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Dogmatism and Theoretical Pluralism in Modern Cosmology

This work discusses the presence of a dogmatic tendency within modern cosmology, and some ideas capable of neutralizing its negative influence. It is verified that warnings about the dangers of dogmatic thinking in cosmology can be found as early as the 1930's, and we discuss the modern appearance of "scientific dogmatism". The solution proposed to counteract such an influence, which is capable of neutralizing this dogmatic tendency, has its origins in the philosophical thinking of the Austrian physicist Ludwig Boltzmann (1844-1906). In particular we use his two main epistemological theses, scientific theories as representations of nature and theoretical pluralism, to show that once they are embodied in the research practice of modern cosmology, there is no longer any reason for dogmatic behaviours.

> *"In cosmological studies, then, a knowledge of the history and philosophy of science is not a superfluity, it is a necessity."*
>
> H. Dingle [1]

1. Introduction

In a recently published article, Matravers, Ellis and Stoeger [2] stressed that the development of cosmology as a scientific disci requires that cosmological models other than the standard Friedmann-Lemaître-Robertson-Walker (FLRW) models be considered as effective alternatives to the standard cosmological scenario. They stated the thesis that the intrinsic power of those alternative models lies in the fact that, by approaching the cosmological problem from a more descriptive and observationally oriented perspective, as opposed to the standard view which starts from broad explanatory premises, one could observationally justify the latter. In this way those phenomenologically oriented cosmologies allow the possible empirical confirmation, or denial, of the basis of the FLRW models.

Matravers *et al*. are aware of the fact that, so far, those complementary approaches, including the one they specifically propose in the article, still lack the comprehensive explanatory power achieved by the standard model, and, therefore, considerable work still needs to be done in order to fill the gap between these two views, or even to implement some of the steps outlined by the alternative approach to cosmology. Hence, they do not regard the standard model as outdated. On the contrary, they wish to keep it in a healthy contact with observational issues and to develop a more self-critical and less doctrinaire cosmology where the two complementary approaches to cosmology can interact in a mutually beneficial way [2, p. 31].

However, it is from within the community favorable only to the standard FLRW model that Matravers *et al*. indicate the existence of a strong resistance in recognizing and allowing that the theoretical work is vulnerable to observational falsification, pointing out that such a resistance is especially prevalent among the researchers favorable to the inflationary scenario [2, p. 35]. Their paper can, therefore, be seen as an attempt to counteract what can only be described as a latent dogmatic tendency coming from within the cosmological community. For this reason, they also stress the need for a more self-critical attitude from researchers of the field in general. In their words:

> *"In fact, a rather serious and disturbing situation has developed within modern cosmology in which some workers promote certain cosmological theories as correct and well-established without seeming to regard the adequacy of their observational or experimental justification as of any importance. At the same time they tend to dismiss more observationally-based approaches - for example the kind of larger justificatory investigation we have just proposed - as being unnecessary or even 'unscientific', simply because such approaches do not unquestioningly incorporate the standard view. This attitude is itself dangerously close to being unscientific, for it elevates theory above observation and relies on simplified geometrical models (certainly of considerable explanatory power) without subjecting them to adequate*

observational testing - or even denying that they should be tested [2, p. 31]."

We agree with Matravers *et al*. that a rather dogmatic and dangerously unscientific attitude has developed in modern cosmology [2, p. 35]. Other authors like Tolman [3], MacCallum [4], Wesson [5], Rothman and Ellis [6], Krasinski [7] and, especially, de Vaucouleurs [8], have also expressed similar views. They all warned of the danger of strongly believing in ideas not confirmed by observations, pointing out that without this confirmation we lose the only way we can distinguish science from metaphysics. For instance, de Vaucouleurs was very clear about this point:

"Unfortunately, a study of the history of modern cosmology (...) reveals disturbing parallelisms between modern cosmology and medieval scholasticism. (...) Above all I am concerned by an apparent loss of contact with empirical evidence and observational facts, and, worse, by a deliberate refusal on the part of some theorists to accept such results when they appear to be in conflict with some of the present oversimplified and therefore intellectually appealing theories of the universe. (...) [That concern] is due to a more basic distrust of doctrines that frequently seem to be more concerned with the fictitious properties of ideal (and therefore nonexistent) universes than with the actual world revealed by observations [8]."

