Dr. Bridgeman,

Since you have locked out Mr. Delano from responding to your last two posts (posts of Saturday, March 5 and Saturday, February 26) he has asked me to respond to you. Hopefully you will not resort to blocking me also, but just in case, my response will be posted to the *Galileo Was Wrong* website at www.galileowaswrong.com.

Now, let's take a look at your critique of Drs. Hartnett and Hirano's paper.

I will comment intermittently.

Robert Sungenis

Tom Bridgman: **Saturday, March 5, 2011**

Quantized Redshifts. VIII. An Uncommon Power Spectral Density "Blooper"

One of the most bizarre errors I've encountered comes from Hartnett & Hirano (2007), hereafter referenced as HH2007 (Galaxy redshift abundance periodicity from Fourier analysis of number counts $N(z)$ using SDSS and 2dF GRS galaxy surveys). When I was first attempting to analyze this paper and duplicate the reasoning and techniques, I was having some strange discrepancies in the analysis that I didn't understand (see John Hartnett's Cosmos. 1. Introduction, John Hartnett's Cosmos. 2. Methodologies). At that time, it had been many years since my last regular use of the PSD and my skills were a bit rusty.

But thanks to the more focused analysis inspired by my geocentrist friends, I've revisited much of the introductory Fourier material and could re-examine HH2007 at a more fundamental level. I discovered the source of my confusion.

Some of the Common Bloopers

HH2207 commits one of the errors described in the previous article in this series (see Quantized Redshifts. VII. Common Power Spectral Density "Bloopers").

HH2007 attempts to split the PSD into a 'survey function' (Figure 6) and a structure signal, failing to validate that these two components would be properly uncorrelated in the source data, as required in the definition of the PSD. HH2007 does not even provide the polynomial fit used to define the 'survey function'!

A Surprising Re-interpretation of the PSD

HH2007 defines the discrete Fourier transform in a somewhat confusing notation:

$$\nu_s = \frac{1}{\sqrt{n}} \sum_{i=1}^{n} u_s e^{2\pi j(i-1)(s-1)/n}$$

where \nu (a symbol usually used to designate frequency) is the amplitude of frequency bin, s; n = number of data bins of galaxy counts; i is the data (redshift) bin number; and s = bin number of frequency bins. Note that the data amplitude, u_s = delta N_i (sic) for some strange reason, where deltaN_i is the number of galaxies in bin i. HH2007 uses 'j' to designate the imaginary unit (a common convention in engineering). While somewhat notationally odd, it is essentially correct.

From this definition, the properly defined 'fourier frequency', as outlined earlier in this series (Quantized Redshifts. IV. The Fourier Transform, FFT & PSD), would be:

$$f_s = \frac{s}{n\delta z}$$

Note the behavior of the fourier frequency in relation to the source data. If the data axis is measured in seconds, the fourier frequency is in inverse seconds (per second, or Hz). For spatial measures of meters, the Fourier frequency becomes inverse meters, or wave number (number of waves per meter). In cosmological redshifts analysis, z, is a proxy for distances, with x = z*c/H0, where H0 is the Hubble constant. For a long time, H0 was so uncertain, it was often written H0= h*100 km/s/Mpc where h was between 0.0 and 1.0.   Therefore distances, x, become

x = z*c/(100*h) Mpc

This made it easier to explore a range of possible values for H0 by varying h. This choice meant that the power spectra of a cosmological dataset expressed the wave number, k, in units of h/Mpc.

When the FFT is computed, the arrays are conventionally generated from low frequency to high, based on this standard definition of fourier frequency (see Quantized Redshifts. IV. The Fourier Transform, FFT & PSD). Therefore, the first few bins should correspond to the Fourier frequencies in the same bins. Using HH2007 that delta-z=1e-3, I will assume n=512, based on other evidence in the article, but HH2007 does not specify this important quantity in the article.

bin number (s)	**frequency**
0	0.0
1	1.953125
2	3.90625

3	5.859375
4	7.8125
5	9.765625
6	11.71875
7	13.671875
8	15.625
9	17.578125
...	...
250	488.28125
251	490.234375
252	492.1875
253	494.140625
254	496.09375
255	498.046875

