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PROPER MOTION SURVEY  
WITH THE  
48-INCH SCHMIDT TELESCOPE

LVI MORE BED-TIME STORIES FROM LICK

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In Nos. XVII and XXXIX of this series I have published criticisms of the Proper Motions in the Hyades region, determined at the Lick Observatory; in No. XLI similar criticisms of the Proper Motions determined relative to Galaxies, and in No. LIII a recapitulation and augmentation of both.

In Lick Publication Vol. XXIII, Part I, a reply to these has been published by B.F. Jones, with an introduction by D. Osterbrock, director of the Lick Observatory.

In the present article I shall deal with this reply taking every paragraph in it seriatim.

This reply begins with an error: I have published scientific papers for more than sixty years, and many of these--including virtually all my papers during the last twenty years--have carried my first name in full, but the director of the Lick Observatory is apparently unable to spell it correctly.

At the end of the first paragraph, Osterbrock states that when he wrote to me (Sept. 1980) I replied "in a long and intemperate letter". What Osterbrock means is that I stated a number of facts--evidently unpalatable--about past actions of the Lick Observatory, to wit:

By early 1966 I had published more than 2500 proper motions for faint stars in the Hyades region, found on nine Palomar Survey plates. In that year the Lick Observatory permitted a graduate student, W.F. Van Altena, to publish in his thesis: "Luyten's survey with the Palomar Schmidt telescope will identify more faint members but the low accuracy of Luyten's proper motions and the very low completeness of his survey result in low weight...." and "this means that Luyten is only 35% complete with respect to my data".... "he could have found all of the stars I found" (*italics mine*). Van Altena further stated that since I had given a value of 0"025 for the mean error of the components of my proper motions, this meant that the errors of my total motions were 0"035!!

In several papers, and especially in LIII - to which Osterbrock is now replying - I have given full documentation to prove that ALL of these statements of Van Altena are false - the last one is something that even a freshman taking his first course in calculus would know better, but it apparently never penetrated to the Lick Observatory.

The Lick Observatory did not send me an advance copy of Van Altena's paper for comment, neither did the Astronomical Journal send it to me for refereeing, nor did the Lick Observatory later offer me space in their publications to reply.

The Lick Observatory did not send me a copy of their present reply in advance of publication, "thus denying me the opportunity to reply in the normal way, in the same series" (which is controlled completely by the Lick Observatory).

Thus Osterbrock's second paragraph, complaining that I denied him the usual scientific amenities, is nothing else but a smokescreen to hide the fact that what he is complaining about has been standard practice at the Lick Observatory for years.

Further gems culled from correspondence received from persons at, or associated with the Lick Observatory are:

When my criticism of the proper motions relative to galaxies appeared Van Altena, then at Yale, wrote me that if I didn't stop that kind of criticism, he would see to it that I got no more funding from NSF.

And, on the same occasion, Vasilevskis wrote me a letter in which he gave a list of some 30 publications dealing with the Lick automatic measuring machine, and ended his letter with "I realize that you may not be interested in all these facts (sic) they are not needed in the fabrication of falsehoods". I repeat what I wrote in LIII: viz. that anyone who has read my critical papers knows that I have always been interested in exposing falsehoods - the fabrication of them can safely be left to the experts at Lick.

All this--and a lot more--was included in my "long" letter to Osterbrock, but he carefully does not mention any of it. And then he really goes to town with as neat a bit of character assassination as I have seen in a long time. Let me mention a few of his remarks addressed to me: "vitriolic", "personal attacks", "privately controlled publications not subject to impersonal refereeing", "...who have incurred Dr. Luyten's

displeasure", "have been excoriated in print", "little astronomical attention will be given to another diatribe", "vilification delivered in its style", "a petty and querulous attack", "his innuendos and highly selected samples".

And while delivering this plethora of choice compliments, Osterbrock pats himself on the back, and states that he is giving an "impersonal" response to my criticism. If Osterbrock wishes to make a spectacle of himself by making all these remarks, I have no objection but why does he then characterize my reply as "intemperate" and without, of course, citing ANY proof or evidence to that effect? Is Osterbrock trying to convince other astronomers that these remarks of his are conciliatory? Little Jack Horner? Or should I say that people who live in glass houses shouldn't squirt hydrofluoric acid around?

Osterbrock glibly talks about my "innuendos" and then himself gives a list of nine persons who have been "excoriated" by me, but carefully omits ALL mention of WHY I criticized these people. Most of that WAS included in my letter to him, but that, At Lick, is not worth mentioning. Since Hanson's and Van Altena's papers on the Hyades will again be dealt with later on in the present article, I shall here take up the case of the other seven.

