

Re: Dayton Miller's Data have no Real Signal

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- *From:* demeo@xxxxxxxx
 - *Date:* 9 Dec 2005 01:20:32 -0800
-

Tom Roberts wrote:

- > Title: Dayton Miller's Data have no Real Signal
- > Author: Tom Roberts
- > Date: December 8, 2005

Dear Tom Roberts,

Since I am the fellow who did the archive work which brought Miller's Mt. Wilson data sheets to the light of day, stimulating a search at the Physics Department at Case Western Reserve University where they were finally located after being submerged for decades, and since I did so much of the preliminary work rehabilitating Miller, probably I could spot flaws in your argument where others might not.

Firstly, you can review my papers on the Miller ether-drift subject here:

"Dayton Miller's Ether-Drift Experiments: A Fresh Look"
<http://www.orgonelab.org/miller.htm>

"A Dynamic and Substantive Cosmological Ether"
(from Galilean Electrodynamics)
<http://www.orgonelab.org/DynamicEther.pdf>

As to the specifics of your paper:

I note you cite Miller's 1933 paper, but nothing else aside from the hit-piece authored by Shankland et al. There are dozens of papers which bear upon these issues, including the positive replication undertaken by Michelson using Miller's methodology on Mt. Wilson (the Michelson-Pease-Pearson experiment), and the work of Yuri Galaev, among others. Galaev's ether-drift experiments used both visible light and radiowaves, and "confirmed Miller down to the details". And from there, as I show, the sidereal-hour variations in Miller's determinations match very precisely to Bernabei's determinations on seasonal variations in "dark matter wind" — another word for ether-drift, in my view. So only from a superficial knowledge of this issue, it appears there are quite a few scientists making nearly

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identical "systematic errors". It is one thing to claim, a guy with a compass in his shaking hand can hardly tell where the needle is pointing, but if he and a half-dozen others all point to the same general location, in spite of shaking hands, it might pay to do more than simply dismiss the issue. But there's other good reason to dismiss your arguments, and retain clarity about Miller's work.

You evaluated Miller's August 1927 data set, but this is hardly mentioned in his 1933 paper which you cited, and which is among his most important ones on the subject. The 1933 paper covered a short history of the ether-drift determinations, but primarily focused upon his significant 1925–1926 experiments undertaken atop Mt. Wilson. The Mt. Wilson experiments are what you should be discussing, not the insignificant tests in Cleveland either before or after Mt. Wilson. You proclaim, without evidence firstly that the direction of ether-drift and velocity determinations were "not significantly better than any other" direction or velocity — this might be true for the 1927 data you examined. I have not seen it so cannot say. But it is most definitely NOT the case for the 1925 and 1926 Mt. Wilson data, which is what is presented in Miller's 1933 paper.

Those Cleveland experiments undertaken by Miller prior to Mt. Wilson often were calibration tests, or experiments to evaluate different materials for the base and composition of the interferometer, under different thermal environments, and so on. It seems probable, the data set you analyzed was another of those calibration tests. Again, the graphs appearing in Miller's 1933 paper were composed from his Mt. Wilson experiments of 1925 and 1926, not from any data from Cleveland in August 1927. Perhaps you can give a citation as to where you noted some kind of graphs for the August 1927 data? I cannot recall any such publication of Miller's post-Mt. Wilson data. After Mt. Wilson, he was finished with the problem, felt he had nailed it down. Is it possible, you have data sets from some of his students? I merely ask, because there is no relevance to the data you undertook to re-analyze.

Shankland, et al, did their best to bury Miller's work forever. They failed, as their approach was sloppy and showed an ignorance of how the ether-drift experiments were undertaken. Both they and you ignored the central issue of the needs for doing these experiments over different times of year. Yes, you can point to one seasonal epoch and try to argue that the systematic pattern in Miller's data is due to this or that. Shankland dismissed the patterns as due to "temperature", but without any proof as such. You say it is some kind of systematic error. But firstly you don't look at Miller's most important data sets, from Mt. Wilson. Even Shankland at least reviewed the correct data sets, though he "cherry picked" only those data sheets by which he could compose a verbal argument. Secondly, and more importantly, neither the Shankland critique, nor your critique, addressed the **SYSTEMATIC SIDEREAL-DAY VARIATION IN THE AXIS OF ETHER-DRIFT, APPARENT DURING ALL FOUR SEASONAL EPOCHS**. The pattern was systematic, as Miller noted repeatedly, as I show in my papers on Miller as well. When the

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data are organized by civil-clock time, no pattern exists. When organized by sidereal-clock (galactic) time, the pattern appears, and is the same for all four epochs. There's simply no way you can use math-arguments to overthrow such a pattern, especially since it has already been confirmed by others.