But HH2007 does something strange. HH2007 INVERTS the definition of the frequency for each bin number, s.

$$F_s = \frac{n\delta z}{s} = \Delta z_s$$

On seeing this definition, my initial focus had been on the physically nonsensical nature of the use delta-z in this context. While z is in principle dimensionless, it is still a proxy for a distance scale and the units. This definition is particularly troubling as the proper scaling to wave number, the unit corresponds to 2*pi/wavelength. The use in HH2007 defines delta-z that corresponds physically to about 1/6 (1/(2*pi)) of a wavelength! One could imagine delta-z corresponding to integral multiples of wavelengths, but irrational fractional multiples of the wavelength? That's just weird!

R. Sungenis: Not weird at all. Equation 2 of (Hartnett & Hirano, Ap&SS 318, 13-24, 2008) which you call HH2007 has a typographical error. His corrected paper now reads as follows:

"Within the i^{th} bin there are ΔN_i galaxies. Discrete Fourier amplitudes ν_s are generated for each integer s > 1 from

$$\nu_s = \frac{1}{\sqrt{n}} \sum_{i=1}^{n} u_s e^{2\pi j(i-1)(s-1)/n}$$

where $u_s = \Delta N_i$, $j = \sqrt{-1}$, n is the total number of ΔN_i bins. Because ν_s are complex their absolute value is taken in the analysis. The Fourier frequencies are calculated from

$$F_s = \frac{s}{n\delta z} = \frac{1}{\Delta z}$$

where Δz is the redshift interval of any periodic spatial separation between galaxies. This is taken from "Fourier Analysis of the Large Scale Spatial Distribution of Galaxies in the Universe" by John G. Hartnett, Second Crisis in Cosmology conference, CCC-2, ASP Conference Series, Vol. 413, 2009, which is available at:

http://www.aspbooks.org/a/volumes/table_of_contents/?book_id=463

Once again, the correct equation (2):

$$F_s = \frac{s}{n\delta z} = \frac{1}{\Delta z}$$

was used throughout HH2007 in the actual analysis, even though the originally published paper had a typographical error that inverted two terms. None of the results were affected.

Bridgman: But it gets stranger still. The HH2007 Fourier frequency is the inverse of the standard definition of Fourier frequency. This means that for each bin, s, HH2007 definitions of frequencies become

bin number (s)	Frequency	delta-z
1	1.953125	0.512
2	3.90625	0.256
3	5.859375	0.170666666667
4	7.8125	0.128
5	9.765625	0.1024
6	11.71875	0.0853333333333
7	13.671875	0.0731428571429
8	15.625	0.064
9	17.578125	0.0568888888889
...	...	...

250	488.28125	0.002048	
251	490.234375	0.00203984063745	
252	492.1875	0.00203174603175	
253	494.140625	0.00202371541502	
254	496.09375	0.0020157480315	0020157480315
255	498.046875	0.00200784313725	

Note this table compared to HH2007 graphics. Examination of the graph suggest the low frequency power is on the left side of the graph, corresponding to the low bin numbers, s. The low bin numbers correspond to LOW fourier frequencies but LARGE delta-z's based on HH2007's definition. Yet the plots such as Figures 5, 7, & 9 in HH2007 have LOW delta-z's at low frequency bins. It appears that HH2007 has flipped the x-axis scale on these plots!

This means that, by the definitions of deltaz in HH2007, all the peaks identified in this paper are WRONG!

R. Sungenis: No, it is only because (Hartnett & Hirano, 2008) changed the x-axis from 1/Delta z [1/Δz] to Delta z [Δz], apparently to make the peaks easier to identify. The change to Delta z [Δz] does not add peaks that weren't there originally. In the end, (Hartnett & Hirano, 2008)'s labeling on the x-axis of plotted Delta z values shows the Delta z periods conclusively.

Bridgman: An examination of some of Hartnett's later redshift periodicity papers (Fourier Analysis of the Large Scale Spatial Distribution of Galaxies in the Universe and Unknown selection effect simulates redshift periodicity in quasar number counts from Sloan Digital Sky Survey) suggests this error is still being made.

