SPIP : a SPIRou twin for TBL @ Pic du Midi

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KEY WORDS – SUMMARY

Summary	We propose to build a SPIRou twin for TBL @ Pic du Midi, that we call SPIP (SPIRou-Pyrénées). In this document, we present the main arguments for having as powerful and ambitious an instrument as SPIP at TBL, discuss the new science avenues that could be explored with SPIP (in the framework of an international science consortium), outline the instrument concept behind SPIP (closely mirroring that of SPIRou) and conclude by giving a preliminary budget & schedule.
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1. Need for a new instrument at TBL

1.1. The past & future of NARVAL

In 2016/2017, NARVAL (installed at TBL in 2006 and offered to the community in 2007) will complete one full decade of operation. As evidenced by the number and quality of original results (in the field of stellar magnetism in particular) that have been produced already, it is fairly obvious that NARVAL is a great success. It is nonetheless legitimate to wonder, after this first highly successful decade of operation and in the framework of the ongoing astronomical long-term perspective, what is **the future of TBL for the coming decade**. More precisely, should operation at TBL be continued as is (or with only moderate & relatively inexpensive updates on the instrument, telescope & dome), eg on the argument that the future science demand on NARVAL is predicted to be high enough (oversubscription factor >2) to justify 100% of TBL time over another complete decade? Or do we already foresee that NARVAL, although still competitive on a number of specific science topics, will not be attractive enough on a worldwide context to require full dedication of a 2m telescope for 10 more years? This is precisely the question that the TBL Science Committee is asking through their recent Call for Ideas concerning next-generation instruments for TBL, and to which we try to provide an answer with the present document.

Before NARVAL starts operating in 2006, TBL was still a multi-instrument telescope, offering in particular the MuSiCoS spectrograph and companion polarimeter (used for all preNARVAL studies on stellar magnetism), the IR camera MOICAM (eg used for EROS campaigns), the STERRENN photopolarimeter and a few other visitor instruments – MuSiCoS being the instrument requested and used most, mainly by the stellar community working on stellar magnetism. The performances of MuSiCoS were however so low, regarding overall throughput (~0.5%, telescope & detector included), spectral resolution (~30K) and single-exposure wavelength domain (450-660 nm) in particular, that it rapidly became obsolete. When NARVAL started operating, it offered **drastic performance improvements over MuSiCoS**, ie a factor of ~30 in throughput (reaching up to ~15%), a factor of ~2 in spectral resolution (~65 K) and a factor of >2 in single-exposure spectral coverage (390-1000 nm); this corresponds altogether to a **gain of ~5 mag in sensitivity over MuSiCoS**, giving the opportunity to users to start **new & original science programs** focussed on new classes of stars that were totally out of reach of MuSiCoS, eg T Tauri stars, solar twins or red giants (being either intrinsically faint or featuring very small Zeeman signatures). Expectedly, this **NARVAL revolution** – in addition to yielding a wealth of forefront science studies, world premieres and press releases – drained a numerous & international community of users and resulted in a large increase in demand, justifying a posteriori the **full dedication of TBL to NARVAL.**

The question that naturally rises is whether this NARVAL revolution will still be ongoing for long enough, justifying in particular another 10 yr of full TBL dedication, or whether we are already foreseeing a decline phase (eg in the demand from the community) in which case a new revolution, of the same breadth of that brought by NARVAL, needs to be undertaken in the near future to justify the continuation of TBL operation on one more decade. As mentioned already, most of the science behind NARVAL deals with stellar magnetism, and more specifically the study of large-scale magnetic topologies of stars and of their impact on the formation and evolution of stars. It is obviously still a young & vivid research field that promises more forefront discoveries in the future, especially if new stellar classes (eg embedded protostars, brown dwarfs) become accessible; if however the accessible stellar sample remains essentially the same, eg by lack of an efficiency boost similar in magnitude to that brought by NARVAL in 2006, the interest of the community will inevitably decline, slowly but steadily, and full dedication of a 2m telescope may become questionable. In particular, we suspect that, with the commissioning of the nIR spectropolarimeter SPIRou @ CFHT, the cool-star community will mostly shift to this new instrument, with which Zeeman signatures are expected to be larger by typically x2.5 (equivalent to a sensitivity increase of 2 mag) and very-low-mass dwarfs comparatively brighter (by as much as x30 and x1000, ie 3.5 & 7 mag, for M6 and M8 stars respectively) than in the visible. We therefore think that **a new NARVAL-like revolution is needed to justify the full use of TBL for one more decade.**

Being already as efficient as the best existing high-resolution echelle spectrographs (eg UVES) and offering the widest possible optical domain accessible to a CCD detector, there is very little latitude to improve the overall sensitivity of NARVAL by more than 0.1-0.2 mag. The main improvement that we foresee concerns operation itself, with TBL opening as much as ~360 nights per year (rather than only ~240 nights per year as of today) and potentially yielding an equivalent sensitivity increase of ~0.4 mag. All these improvements would certainly be interesting and worthwhile and would offer in particular the opportunity to carry out the current science programs significantly faster than they presently are. However, all these individual contributions will add up into a **performance improvement no larger than ~0.5 mag,** much smaller in particular than that brought by NARVAL in 2006; in other words, trying to achieve a new instrumental revolution by only improving NARVAL and/or the operation at TBL will fall short of the mark by a long distance. Our conclusion is thus that nothing else than a **new & ambitious instrument is capable of accomplishing a NARVAL-like revolution** that will justify the full dedication of TBL over the next decade.

1.2. The SPIP revolution

Our proposition is **to build for TBL a SPIRou twin, called SPIP** (for SPIRou-Pyrénées). Being simultaneously a high-resolution spectropolarimeter (as ESPaDOnS & NARVAL) and a high-prevision velocimeter (as HARPS & SOPHIE), SPIP will gather a large community of users from various research fields and especially all those working on exoplanets and stellar magnetic fields (see Sec 2 where these programs are described in more details), not only within France (mostly within the PNP & PNPS communities) but also within Europe (eg OPTICON) and further (eg Canada, Brazil, China, Australia, Argentina, USA). More specifically, SPIP, as a spectropolarimeter, will be more sensitive than NARVAL by at least 5 mag for very-low-mass dwarfs (spectral types M3 and later); as a velocimeter, SPIP will be as precise as HARPS, reaching radial velocity (RV) precisions of <1 m/s. As a result, the SPIP community should be significantly more numerous than the one presently using NARVAL, **ensuring that demand on TBL will remain high for at least one decade.**

