2014 Montreal Interferometry Forum Report

Forum Organizers:
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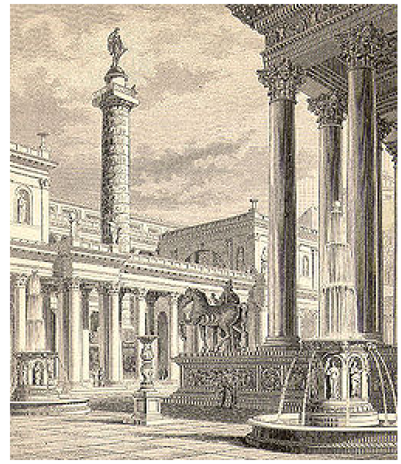


OLIVIER CHESNEAU

Dedication

The attendees of the 2014 Interferometry Forum wished to dedicate the forum and its report to our recently passed colleague Dr. Olivier Chesneau. Not only was this subject near and dear to Olivier's heart, it seemed particularly fitting since the meeting was being held at U. de Montréal, where he received his doctorate – his graduation photo (left) hangs just outside the room in which we met.

Introduction

The forum was organized in a manner similar to the 2013 Forum, around a list of topics – each topic had a moderator and an archivist. Each participant in the forum had one or more assignments – this was not a meeting for passive participation.

The following summaries are a slightly edited version of those notes; conclusions and recommendations are presented at the end of the document.

Opening / Context of the Forum

Moderator: van Belle
Archivist: Absil

Recent developments – as an outcome of the 2013 Forum – include an increased web presence. In particular, the old OLBIN page has been (largely) ported to an IAU C54 wiki, along with an increased social media presence of IAU C54. The format of the forum was discussed briefly; Steve Ridgway noted there was no predefined objective during last forum, and yet it produced tangible results in the form of the wiki, and aroused interest from the community. The duration of the format - 1 day (2014) or 2 days (2013) was considered with no strong feelings about either. Having an emphasis on discussions rather than slide-driven presentations from individuals for both 2013 and 2014 was deemed a better approach for the Forum.

Notable Technical Developments from the SPIE

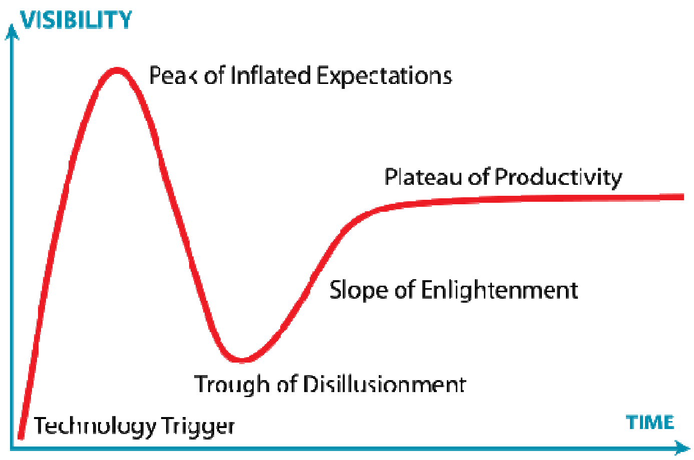
Moderator: Mérand
Archivist: Armstrong

There was more of an emphasis on making things work at current facilities than there was discussion of big new ideas – or even big old ideas, such as space interferometry or a facility at Dome A. The only discussion of large new facilities was the necessarily imprecise Planetary Formation Imager, which of course at this point is a "unifying theme for activities over the coming years" rather than a specific architecture or design.

Antoine Mérand, leading the discussion, started us off with a list of technology developments that came up at the meeting. To take them briefly:

- *The dawn of IR APD detectors.* Mike Ireland concurred with Antoine that they are the most promising new technology, but others said that they've actually been around for a while, with mixed results.
- *Beam transport via fibers.* There was an interesting paper on temperature-controlled fibers by Matt Anderson [talk 9146-54], but no discussion of it today.
- *The return of heterodyne?* Maybe important, but mentioned today only briefly.
- *Polarization control rising as an issue.* Not mentioned today.
- *Maturity of integrated optics.* For me [T.A.], it seems that integrated optics is mainstream, which may be why it wasn't mentioned in today's session.
- *AO coming slowly to CHARA and VLTI.* Also not mentioned today, although I think it could make a significant contribution.