Lyttleton. In the thirties, Lyttleton produced a new theory for the origin of the solar system, which was later augmented by Hoyle. This visualized the sun as a component of a double star, while a third star approached, and first hit the other component, glanced off, and then hit the sun, which, at that very moment became a nova, and thus gave birth to the planets. Since, by that time, I had become convinced that the origin of the solar system was an "inside job", I took a dim view of Lyttleton and Hoyle's game of Cosmic Billiards, and said so in print. Whereupon I was accused by Hoyle of belonging to those people who in earlier days had put Galileo in jail. My final sentence on this occasion was "When a theory such as Lyttleton's proves to be astrophysically objectionable, as well as dynamically untenable, and is, in addition, superlatively improbable, then I believe we are justified in assuming that it has been removed from the realm of scientific discussion (The Observatory, 63:72, 1940).

Eggen. Rather than go back to my earlier criticisms let me merely point out that, a few years ago, Eggen announced a yellow degenerate star of the tenth magnitude which was nearer than Proxima Centauri--but it turned out to be an ordinary, solar-type main-sequence dwarf with a negligible parallax.

Greenstein. Full documentation of this "distressing" episode (description by the President of Cal Tech) has been given in LII of the present series. Suffice it here to remind astronomers that I have found and published more than 6,000 White Dwarfs--certain, probable, and possible--while Greenstein has NEVER discovered even ONE White Dwarf. But some years ago, Greenstein proudly stated, "...since I have discovered practically all known White Dwarfs..." and habitually assigns his own discovery numbers to all white dwarfs he observes, thus usurping and annexing other people's discoveries.

Oort. In 1975 (Astr. and Astroph., 43:423) Pels, Oort, and Pels-Kluyver announced finding 119 NEW members of the Hyades cluster. I merely pointed out that about half of these were well known before--and that some had been known for more than forty years, and had been found by Oort's teacher, Van Rhijn. In LIII I pointed out that in Palomar region 415, which covers the central region of the Hyades I listed 97 new candidates for membership, found on Palomar plates, whereas Oort's list had 37 stars in this region, but NONE of those were new.

Kopal. I am not, at the moment, able to lay my hands on my alledged "displeasure and excoriation" so I have to skip this.

Giclas. In XXXVIII of this series I showed from an analysis of 1879 proper motions announced by Giclas that his motions were too large, and required negative systematic corrections which amounted to  $-0''03$  for his smallest motions of  $0''27$ , but that this correction increased to  $-0''07$  for motions claimed to be  $0''5$  dropped sharply to almost zero by the time the motion reached  $0''6$  to increase again to  $0''04$  for motions approaching  $1''$ . Thus, by this simple device, Giclas was able to claim that he had discovered 880 new motions larger than  $0''5$  annually, whereas, when making up the LHS catalogue I found that the actual number is only 574. Is this kosher?

Weistrop. In XXXVIII I have given full and detailed documentation of this situation. Briefly, Weistrop determined, and discussed colors and magnitudes for some 6000 faint stars near the North Galactic Pole, and drew a number of far-reaching conclusions from it. Twelve years earlier I had done and published exactly the same thing for 4000 stars near the South Galactic Pole, and this had been done with the very same telescope which Weistrop used, but she carefully ignored this paper and later, in what purported to be a scientific paper, lied about it. Furthermore, some of her B-V colors were too red by nearly  $0^m2$  which, for M dwarfs on the main sequence, makes these stars  $1^m5$  too faint, and that, in turn makes the distance too small by a factor of 2 and the resulting star-density too high by a factor of 8. A whole group of astronomers whom I have called the Messiahs of the Missing Mass climbed on the bandwagon, a plethora of nonsensical papers appeared, culminating in Maarten Schmidt's new luminosity function which demanded that there were still TWENTY-FIVE undetected stars brighter than the twelfth magnitude, and nearer than TWO parsecs. Whoever believes such nonsense?

At this point I may again quote Lord Peter Wimsey's remark in Dorothy Sayers' "Gaudy Night" viz: The point about it is that the only ethical principle which has made Science possible is, that the truth shall be told all the time. If we do not penalize false statements made in error, we open up the way for false statements made by intention.

It was because all the incidents mentioned were so patently in error that I wrote my criticisms, but evidently Osterbrock believes that all these persons show--like Hanson--"quality of research, and precision of judgment" and should, therefore, be above criticism. Or has Osterbrock such a strange understanding of scientific ethics that he considers it perfectly allright for people to plagiarize Luyten's work--but for Luyten to complain about it, why that is TERRIBLE!

Osterbrock makes much of the fact that he has "sought counsel widely" but doesn't seem to understand that in Science there is no room for anonymous opinions. In Science, any opinion given should be based on facts, and the person expressing such an opinion must be willing to stand up and be counted, and should not hide in anonymity. Throughout his introduction Osterbrock suffers from what Disraeli once called a diarrhea of opinions but a constipation of facts.

