Integrating and strengthening the European Research Area

ERANET

Coordination Action

ASTRONET

Coordinating Strategic Planning for European Astronomy

Contract n° 026075

Starting date: 1 September 2005 **Duration : 64 months**

Deliverable number	**D26**
Title	**Report on a wide field, highly multiplexed spectrograph**
Work package	**WP 3**
Due date	**01/03/2010**
Submission date	**28/09/2011**
Organisation name(s) of lead contractor for this deliverable	**STFC**
Prepared by	**G. Kauffmann**
Approved by	**Board**
Released by	**J.-M. Hameury**
Nature	**Report**
Revision	**V**

Project co-funded by the European Commission within the Sixth Framework Programme (2002-2006)

Dissemination Level		
PU	Public	**X**
PP	Restricted to other programme participants (including the Commission Services)	
RE	Restricted to a group specified by the consortium (including the Commission Services)	
CO	Confidential, only for members of the consortium (including the Commission Services)	

Contents and collaborators

ASTRONET Wide-Field Spectroscopy Working Group: Final Report

The Working Group has considered the most pressing needs for new instruments that should become operative around 2015-2016 that would help solve forefront problems in three different subject areas: 1) The formation history of the Milky Way, 2) Galaxy evolution across cosmic time, and 3) Cosmology. Three panels were created within the WG to identify the specific needs of each area, and the WG has convened in a joint session and by telecon and email to draft this final report.

The immediate needs of both Milky Way archaeology (complement to GAIA) and Cosmology (Constraints on the equation of state of dark energy) could be well served by a single instrument at a 4m class telescope with the following characteristics: field of view of 1 to 3 degree diameter, multiplex of at least 3000, and spectral resolution of at least 5000. This instrument is of some interest to the galaxy evolution community as well. Major progress in this field will, however, depend on the availability of a near-IR JH-band spectrograph at an 8m telescope, operating over a field of view of at least 30 arcminutes diameter, with multiplex >400, and with spectral resolution 3000-5000.

In the longer term, Milky Way studies would also profit by a high multiplex instruments with a higher resolution mode (15,000-20,000). All three subject areas would benefit immensely from a 8m class telescope with wide field-of-view that is fully dedicated to spectroscopic surveys.

This WG strongly believes that a significant investment in surveys using the existing facilities over the next 3-5 years would be very valuable. An important step has now been taken to address the needs of the Milky Way community, but the needs of the galaxy evolution and cosmology communities have yet to be resolved.

Wide Field Spectroscopy Working Group
Galactic Archaeology and Gaia Follow-up

Panel Members:
V. Belokurov, A. Ferguson, V. Hill

Summary Recommendations

The most outstanding need in the area of Galactic Archaeology is to provide facilities to conduct largescale spectroscopic surveys in support of Gaia. These surveys must be capable of measuring radial velocities and metal abundances with a range of accuracies for huge samples of stars in both the local disc and across the entire Galaxy. We recommend:

• an optical spectrograph capable of delivering low resolution (R>5,000) and high multiplex (>1,000) over a field of view of >1–2 square degrees. This instrument will provide radial velocities (2–5 km s^{-1} accuracy) and global metallicities (0.2 dex accuracy) for stars in all the major structural components of the MilkyWay down to V~20.

• an optical spectrograph capable of delivering high resolution (R> 15,000-20,000) and high multiplex (>500) over a field of view >1–2 square degrees. This instrument will provide detailed elemental abundances (0.1 dex) and high accuracy radial velocities for stars down to V~17 (primarily local disc).