- *Image reconstruction algorithms.* These were mentioned obliquely today, in that we need more images, but the Beauty Contest reconstructions inspired some skepticism, at least for me.
- *Astrometry is difficult.* For me, the cancellation of PRIMA was the single biggest technical story, but it did not lead to much discussion in today's session.



Much of the discussion was motivated by Antoine's display of the "Hype Cycle" diagram (right): the technology trigger, followed by the peak of inflated expectations, then a plunge to the trough of disillusionment before we either "auger in", as Gerard van Belle put it, or climb the slope of enlightenment and reach the plateau of productivity. Technical developments can be found at all of these stages, and there was a fair amount of discussion of which techniques are where, as well as an acknowledgement that the diagram is an oversimplification even for a single technique. Much of the rest of the discussion took place in the context of the Hype Cycle diagram, in particular the issue of over-promising.

Several participants pointed out that interferometry has usually over-promised in terms of sensitivity or imaging capability. It's probably too late to erase that perception, but Stephen Rinehart pointed out we shouldn't damp the peak of expectations, because it's hard to get projects started. Dave Mozurkewich pointed out that many projects eventually end up being even better than promised. The VLA is an example: it was designed with then-current technology [with low dynamic range by today's radio interferometry standards], but then CLEAN came along. Other examples were cited as well: HST was built to measure the Hubble parameter, and so on.

Stefan Kraus pointed out that the lack of images is a disappointment to the wider community and that it would be useful to move from pushing limiting magnitudes to improving imaging fidelity. In his talk during the PFI session, John Monnier told us that we will need something like 20 x 20 pixels to start making good images, which translates into 20 or more array elements. With three configurations with four elements each, the Beauty Contest entries had a fair range of morphologies in spite of being based on the same data set. The simulations done by Rainer Koehler with a similar multi-configuration data set showed that reconstructing an artificial T Tauri star with a companion did not work well.

Much of the rest of the discussion focused on how to move forward. We need to nurture the seeds of future developments; for Mike Ireland, the IR-APD arrays are the most promising, but others pointed out that they've been around for a while and aren't a panacea. To nurture these seeds, access to facilities for testing is needed, the role that IOTA played earlier. CHARA's baselines are long, and it's oversubscribed. Funding for testing is also hard to come by. Steve Ridgway pointed out that funding for risky projects has dried up, although incremental improvements may get funding. The US NSF's Mid-Scale Innovations Program [MSIP] made only three or four awards this time around, with the maximum being about $9M.

As the discussion wound down, a few technologies that could be important were mentioned: Upconversion technology may convert from IR to telecom wavelengths while preserving phase, with no cost in quantum noise. Tracking at H or K while observing in the visible could help CHARA. Looking at correlated flux (rather than visibility normalized by the total flux) made it possible to observe a couple dozen AGNs with MIDI. That project produced one of the most-cited VLTI papers – which unfortunately makes it only a moderately well cited paper.

In wrapping up, Antoine averred that with the current facilities, we've reached a stage of maturity: facilities are working, but nothing super exciting is coming along, and we won't build anything big in the next few years.

Planet Formation Imager: Design

Moderator: Monnier
Archivist: ten Brummelaar

PFI will be science led: rather than thinking about a new technology we need to think about a main science case that non-interferometrists can get behind, but may just happen to require an interferometer to do it. We have already had a good response from scientists outside the field and many have signed up to the project.

So far, we have determined that the resolution needs to be of order 0.2 mas in order to have the same scale as the accretion disk, and that the 10um band is likely to be the most interesting. Apart from imaging planet formation areas, a large number of other interesting science programs could be pursued with this technology.

The main questions we need to address are:

- What architecture is appropriate?
- What wavelength?
- What are the enabling technologies?

Summary of Group Discussion

Telescopes

It seems likely that PFI will require 4m class telescopes. The large number of telescopes, and our very specific requirements like a very small field of view, have the potential to reduce costs. However, there was nevertheless a consensus that the telescopes will be major cost driver. Many expressed a dissatisfaction with the response of telescope vendors in the past, in the sense that they simply wish to provide what they are used to providing and are rarely interested in investigating new models of construction. Several specific telescope architectures, numbers of telescopes, and array layouts were discussed, for example lightweight carbon fiber frames, but there was no consensus on what would be best, nor on what the scaling law for telescope costs with size and number of units might be.

If there was some way to get enough sensitivity to enable fringe track in the science band – in other words do it all heterodyne and have no delay lines at all – this would be a major simplification on many subsystems, and in particular the telescopes. Longer wavelengths (25-40 microns) might enable this.

