The Detection of Habitable Earth-Mass Planets Using Ground-based Optical Telescopes and Precision Radial Velocities

Astro2010 Whitepaper

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There are many techniques presently in use or being planned for the future for the detection and characterization of exoplanets. This contribution will focus mainly on using large ground-based telescopes and high dispersion spectroscopy of nearby stars to detect and study their planets using high precision radial velocities. The discussion is restricted to the nearest stars (within 100pc) as these are the only ones that we could ever hope to sensibly follow up on in the foreseeable future using other techniques to directly image their planets and obtain spectra of bio-signatures in their atmospheres. Ground-based and space-based techniques involving astrometry, interferometry, transit surveys, gravitational lensing, coronography, and adaptive optics are some of these other techniques that will also produce planet discoveries. Their discussion is left to others. Techniques involving multiple-object spectroscopy will also not be discussed here as they require target field densities on the sky that push the targets out beyond 300-600 parsecs, well outside the regime of "nearby" stars. While such studies could provide useful information on the statistics of exoplanet formation, they are not well-suited to finding the nearest planets. And there is no special magic in multi-object vs. single object spectroscopy. Most any high-resolution spectrometer could, if so desired, be designed to accept a multi-fiber feed, making the usual trade-offs (required of any instrument) between wavelength coverage and number of objects (for a given number of pixels).

Radial velocities from ground-based optical telescopes will continue to figure prominently in exoplanet discovery and characterization for the next two decades

Europe and America have been pouring hundreds of millions of dollars into planning and preparing for space missions that will look for planets. These very expensive missions demonstrate the extraordinary value of this science. Curiously, the radial velocity (RV) method, wherein planets are sensed by detecting the reflex barycentric motion of the parent star, has never been very high on the priority lists of NASA's roadmap to exoplanet discovery. NASA's main thrust and budgetary priority seemed always toward space-based astrometry, coronography, and interferometry. And yet, ironically, it is the RV method that has emerged as the only successful method for routine exoplanet detection, and has provided over 95% of the currently-known exoplanets. RV surveys continue to lead the field in discoveries, accounting for the first exoplanets, the first multiple-planet systems, the first transiting planets, the first sub-Saturn-mass planets, and recently the first super-earth-mass rocky planet. RV surveys will soon also provide the first detections of True Jupiter Analogs, exoplanets in circular 5-10 AU orbits. And RV surveys are even now gearing up to find habitable earth-mass planets. It is also fair to say that all of this RV work has been done so far at truly minimal cost to the taxpayer, orders of magnitude less than the cost of even a single NASA space mission. So the RV technique for exoplanet searching represents tremendous value of scientific return for the taxpayer's dollar and that will continue for the next decade.

However, NASA's science priorities recently shifted dramatically. The Space Interferometry Mission (SIM), Terrestrial Planet Finder (TPF), and Darwin are planned to find planets under 10 Earth-masses; but all three are technically unproven and suffering from severe budget cuts and likely cancellation. At this point in time, it seems a safe bet that none of those three missions will have yielded any planets before 2015, if indeed they launch at all. NASA now seems to be concentrating its resources on the Moon and Mars. In 2015 the only major game in detecting planets orbiting beyond 0.1 AU around nearby stars will likely be Doppler velocities. Not only will it be the only major game, it will likely be the only game, with perhaps a small contribution from a VLT-Interferometer. And even if the Keck Interferometer, VLT-Interferometer, and SIM were to all come on line by 2013, for planets other than Hot Jupiters (which are now being discovered at a rapid pace by transit photometry), Doppler velocities would still continue to dominate well into the 2020s. So if the NSF and NASA are truly interested in furthering this type of research, it is now time to put some serious funding support into ground-based precision RV work for the coming decade.

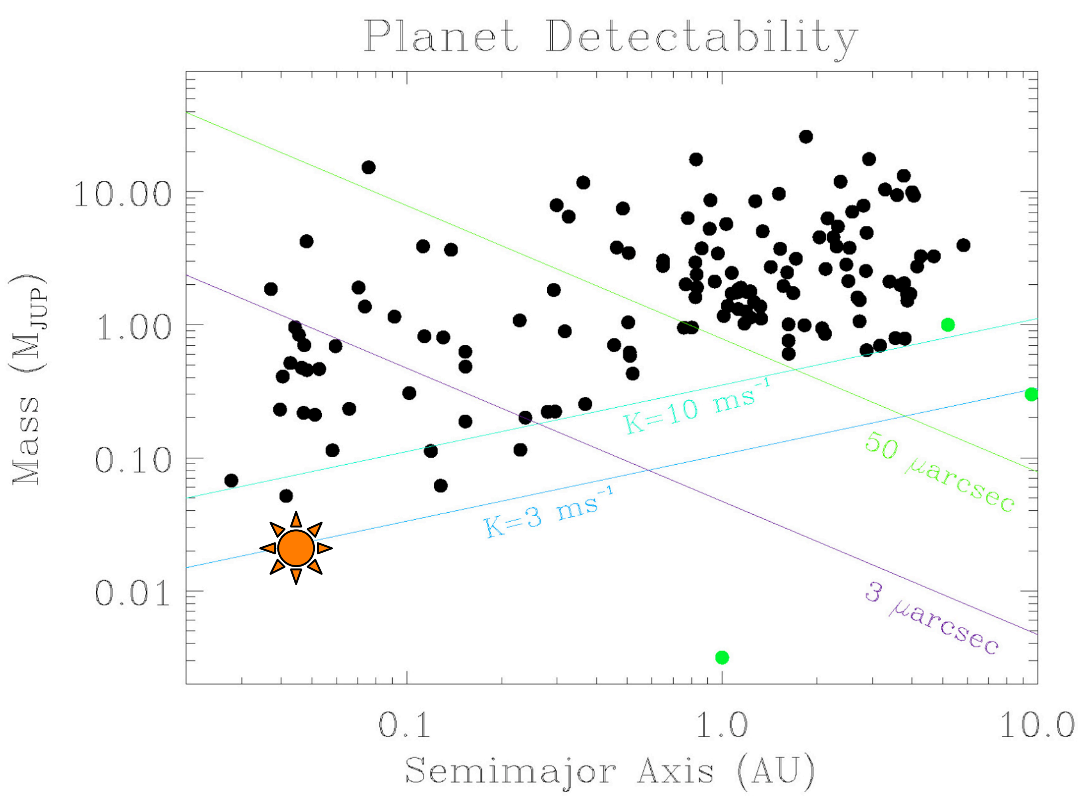

Figure 1- Planet discovery space for stars at 15 parsecs.

Figure 1 is a "discovery space" figure (kindly provided by R. Paul Butler) comparing Doppler velocities and astrometry for stars at 15 parsecs. (While the typical comparison is made at 5 pc, there are only seven late F, G, and K stars within 5 pc -- and two of them are Alpha Centauri A and B. At 15 pc, there are about 150 target stars.) The solid points are known exoplanets, all discovered via the RV technique. The two upward slanting lines are Doppler semi-amplitudes of 3 and 10 m/s, corresponding to measurement precision of 1 and 3 m/s. The two downward slanting lines are astrometric semi-amplitudes of 3 and 50 micro-arcseconds, corresponding to precision of 1 and 15 micro-arcseconds. The 50-micro-arcsecond line corresponds to the most *optimistic* expectation of astrometric precision of the Keck interferometer (in 3-4 years), and for ESA's GAIA, a rather dismal benchmark indeed for the limits of both ground-based and space-based astrometry over the near-term.

Figure 1 shows that, for orbital semi-major axes out to 0.6 AU, 1 m/s Doppler velocities outperform even for a fully-working SIM. It should be emphasized that this was the best possible SIM case. For the nearest 1,000 stars out to 30 parsecs, Doppler velocities do much better than astrometry. The red symbol is our recent detection of a 7.5 Earth-mass rocky planet around the dM4 star GJ 876 at 4.7 pc (Rivera et al. 2005).