In order to match the interim releases of the Gaia data (expected 2016-2017) and ensure European competitiveness in the field of Galactic Archaeology, these facilities must be in place within the next 5 years. Given the similarity in the desired FOV and multiplexing, it is possible that the two instruments could be combined into a single instrument with two resolution modes or, alternatively, be proposed as two separate implementations on two different 4m telescopes. The panel feels strongly that both the low and high resolution surveys are essential for full exploitation of the Gaia dataset. Hemisphere access is an important consideration for studies of Galactic archaeology and ideally this should be all-sky. The present document focuses solely on opportunities that can be realised with 4m-class telescopes. There is also an important role for 8-m telescopes in Gaia follow-up as they provide the only means to conduct detailed chemical abundance analyses at V>17.

Science Drivers

The Milky Way is the system for which we can hope to constrain in most detail the physical processes that play a role in the formation and evolution of galaxies. Gaia is a European space mission that will be launched in 2013 and during its 5 year mission will produce superb positional and proper motion data for a billion stars in the Milky Way. The ultimate goal is to obtain a comprehensive census of the orbits, ages, and chemical compositions of stars in all the major structural components of our Galaxy, enabling a complete reconstruction of the history of its formation and subsequent evolution. However, Gaia will not provide radial velocities for stars fainter than V= 17, chemical compositions of stars fainter than V~12 or elemental abundances of any sort. As a consequence, the dynamical map of the Milky Way will be incomplete and restricted to a few kiloparsecs within the Sun. Furthermore, Gaia will contribute little directly to understanding the chemical enrichment history of our Galaxy. Both of these factors will seriously limit our ability to address many high-priority science questions.

In order to fully exploit Gaia's dataset, there is an outstanding need for large spectroscopic surveys which can provide complementary radial velocities and chemical abundances with a range of precisions. While Gaia will provide high accuracy distances and transverse velocities for stars across the entire Galaxy, it will provide especially exquisite data in the local disc. As outlined below, this mandates two surveys – a low resolution survey aimed at obtaining kinematics and overall metallicities (i.e. [Fe/H]) for all major structural components of the Galaxy and a high resolution survey aimed at obtaining detailed chemical element abundance information, necessary for facilitating stellar age determination, in a volume of a few kpc around the Sun. The importance and urgency of the Gaia follow-up program has been clearly stated in the Astronet Science Vision report (see page 67 of the report) and most recently, is re-emphasised in the report by the European Telescope Strategy Review Committee.

The need for a V> 17 Low Resolution Survey

Breakthroughs in understanding the formation history of theMilkyWay are likely from detailed studies of the kinematic structure (and substructure) and chemical content of all the major components of the Milky Way – thin and thick discs, stellar halo and bulge. This can be achieved by velocity mapping with errors smaller than 5 km s^{-1} and global metallicities (e.g. [Fe/H]) with accuracy ~ 0.2 dex. Such data will be sufficient to retrace the orbital history of stars, characterize the origin and importance of accretions and identify resonances caused by the bar and the spiral arms. Knowledge of metallicity distribution functions will aid considerably in disentangling structures and yield much insight into the star formation and mass assembly histories of each Galactic component.

The low resolution study will be of particular importance for deciphering the mode of assembly of the Galactic thick disc, for measuring the mass profile and lumpiness of the dark matter halo and for constraining the accretion history of the Milky Way. Given the dynamical times at the Galactocentric distances accessible to Gaia, few spatially coherent substructures will remain and it is likely that the remnants of past accretion events are discernible only in the full 6D phase space (and even then, the addition of metallicity information will be a crucial complement). Dynamical maps of cold streams in the halo extending several tens of degrees on the sky (only a few are known to date and they have already yielded significant constraints on the circular velocity at the position of the Sun; many more are expected to be discovered by Gaia) will be extremely powerful tools with which to determine the Galactic potential, its shape, graininess and evolution, if proper motions and accurate radial velocities (<5 km s^{-1}) are available.