Delay Lines and Beam Transport

Even if it is assumed that Heterodyne beam combination will enable the science case, large delay lines and beam transport systems will be necessary for fringe tracking, albeit over smaller baselines than the full array. It is not clear that the current delay line technology will scale up to the kilometer lengths that will be necessary. A major concern is the size of the beam required to avoid diffraction problems, although these can be to some extent offset by using a variable curvature system such as currently used in the VLTI. Very long vacuum systems will also be expensive. The use of fibers would avoid these problems, but it was thought that the use of fiber technology for beam transport and delay control was not at this time mature. Nevertheless, fibers are already playing roles in specific parts of some subsystems, for example beam combiners, and if ways can be found to use them in beam transport they will have a major impact. For example, if you are using heterodyne beam combination for the science, you only need to have beam transport for one small band for fringe tracking, and this may already be possible with fibers.

Some array morphologies can be used to reduce the need for very large delay lines, but would require using the 3rd dimension, with for example balloons. Beam transport and delay lines will, like the telescopes, be a major cost driver.

Automation

PFI will not be possible without a large amount of automation, perhaps more than exists in current interferometers. Fortunately there are enough examples of robotic telescopes, even within our own community, and it is unlikely that this will be a major driver for either cost of complexity.

Economics

Every aspect of the discussion eventually led to cost drivers. Whatever the scaling law turns out to be, this will have to be the foremost thing in our minds. No one present felt that it was not possible to build PFI, even with our current ideas about beam combining and beam transport, but the cost will likely be high. Putting this another way, given enough funding we are confident PFI could be built, and that to some extent we need to have a minimum science case, and we need to stick to that minimum independent of cost. Furthermore, it is very difficult, if not impossible, to arrive at a realistic costing now, and even possibly when we start looking at specific architectures. For now, we cannot even agree on a scaling law.

PFI in Space

It is quite possible, indeed likely, that a solution to the PFI architecture may be a space mission. There are concerns about free fliers, but many felt that this has already been demonstrated, but that this has yet to be recognized by some parts of the community. It is a matter of system complexity cost for PFI, not an ability to maneuver in space. Fortunately, you only really need to have detailed knowledge of the baseline, and not necessarily detailed control. Given this, we need to ensure that PFI is decoupled from the old discussions of TPFI. More than the difficulties of free fliers, one major area of concern in a large baseline interferometer in space is the cost in fuel of re-pointing it.

Planet Formation Imager: Science

Moderator: Kraus
Archivist: Payne

The suggested primary science cases include: Protoplanetary Disk Structure & Physics, Planet Formation Signatures in PMS Disks, Protoplanet Detection, Late stages of planetary system formation, Exoplanetology/ Population Synthesis, Planet Formation in Multiple Systems, and Star Forming Regions/Target Selection. Following Stefan's introduction a number of additional science goals were suggested. These included the study of planet migration, the study of planet formation at different evolutionary stages, the study of moons and rings of binary planets, and of magnetic fields, or of formation star multiplicity on scale of solar systems. But there was a general feeling too many goals dissipated the impact of one overwhelming science case. One primary, killer science goal is required and all the others are secondary science cases under the umbrella of the primary goal and that planet formation is the top level case. This despite the suggestion that star formation is a key part of planet formation.

It was acknowledged that it is hard to get to planet formation, harder to get to terrestrial planet formation. Additionally, we have to investigate questions such as: how small of a planet we can go, and for how long the thermal signatures of transient impacts like the moon forming impacts can be detected.

The Science Case and Technical Requirements
Discussion then turned to the connection between the science case and the technical requirements. A strong point was made that we need standardized descriptions of the primary and secondary science cases so has to help the technical people - i.e., what sensitivity, what wave band etc...

It is especially important that other technologies are evaluated so as to ensure that there is not a cheaper or better way of doing the science.

Wavelength
There was prolonged discussion of the optimal wavelength band which can be summarized by the table below :

Thermal dust emission (disk structures, protoplanets)	MIR
Thermal dust emission (disk structures, protoplanets)	MIR
Line emission (Accretion, Water, …)	NIR(1-3 μm)
Polarized scattered light (increased contrast)	NIR

On the one hand, many scientists would be interested in what is happening with the chemistry, where the carbon is, and this is a broad band study; we need to decide which part of the IR band is the best.