Concerning the limits of precision of the RV technique

The radial velocity technique has long been criticized as being ultimately limited in precision to levels that make the detection of earth-mass planets impossible. Precision in radial velocity measurements is a combination of several factors: 1) photon noise (Poisson statistics), 2) noise from stellar surface activity, and 3) systematic noise in the data acquisition and data reduction processes ("instrumental noise"). The world's top planet search teams (using optical spectroscopy) have now reduced their instrumental noise to levels below 1 m/s and are mostly photon-noise-limited for many of their targets. Photon noise is reducible even further, to levels below 1 m/s, by using large enough telescopes and longer exposure times (and/or multiple exposures). Noise intrinsic to the star (due to surface activity of spots, flares, plages, convection granulation/systematic surface flows, low-degree p-mode oscillations, etc.) remains as a potentially-limiting factor. Here, the low-degree global p-mode oscillations occur on short (<20-minute) time scales and are already being averaged over effectively by leading planet hunters through proper observing techniques (averaging several exposures).

Stellar surface noise is also a function of stellar age, spectral type, metallicity, and height above the ZAMS. Proper choice of targets can yield stars that are known to be quieter than 1-2 m/s. For the quietest stars, nobody yet really knows what the ultimate limit of precision is due to jitter from photospheric motions. And it may be possible to reduce such noise even further through use of spectral line bisector correlations, and by correcting for systematic height differences in surface flow velocity using knowledge of line formation depth.

We have been achieving <1.0 m/s for several years now with the most stable stars on the California-Carnegie Exoplanet Survey. Figure 2 (kindly provided by R. Paul Butler) shows a representative sample of some of the stars in this Survey that are quiet at the m/s level over time scales of at least 2-3 years.

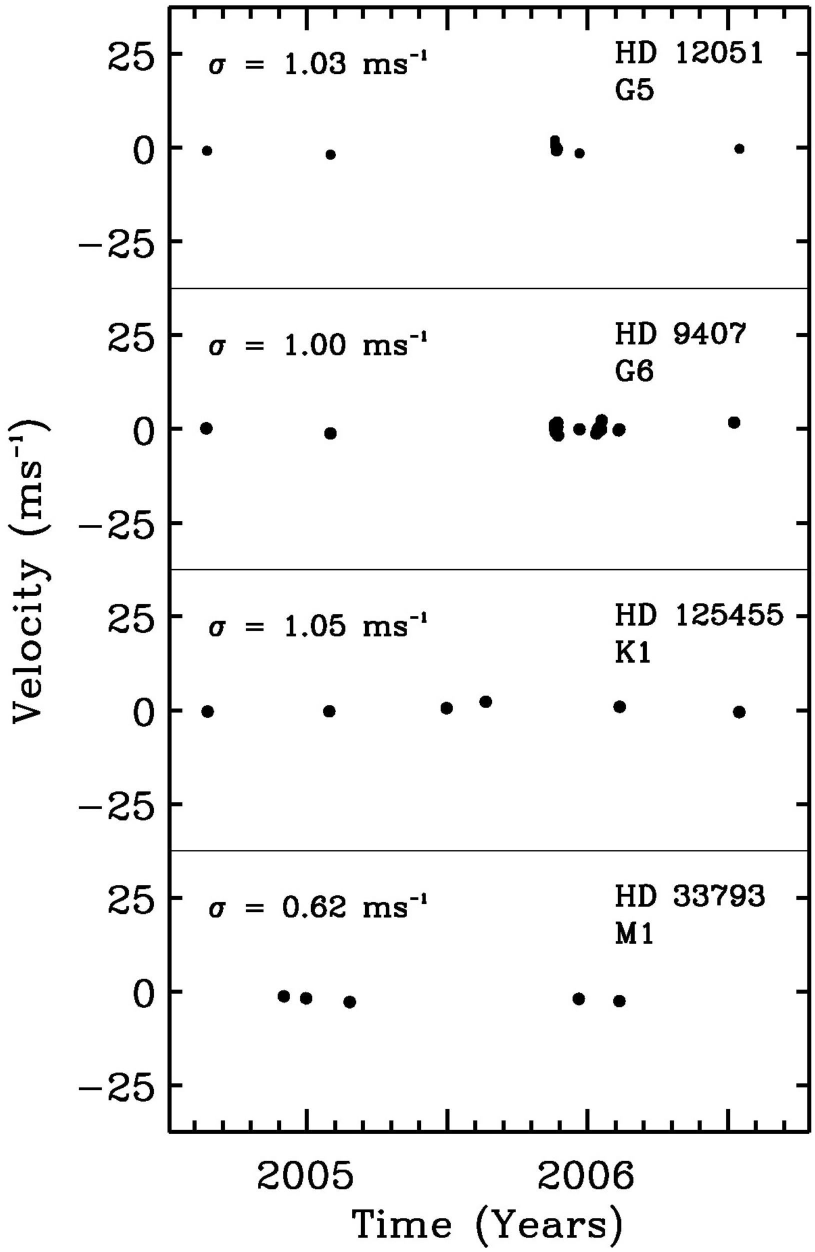

Figure 2- Some of the quietest stars on the California-Carnegie Exoplanet survey.

Note that the RMS (σ) values shown for each star in Figure 2 are actually the sum of (1) noise intrinsic to the star, (2) photon shot noise, (3) instrumental noise, and (4) signals from any unseen planets. If any of these stars has a 10 Earth-mass planet in the inner few AU, it is already contributing significantly here to the RMS and will eventually be detected. The main point here is that our actual instrumental Doppler precision is certainly much better than 1.0 m/s. And, assuming for simplicity that all four noise sources contribute equally, then these stars are also holding stable to 50-80 cm/s, over the 2.5-year time interval since the upgrade of the HIRES focal plane. It is also worth mentioning that no "nightly corrections," linear de-trending, or other ad hoc systematic corrections have been applied to these data.

Our group and the Geneva group are also tracking the ultra-quiet star HD 69830. This remarkable star hosts a system of 3 super-earth-mass planets (~10 Me). The combined fit to both of our independent data sets for this 3-planet system has shown that the star itself has a jitter level at least as low as 70 cm/s. Indeed, when unresolved p-mode oscillations are properly smoothed over by taking 5-10 minute integrations, the stellar jitter is reduced to less than 25 cm/s. It is clear than that some stars demonstrate extremely low levels of intrinsic surface noise, and are quite amenable to the detection of super-earth's and even earth-mass planets.

So for the very quietest late G stars, and for many old K and M dwarfs, the jitter noise floor is likely to be less than 1 m/s. Taking a few exposures to average over convection will diminish this to ~50 cm/s jitter. The Swiss Exoplanet Search group claims similar sub-m/s precision results in the OWL CODEX Concept Study (OWL 2005). They are also studying noise sources attributable to stellar atmospheric motions, and likewise conclude that such noise sources can be overcome through choice of star and proper observing techniques, down to sub-m/s levels (Lovis et al. 2005). At such levels, detection of the 5-10 cm/s signal of earth-mass planets becomes entirely feasible, given enough observing cadence.

Optical vs. near-IR spectroscopy for finding habitable earths around late-type stars

In recent years, nearby M stars have become an important discovery space to search for earth-mass planets in habitable zones. M stars have lower masses, resulting in larger reflex velocities from a planet, allowing lower-mass planets to be detected. The amplitude of the reflex barycentric motion of the star goes as $M_{star}^{-2/3}$. So a planet orbiting a 0.39 solar-mass M2V star produces about twice the velocity amplitude of its host star as with a 1.0 solar mass star. A 0.2 solar-mass M5V star brings the amplitude up almost a factor of 3.

In addition, the habitable zones of M stars are fairly close in (<0.2 AU), resulting in orbital periods of weeks-to-months instead of years. With such shorter periods, in a reasonable amount of time, multiple orbits can be observed, allowing one to further increase radial velocity precision (like a lock-in amplifier, detecting a strictly periodic signal in the presence of white noise) by the square root of the number of observations (or phases covered).

