Discussion following panelists' presentations

White: I'd like to go back to a question that Bob Garrison raised about the variability in measurements of the same quantity made by different people. Why all this trouble? Why the spread? Is it a resolution problem?

Garrison: No, it's not resolution, it's sloppy observing.

Ayres: I would say difficult observing, not sloppy.

Garrison: No, it's often been sloppy.

Ayres: All right, *difficult* sloppy observing [laughter].

Garrison: If you wish. But in the case of $B-V$, it is very difficult to do accurate work without spending an awful lot of time on standards, and most people don't spend much time on standards. They just take mean coefficients and things like that. In the case of MK types: determining MK types is not hard. In fact, it's quite easy to do. But, you have to take the standards! And you have to take them carefully, with the same equipment, and *most people don't do that.* That's where the problem lies. They take a few standards, or they take a few sub-standards that somebody else has suggested, they don't observe them well, they don't use standard equipment, they look at a print from the atlas rather than taking standards themselves: all these things come into play. Some people are just a lot sloppier than others. But if you carefully do MK classification, you can come with half a subtype of Morgan and Keenan.

White: But these schemes are devised in such a way that done carefully, they're independent of little nasties like spectral resolution, and scattered light in the machinery, and all that.

Garrison: Well, not altogether. But if one person determines a thousand $B-V$ values, and does it carefully, then you can rank the stars quite nicely, and rank them very accurately. But if you grab one $B-V$ from Joe Blow and another from Jane Doe, you run into the problem of these inhomogeneities. And it's simple sloppiness, and often it's propagated by big telescopes, because you get so little time on the darn telescope, you don't have time to properly do the standards. So you take what you can and do the best you can with it. Overall, though, I think it's a problem of sloppiness.

Ayres: Well, I'm going to disagree with you, and I think you are contradicting yourself in a fundamental way. I very much like your point that if you take one person, a very careful observer, who has produced a huge amount of material on a whole range of stars, then a believable ranking will emerge from that list. That's fine, and for the stars it works great. but what about the Sun? You can't observe the Sun with the same equipment you use for the stars, so immediately you have a dichotomy. You have to figure out some way of observing the Sun with more or less the same equipment. It's sort of the approach Jeff is taking with his fiber feed on the SSS spectrometer. But still, even that is not *exactly* the same.

Garrison: Yes, and it's also during the day.

Ayres: That's right. I would prefer to use your aluminum sphere, but even that's going to have problems. I've been very interested in this $B-V$ controversy. It's gone on for years and years. And I think it's coming from just the point you made: you *can't* observe the Sun the same way you observe the stars. That immediately leads to a large potential systematic error, and I think most of the dispersion is due in fact to systematic errors.

Garrison: Well, there's two effects at work here. One I call the *bandwagon effect*. Someone suggests something, and everyone for the next 7 years finds that lo and behold, their observations fit that model. Then the model has to be discarded, and everybody swings back to the other value. Just look at the solar $B-V$s. As soon as Hardorp suggested that the Sun was really G5 (well, the Sun is G2 by definition, so we've now got to change all the other stars for starters [laughter]), as soon as he got 0.66 or 0.68, then everybody started getting that value, and now you notice for the past 5 years, everybody has been getting the "old" value of 0.64 or 0.65. This is a textbook illustration of this bandwagon effect. We're not so objective as we like to think we are! Secondly, there's a *don't rock the boat* effect, in which we say, "Well, we like the *status quo*, don't tell us anything new." The solution to all this, of course, is to observe wisely and carefully.

Guinan: I had a comment on Tom's remarks about observing large numbers of stars. It's already happening; there's microlensing programs going on that have millions of stars. But these are biased toward giants, and it's hard to find the solar-like stars in there. I also had a comment for Derek: you plotted mass on your diagram, which is one of the things that we *don't* get, so you contradicted what you were saying about using observable parameters for finding solar analogs.

Buzasi: That's true, but I'm running a theoretical model; I have to use a physical quantity. Then of course, we get back to the issue of converting that into something that's measurable. I tried to indicate what rough MK type the results corresponded to. There's slop on both sides, so I would just say, "be conservative."

Guinan: Why didn't absolute magnitude appear as a quantity on your parameter list?

Buzasi: It probably should have.