The Lick Observatory used to send me personal copies of all their publications dealing with astrometry. They are perfectly well aware of the fact that, since my retirement in 1967, I have had no connection whatsoever with the present astronomy department much less even than Lick has with Berkeley, Davis, or San Diego yet from the moment I published my criticism of Hanson's thesis and sent personal copies to both Osterbrock and Vasilevskis (with a brief personal note attached) they have stopped sending me their publications. Not even the "note added in proof" added to Hanson's thesis--in which he claimed to have found virtually all the mistakes which I had previously pointed out, nor of the note which I understand they published after my criticisms of the proper motions measured relative to galaxies, also admitting some of the mistakes I pointed out.

Osterbrock's last paragraph I shall deal with at the end of this article.

We now turn to the actual "reply" by Jones.

In the opening paragraph of Jones' alleged reply he states flatly that "the substance of his criticisms is without foundation". I shall return to this profound pronouncement several times, and at this time shall point out only that, of course, it is very advantageous to state a completely exonerating conclusion right at the beginning of one's article, and thereafter arrange for such arguments as will support this, but ignore anything that would not.

I prefer to do it the proper way: present FACTS first, and draw conclusions later.

The second paragraph states that Van Altena's proper motions in his first paper were measured in a comparator, the least reading of which corresponded to a motion of  $0^{\prime\prime}019$ , which Van Altena took as his error. I am quite well aware of this, since I have repeatedly pointed out that Van Altena originally referred to these motions as having been "measured" but later, when I proved how inaccurate they were, he suddenly called them "estimates". Most hand-operated measuring machines using a screw have a

smallest reading to one micron. Does anyone who uses such measuring machines believe that therefore (as Jones implies) the measuring error in ALL plates measured is one micron?

Van Altena referred to his early proper motions as "measures" in order to justify himself in making his derogatory remarks on the accuracy of my measures. Later, when he called them estimates he even went further and, in a letter to me, referred to them as similar to "heliometer parallaxes".

The next two paragraphs of Jones' article are statements of fact, and as such have nothing to do with my criticisms. But in these, Jones reiterates again that Hanson received the Trumpler award for his work on the Hyades. I had hoped that this item could have been properly laid to rest but since the Lick people continue to point this out, I shall also have to return to it. Since, as I have pointed out before, through circumstances beyond my control, my criticisms appeared BEFORE Hanson's thesis was officially published, the Trumpler award, likewise was not given until long after my criticisms appeared. Is there any doubt in anyone's mind that the award was given BECAUSE of my criticisms, or that the Lick Observatory, must, at least, have condoned or connived at it, even if they did not initiate it? As I have said before, in giving an award named after a great, and accurate astronomer to such an extraordinary collection of gross errors means that the Astronomical Society of the Pacific perpetrated a monumental disservice to Science, and to the memory of Trumpler.

We now come to the Hyades photometry. Jones is quite correct in saying that my statement about the photometric errors of Van Altena really refers to those of Hanson. What happened is that the typist left out a whole line--the one that listed Van Altena's errors--and this was not caught in the proofreading. This is regrettable and I am extremely sorry about it, but fortunately it does NOT affect my statement that the difference Hanson minus Van Altena should be subject to a mean error of  $0^m28$ . With the values correctly given by Jones, viz. 0.16 for Van Altena and 0.23 for Hanson, we obtain for the root-sum-square of these two 0.28 as given.

Further calamities occurred in the actual publication since the printer interchanged pages 3 and 5 and also pages 60 and 62 at the end.

In the next paragraph Jones drags out the old red herring that "Luyten ignores misidentifications that were pointed out by Hanson in his "note added in proof". How many times do I have to point out that Hanson ADMITS that he read my criticisms a month BEFORE he wrote his "note added in proof", but decided not to mention that I had pointed these out first because this was merely a "minor matter". Hence it is clear that Hanson now cannot claim credit for having discovered these misidentifications.

And since we are now on the subject of identifications, how do Osterbrock, Jones and Hanson explain why Hanson in his Table 3 identifies 50 of his stars as having been published by me earlier (including five misidentifications) but omits to mention that there are another 53 stars in his list for which I also published motions before Hanson did.

Jones also does not mention--and what I have pointed out several times before--that Van Altena stated that his plate limit was between 19.5 and 20.0 (blue) but, in his photometry, gave a number of magnitudes down to 21.41 (VA 149)--but Van Altena and Jones do not comment on such trivialities.

When two different persons, working at the same observatory and using the same plates manage to produce enormous differences in their magnitudes, running up to  $2^m5$  in V, as well as several stars which are blue on the Palomar prints but red on the Lick plates (and Hanson does not seem to have noticed this) then I believe one is entitled to point this out and conclude that all is not well with the Lick photometry. But, to be sure, Jones relegates all this to having no substance or foundation.