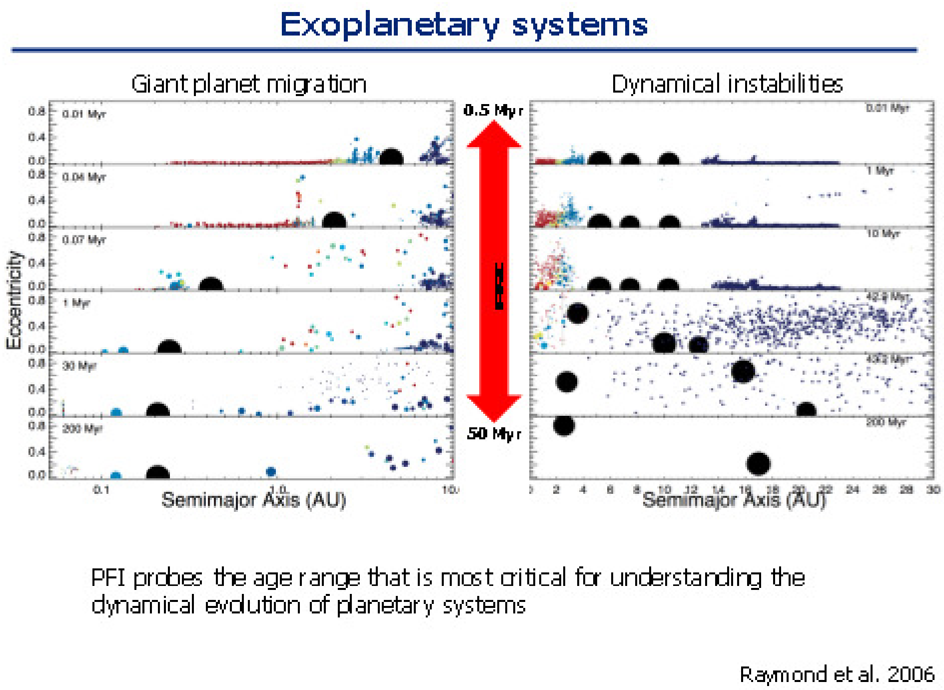
If you want to see where the higher energy phenomena then near IR, 4 microns, is what is needed. The trade-off between near IR and submillimeter is important so that people cannot say that the work can be done with an outlier of ALMA.

Heterodyne beam combination provides excellent spectral resolution.

Field of View
A decision regarding the optimal field of view has a bearing not only on the science case but also on the on the physical design of the interferometer, in this case the baselines. The statement that anything beyond 50AU is outside our interest, so the optimal FOV is 10 - 50 AU and since -the field of view is related to the baselines, maybe we should stay with long baselines, was followed by a gradual narrowing of the FOV. Firstly, by following a line of

thought that wide planets are rare, and that the core accretion process, all the interesting action, occurs within 5AU, so PFI has to be 1 - 5AU. Further discussion regarding the fact that significant flux is in a small field of view, seemed to suggest and even narrower range for the FOV, though the questions remains open as to whether it is enough to see only at 1AU.

Age Range



What age range should we go for? 10 million years? or 600 million years which covers the change in order of the planets?

The statistics of late planetary arrangement can be better done with other instruments. So PFI should stick with early phase of giant planet formation?

Polarization
Do we need the ability to measure the degree of polarization? Or should wait wait until all the results of MATISSE are in?

Not a Pathfinding Testbed
It should be stressed that the concept of PFI being a pathfinder or a test bed should be avoided. In fact, the words “pathfinder” and “test bed” should be avoided.

Conclusion
The most significant science case will only develop after the PFI is operational.

VLTI developments / VLT in the ELT era

Moderator: Berger
Archivist: Malbet

On the immediate VLTI horizon are a couple of meetings; namely, the VLTI Community meeting at the end 2014, and then a more general ESO in the ELT era conference, early 2015. The overall context of VLTI currently is as follows:

- The aftershocks after the cancellation of PRIMA astrometry. Problems of mismanagement were specifically named
- Still, a recent Visiting Committee specifically identified "VLTI is the future of milliarcsecond astronomy … even in the ELT era"; VLTI remains very high in the ESO priorities
- Major infrastructure modifications are currently ongoing, including full transformation of the lab; dual-beam star separators on all telescopes; AO on ATs; infrared wavefront sensors at the UTs; and a lot of expectations on GRAVITY from the broad ESO audience

On that last point, as of mid-March next year, VLTI will be shut down for "open heart surgery" for the installation of GRAVITY (and removal of MIDI); the shutdown is anticipated to last approximately 6 months. MATISSE is also anticipated for delivery in 2016. This instrument completed its last PDR noting that fringe tracking was not absolutely required to meet its science case; however, noting the success of PRIMA FTK for MIDI, the current situation is that GRAVITY is being examined as the FTK for MATISSE.