There has been growing discussion in recent years about the great promise of using near-IR spectroscopy (0.9 – 2.5 microns) to obtain the precision radial velocities for late-type stars. Proponents claim that this will be much more efficient than optical spectroscopy at finding planets around late-type stars. Indeed, The Gemini Observatory's recent "Aspen Process" (Abraham et al. 2004) resulted in an explicit recommendation that a high-resolution near-IR spectrometer (HRNIRS) be built for exoplanet hunting around late-type stars, and issued calls for a pathfinder study for a PRVS (Precision Radial Velocity Spectrometer). Various groups have responded with schemes incorporating immersed near-IR echelles and spectrometers crossed with interferometers (c.f. Hinkle et al. 2006; Ge et al. 2006). Another group is building TEDI, an Externally-dispersed-interferometer mated with the Cornell TripleSpec near-IR spectrometer on the Palomar 5-m telescope (Edelstein et al. 2006).

There are good reasons to be skeptical about the near-term promise of near-IR spectroscopy for exoplanet work. The simplistic reasoning behind the notion that moving to the near-IR improves RV precision on late-type stars is that, since cooler stars have more of their flux in the near-IR, this is where detection of their subtle reflex velocities will be easiest. But history shows otherwise and a look at what is (and is not) being accomplished with today's best near-IR technology on the world's largest telescopes provides an important reality check.

To date, over 200 exoplanets have now been discovered, using precision radial velocities from the optical region of the spectrum. None of these exoplanets was detected, ***or has even been confirmed***, using near-IR or mid-IR spectroscopy. Among these known planetary discoveries are a Neptune-mass planet around an M2.5V star (GJ 436), and even a triple-planet system around a nearby M4V star (GJ 876). In fact, one of the planets around GJ 876 has a mass of only 5-7 Earth-masses, and is thus far one of the lowest mass planets known (Rivera et al. 2005). All these detections were done with precision radial velocities using optical spectroscopy. The California-Carnegie Exoplanet Team long ago appreciated that M dwarfs would be an important discovery space for habitable earth-mass planets, and has had over 120 M dwarfs under survey for the past decade. (We would have liked to have even more, but with limited Keck time and a total target list of over 1300 stars to cover, one does not want to apportion too much of one's survey time to stars at the fainter end). We have targets as late as M5.5V, and apparent magnitudes as faint as V=13.5. The Swiss exoplanet group is also surveying a large number of M dwarfs.

In stark contrast, not a single discovery or confirmation has been made to date of exoplanets around M, L, or T dwarfs (or any other type of star for that matter) with any near-IR spectrometer. This is not for lack of trying. Walker et al. (2003) used UKIRT's CGS4 to get radial velocities for GJ 229B, a T6 dwarf of magnitude J = 14.0. Their precision was worse than 1 km/s. McCaughrean et al. (2004) used NAOS/CONICA on the VLT to observe the nearest known brown dwarf binary system Epsilon Ind Ba, and Bb, a pair of very nearby T1 and T6 dwarfs at 3.26 parsecs. Their spectroscopic observations were done in the H-band, and took 24 minutes to get even a low-resolution R = 1000 spectrum (with an 8-m AO-corrected telescope). Nakajima et al. (2004) used Subaru's CISCO to obtain R = 400-600 spectra in the J, H, and K bands of L and T dwarfs at J = 15-16. Their integration times were ~1000 seconds, even at this very low resolution. Smith et al. (2003) used the PHOENIX high-resolution infrared spectrometer on Gemini South 8-m telescope to get radial velocities of Epsilon Ind Ba, and Bb. Here, their resolution was 50,000 and yet their stated radial velocity precision was only ~700 m/s. As a final example, Lebzelter et al. (2005) used a NICMASS detector on the 74" Mt. Stromlo coudé to do a radial velocity study of long-period variables in 47 Tuc at J = 7.5-8.5 (similar to the J-band brightness of GJ 436 discussed above). Their spectral resolution was 37,000, but they achieved only ~400 m/s velocity precision.

For the past 3-4 years, a group has been trying to use the state-of-the-art NIRSPEC near-IR spectrometer (R = 25,000) on the Keck 10-m telescope for planet detection around brown dwarfs and very low-mass stars at the K-band (Charbonneau 2004; White 2007). The technique involves using telluric methane absorption features superimposed on a ^{12}CO R-branch band at 2.3 microns (c.f. Deming et al. 2005). So far, the results have not been encouraging. After 4 -5 years of effort, the velocity precision achieved thus far by this approach (Wizinowich et al. 2007; White 2007) is only 60-70 m/s (and demonstrated over only a 3-day timescale) for two M3.5V stars: GJ 725A and B. These stars are only 3.6 parsecs away, and at V magnitudes of 8.9 and 9.7, and K magnitudes of 4.3 and 4.7 respectively, are both very bright nearby M dwarfs.

This present level of velocity precision from Keck/NIRSPEC on M3.5V stars is abysmal by comparison with the precision of the Keck/HIRES data of the California-Carnegie Exoplanet search. And though the NIRSPEC work is perhaps still in its preliminary stages, a direct quantitative comparison is sobering. NIRSPEC's stars GJ 725 A and B are also on the California-Carnegie exoplanet survey. They have been monitored since 1997 and now have almost 10 years of coverage. Keck/HIRES exposure times for these two stars were 8 minutes each and the long-term RMS velocity variation observed for each star (with respect to the linear velocity trends due to their mutual orbit) is ~5 m/s. That RMS is the sum of photon-statistics, stellar jitter (~2 m/s expected), instrumental noise, and any as-yet-undetected planets that may be there. Clearly, we are achieving < 5 m/s precision, held over a decade now, on each of these stars. Moreover, both of these stars are so quiet that we have added them to our Keck Rocky Planet list, a highly-selected set of stars that will be monitored at higher cadence for rocky planets.

NIRSPEC used much shorter exposure times (10 and 18 seconds for GJ 725 A and B respectively), resulting in expected S/N values (from formal photon-statistics) of many hundreds. But there are apparently rather problematic stability and calibration issues with the NIRSPEC detector (Raytheon ALADDIN-III InSb 1024x1024) that have made optimization difficult. The use of atmospheric methane absorption lines is also expected to introduce errors at levels of 10-50 m/s from high altitude winds. At the end of the day, after 3 years of effort, NIRSPEC delivered only 60 m/s precision (over a time-interval of 3 days) on these M3.5V stars. Perhaps future advances in near-IR detector technology will improve performance, but even if so, a hard-limit is expected to be hit at the 10-20 m/s precision level due to the instability of the telluric methane lines (White, 2007). By contrast, velocities from HIRES are photon-noise-limited and are not affected by telluric noise. We would achieve similar 60 m/s precision with only 3-second exposures. And, at V= 8.9 and 9.7 respectively, GJ 725A and B are some of the brightest M dwarfs on the California-Carnegie Survey, and not even close to the limit of HIRES. The faintest M dwarf on our survey is GJ 406, a V=13.5 M5.5V star, and holding stable (with 8-minute exposures) at < 10 m/s levels over 7 years. The Geneva group has also demonstrated routine m/s precision, and detection of super-earths around M dwarfs (Udry et al 2007).

The above comparison suggests that, for precision RV work on dwarfs as late as M5V, NIRSPEC, a state-of-the-art near-IR spectrometer working at the K-band on the world's largest telescope, is not yet even close to being competitive with present state-of the art optical velocities from instruments such as HIRES and HARPS. Unless dramatic advances are made in IR techniques, the present precision levels of 60-70 m/s on the brightest M dwarfs with NIRSPEC do not inspire much confidence that the meters/second levels of precision needed to detect earth-mass planets around even the lowest mass stars will be achievable in the foreseeable future with near-IR spectroscopy. And unless a way can also be found to avoid using terrestrial absorption lines as a velocity reference (and to avoid their noise contribution in each and every spectrum), near-IR spectroscopy seems unlikely to surpass even 10-20 m/s precision.