Jones now reiterates the smallness of Hanson's errors for stars with V between 7 and 14 but carefully does NOT mention the fact that VA 599=RH 497 (which is really my LB 219=LP 415-152) and was announced by me as a faint blue star twenty five years ago) is really a blue object, and dismisses it simply as a 15 th mag star which Van Altena found too blue. Is Jones trying to tell us that Hanson's photometry is extremely accurate down to V-14, but that immediately thereafter it becomes so bad that

Hanson cannot tell the difference between a blue white dwarf, and a star with the color of G5? Or is this, again, "without substance".

Two other stars listed in my Table I, VA 8 = RH 10, and VA 47 = RH 74 as well as a third, VA 46 = RH 72 may be specifically mentioned. For the first two stars we have: VA 11, 8, 12.9 Luyten 11.3, 12.2 g-k, Hanson 14.8, 14.9, and VA 11.8, 12.4, Luyten 11.3, 11.9, 8, Hanson 14.9, 15.0 (Lowell had 12.2 pg) and since these are well within the alleged accuracy limit of Hanson it is strange that here Hanson made them THREE magnitudes too faint, as well as too blue. For the third star we have VA 17.6 18.3, Luyten 17.1, 17.3, f, RH 18.8 20.4 and while this star is outside the "accurate" limits, it is strange that here Hanson suddenly makes this star much too red. If, again this is "without substance", does Jones mean by that that the numbers printed by Van Altena and Hanson in their photometry are without substance?

We may also mention the white dwarf LB 1320=LP 475-70=VA377, which I published as a faint blue star in 1957. Hanson lists this as RH 372, and although this star is only one magnitude fainter than his accurate limit, he manages to give this white dwarf a B-V of +1.0!

Further there is RH 613. Actually there are TWO stars here, separated by 38" in RA. Van Altena listed the preceding one as VA 775, 15.6, 16.9. Hanson lists the following star and, although his position differs from Van Altena's by 1<sup>s</sup>62, identifies his star as VA 775. My automatic machine picked up both stars and gave proper motions of 0"049, 74° and 0"046, 88°, respectively, hence the two stars probably have common motion. On the Palomar plates the two stars look to be of the same color, and I gave them magnitudes of 15.3, 16.4, k and 16.3, 17.4, k, respectively, but Hanson suddenly makes the following star much brighter visually, V=14.7, and fainter in the blue, 17.4, with a B-V of +2.8.

For +16:590 I have given all the facts in LIII. How can Jones shrug off the fact that Hanson gives this star of spectral class G in the H.D. a B-V of -1.73 (or five times bluer than a star of infinite temperature) with the simple statement that Hanson noted that the B-V was "suspect". Why publish such data if one knows they are crazy? Does no one at Lick ever check anything?

For Os 996=RH 616 I have likewise given all the known facts in LIII. A star of the twelfth magnitude with color similar to that of a K dwarf, and possibly a member of the Hyades cluster is given a blue magnitude of 4.72 or 700 times too bright. Again Jones shrugs this off saying that Hanson added that "all data may be suspect". This must be the understatement of the year, even at Lick, or are all these FACTS again dismissed as "of no substance"?

The four stars VA 19=RH 23, VA 108=RH 167, VA 713=RH 569, and VA 771=RH611 all fall within the region of Hanson's "accurate" photometry yet for these four stars the mean difference in V, RH minus VA amounts to -1.23. Is this again "without substance"?

The final four statements given by Jones will now be dealt with. Jones begins by stating "Luyten ignores the fact..." I did not. But when Van Altena states that his plate limit is around 19.7 and then determines blue magnitudes from the Palomar prints down to 21.4 there is something seriously wrong with his photometry.

Jones' second statement: "Luyten misrepresents..." is due to that one sentence having been left out, and as I have shown before, my calculation of 0.<sup>m</sup>28 as the alleged mean error of the differences Hanson minus Van Altena was correct.

Jones' third statement: "Luyten completely ignores..." is, as I have now come to expect from such pronouncements from Lick, completely FALSE. I need not repeat again here what was stated earlier, about the "note added in proof" but Jones continues to ignore the fact--which must be very well known to him--that whatever Hanson wrote in his "note added in proof" was obtained from my PREVIOUSLY PUBLISHED paper, which Hanson admits he saw.

In connection with Jones' fourth statement, I can say again what I said about the Lick motions, viz that it is quite possible that the Lick photometry for a number of the brighter stars may prove to be reliable. But when ALL photometry is given to 0<sup>m</sup>01 while errors up to SEVEN magnitudes occur it would be unsafe to accept any of the Lick photometry at face value--unless verified by others. How does Jones think

he can get away with stating that Hanson's photometry is as accurate as he says while he is ignoring Hanson's Nos 10 and 74 where Hanson is off by 3<sup>m</sup>.

We now turn to the motions.