The need for a 12 <**V**< 17 **Intermediate Resolution survey**

The bulk of the stellar mass in the Milky Way resides in the disc components however our understanding of how these components came into being remains rather crude. Gaia promises to change that by providing detailed insight into the star formation and chemical enrichment history of the local disc(s). While Hipparcos allowed a view of the star formation history within 100 pc of the Sun, Gaia will extend this to a volume several tens of thousands of times larger, enabling the study of gradients along and perpendicular to the disc as well as their evolution over time. Key questions include addressing whether the Milky Way disc(s) formed inside-out or outside-in, the extent to which radial migration has rearranged disc material over time, the detailed relationship between the thin and thick discs as well as other Galactic structures, and searching for evidence that the MilkyWay has directly accreted stellar material into the disc plane.

Such studies necessarily require a ‘clock’ against which to measure time. This is most directly achieved through stellar age determination, a straightforward process if stellar parameters such as distance, [Fe/α], T_{eff} and logg are known to very high accuracy. Gaia will perform exquisitely in the local volume where the distance accuracy will be ~ 1% within 1.5 kpc (10^7 stars) and ~ 10% within 3 kpc (10^8 stars). Unprecedented insight into the formation and evolution of the disc(s) can be gleaned if Gaia data for these populations is complemented with high precision chemical abundance ratios (accuracy ~ 0.1 dex for at least 5–10 elements) and radial velocities (< 1 − 2 km s^{-1}). Further afield, chemical element ratios themselves can provide a crude clock. Because different elements are released into the interstellar medium on different timescales, chemical abundance ratios contain information about the conditions in the gas out of which a given generation of stars formed, and in particular the relative contributions of Type Ia and Type II supernovae at that time.

The need for a high resolution survey is thus to capitalize on the precision data that Gaia will provide for stars in the local volume. Such a survey is currently being planned in the southern hemisphere with the HERMES instrument on the AAT. A European-led northern counterpart would be highly complementary and ensure European competitiveness in this key area.

Technical requirements

- **Low Resolution Survey:** The survey requires a minimum resolving power of 5000 in order to provide radial velocities with accuracies < 5 km/s and metallicity measures to 0.2 dex for stars in the range 17 < V < 20. This radial velocity accuracy provides an excellent match to the expected transverse velocity accuracy to be obtained with Gaia (~ 5 km s^{-1} out to ~ 10 kpc). A range of target densities is envisaged depending on Galactic latitude but there is a clear need for a multiplex of at least 1000 over a FOV of >1–2 square degrees to efficiently survey regions near or in the midplane. There is no strong demand on wavelength coverage but the spectral window and resolution should offset each other in a way that preserves the requisite radial velocity accuracy. For example, a narrow spectral window centered around the Calcium triplet is feasible only if the resolution is significantly higher than R~ 5000 in order to compensate for the lack of lines available to work with. Alternatively, a broad window from 4000-9000Å at the minimum resolution is also viable. A survey of approximately 10^6 stars would provide excellent sampling of all major structural components of the Galaxy, including the remote stellar halo.

- **High Resolution Survey:** The survey requires a R > 15,000-20,000 in order to provide abundance measurements for at least 5–10 chemical elements with accuracies of ~ 0.1 dex and radial velocities with < 1 − 2 kms^{-1} accuracy in the range 12 < V < 17. The primary pointings will be the near and in-plane regions of the Galactic disc hence target densities will be high and a high multiplex (>500) over a field of view >1–2 square degrees is again required. A wavelength coverage of *at least* 500-600Å is required although this need not be contiguous and there is flexibility in the exact choice of spectral windows. A sample size of 10^6 stars is again envisaged, composed mostly of local disc dwarfs but also a sizeable number of distant giants.

Synergies

The proposed spectrograph specifications for the low resolution survey match nearly perfectly the requirements dictated by the cosmological science drivers. In other words such an instrument could serve as both a machine for Galactic Archaeology as well as one for measuring redshifts, allowing a highly efficient survey strategy - dark, grey and bright time could all be used prudently and no fiber would be spared regardless of the stellar tracer density.

