Session 2

High Precision Photometry

High-Precision Photometry

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Abstract

The current precision of differential comparisons, within a fixed instrumental system, reaches about a millimagnitude. The current accuracy of transformations between systems is on the order of 0.01 mag, but is worse in some cases. The transformation errors are due to mis-matches between instrumental and standard systems, and cannot be transformed away without further information. A simple geometric model is proposed, which illuminates the transformation problem, and suggests means of minimizing it. Misplaced band edges are the main cause; tighter specifications on filters will help to reduce these errors. The ultimate solution is to use sampled systems in which some bands look like the derivatives of their neighbors. This automatically produces enough band overlap to comply reasonably well with the sampling theorem.

1. Introduction

By ordinary standards, the phrase "high-precision photometry" is an oxymoron. Basic physical quantities like mass, length, and time are routinely measured in *absolute* SI units with an accuracy of a few parts per million with off-the-shelf commercial equipment these days. Even cheap digital watches are accurate to one second per day, or about a part in 10^5. But photometrists are proud to make *relative* measurements with a single instrument agree to 1 part in a thousand, and are hard pressed to make relative measurements with different instruments agree to one per cent — as Sterken and Manfroid (1987), Menzies et al. (1991), and Pel (1991) have shown. In contrast, absolute measurements of length having such accuracy are regularly made by undergraduates using meter sticks.

So perhaps we should not be surprised to hear observational astronomers called second-rate scientists; or that "astrophysical accuracy" is a bad joke even among astrophysicists. Given the meager progress in photometric precision and accuracy in the last 20 years, it is hardly surprising that observatory directors close down photometric telescopes, and regard photometry as an obsolete art to be relegated to the amateurs these days. If we are to overcome this erosion in the status of photometry, we must do substantially better than we have done in the past. Just doing *more* photometry of modest precision is not enough, even if it is useful.

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2. Precision and accuracy

As all the measurements we call "photometry" are relative comparisons between stars, the term "precision" strictly applies to all our work. We rarely refer our observations to SI units, so "accuracy" seems inappropriate. But, as it is useful to distinguish between differential and all-sky photometry, one can regard the discrepancies in differential comparisons of stars made with a single instrument as estimates of *precision*, and the discrepancies in comparisons between different instruments as estimates of *accuracy*. However, as the first Workshop on Improvements to Photometry (Borucki and Young, 1984) concluded, rather than talk about "absolute" or "relative" measurements, we should specify the typical differences in time, position, and spectral functions of the measurements being compared.

The current state of photometric precision and accuracy is well represented in the papers of the first (Borucki and Young, 1984) and second (Borucki, 1988) Workshops on Improvements to Photometry, and in "Precision Photometry" (Philip et al., 1991). The first Workshop dealt with fundamentals. The second concentrated more on hardware, primarily fiber optics, multichannel systems, and silicon detectors. Dave Philip's meeting concentrated on actual results. Together with the papers at our current meeting, these proceedings give an up-to-date picture of the state of our art.

The future of photometric precision is discussed in hopeful terms by Young et al. (1991); there appear to be techniques within reach that should allow some further improvement. Rapid chopping between stars; photometer designs that promote better uniformity of response across the field, together with automatic centering; and improved modelling of extinction and other instrumental variables, should all contribute to better results.

Briefly, one can say that differential comparisons of neighboring stars can be done to about a millimagnitude. This is possible with CCDs, provided that one is very careful about handling the nonuniformities of the chip (Gilliland et al., 1991). This may require keeping the same stars on the same diodes, in addition to very careful flat-fielding. Similar results in night-to-night comparisons of neighboring stars have been done with photomultipliers, and with silicon diodes; and, as laboratory work with PMTs has demonstrated short-term stability of a part in 10^4 or so, and rapid chopping between stars should remove most of the atmospheric transparency variations, there is reason to hope for still better results with larger telescopes.

In the laboratory, silicon diodes seem to be somewhat more stable than photomultipliers, and are the detector of choice if adequate light is available. At the telescope, our signals are weak; giving up photomultipliers means giving up 15 magnitudes or more of electrical sensitivity, and returning to the days of electrometers and DC amplifiers (though now in modern guise), with their long time constants, slow readouts, and linearity problems. I am not sure this tradeoff is wise, except for some special purposes.