SPIP can be used in particular **to efficiently contribute to the observing effort of the international SPIRou/SPIP science consortium**, and in particular by systematically participating to the various Large Programs (LPs) that will be carried out with SPIRou @ CFHT (both in velocimetry & spectropolarimetry, see Sec 2). In this respect, SPIP @ TBL and SPIRou @ CFHT could cooperate in a similar way to NARVAL & ESPaDOnS - which proved extremely fruitful in terms of observing optimization (eg with TBL concentrating on the brightest targets while CFHT focusses on the faintest ones, or with both sites observing together when temporal coverage is crucial) and efficiency maximization (eg with TBL partly compensating for poor weather episodes at CFHT) - or to SOPHIE & HARPS, which showed beneficial synergy in exoplanet searches and studies (eg in the follow-up of CoRoT targets, with SOPHIE & HARPS sharing observations depending on the required data quality, temporal sampling and RV precision). Making this cooperation automatic (eg by allocating up to 50% of TBL time to LPs selected by CFHT) and explicit from the beginning (eg with references to TBL for all publications resulting from data collected with such LPs) would obviously boost the science return of SPIP as well as the impact factor and visibility of TBL.

As detailed in the science case of SPIRou (see RD1), up to ~250 CFHT nights per year over 5 years are needed to complete the LPs addressing the 2 main science goals, equivalent to ~4 yr of full-time operation (if we take into account the ~85% chance of good weather). Since CFHT invests a maximum of ~40% of its time on LPs, it will thus take in practice 10-20 yr to achieve the main science goals with SPIRou only (depending on whether SPIRou is granted 100% or 50% of the available CFHT LP time). **Having SPIP available will thus this be extremly useful to significantly shorten the time needed to complete the LPs.** More specifically, 10 yr of dedicated SPIP operation can contribute up to 1.25 yr of equivalent full-time CFHT observing, given both the reduced photon-collecting power of TBL (~25% that of CFHT) and the ~50% chance of good weather. SPIP@TBL can thus potentially provide in one decade as much as 30% of the LP data, and hence significantly reduce the time needed for SPIRou@CFHT down to 2.75 yr of equivalent full-time CFHT observing, ie to one decade if 25-30% of CFHT time is granted to SPIRou/SPIP LPs. Thanks to SPIP@TBL, the LPs addressing the 2 main science goals can realistically be carried out in one decade.

Depending on when SPIP is selected, the duplication of SPIRou into SPIP can be either almost completely simultaneous (the best option for minimizing costs and delays) or immediately sequential (more realistic regarding money rising). In the first option (requiring that the budget for SPIP is available as early as 2013), SPIRou & SPIP will be commissioned more or less at the same time, ie in 2015, and offered to the community in 2016; in the second case (assuming that the SPIP budget is available by 2015), SPIP will be commissioned ~2yr later than SPIRou, ie in 2017, and offered to the community in 2018, just as NARVAL was offered to the community ~2 yr later than ESPaDOnS. Considering that the SPIRou/SPIP LPs requires about 1 decade to reach completion (see above), this potential 2 yr delay between the commissioning of SPIRou and SPIP is not necessarily problematic.

We also recall that implementing SPIP will not necessarily imply decommissioning existing spectropolarimeters / velocimeters, ie NARVAL@TBL or SOPHIE@OHP. For TBL in particular, it is rather straightforward (technically speaking at least) to implement ways for easily switching from SPIP to NARVAL in a matter of minutes, hence within a given night; if some science programs requiring NARVAL are judged worthwhile by the TAC, ie of at least the same science value than those proposed for SPIP, then there is no reason for not carrying them out as they will obviously be worth the investment just as well. For instance, one can imagine using NARVAL to complement observing programs with SPIP for which additional (though not simultaneous) optical spectra will be necessary. For the reasons mentioned above however, we think that such NARVAL programs are unlikely to represent more than 10-20% of the full observing capacity of TBL. Similarly, we can imagine SOPHIE@OHP contributing to SPIP observations by providing additional (and potentially simultaneous) high-precision RV data at visible wavelengths. Both NARVAL & SOPHIE may be helpful to complement SPIP/SPIRou data and further help disentangling the respective effects of exoplanets & magnetic activity on RV curves - a major challenge for future searches of habitable Earth-like planets.

1.3. Is SPIP worth the cost?

But how much ambitious an instrument can TBL really afford in practice? The NARVAL revolution was brought at a relatively moderate cost (~1 M€ in 2005, co-funded by Région Midi-Pyrénées, MEN & FEDER, with an additional contribution from Département des Hautes Pyrénées). Clearly, accomplishing another revolution of the same breadth for a similar amount (ie ~2 M€ in today's €) will be quite tricky in practice; at the same time, it may not be reasonable to invest too much on a telescope whose collecting power is limited. The best way to quantitatively estimate whether an instrument is worth the expense is to see **how its cost compares to the operation of the telescope** on the minimal observing time needed to complete the main science goals driving the construction.

As detailed above, the time needed to complete the LPs addressing the main science goals of SPIRou amounts to ~4 yr of equivalent full-time CFHT operation (spread over typically one decade). Since the consolidated cost of 1 full year of CFHT operation is worth ~6 M€ (in today's €), we conclude that 4 yr of operation over the coming decade are worth ~36 M€ (again in today's €, and accounting for inflation by multiplying the cumulated amount of 4x6=24 M€ by a factor of 1.5). Given the consolidated cost of SPIRou (~10 M€, including salaries), we conclude (i) that the **full cost of carrying out the SPIRou LPs is ~46 M€** and (ii) that **the operational cost of CFHT is by far the largest part** (~80%) of this budget. Obviously, SPIRou is worth the expense since it was selected by CFHT (and hence by all the research agencies funding CFHT) as the only next generation instrument to be constructed.

Since SPIP@TBL is expected to participate to the SPIP/SPIRou LPs at a relative level of ~1.25 yr (out of a total of ~4 yr) of equivalent full-time CFHT operation (again spread over one decade), we conclude that the science contribution of SPIP is worth the corresponding fraction of the full cost (of ~46 M€), ie ~15 M€ – to be compared to the actual cost of constructing SPIP and operating SPIP@TBL over one decade. To estimate this cost, we proceed as before; we start by cumulating the operational cost of TBL on one decade at the current nominal (ie OPTICON quoted) rate of 0.9 M€ per year, yielding a total of 13 M€ (taking into account inflation as previously); to this number, we add the construction cost of SPIP, which reduces in practice to the duplication cost of SPIRou, ie 4 M€ (see Sec 4). The **total construction & operation cost of SPIP@TBL over one decade is thus ~17 M€**, most of which (~75%) again corresponding to the operation cost of the telescope. We note in particular that this cost is similar to that of the expected science contribution; the relatively small overrun (of ~2 M€, ie ~12% of the full constrution & operation cost) corresponds to the cost of the added convenience of having 2 instruments instead of just one and allows in particular to carry out the whole LPs in a smaller overall time span (as discussed in Sec 1.2).

