolution of about 1000, suitable for studying Galaxy formation/diagnostics (clustering, abundance, star formation., kinematics), Active Galactic Nuclei, and young stellar clusters (Initial Mass Function (IMF)/stellar populations). There will also be a mid IR camera and spectrometer covering 5 to ~28 μm with about 4 square arcminute field, with a spectral resolution up to about 3000, suitable for studying the physics of old stars at high redshift, z~5 obscured star formation and Active Galactic Nuclei to z ~ 5, PAHs to z~5, Hα to z~15, the cool stellar IMF, protostars and disks, KBO sizes, and comets.

The predicted sensitivity of NGST is extraordinary, significantly less than 1 nJy for long exposures at near IR wavelengths. The sensitivity is worse at wavelengths > 5 μm where zodiacal light becomes bright, and worse again for wavelengths > 12 μm where the stray light from the warm parts of the sunshield becomes brighter than the zodiacal light.

Nevertheless, NGST will still be the most sensitive telescope over its entire wavelength range. NGST will be capable of seeing individual young globular clusters and galactic nuclei all the way back to their hypothetical formation time at $z > 10$, and individual Type Ia supernovae back to $z > 5$.

The ALMA objectives are similarly ambitious, and the ALMA web site gives many pages of quantitative goals. For the Distant Universe topic, they offer the following:

- "Image thermal dust emission in evolving galaxies at epochs as early as z=10.
- Yield kiloparsec-resolution images of dust emission in active galaxies and QSOs.
- Detect CO, CI, CII emission lines from Galaxies and QSOs. A line luminosity of 10,000,000 solar luminosities at z=0.5 will be detectable in under 10 minutes.
- Image the microwave decrement in galaxy clusters; together with AXAF observations, this will provide an accurate determination of the Hubble constant.
- Resolve regions of particle acceleration in the jets and lobes of radio galaxies.
- The MMA, with the world's largest aperture at wavelengths around 1 mm, will be crucial to millimeter-wave VLBI. Accretion disks in galaxies as distant as the Virgo Cluster will be seen at 20 microarcseconds resolution."

It is clear that this project has enormous potential for detailed analysis of complex phenomena, and for discovery of completely unexpected types of objects.

I will now engage in a little crystal ball gazing. I think it likely that these and other major instruments will achieve some significant portion of the following discoveries in the next 15 years:

1. Measurement of the cosmic background radiation anisotropy on all scales, and of its polarization on small scales, leading to consensus on the development of large scale structure, and of the amounts and types of dark matter at $z \sim 1000$
2. Measurement of the cosmic dark matter clustering by weak lensing at $z \sim 1\text{-}5$
3. Measurement of cosmic dark energy by its effect on the acceleration of the universe and supernova brightness
4. Measurement of neutrino masses in terrestrial labs deep underground, confirming that they form an appreciable part of the cosmic dark matter
5. Small chance: detection of non-neutrino dark matter in the lab
6. Development of a new theory of elementary particles, perhaps based on strings or m-branes, including a first apparently successful theory of quantum gravity, with "predictions" explaining the inflationary period
7. Measurements of frame dragging in general relativity, by GP-B (around Earth) and Con-X (around black holes)
8. Detection of gravity waves with LIGO and LISA (later) due to binary collapsed objects, and maybe asymmetric supernovae
9. Discovery of the first luminous objects, including star clusters, galactic nuclei, ordinary supernovae, hypernovae, and supermassive primordial metal-free stars