Furthermore, commercial laboratory spectrophotometers using photomultipliers have been good to 0.1% — that is, a millimagnitude — for twenty years (Hawes, 1971); and the best laboratory instruments, using 10-second integrations with photo-

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multipliers (very much like ordinary photometric observations) have achieved a level of accuracy of one part in 10^4 (Mielenz et al., 1973), and a precision of a few parts in 10^5. Therefore, even though photomultipliers *can* have problems at the 1% level, I do not think photomultiplier problems are the main obstacle to better photometric accuracy, which is typically 100 times worse than the best that PMTs are capable of.

CCDs are complex devices whose properties are not as well understood as either silicon diodes or PMTs. They have limited dynamic range, and undersample the focal plane. However, their specialized ecological niche is becoming better appreciated: they are an excellent replacement for the photographic plate in crowded fields. But their long readout times and complex data-handling problems make them less than optimal for general photometry.

Still, as millimagnitude or better comparisons have now been done with CCDs, PMTs, and silicon diodes alike, and all have performed better in the laboratory than any has yet done at the telescope, I believe we can say the limits of current detectors have not been reached, and that all of these types are suitable for high-precision photometry. In any case, I do not think that detectors are limiting our precision, nor that we will do substantially better in the immediate future with any new detector. If there are advances to be had on the instrumental front, they must come from areas other than detector technology.

One area that has received considerable attention is the question of nonuniform responsivity across the focal plane, and the concomitant centering errors. There is no doubt that this is a serious problem when inclined photocathodes are used, because the field lens in the photometer transforms positions in the focal plane to angles of arrival at the detector, whose angular response is non-uniform, particularly well off normal incidence. This explains the attention given to fiber optics at the Second Workshop, and is discussed at length by Young et al. (1991).

Charles KenKnight's contribution at the first Workshop, on the extended wings of the star image, is especially important. The wings are due to polishing errors in primary mirrors, and require large focal-plane apertures to secure insensitivity to seeing and guiding variations, quite apart from the perfection of the photometer itself. Large field stops permit very reproducible photometry; guiding and centering must be reproducible within a moderately small fraction of the stop size, but this is entirely possible (especially with automatic control).

As far as the photometer itself is concerned, conventional optics of generous diameters, especially if used to image the pupil on the filters as well as the detector, certainly can give millimagnitude uniformity over some fraction of a minute of arc. If the highest precision is required, moderately efficient scramblers have been designed that provide uniformity of tens of micromagnitudes, but at the cost of a magnitude or so in lost light. Again, I note that sub-millimagnitude precisions have actually been achieved at the telescope. In short, there are several ways to achieve good uniformity even without going to fiber optics, which present serious coupling problems, especially those arising from dust particles and small surface defects.

Thus, "precision" is in relatively good shape — maybe not by comparison with other physical measurements, but at least by comparison with photometric "accuracy". There, we still have unresolved systematic discrepancies of several hundredths of a magnitude. This dismal problem has remained practically unchanged for twenty years or more, and has gradually grown more acute, partly as experience has accumulated, and partly as more severe errors have appeared in work done with non-standard detectors and filters. Systematic discrepancies of several per cent remain between the results of photometric projects that have taken tens of man-years of effort, with good instrumentation, at excellent photometric sites — I remind you again of the studies by Sterken and Manfroid (1987), Menzies et al. (1991), and Pel (1991).

Yet each group had demonstrated *internal* precision of a few tenths of a per cent — roughly an order of magnitude smaller than the disagreements among them. And, as both Wes Lockwood and V. Grossmann report here, even sub-millimagnitude precision can be obtained with a single, conventional instrument. The contrast between internal precision and external accuracy is quite striking.

Recently, Russ Genet suggested to me that the difference between precision and accuracy is that precision is an instrumental problem, but accuracy is an astronomical problem. This is a useful distinction to make. Even so, our best precision is only about an order of magnitude better than our best accuracy, and falls far behind that of (for example) the people who compare masses with mechanical beam balances, where the best precision is now about a part in 10^{11} (Quinn, 1992). Our poor showing, compared to other physical measurements, suggests that, although the best ground-based photometric precision has been limited by scintillation, it may soon be limited by small variations of the effects now limiting our accuracy. The "precision" or instrumental side was recently reviewed in some detail by Young et al. (1991), and is treated further in the session on New Techniques at this Colloquium, so I shall emphasize the "accuracy" or astronomical side of the problem here.

While there is certainly a difference between instruments that are well designed and built and those that are not, my current impression is that the instrumental side of this problem is in much better shape than the astronomical one. The Workshops on Improvements to Photometry (Borucki and Young, 1984; Borucki, 1988) did not turn up any instrumental innovations likely to make order-of-magnitude reductions in photometric errors, though many interesting ideas were discussed.