There are many reasons why detection of planets around M dwarfs (or any star for that matter) has thus far completely eluded near-IR spectrometers. While it is true that M stars are “flux-challenged” in the optical relative to the near-IR, this is partly because of heavy atomic line blanketing. The continuum of an M star does not even exist in the optical due to such heavy line blanketing. However, it is precisely this abundance of atomic lines that makes the optical region so rich in spectral line-slope information. Atomic line density falls off rapidly into the IR. Molecular bands may be usable to some advantage, yet, as shown above, NIRSPEC’s radial velocities from near-IR molecular bands at 2.3 microns fell seriously short of results from the optical. Mere flux deficiency alone in the optical is not a sufficient reason for preferring the near-IR over the optical wavelength region, at least as far down as M6V stars.

Another serious problem with near-IR spectroscopy is lack of a suitable wavelength fiducial. Iodine works exceedingly well in the optical, providing a stable and easy-to-use reference in the 480-620 nm region, a region rich in radial velocity information for G through late-M dwarfs. Perhaps a similarly good substance exists for a gaseous absorption cell for the near-IR, but none has yet emerged. Species such as the halogen hydrates HBr, HCl, and HI have been suggested, but they probably don’t have the line density needed to calibrate a spectrum to the level of 0.001 pixel. They offer a few hundreds of lines, in comparison to Iodine’s thousands of lines. Other species such as CH3, CO, HCCH, HCN, NH3, H2S, and HF have also been suggested. But despite all the talk, no one has yet actually built such a cell, and shown that it can provide the requisite level of calibration for near-IR spectra.

These days, there is an outsized fascination with exotic "laser-comb" calibration sources. Such pulsed laser emission line sources, tied to atomic vibration standards, are predicted to revolutionize exoplanet hunting by providing an enormously stable wavelength calibration source over a wide spectral bandpass. However, again, skepticism is warranted. All existing discussions of this technique sidestep very challenging issues of how one can mix that comparison comb into the seeing-limited stellar beam at m/s levels of precision. It also remains to be proven if such m/s stability can be held over years in the face of aging dispersive multi-layer coatings and other complex system components. Finally, the accuracy of the wavelength calibration source is only one term in the overall error budget. Noise sources from stellar jitter and from Poisson statistics of the available photons are both serious practical concerns at the m/s level for most stars of potential interest, and will add in quadrature.

The Keck/NIRSPEC precision radial velocity survey of brown dwarfs used telluric methane absorption lines as the velocity reference. However, terrestrial absorption features from the Earth’s atmosphere are severely limited at the 10-50 m/s precision level by wind currents, and may have contributed substantially to lowering NIRSPEC’s achieved precision. These terrestrial absorption features are a serious source of noise concern in the near-IR. Iodine is in a unique sweet spot in that it provides a dense forest of calibration lines, in an interval that is both rich in spectral features from F7-M6 stars, and almost completely free of terrestrial absorption lines.

Yet, even in the relatively dry and pristine Iodine region (480 to 620 nm), weak terrestrial water features enter at the level of a few percent depth, and must be masked to avoid their noise contribution. To achieve 3 m/s, we filter out ~2% of the pixels in the Iodine region due to terrestrial water. By dramatic contrast, the near-IR is heavily blanketed with water, methane, OH night-sky emission, and other molecular absorption bands, features so strong that they eliminate entire chunks of the spectrum, carving it up into a literal alphabet soup of R, I, J, H, and K bands. Even in the very clearest of these regions, terrestrial features abound at depths of many percent. Such features vary unpredictably in both depth and velocity at scales of tens to hundreds of m/s, and on time scales similar to the observations, thereby introducing radial velocity noise that would dwarf the 1-m/s signal from a habitable earth planet. A similar filter in the IR would remove 98% of the pixels. It seems unlikely that any large clear regions could be found that are demonstrably clean of velocity-noisy terrestrial lines.

A final problem involves difficulties in photometric calibration of background-challenged IR spectrometers. It takes S/N of ~300 and 0.001-pixel position calibration to achieve one-meter/second velocity precision. In the optical, modern high QE low-readout noise CCD's can deliver this S/N with ease, and can also be flat-fielded to this level by taking reasonable care in calibration procedures. For near-IR spectrometers however, calibrating spectra to this S/N level seems to be problematic. The NIRSPEC work described above had more than enough photons to reach S/N of many hundreds, and yet it was apparently not possible to calibrate out the various background signals, charge persistence, and pixel-to-pixel bias, dark current, and gain factors to anywhere near that level, resulting in quite low velocity precision. The NIRSPEC detector is a state-of-the-art ALADDIN-III 1024x1024 array with < 25 electrons of readout noise and 80% QE. It was built by the UCLA IR group, a team with many years of expertise in IR detectors and their associated electronics. And yet, even this state-of-the-art system seems to have problems that render high S/N work elusive. But that level of stability and precision is just what it takes to reach the sub-m/s levels of precision involved in detecting even super-Earth-mass HZ planets around M dwarfs, not to mention habitable earth-mass planets.

It is not yet known whether these photometric calibration difficulties in the IR are fundamental limitations imposed by laws of physics, by detector/electronics design, or rather are temporary limitations that can be mitigated through cleverness in observing strategy, and/or data reduction strategy. The onus of that analysis is on the near-IR proponents. It suffices to say that it is not good enough to base performance projections simply on the Poisson statistics of expected photon rates. Loose claims that near-IR spectrometers will be superior to optical spectrometers for planet searching must be backed up with detailed analyses of all noise sources, and actual demonstrations at the telescope of achievable S/N and wavelength calibration stability. Even better (and more believable) would be some simple confirmations of known M-dwarf exoplanets to plant some firm benchmarks in the field. Until more promising performance benchmarks published in the peer-reviewed literature, one should prudently reserve a healthy dose of skepticism of the promise of near-IR for planet hunting.

It is also important to keep in mind that 99.9% of all known nearby M dwarfs are spectral type M0-M5V (Ge et al. 2006). So surveys that reach down to spectral type M5V get essentially all of the nearby M dwarfs. Yet, as shown above, over this M0-M5.5V range, near-IR spectroscopy does not yet seem to be able to compete effectively with optical spectroscopy for precision velocity work. Of course it would be nice to know if any of the few hundred or so known L or T dwarfs harbors planets, but these constitute < 0.2%

of the local population of low-mass stars. And as shown above, even the nearest ones seem to be already at such a faint level in the near-IR that achieving few m/s precision seems orders of magnitude beyond the present state-of-the-art of near-IR spectroscopy.

The bottom line is that, despite being flux-challenged, the optical region works amazingly well at the present time for M-dwarf planet detection. Indeed it is a remarkable and fortunate happenstance that there is still enough flux and Doppler information in the Iodine region to routinely reach precision levels of sub-10-m/s on 99.9% of all nearby late-type stars. For the nearest few hundred or so M dwarfs, exposure times on a 10-meter telescope are also comfortably short (8 minutes). This work is presently limited in precision and target faintness only by the relatively limited availability of observing time on the Keck 10-m. Longer exposures and/or more nights would immediately bring higher precision and fainter M dwarfs into reach. Working routinely to V=13 brings several thousands of M dwarfs into reach, and at V=14, tens of thousands become reachable. The target list (and discovery rate) would explode if we pushed just 1-2 magnitudes fainter. More telescope nights on 6-m to10-m-class or larger telescopes would enable this without the risk or expense of new technical development.

How NSF/NASA can help qualified teams extend the reach of exoplanet searches, accelerate the pace of discovery, and extend sensitivity limits into the habitable-earth regime

There are many teams trying to detect planets through the RV technique. Some are much more effective than others (i.e. produce a dramatically larger yield of planets per observing night). The leading teams are now photon-noise limited in their precision and have their instrumental and systematic errors at or near 1 m/s and held over long time scales (years). As importantly, they have acquired a substantial legacy of years of precision RV data, and those data bases allow them to know which stars to observe and how often. They are recording spectra with sufficient spectral resolution (R > 50,000) to extract essentially all of the velocity information inherent in a star's spectral line profiles. They also have the requisite sophisticated data reduction pipelines in place to produce fully-reduced precision velocities essentially at the telescope, or within a few days of the run. In what follows, I shall refer to such teams as "qualified" exoplanet teams.