We thus conclude that SPIP is worth the cost; in particular, we emphasize that **the construction cost of SPIP is only a small fraction of the expected operational cost of TBL over one full decade.**

2. SPIP Science goals & consortium

We recall here the main science programs of SPIP, derived & adapted from those of SPIRou. Like SPIRou, SPIP will mostly concentrate on two main scientific goals. The first one is the **search & characterization of habitable exo-Earths orbiting low-mass & very-low mass stars** (LMSs & vLMSs) using high-precision radial velocity (RV) spectroscopic measurements. This search will expand the initial, exploratory studies carried out with visible instruments (HARPS & SOPHIE) and will survey in particular a much larger sample of stars.

The second main goal is to **explore the impact of magnetic fields on Sun-like star & planet formation**, by detecting magnetic fields of various types of low-mass protostars (eg classical T Tauri stars / cTTSs, weak-line T Tauri stars / wTTSs) and by characterizing their large-scale topologies. SPIP will also be capable of investigating the potential presence of close-in giant planets (hot Jupiters / hJs) around protostars, with the aim of clarifying how they form & migrate.

Being both a velocimeter and a spectropolarimeter, SPIP should in particular offer the opportunity of implementing novel techniques to correct RV curves from the activity jitter - a major challenge for future searches of habitable Earth-like planets around Sun-like stars.

SPIP will also be able to tackle a wide variety of other science issues in stellar & planetary physics, as well as in galactic astronomy.

The observing time needed to complete the 2 main science goals only (described in Secs 2.1 & 2.2) **is estimated to ~190 nights per year for one decade** (see below), comparable to the maximum observing load that TBL can take (given the ~50% clear weather statistics). Counting in all additional programs on SPIP and NARVAL as well, it guarantees in particular that demand for observing time on TBL remains quite high for at least one decade.

We detail below these various science objectives, starting with the 2 mains science goals, emphasizing in particular those on which SPIP will contribute and mentioning the potential observing synergies between SPIP and SPIRou in the context of the global international SPIP/SPIRou science consortium mentioned above.

2.1. Exoplanets around low-mass stars through velocimetry

2.1.1. Searching & characterizing planets around low-mass stars

More than 700 extrasolar planets have been detected since the discovery of the hJ orbiting the solar-type star 51 Peg by Mayor & Queloz (1995). This vast body of results makes exoplanetology one of the most dynamical and exciting areas in today's theoretical and observational research in astrophysics, with deep philosophical implications and great interest for the public. It reveals a large diversity of planetary systems and provides constraints to better understand the physical processes driving their formation and evolution.

As observational technics improve, exoplanets with increasingly smaller sizes and longer orbital periods are detected. Presently, a huge effort is invested into detecting and studying Earth-mass rocky planets, and in particular those located in the habitable zone (HZ) of their host stars, ie at a distance where liquid water can be stable at the planetary surfaces. Discovering planets at the surfaces of which, as on the Earth, life has appeared and evolved is now a reachable goal from the scientific point of view. Such discoveries would obviously have numerous and far-reaching impacts.

Most of the exoplanets were discovered with the radial velocity (RV) method, through measurements of the periodic Doppler shifts of their host stars using extremely stable high-resolution spectrographs (called velocimeters). It is now possible to detect terrestrial planets orbiting distant stars, with the most stable of such velocimeters reaching RV precisions of order ~1 m/s (HARPS@ESO, HIRES@Keck, SOPHIE@OHP). But this is still at least one order of magnitude away from the precision needed to detect an Earth analog orbiting a Sun-like star at ~1 AU (generating RV variations with a semi amplitude of ~0.1 m/s and a 1-year period). Whereas ultra-high-precision photometric surveys from space (as those carried out with CoRoT or Kepler) may detect Earth-like planet candidates transiting in front of Sun-like stars, RV follow-ups are nonetheless mandatory in most cases to firmly establish the planetary nature of such candidates and to measure their masses. However, most of these candidates are found around faint stars (especially with Kepler), making it difficult or even impossible to carry-out follow-up studies with existing velocimeters and hence to confirm the planetary nature of most candidates.

As a first (easier) step in the search for habitable terrestrial planets, one option consists in **studying the cooler, less massive and much more numerous red & brown dwarfs** whose RV curves are comparatively more sensitive to the presence of habitable exoplanets of a given mass. These low-mass stars present several key advantages, and in particular:

- thanks to their lower masses, their reflex motions are larger (for of planet of given mass and given orbital inclination);
- thanks to their lower surface temperatures, their HZs are tighter (see Table 1), implying that planets located in the HZs (having both shorter orbital periods and larger orbital velocities) are easier to detect (eg RV semi amplitude of ~0.8 m/s for a 1-$M_\oplus$ planet orbiting a M6 star in ~20 d, see Table 1, ie 8-10 times larger than for the Earth around the Sun) and have higher probabilities of being transiting;
- thanks to their lower radii, transiting planets generate deeper transits (eg 0.4% for a 1-$R_\oplus$ planet transiting in front of a M6 star, compared to 0.008% for the Sun, see Table 1);
- they vastly dominate the stellar population in the solar neighborhood and thus likely host most of the planets in our Galaxy.
- they allow to extend the current knowledge of planet formation around Sun-like stars to low-mass and very-low-mass stars, hard to reach with existing velocimeters.

Table 1: Typical properties of M dwarfs, including the location & extent of their habitable zones, the corresponding range of orbital periods, the RV semi-amplitude $K_\oplus$ & $K_{10\oplus}$ expected from habitable planets of masses 1 & 10 $M_\oplus$ (the full RV variation being 2x larger) and the maximum depth of photometric transits from Earth-size planets. Green figures outline cases where 25 RV measurements @ 1 m/s precision can yield significant detections.

ST	M ($M_\odot$)	R ($R_\odot$)	T (K)	HZ (AU)	Porb (d)	$K_\oplus$ (m/s)	$K_{10\oplus}$ (m/s)	ΔM (mmag)
Sun	1	1	5780	0.8-2.0	260-1000	0.1	**1**	0.08
M4	0.30	0.30	3400	0.10-0.28	24-100	0.4	**4**	0.9
M6	0.13	0.15	3000	0.04-0.12	9-40	**0.8**	**8**	3.6

This led to several dedicated exoplanets surveys targeting low-mass dwarfs, including MEarth (photometry) or HARPS (RV); MEarth allowed the detection of the close-in super-Earth GJ1214b (Charbonneau et al 2009) whereas HARPS detected the first super-Earths in the HZ of their host stars GJ581d (Mayor et al 2009) and Gl667Cc (Delfosse et al 2012). The HARPS sample also revealed that at least 1/3 of M dwarfs harbor habitable planets (Bonfils et al 2011).

