@ issue

Correspondence, conference threads and debate.

Petr Beckmann (1924-1993)

I first became acquainted with Petr Beckmann by reading his *The History of Pi*. Acknowledging that he was neither mathematician nor historian, he was not bound by the constraints of either: he would not present the full mathematical rigor that a mathematician would, nor would he feign the objectivity of a historian. The book is about *p*, to be sure, but it's also an assessment of the mental/political health of past societies. *Every* civilization of any value has faced the problem of determining $p = C/D$ for a circle, which is neither easy to measure nor easy to calculate. How this universally important parameter is estimated provides a good window into the mental workings of the society.

Characteristic of Petr's perspicacity was his comment that he did not place terribly much blame on The Church for their condemnation of Galileo. One simply *expects* such behavior from the reigning authority, particularly when their power is unchecked. He placed the blame for Galileo's misfortunes squarely on the heads of Galileo's fellow intellectuals who offered no support. Petr was always one to stand up and be counted.

My next exposure to Petr's thinking was through his provocatively titled *The Health Hazards of* ***Not*** *Going Nuclear*, of which Petr sold some 50,000 copies. This gem was the scourge of the anti-nuclear lobby, because it provided a very large number of people with ammunition and common-sense reasoning so missing from the usual diatribes of the energy left. They view(ed) nuclear energy as 'dangerous;' Petr asked, "compared to what?" Petr compared the hazards of the whole nuclear cycle—from mining, through use, including accidents, to burial of wastes—to those of coal, on a step-by-step basis. Nor was Petr afraid to compare the hazards of nuclear energy with those of solar energy; *on a per unit energy delivered basis*, nuclear is still safer. Petr showed that if nuclear power had nothing in its favor except waste disposal, nuclear power would be preferable to the alternatives.

After *Hazards*, Petr began writing *Access to Energy*, unabashedly "A Pro-Science, Pro-Technology, Pro-Free Enterprise Monthly Newsletter." Many an errant politician or publicity seeker was stung by Petr's sharp wit and analytical mind. He reminded people that "there is no such thing as 'safe energy;' to ask for safe energy is to ask for gasoline that won't burn." "Access" carried technical articles, always educational and easily read, but it also served as a constant reminder about the anti-scientific, anti-technological drumbeat of the news media.

I have long had an interest in the energy situation, and it was through many communications on the subject that I got to know Petr better, if only by mail and an occasional telephone call. One phone call was particularly important, because he asked me if I would be willing to read over a book he was writing, not about energy or technology, but about Einstein. He wanted me, as a physicist of essentially standard education and training, to probe his book for errors, to keep him from making a fool of himself, because he would be asserting that the speed of light wasn't constant the way everybody believed, for example. Recognizing the potential for harm, he cautioned me that I might possibly agree with him, and find myself on the receiving end of scorn from colleagues.

I agreed to look at the book, *Einstein Plus Two*, feeling certain that I could easily find the experiments, so often cited in textbooks, that would show Petr that he wasn't right. He was clearly inclined to trust experiment over theory, and he was obviously intelligent enough to understand the experiments, so I agreed to the task. Well, if the speed of light is constant—and in SRT that means *isotropic*—then the Lorentz transformation equations are one way of explaining the fact, and (especially from what I have seen from people who have offered alternatives within that framework) the simplest. But Petr was contending that the speed of light was not constant with respect to the *observer*, but rather, with respect to the locally dominant field. In the case of the earth-bound laboratory, this was the gravitational field of the earth.

An immediate consequence, considering the non-null result of the Michelson-Gale experiment, was that the speed of light should be $c - v_{rot}$ toward the east and $c + v_{rot}$ toward the west, where v_{rot} is the surface velocity of the laboratory due to rotation of the earth. Let it be understood that this is a scientific question, requiring an experimental answer. I looked up the references in the standard textbooks to the experiments that supposedly showed isotropy to very tiny uncertainties. I ferreted out all of the famous references and found out to my surprise that Petr was, at least, not demonstrably wrong. There were no purely optical experiments showing east-west isotropy to the accuracy that would show Petr was wrong. There was one—and only one—experiment *claiming* isotropy in laboratory coordinates, but it was not purely optical: was one learning about light or about moving clocks? (In the end, it proved *an*isotropy, when one accounted for the clock motion.)

I wrote a paper about my survey of the speed-of-light literature, and submitted it to *Am. J. Phys*., regarding it as primarily an educational matter. After all, there was nothing to prove that Petr was right; there was only the absence of proof that he was wrong. But most of all, I felt rather betrayed in the education I had received, and in the standard textbooks, because there was nary a mention of the Sagnac experiment, of Michelson-Gale, or of many of the (in no way controversial) experiments Petr had cited in *E + 2*.