R. Sungenis: Since there was no error in the final analysis of the HH2007 paper, then no error is being perpetuated unchecked in Hartnett's other papers. The only error made here is Tom Bridgman's error of presuming that Hartnett made an error in this and other papers.

Bridgman: The errors described in these last two posts describe fundamental errors in how the power spectral density works or is interpreted. If papers with these types of errors had been turned in as part of a mathematical methods course (a standard class in most physics and astronomy graduate program curricula), the papers almost certainly would have received a failing grade. It's bad enough that the researchers who made these types of errors did not research their analysis tools sufficiently to understand they were in error. But how did this type of error make it through the journal's peer-review process?

I'd like to thank Rick DeLano of Galileo was Wrong. His persistence inspired me to dedicate more energy to completing this effort. And there is still more to come...

Next Weekend: *A Brief Random Mathematical Interlude as preparation for testing the Null Hypothesis for Redshift Quantization...*

R. Sungenis: So why didn't Dr. Bridgman write an Erratum paper to *Astrophysical and Space Science* journal and tell the peer-reviewers that they made a mistake in approving HH2007 and that there are no periodicities in galaxy distribution? I think Dr. Bridgman knows that the reason he hasn't or won't is because his objection would be rejected, especially since the objection would have to be based on a typographical error. Moreover, Dr. Hartnett is not the only one who has seen these quantizations. There is a whole history of papers reporting quantizations, beginning with those who saw them since the early 1970s (*e.g*, Tifft, Napier, Burbidge) to those like Hartnett and Hirano who see them more precisely in relation to our void area. Cosmologists have seen these quantizations not only in redshifts in galaxies but also in gamma ray and x-ray sources, in BL lacertaes, in quasars and excess redshifts in globular cluster stars. The evidence is just dripping from the sky. In fact, Dr. Hartnett has published a 2009 paper ("Unknown selection effect simulates redshift periodicity in quasar number counts from Sloan Digital Sky Survey," Astrophysics & Space Science, 324, 13-16, 2009) regarding the quantizations of quasars that was cited by the SDSS Seven Year Data Release, reported in *Astrophysical Journal* (139: 2360-2373, 2010). Be that as it may, evidently, a typographical error in the original HH2007 paper, which had no bearing on the results, and a mere relabeling of the x-axis is not going to change any of the quantization tables.

May I suggest that perhaps the real reason Dr. Bridgman is trying to make a federal case out of this issue is the same reason he locked out Mr. Delano from responding on Dr. Bridgman's blog. Dr. Bridgman obviously has an agenda against anyone who finds evidence against the Copernican Principle, and against anyone who would demonstrate intelligent design in the universe, especially a design that seems to put Earth in the center of it all. If Dr. Bridgman wants to prove his position then I suggest he submit his own papers to *Ap&SS* or *Physical Review* instead of locking out opponents from his blog who disagree with him and making a mountain out of a mole hill regarding typographical errors. But has Dr. Bridgman ever submitted a paper to *Ap&SS*, *Physical Review*, or *Ap. J.*, much less one dealing with quantization of galaxy distribution? I don't know of any. That speaks for itself.

R. Sungenis: Below is Dr. Bridgman's previous attempt at debunking Quantization. I will respond intermittently.

Tom Bridgman: **Saturday, February 26, 2011**

Quantized Redshifts. VII. Common Power Spectral Density "Bloopers"

A surprising number of researchers seem to regard the PSD (power spectral density) function as some kind of magical tool that 'finds frequencies'. The PSD is a very powerful tool, but one with which you can 'shoot your own foot off' if you are not careful.

I've laid out the basic mathematics of the PSD, and this will enable us to explore a few basic claims that have been posed in the creationist and other literature.

Can the Power Spectral Density identify a "Center" of a distribution?

First we should make clear that

1. In Big Bang cosmology (BBC), in the underlying Friedmann-Robertson-Walker metric, every point is a center. No matter where the observer is located, they will interpret the universe around them as being centered on them. See Misconceptions about the Big Bang (Scientific American), "Is it possible to point to a direction in the sky and say "that way is the center of the universe, where the Big Bang started?"