Red & brown dwarfs are however fainter than Sun-like stars in the optical range but their low temperatures make them much more accessible at nIR wavelengths. This led to the development of the SPIRou@CFHT, aiming at carrying out systematic RV surveys of M dwarfs (with precisions of <1 m/s in the nIR) to investigate the statistical properties of their planetary systems. By comparison with HARPS-like surveys, it will allow to probe dwarfs with masses <0.3 $M_\odot$ and to extend existing surveys of early-M stars.

SPIP@TBL aims at complementing these studies, in a join effort with SPIRou, and will be particularly well suited to study the intrinsically brighter early-M dwarfs (while SPIRou will concentrate mostly on the late M ones).

2.1.2. Habitable exo-Earths around mid-M dwarfs

Our first goal is to look for planets with rocky/icy cores and masses of 1-10 $M_\oplus$ that are located in the HZ of their host stars. Given the RV semi-amplitudes they can typically generate (see Table 1), one can detect (with 25 RV measurements at <1-m/s precision) habitable planets of >3 $M_\oplus$ around 0.3 $M_\odot$ (ie M4) dwarfs and of >1 $M_\oplus$ around 0.13 $M_\odot$ (ie M6) dwarfs. A stability of 1 m/s is thus mandatory for detecting habitable Earth-like planets around M dwarfs.

Due to their low temperatures, low-mass stars are faint in the optical but brighter in the nIR bands. They feature thousands of atomic and molecular lines in Y, J, H & K bands (ranging from 1 to 2.4 µm), allowing accurate RV measurements. Studies for SPIRou showed in particular that RV precisions of ~1 m/s are attainable for mid to late M dwarfs provided a peak S/N of ~160 (per 2 km/s pixel), with spectral lines from the K band being a key contributor to the RV precision (see RD1).

SPIRou aims at performing a ~1 m/s-precision RV survey of ~300 mid- to late-M dwarfs within 15 pc, with ~25 visits per star and peak S/Ns of ~160 (per 2 km/s pixel). As SPIP@TBL will be ~1.5 mag less sensitive than SPIRou (as a result of the x4 lower photon collecting power of TBL), it can efficiently concentrate on the brighter (ie most

Table 2: JK magnitudes @ 15 pc, corresponding exposure times needed to reach S/N=160 (per 2 km/s pixel), number of dwarfs available (within 15 pc) and total exposure time for mid-M dwarfs of types M4-M6. Observing (once) an unbiased sample of ~250 stars requires ~230 hr while the full monitoring (25 visits per dwarf) requires ~70 nights/yr for ~10 years.

ST	J	K	t_{exp} for S/N=160 (min)	$n_\star$	t_{tot} (hr)
M4	8.7	7.9	32	165	88
M5	9.6	8.7	64	128	136
M6	10.4	9.5	136	34	76
				total=327 ★	total=300 hr

massive) targets of this sample. **On a timescale of 10 yr with ~70 nights per year, a survey of ~250 M4-M6 dwarfs within 15 pc can be completed by SPIP**, allowing 25 measurements to be secured by target at a 1-m/s precision level. According to the first results of the M-dwarf survey performed with HARPS, up to 30% of low-mass stars harbor super-earths. This suggests that **several tens of habitable extrasolar planets can be detected with SPIP** by the end of the survey. This is a thrilling perspective for a new instrument at TBL.

Note that the proposed SPIP survey, although including as much as 5/6 of the full SPIP/SPIRou sample for this specific science goal (ie ~250 out of ~300 stars), only requires an overall equivalent observing time of ~35 CFHT nights per year over 5 yr, ie 1/3 of that needed to cover the whole sample (see RD1); this demonstrates that **SPIP can survey a significant fraction of the sample** (ie all the mid-M dwarfs) thereby helping SPIRou to focus on the faintest targets (ie all the late M dwarfs) that cannot be observed with a 2m telescope. Thanks to the complementary longitudes of Pic du Midi & Hawaii, coordinated SPIP & SPIRou observations will also be extremely useful for estimating orbital periods of detected exoplanets, and in particular for attenuating the period aliasing that single-site data are always subject to.

2.1.3. Detailed statistics of planets around early-M dwarfs

Besides detecting new, interesting exoplanets, SPIP can also provide key statistical information on planetary systems around low-mass dwarfs. Trends are beginning to emerge between the properties of exoplanets and those of their host stars, eg gaseous giants being more frequent around stars with larger masses or higher metallicities; these trends provide observational constraints for theoretical models describing the formation and evolution of planetary systems. In particular, such trends can potentially discriminate whether planets form through the so-called «core-accretion model» (where planets build up by accreting dust and gas around solid cores) or through gravitational instabilities in the accretion disc (the cooling disc fragmenting into small clumps evolving into protoplanets).

More specifically, SPIP can survey an extensive sample of early-M dwarfs, much larger in particular than the existing HARPS one (essentially limited to the 100 brightest southern early-M dwarfs within 10 pc). There is ~8,300 early-M dwarfs for which a precision of 1 m/s can be reached with SPIP in a <1hr exposure (ie with J<9.5, see RD1). Within 15 pc, there is ~500 of them (with J in the range 6.5-9.5) whose **full survey with SPIP only** (including 25 visits per star) **requires ~50 nights per year over 10 yr.** SPIP will thus carry out most of the survey on this specific science goal.

2.1.4. The interesting case of transiting planets

Out of the 700+ exoplanets known to date, more than a quarter of them transit their parent stars as seen from the Earth. Transiting planets are particularly interesting as they allow to simultaneously estimate planet radii and masses, and thus to constrain their average densities; they also allow studying planetary atmospheres, either through their absorbing properties (during transits) and through their emission features (during occultations); finally, they also provide additional orbital information, eg through possible transit timing variations (TTVs) or obliquity measurements thanks to the Rossiter-McLaughlin effect. The power of these analyses triggered numerous surveys specifically concentrating on transiting planets; most of them were discovered in the last five years, and the detection rate is still growing.

However, only a handful of bright transiting systems have been discovered up to now, most of them being giant gaseous planets. Small-size planets discovered by Kepler are generally too faint to allow follow-up RV studies. Detecting candidate Earth-like planets transiting in front of their host stars through photometric monitoring is much easier for low-mass dwarfs, transits being comparatively deeper as a result of the smaller radii of the host dwarfs (see Table 1). Only a few are known today, including GJ436b and GJ1214b.