NASA (and to some extent the NSF) are fundamentally socialistic organizations, responsible to spread access to taxpayer-funded scientific resources to the widest community of users. Yet today's state-of-the-art of precision RV work is highly refined, and requires enormous investment and a steep learning curve to climb. And contrary to the opinions of some TAC's, the honest truth is that even a very capable astronomer who gets awarded time on a telescope cannot simply push some monkey-buttons on a spectrometer GUI and walk away with 1 m/s long-term velocity precision. Today's leading search teams have each invested literally man-decades of development in their Doppler analysis at sub-millipixel levels. They also understand how to pick stars that are most likely to yield breakthroughs in whatever corner of exoplanet discovery space. They already have years-long head starts on the long time baselines required to see planets in low-amplitude and/or long-period orbits (such a true Jupiter analogs).

Qualified exoplanet teams also understand how to avoid wasting precious telescope time on unfavorable targets. There is always a component of serendipity in the hunt for anything, but exoplanet hunting is long past the Easter Egg Hunt phase. Most of today's advances are the result of years of careful strategizing about observing programs, and drawing upon growing databases of precision data, to glean the most amount of science from the least number of nights. Any astronomer can point a telescope at a set of favorite stars and, with 20-30 m/s precision, get lucky and find an occasional planet. But that blind "point, shoot, and hope" approach is highly inefficient and of very limited value when it comes to advancing the state-of-the-art in the field.

So it is also crucial that any new resources for exoplanet research be given only to qualified teams. Furthermore, it would be best to restrict the number of such teams. If the time is split up among competing teams, it will be used much less efficiently, since one of the key ingredients for efficient planet detection is developing a growing legacy of precision velocity information on any target star. Spreading the limited time among competing groups only assures that nobody gets enough data legacy to be able to hunt efficiently.

Since the best exoplanet hunting teams are now photon-noise limited, improving on the discovery rate and mass limits of exoplanets requires dramatically increasing the photon rate to these qualified teams. This increase in the photon rate can only come from providing more nights on large telescopes equipped with precision RV capability, and providing more efficient spectrometers (higher efficiency and/or larger usable wavelength coverage). The number of nights can be increased by purchasing nights on existing telescopes with precision RV capability, by retrofitting existing telescopes with new precision spectrometers, and by building new telescopes dedicated to this work. The increase in spectrometer efficiency can come by replacing existing old-generation spectrometers with more efficient modern versions, and by building the next generation of spectrometers for upcoming GSMTs.

None of these ideas are very trendy, nor do they offer the shock-and-awe of heroic space-engineering missions, or the caché of clever new instrument schemes. But they do represent, in my personal opinion, the most cost effective and guaranteed way for the NSF and NASA to empower qualified exoplanet teams to make substantial progress over the next decade in the hunt for habitable earths around nearby stars. Here are a few specific ideas for how this could be done.

1) Purchase exoplanet nights on facilities with existing precision RV capability

A straightforward zero-risk approach in the near-term is simply to purchase nights on existing large telescopes that are already equipped with precision optical RV spectrometers, and make these nights available to qualified exoplanet teams. Facilities capable of at least few-m/s precision exist at the AAT, HET, VLT, and at both Lick and Keck. The California-Carnegie Exoplanet Team is using the HIRES/Keck facility to target 1-20 M_{earth} planets within 0.5AU of their parent star down to at least V = 8 routinely, with exposure times of only a few minutes. Around low-mass stars, some of these planets will lie in the habitable zone of the star. The MAGELLAN telescope is expected to have precision RV capability by the end of this year when their new spectrometer is commissioned. The LBT should also soon have this capability once

PEPSI is commissioned. No doubt there are others, this was not meant to be a complete list. The rough cost of a night on the AAT is $15K and will drop to $13K next year. The TSIP cost (http://www.noao.edu/system/tsip/) of a Keck night is ~$48K, for MAGELLAN is $17K, and for LBT is $52K.

Ideally, nights should be given in strategic blocks to allow removal of aliases as quickly as possible. This is almost impossible to do at present with a publicly-shared facility. Such "cadence blocks" would be an important component of the overall search strategy and should be included up-front in any negotiations. NSF and NASA should poll the world's observatories and see what possibilities are available for such TSIP programs.

2) Fund construction of new precision high resolution optical spectrometers for existing 3-5m-class telescopes

NOAO runs 4-m telescopes at both Kitt Peak and Cerro Tololo, and Palomar Observatory has the venerable Hale 5-m telescope. To my knowledge, none of these large telescopes has a high-dispersion spectrometer capable of meter/sec precision. But for a relatively modest investment (perhaps $3-4M each) a precision RV-optimized spectrometer could be built for any of these telescopes. MAGELLAN has a moderate-resolution MIKE spectrometer but that has proven difficult to use for precision RV work. When replaced with a true velocity-optimized spectrometer, MAGELLAN will be the most powerful southern-hemisphere facility for exoplanet work. A decision was recently made to loan the Gemini bHROS spectrometer to the SALT telescope in exchange for nights. But bHROS is far from optimized for planet work, and will likely never be a very useful exoplanet hunter. A properly-designed efficient spectrometer should be built for SALT. As a wild guess, this might cost $4-6M for a telescope of this size.

There has been no attempt here to come up with a list of all the possible telescopes of the world where improvements to precision RV capability (either building new spectrometers or upgrade existing spectrometers) could be imagined. Also, such instrument upgrades would need to be closely tied to availability of nights; it wouldn't make sense to pay for a new instrument on a large telescope if you couldn't then get enough nights for qualified exoplanet teams to use it effectively. There may be many large telescope observatories out there that are looking these days for sources of external funding to support their facility, and who might be eager to hand out nights in exchange for instrumentation and cash. Again, NSF and NASA should poll all the world's observatories in search of opportunities to fund precision RV spectrometer construction, purchase nights and give them to qualified exoplanet teams.

3) Build dedicated robotic 6-8-m class Automated Planet Finders

Every publicly-run observatory must serve a wide community of users, and hence no one user ever gets much time. Survey-style research is thus quite difficult to do on such facilities, particularly if observing cadence is required. And proper observing cadence is a really huge aid to enhancing planet detectability. One needs to search the Fourier space of allowable periods as efficiently as possible (i.e. with the fewest number of observations) and eliminate spurious aliases quickly. Hitting the same target repeatedly with the cadence of strategically-chosen observing blocks quickly eliminates sampling aliases. A dedicated facility, able to observe the same targets nightly or weekly is thus crucial to efficient planet hunting, particularly for the lowest mass planets.

The upcoming UCO/Lick 2.4-m Automated Planet Finder (APF) telescope was funded through a DOD grant and will be an important component of the California-Carnegie team's strategy toward the detection of rocky planets. The APF will be fed stars harvested from the large target lists of the Lick and Keck survey. These will be stars that are highly pre-selected, already known to be quiet enough to reveal earth-mass planets, yet have larger velocity variations than they should be based on their activity indexes (and hence probably showing a planet signal). With such highly pre-selected candidates, the intense cadence of the APF will then quickly be able to lock onto the period, and dig out the small signals through brute sampling statistics (monitoring a strictly periodic signal against a background of white noise). Unfortunately, at only 2.4-m aperture, the APF will require ~45-minute exposures at V ~ 6 to achieve 1 m/s. So a 2.4-m telescope can only survey the ~100 nearest stars with difficulty. Moreover, it cannot do better than 1 m/s because a 2.4-m telescope simply does not deliver enough photons.