10. Discovery of highly obscured luminous infrared galaxies at high redshift, showing that the first galaxies immediately produced dust
11. Discovery of the objects that re-ionized the early universe at redshift around 6
12. Beginnings of mapping the intergalactic hydrogen distribution through its influence on QSO absorption, its Lyman alpha emission, and its 21 cm emission, using advanced imaging spectroscopic equipment to map much deeper than is now possible
13. Discovery of metal-poor Population III and completely metal-free Population IV stars from the very early universe, still orbiting within or around our Galaxy
14. Agreement on the formation sequence of galaxies, probably from small objects building up by mergers, both by observations of distant objects, and from kinematic reconstruction of the history of the Milky Way and nearby galaxies
15. Discovery of black holes (quasars) at extremely high redshifts in the first luminous objects, fully equipped with dusty torii
16. Theoretical understanding of early (cool) stages of star formation, confirmed by detailed analysis of atomic and molecular lines seen with ALMA
17. Theoretical understanding of late stages of star formation, confirmed by detailed analysis of spectra taken with NGST and ALMA
18. Direct detection and characterization of planets in orbit around nearby stars, through clues from Doppler and astrometric measurements, transits, analysis of structure in dust clouds, deployment of a coronagraph on NGST or on a new special purpose planet finding space or ground-based telescope, and application of new search strategies. Determination of atmospheric properties from transit spectroscopy.
19. Discovery of large numbers of loose planets in star clusters and in the Earth's neighborhood
20. Understanding some of the effects of past astrophysical events on Earth, including passage through dust clouds, and nearby supernovae
21. Understanding the formation and stability of planetary systems through migration of bodies and interpretation of chaotic dynamics
22. Discovery of large numbers of Kuiper belt objects, and the beginning of mineralogical interpretation from their colors
23. Recovery of many more Antarctic meteorites from Mars, including one that has startling and maybe convincing evidence of water and life
24. Imminent return of Mars surface samples; when they are brought back they will be disappointing because they won't come from the hypothetical wet spots.
25. Interpretation of solar system dynamics to understand the formation history of the planets and the asteroid belt, including the event that broke up the iron meteorites from parent bodies
26. Theoretical analysis and computer modeling for all categories of mysterious objects, based on the continuation of Moore's law, and the continued development of efficient 3-D hydro and gravity codes

3. "What Ain't We Got? You Know Darned Well!" (R. Rodgers, O. Hammerstein, 1949)

We'll have sunlight on the sand and moonlight on the sea, but (at least) one subject will still be mysterious. Half of the light in the universe, the part that's converted from starlight to far infrared, will still be observed only with the small SIRTF and limited ground-based facilities. After all the progress made with other instruments, astronomers will still be concerned that they are studying their elephant blindfolded, touching the toenails to measure the mass and luminosity and origin of the elephant. The essentially unobservable objects and mysteries will include:

1. Some object classes we already know about that emit primarily in the far IR. We'll still wonder what they're doing there, whether they harbor marvelous surprises.
2. Some bright and rare object classes. Without a full sky survey at the relevant wavelengths we won't know they exist. NGST and ALMA are not survey instruments, but the LSST (Large Synoptic Survey Telescope) will do a wide field survey at visible wavelengths. However, this is certainly far from the far IR.
3. Some objects that emit primarily in the far IR will be too faint to discover at any other wavelength. To elude discovery with the NGST, which is background limited out to about 12 μm, they have to be either highly obscured, or quite cold, or highly redshifted.
4. To elude discovery and detailed study with ALMA, they need to be either faint or camouflaged or rare. To be camouflaged, they need only have featureless spectra that do not distinguish highly redshifted warm dust from local cool dust at the long wavelengths that penetrate the atmosphere enough for ALMA to see them. To have featureless far IR spectra, they need to have significant far IR optical depth, so that the dust outshines the line emission, or sufficient ionizing radiation to destroy atomic and molecular emitters at far IR wavelengths.

As there are about 2 orders of magnitude in wavelength between the bands where NGST (12 μm) and ALMA (1 mm) are at their best sensitivity, I think that there are many possibilities of objects that fit these categories. It is straightforward to plot a blackbody or a thermal emitter with an emissivity proportional to frequency on the sensitivity plots for NGST, SIRTF, and ALMA. An emitter with a flux of $\nu I_\nu = 5 \times 10^8$ Hz-Jy at 100 μm and a temperature of about 70 K in the observing frame could easily escape detection by all these instruments. I translate these numbers into luminosities of 2.35×10^{-4} L_{sun} at 1 kpc, 2.35×10^2 L_{sun} at 1 Mpc, 2.35×10^8 L_{sun} at 1 Gpc, and 6.1×10^9 L_{sun} at 3 Gpc. (I've made an approximate correction for redshift at 3 Gpc). If the temperature is a different, the undetectable luminosities are lower.