R. Sungenis: If the idea that you can point to any direction in the universe and claim that it is the center of the universe sounds like double talk, that's because it is. We need only quote from well known experts in cosmology who know as much or more about this phenomenon than Dr. Bridgman. For example, although Dr. Bridgman makes it seem as if the FRW universe is a fact, Stephen Hawking, who is a bit more honest with the data, has stated in his book *A Brief History of Time* that the FRW universe is merely a preferred model he chooses based on his penchant to be "modest," but that he has no way of proving the FRW model. Note these words:

> "There is, however, an alternate explanation: the universe might look the same in every direction as seen from any other galaxy, too. This,

> as we have seen, was Friedmann's second assumption. We have no scientific evidence for, or against, this assumption. We believe it only on grounds of modesty" (p. 42).

What is even more remarkable is that Prof. Hawking is honest enough to admit that, as he can't prove his own preferred FRW model, the idea that the Earth may be in the center of the universe is not something he can easily refute. Note these words on the same page:

> "...all this evidence that the universe looks the same whichever direction we look in might seem to suggest there is something special about our place in the universe. In particular, it might seem that if we observe all other galaxies to be moving away from us, then we must be at the center of the universe."

We might also add that just in the last few years the evidence from cosmology is telling us that the scale is tipping toward the geocentric side of Hawking's see-saw. For example, in the ongoing desperate attempt for Big Bang advocates to find at least some anisotropy in the CMB in order to account for galaxy formation, they actually found much more anisotropy than they bargained for. Recent studies from the 2001 WMAP and the 2005 SDSS show that the CMB dipole, quadrupole and octupole harmonics are in exact alignment with our ecliptic and equinoxes. If there was a Big Bang, how could the CMB even know we exist, much less be in exact alignment with our ecliptic and equinoxes which are supposedly 13.7 billion years in the future of the last scattering surface? Well, let's see how the leading cosmologist in the United States today, Dr. Laurence Krauss of Arizona State University, interprets this amazing data from the SDSS:

> Alan Guth was sitting next to me at the conference when I handed him the article. He was smiling, but he always smiles, so I didn't know what to make much of it, but I think that the results that came out of the cosmic microwave background (CMB) studies were twofold.
>
> Indeed, as the Times suggested, they validate the notions of inflation. But I think that's just journalists searching for a story. Because if you look at what quantitatively has come out of the new results they're exactly consistent with the old results. Which also validate inflation. They reduce the error bars a little bit, by a factor of two. I don't know

> if that is astounding. But what is intriguing to me is that while everything is consistent with the simplest models, there's one area where there's a puzzle. On the largest scales, when we look out at the universe, there doesn't seem to be enough structure — not as much as inflation would predict. Now the question is, is that a statistical fluke?
>
> That is, we live in one universe, so we're a sample of one. With a sample of one, you have what is called a large sample variance. And maybe this just means we're lucky, that we just happen to live in a universe where the number's smaller than you'd predict. But when you look at CMB map, you also see that the structure that is observed, is in fact, in a weird way, correlated with the plane of the earth around the sun. Is this Copernicus coming back to haunt us? That's crazy. We're looking out at the whole universe. There's no way there should be a correlation of structure with our motion of the earth around the sun — the plane of the earth around the sun — the ecliptic. That would say we are truly the center of the universe.
>
> The new results are either telling us that all of science is wrong and we're the center of the universe, or maybe the data is simply incorrect, or maybe it's telling us there's something weird about the microwave background results and that maybe, maybe there's something wrong with our theories on the larger scales. And of course as a theorist I'm certainly hoping it's the latter, because I want theory to be wrong, not right, because if it's wrong there's still work left for the rest of us.
>
> (THE ENERGY OF EMPTY SPACE THAT ISN'T ZERO,
> A Talk with Lawrence Krauss
> http://www.edge.org/3rd_culture/krauss06/krauss06.2_index.html)

Is Krauss overstating his case? Well, the question of the data possibly being a "statistical fluke" has been discounted by many others (*e.g.*, "The Axis of Evil," Land and Magueijo, 2005, arXiv.astro-ph/0502237v2 22Feb2005). And although Krauss may be missing a third plane in order to confine the center to one point in the universe, the two planes he has (the ecliptic and the equinoxes) show that the center of the universe is in our vicinity, and that there is, logically, only one center, and not, as Dr. Bridgman is trying to convince us, an infinite amount of centers anywhere you look. This is the very reason Dr. Bridgman is trying desperately to discount the quantization phenomenon in HH2007, since if there is quantization around our void area this shows that our void area is acting as a center for all the galaxies in the universe that we can see. A center is anathema to cosmological ideologues like Tom Bridgman, since it destroys the cherished Copernican principle by which he governs his life.