By 2015, a large number of Jupiter-mass planets will be known. The cutting edge of the field by then will be the hunt for habitable Earth-mass planets around very low-mass nearby M stars. But dedicated exoplanet facilities in the 2-4-m class will just not have the light gathering power to reach the M stars en masse. Stars just get too faint at the end of the main sequence for a 3-m telescope in high dispersion spectroscopy at the required m/s precision levels. A totally dedicated pair of 6-m to 10-m-class telescopes (one in the southern hemisphere and one in the north) would be able to provide this capability, at a small fraction of the cost of most space missions. For example, one could envisage North-South clones of the MAGELLAN telescope at ~$50M each and instrumented (for perhaps another $5M) with a HIRES-style or UVES style high resolution spectrometer specifically optimized for precision radial velocity work. For $110M, you'd have an extremely powerful planet-finding engine that would discover hundreds of planets/year, and would also be able to get the required cadence to push well down into the habitable-earth regime.

Located at existing observatory sites (with existing support infrastructure), these telescopes would be totally robotic, and able to function without human oversight most of the time. If properly designed, and located at a site with existing infrastructure, operating costs should be minimal. Local support staff could quickly intervene or call for backup should occasional problems arise. The targets for RV work are all fairly bright stars (V < 15) with excellent coordinates, so robotic target acquisition and guiding will be straightforward with today's technology. Aside from periodic maintenance, all that would be required of observers is to feed the telescope prioritized target lists each night, and manage the data pipeline. Data streams from the robotic telescope would be reduced and analyzed using sophisticated data processing pipelines such as are now already in place by qualified exoplanet teams.

4) Fund the construction of precision RV spectrometers for the next generation of GSMT's and purchase nights for planet hunting.

Covering a large sample of faint nearby M stars at 1 m/s precision taxes even the capability of 8 to 10-m class facilities. A next-generation GSMT, instrumented with an optimized precision radial velocity spectrometer, would have unique capabilities to search for habitable Earth-mass planets around nearby stars later than M3, and Brown Dwarfs as well. It would also provide the crucial follow-up capability required for determining masses (or upper limits to masses) of KEPLER and COROT candidates. The larger aperture of a GSMT would also make possible very high S/N spectroscopy of the atmospheres of hot Jupiters, enabling studies of their atmospheric constituents in transmission (during transits) and in reflection.

MTHR (Moderate to High Resolution spectrometer) is one example of what a GSMT-sized spectrometer might look like (Vogt and Rockosi, 2006). Developed specifically for the TMT, MTHR is adaptable to any of the GSMT concepts. The "M" in MTHR stands for Moderate, as it has a moderate-dispersion multi-object mode. For the present purpose, forget the "M" and focus only on the "HR" or High Resolution mode, which is the mode that would be used for exoplanet hunting.

The MTHR concept combines the best advantages of both VLT's UVES and Keck's HIRES spectrometers. The dual-white-pupil/dual-arm configuration of UVES is used to limit the sizes of the echelle, cross disperser, and camera, and a HIRES-style camera is used to allow for a much larger camera as the spectrometer is scaled up to match TMT's enormous 30-m aperture. The resulting design works very efficiently on TMT, already exceeding the Throughput (slit width * resolution product) of any existing spectrometer by about 15% (46,000 arcsecs vs. 40,000arcsecs), with even higher Throughput possible upon further scaling or using a steeper echelle. Furthermore, MTHR does so without the need for AO or image slicing.

Figure 3 shows a CAD rendering of MTHR on the TMT. The present concept shows MTHR situated underneath the (ghosted) left Nasmyth platform at an f/15 focus. It occupies a footprint of about 10 x 11 meters (about half of a tennis court).

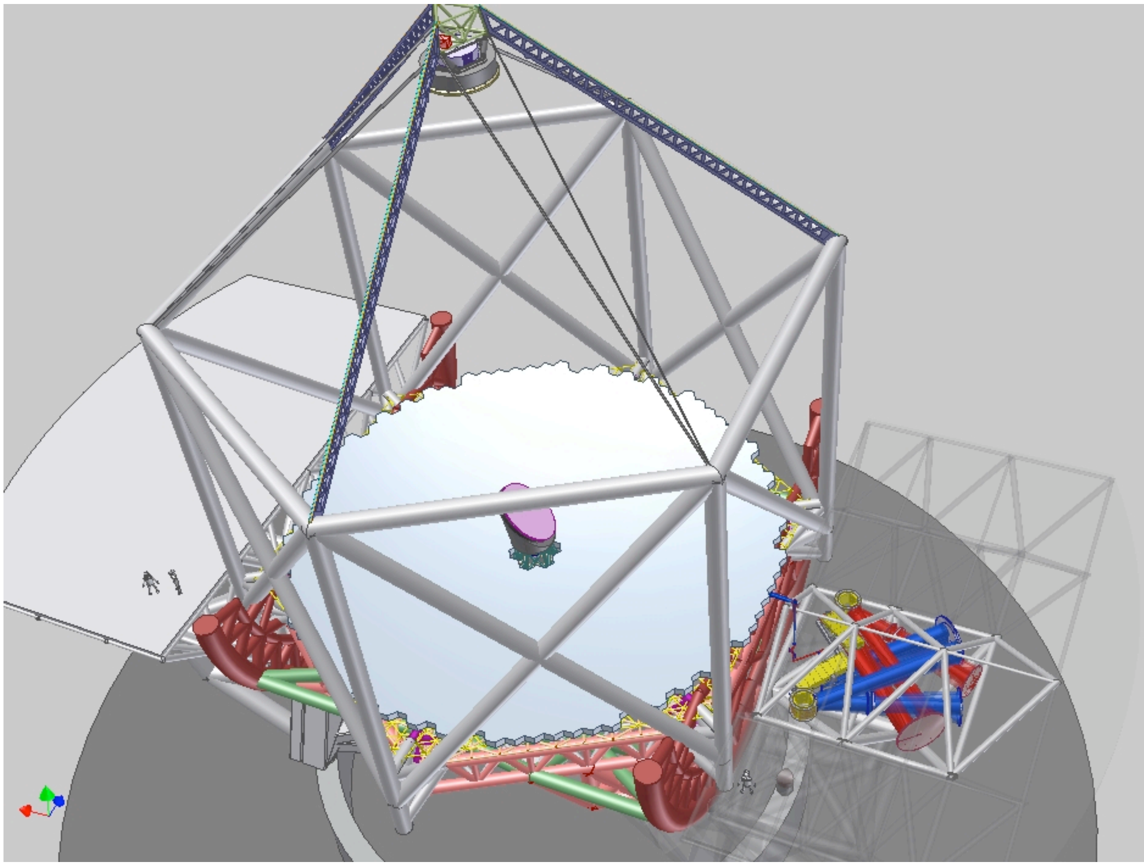

Figure 3- CAD rendering of MTHR at an f/15 focus, under the left Nasmyth platform.

Figure 4 shows a detailed top view of the high-resolution mode. The full-up MTHR is a dual-beam instrument consisting of two parallel (and functionally identical) red and blue arms. Each arm is a grating-cross-dispersed echelle spectrometer in dual-white-pupil configuration, with a scaled-up HIRES-style camera feeding a CCD array of 8k x 8k x 15-µm pixels. The blue arm is coating-optimized for 0.3 to about 0.55 µm, while the red arm is optimized for the 0.45 – 1.1 µm region. However, since the HIRES-style cameras and mirror-collimators are almost totally achromatic, either arm can work over a much larger wavelength region. A dichroic mirror splits the beam into red/blue arms.