The question I raised in the paper was simple enough: "experimentally, is the speed of light the same east as it is west, or, as Petr Beckmann claims, is it $c \pm v_{rot}$ westward and eastward, respectively?" I must confess to being surprised at the way the way the question was treated by referees. After all, in SRT the speed of light isn't *supposed* to be isotropic in a non-inertial frame, namely the earth-bound laboratory, so why should anybody be surprised if it turns out anisotropic? Moreover, regardless of the motivation behind the question, why would anybody object to raising a purely experimental question?

During my unsuccessful attempts to get the paper published in *AJP*, and later in *Foundations of Physics*, I began really to understand why Petr had issued his caution. After a two-year-long fight with obtuse referees, I simply quit trying to get the paper published, because I was continually rewriting it to please them—to no avail—and doing nothing else. (In the end, the paper was published in *Essays in Physics*.)

By that time, Petr had made up his mind to start a journal of his own: *Galilean Electrodynamics* was born. Clearly, if I was having so much trouble publishing a paper merely asking what experiments said about the speed of light, others must be having similar difficulties. He told me that he had wanted for a long time to start a journal with that name. It was simply part of Petr's character to be deliberately provocative in his wording. (Indeed, a printer recently asked me about the title, noting that Galileo probably didn't know about electrodynamics.)

After many years of communicating with him, I finally got to meet Petr at his home in the mountains west of Boulder, Colorado. I was well acquainted with his acid humor, but his impish grin made me appreciate the humor even more than I did when I saw the humor in print. I found him to be extraordinarily gracious, and that impression was confirmed by a secretary from the University of Colorado department of electrical engineering who said as much when she spoke at his memorial service in August. The postal clerks in Boulder volunteered similar comments.

For several years, we were in contact on a daily basis via e-mail, mostly for brief notes, but often for serious discussion. I cannot begin to convey how much I miss that contact. Yet with all of it, I was only vaguely aware of his personal history, and I learned a lot at his memorial service.

Petr was born in Prague, Czechoslovakia, in 1924, and was therefore 15 years of age when World War broke out in earnest in 1939. As his father was Jewish, the family qualified as refugees, and they moved to England in that year. In 1943, he joined the Czech squadron of the RAF, and worked in a repair facility for the fledgling radar systems. After the war, Petr studied physics and electrical engineering at the Prague Technical University, leading to his Ph.D. It is of interest that one of his books, *Scattering of Electromagnetic Waves from Rough Surfaces*, contains much that you would want to know if you were using radar to try to detect a periscope against a background of salt water. Petr was the first to calculate the radar cross-section of the moon. He was a Fellow of the IEEE.

It was his expertise in scattering from rough surfaces that got Petr invited to the University of Colorado for a sabbatical year. While he was there, his father died, and he had no reason to return to Prague. He defected, became a US citizen in 1964, and married Irene Müller in 1965. In lieu of a dozen employees ("an employee is a paid enemy"), Petr had Irene, who was at once paper-shuffler, box carrier, secretary, confidante, printing-press master, copy editor, travel agent, and the one who (if anybody could) kept him in check in his too-enthusiastic moments.

Professor Frank Barnes related one interesting story. Petr walked into the office one day, hopping mad. A paper, in which he had done an exact calculation that had never been done before, was rejected by the referee—on the grounds that it did not agree with the approximate calculation that everybody had been using for years. Petr had made no reference to that calculation, *but he was the author of the approximate calculation as well*.

It is no news to readers of *Galilean Electrodynamics* that Petr regularly translated articles from our Russian colleagues (I wish I could!); what they may not know is that Petr also taught *English* once upon a time. His command of *my* native tongue was superb, even compared with the average Ph.D. in my acquaintance. He wrote a book, *The Structure of Language*, that evidently made a considerable splash in linguistics. He wasn't writing anything to displace the standard litany, but rather to supplement them all. Electrical engineers learn about error-correction codes in the transmission of signals; Petr showed that languages (all of the several he knew) used subtle redundancies to keep messages from becoming garbled.

As a youth, Petr joined the Communist party in Czechoslovakia, but before long became disenchanted. Initially he had the idealistic idea, probably from his father who was one of the founders of the party, that the communist system was a good form of government; the fact that it didn't work was simply because there were bad and/or incompetent people in charge. Only later did he decide that it was the *system* that was bad. Any engineer or scientist designing a control system soon learns that feedback loops need to be kept short, or else the output will oscillate uncontrollably. (Alternatively, with low gain, *nothing* happens.) But that is exactly what a centrally controlled system is. That is, if the decision about how much toilet paper to have on the shelf is made in Washington, DC, the store will find itself very short, but after repeated pleas for more, will find itself inundated as the orders finally come in.