There are 4 levels of developments driven by science

- small instrument upgrade
- new visitor instrument ? PIONIER has been *very* successful
- 3rd generation instrument
- VLTI extension (e.g 10 fixed ATs): vs. ELT, PFI pathfinder; however, the current ESO administration is not receptive to the notion of additional telescopes.

As with the 2013 Forum, the notion of combined CHARA-VLTI observations was raised. Each facility could provide very complementary data on mutually observable objects.

Steps to space-based facilities

Moderator: Rinehart
Archivist: Mozurkewich

"The power of beam combination in space"

This is a US-centric view. One decade ago, the future of interferometry in space looked rosy. SIM looked good. DARWIN and TPF-I were both viable programs and a combined mission seemed possible. We also had mission studies of SPIRIT, Stellar Imager, SPECS and MAXIM. But within two years, everything fell apart. About 2004 TPF-C appeared as an alternative to TPF-C. Around 2006, TPF-O began to look like a viable alternative. In 2007 TPF-I and TPF-C were put on hold, and the SIM budget was zeroed out. Then the ASMCS study came out where the only interferometric mission was Planet Hunter, a stripped down version of SIM. Since then, interferometry in space has been dead. But we can still benefit from all concept studies.

Today, all we have are two balloon experiments FITE (Japan) and BETTII (USA), both FIR instruments. Ground-based interferometers continue to advance, and are pushing the state-of-the-art, but laboratory testbeds that were doing development specific to space-based interferometery have largely stopped work due to lack of funding.

Although the present state of interferometry in space is dismal, there may be reasons to be optimistic about the future. Last year, the NASA astrophysics road-map committee reported that in the near future we need a FIR interferometer and longer term at other wavelengths. In fact in the visionary time frame *every* NASA mission is an interferometer.

These missions are all a long way in the future; what we can do now? Perhaps, we need to consider a small, technology demonstrator in the $50-$100M range. There is no need for science. Funding through NASA but the exact route is unclear, perhaps the STMD.

Perhaps another possibility is nanosats for tech demonstration. Free-flying technologies have already been demonstrated and a free-flying nanosat mission (CanX-4 and CanX-5) will be launched within the next couple of days (note: launched on an Indian rocket on June 30).

NASA funding is essentially flat, including big missions such as JWST. When JWST launches and it's funding drops, that money will go into WFIRST. We need to be ready for when WFIRST-AFTA launches.

Going into the next Decadal, there may be an opportunity for interferometry in the far-infrared. The exoplanet and optical/UV communities are forming up behind ATLAST, a 8-16 meter telescope with a coronagraph (or potentially an occulter). If history is a reliable indicator of the future, missions tend to drop in size after a big one so the mission so as WFIRST-AFTA is smaller than JWST, the mission following WFIRST will be smaller than WFIRST. Since ATLAST might be seen as too big, there may be an opening at that time for a "small" interferometer (price tag of <$1B) with a clear, targeted science goal at that time. Perhaps an FIR interferometer.

The question put on the table for discussion were

1) Should an interferometer be put in space?

2) What lessons can we learn from the 2006-2008 space interferometry implosion and how can we avoid similar events in the future?

3) What is the smallest, i.e. cheapest interferometer that can provide science return that can justify the money? Perhaps, the least-expensive option that does science is too large; if so, is the only feasible option a technology demo?

4) What should we be pushing to the decadal committee and can we present a unified story from our community?

van Belle pointed out that SIM and TPF-I were not really interferometry missions since astrometry and nulling are much harder problems than detecting fringes. There have been no real efforts to put an imaging interferometer in space.

van Belle also pointed out the reality that all space missions have a near-death experience before they get funded. That the interferometry missions are in low esteem today, does not mean they will never fly; we just need to keep advocating.