Jones' opening statement is obviously calculated to give an erroneous impression by stressing that Van Altena's motions were NOT determined with the Lick automatic measuring engine. How many times will I have to stress the fact that Van Altena himself called these motions "measures" and stressed their high accuracy, only, when later on it turned out that they were NOT so accurate, to shrug them off as "estimates" and similar to heliometer parallaxes. Jones' statement "we are certain that most of Luyten's criticisms of this paper could be effectively answered..." in NOT true, and Jones KNOWS this. Even a cursory glance at my Table II in LIII which contains data for 57 Van Altena stars shows that there are many extraordinary discrepancies between VA I and VA II. Let me mention just one: for the sixteen stars VA 23, 39, 109, 110, 136, 161, 336, 556, 585, 596, 631, 662, 703, 744, 767, and 773, the differences in the angles as given in VA I and VA II range from 42° to 140°, and average 111°. Does this belong to the "small amount" of my criticism that Jones CANNOT explain?

In addition, several stars with no measurable motions are given motions of 0"1 annually, while three plate flaws are endowed with very large motions. All these are FACTS which anyone looking at Van Altena's two papers can verify, but as usual, the Lick people prefer their own (favorable) opinions to facts.

In his pages 4 and 5 Jones goes into a great deal of detail in determining the real errors of the published motions of VA II, RH, and myself, and arrives at values of 0"012, 0"006, and 0"017 respectively. He does NOT mention that in XXXIX I had derived values of 0"022, 0"013 and 0"022, respectively. Neither does Jones mention that I derived a value of 0"031 for VA I nor does he mention the fact that Van Altena "determined" a value of 0"035 for the error of my motions--on which he based his derogatory remarks. In deriving his smaller values, Jones rejected all stars brighter than V=9 which Hanson states is the "bright limit" for accurate proper motions. He carefully does NOT mention that for six Hyades common to Hanson and Van Bueren, with a mean V=6.5 the mean errors for their proper motions given by Hanson are 0"0073--barely half of what my value for Hanson's average errors was--the average systematic difference Hanson minus Van Bueren is 0"046, while Van Bueren's error are given as 0"0053 on the average, thus indicating a mean error of these differences of 0"009. For these stars Van Bueren's motions averaged 0"111, while Hanson's averaged 0"0157 yet; in spite of this Hanson wants to push the Hyades further away than Van Bueren. Lest the Lick people--as is their wont--should attempt to "explain" these large differences as entirely due to systematic errors in the meridian motions, one might point out that the meridian motions are based on something like a century's observation, and hence, if they are due to systematic errors, these must be of the order of 5" in the positions. To top this all: the average motion for these six stars as given by Van Altena is 0"122--much closer to the real value than Hanson's. Yet, in spite of such an alarming discrepancy, the citation of Hanson's Trumpler award included the statement: "the long standing discrepancy between ... is now completely resolved".

Jones bases his data for these several errors on 141 stars in the case of Hanson minus Van Altena II. Hanson's list contains some 380 stars in common with Van Altena. What happened to the other 240 comparisons? Yet it is Osterbrock and Jones who have the effrontery to say that my criticisms are based on "highly selected samples".

Since Jones gives as the observed dispersion in the differences Hanson minus VA II 0"0132, this would give for the sum total of the squares of the actual differences for 141 stars a value of around 24500 (using 0"001 as unit). In my table II (LIII), there are given comparisons between Hanson's and Van Altena's motions for ten stars. Using only the total-motion differences we find for the sum of the squares of the differences a value of more than 45000. And even this is not the whole story. For the largest five motions among these ten we have a mean total motion of 0"075 but a root-mean-square of the angle differences of 35°, and for the smaller five these values are 0"042 and 68° respectively. When data are as discordant as these, one should not, of course, take merely the differences between the total motions. A case

in point is the star VA 236 for which the difference in the total motions is only 0"007 but the difference in angle is 129°. In such cases one must take the differences between the components of motion, and if this is done, for the ten stars listed in my Table II, (LIII) and allowing for a systematic difference RH minus VA II of -0"020 in RA, +0"012 in Dec, we find for the total sum of the squares of the differences some 55000 in RA and 12000 in Dec. Hanson's published errors for these motions would add up to only 11500 and 6100, respectively.

Jones' last paragraph on his page 4 is likewise completely wrong, where he states: Luyten seems to be under the impression...". I never stated that Hanson and Van Altena worked closely together. Jones apparently wishes to give the impression himself that Hanson and Van Altena did NOT use the same plate material. Both Van Altena and Hanson gave details about the plates they used, and it is very plain from this that while Hanson had five blue, and three yellow plates, two of his blue plates were the identical ones which Van Altena used.

Using Jones' phraseology, therefore, it is plain that his attempt to show how accurate Hanson's and Van Altena's proper motions are is totally without substance.

Now we get to the real alibis. Jones states that "some misidentifications were inevitable", and "Van Altena measured grain clumps--these were not plate flaws". When a "grain clump" is mistaken for a star, endowed with a magnitude, a color and a sizeable proper motion I call it a plate flaw and I believe most real astronomers will agree with me. Jones further states "Thus the explanation about VA 37=RH 47 noted as such by Hanson in his paper".