If there are recurrent themes in Petr's profile, they are these: feedback, as seen in his analysis of language, the core problem of the communist system, and the self-oscillations of the electron in *E + 2*; an enormous range of interest, with contributions ranging from music to Einstein; a hard-nosed intellectual honesty ("a strong case doesn't need to rely on weak arguments."); great warmth in his regard of his fellow man; and a strong belief in the superiority of empirical fact over authoritative statement. He did not suffer fools lightly.

Petr, we miss you!

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Universal Expansion and the Velocity of Light

Astronomers usually rely on the assumption that the "redshift", or change of frequency of the light from distant stars as observed on earth, is due to the recession of these stars from us, the observers. While a recession of a light source from the observer is known to cause this Doppler effect, this is not the only cause that can give rise to a decrease of the frequency of the observed light.

A number of questions relating to the propagation of electromagnetic radiation have remained unanswered so far, despite the very skilled and highly expert work directed towards their elucidation.

Treating them in a simple, but logical manner is said to belong to metaphysics and astronomy, not a task for journals of physics, even though many questions of a physical nature are involved. Be that as it may, these questions are important, even if no answers may be available at present. The purpose of this note is merely to point out that a search for simple logical answers is required and important for astronomy in particular.

The first and most important such question is the one concerning the applicability of our physical "laws" to events occuring at times and locations very remote from us. These laws have been established in our small domain in which it was not essential to assume a constant velocity of light throughout the whole universe. The widely accepted law of the constancy of the velocity of light has never been stated in an unambiguous way.

It is, of course, true beyond reasonable doubt that many local measurements tended to confirm this law which is now often regarded as an established fact. But apart from known deviations which have been investigated and explained by Einstein and many others, we were never told relative to what this velocity equals the constant value "*c*".

Einstein's statement that the velocity of light is constant in empty space cannot be used without a clear definition of what is meant by "empty space". We do not know how this empty space moves locally,or if it is assumed to move at all. A strong indication that the ether does move relative an observer at rest on the earth was found experimentally by Michelson (1925), of all people! With Gale, he found that the speed of light towards east and towards west *in vacuo* at different locations was not identical, as measured quite reliably at a single location! If the ether is assumed to be rigid, which assumption is not justified, then we are back to Newton's kinematics, using only different words. It is remarkable that the above mentioned work of Michelson is disregarded by most physicists.

That *c* is not constant relative to the source of light has been proven beyond reasonable doubt. That it cannot be constant relative to the observer is quite evident, since neither the location nor the velocity of the observer is usually known at the time and location of the emission of the photons in question.

An interesting experiment by Hoek (1868; Born 1965) is one of the exceptions whereby we do know the state of movement of both the source and the observer. This experiment showed 15 years before the famous experiment of Michelson and Morley (1887) that the earth was not moving relative to the ether.

The validity of Hoek's similar conclusion was recently denied by Zheng (1983) and Aspden (1985). They claim that Hoek's experiment would have found the same result if the earth were moving relative to the ether. An article pointing out an error in their reasoning was submitted to the two journals, but neither of them published this objection. A repetition of that experiment under somewhat different conditions would show that the objection was valid, unless Einstein's statement was an error. He wrote—referring to Fizeau's experiment—"the propagation of light always takes place with the same velocity with respect to the liquid, whether the latter is in motion with reference to other bodies or not".

Even if Einstein's dictum (Einstein 1960) were not relevant to this particular case,the reasoning used against the validity of the Hoek experiment was erroneous. A description of a modified version of Hoek's experiment was not accepted for publication by several journals of physics, because the erronous mathematical proof showed that it could not succeed. Doubts about the outcome of an experiment should not be a sufficient reason for not performing it. Particularly if these doubts were not fully discussed and agreed upon. An outline of the suggested experiment is given here to invite comment, so that the theoretical basis for this fairly simple experiment might be clarified.

The experiment is the same as that of Hoek, but it should be performed at high velocity relative to the ether, or space if you like. It uses a simple compact apparatus which sends a split light ray parallel through glass and air (or vacuum) respectively and then observes their interference upon reunification. It measures changes of the pattern, if any, when the apparatus is turned away from the direction of the real or suspected movement relative to the ether. The performance of this experiment may require the use of a rocket, preferably with the cooperation of NASA (which was sought unsuccessfully).