Astronet/Opticon Wide Field Spectroscopy Working Group
Galaxy Evolution Panel

Panel Members:
Carlton Baugh, Jean-Gabriel Cuby, Guinevere Kauffmann, Roberto Maiolino,
Paul Nandra, Alvio Renzini

Scientific Rationale: Galaxy mass growth and assembly, the formation of black holes and stars all peak at a redshift of around 2. However, the redshift interval $1.4 < z < 2.5$ has proven so difficult to explore spectroscopically that it has gained the nickname "redshift desert". Beyond $z \sim 1.4$, the most prominent spectral features in galaxies move out of the wavelength range currently covered by optical CCDs, into the near-IR. The strongest emission lines of star-forming galaxies ([OII]λ3727, [OIII]λ5007, Hβ and Hα) and the strongest stellar absorption line features in passive galaxies (CaII H&K, 4000Å break) all become inaccessible.

It is important not to repeat the mistakes of the past. Important lessons should be learned from the zCOSMOS survey, which secured almost 10, 000 spectra for galaxies at redshifts $1.4 < z < 3$ using VIMOS at the VLT. Unfortunately, the success rate of redshift determination was found to be a strong function of redshift itself, depending on whether Ly-α (either in emission or in absorption) lay within the accessible spectral range. With VIMOS, Ly-α was not accessible until $z \sim 1.8$; the range $1.4 < z < 1.8$ turned out to be the harshest part of the redshift desert. Over this interval, redshifts had to be estimated using very weak absorption lines in the rest-frame UV continuum. In star-forming galaxies, these features were often heavily extincted by dust and spectroscopy was limited to objects brighter than $B \sim 25$ (Lilly et al. 2007, ApJS, 172, 86). In passive galaxies, the UV continuum is intrinsically very faint, so extremely long integration times (> 30 hours) were required to obtain spectra with reasonably high signal-to-noise (e.g. Cimatti et al. 2008, A&A, 482, 21).

Even more alarming than the modest success rate of optical spectroscopy in the redshift desert, were the strong selection biases introduced in the surveys. If one considers a complete, stellar mass-selected sample of 30,000 *BzK*-selected galaxies at $1.4 < z < 2.5$ over the COSMOS 2 deg^2 field, those brighter than $B = 25$ account for only ~ 16% of the total star formation rate (SFR), and a similarly small fraction of the total stellar mass (Renzini & Daddi 2009, ESO Messenger, 137, 41). Many of the most intensively star-forming, and most massive galaxies were missed. These galaxies are faint in the B-band, but very bright in the *J* and *H* bands, and hence easy to observe in the near-IR.

We have now learned that only by combining a massive spectroscopic survey in the optical with coverage of the same field in the near-IR, will it be possible to put together a large, unbiased sample of $z \sim 2$ galaxies. A survey with at least 10^4 galaxies is required to properly map the large scale structure (LSS), and assess the role of environment in influencing galaxy formation at this critical cosmic epoch. We also note that successful spectroscopic studies of galaxy evolution require *high signal-to-noise spectra* in order to make high fidelity estimates of physical parameters such as stellar mass, velocity dispersion, star formation rate, metallicity and black hole accretion rate. This creates some tension with the cosmology community who generally only care about surveying large volumes, and also do not concern themselves with selection effects that impair ones ability to track all galaxy populations across cosmic time. Although the two communities can in principle share the same facilities, we recommend that they should not share the same *surveys*.