Even now with the short list of objects seen with SCUBA there are many that are not known at other wavelengths (Smail et al., 2002). I suggest the following possibilities:

1. Redshift 10-20 objects with ordinary late type stellar colors, due to some first generation galaxies that have already lost their gas supply because of supernovae blowing them apart, and therefore have only old stars. These could be a new category of object, and could change our view of the early universe and the formation of galaxies.
2. Redshift 10-20 objects that are so compact that dust clouds surround them and hide all the hot, bright stars within; these may be AGN or star-forming regions. It is generally thought that the first objects were UV bright and dust-free, but we don't know how quickly dust may be released. If the lifetime of the objects is 10^9 years but dust is copiously produced after 10^7 years, then only 1% of the objects will be found with redshifted UV radiation. It's more likely that most objects have some UV stars outside the dust cocoons even after dust is released, but we already know that some of the most luminous objects are completely obscured.
3. Molecular hydrogen cooling lines at rest frame 17 and 28 µm from the formation of the first galaxies. These lines could be quite faint, and covering large areas of sky, so we'll need a wide field very efficient spectrometer of modest resolution to find them. Maybe this will be an imaging Michelson spectrometer.
4. Redshifted nebular 5-20 µm lines of highly ionized neon, etc., which distinguish starburst galaxies from AGN. These occur in the far IR out to very high redshift, there are no known equivalents at radio wavelengths, and the UV is often obscured so is not sufficient.
5. Redshifted C^+ (157 µm) and N^+ (122, 205 µm) cooling lines from the interstellar medium in galaxies in the range out to redshift 5 or so; at higher redshift these lines come into view for ALMA at 1 mm. At lower redshift ALMA could see them if they're bright and they fall in the atmospheric windows.
6. Cool dust clouds around most stars, due to comets and asteroids as in the zodiacal light. These might be best detected as far IR excesses, and with sufficient angular resolution to recognize structure, they could lead to many inferences about the presence of planets.
7. Molecular and atomic line cooling systems for interstellar gas where the dust has been vaporized or has never formed.
8. The large scale polarization of the CMBR. This will still not be measured yet at the levels interesting to Big Bang theorists, as it will require very large and sensitive far IR arrays, as well as very good understanding of the astrophysical foregrounds and methods for removing them. Far IR observations (i.e. at wavelengths shorter than the CMBR) may be required in support of the CMB polarization measurements.

4. Other Possible Advances, Scientific and Technical

Some possible technical advances will also make far IR technology ready for space and for the astronomical challenges of extreme sensitivity. I think the following are likely developments in the next 10 years, particularly given support from NASA.