Rinehart pointed out that explorer missions have a real advantage over larger missions because they can be funded without support from a large community. They just have to make a compelling though narrow science case. What is the largest interferometer that can be put in space as an explorer mission (~$200M) that may be realistic? Stephen is taking a serious look at this. Connected to this is the promising FP7 activity in Europe callsed FISICA (Far Infrared Space Interferometer Critical Assessment)[1].

As far as a good science case is concerned, Buscher claimed that it is important to propose science that cannot be done from the ground. Theo added the obvious: astrometry and nulling should be done from space and therefore always provide justification for a space mission. Although that may be true, an interferometry mission will never compete with GAIA.

Matt Muterspaugh asked if studying exoZodis was good space science? Although many present thought this was reasonable, missions proposed for space (i.e. FKSI and Pegase) are only about a factor of a few better than what people say they can do from the ground. We can argue about whether the claims of ground-based performance are realistic but it is the claim rather than the true capability that determines the scientific case for going to space.

HST has an interferometer on it the fine guidance sensor but few one used it; how do we get the community to use one? A key limitation for general HST FGS use has been the lack of an available, easy-to-use data pipeline.

Mike Ireland responded to question 2 with the statement that the cost for interferometry is way higher than other missions. In support of Ireland, Rinehart pointed out that the cost of JWST is not really all that bad. Spitzer was the smallest of the great observatories at the same cost as Kepler, $800M. Also note that JWST is an interferometer in the sense that it has several mirrors that need to be co-phased.

Even though interferometry may be inevitable, we still need to worry about this issue because it is not clear that the US will lead a future interferometry mission. In 2 to 3 decades, neither NASA nor ESA will be the largest space agency.

The NASA astrophysics road-map includes FIR interferometer now so we will be readier to do interferometry at other wavelengths when the time comes.

If Rinehart had absolute power (and that would be scary), we would be funding a 10 meter interferometer in space today. The group voiced some agreement with this wish but argued that 10 meters is a little too short.

An important question: what is the longest boom we can reasonably put in space? 20 meters?

At this point in the discussion, Gerard van Belle noted that the ability to put a 3-D printer in space could be a real game changer. Then we could build the large structures we need on orbit. This has the potential of eliminating the current constraint on shroud size. Will this require interaction with astronauts?

On Tuesday, there was a talk (Polidan's talk in UV/Optical/IR Space Missions) about a space telescope that starts with 3 hex segments. Then every 5 years or so, 3 more segments are added until it expands from a small 3 m telescope into a 16 meter telescope. One should be able to do the same thing with a structure for interferometry by adding segments a few at a

[1] http://www.fp7-fisica.eu/

time; the SPIE papers probably contain more detail on this topic. Much of the issues with a space telescope deal with the first 8 minutes of life (the launch). By building the components in space with a 3-D printer, that constraint and the cost associated with it is diminished. There is also a lot to be gained from launching identical components, Perhaps as much as a 2x or 3x reduction in cost.

The big structures we need for interferometry will be floppy and will need metrology. The metrology developed for SIM was really pricey and gives space metrology a bad name. However, SIM needed metrology a couple orders of magnitude higher than our needs. The important point here is that metrology gets really difficult in the couple nanometer range where self-interference and a host of diffraction and thermal problems that all start to contribute to the error budget. The interferometry needed for an imaging interferometer is (arguably) at or just above that cost knee making metrology for an imaging mission much more realistic (despite perceptions).

Peter Tuthill asked if putting an interferometer on the moon is still as crazy as it was 10 years ago when it was seriously being proposed. There are claims that dust on the moon is a show stopper but the corner cubes are still as reflective as ever. That said, the dust has a smaller effect on optics than it will have on mechanical mechanisms. It is claimed that the fine dust will get into bearings and quickly grinds them into uselessness. No one present knew whether that concern is real. If it is real, there should be an easy way to seal the bearings to mitigate the problem.

If we are dropping a big instrument into the moon's gravity well, it should not happen until we have a moon base since it is easier to get to space than it is to get to the moon. There were mixed opinions in the room on whether the moon is helpful or a hindrance for an interferometer. It could help by providing a solid platform on which to work and it rotates so we don't have to move the telescopes. On the down side, it gets in the way (blocks half the sky) and has large temperature swings. Automatic deployment is a lot easier in space because you don't have gravity pulling on everything. Nevertheless, there may be good places for an interferometer on the moon. Shackelton's rim (near the south pole of the Moon) is always in shadow with a nearby peak that's always in the sun. It was argued that it is hard to get that in space even though one side of a spacecraft is always in sun with the other side in shadow.