Existing Near-IR Multiobject Spectrographs

Several near-IR MOS instruments are now in operation or about to be completed. Most of them cover the *K* band, so they are cryogenic instruments. The extended spectral coverage extending to the thermal IR (K-band) comes at the expense of the field of view, which ranges from ~ 12 to 40 arcmin^2. The number of telescope nights required to cover 1 Deg^2 and secure ~ 10, 000 spectra is around 200-500. In principle, the *JH*-band spectrograph FMOS at the Subaru telescope should have been be far less demanding in terms of telescope nights (only around 50 would be required), because of its large field of view and high multiplex capability, if it were to achieve its nominal sensitivity. FMOS is not a cryogenic instrument, so it is possible to expand its field of view. Thanks to its fiber links, it can achieve a factor 10 higher multiplex than cryogenic instruments. The surface density of fibers on FMOS (~ 0.5 arcmin^{-2}) is also a good match to the surface density of interesting and feasible objects at $z \sim 2$ (1-3 arcmin^{-2}). Therefore, had FMOS reached its nominal sensitivity, it would have been the *only* instrument worldwide where a massive spectroscopic *JH*-band survey of $z \sim 2$ galaxies could possibly be attempted.

Why Europe needs its own highly multiplexed near-IR spectrograph

All 8-10m class telescopes are equipped with optical and infrared imagers, adaptive optics devices, highresolution optical and near-IR spectrographs, but only one such telescope has a high-multiplex *JH*-band spectrograph, and it apparently is not working according to specification (The low throughput of FMOS is apparently associated with its OH suppressor system.) This panel feels that it is extremely important for European astronomers to have access to an FMOS-like instrument with (hopefully) better performance. Now that VISTA has started its science operations, European astronomers will have access to the largest near-IR imaging surveys in the world (in terms of coverage/depth combinations). VISTA surveys will inevitably generate a high demand for near-IR wide field spectroscopic follow-up, for a variety of cosmological and astrophysical applications.

Near-infrared integral field spectrographs, such as SINFONI and KMOS, require targets with known redshifts, so that critically important spectral features (e.g., Hα, [OIII]λ5007, CaII H&K, etc.) are free from atmospheric absorption, and the maximum physical information on the sources can be obtained. Surveys undertaken with such instruments will be limited to samples of a few hundred galaxies at most. It is very important that the targets for these limited surveys be culled from large, complete samples that are as free from instrumental selection biases as possible. As we have discussed, this is only possible if we are able to extend current wide field optical spectroscopic surveys to near-IR wavelengths.

The minimal specifications of the near-IR facility can be summarized as follows:

FoV: 25 arcmin diameter
Multiplex: > 400
Spectral coverage: ~ 0.9 − 1.8 μm
Spectral resolution: ~ 5, 000
Total throughput: > 15% (i.e. a factor 2-3 better than the current performance of the FMOS instrument:
http://www.naoj.org/Observing/Instruments/FMOS/performance.html)

Extreme wide-angle multi-object spectrograph for cosmology

Panel members:
Will Percival, Carlton Baugh, Txitxo Benitez, Rita Tojeiro

Summary We recommend a multi-object (~4000 fibre) spectrograph, working in the optical, with a minimum spectral resolution of R = 2000 to be put on a 4m telescope. A crucial component of the instrumentation package will be a wide field corrector that would ideally take the field of view to a minimum of 3 square degrees, although a field of view of a few square degrees would still be competitive given the state-of-the-art multiplex capability. To be competitive, this spectrograph would need to be on a 4m telescope within 5 years.

Brief review of cosmological science drivers: Understanding dark energy is one of the most compelling problems facing physics today. Galaxy redshift surveys provide two key mechanisms by which we can measure the properties of dark energy: Baryon Acoustic Oscillations act as a "standard ruler", giving a robust tool for studying the accelerating expansion of the Universe as a function of redshift. Galaxy surveys provide complementary information about the rate of build-up of large-scale structure through redshiftspace distortions. These are caused by coherent comoving galaxy velocities, which lead to a measurable anisotropic clustering signal when these velocities are misinterpreted as being due to the Hubble flow. In addition, by comparing galaxy clustering measurements for galaxies of different types, and by combining with CMB observations, we can constrain galaxy formation models, and measure the matter density Ω_m, galaxy bias, the sum of the neutrino masses and the primordial power spectrum shape created by inflation.