THIS IS NOT TRUE.

What Hanson DID write in his paper is: "additionally, the use of five epochs rather than two provided more reliable (sic) proper motion data since spurious measurements (Luyten 1967, 1969) can be detected". Hanson then proceeded to give new proper motions for these same plate flaws. Hanson wrote his "note added in proof" AFTER he admitted he had seen my criticisms which had pointed all this out.

Exactly the same applies to Hanson's "explanations" and Jones' misstatements of facts about VA 422 and VA 451, mentioned by Jones on his page 5.

Even these deliberate misstatements of facts are not enough for Jones. In his very last statement in this section he writes "Hanson's measurement of additional epochs as showed that a few of Van Altena's faint stars were, in fact grain clumps. They were not plate flaws, a term used only by Luyten. By thus confirming the discrepancies previously pointed out by Luyten, Hanson did, as he stated, improve the accuracy of the proper motions" (underlining mine). The actually published values for the proper motion of this object are:

Van Altena	+709 ± 019	-426 ± 019
Hanson	+232 ± .135	+113 ± .151

Even if this object had been a real star, and not a plate flaw, would anyone say that the proper motion had been "improved" when the mean errors had increased by factors of seven and eight times? But, since the object in question does not exist how can one even think of improving the proper motion? This is the most arrant nonsense I ever read. May we now soon expect a publication entitled "Improved Lick Astrometry of Non-Existent Objects"?

Hanson knows perfectly well that what I have stated here is correct.

The dictionary defines a LIE as "A falsehood told with intent to deceive".

In this section Jones mentions two further stars, VA 307 and VA 670. I did not object to the large error for VA 307, as given by Hanson. I merely pointed out that it is meaningless to publish a value of  $+107 \pm .248$  for the motion of any star, and further that such errors imply errors in position of more than 100 microns. The fact that Hanson publishes such data without comment justifies my statement that at Lick whatever the computer spews out is taken as gospel truth, and IS NEVER CHECKED.

VA 670=RH 527 is really Os 775 though Hanson does not bother to mention this, but gives its motion with ZERO errors. Such errors are simply boloney, and not worth discussing any further.



I may add one more star myself, LP 475-50=VA 314=RH 315 and again Hanson does not bother to mention my designation. For this star I published a motion of  $0^{\circ}024$  in  $100^{\circ}$  in my Hyades book. Van Altena "estimated"  $0^{\circ}083$  in  $91^{\circ}$  but Hanson published  $+^{\circ}182$   $+^{\circ}022$ ,  $+^{\circ}006$  +  $+^{\circ}018$ , the difference in RA motion being  $4\frac{1}{2}$  times his mean error--but gives no comment.

We now come to the last section in which Jones attempts to answer five "claims" I made.

1. Hanson's proper motions differ from those of Van Altena for five stars. I presume that Jones here refers to the five (now six) Van Bueren stars. These I have discussed before, and Jones alibi that these stars are brighter than Hanson's limit for accuracy is completely meaningless in view of the fact that Hanson himself published mean errors for his motions which, for these stars averaged only  $0^{\circ}0073$ , whereas the differences involved were six times larger.

2. The five faint stars. I presume Jones means the five stars mentioned in section 5 of my paper XXXIX. The first star, which is LP 414-115 instead of 415-115 as misprinted in my paper, was correctly identified by Hanson with his RH 57. The motion I gave in my Hyades book was  $0^{\circ}17$  in  $113^{\circ}$ , or  $+^{\circ}16$ ,  $-^{\circ}07$ . Hanson gives  $+^{\circ}154$ ,  $-^{\circ}150$ , but with the usual Lick supremacy, Jones considers that my values constitute no evidence that Hanson could be wrong. Well, from Graham Hill's plates, with an interval of 24 years, and yielding proper motions with average mean errors of  $^{\circ}007$  I now find  $+^{\circ}118$   $-^{\circ}086$ . Anything further, Mr. Jones?

The second star, LP 474-280 was misidentified by Hanson as RH 177 which has no motion, but it should have been RH 176. The fourth star LP 475-80 was likewise misidentified by Hanson as RH 402, but it should have been RH 409. The third star, LP 415-875 is identified by Hanson with his RH 380 whose position differs by  $13^{\circ}$  and  $2^{\circ}2$  from mine, so this is almost certainly another misidentification. The last star, LP 415-113 was again misidentified by Hanson with his RH 416 which has no motion. Thus we find, among these five stars one Hanson motion that is badly off and four misidentifications but evidently, according to Jones this is all my fault and no blame attaches to Hanson!

3. Jones' next point mentions 25 stars for which I found a decided difference between Hanson's and Van Altena's motion. Since Jones does not specify which section of paper XXXIX or LIII this refers to, I cannot reply to this, but would only point out that Jones states flatly "There are 377 stars in common" between Van Altena and Hanson--I find some 380, and further "we attach little significance...". This again is the same story: any criticism of Lick results is without substance. Very simple isn't it?