Databases

Moderator: Xavier Haubois
Archivist: Guillaume Mella

Xavier Haubois introduced the subject before opening the discussion. He reminded people that the 2013 Forum Executive Summary encouraged us all to share catalogs of past observations as well as providing access to existing archives. All the major current and future data providers were listed and the web page oidb.jmmc.fr was presented a way to connect them all. One plot about the use of interferometric facilities was shown to advertise the OLBIN publication database. A survey was circulated and thanks to the replies of 29 interested persons (2 were not), we found that people want mainly calibrated data in OIFITS format. An efficient submission process is also desired. Some concern was voiced about reduced data being public before astronomers can actually publish them, suggesting that embargo policies must be discussed.

Comments about the OiDb (oidb.jmmc.fr) showed the need to have data from more instruments than currently available as well as preparation for the future GRAVITY and MATISSE data. The OiDb web portal should also give information about data quality and calibration method.

The following points were raised during the discussion:
- Describe the calibration step: a grading scheme needs to standardize
- Other data products (e.g. reconstructed images in FITS, models)?
- Legal aspects of data sharing
- Hosting, embargo, private access, web 2.0
- Establish a coordination road-map
- How do we attract other communities to use our data and DBs: advertisement in conferences, what else?

* First reactions encouraged data sharing and expect that such databases provide good references to publications and incorporate additional data than OIFITS. Access to these data is intended to be open to any astronomer. It should give information to help interpretation, and make interferometry less scary in order to help science programs.

* We need to demonstrate that we have tools to diffuse and promote data to make proposals stronger.

* Archiving at Keck had good results because it was fully automated. Good submission process and coordination will have to be organized to reduce the burden on the user. For example, at CHARA, a huge backlog of data exists that does not contain the name of the program PIs, etc. The PI name has been provided in the file headers since late 2013 only. Fixing this backlog is probably too much work and is probably not necessary for the moment.

* Following the last beauty contest, we can see that we are not ready for 100% reliable image reconstruction. **We therefore choose not to distribute reconstructed images for the moment.** Model results, however, could be data-product to be made available in the database.

* Despite this lack of maturity, we should not be too worried about inviting all users to search the database. Indeed we should make it attractive to do so, and encourage people to use these data to complement other observations. **We should therefore focus on making a good database for interferometrists in the first place.** If a user needs interferometric data, s/he might collaborate with a specialist, that is someone who can deal with coherence variance for example. Databases should focus on providing good data for reuse. Some people said OiDb should allow specialists to get data, non-OI specialists will get that from the literature.

* Comments on the data by users should be available in order to improve feedback and collaboration (Web 2.0 tools). Multiple revisions of a same original file can also be imagined, for example data calibrated by different people or by using different calibrators. The difference between versions must be explicitly mentioned during the submission step.

* CHARA tries to fill observation logs with user metadata, but it is not easy, takes time, and users do not always understand the process or want to put in the time. At CHARA, observing logs automatically provides environmental data / weather+seeing/quality control information. Moreover, observers can add comments. At ESO data are graded examples

and are based on quantitative and qualitative criteria. Requests have been made to move towards a common description and specification of calculations.

* Some people do not trust calibrated data, or data calibrated by someone else. ESO push the idea of node and regional data-centers that could help a lot of users by reducing their data immediately following the night of observation. A lot of complexity in the use of OI data would then vanish. However, most of the early archiving effort should be put on published data. These data must be public so one can do additional computations, compare, modeling... or at least reproduce the published results.

* Publication is probably the best time to pickup data before the PI relocates or forgets where they are. Getting scientist to share their published data is very hard even though journals already provide hosting services for digital data (online material). **This sort of action should become compulsory and an official recommendation from the IAU Commission 54 for PIs to make their published data publicly available has been adopted during this session.** OiDB could then harvest these data to centralize their access.

* A question was asked to probe the interest for having quick-look/analysis tools to automatically process data selected on OiDb. The replies suggest creating shortcuts to analysis tools such as LITpro for example. But it is too premature for image reconstruction. It would be interesting to see feedback in coming years.

* We have to take care to avoid the problem of people can writing papers without getting in touch with the data providers. Training and tutorials will have to be provided. We should also address the issue of creating a bad first impression. Interferometry is not easy and it should not give the impression that we can't rely on OI data-products. This may be time-consuming, but we should provide points of contact if users want to get help on a specific dataset.