1. Far IR detector arrays will continue to improve, with improved coolers routinely available below 0.1 K, with superconducting readout electronics, and the possibility of far IR photon counters. When they reach sufficient maturity, a new generation of telescopes will become an obvious next step. This may take another decade, depending on funding and commitment levels.
2. Coherent receivers will improve, so that the ALMA looks much more attractive at 350 µm when the weather is good. There may also be a call for a warm 15 m space telescope operating at shorter wavelengths, to follow the Herschel with coherent receivers and improved spectroscopic back ends.
3. A low-power correlator will be invented that could enable a space version of ALMA, with several or many dishes flying in formation around a correlator hub spacecraft. With warm dishes and a reduced requirement for precise formation flying, this will look very attractive for observations of many ALMA and Herschel types of targets.
4. UV detectors will also continue to improve, and the UV astronomy community will correctly argue that there are important scientific goals that are ripe for their approach.
5. Innovative new spectrometer designs will enable the use of far IR arrays for efficient high-resolution spectrometry, combining dispersion with Fourier or Fabry-Perot interferometry. These will be tested first on SOFIA and then will serve as the basis for a new observatory.
6. Earth scientists will fly an imaging FTS (Fourier Transform Spectrometer) in geosynchronous orbit for the 10 µm region in the NASA EO3 technology demonstration program, demonstrating the principles and developing the signal processing technology.
7. NGST will demonstrate deep passive radiative cooling, and engineers will develop concepts for two-stage radiative coolers that reach 7 K, the zodiacal light limit at Earth
8. Active coolers will be developed for NGST and TPF, capable of large capacity at moderate power consumption, providing cooling for the telescope and for the detectors without stored cryogens.
9. Someone will develop formation flying for constellations of many spacecraft looking down at the Earth.
10. Ground-based interferometry will develop quite well, using AO on large telescopes to boost sensitivity by orders of magnitude over present capabilities. Astronomers will develop an appreciation for the possibility of high-resolution imaging of stellar surfaces, cores of AGN, dust in close proximity to young and old stars, planetary surfaces in the Solar System, and star-forming regions.

5. Implications for Far IR Space Mission Planning

It's clear from all this that the crystal ball is very cloudy. The next conclusion is that all areas of astronomy benefit from improved detector technology, so that should be the top priority for long term funding. Larger scale technologies like coolers, radiative shields, mirrors, and formation flying may have other customers who can develop them without specific requirements from the far IR astronomy community.

Then, based on the principle that we must do everything simple and cheap before we can do anything complicated and costly, there's a logical progression of far IR projects to do.

1. A wide field sky far IR survey, ideally of the whole sky, whenever the detector arrays are ready. There are many previously unknown objects that could be found with even short exposures and a small mirror, and some will represent new classes that are not expected. This mission is too large to fit the existing Explorer competition guidelines, but is probably not so large that it requires the full NASA strategic planning process. It might be suitable for a competition in an Explorer-Plus category if this could be made available. If my arguments made above are sound, it could be a winner.
2. At about the same time, a Cosmic Background Polarization mission could be built. It would like be an all-sky survey mission as well, and depends on much the same technology but requires a very different mode of operation. It might be in the same price range as the wide field far IR survey. Given the scientific backing of the community this could also be a winner, but only if the predicted astrophysical foregrounds are sufficiently well understood at the time.
3. At about the same time, a single aperture far IR telescope, SAFIR, as advocated by the Decadal Survey. This would be a follow-on to NGST, with as large an aperture as is affordable given the NASA budget possibilities. It would have to be a strategic mission, and it would have to compete with other such missions at the advisory committee and National Academy level. This could have a budget less than the NGST, if the NGST aperture is sufficient, or larger, if the scientific goals are sufficiently compelling to justify a larger telescope. It could have a variety of instruments like NGST, optimized for imaging and spectroscopy over the whole range from the NGST zodiacal light limit of 12 μm out to maybe 500 μm, depending on detailed comparison with capabilities on the ground.
4. After SAFIR, I think it will be clear that we need more angular resolution to study the objects we have discovered, and to beat the confusion limit at wavelengths > 100 μm. To do this we will need either an imaging interferometer or a new telescope 3 times as large; a smaller gain in aperture would not be scientifically compelling, and a larger one could be seen as too great a leap. An interferometer span a few times the aperture of SAFIR will be enough to make a major advance, so I think some form of single-spacecraft interferometer will be the next step. On the other hand, by that time, it may be possible that formation flying technology has been fully developed by other fund sources, and a separated spacecraft

mission like SPECS will be competitively priced. It might also be that really large aperture telescope mirrors will be possible by then, based on stretched membranes of some sort.

6. Conclusions

The short-term actions for NASA should be to 1) support far IR detector development, 2) start small study efforts to develop concepts for the first three possible missions on this list. The time scale to develop a scientific consensus and a mission concept is of the order of 4 years, just in time to get ready to do the next big thing after NGST, ALMA, and GSMT. It is important to start this journey of a thousand miles with a single step.

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