The few quotations and references above do not indicate a widespread presence of dogmatism among cosmologists, but simply its presence among at least some of them. However, the fact that at different times, different people possessing different theoretical and observational perspectives and motivations, not only acknowledged the presence of dogmatism in cosmological research, but were also worried about its influence, is enough to show that such an influence is not negligible and should be investigated. Therefore, it is reasonable to say that dogmatic tendencies have been felt in the field since at least the 1930's.

Nevertheless, it is clear that those dogmatic tendencies within modern cosmology have little to do with the FLRW model itself, whose achievements so far can only be described as impressive. They result from the *attitude* of not accepting that some key features derived from those models can be checked, l*et al*one questioned, by observations. When a feature of a model is ascertained through imposition rather than by experimental or observational check it is unscientific because it is *only* based on personal choices. In other words, a certainty achieved that way becomes a dogma.

Here we shall discuss the issue of dogmatism in modern cosmology. We accept the quotations and references above as sufficiently enough evidence of its presence in modern cosmology, and we will propose a way capable of neutralizing its influence. In our opinion, that can be achieved if researchers in the field embody the epistemological principles advanced by Ludwig Boltzmann (1844-1906) from the end of the 19th century until his death.

2. Dogmatism in Modern Physics

It is generally accepted that scientific truth is achieved when theory is directly confronted with observations (or experiments). Since the time of Galileo Galilei (1564-1642), observation and/or experimentation has been used to confirm or to falsify a theory, and without such crucial tests no theory can be considered scientific. To accept a theory without this experimental/observational validation is to accept it as a dogma.

However, things are not so simple when such a validation is not clear cut and free from ambiguities, which in practice is the case most of the time when doing real science. In such situations dogmatic tendencies can thrive. Those tendencies appear in the history of physics more frequently than one may imagine. An example particularly important to the subject discussed in this article was the debate between Boltzmann and Wilhelm Ostwald (1853-1932) at the end of last century concerning the atomic view of the world. At stake was the definition of a scientific theory, what should be its aims and methods, and the definition of scientific truth.

2.1 Boltzmann and Dogmatism in Physics at the end of the last century

By the end of the last century, Boltzmann was engaged in a passionate defense of the atomic concept which, at the time, was facing a growing number of powerful opponents, like Ostwald and Georg Helm (1851-1923), who considered the atomic picture of the world outdated [9,10,11,12,13], [14, p. 42-61]. They then advocated its replacement by the view that the physical world could only be correctly described by means of the concept of energy conservation and its derivatives, which implied the denial of the atomic idea [13,15].

Boltzmann feared that such a purely energetic representation would lead physics to become dogmatic, a fact that would inevitably also lead to its stagnation. He then wrote many epistemological texts about the development of physics in general, whose conclusions led him to advance what is now considered his two main epistemological theses [14,15]. The first one stated that a physical theory is nothing more than a representation of Nature, and the second thesis stated that Nature can be represented by many different theories, which can even be opposed to each other. Nowadays this last thesis is known as *theoretical pluralism*.

Among the physicists of the last one hundred and fifty years, Boltzmann was one of the few, if not the only one from within the scientific community and, therefore, from the perspective of an active and eminent physicist, to discuss dogmatism in an epistemological context. His epistemological thinking covers issues like what a scientific theory is and how it develops. For those reasons, we believe that his ideas give us the appropriate epistemological framework which allows us to identify and counteract what can inhibit the development of scientific theories, namely dogmatism.

Nevertheless, one may ask the question: how is it possible that dogmatic tendencies can thrive even when the scientific community openly accepts that the ultimate test of a theory is experimentation? We shall discuss this point next.

2.2 What is “scientific” dogmatism?

It is generally accepted nowadays that in science nothing is in principle unquestionable, but, inasmuch as the validation of new theories and models usually takes time, a certain degree of conservatism towards new theories and models, and skepticism towards new observations, is, nevertheless, necessary since it is not possible to build a sound conceptual and experimental scientific body when there is a continual change in the fundamental scientific concepts. Such skepticism is also evidence of the existence of critique in science, which is one of the most important ingredients of modern scientific reasoning and practice. Therefore, orthodoxy plays the healthy role of preserving the scientific knowledge obtained on solid bases until new theories prove to have sufficient internal consistency and experimental validation.

However, when strong conservatism and orthodoxy becomes deep rooted in the scientific community, a situation may arise that, if not effectively and successfully challenged, may lead the community to avoid altogether any kind of change of the established ideas. Such a rejection to change may easily turn to aggression towards the proposers of new ideas. In such an environment the established theories crystallize, becoming dogmatic, and scientific debate ceases to exist.