Beating the competition: In order to continue with recent progress in the development of galaxy redshift surveys, the next generation of surveys will need to provide ~ 10^7 spectra over the full extragalactic sky visible from any site - up to 14 000deg^2. In terms of redshift, these surveys will need to push beyond z = 0.6, the current redshift limit of the SDSS-III, Baryon Oscillation Spectroscopic Survey. Between $0.6 < z < 1.0$ we can still select massive galaxies and expect to be able to measure redshifts from the standard features such as the H & K lines. For redshifts $1 < z < 1.4$, we will need to select emission line galaxies from the Balmer absorption break using standard grz images from wide-field imaging surveys. These include Pan-STARRS in the North, or from DECam or VST in the South.

Field of view and multiplex: To survey 10^7 galaxies in a reasonable total survey time (~ less than 6 years), even with a dedicated telescope, will require an instrument with an extremely wide field of view - of order 3degrees in diameter. In addition we require that approximately 4000 galaxy spectra be simultaneously measured from each telescope pointing.

Wavelength range and resolution: For the emission line galaxies, the primary spectral features useful for redshift determination ([OII], D4000, Balmer lines) are redshifted into the red region of the spectrum (600 to 1000nm). For the galaxy evolution science and Ly-α cosmological studies, the blue region (down to about 390nm, or z~ 2.2 for Ly-α) is essential. The desired wavelength coverage is therefore from approximately 350nm to 1100nm. For the cosmology case, the resolution can be quite low. The limiting factor is that we can observe between the telluric lines and maximise signal-to-noise ratio. We estimate that a minimum resolution of ~ 2000 is required. For the galaxy archeology case, a resolution closer to
~ 5000 is desired.

Observing time & overhead: we expect that integration times of order 1 hour on a 4m telescope will provide spectra of sufficient signal-to-noise to provide redshifts for the majority of an interesting sample of galaxies. The overhead requirement is only limited by the overall timescale of the survey. The survey should not take longer than 6 years from first light.

A fibre fed solution: if the instrument is to to be based on a fibre fed design, the focal plane will need to be populated by 4000 fibres that can be individually deployed such that all points on the focal plane can be reached by at least one fibre. The fibres should have approximately 150 micron cores, projecting to 1.5 arcsec diameter on the sky. If an alternative solution is to be specified we require that the same total number of spectra can be measured in a single telescope pointing and that for a particular telescope pointing, a spectra can be obtained at any point on the focal plane.

Report from an ASTRONET telecon,
held 13 July 2011, and subsequent follow-up discussion over email

On the phone:

Guinevere Kauffmann, Gerry Gilmore, Alvio Renzini, Annette Ferguson,

Will Percival, Carlton Baugh, Roberto Maiolino, Jean-Gabriel Cuby

The purpose of the telecon was to discuss "latest develoments" with regard to the out come of the ESO Public Surveys Call and theWF Spectrograph Development call. We went through each of the three subject areas and asked which science needs would be met by the surveys and/or instruments under development, and what was still missing.

1. GALACTIC STRUCTURE A public survey using FLAMES and UVES on VLT was approved that will obtain R~20,000 spectra for 10^5 stars and R~50,000 spectra for 10^4 stars. This will be an unbiased sample of all Milky Way components, including bulge, disk, thick disk and halo. It will be carried out over a period of 5 years. This survey will inform target selection strategy for next generation efforts to obtain large samples of very high resolution spectra.
With regard to large surveys of stars at low resolution (R=5000-7000), preference was expressed for the 4MOST spectrograph, which is proposed to go on NTT or VISTA. This was said to be preferable to MOONS because of the large field-of-view and high multiplex. Ideally, a lower resolution survey would cover both hemispheres. The WEAVE instrument on WHT was one possibility. MOONS would, on the other hand, provide a valuable database of high resolution spectra of reddened stars in the inner disc and bulge of our Galaxy.