Instead, I believe that it is the multidimensional property of photometric measurements that is the basic cause of our difficulties. All the physical quantities that can be measured so much more accurately than starlight, such as mass, length, and time, are simple and one-dimensional. But what we are trying to measure is really a (spectral) function, not a single point value.

3. Photometry as spectroscopy

At the Buenos Aires General Assembly, I argued that we do not understand our data well enough to reduce them correctly, and that the real problems are connected with spectral distributions: of response in our instruments, and of irradiance in the starlight we observe. Once we have converted the light to an electrical signal, the electrical measurement is straightforward, and can be made to much higher accuracy than we now reach. But it does little good to have excellent transducers, if we do not know what we are transducing — namely, these spectral distributions.

To understand photometric measurements in detail, we need a detailed and accurate model for measurements of stellar spectra. Unfortunately, the theory we have is relatively crude and undeveloped — and even this theory is often ignored by observers, who resort to purely empirical reduction methods. Yet, as a great scientist once said, "There is nothing more practical than a good theory" (Hampel et al., 1986).

Photometry is a sort of spectroscopy, done at a resolution so low that individual spectral lines are completely lost by blending. Worse, each instrument blends things together in a different way, so that measurements made with different instruments cannot be compared directly. Thus, the first thing we need is a good theoretical model for heterochromatic measurements. Until we have such a theory, we will remain unable to remove instrumental signatures completely from our measurements. I believe this problem is the most serious limitation of photometry today.

However, it seems to be a tractable problem. There already exists a mathematical description (Kolmogorov and Fomin, 1961; Oden, 1979) of the kind of operation we perform when we make a photometric measurement. Unfortunately, it seems to be taught only to mathematicians, not to astronomers and physicists.

4. Transformations

The problem is that while minutely examining the stability of *individual* instruments, we have neglected the reproducibility from one instrument to the next. That is, we have failed to investigate the differences between different instruments that are intended to reproduce the same system.

These differences are often swept under the rug of some empirical "transformation" from one instrument's measures to those of another, using a color index as the only independent variable. These transformations are usually represented as linear by textbook writers, who begin with the unrealistic assumption that stars are black bodies obeying Planck's law (or worse yet, Wien's); apply two or three more crude approximations; and end up "deriving" a linear equation. The worst part of this is that photometrists are led to believe there is something wrong with their data if their transformations are not linear and single-valued.

The careful photometrist, on the other hand, usually finds that a straight line won't quite fit, and is reduced to using piecewise-linear approximations of 2 or 3 parts (or cubics; or worse!). Different relations turn out to be necessary for stars of different luminosity, metallicity, or reddening. However, closer examination (King, 1952; Young, 1992) shows that these transformations are *inherently* non-linear. The latter investigation in fact shows that cubic or even quartic terms must be significant; furthermore, the relationships are necessarily multi-valued, if a single color-index is used as the only independent variable.

The large size of the high-order terms in the classical series expansion is evidence that we have chosen a model ill-suited to the job at hand. Furthermore, current photometric systems fail to capture essential astrophysical information, such as derivatives, that any model needs for accurate transformations. I wish to show that a different approach should lead to simpler — indeed, exactly linear — transformations. However, the price of linearity is to abandon our favorite nonlinear transformation: the logarithmic one from intensities to magnitudes.

5. Functional analysis of photometry

I must admit there is an additional price. One must delve into a fairly obscure corner of mathematics, to pick up the necessary tools. This area, known as functional analysis (Kolmogorov and Fomin, 1961; Oden, 1979), is standard stuff for mathematicians, but rarely encountered by astronomers. I have room here only to sketch the main results we will need.

Functions of the sort we encounter in photometry, such as instrumental spectral response functions and stellar spectra, are quite well-behaved. Their integrals over wavelength exist and are finite, as are the integrals of their squares and products. Such functions belong to a class that can be mapped onto the infinite-dimensional vector space known as Hilbert space (Kolmogorov and Fomin, 1961; Oden, 1979). This is a straightforward extension of ordinary Euclidean space to a countably infinite number of dimensions. One might think of the projection of a function onto the nth axis of Hilbert space as the value of the function at the nth of the denumerable infinity of rational values its argument can assume.