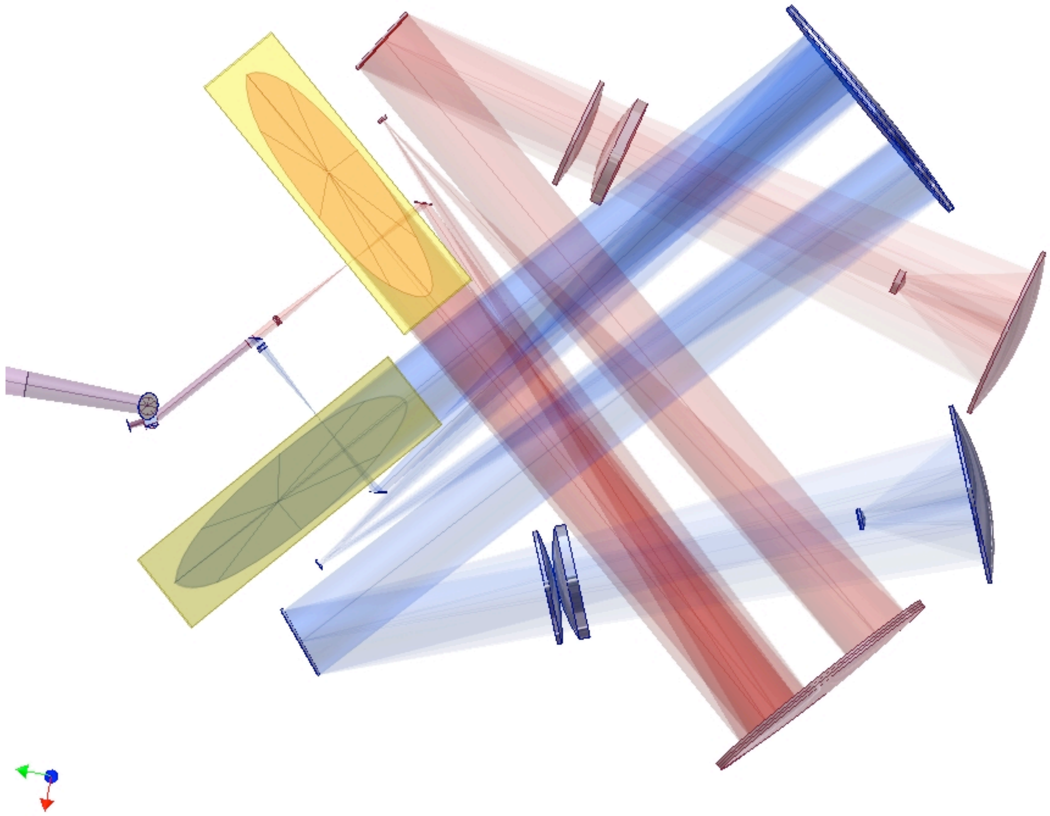

Figure 4- Top view of MTHR's high-resolution mode.

The optics in MTHR are large and challenging. The echelles themselves are 1.0-m by 3.5-m, about the size of ping-pong tables. The collimators are 2-m in diameter and are off-axis sections of a 3-m parent paraboloid. To lower costs, they could probably be made spherical and warped into off-axis paraboloids by stressing harnesses. While challenging, none of the MTHR optics are deemed beyond the present state-of-the-art, either of techniques or component availability.

MTHR would always be on hot stand-by and available to users 24-7. At any time during the night, it could be brought into operation on the sky in as little time as it takes to rotate the tertiary to feed the light onto its slit. Wavelength and flat field calibrations would all be done during the day, and stability will be such that no calibrations will be required during evening hours. MTHR would be a queue-scheduled instrument, ready to go on sky any time prevailing conditions make other modes unworkable (such as hazy sky conditions with AO). MTHR can work quite effectively in most all sky conditions (clouds, marginal seeing, etc.).

The full-up version of MTHR costs $44M. But exoplanet hunting could be done perfectly well without the moderate resolution multi-object mode and with only the red arm of the spectrometer, cutting the cost to $26M.

What a GSMT with a MTHR could do for exoplanet hunting

We now know for certain that M stars have rocky planets (Rivera et al. 2005). Figure 5 shows an artist's rendering of the triple planet system around the nearby M4 dwarf GJ 876. The object in the foreground is GJ876d, the 7.5 Earth-mass planet we detected several years ago around this low-mass star, and denoted by the red symbol in the discovery space plot of Figure 1. GJ 876d is almost certainly a rocky planet, but with a thick atmosphere and internal heating due to tidal pumping from the other planets in the system. The two other planets in the background are Jupiter-size planets in 30 and 60-day orbits, locked in a 2:1 mean motion resonance. The perspective in this rendering is accurate. This remarkable system of three planets, around one of the nearest stars, portends that planets abound around M stars and that some of them are quite low mass, right down into the rocky planet regime. The local neighborhood of nearby M stars (tens of thousands) is certain to be fertile ground for planet hunting!

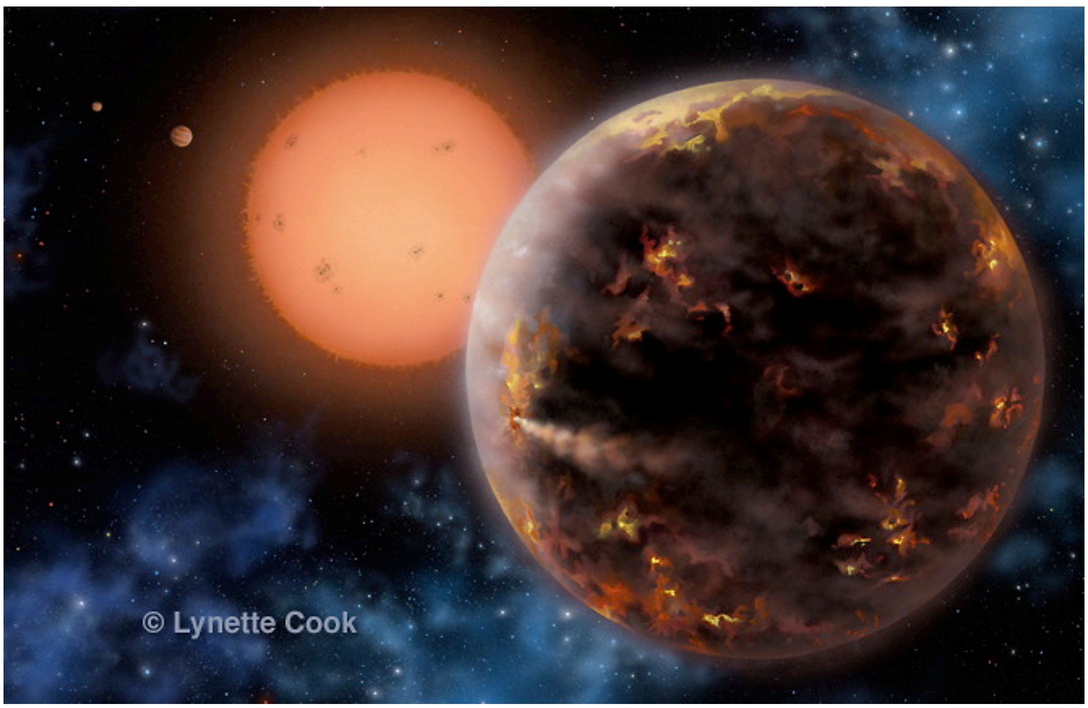

Figure 5- The remarkable triple planet system around the nearby M4 dwarf GJ 876.

Habitable zones of early M dwarfs are in the 30-100 day period range, and the reflex barycentric motions of low-mass M stars will be large enough for 1 m/s precision to reveal rocky planets. For select stars, noise sources also appear to be under control. Noise sources involving intrinsic stellar jitter are white and random, or are at short periods, typical of p-mode oscillations. Short period p-mode noise (5-15 minute time scales) is easy to distinguish from a planet signal and can be effectively attenuated by proper observing techniques. False signatures arising from stellar rotation will be more difficult, though not impossible to discriminate against using precision broadband photometric monitoring. Photospheric convection noise is also basically white and random. It can be suppressed by observing many cycles of a short-period orbit, locking-in on that strictly periodic Keplerian signal. In fact, the real beauty of looking for planets in the habitable zones of M stars is that the periods are short enough (30-100 days) that many orbital cycles can be observed over only a few years, thereby gaining in S/N by the square root of the number of cycles.