2. In addition, because of the finite speed of light (which many creationists try to ignore), when we look into the distant cosmos, we are looking into the distant past. Since BBC initiates a starting point from which structures grow and evolve, the further out we observe, the universe will begin to look different as we see younger and younger cosmological structures.

R. Sungenis: But the problem with modern cosmology is that we do not see "younger and younger cosmological structures." We see wholly formed galaxies at the outer edges of the universe just as we see close to us. Ivan King stated that the galaxy formation problem was a "flagrant scandal that is rarely mentioned in public" (*The Evolution of Galaxies and Stellar Populations*, ed. B. M. Tinsley and R. B. Larson, New Haven: Yale University Observatory, 1977). A recent study by Johns Hopkins University with a press release by Karl Glazebrook on July 7, 2004 stated:

> "It seems that an unexpectedly large fraction of stars in big galaxies were already in place early in the universe's formation, and that challenges what we've believed. We thought massive galaxies came much later….This was the most comprehensive survey every done covering the bulk of the galaxies that represent conditions in the early universe. We expected to find basically zero massive galaxies beyond about 9 billion years ago, because theoretical models predict that massive galaxies form last. Instead, we found highly developed galaxies that just shouldn't have been there, but are."

Alan M. MacRobert confirms the dilemma:

> "Astronomers thought they had a nice, clear picture of how galaxies formed billions of years ago – but now the picture is suddenly turning muddy. A team studying the faintest galaxies ever to have their spectra taken is finding far too many big, mature galaxies similar to our Milky Way much too early in cosmic history. 'Theorists are not yet at the point of panic, but they're getting there'" (Sky and Telescope, "Old Galaxies in the Young Universe," January 6, 2004).

The BBC, in “Hubble’s Deepest Shot is a Puzzle,” reports of the 800 exposures in a patch of Hubble’s Ultra Deep Field that there are far fewer stars existing than expected, stating that this “brings into question current ideas on cosmic evolution.” Leader of the survey, Dr. Andrew Bunker, stated: “Another possibility is that physics was very different in the early Universe; our understanding of the recipe stars obey when they form is flawed” (BBC News, Sept. 23, 2004).

Bridgman: The popular retort that the universe will look different from different locations works only in a case where the universe is static (not evolving)

R. Sungenis: Not so. Logically, if a static (or non-expanding) universe has a center, then the same center will appear if we uniformly expand that universe. The center of concentric circles doesn’t change, Tom. Thus, if we move away from the center of either the static or expanding universe, the universe will look different. The only reason you think you have a solution to the disdain you have for a isotropic and non-homogeneous universe by appealing to an “evolving” universe is that your chosen FRW metric puts us and the galaxies on the expanding surface (of the famous balloon analogy). But in a flat Euclidean space it may either be bounded or unbounded and can have a unique center as well. The balloon analogy assumes a certain spatial geometry, but a flat Euclidean universe is totally consistent with all observations.

Bridgman: and the speed of light is very large or infinite (which violates loads of atomic spectra we observe in distant galaxies as well as using redshift as a measure of anything).

R. Sungenis: No. If the galaxies were created in an envelope near the Earth and then subsequently expanded away from the Earth at the speed of light, then the original light would still be seen on Earth. And since you can’t tell us how large the universe is (and thus how far light would have to travel) without begging the question about how your LCDM universe can work without the Men in Black (Dark Matter and Dark Energy), then you really don’t have much room to talk. Moreover, you have your own “horizon problem” since your universe is apparently too big for its britches. Trying to solve it by invoking a spatial inflation which you allow to exceed way beyond the speed of light in order to preserve the speed of light from being violated in every other realm of life seems a bit contrived, doesn’t it?