A positive outcome would provide a means for performing many other tests of ideas relating to the nature, the relative movement and, indeed, to the very existence of an ether.

Another aspect of the same problem relates to the rule that light travels at the same speed in all directions on the surface of the earth. This is not always true. Proof of this has been found by Michelson himself together with Gale (Michelson 1925). In a loop consisting of a very large rectangle of evacuated tubes they found that the speed of light towards east was not the same as it was towards west. Some comments were made that this experiment did not come under the rules of the Special Theory of Relativity because, on the surface of the earth, straight lines were in fact curved. Although this is true, it is not likely to be an explanation of the effect. The fact that one tube was closer to the equator than the other could be more significant. This experiment deserves more attention and discussion than it has received.

The influence of the medium of propagation on the speed of light along a curved path was noted by Sagnac (1925; Post 1967). The suggested experiment differs in that it tries to establish this influence for the case of a straight light path.

It is a widely held opinion that the observed redshift of characteristic frequencies must be due to the recession of the galaxies. A number of theoretical objections to assumptions of a pure Doppler effect have been raised. It is quite well known that changes of gravity, or effects of inelastic scattering can cause such changes. And, last but not least, it is also possible that a frequency lower than expected may be partly due to a frequency of emission lower than that which we assume.

Hubble found the observable redshift to be proportional to the distance of a galaxy, but not that it was also proportional to the speed of its recession. Such an interpretation of Hubble's work is based on the big bang Theory of Lemaître, Eddington and Gamow. It should therefore not serve as supporting evidence for that theory, or to completely dismiss all theories not based on an expansion caused by a big bang.

While the proportionality of the distance of a star from us and the redshift of its light upon reception has been amply confirmed, there are exceptions. Arp (1990) has described a very considerable deviation from proportionality. This involved a quasar and an adjoining galaxy showing very different redshifts. One may assume that this discrepancy was due to the quasar being one of those "embryos" ejected by the galaxy, as mentioned by McCrea (1964). Conditions prevailing within such an embryo are quite unknown, and the photons emitted by it could well have a lower frequency than we assume. Of course it is also possible that this quasar moves away from us at high speed, while the galaxy near it at the time may move away from us slower, if at all.

An important consideration said to strongly support the theory of the expansion of the universe is the interpretation of the cosmic background radiation. The presently accepted version holds that its frequency is due to a redshift of the extremely high frequency of the radiation emitted by the big bang. The fact that it is fairly isotropic with regard to its range of frequency and intensity is said to be proof of its emission at one time and from one location, *i.e.* in the big bang explosion.

But this isotropy would, rather, suggest emission from many locations at nearly equal distances from our present position, and therefore probably within our Galaxy. If it came from a single source, it could not reach us coming from all directions through paths of nearly equal length, hence subject to very similar redshifts.

This interpretation is dismissed as being due to thinking in three dimensions, which is said to be a serious mistake. One should use a fourth spatial dimension, other than the dimension (time) introduced by Einstein. But how this can answer the question is never stated in physical terms. A mathematical answer is not sufficient for understanding, even if several *ad hoc* assumptions are made to change the relevant geometry completely.

If, however, the cosmic background radiation is ascribed to the blackbody radiation emitted by all matter in the universe, being a relic of the big bang, why is it then isotropic? Surely the band of frequencies would then be very broad! If all matter had been hurled into space at different velocities, why should each lot have possessed the same thermal energy to start with? (The velocities must have been different,or else the universe would have a shape of a bubble with all galaxies situated at its surface.)

Most of the emission by a big bang is generally believed to have been in the form of radiation. How did that radiation lose energy so as to reach a temperature at which its conversion into matter became possible? With its speed constant, a loss of energy was possible only by a decrease of frequency. If the radiation emitted by a cosmic egg lost energy by a loss of frequency, then surely we cannot deny a similar loss of frequency causing a tired light effect of present day radiation which travels through space that contains matter, even if tenuous, while the path of the primordial radiation is assumed to have been absolutely empty.

Without satisfactory answers to these questions,the cosmic background radiation's ability to falsify any theory which does not include the creating effect of a big bang remains very doubtful. Some answers to the questions mentioned here may be available already, others will probably be found soon. When they become available and accepted,they will give rise to new theories of physics and cosmology. Of course it is not claimed that the experiment suggested here will provide these answers. But it may be of some small help, and should be performed.