Such strong conservatism and orthodoxy very frequently come about when researchers mistakenly take their theories to be the researched objects themselves, believing that the former coincide with the latter. By doing this, they identify theory with object, and in this identification it is implicitly or explicitly assumed that the role of a scientific theory is to bring to our knowledge Nature itself. Therefore, for those researchers scientific truth means exactly such identification.

This behaviour can be detected when scientists become unreasonably over-confident that their theories are *true*, in the sense that, in their opinion, Nature does follow them. Besides, those who are prone to this kind of behaviour frequently do not accept any challenge to their way of thinking. That makes matters even worse, since they may reinforce the conservatism present at some particular moment by helping to turn a healthy skepticism towards new observations that challenge established ideas into an out of hand rejection of them.

When a situation like that takes place, it creates an environment where, in the view of those described above, the theory considered as the best “realization” of the researched object assumes the role of the supreme and only truth, never to be questioned. In these days, however, science has become very dynamic and the theory elected to be *the* true representation of Nature in some particular area, and at some particular time, can be quickly toppled from its position due to the unexpected arrival and imposing character of new data or new discoveries. Then, if the dogmatic attitude remains, what happens next is the urgent search and eventual replacement of the toppled theory by a new “supreme theoretical truth” which then becomes the dogma of the day.

The conservatism in the process described above can throw the scientific community into deep confusion because, if the community is large enough, opinions may be different among the different research groups in the search for the new dogma. Then one may expect a disagreement about the choice of “the best” theory, with the different groups choosing different theories. So, we end up having a conflict of dogma within the community rather than a scientific debate, which in practice becomes marginalized or may even cease to exist.

One may therefore define *scientific dogmatism* as being the unreasonably and unjustified over-confidence in certain theory, over-confidence which stems from the misleading, and often unconscious, identification of the researched object with its correspondent theory. Such identification implicitly or explicitly assumes that the role of a scientific theory is to reveal Nature itself to us. Such dogmatism causes over-confident researchers to deliberately refuse as scientifically valid any theoretical pictures different from theirs since, in their view, those different theoretical pictures would "contradict Nature itself". As we shall see, Boltzmann's epistemological theses are particularly useful in clarifying this question. While they preserve the freedom of personal choice in the creative theoretical work, they also deny the notion that we can ever achieve an ultimate knowledge of any scientific question, that is, they deny that we can reach Nature itself, since in Boltzmann's view *any theory is nothing more than a representation (image) of Nature* [16,17].

3. Scientific Theories as Representations of Nature

As stated above, at the end of the last century Boltzmann was engaged in a passionate defense of his viewpoints, where he sought to show that all scientific theories are nothing more than representations of the natural phenomena. By being a representation, a scientific theory cannot aim to know Nature itself, knowledge which would explain why the natural phenomena show themselves to us the way we observe them, since such ultimate knowledge is, and will ever be, unknowable. As a consequence, a scientific theory will never be complete or definitively true. This viewpoint actually redefines the concept of scientific truth by advancing the notion that it is impossible to identify theory with the researched objects since scientific theories are nothing more than images of Nature (see §3.2 below). In other words, a scientific theory can, one day, be replaced by another. It is the possibility of the replacement of one theory by another that defines and constitutes the scientific progress, and that is diametrically opposed to dogmatism [11,12].

Boltzmann's ideas about scientific models as representations are clearly stated in the passage below, quoted from the entry "Model" in the 1902 edition of the *Encyclopedia Britannica*:

> *"Models in the mathematical, physical and mechanical sciences are of the greatest importance. Long ago philosophy perceived the essence of our process of thought to lie in the fact that we attach to the various real objects around us particular physical attributes - our concepts - and by means of these try to represent the objects to our minds. Such views were formerly regarded by mathematicians and physicists as nothing more than unfertile speculations, but in more recent times they have been brought by J. C. Maxwell, H. v. Helmholtz, E. Mach, H. Hertz and many others into intimate relation with the whole body of mathematical and physical theory. On this view our thoughts stand to things in the same relation as models to the objects they represent. The essence of the process is the attachment of one concept having a definite content to each thing, but without implying complete similarity between thing and thought; for naturally we can know but little of the resemblance of our thoughts to the things to which we attach them. What resemblance there is lies principally in the nature of the connexion, the correlation being analogous to that which obtains between thought and language, language and writing. (...) Here, of course, the symbolization of the thing is the important point, though, where feasible, the utmost possible correspondence is sought between the two (...) we are simply extending and continuing the principle by means of which we comprehend objects in thought and represent them in language or writing [18, p. 213]."*

It should be noted that the idea that scientific theories are representations is still being echoed today, and an example of a recent discussion can be found in [19].