Face it, Tom. Your cosmology is in a heap of trouble. It's why the latest paper from your colleagues can't go along with your hocus pocus any longer. They are admitting things like the following:

> "The existence of non-trivial cosmic topology and of anisotropic geometry are questions that can only be answered observationally. In this regard, it is worth noting that our record at predicting the gross properties of the universe on large scales from first principles has been rather poor. According to the standard concordance model of cosmology, over 95% of the energy content of the universe is extraordinary (dark matter or dark energy whose existence has been inferred from the failure of the Standard Model of particle physics plus General Relativity to describe the behavior of astrophysical systems larger than a stellar cluster) while the very homogeneity and isotropy (and inhomogeneity) of the universe owe to the influence of an inflation field whose particle-physics-identity is completely mysterious even after three decades of theorizing. The stakes are set even higher with the recent discovery of dark energy that makes the universe undergo accelerated expansion. It is known that dark energy can affect the largest scales of the universe -- for example, the clustering scale of dark energy may be about the horizon size today. Similarly, inflationary models can induce observable effects on the largest scales via either explicit or spontaneous violations of statistical isotropy. It is reasonable to suggest that statistical isotropy and homogeneity should be substantiated observationally, not just assumed." ("Large Scale Anomalies in the CMB," Copi, Huterer, Schwarz and Starkman, Nov. 2010)

So I suggest you try to do science by not first assuming that your universe is correct and that all the data you find must fit into it.

Bridgman: Myself and others have explored the 'decaying speed of light' claims of Barry Setterfield elsewhere (A Changing Speed of Light?).

I have no doubt that many Big Bang opponents will keep repeating this lie.

R. Sungenis: The big lie is that the universe is as big as modern cosmology would have us believe, as well as how the "finite" speed of light determines what we know.

As to the first issue, it is interesting to see what happens when we use Big Bang cosmology's very own formula for measuring the age of distant objects. The age is calculated by the formula $t = t_0\ (1 + z)^{-3/2}$, where t_0 is the current age of the universe and *z* is the redshift factor of the object.

[Incidentally, this *z*-factor formula is based on the so-called "dust model" of the universe wherein the major components of the universe do not exert any pressure on their surroundings. But if one were to base the z-factor on the radiation of the CMB in terms of number of particles, the formula would be $t = t_0\ (1 + z)^{-2}$. This again, shows the complete arbitrariness of the formulas since they invariably depend on one's unproven assumptions, in terms of what is the dominant energy component of the universe at any given epoch.]

Be that as it may, let's say the Hubble telescope finds a distant object in the sky and assigns it a z-factor of 1. Modern cosmologists, like Tom Bridgman, will then plug in the value for t_0 as 13.78 billion years and will compute a value for *t*, which is understood as the age of the universe when the radiation emission of the distant celestial object took place. In the case where $z = 1$ then $t = 4{,}844{,}413{,}013$ years. Since using the number 13.78 billion years is completely arbitrary (for it is based on the unproven Big Bang assumptions of the universe), let's say we assume t_0 is 10,000 years instead of 13.78 billion. In this case, where $z = 1$ then $t = 3{,}536$ years. In other words, when an astronomer sees a star with a *z*-factor of 1, he might just as well assume the universe was 3,536 years old rather than 4.8 billion years old, since the *z*-factor is only a function of one's assumption regarding the beginning of the universe. If an astronomer finds an even more distant object that correlates to a *z* factor of 2, then the age of the universe when the object began radiating was 1,924 on the biblical scale but 2.6 billion years on the Big Bang scale.

Interestingly enough, if we use modern science's formula for measuring the age of the universe when the CMB was released, we get very close to the time we have predicted that the firmament would create the 2.73º Kelvin temperature. The formula is $T = T_0\ (1 + z)$. Plugging in a *z*-factor of 1089 for the CMB, the Big Bang theory arrives at a universe age of 380,711 years after the primordial explosion for the arrival of the CMB, whereas using the same *z*-factor we would obtain 0.278 years, which puts the CMB well within the first three months of the first year of creation.