The photometric quest for transiting super-Earths around dwarfs has already started from the ground (eg MEarth project focussing on M dwarfs) and from space (with CoRoT and Kepler) and should progressively become more sensitive and ambitious in the future, aiming at planet masses and radii closer to those of the Earth. To detect habitable Earth-size planets through photometry, M dwarfs are particularly well suited, the relatively small size of their HZs strongly increasing the probability of transits. While results from CoRoT and Kepler on this topic will remain limited, ground-based and space-based photometers (eg TESS) should detect a significant number of Earth-size planets.

Ground-based nIR spectroscopy is essential to this quest; spectroscopy is indeed mandatory to establish the planetary nature of most transiting objects detected around low-mass dwarfs through photometric monitoring, by discarding false detections, eg caused by background eclipsing binaries. RV measurements allow masses of confirmed planets to be measured, and are thus a necessary complement to photometric surveys. A high-precision velocimeter working in the nIR will thus be essential to monitor all candidates detected with ground & space photometers around M dwarfs, and in particular around the late-M ones that are too faint to be accessible to velocimeters working in visible light like HARPS, HIRES or SOPHIE.

A nIR spectrograph can also usefully contribute to the quest for close-in transiting exo-Earths around M dwarfs through a systematic survey prior to any photometric observations; at orbital periods <2 d, planets have a 1 in 10 chance to be transiting and could thus be easily monitored photometrically (at predicted phases of potential transits specifically) once discovered through their periodic RV variations.

Finally, obliquities of exoplanetary systems can be estimated through the observation of spectroscopic transits. While spin-orbit alignments are often observed for Jupiter-mass planets, significant misalignments were recently reported, including even some retrograde orbits; this suggests that the standard model explaining hJs through standard migration in circumstellar disks (favouring spin-orbit alignments) is at best incomplete and needs to be revised (eg to incorporate as well gravitational interactions between various planets in a given system). Expanding such measurements to low-mass planets around M dwarfs will provide **additional constraints on models of planetary formation and evolution**. A super-Earth transiting an M-dwarf typically provides a 10 m/s Rossiter-McLaughlin anomaly (provided a rotational broadening of a few km/s), which is detectable with SPIP.

2.1.5. Filtering the activity jitter

Dark spots, bright plages or magnetic regions at the surfaces of low-mass active stars are known to generate significant distortions in spectral line profiles and therefore to produce apparent RV shifts in the collected spectra. At best, these surface features are short-lived and amount to no more than additional noise (called activity jitter) that can be averaged out on long time-scales at the cost of more measurements. However, surface features can be long-lived (especially in M dwarfs) and persist for several years, producing in some cases RV fluctuations closely resembling those caused by orbiting planets and difficult to average out over time.

At near-IR wavelengths, the spot/photosphere brightness contrast is lower than at visible wavelengths by factors of 5-10, decreasing accordingly the amplitude of spectral line distortions caused by surface inhomogeneities and thus the associated impact on RVs. It suggests that the corresponding activity jitters in the nIR are lower than 1 m/s and potentially even below 0.5 m/s.

While the majority of early-M dwarfs are no more than weakly active and slowly rotating (with a rotation period longer than 30 d typically), late M dwarfs behave differently with a dominant fraction exhibiting intense activity and rapid rotation – the transition between both regimes occurring at spectral type ~M4. Activity jitters are thus expected to be potentially problematic (even in the nIR) for searches of exo-Earths orbiting low-mass dwarfs. Filtering out such jitters (at least partially) is however possible (Queloz et al 2009, Boisse et al 2011). A promising filtering option consists of simultaneously monitoring the large-scale magnetic field of the host star by means of spectropolarimetry and using empirical relationships between field topologies and activity jitters derived from standard M stars of various activity levels. Being both a velocimeter and a spectropolarimetry, **SPIP, like SPIRou, will allow to filter activity jitters in RV curves in a more efficient way**; SPIP will also be very useful to further ascertain the detection of planets through visible RV surveys and to disentangle activity induced RV fluctuations from the true planetary signal.

This filtering scheme, making a simultaneous use of both velocimetric & spectropolarimetric capabilities of SPIP, is expected to be helpful for many different science programs (eg Sec 2.2.4).

2.1.6. Impact of telluric lines

Numerous telluric lines from the Earth's atmosphere show up at nIR wavelengths, especially in the J, H & K bands (see Fig 3.5). Since they blend with stellar spectral lines and move with respect to the barycentric rest frame, they can significantly affect RV estimates if not properly taken into account. Although known for very good sky transparencies (allowing in particular chromospheric & coronal monitoring of the Sun since Lyot invented coronography), Pic du Midi is nevertheless expected to be worse than Hawaii regarding telluric lines; more quantitatively, we assume that Pic du Midi features H_2O column densities of ~5 mm, comparable to those of Kitt Peak and ~3x larger than those of Hawaii (see RD1).

As with SPIRou, we will subtract telluric lines as part of data reduction; the subtraction technique is expected to be efficient for H_2O no deeper than 20% and for all other telluric lines (from CO_2, O_2 & CH_4, known to be much more stable and better mixed within the atmosphere that those from H_2O) no deeper than 50% – thus leaving in spectra telluric residuals with depths <5%.

The ~3x deeper telluric spectrum at Pic du Midi is naturally expected to slightly narrow the accessible spectral domain (see RD1); our simulated spectra demonstrate that this added pollution amounts to an average sensitivity drop of ~10% (0.1 mag) for all RV programs.

2.2. Magnetic fields & star/planet formation through spectropolarimetry

2.2.1. Magnetic fields & star/planet formation

Magnetic fields are known to play a significant role throughout the life of low-mass stars, from the cradle to the grave; for instance, they are very efficient at spinning down young Sun-like stars by dissipating a large amount of angular momentum through magnetic braking, via mass loss in large-scale field topologies. Yet, **magnetic fields have an even bigger impact during the early phases of stellar evolution**, when stars and their planetary systems form from collapsing parsec-sized molecular clouds, progressively flattening into large-scale magnetized accretion discs and finally settling as protostars surrounded by protoplanetary discs. Throughout this formation process, magnetic fields have a critical role in many different steps, eg by dissipating the excess angular momentum & mass and by drastically scaling up the amount of turbulence (through various instabilities) and inhibiting the fragmentation process within the disc (eg André et al 2009 for a review).