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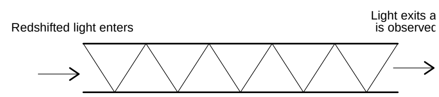
Revitalizing redshifted light

In a recent letter (Love 1993), I suggested that the redshift is due to the expansion of the light rather than an expansion of the universe or a Doppler-shift. Some experimental evidence is required to substantiate this claim. If, as I suggested there, the photon expands or dilates but does not lose energy, it should be possible to reverse the expansion, hence reverse the redshift and force the light to contract to its original frequency.

Following the spirit if not the letter of the Transactional interpretation of Quantum Mechanics (Cramer 1986), it seems that the light wave can expand because it has not interacted with matter. it follows then that in order to contract the light, we must have it interact with matter, but not be absorbed until it is revitalized. A simple arrangement of two parallel mirrors suggests itself. Light enters at one end and bounces back and forth until exiting at the other end:



If the red-shifted light is just a dilated wave packet, carrying the original amount of energy and, if the interpretation of quantum mechanics is valid, the exiting light will have a frequency closer to the original frequency (the greater the number of reflections, the closer) while un-redshifted light would experience no frequency change.

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The "Ampere paradox"

This could be called "the Ampere paradox", and should be of particular interest to Thomas E. Phipps, Jr. and to Peter Graneau, because of their long-term interest in Ampere Tension/Ampere Force. Any comments would be appreciated.

Consider a cubic centimeter of copper attached between two massive copper busbars. Pass one million amperes through that cubic centimeter for a time of one microsecond. The resistance of the 1 cc of copper is about one micro-ohm. Since I^2R equals one watt, one megawatt was delivered for one microsecond, to a total energy of one watt-second; not enough to raise the temperature of the 1 cc. of copper by even one degree Centigrade.

But in fact, if you pass one megawatt for one second, the copper will explode, because copper cannot dissipate heat at that rate, of 2.4×10^5 cal/second. The thermal conductivity of copper is only about 1 cal/sec/cm^2 per degree difference across an interface. The thermal capacity of copper is about 1 cal/degree rise. The heat input of one megawatt is about 2.4×10^5 cal/sec, while the heat dissipation rate is only about 1 cal/sec/°C. Thus the temperature could reach 2.4×10^5 degrees, while the boiling point of copper is only 2336 °C.

Since only one watt-second was applied, the amount of copper actually vaporized is only about one milligram, but at great pressure.

Some years ago I witnessed such an explosion at a small "quiet" lab of Armand Hammer's Occidental Petroleum Company in California. For safety, we had to stand just outside the lab while the device was "fired". It sounded like a 45 caliber pistol firing, but I don't know whether the sound came from the exploding copper, or from the electrical contactor switch. After the firing the entire lab of about 20 by 30 feet was covered with a coarse, hard, sharp grit that felt like silicon carbide grit. More than one cc. of copper had disappeared.

At the time no significance was attached to the explosion, except consternation that it had interfered with the magnetic test at hand, and required installation of a replacement busbar.

My question is: How do we distinguish between 'Ampere Force' and this 'Force of Amperes?' *A paradox*.

There is a simple analogy: A new electric coffee pot was marketed which had a very small, very hot heating element. This model was discontinued because, although it steamed instantly when turn on, it made *cold, burned,* coffee! And there are many other examples I can think of, such as flushing a toilet, or repaying a bank loan: "time-is-the essence."

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Reply from Peter Graneau

The one cubic-centimeter of copper considered by Gifford is likely to explode as a result of the one mega-ampere current pulse, just like all the wires which are exploded by far smaller current pulses (see references). The first point to note is that the resistance of the copper tube is a minimum at room temperature and will race up to very much higher values as the temperature increases through the melting and boiling points. Hence, much more Joule heat is being generated than assumed by Gifford.

Secondly, there will be back-e.m.f. induced in the copper cube, which has to be overcome by the current, and in the process converts electrical energy directly into mechanical energy, as in a motor. It is the latter energy which is primarily responsible for wire explosions.

When these facts are taken into account it will probably be found that there is sufficient energy available to evaporate the copper tube, or more likely explode it in the liquid state, as indicated by the sharp grit produced when molten metal droplets hit stone floors and walls. In (Graneau 1985), there is a graph which suggests that at one mega-ampere the Ampere tension will rise to many tons of force. It would be hemmed in by the busbar extensions, and in the liquid state, the tension would be converted to axial compression. Explosions of this nature in my MIT laboratory sounded more like a cannon than a pistol. We customarily wore ear protection.

In conclusion, therefore, if one believes in the Ampere-Neumann electrodynamics the paradox disappears.

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