2. COSMOLOGY No ESO public survey of interest was approved in the last round of applications. The 4-MOST spectrograph concept was discussed, as this was recently approved for further development by ESO. In order for this instrument to be of interest to cosmology, it needs to push towards 3000 fibers (the upper end of the range being considered). If this cannot be achieved, the cosmology experiments that could be performed will not be competitive with regard to proposed US experiments: DESpec on the CTIO 4m in the south, a project that is being put forward by Fermilab, and BigBoss in the North. It was agreed that in the ideal world, the Europeans and Americans could coordinate their WF spectroscopy efforts on 4m telescopes such that the Americans

would support a project in one hemisphere, and the Europeans a complementary project in the other (i.e. WEAVE/DESpec or BigBoss/4-MOST).

3. GALAXY EVOLUTION No public survey was approved by ESO, due to the fact that there was too much time already allocated to the fields of interest, making new surveys impossible to schedule for at least a year. The main instrument that is of interest to galaxy evolution science is the MOONS NIR spectrograph proposed for the VLT with a field of view of 25 arcmin, wavelength coverage from 0.8-1.8 micron, R=3000-5000, and 500 fibers (500 additional fibers would have to be on sky). Unlike FMOS, MOONS does not have an OH suppressor. The high resolution ensures that more than half of the H-spectrum is free of OH sky emission.

   There is also interest in highly multiplexed IFU surveys of galaxies, such as those suggested for the WEAVE instrument. We note, however, that plans are under discussion to conduct a massive IFU survey of 10,000 nearby galaxies using the exisiting SDSS spectrograph on the Apache Point Observatory 2.5 meter telescope in New Mexico, and there is substantial European involvement in this effort already.

Finally, the committee noted that the Prime Field Spectrograph on Subaru was significantly more ambitious than any of the instruments described above. The project would include installation of 2400 fibers at Subaru's prime focus, with a 1.5 degree field-of-view. The wavelength coverage is envisaged to extend from 3800Å to 1.3 μm with a "triple spectrograph" costing around 50 million dollars. The resolution in the optical will be around 2000 (twice as high in the near-IR). We note that no comparable project is currently under serious discussion in Europe. A partnership with the Japanese involving a small number of institutes in the US and Europe is currently falling into place and operations are sleighted to begin in 2016.

In principle, PFS could cover most of the main science requirements of *all three subject areas*: a) It would permit wide angle radial velocity and global metallicity surveys of Milky Way stars down to significantly fainter limits than possible with 4-MOST (Subaru is an 8.2 meter telescope at an excellent site), b) Because of the continuous wavelength coverage from the optical to the near-IR, there is no "redshift desert" with PFS ([OII] is detectable out to z = 2.5 and Lyα from z = 2.1 to z = 10, c) The wide fieldof-view makes PFS ideal for cosmological applications. If PFS goes ahead, this would place the Japanese and their partners well ahead of the competition for the foreseeable future.

We note that one factor limiting the impact of an instrument such as PFS will be the time available to conduct surveys, because of pressure from the Japanese "non-survey" community. In addition, coverage of the surveys would then be heavily skewed to the Northern Hemisphere, which would disadvantage followup efforts by the ALMA telescope, where there is heavy Japanese investment. Although the Japanese astronomical community has traditionally been shy about international collaboration, we feel that the scale of the likely impact of the PFS on the development of astronomy does merit some change in thinking by ESO.

Appendix:
Terms of reference of the
European Wide-Field Spectroscopy Working Group

Final version, 17 Sept. 2009

Preamble

The ASTRONET Infrastructure Roadmap made a specific recommendation concerning European access to wide-field multiplexed spectroscopic capabilities (Sect 8.2.2):

Wide-Field, Multiplexed Spectrographs
There are compelling and fundamentally important scientific cases for the development of wide-field, highly multiplexed spectrographs to be placed on an existing 8-10m class telescope, and consequently such a project was given very high scientific priority. It should enable massive spectroscopic surveys of a million or more objects at a speed and on timescales compatible with the next generation of wide-field imagers, e.g. the Large Synoptic Survey Telescope (LSST). The primary science drivers are the determination of the equation of state of Dark Energy, the study of stellar populations over a large fraction of the history of the Universe, and the study of the structure and formation of the Galaxy and Local Group by determining in a quantitative manner the kinematical and chemical signatures of the different stellar components.