4. Jones' statement here probably refers to the four stars listed in section 2 of my paper XXXIX. I said that of these four stars called blue by Van Altena I found only one bluish and that Hanson's mean B-V for the other 3 was +1.52. Since Jones calls this a misstatement of facts does he mean that when one person calls three stars blue and another gives as his color B-V = +1.53 that this constitutes good agreement? Anyone looking carefully at the data I gave in my section 2, paper XXXIX will agree with my statement that Van Altena's and Hanson's data are incompatible. Jones also, and again, claims an alibi by mentioning "misidentifications" knowing full well that if there were any, Hanson found these out from my paper, which he admits seeing before he wrote his note.

Jones' point 5 I have dealt with before, under plate flaws.

Summing up the entire Hyades business I state again that I have proved conclusively that:

1. For six Van Bueren stars, with published errors averaging  $0^{\circ}0053$  in their motions, Hanson published motions--with mean errors of  $0^{\circ}0073$ --which were  $0^{\circ}046$  or 42 percent too large.

2. There are a number of cases where Van Altena claims stars are blue, when Hanson finds them red.

3. Hanson calculates mean errors for all his motions but publishes such completely meaningless data as  $0^{\circ}17$  +  $^{\circ}121$   $^{\circ}107$  +  $^{\circ}248$ , and  $^{\circ}113$  +  $^{\circ}151$  as well as a number of errors of  $0^{\circ}000$ .

4. According to Jones, Hanson determined "improved proper motions for clumps of grain", knowing full well that these were non-existent.  
Is this further proof of "the quality of his research and the precision of his judgment"?

My criticisms have been directed at the results published by the Lick Observatory--whether these are due to malfunctioning of the automatic machine, or to the sloppiness of the operators, or to both is immaterial.

We now turn to the proper motions determined relative to galaxies.

Jones starts off his reply to my criticisms by dragging out the same old red herring that "much of Luyten's criticism...revolves around Lick Region 29" and then dismisses it all by saying that this was a region of zero weight, and that the problem for this region "is in the reductions, not in the measurements" (underlining mine). Now in my measurements of proper motions on the Palomar plates I have always determined relative proper motions, of course. These were relative to stars of the seventeenth magnitude, approximately and, if I wanted to render the motions absolute I tried to estimate the corrections from relative to absolute motion. For Lick area 29 I estimated these to be about + $0^{\circ}003$  in RA and - $0^{\circ}002$  in Dec. While these corrections are not now known accurately, I do not believe that, at the outside, my values could be in error by more than, say  $0^{\circ}005$ . Jones must know this as well as I do. Now the Lick motions are given, to  $0^{\circ}0001$ , a completely meaningless accuracy. And on this plate, Lick region 29 ~~there~~ are listed SIX objects within one square degree with motions between  $0^{\circ}2000$  and  $0^{\circ}4000$ , all going the wrong way as far as reflex of solar motion is concerned, and these objects do not exist.

Hence Jones' statement that in this region the problem is due to the reductions and NOT to the measures, is completely false, and he KNOWS it. So does Osterbrock yet Osterbrock has the nerve to say that "Jones sets out the facts quite well". It is evident that the Lick Observatory has now reached the second and last stage in Lord Peter Wimsey's comment.

Jones goes even further and states that since the reader has now been properly warned, that the proper motions could easily be corrected if a reliable reference frame became available. How corrections of at most  $0^{\circ}0050$  can correct erroneous motions of  $0^{\circ}4000$  is something only Lick can achieve.

Jones likewise does not comment on my table listing 31 B D stars, (XLI) and showing the systematic differences between the Lick motions and the meridian motions. IN this region where one would expect an average motion slightly positive in RA and slightly negative in Dec the mean Lick motions are - $0^{\circ}0049$  and - $0^{\circ}0303$ , whereas the mean meridian motion--which, according to Lick must be affected by serious systematic errors, comes out as + $0^{\circ}0017$  - $0^{\circ}0005$ .

Jones likewise does not comment on what I pointed out in XLI that, for the 105 stars listed by Lick in this region, the average motions are nearly the same for all stars, from brightest stars to faintest, and that, if any the average motion of the faintest stars is the largest--and the wrong way. Again, if the maximum errors that can have been introduced by the reductions are of the order of  $0^{\circ}005$  how does Lick explain the existence of systematic errors SEVEN times larger?

Next we turn to the two regions 231 (11:12, +50) and 707 (12:00 +15) both of which are near the North Galactic Pole, and thus should have plenty of galaxies available, and one would expect the motions there to be predominantly negative in RA and likewise but smaller, in Dec. As I pointed out, in area 231 there are 44 + and 51 - motions in RA while actually the mean motion of 46 stars of magnitude 15.9 is slightly positive in RA. In region 707 sixteen of the brightest stars go almost straight south. Is not all this a clear indication of systematic error?