Guinan: Well, it has to, with HIPPARCOS data available now.

Buzasi: With HIPPARCOS, yes. As I said, I've softened on that one somewhat.

Guinan: One comment on 18 Sco. The IUE measurements show that the Mg II hk feature is right in the middle of where the Sun falls, so that's probably not agreeing with you, Jeff. I find that star to be a *very* good match for the Sun in h and k.

Hall: Yes, I'd like to clarify Derek's remarks about my 18 Sco results. I find 18 Sco's H and K – calcium H and K, I mean – to match solar minimum HK values virtually exactly, and it's pretty flat. The variability Derek was referring to is sort of in a different venue. It's a tentative result, and I certainly haven't published it yet! It's just regarding the variability in the photospheric lines; they seem to flicker a bit more than those in 18 Sco.

Guinan: Is that because it's slightly fainter than the rest of your stars?

Hall: No, I have normalized the results to the same signal-to-noise and the same airmass, so simple differences in spectrum quality shouldn't be a problem.

Guinan: Well, the photometry also shows that this star is *not* varying, down to the millimag level. I get less than 0.005 mag rms, which I think is rock stable, although it is only a year of data.

Hall: I think that's entirely believable. Our first observation of that star is from somewhere around April 1995, and the calcium K-index is *flat*, well within the error bars. We're discussing two issues here.

Ayres: But the magnesium measurements would be a better test of the activity level than calcium, because the magnesium is less subject to calibration uncertainties.

Guinan: We're measuring it as an index, taking a swatch at the bottom of the line, compared to the line emission, so that is kind of taking care of abundances, too. My other comment is for Richard: it turns out that if you look at activity levels of stars, after about 2 billion years old to 9 billion years old, there's no difference! So if you want a solar twin, you can have it be 2.5 billion, 7 billion, in terms of activity.

Wade: But have you restricted yourself to the same effective temperature or the same mass? Because a nine-billion-year-old star with the same effective temperature as the Sun is not the same mass.

Guinan: Well, right, you'd have to go along an isochrone. But between two and nine billion, for a one-solar-mass star, the activity is the same.

Soderblom: I don't understand that claim. What are you basing the ages on?

Guinan: I'm basing ages on clusters and rotation. The best relationship, as Derek pointed out, is rotation. It's one of the cleanest indications of age.

Soderblom: We might *hope* that, but I'm not sure we *know* that.

Guinan: Well, we *do* know that, we have rotations for stars; we just did β Hyrdi from the IUE data...

Soderblom: One star.

Guinan: And α Centauri A. And 51 Peg. It's being done. Now yes, one of the problems for rotation in older stars is that it doesn't exist. We have three G stars with rotation: the Sun, α Cen A, and β Hyi.

Ayres: I think it's fair to say that the activity levels through which the Sun passes during its cycle more or less cover the range of activity levels exhibited by stars of those ages.

Wade: Well, I thought that Mark's data showed a considerably larger range.

Giampapa: Forty percent of the stars were within the solar excursion over its cycle, but yes, about a third of them were well above solar maximum. Some we know are spectroscopic binaries, but others could be just high-activity stars.

Cayrel: Hardorp did not attribute a $B-V$ directly to the Sun. As I said yesterday, he went through its preferred solar analogs and stated that they have the same ultraviolet features as the Sun. Now, we still need to study the UV features in the Sun and its analogs, why they are the same. But, Hardorp's $B-V$ was higher because the stars he used were more metal-rich than the Sun. The solar $B-V$ question has nothing to with the attribution of Hardorp. I worked with him for five years.

Garrison: Well, the stars that he used, for whatever reason, do not fit. So, he was wrong in the way he ranked them. And in fact, six months before he died, he came to Toronto, and sat in my office, and said, "Bob, you were right."

Ayres: This is an important point against using single color measurements. $B-V$ for example, is quite susceptible to metallicity, and we just need to be cautious.

Giampapa: Suppose you want a solar spectrum, measured through your instrument. We've been talking about identifying stars or solar system objects to let you do that. How practical would it be to take a flux-calibrated, full-disk spectrum of the Sun, and to fold that through the response function of your instrument. Is that perhaps a more reasonable approach for certain applications than trying to actually observe a so-called analog? The problem then just boils down to thoroughly understanding your instrument.