Let us represent an instrumental spectral response function $R(\lambda)$ by a vector in Hilbert space, from the origin to the point corresponding to the function. The squared length of this vector is just

$$\int_0^\infty R^2(\lambda)\, d\lambda.$$

This integral is the analog of summing the squares of the coordinates of a point in a finite-dimensional space to find its Euclidean distance from the origin. It will be convenient to assume the vector is normalized, so that this length is unity.

Similarly, a stellar spectral irradiance $I(\lambda)$ corresponds to a vector. But what we actually measure is not $I(\lambda)$, but the integral

$$L = \int_0^\infty I(\lambda) \cdot R(\lambda)\, d\lambda.$$

This integral is instantly recognised by the mathematicians as the inner product of I and R. Its geometrical representation in Hilbert space is the length of the *projection* of the vector $\mathbf{I}(\lambda)$ onto the normalized vector $\mathbf{R}(\lambda)$.

Now, suppose we have two response functions — say, those for the standard B and V bands. Their vectors span (i.e., determine) a two-dimensional subspace. The **B** and **V** vectors form a *basis* (though not an orthogonal basis) for this subspace. In fact, we can compute the angle, θ, between these vectors. In general, if two functions (i.e., vectors) are $f(\lambda)$ and $g(\lambda)$, the angle θ between them is given by

$$\cos\ \theta = \frac{\int f(\lambda)g(\lambda)\ d\lambda}{[\int f^2(\lambda)\ d\lambda \cdot \int g^2(\lambda)\ d\lambda]^{1/2}},$$

where the integrals all run from 0 to ∞. Because of the small overlap of the B and V response functions, the projection of one on the other is very small — that is, they are nearly orthogonal. The angle between the **B** and **V** vectors in fact turns out to be about 84°.

When we measure a stellar irradiance distribution with this system, we are projecting the vector $\mathbf{I}(\lambda)$ onto the subspace spanned by the **B** and **V** basis vectors. The lengths of the subsequent orthogonal projections of this projection **P** on the basis vectors are in fact the standard measurements. Because a fixed color index corresponds to a fixed ratio of B and V signals, all stars with the same B–V color index have projections that lie in the same direction in the (B, V) subspace.

Stars with different spectra have irradiance vectors with different directions in Hilbert space. If stars were black bodies, their irradiance vectors would define a warped surface in Hilbert space. Then transformations would be easy. But the line features of stellar spectra form rapidly-varying functions of wavelength that are nearly orthogonal to all the Planck functions. Thus, the spectral lines expand the regime of stellar irradiance vectors from a warped surface to a higher-dimensional manifold that encloses the black-body surface. Similarly, the effects of interstellar reddening add new dimensions to the subspace spanned by actual stellar irradiances. It is precisely these deviations of stellar spectra from black bodies that contain the interesting astrophysical information about stars.

If we have an instrumental system, with bands b and v, it will generally define a two-dimensional subspace that differs from the standard one. The projection $\mathbf{P}'$ of **I** onto it will differ somewhat from the projection **P** of **I** onto the (B, V) subspace. Because of the difference between the two subspaces, there is a component $\mathbf{P}_\perp$ of $\mathbf{P}'$ that is orthogonal to the (B, V) subspace; that is, orthogonal to **P**.

Then different stars whose irradiance vectors all lie in a plane through **P** and normal to the (B, V) subspace, all have projections on the (B, V) subspace in the direction of **P**; so these stars must all have the same color in the (B, V) system. But, as their plane is, in general, *not* orthogonal to the (b, v) subspace, their different components along $\mathbf{P}_\perp$ have non-zero projections on (b, v); so they have *different* colors in the instrumental (b, v) system, by amounts proportional to the length of $\mathbf{P}_\perp$. Clearly, the transformation from one system to the other cannot be one-to-one. Accurate transformation is impossible.

Such transformation errors represent the loss of the astrophysical information contained in the $\mathbf{P}_\perp$ component of the stellar irradiances. They necessarily appear in comparisons between instrumental systems whose passbands differ in their ability to capture the astrophysical details of stellar spectra.

The size of the transformation errors is proportional to the length of the perpendicular vector $\mathbf{P}_\perp$. The foot of this perpendicular marks the projection $\mathbf{P}$ of the instrumental vector onto the standard subspace. But that projection is just the least-squares approximation to the instrumental vector by a linear combination of the standard vectors; and the orthogonal component $\mathbf{P}_\perp$ is just the residual vector of the least-squares fit. The smaller the difference between the subspaces, the shorter will be $\mathbf{P}_\perp$, the least-squares residual vector. Ideally, the difference should be zero, so that the orthogonal component vanishes. So to make the transformation between photometric systems one-to-one, their basis vectors must span the *same* subspace.