At a typical age of 1-10 Myr, low-mass protostars have emerged from their surrounding dust cocoons and are still in a phase of gravitational contraction towards the main-sequence (MS). They are either **classical T Tauri stars** (cTTSs) when still surrounded by a massive (and presumably planet-forming) accretion disc or **weak-line T Tauri stars** (wTTSs) when their disc has mostly dissipated, members of both classes having similar ages on average and mostly differing by the lifetime of their accretion discs. TTSs have been the subject of intense scrutiny at all wavelengths in the last decades given their obvious interest for benchmarking current scenarios of low-mass star and planet formation (eg Bouvier et al 2007 for a review).

Magnetic fields of TTSs play a key role in the formation process. In particular, large-scale fields of cTTSs are strong enough to evacuate the central regions of the accretion disc, to funnel the disc material from the inner disc rim onto the stellar surface, and even to enforce corotation between the protostar and the Keplerian flow just outside of the magnetosphere, forcing cTTSs to rotate much slower than expected from the cloud contraction. Magnetic fields of TTSs are also crucial to generate a hot corona and thus to boost the leakage of angular momentum (through magnetized winds and coronal mass ejections) that will eventually slow down the star within the first few 100 Myrs of its MS life. Last but not least, magnetospheric gaps and winds of cTTSs may also be vital for the survival of hJs, stopping their inward migration within the accretion disc at distances of ~0.05 AU (typical to hJs and compatible with observed magnetospheric gaps of cTTSs) avoiding their falling into their host star (eg Lin et al 1996).

Beyond this rough sketch, there are obviously **many crucial questions that remain unanswered.** For instance, understanding how in practice the central protostar magnetically interacts with, accretes from, is slowed down by & eventually disperses its disc is critical if we want to obtain a more quantitative description of how Sun-like stars & their planets form. Estimating magnetic field strengths & topologies in young protostars & their accretion discs is the most direct way to address these issues and to assess quantitatively **the origin & impact of magnetic fields on low-mass star & planet formation**. This is the second main science goal of the SPIRou/SPIP science program.

2.2.2. Detecting magnetic fields @ nIR wavelengths

Direct magnetic measurements constraining models of star formation are difficult & rare (eg Donati & Landstreet 2009 for a review). For the moment, they mostly concern three stages of the formation process: the dense cores of molecular clouds, the protostellar accretion discs and the cTTSs. Given their very low temperature, dense cores can only be observed at radio wavelengths (eg Crutcher 1999); in particular, ALMA is expected to provide soon a wealth of information concerning their magnetic fields and thus to characterize how the field impacts the earliest formation stages. ALMA should also be able to detect magnetic fields & sites of planet formation in the outermost regions of close-by or very extended accretion discs.

However, magnetic fields in the inner disc regions or at protostar surfaces will be totally out of reach of ALMA and can only be investigated through nIR spectroscopy (eg Johns-Krull 2007) or optical spectropolarimetry (eg Donati et al 2010). In both cases, magnetic fields are detected through the Zeeman effect on spectral lines, and more specifically through the additional broadening (with respect to non magnetically-sensitive lines of otherwise similar properties) and the polarization structures they generate within line profiles. A first exploratory survey (limited to ~15 well known protostars) has been carried out using ESPaDOnS@CFHT & NARVAL@TBL.

Our knowledge of magnetic fields of protostars and their accretion discs is however still rudimentary. Up to now, only one accretion disc (FU Ori) has been reported as magnetic (Donati et al 2005, Nature 438, 466); moreover, field estimates are mostly available for relatively evolved objects (ie cTTSs & wTTSs, also called Class-II & Class-III protostars), whereas only one (very recent) measurement exists for younger, more embedded objects (called Class-I protostars) that are supposedly still in the process of accreting much of their mass at high rates (Johns-Krull et al 2009). A much more systematic & sensitive survey of magnetic fields of all classes of protostars, including very-low-mass protostars (down to ~0.1 $M_\odot$), is thus needed.

Working at nIR wavelengths can strongly boost the sensitivity of such a survey. Class I protostars are indeed far less obscured @ nIR wavelengths than in the optical domain (see RD1); moreover, **the Zeeman sensitivity is also much stronger than in the visible**, the Zeeman splitting growing linearly with wavelength with respect to the Doppler line width. Thousands of atomic spectral lines featuring a large range of magnetic sensitivities are present in the nIR, demonstrating that multiline techniques like those used in optical spectropolarimetry should be straightforwardly adaptable to the nIR and very efficient at detecting & characterizing Zeeman signatures from magnetic protostars. Although less well known than their atomic counterparts, molecular lines are both numerous in the nIR & magnetically sensitive and can also bring additional information. The improved sensitivity brought by nIR observations also allows to recover not only large-scale surface magnetic fluxes (derived from phase-resolved sets of polarized line profiles) but also the relative amount of unresolved small-scale magnetic fields packed in close bipolar groups (eg by comparing field fluxes obtained from polarized Zeeman signatures with field strengths derived from Zeeman broadening of unpolarized spectral lines). With nIR spectropolarimetry, one can thus potentially undertake a **very extensive & ambitious survey of magnetic fields in protostars & inner accretion discs** to investigate directly how these fields influence in practice the formation of Sun-like stars and their planetary systems.

This SPIP/SPIRou survey can ideally complement **magnetic studies of prestellar cores & outer accretion discs with ALMA** (that will trace fields at still earlier formation stages) and thus provide a broader context for exploring how magnetic fields impact stellar formation. While SPIRou@CFHT will explore embedded class-I protostars and the faintest cTTSs/wTTSs (with H>10 typically) to take best advantage of the higher collecting power of CFHT, SPIP@TBL will efficiently contribute to this survey by covering the bright fraction of the selected cTTS/wTTS sample.

2.2.3. Large-scale topologies of cTTSs/wTTSs

By detecting Zeeman signatures in photospheric absorption & accretion-powered emission lines of young protostars and by monitoring them as the stars rotate, we can model their large-scale magnetic topologies (eg Donati et al 2010). From this, we can quantitatively address, for a statistically significant sample of protostars, several key issues regarding the **impact of magnetic fields on the fate of newly born Sun-like stars and their planetary systems**, and in particular points such as:

- how do magnetic topologies of cTTSs/wTTSs depend on fundamental parameters such as mass, age, rotation rate & accretion rate and how are they likely to evolve throughout the lifetime of the accretion disc?
- how do these dynamo-induced large-scale fields vary with time over one decade and do they undergo cycles during which the dipole/octupole component of the large-scale field switch sign?
- do cTTSs/wTTSs host hJs as often as MS low-mass stars and are the temporal variations of the large-scale field (and of the associated magnetospheric cavity) likely to affect the survival of hJs?

In practice, we plan to **survey ~200 Class II & III protostars, sampling a selected range of masses, ages, rotation rates and accretion rates**, with which we will address the above-mentioned issues; all stars will typically be observed on 20 different epochs to properly cover the rotational modulation of their Zeeman signatures.