Garrison: The more corrections you put in, the more models you put in, the more theoretical data you put in, the more problems you have. I prefer to approach the problem in several different ways, and see how well the answers agree.

Buzasi: I think there are strengths to that approach. It's not that different, for example, from observing the Sun as a star through a 200-micron telescope, a la SSS. But there are still problems: you still observe the Sun during the day. I don't know exactly how big of a problem that is.

Radick: I think I need to pick up a hammer and react to a claim that Derek made about photometry. If I understood him correctly, he said that the photometric variability is sensitive only to spots, and that's clearly not true. Even in the solar case, the radiometry is sensitive to anything that affects the radiative output of the Sun, both dark spots as well as bright faculae. In this context, there's an interesting story from the early days of solar radiometry. The first data from SMM, which was launched in the early 1980s, were widely distributed about a year later, and the most prominent features were the large dips associated with the disk passage of sunspots. Some people saw those data and concluded right away that only the spots affected the total irradiance. So they went out and made predictions and reconstructions, from spot measurements, of what the cyclic variability of the solar irradiance would be. Naturally they found that the Sun would be faintest at activity maximum. What they did, of course, was to forget the faculae. Admittedly they produce a subtle effect, but they are clearly there. You can see them both as positive "shoulders" on the sunspot dips in the irradiance, but more prominently when SMM continued to measure irradiance over the extent of the cycle. Then they began to pick up the distributed faculae, which raise the baseline but tend not to show up in the short-period variations. So, I think that we have to conclude that the stellar photometry, like solar radiometry, is probably sensitive to bright features as well as dark features. The photometry will, however, preferentially pick out the spots, because they have more restricted time effects on the observations.

Buzasi: Yes, you're right, I overstated my case. But, the other problem with the photometry is that it's difficult radiometrically to tell the difference between, say, the Sun and a star with twice as many spots but also twice as much facular contribution. It gives you the same net effect, but it's clearly a very different stellar surface.

Radick: I would argue that you need a mix. You need something like an HK index, as well as photometry, to help sort out the mess. You have more than one component contributing to the net effect.

Buzasi: Right, that's really my point; photometry gives you the net effect only.

Ayres: And it gives you the net effect only in a narrow band.

Radick: You also have to make the correction to wide band. It's effectively a bolometric correction.

Ayres: That's simple if you know what the correction is!

Mello: A question and a comment for Dr. Garrison. All of the solar analog claims have been made from spectroscopy, and all of the people working with high-resolution spectroscopic data, like Giusa and me, have always used some source for the solar light. Perhaps it was the Moon, or the sky, or an asteroid. I would like to know, how trustworthy are all these sources, let's say from 4000 to 7000 Å, and what are the pitfalls? And, as a comment about the MK types, in the Bright Star Catalog, 16 Cyg A is given as G1.5 and 16 Cyg B as G2.5. These data come from Keenan & Yorka 1985, and in the same publication they explicitly mention 18 Sco as G2 V. In all data they imply that 18 Sco lies between the two 16 Cyg stars. The MK types are quite clear in placing the Sun between the 16 Cyg stars, and 18 Sco lies quite close to this point.

Garrison: Well, I would ask how reliable solar sources are from zero to the radio region, not just 4000 to 7000. I think it depends on your equipment, how well-baffled the spectrograph is, how it handles scattered light, and on many other factors. So I can't give you a single answer. I do know that the asteroids, which are my favorites for the blue-violet region, would not be good in the infrared. They are probably not very good in the ultraviolet, either. In terms of taking solar spectra, you have to also remember you are taking different layers in the Sun when you look at different colors. When you look in the ultraviolet, you're looking at the chromosphere. In the blue-violet and visual you're looking at the photosphere. As you go toward the red, you start to be influenced by winds. The only single source I can think of is this aluminum ball, and that has its flaws, too. But at least it would give us a standard we might all be able to agree on.

Mello: All my data rely on Ganymede, which may possibly have an atmosphere. It could be a problem.

Cayrel: I have nothing against asteroids. I would like to observe them. But you know better than I that with just two nights at CFHT, I cannot spoil half a night – well, I did it once! I saw that the differences in the observations were within the epsilons, and I was satisfied, and I went back to my scientific work.