What is the mathematical condition that ensures this? It is that the basis vectors or functions of one system must be a *linear combination* of the basis vectors or functions of the other. Then the measured *intensities* in one system will be that same linear combination of the intensities in the other! This unique, linear transformation of intensities between the two photometric systems is exact. It is valid for all stars, regardless of metallicity, reddening, and all those other astrophysical phenomena that complicate transformations in existing systems.

In fact, such linear combinations of intensities and response functions were already used by Harold Johnson (1952) in one of the most important papers he ever wrote. Johnson showed that the old International Photographic System and some of its successors could be closely approximated by a linear combination of the (still unnamed) ultraviolet and blue passbands of what later became the UBV system. Perhaps because the U and B bands were not familiar to photometrists at the time, or perhaps because of Johnson's emphasis on a single spectral feature (the Balmer continuum) rather than on general principles, the deeper lesson of this paper was not widely appreciated.

We also have natural additive linear combinations of intensities in the composite light of double stars, clusters, and galaxies; the approach advocated here obviously yields exact transformations for these objects. Furthermore, subtractive linear combinations of intensities have long been used to correct for red leaks. Gilliland et al. (1991) have used additive combinations most recently. So linear combinations of intensities have been around for a long time, but only in a supporting role. We should now move them to center stage.

6. Practical considerations

Given the limitations on real filters and detectors, we cannot guarantee to make one instrument's response functions exact linear combinations of another's. Nevertheless, we can work in that direction, knowing that the better we can approximate this condition, the better we can transform our data. At least, we now know what to aim for.

Geometrically, we must strive to keep the difference between the spaces spanned by different instruments as small as possible, for the transformation errors are proportional to the length of the perpendicular from a point in one subspace to the other subspace. In particular, we want to keep each response vector in an instrumental system as close to its projection on the standard system as possible.

Let an instrumental response function be $r(\lambda)$ and its least-squares approximation in the standard system be $R(\lambda)$. Because the square of the perpendicular distance is

$$P_{\perp}{}^2 = \int_0^{\infty} [r(\lambda) - R(\lambda)]^2 \, d\lambda,$$

we simply want each passband of one system to be accurately approximated, in the least-squares sense, by a linear combination of the passbands in the other system, to minimize this distance. This least-squares criterion is very simple to apply, and useful in guiding the design of photometric passbands.

Clearly, in regions where a response function has a steep edge, it is imperative to maintain the placement of that edge very exactly in the right place; for a small error in wavelength, multiplied by a large slope, means a big difference $r - R$ between the intended and realized response functions; a large contribution to the squared-difference integral; and, consequently, large transformation errors. Unfortunately, it turns out that the exact placement of sharp edges is technically difficult. But, now that we know this is a weak spot, we can concentrate attention on shoring it up.

Notice that it is more important to keep a steep edge in the right place than to maintain the effective wavelength of the passband as a whole in the right place. Therefore, one should worry more about steep-sided filters than about the gentler slopes imposed by variations in spectral response from one detector to another.

If we use absorbing glass filters, as in the UBV system, the sharp-cutoff glasses are colored by heat treatment. The thermal history of each piece of glass determines the edge placement, which can therefore vary substantially within a melt. If one looks at the tolerances in the Schott glass catalog, one finds that the stated wavelength tolerances (typically ± 6 nm) correspond to a variation in glass thickness by a factor of 2 on either side of the nominal thickness.

The obvious step to take here is to measure the spectral transmittance of the actual piece of glass to be used, and have it ground to exactly the thickness needed to put the cutoff where we want it. This is, in fact, a relatively inexpensive operation.

There are also technical problems in placing sharp edges exactly in interference filters. But one can negotiate with manufacturers to try to achieve the best possible placement. As most of the cost of making interference filters is setup and tooling charges, we can obtain somewhat better than "stock" accuracy in band placement for only a modest increase in price — perhaps 15 or 20 per cent. Ask your favorite filter maker what it would cost to cut the usual errors in half.

I have said nothing about extinction corrections. They are simply a transformation between an instrument that contains the yellow atmospheric filter and one that does not. At least in the visible, the atmosphere modifies the shape of our passbands only slightly; the typical angle by which atmospheric reddening rotates the B vector of the UBV system is only about four degrees. Notice that the atmosphere modifies the spectral distribution within the passband, but does not displace its edges.