As for Dr. Bridgman basing his case for the Big Bang solely on the finite speed of light, this is simply a case of selective cosmology. The Big Bang itself is a mass of contradictions when it comes to appealing to the finite speed of light. For example, Big Bang proponents will not allow gravity to go faster than the speed of light but they allow "space" to expand much faster than the speed of light in order to answer their "horizon problem." They don't have an explanation as to how gravity works or what constitutes the "space" of their expanding universe, but somehow they are certain that the former must be limited and the latter is unlimited. They are only "certain" because without these two parameters their Big Bang universe wouldn't work, so to them their invented postulates must be true. Of course, they run into another anomaly when they have to admit that the speed of light is only finite within the postulates of the Special Relativity but not the General Relativity where light can travel any speed it wants. And if the Big Bangers find the Dark Matter and Dark Energy they need in order to propel their Big Bang expansion, well then they will have enough gravitational substance in the universe to allow light to go a billion times faster than it does here on earth. Welcome Barry Setterfield. Seems like the Copernicans are caught between a rock and a hard place. (For more information on these issues, see my book, Galileo Was Wrong, Vol. 1, Appendix 1: Anomalies Concerning the Speed of Light).

Bridgman: It has been suggested that the very existence of periodicities in extragalactic Power Spectra is evidence of a geocentric distribution.

R. Sungenis: No, it has been rather firmly stated in peer-reviewed papers, as has also been the case of the CBM's alignment with our ecliptic plane and equinoxes. In the end, we have such strong evidence for a geocentric universe – evidence so strong that it would have laughed Galileo off the stage – yet Tom Bridgman, due to his Copernican myopia, simply won't allow himself to see it.

Bridgman: Russ Humphreys explored this, suggesting that broader peaks in the PSD implied that the Earth was actually slightly offset from the center of the Universe in Humphreys' strangely titled "Our galaxy is the centre of the universe, 'quantized' redshifts show".

R. Sungenis: You can check Humphrey's paper for yourself at

http://creation.com/our-galaxy-is-the-centre-of-the-universe-quantized-redshifts-show

and you will see that he doesn't use PSD. Rather, he uses the peer-reviewed papers of William Tifft, Cocke, Napier, Guthrie, *et al*.

Bridgman: The major problem with such a claim is that it can be rigorously demonstrated that the PSD is independent of the location of the 'center' of a dataset, and therefore cannot identify the location of the 'center'. I hinted at this by my question in my previous post (Quantized Redshifts. V. Exploring the PSD with Simple Datasets). How do you prove this?

R. Sungenis: Humphrey's only problem was that he used a 1D data series, but 2D and 3D studies done since then confirm that Humphrey's was at least on the right track. The periodicities are present, and in the same intervals, whether one looks at them from the perspective of the CBM or the Earth. The issue isn't the PSD. That is only Dr. Bridgman's red herring to get our eyes off the real issues.

Bridgman: Consider the 1-dimensional case of a some function, f(x) with a 'center' defined as x=0. Now displace that function on the x-axis by the distance, a, so the 'center' is now at x=a. This defines a new function, f':

f'(x') = f(x-a)

because x'=x-a.

To see how this affects the PSD, first, take the Fourier transform of both of these functions:

$$F(k) = \frac{1}{\sqrt{2\pi}} \int f(x) e^{ikx} dx$$

and

$$\begin{aligned} F'(k) &= \frac{1}{\sqrt{2\pi}} \int f(x-a) e^{ikx} dx \\ &= \frac{1}{\sqrt{2\pi}} \int f(x) e^{ik(x+a)} dx \\ &= \frac{e^{ika}}{\sqrt{2\pi}} \int f(x) e^{ikx} dx \\ &= e^{ika} F(k) \end{aligned}$$

So we see that the Fourier transform of the original function gets multiplied by a complex (real+imaginary) factor

$$e^{ika} = cos(ka) + i\ sin(ka)$$

when displaced in position. But the Power Spectral Density is the Fourier transform multiplied by its own complex conjugate

$$\begin{aligned} f &= a + i\ b \\ f^* &= a - i\ b \end{aligned}$$

so,

$$
\begin{aligned}
PSD(f') &= (F'(k))^*(F'(k)) \\
&= (e^{ika}F(k))^*(e^{ika}F(k)) \\
&= e^{-ika}F(k)^*e^{ika}F(k) \\
&= (e^{-ika}e^{ika})F(k)^*F(k) \\
&= 1\ F(k)^*F(k) \\
&= PSD(f)
\end{aligned}
$$

Therefore the PSD of the original function is EXACTLY the same as the PSD of the function with the displaced center. It is impossible for the PSD of a distribution to be used to identify its center.