* Some people watch actively and know very fast which targets have been observed. For example. at ESO people look at the archive every day. A data embargo should protect PIs before publication. Some long-term observation programs must get longer embargo periods before being published. PhD students could also request an adjustable embargo period. This must also be done at the raw data level. At ESO longer embargo periods can be asked for the archive side if justified. But public access must be automatic after embargo period.

* JMMC provides analysis tools with web interfaces/web applications. There was some concern that next the version will accept only data coming from the database. The use of such portals is encouraged, but tools must also work on non-database data.

* The last message is an invitation to the OLBIN community to provide feedback on OiDb in the coming weeks by filling a form (in the help menu) and wait for the official release of the first version of the database before advertising to a wider community.

New Developments for Operational Facilities

Moderator: Tuthill
Archivist: Thiébaut

Some minor corrections about the slides:

* fix "current facilities" table for the VLTI: AMBER has high dispersion not PIONIER (only 7 spectral channels)
* perhaps complete this table to indicate actual spectral resolution of the different instruments

About the evolution of current facilities:
* Should we fill the boxes, i.e. have all fields (vis, NIR, MIR, nulling, astrometry) covered by all facilities or by at least one for the North hemisphere and one for the South hemisphere?

Some concerns about astrometry:
* NPOI astrometry capabilities will be challenged by Gaia results, although the specific bright star performance of the space mission remains unknown. NPOI has the capability to extend astrometric work into the NIR/MIR, though.
* Astrometry requires at least accuracy 100µas but it is still hard to achieve <1 mas (cf. PRIMA). GRAVITY should improve on that, building upon a PTI PHASES-like capability that already demonstrated extreme-narrow-angle performance in the ~10µas regime.

Spectral resolution:
* Andrea Quirrenbach would like to have very high spectral resolution (at the level required by spectroscopists). Vega has the highest resolution (30,000) among the existing instruments. This is however in the visible and not so many astronomers use that resolution with Vega.
* Without noiseless detectors, high spectral resolution is only possible if coherent fluxes can be integrated (typically during a couple of minutes). This highly depends on fringe tracker performances. Otherwise, high spectral resolution will be limited to very bright stars. There are great hopes in the fringe tracker of GRAVITY.

Facilities upgrade (i.e. what can be done in practice to extend exiting facilities?):
* Going toward high precision, e.g. by using differential measurements.
* A 7th (or even an 8th) telescope for CHARA to increase the largest baseline or to fill the gap at the smallest baselines.
* It seems to be worth extending NPOI to the MIR.

Other/new facilities:
* Dave Mozurkewich mentioned SPOT(?) which is a 3x1m telescope interferometer with AO, fiber fed visible beam recombiner, H-band fringe tracker, up to 100m baselines movable within minutes (which assumes that calibration mostly depend on the observation time and baseline orientation rather than on the baseline length). This interferometer is not supposed to be an astronomical facility but it is not yet funded and they may be interested in collaborating. Nevertheless it will use stars for calibration so it should be ready for astronomy.
* There are long term developments at IRCOM (Limoges) to exploit telecom fiber technology (for beam transportation, delay lines and recombiner). This requires wavelength shift/conversion (but this is not the same as heterodyne mixing).
* Is intensity interferometry a valuable niche for very bright stars?

Wrap-Up & Endorsement

Moderator: van Belle
Archivist: Buscher

The 2014 Interferometry Forum formally endorses the Planet Formation Imager as a unifying theme for future development of optical interferometry.

Forum "Charter", version 2014

The International Interferometry Forum

The Forum will organize occasions and channels for communication, facilitate coordination in planning, and encourage and promote opportunities for technical and scientific collaboration, both within and beyond the interferometry community.

The Forum will operate as an element of IAU Commission 54. The commission officers will take initiative and personal responsibility for ensuring some Forum activities. Initially, these will include: organizing annual Forum gatherings, continuation of the online OLBIN functionality in a more sustainable incarnation, and implementation of social media networking opportunities, such as an IAU C54 Facebook page.

Forum participation will be open to the community. The IAU officers will call on and benefit from the support of Forum participants in carrying out their Forum responsibilities.

Forum Participants

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Noel Richardson (U de Montreal, local host)
Olivier Absil (Université de Liège)
Tom Armstrong (NRL)
Ellyn Baines (NRL)
Jean-Philippe Berger (ESO)
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Vincent Coudé du Foresto (Observatoire Paris-Meudon)
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