Garrison: That's exactly the point I made. I've been fortunate in my professional life to have all the telescope time I could possibly use. So I use a small telescope at home, in a lousy climate like Toronto. But 30% of the telescope time, as often as I like, even in a lousy climate, is a big advantage. That's why I quit using CFHT; I couldn't take standards!

Cayrel: I think it is better to take your standard *immediately* after your target star, even if it is not an asteroid.

Garrison: Well, because I have lots of telescope time, I take the four or five brightest asteroids every time I can possibly get them, I take Moon spectra, I take sky spectra, Jupiter satellites, and compare them all. I even did Uranus, and Uranus was a mistake in the ultraviolet, too [laughter]. You have to live and learn – but with big telescopes you *can't* live and learn, you can't experiment. So I don't know how you solve the standard problem using big telescopes.

Hall: Throwing in my two cents from SSS on this topic, we have observed the sky, the Moon, and Ganymede and Callisto. This goes with Tom's comments on making sure you're looking at the same thing when you observe the Sun and stars. We found the Moon and Jovian satellite spectra were indistinguishable from the solar spectra, but sky certainly *was*. There was filling-in of, for instance, the K-line core, and that agrees with the absolute fluxes published in 1979 by Linsky et al.. They also have Moon, sky and I think Jovian satellites as well, and it's exactly the same: noticeably higher fluxes from the sky, in the H and K lines, than the Sun. So I certainly wouldn't trust anything based on a sky spectrum, at least with our instrument. I think it's probably equipment-dependent; you just have to understand your own equipment very well.

Garrison: I think what happens is that the airflow will fill in some of the most important saturated lines, making it look later.

Lockwood: Derek, you discounted absolute energy distributions of the Sun and stars as being important, and I think that's because the data are so bad...

Buzasi: I didn't discount them as being important. My discounting them was based on empirical reasons, but for the reason you say, yes.

Lockwood: Well, I've touched that tar baby, and I'm still stuck to it [laughter]. I was embarrassed to see yesterday how bad the Lowell spectrum is in the red, but on the other hand, the blue spectra of some other people aren't so good either. I'm just wondering what kind of recommendation is floating around here. It seems like the only group active in this right now are the Russians, and the rest of us have been stung by it, and have quit working on it without really doing the best possible job. We need to know the solar spectra energy distribution to 1%,and we certainly don't at this point. I don't think any of us knows what to do about it.

Buzasi: I agree. At least it's still there to make a living doing.

Lockwood: It's not much of a living [laughter].

White: I'm waiting for Gary to weigh in on this one...

Rottman: That's all in the future. We do plan to launch an instrument to get the spectral distribution of the Sun from space.

Buzasi: I think you're doing it now in the right place. I think the most important part of the spectral distribution to know is the UV. It's the most sensitive to small changes in solar activity.

Rottman: Another comment: I'm still confused, because I don't do sun-and-stars for a living. I would think the type of device that you have, Jeff, with a fiber for the Sun, and a telescope feeding starlight into a fiber, and all of it going into a spectrograph, is a fairly good way to go. So if it's just the diurnal variation in the atmosphere that's causing the problem, maybe you need to go into space, to look at the stars and the Sun at the same time without an atmosphere.

Lockwood: I wonder how that would be calibrated.

Wade: Well, can you take a platinum blackbody into space?

Lockwood: Not personally, no [laughter].

Guinan: There are satellites in space you could use.

Lockwood: You can't track them with a telescope, though.

Garrison: That's an interesting point, but they'd have to be stationary satellites, because my telescope doesn't move very fast. We'd also have to calibrate them.

Guinan: They have been calibrated, several reflective satellites. One of them is military.

Garrison: Hmm, perhaps they would let us photograph it [laughter].

Lockwood: OK, OK, it's almost ten, so how about one more question...Dick.

White: One final comment. In doing these problems well, it is vital to have dedicated instruments that don't do anything else. It's the kind of approach that was behind the design of SSS here at Lowell. Nobody tampers with it, they don't take it apart between runs. That's been the grief with the H and K program at Kitt Peak. We get 4 days a month, and every time we come back to our observing, the instrument has been fiddled with. So I think these ongoing, dedicated programs are the way to go.

Garrison: Amazing. We've been sitting here for an hour and a half praising poor sites [laughter].