With reasonable cadence over a single observing season, a GSMT equipped with a MTHR-style spectrometer could find planets as small as an Earth-mass in habitable zones around nearby M dwarf stars. Furthermore, it would not take a fully-appointed MTHR to do the job, and much of the work could be done in queue-schedule mode. Queue-scheduled observing simulations (kindly provided by Geoff Marcy) illustrate what can be done. The simulations involve detecting a 5 Earth-mass planet in the habitable zone (p=50 days) around an M2 dwarf. Precision was assumed to be 1.0 m/s (TMT/MTHR, one will get 1 m/s precision at V=13.0 in about 8.3 minutes). Mock velocity curves (sinusoids) were generated for the desired orbital period (50 days) and the value of K (0.97 m/s) that corresponds to the planet mass. As regards sampling, one does not want to sample every night because that over-samples a 50-day period. Sampling less often is better for a given amount of observing time. Ideally one would employ logarithmic sampling to capture short and modest periods, with minimal aliasing. Various samplings were explored.

Suppose that only three nights (~30 hours total) were allocated for exoplanets on the GSMT. Ideally, one would want to parcel the time in stints of ~30-minutes spread over 150 nights to get good time coverage for orbital periods of ~50 days in the habitable zone. We simulated such a parceling of telescope time, observing a star 50 times spread over 150 actual nights (one season), with uneven time sampling. Figure 6 shows the result (note the higher cadence at the beginning). The periodogram (center panel) shows the 50-day period clearly, and it is also clearly visible in the phased velocity plot (bottom panel). Here, K = 0.97 m/s similar to the errors of 1 m/s. The equilibrium surface temperature for this planet is 79˚ C, i.e., in the habitable zone. And while some may view this as only an “indirect” (i.e. not real) detection, that peak in the power spectrum is every bit as significant as any “single pixel of light” image from the first Extreme-AO detection of a planet.

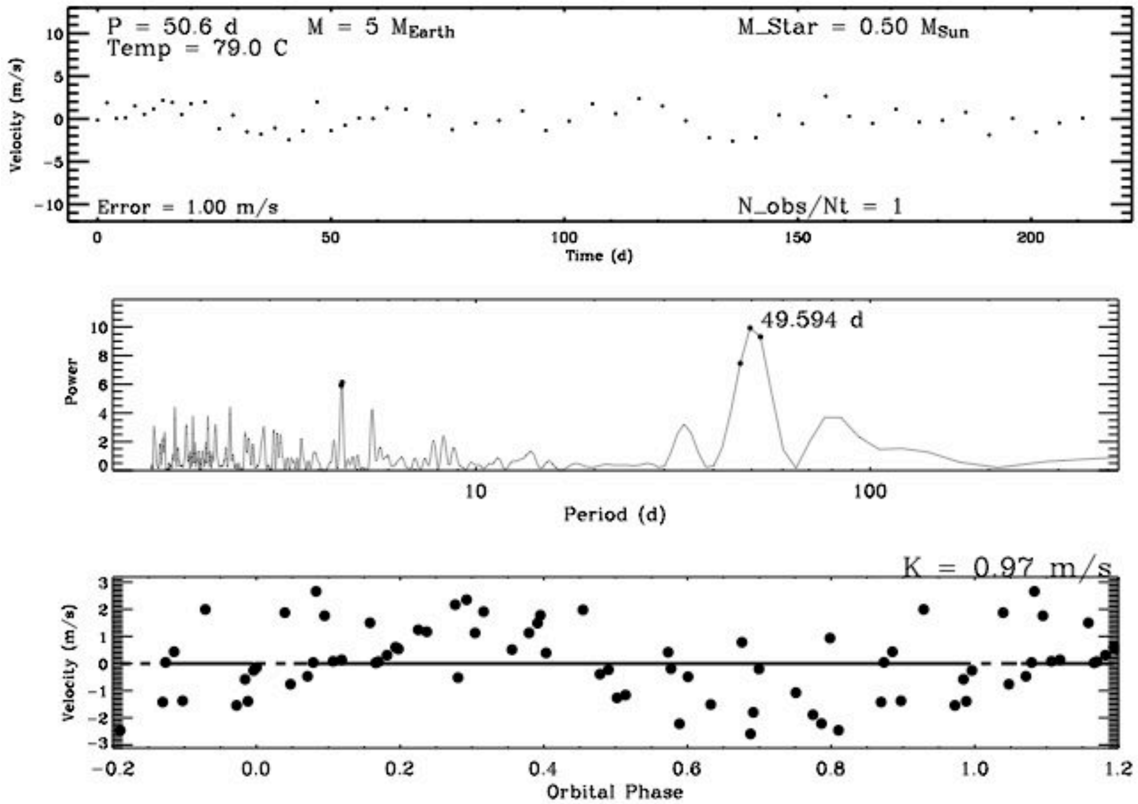

Figure 6- Detection of a 5 M_{earth} planet in the HZ of an M2 dwarf.

A GSMT equipped with the equivalent of a MTHR high resolution spectrometer would be fast enough to deliver true 1 m/s precision on thousands of M dwarfs. Stellar jitter will not be more than 1 m/s, we suspect, based on Keck velocities. And if one can queue-schedule, parceling the time over a duration greater than ~50 days, then even terrestrial-mass planets in the habitable zone of nearby M dwarfs will be detectable.

Achieving 1 m/s radial velocity precision on a reasonable sample (many hundreds) of M dwarfs (V 12-13) is currently not practical with an 8-10m-class telescope, requiring 1-hr to 2.8-hr exposures. However, with a 30-m GSMT, exposures for 1 m/s precision are:

- 3.3 minutes at V=12.0: reaching ~540 nearby M0 stars
- 10.0 minutes at V=13.2: reaching ~2260 of the nearest M0-M4 stars
- 25 minutes at V=14.2: reaching ~9000 of the nearest M0-M4 stars

At these exposure times, what is now a stretch on the brightest M dwarfs with 6-m to10-m-class telescopes becomes fast enough on a GSMT to cover a reasonable sample (thousands of stars) in a reasonable amount of nights (or fractions of queue-scheduled nights). Imagine the discovery possibilities from a survey of the nearest 9,000 M dwarfs within 100 pc for rocky planets and habitable earths!

It is also worth noting that this kind of radial velocity work requires only the Iodine region of the spectrum, from 480 to 620 nm. Thus it can be done with the significantly less-costly "single-arm white" version of MTHR, which could be built for only $26M.

All of the world's GSMT projects trumpet the importance of being able to do exoplanet hunting; it's generally one of the top bullets. But despite all the rhetoric, widespread community support for providing the instrumentation for such work may be thin. Indeed, the TMT decided not to make MTHR a 1st-light instrument. I suspect that his disconnect occurred because precision RV work is a very specialized and highly technical endeavor, not available to the general user unless they are explicitly part of an experienced exoplanet team. It is much more like P.I.-based legacy science than community-based science, and therefore does not engender bandwagon support from the user-community. Perhaps private donors are the only solution for building such mission-driven facilities.

There is also the issue of nights. Most of the GSMT's will be heavily shared facilities, with lots of major partners. Unless blocks of time can be arranged, or regularly queue-scheduled time can be obtained, it will be hard for even one qualified exoplanet team to get the amount of time necessary to make real progress on searching for habitable earths around all the nearby M stars.

In summary, a GSMT equipped with a MTHR (or equivalent) high resolution spectrometer would easily give a 2-3 magnitude boost to enable enormous progress along the road to discovery of habitable earth-mass planets around nearby stars. While perhaps not as trendy or heroic as exotic new instrumental approaches or large space missions, in the hands of a qualified exoplanet team, such a facility would allow us to explore a large sample (~10,000 stars) of the nearest M dwarfs at 1 m/s precision. Within a few short years, we would be finding habitable-earths, if they are there, or determining conclusively that they are not there.

Since MTHR has not been selected currently as a 1st-light instrument, there may be an excellent opportunity for the NSF and NASA to step in and fund this spectrometer. Their contributions would be highly leveraged and, if matched with significant queue-scheduled time, would add a tremendous boost over the next two decades toward the hunt for earth-mass planets in the habitable zones around nearby stars.

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