Considering the enormous scientific value of wide-field spectrographic surveys and their under-representation compared to imaging initiatives, we recommend setting up a working group, under the auspices of ASTRONET, with OPTICON, with the task of i) developing the top-level requirements of the surveys, ii) identifying implementation options on a European scale, iii) establishing the merits of these options with a trade-off analysis and proposing an implementation plan to provide a facility for the whole European community in the 2015-2020 time-frame.

In that context, the ASTRONET Board has decided to establish a Working Group with remit: "to identify specific opportunities for highly-multiplexed spectroscopic capabilities on 4m-10m telescopes with European access, to deliver the relevant aspects of the ASTRONET Science Vision''.

The expansion of the charge (vis-à-vis the Roadmap) to also consider 4m telescopes was driven both by the prospect that some of the most exciting science cases may be done at comparable survey speed at smaller telescopes and by pragmatic considerations of availability of such telescope resources. While other science may potentially be accomplished through large spectroscopic surveys, the group should mainly focus on assessing the science cases and possible implementation avenues for the science areas spelled out above.

Terms of Reference

The European Wide-Field Spectroscopy Working Group (WFS-WG) is composed of scientists with internationally recognised expertise in the science that can be drawn from very large-scale spectroscopic surveys and/or in spectroscopic instrumentation. Formal representatives of those (partially-)European observatories with ~4-10m telescopes, where wide-field multiplex spectroscopy is potentially realizable will be invited to attend as observers in order to provide the requested technical input. The Chair and members of the WG are appointed by the ASTRONET board in co-ordination with the OPTICON Executive Committee.

To fulfill its remit, the WG will in particular undertake the following

1. Scientific requirements

Define, largely by collating the several extant project studies, the scientific requirements for very large-scale spectroscopic surveys, which address the key science questions identified in Science Vision document and referred to in Section 8.2.2. of the ASTRONET Roadmap document. Evaluate which of these science cases have been worked out in sufficient detail to map them into a possible telescope/instrument/survey implementation. Identify the science cases that likely require 8m telescopes, and those that may be feasible to implement on 4m telescopes.

Survey world-wide space and ground based wide-field imaging and spectroscopic projects and near-term proposals addressing these scientific questions.

Establish appropriate metrics to determine the efficacy of the various projects in contributing to each scientific question, identifying a minimum set of projects which will ensure significant advance in each key science area. Identify the top science goals from referred to in Section 8.2.2. of the Astronet Roadmap, for which no (sufficient) ongoing implementation plans exist.

2. Technical implementation options

Identify possible project implementations which could be made available to the European scientific community, taking into consideration extant ~4m and ~10m telescopes and their capabilities, financial implications and project timescales.

For each option, provide an indication of technical feasibility and an approximate estimate of cost on the proposed facility/facilities.

3. Trade-offs and recommendations

Perform a trade-off analysis including scientific merit, development timescales, cost and competitiveness with other projects in a global context.

Recommend one or more implementation options, considering a mix of instrumentation, host-facility, and operational approach, potentially giving Europe a leadership position in each of the key scientific areas, identified in step 1. Global strategies, i.e. involvement of facilities without strong European participation at present, may be considered.

These activities will need direct interaction with and support from potential host Observatories, through their WG representatives. Other technical support (travel, administrative support, …) will be provided by ASTRONET and/or OPTICON as appropriate.

The WG will report one year after the formal start of the task.
The ASTRONET Board will publish this report.