Existing results (obtained with ESPaDOnS@CFHT & NARVAL@TBL on ~15 cTTS) suggest that the properties of these large-scale fields are closely linked to the internal structure; stars with relative convective depths larger than about 50% (in radius) are apparently capable of triggering strong, mainly poloidal and axisymmetric magnetic fields, whereas stars with shallower convective zones exhibit more complex fields (with a significant toroidal component and a moderate, mostly non-axisymmetric poloidal component). More specifically, fully convective cTTSs are observed to host mainly-aligned time-variable dipolar fields while dominantly (but non-fully) convective ones all harbor mainly-aligned (and also time-variable) octupolar fields. These results demonstrate in particular that magnetic fields of PMS stars are produced by non-stationary dynamo processes (similar to those of MS stars) rather than being fossil remnants of the interstellar field; they also provide a direct method for observing astrophysical dynamos in a much more general context than that of the Sun or the Earth, and should ultimately guide us towards modern dynamo theories applicable to a wide range of astrophysical objects.

The extended cTTS/wTTS survey we propose to carry out with SPIP/SPIRou should allow to firmly confirm these conclusions and investigate whether large-scale fields of cTTSs and wTTSs are statistically similar; in particular, this survey will allow to **model more accurately the evolving magnetic topologies of young Suns in the age range 1-10 Myrs**. By repeating (eg ~5 times) observations of a subsample (of ~20 cTTSs/wTTSs) over one decade, we will also examine how these large-scale fields and the corresponding magnetospheric cavities change with time.

SPIP will carry out most of this survey, concentrating on the cTTSs/wTTSs with H<10 (ie most of them in Taurus). **The time needed to cover ~200 protostars with 20 visits per star** (~1 hr per visit and per star on average) and 10% of the sample observed at 5 different epochs **amounts to ~70 nights per year over one decade.**

2.2.4. Detecting hot Jupiters around young Suns

Our survey of TTSs can also be used to attempt detecting hJs around very young Suns. Since their initial discovery ~15 yrs ago, hJs are a real challenge to theorists on planet formation and are thus very interesting despite their relative sparseness. Obviously, hJs cannot be formed in situ given the limited & hot disc material at so short distances from the host star (eg Lin et al 1996). The most plausible scenario is thus that they form much further out in the protoplanetary disc and migrate inwards, either under the non-zero gravitational torque from the accretion disc (eg Goldreich & Tremaine 1980) or through planet-planet interaction / scattering (eg Rasio & Ford 1996). While the second scenario may explain the (small) fraction of hJs with highly inclined orbits, **disc migration remains the most likely option for the majority of hJs**; in this case, both the formation & migration processes must occur on a timescale significantly shorter than the lifetime of the disc to allow hJs to end up so close to their host star. Moreover, hJs (at least a fraction of them) survive the migration, stoping at a distance of ~0.05 AU, and avoid falling into their host star; having typical radii of 0.1 AU, **magnetospheric gaps may be the most natural way to achieve this** (Lin et al 1996). If this is confirmed, it would imply that magnetic fields of low-mass protostars are a key parameter of this survival.

Using our TTS survey, we will be able to look for periodic RV changes that may reveal the presence of hJs (producing typical peak-to-peak RV amplitudes of 0.1-1 km/s on periods of a few d). Though non-trivial given the high level of activity of TTSs (generating RV jitters at visible wavelengths comparable to or larger than the RV reflex motion of potential hJs), detecting hJs is nevertheless possible with SPIP. As mentioned above (see Sec 2.1.5), RV jitters are expected to be 5-10 times smaller at nIR wavelengths than in the visible; moreover, by accurately modeling the activity of wTTSs with advanced imaging methods, one can **filter most of the activity-induced RV changes down to the level at which hJs should become detectable.**

The predicted number of hJs orbiting TTSs is unclear as of today. Since slightly less than 1% of mature Solar-like stars host hJs (eg Mayor et al 2012), one can expect that TTSs should also host hJs with at least the same frequency if these hJs are generated through disc migration; alternatively, hJs should be far less numerous if hJs are formed through planet-planet interaction / scattering. If the fraction of hJs around TTSs is ~1%, we expect to find ~2 of them in our sample of ~200. Our observations should then **bring the first observational confirmation of whether disc migration is indeed the main mechanism for generating hJs, or whether planet-planet interaction / scattering is to be preferred.** Obviously, this result would represent a major observational achievement and would be a significant step forward in our understanding of hJs. Note that, to achieve this goal, we will make a simultaneous use of both velocimetric & spectropolarimetric capabilities of SPIP.

2.3. Additional science goals

2.3.1. Dynamo processes in red dwarfs

In the last 20 years, red & brown dwarfs have triggered an increasing amount of interest. Due to their intrinsic faintness, little was known about these objects before – and despite considerable progress in recent years, their physics, internal structure & atmospheric properties are still subject to discussions, with a number of key issues remaining largely speculative. Radii of active red dwarfs, observed to be significantly larger (by 10% or more) than those of non-active dwarfs provides a striking example of these critical modeling puzzles for which no definite answer is yet available. One of these problems is to understand dynamo processes in fully-convective bodies to the point of correctly predicting their large-scale magnetic topologies. Whereas new theoretical studies & numerical simulations now agree that red dwarfs can generate large-scale fields, the exact topology of these fields is still unclear.

A first magnetic survey of active M dwarfs unambiguously established that their large-scale fields drastically change when the depth of the convection zone exceeds 50% of the stellar radius, getting stronger, simpler & almost purely poloidal & axisymmetric at spectral types later than M3 (eg Morin et al 2008, 2010), being possibly even subject to bi-stability in very-low mass stars (eg Morin et al 2011). Obviously, **time is ripe for an ambitious & systematic exploration of the magnetic properties of M & early L dwarfs** – both at small & large scales – to guide modern models & numerical simulations of fully-convective dynamos towards a more global & successful description of magnetic field generation in low-mass stars, as well as to understand better the impact of dynamo fields on stellar structures & atmospheres.

Conducting this spectropolarimetric survey simultaneously with the velocimetric survey described in Secs 2.1.2 & 2.1.3 will allow to **reach both science goals with a single data set and no addition of telescope time**, given that both studies require similar spectrum qualities (of S/N~160); for this survey SPIP will cover early- and mid-M dwarfs while SPIRou will focus mostly on late-M and early-L dwarfs.