The Cleveland experiments, pre-Mt. Wilson or post-Mt. Wilson, are not significant for the question of ether-drift on a number of counts. Firstly, as mentioned, they were mostly calibration experiments -- can you cite a publication somewhere which reports on those post-Mt. Wilson experiments in Cleveland? Are you sure you do not have, by some accident, the data sheets from 1927 of Michelson-Pease-Pearson? (If so, I'd like to get a copy of them!) I would ask, where did you get your set of Miller data sheets? The only ones I know about include the full sets from Mt. Wilson, so it seems strange to me that you'd get only the 1927 data sheets, but not the others from 1925 and 1926 -- I searched both the Miller and Shankland Archives, plus materials from Einstein's archives in Jerusalem, and found only one or two of his data sheets. An unpublished interview with Shankland suggested he (Shankland) had burned them. With that alarming possibility, I pushed one of the Case Physics professors to undertake a search in some old rooms where Miller's stuff had been stored for years, collecting dust -- and he found them, turning them over to the Case Archive department. I had copies made of the full data sets -- over 1000 pages. So how did you get possession of only the 1927 data?

As Miller wrote in his 1933 paper (p.228):

"More than half of [my 200,000] readings were made in the Mount Wilson observations of 1925 and 1926. ... 12,800 single measures of the velocity of ether-drift and 25,600 single determinations of the apex of this motion."

These were in April, August and September of 1925, and February of 1926. NOT in August of 1927.

There is a reason why the Mt. Wilson experiments yielded good positive results, and nearly all other such experiments did not -- it is because the altitude of the experiment above sea-level influences the result, due to Earth-entrainment (slowing down) of the Ether. I discuss this fully in my papers, and so won't repeat it here. The original Michelson-Morely experiment, the Morley-Miller experiments and the Miller experiments outside of Mt. Wilson were all performed in low-altitude Cleveland, often inside the stone structure of the Case Physics building. The MM experiment also took place in a basement location, and Miller demonstrated that only by going to higher altitudes and also removing all dense materials at the light-beam path -- the metal or heavy wood covers as used by so many others -- would the ether-drift show itself more easily. Miller did the high-altitude experiments and got a positive result. Same with Michelson-Pease-Pearson, and a few others. Galaev more recently

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confirmed it. How long will modern physics refuse to look at this issue with open eyes and intelligent, fair-minded critique? Sorry to say, Tom, your analysis is faulty on a number of levels, and does not touch Miller's findings and conclusions anymore than the Shankland hit-article did. It is a pity you did not consult with the advocates of ether-drift prior to undertaking your analysis, as it could have saved you a lot of time, and perhaps guided you to analyze the proper set of data, from Mt. Wilson. But I still don't see how your method can do more than point out the obvious, that the signal is often buried in the noise. Lots of scientific problems suffer from this difficulty, but progress nevertheless towards deeper understandings.

There is more which could be said, but it is late, and I'm sure you'll have a reply to what's here already. I suggest firstly to review my papers on the subject, so I won't necessarily be repeating myself.

Regards,

James DeMeo

• *Follow-Ups:*

- ◆ ***Re: Dayton Miller's Data have no Real Signal***
◇ *From:* Ken S. Tucker
- ◆ ***Re: Dayton Miller's Data have no Real Signal***
◇ *From:* Tom Roberts
- ◆ ***Re: Dayton Miller's Data have no Real Signal***
◇ *From:* Dirk Van de moortel
- ◆ ***Re: Dayton Miller's Data have no Real Signal***
◇ *From:* Dirk Van de moortel

• *References:*

- ◆ ***Dayton Miller's Data have no Real Signal***
◇ *From:* Tom Roberts

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