Here again, we see the importance of edge placement. Thus, the Hilbert-space approach shows why atmospheric extinction is so much more tractable than the general transformation problem.

In the infrared, on the other hand, water-vapor absorption can produce much larger effects. This has led to a proposal for modified passbands for infrared photometry; see the paper by Milone and myself at this meeting for the details. Again the emphasis is on band edges: one should try to keep them gently sloping rather than steep, and far enough from strong telluric absorptions to avoid displacement by the atmosphere.

7. The future

In the design of future photometric systems, we need to keep transformability in mind. Because the astrophysically important spectral features make stellar irradiance vectors span a multidimensional manifold in Hilbert space, we must be able to measure significant parts of this whole manifold if we are to capture the astrophysical information in stellar spectra. As missing astrophysical information is what causes transformation errors, we must try to capture all the information available at our chosen spectral resolution.

Although correlations among spectral features reduce the number of necessary dimensions, photometry clearly requires several passbands to span a significant number of the dimensions in which the subspace of interest extends. The practical consequence of this requirement is that we must design systems so that linear combinations of their bands are easily realizable with actual filters and detectors, allowing for realistic manufacturing variations.

For example, we should avoid steep-sided, nearly rectangular passband profiles, if we have a choice. Instead, we should use smooth functions that look like Gaussians, or cosine-squared profiles. One can design systems with gently-sloping rather than steep passband edges, so that a given error in edge placement contributes less to the mean-square difference integral. Notice that this condition is independent of spectral resolution, and applies to spectrophotometric samples as well as to broadband photometry.

Furthermore, small errors in band placement produce differences from a nominal band profile that look like its derivative. Therefore, we can achieve good linear combinations if some of the bands look like the derivatives of others. In particular, we should try to space bands so that their peaks fall at the steepest parts of neighboring bands.

These prescriptions for band shape and spacing are similar to what I have advocated on other grounds, namely, satisfying the requirements of the sampling theorem (Young, 1974, 1988). Evidently both approaches lead to similar conclusions about the design of photometric systems. This is not surprising, as a central feature of both is to obtain complete, rather than partial, information about stellar spectra at low resolution.

If these requirements are observed in the future, we can look forward to considerable improvements in not only photometric accuracy and reliability, but also in the amount of useful astrophysical information we obtain by multicolor photometry. If they are not, we can anticipate continued transformation problems, and continued degradation of the status of photometry.

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Discussion

R.M. Genet: *What is the best shape for filter band passes? How should filters be spaced?*

Young: You need to have a filter that looks like the *derivative* of each neighbouring pass-band. To avoid introducing higher-order terms in the Fourier transform of the spectrum, a sinusoidal profile is ideal. Then you need to place the neighbouring bands so their peaks fall on the steepest part of the profile of the central band. Thus this vector-space approach leads to the same conclusion as the sampling theorem does.

L.E. Hawkins: *How significant are ambient temperature and polarization effects when doing precision photometry?*

Young: Polarization effects are significant. A good example of ambient temperature effects can be seen in Park's poster paper. It is an easily measurable effect, particularly with sharp cut-off filters. The Geneva photometers have always been temperature controlled for this reason, and failure of the temperature controller is noticeable in the transformations.

D. Crawford: *I have three questions. Many who have developed a photometric system have chosen filters to isolate and to avoid strong features in the spectrum. Your approach seems to imply something different; use many filters and space them to sample the spectrum. Will you comment on these two different approaches?*

What about Hβ or Hα photometry, as such?

It seems to me (as we proposed many years ago) that an excellent photometer would be a well chosen spectrograph with cross-disperser to put the spectrum onto a CCD, filling the chip with all the spectrum to which it is sensitive (sky included). What do you think?

Young: Isolating a feature is fine if your instrumental system never changes, and if no one else tries to reproduce your results. But if you need to transform between two instruments, you need local spectral derivatives. That requires overlapping passbands to measure those local slopes accurately, because there are many stars with strong local spectral features at any wavelength.

On your second question; these are both very strong features. The wide and narrow passbands used represent basis vectors at a large angle, about 70°.

On your third question; spectral dispersion helps define accurate wavelengths, though there is still the problem of relative calibration of the red and blue sides of a passband, Jaap Tinbergen will have more to say about this in his talk later in the Colloquium.

S.B. Howell: *With regard to your comment on non-uniformities in filter glass over large areas, how will this be important for large filters to be used with large format CCD's?*

Young: Laboratory measurements in the optics literature show spatial variation of several percent in a distance of a few millimetres across filters.