2.3.2. Cool magnetic spots on active stars

Cool stars like the Sun host dark spots & magnetic fields at their surfaces. Their periodic photometric, spectroscopic & spectropolarimetric changes are usually interpreted as the signature of cool spots & magnetic regions coming in and going out of view of an Earth-based observer; in particular, time resolved sets of line profiles collected throughout complete rotation cycles are very sensitive tracers of how spots & fields are distributed over the surfaces of rotating active stars, offering a way of investigating activity cycles in stars other than the Sun through their surface brightness distributions, large scale field topologies and their evolution with time.

However, at visible wavelengths, cool starspots are mainly detected by their lack of continuum photons with respect to nearby regions at photospheric temperature. In Sun-like stars (spectral types GK), the temperature contrast between spots and the surrounding quiet photosphere is ~1,500 K, implying a 10-fold brightness contrast in V; this comparative lack of continuum photons is what generates the line profile bumps & photometric modulation through which starspots reveal their presence. As a consequence, little information about the physical properties of these spots can be derived apart from their temperature & spatial distribution; in particular, measuring field strengths within cool spots is virtually impossible, except for the brightest stars.

At nIR wavelengths, the spot to photosphere brightness contrast is much smaller, by typically a factor of 4 in H, than in the visible; **nIR spectra are thus far more adapted for studying the intrinsic properties of starspots**, and in particular their magnetic field strengths & orientation thanks to the higher Zeeman sensitivity that longer wavelengths bring. For only a few nights per year over one decade, SPIP will be able to reveal fundamental characteristics of starspots and allow fruitful comparisons with their solar analogs, for typically a few tens of active stars of various masses and rotation rates.

2.3.3. Planetary atmospheres

NEED TO EXPAND THIS SECTION TO SOLAR SYSTEM PLANETS

Finally, SPIP could contribute to exoplanet atmosphere studies by doing absorption spectroscopy against the stellar background flux during transits in the the molecular bands available in the nIR domain. SPIP could be limited in that field due to its moderate throughput but some planets with particularly large scale height would be favorable. The high stability of SPIP would be a great advantage here, allowing an accurate comparison between data secured in and out transits, in order to separate and subtract stellar and telluric lines from the planetary signal.

2.3.4. Chemical evolution & kinematics of the Milky Way

Understanding the chemical evolution of galaxies starts with unveiling the physics of the main galactic chemical factories, ie the stars. In this respect, high-resolution nIR spectra offer a unique opportunity to study the chemical properties of stellar populations & stellar evolution. Stars ascending the red giant branch (RGB) & evolving on the asymptotic giant branch (AGB) are the main targets for such studies, as their surface chemical compositions is strongly modified during their evolution. Other metals with specific sensitivity to the star-formation history & the initial mass function, such as iron-peak, odd-Z, & alpha-elements can also be studied in the context of galactic archeology.

Galactic bulges are a key to galaxy formation; their origin & history remain however poorly understood. In the particular case of the Milky Way, detailed chemical composition studies of bulge stars, carrying characteristic signatures of processes enriching the interstellar gas, are essential; in particular, abundance ratios are sensitive to the time-scales of star formation, to the initial mass function, and may disclose relations between different stellar groups. Obscuration towards the bulge is considerably reduced in the nIR, allowing one to access heavily reddened regions; nIR spectra also suffer much less line blending than optical spectra, yielding more reliable abundance estimates.

SPIP can significantly contribute to these studies, thus expanding on the work presently carried out with NARVAL@TBL as science preparation for GAIA.

2.4. The SPIRou/SPIP international science consortium

As already outlined above, we suggest that SPIP@TBL is mostly used to participate & contribute to the observing effort of SPIRou@CFHT, in the framework of the international science consortium that will be set up along with the construction of SPIRou. This science consortium will of course include contributors from many

European (including OPTICON) countries, eg UK, Switzerland, Portugal, Italy, but also key partners from other continents (eg Brazil, Canada, Argentina, USA, Australia, China).

The role of this science consortium will firsly consist in optimizing & finalizing the various science programs to be tackled with SPIRou & SPIP, in preparing the preliminary catalogs & final selections of sources to be observed at CFHT & TBL, and in building up the full-size proposals to be submitted as LPs to CFHT & TBL (and to other telescopes whenever complementary observations are needed). The consortium, including both observers and theoreticians from all research fields of interest for SPIRou/SPIP, will also be in charge of modeling the observed data with adequate tools, of proposing (whenever needed) updated theoretical frameworks in better agreement with observations, and of publishing the full body of results in the refereed literature.

3. Instrument concept

We recall here the instrument concept underlying SPIP and outline the expected performances; we finally compare SPIP with existing / planned similar instruments, demonstrating that SPIP definitely fills a gap in the suite of instruments available to the OPTICON community.

THIS SECTION WILL BE FILLED ON WK 17

3.1. The Cassegrain module & fiber link

3.2. The cryogenic high-resolution spectrograph

3.3. Performances

3.4. Comparison with existing / planned instrument

4. Estimated cost, schedule & team

We finally recapitulate the estimated cost of making a twin copy of SPIRou and suggest a tentative schedule and project team for SPIP.

THIS SECTION IS PRELIMINARY & WILL BE EXPANDED ON WK 17

4.1. Estimated cost & schedule

The plan is to duplicate SPIRou as it is, with no modification whatsoever except for the very minor ones required to adapt the instrument on the TBL. The aim is of course to minimize both time & cost, by only duplicating hardware and saving on studies.

The estimated cost in this context is 4 M€. Funding will be looked for at Région Midi-Pyrénées, MEN, FEDER, CNRS/Equipex, with additional contributions yet to be identified.

Depending on when funding is available, the construction of SPIP by duplication of SPIRou will be carried out either simultaneously or sequentially with that of SPIRou.

In the first case, SPIP will be commissioned as early as 2015; in the second case (more likely), **SPIP will be commissioned in 2017**, construction, assembly and tests taking about 2 yr to complete.

4.2. Science project team

The SPIP project team involved scientists already involved in SPIRou, and gathers in particular experts in the fields of velocimetry and/or spectropolarimetry to ensure that the user community is well represented within the project team.

More specifically, the key science persons involved in the project team of SPIP will be (in arbitrary order):

- PIs: G Hébrard & JF Donati
- PSs: J Morin & TBC
- DRS: X Bonfils, P Figueira, F Bouchy, C Lovis, I Boisse, X Delfosse
- HIA science contact: C Marois

4.3. Technical project team

The technical team still needs to be refined, in discussion with the various partners. It will likely include some of the key technical teams already involved in SPIRou, and in particular:

- TBL / IRAP / OMP: duplication of the Cass module & fiber link, AIT
- OHP / Obs Geneve: duplication calibration module, DRS
- HIA: duplication of the cryogenic spectrograph

5. Conclusions

THIS SECTION WILL BE FILLED ON WK 17