Jones now admits that the proper motions of the bright stars are affected by a systematic magnitude error, yet when I spoke of systematic errors in the Lick motions he calls my criticism "obscure".

On page 10 Jones writes "Luyten claims there are eight stars in the Pilot Program...which do not move at all". He now admits that two of these are Lick mistakes, but attempts to shrug off the other six by saying that "they were discussed above". What Jones means is that he falsely claimed that these six completely spurious motions were due to the reductions, and NOT to the measurements.

Finally we come to the Solar Motions. I am here reproducing data from their Tables I and III, and it should be added that the numbers of stars in the two bright star groups are of the order of 1000, while those in the four faint star groups are of the order of more than 3000. It is further seen that the increase in the declination of the solar apex as one moves from "slight" to "severe" cut-off (of large proper motions, i.e. of high velocity stars) is as follows:

Solar Apex from Lick Table I												
	Bright				Medium				Faint			
slight cut-off	278°±	6	40°±	4	288°±	5	44°±	3	292°±	7	48°±	4
moderate cut-off	272	7	47	4	291	4	45	3	296	6	50	4
severe cut-off	270	10	55	5	293	5	53	3	297	6	52	4
from Table III												
slight cut-off	273°±	7	45°±	4	285°±	4	48°±	2	292°±	8	53°±	3
moderate cut-off	272	7	49	4	289	4	49	2	293	8	54	3
severe cut-off	263	8	57	4	290	6	53	3	294	6	56	3
Increase in Declination of Apex when going from slight to severe cut-off												
Bright Stars	+15° ± 7°			and	+12° ± 6°							
Medium	+ 9 4				+ 5 4							
Faint	+ 4 6				+ 3 4							

In all six cases the increase in declination is continuous, without a single reversal yet Jones shrugs this off by saying that the errors are large and that the positions are nearly the same within the errors.

It is amazing that those who use the Lick Automatic Measuring Engine (LAME) invariably give their results to too many decimals. Hanson gives his positions to 0<sup>s</sup>01 and 0<sup>m</sup>1 which comes to about 0.002 mm on his plates; in the pilot program of proper motions measured relative to galaxies positions are given to 0.0001 mm--but since this is on plates with a scale no one else has, it is virtually impossible to identify these stars even on the Palomar plates--Van Altena and Hanson give all their photometry to 0<sup>m</sup>01 but errors of a whole magnitude are common, and these run up to seven magnitudes. Proper motions in the Pilot Program are given to 0<sup>"</sup>0001 but errors run up to 0<sup>"</sup>4000, yet when such gross errors are pointed out the Lick people suddenly take refuge behind the fact that in this particular case the errors are very large.

Jones terminates his paper with a list of other pieces of research done with the Lick automatic machine and gives a catalogue of 43 references. My criticisms were directed at three papers on the Hyades, and two on Proper Motions relative to Galaxies. To mention other programs, therefore is, in the words of Perry Mason, that fictional court-room lawyer of Earl Stanley Gardner, "incompetent, irrelevant and immaterial".

On the other hand, Jones does NOT mention two other papers, viz "On the Distance of the Hyades Cluster" by Harold McAlister (A J 82:487, 1977) which paper concludes that there exists a well-defined magnitude equation in Hansons proper motions in RA, and "A Rediscussion of Determinations of Precession and Galactic Rotation from Lick Proper Motions referred to Galaxies" by B du Mont (Astr and Astroph 66:441, 1978) which paper also finds systematic trends in these proper motions--and this second paper was even discussed in my paper LIII.

Osterbrock deplores the fact that a young astronomer at the beginning of his career is severely criticized. The Lick Observatory would have served this young astronomer better if they had assigned him to a principal adviser who knew something about astrometry, and who could have pointed out to Hanson the pitfalls of blindly trusting computerized measurements without checking them.

Summarizing, I have criticized--giving full documentation--three papers on the Hyades and two on Proper Motions relative to Galaxies. In the reply now issued by the Lick Observatory, its director:

1. indulges in derogatory remarks about my writings without, of course, giving ANY examples or proofs.
  2. has, in the past, at least condoned the giving of an award to one of those who used the Lick automatic machine, and who produced a thesis with a plethora of gross errors, and has acquiesced in the characterization of "the quality of research, and the precision of judgment" of this person.
  3. has at least acquiesced in the person writing the technical answer to my criticisms shrugging off many of the large discrepancies I pointed out as "misidentifications"--all of which were done at Lick.
  4. has condoned, and even praised--by calling it a "measured reply"--the answer given to my criticisms which contains several false statements of facts which both Osterbrock and Jones knew to be false.
- It is sad to see a once-great observatory descending to this level of printing falsehoods merely to avoid having to admit they made some errors.