R. Sungenis: The way to do it is to construct a 3D sphere with shells such that if you bin the abundance of the density as a function of radius from the true origin or center and then do a Fourier transform, you will see a peak in the spectrum at the period of the inverse shell spacing. If one then moves the origin from the common center of the concentric shells and the data is rebinned one would get a new histogram that is not, as Dr. Bridgman insists, a simple translation.

Bridgman: Potentially, one could use the Fourier transform itself to identify a value for the complex offset value found above, but the only way to do this would be to compare the Fourier transforms with a specific model distribution.

R. Sungenis: The real importance is comparing a real model to a simulation.

Bridgman: This proof is easily extended to three- and four-dimensional transforms.

R. Sungenis: Not true.

Bridgman: The bottom line is *the PSD cannot identify the center of a distribution.*

R. Sungenis: As I said above, if one uses a 2D or 3D structure and builds a histogram from the origin or center point, you will get periods from the PSD if there is a symmetric structure around the origin or center. The effect is diluted as you move your origin away from the true center.

Bridgman: Adding & Subtracting PSDs

Consider a signal, s(t), that is the sum of two signals, s1(t) and s2(t)

$$s(t) = s_1(t) + s_2(t)$$

Because the Fourier Transform is linear, it is easy to show

$$F(s) = F(s_1) + F(s_2)$$

But what about the PSD? From the definition of the PSD:

$$\begin{aligned} PSD(s_1 + s_2) &= (F(s_1) + F(s_2))^*(F(s_1) + F(s_2)) \\ &= (F(s_1)^* + F(s_2)^*)(F(s_1) + F(s_2)) \\ &= F(s_1)^* F(s_1) + F(s_2)^* F(s_1) + F(s_1)^* F(s_2) + F(s_2)^* F(s_2) \\ &= PSD(s_1) + F(s_2)^* F(s_1) + F(s_1)^* F(s_2) + PSD(s_2) \end{aligned}$$

We see that the PSD of two summed signals are not necessarily equal to the sums of the individual PSDs. There is an additional component which is a type of mixed product of the Fourier transforms of the two input signals. There are useful cases where you CAN demonstrate the mixed term is zero, for example, if you are collecting signal from a source and there is an uncorrelated (random) background signal (see Effects of background counts in RMS normalization).

But in general, *this mixed product is not automatically zero.*

A similar relationship holds if we wish to subtract one signal from another.

R. Sungenis: Correct, but irrelevant.

Bridgman: Some researchers attempt to split the PSD into two additive components to justify subtraction of the components they perceive to be part of the distribution of the data. This is an attempt to leave only the oscillating components. The problem is the distribution they wish to subtract often has significant variation as well. *It is the responsibility of the researcher to demonstrate that the input components have no correlations, or are correlated in a simple way, but I have not seen this done.*

R. Sungenis: Correct, but if the part that is subtracted is a very low order (i.e., slowly changing compared to any structure), subtracting it only helps accentuate the signal one is searching for.

Bridgman: The errors described above are made in a number of papers claiming quantized redshifts and represent a basic misunderstanding of how the Fourier transform and PSD works. These are basic errors that anyone who has who taken a course in mathematical methods for physicists (wikipedia) should not make. One might expect a first-year graduate student or a neophyte to make these types of errors, but not someone with long experience with the PSD.

But what should really be an embarrassment to some journals is that their peer-reviewers did not catch these errors either!

R. Sungenis: Dr. Bridgman's trumpet blowing isn't going to get him anywhere. We have conclusively shown that it is Dr. Bridgman who has made the errors in analysis, not Dr. Hartnett and Dr. Hirano. If Dr. Bridgman's thinks differently, then let's see him write an Erratum letter to Ap&SS and tell them so. He won't, because it is obvious that he would be laughed at for making such petty claims.