3.1 Theoretical Pluralism

The most important epistemological conclusion which was reached by Boltzmann from his debate against Ostwald's energeticism, and which constitutes the core of his philosophical thinking, is usually called theoretical pluralism. This is a consequence of the thesis that all scientific theories are representations of Nature. By being a representation, a scientific theory is, therefore, initially a free creation of the scientist who can formulate it from a purely personal perspective, where metaphysical presuppositions, theoretical options, preferences for a certain type of mathematical language, and even the dismissal of some observational data, can enter into its formulation. All that in the period where the theory is formulated. However, in order to make this theory eligible to become part of science, it is necessary for it to be confronted by the experience [20, §16, p. 286], [21, p. 107]. If it is not approved in this crucial test, the theory must be reformulated, or even put aside [20, p. 286], [21, p. 225-226]. Boltzmann also emphasized that, since all scientific theories are, to some extent, free creations of scientists, scientific work is impossible without the use of theoretical concepts, which originates from the fact that it is impossible the formulation of any scientific theory simply from the mere observation of natural phenomena.

The theoretical pluralism also states that the same natural phenomenon can be explained through different theories. Still according to Boltzmann, this possibility has its origins in the fact that, as seen above, any theory is a representation, a construction, an image of the natural world. And nothing more. One cannot do science in any other way. Either it is a construction, a representation, or the theory is not scientific [14, p. 173-176], [22, p. 216], [23, p. 91]. In Boltzmann's words,

> *"(...) Hertz makes physicists properly aware of something philosophers had no doubt long since stated, namely that no theory can be objective, actually coinciding with nature, but rather that each theory is only a mental picture of phenomena, related to them as sign is to designatum."*

> *"From this it follows that it cannot be our task to find an absolutely correct theory but rather a picture that is, as simple as possible and that represents phenomena as accurately as possible. One might even conceive of two quite different theories both equally simple and equally congruent with phenomena, which therefore in spite of their difference are equally correct. The assertion that a given theory is the only correct one can only express our subjective conviction that there could not be another equally simple and fitting image [23, p. 90]."*

In summary, theoretical pluralism synthesizes the fact that, since knowledge of Nature itself is impossible, a theory can only be better than another, not truer in the non-Boltzmannian sense (see §3.2 below). It is the necessary mechanism which prevents science from running the risk of stagnation. Within this perspective, truth can only be *provisional*, and is in fact an approximation achieved by different means, that is, by different theoretical images [20, p. 273, § 3], [21, p. 115-116].

When Boltzmann advanced theoretical pluralism, he also had another goal: to establish a clear and unreachable limit for dogmatism, that is, a limit which it could not surpass. Boltzmann believed that once theoretical pluralism were accepted and embodied in research practice, it would not allow that, once proposed, a theory could be excluded from the scientific scenario.

Boltzmann also pointed out that the thesis that a scientific theory is a representation was not new. Kant, in the 18th century, and Maxwell, one of the most important influences upon him in the middle of the last century, had both defended similar theses. Other contemporary physicists, like Hertz and Helmholtz, shared similar views [22, p. 206], [23, p. 83]. By remembering that others like Kant and Maxwell had already expressed similar propositions, Boltzmann wished to make sure that any theory or model would be continuously perfected, without being excluded by any other "tribunal" than the experience [14, chap. 4]. But, before we go into the relationship between Boltzmann's theoretical pluralism and modern cosmology, we need to discuss in more detail Boltzmann's notion of scientific truth.

3.2 Boltzmann's Concept of Truth

One of the main features of modern science is that since the beginning of the modern scientific revolution with Galileo, scientists began to define truth as the *correspondence* between models and observations. Nevertheless, since Boltzmann's theses state that all scientific theories are representations of natural phenomena, that is, they are not capable of determining what *really* constitutes Nature, the concept of truth in modern science should no longer be one which seeks to determine Nature itself. Therefore, within the context of Boltzmann's epistemological thinking, this concept of correspondence as scientific truth becomes outdated as Boltzmann's views are based upon the principle of theoretical pluralism. As a consequence, since more than one model, or theory, may well represent the same group of natural phenomena and/or experimental data, how it is possible that scientists can choose one model among the possible ones?

At this moment Boltzmann advances another definition of scientific truth: the *adequacy*. According to him, theory A is more adequate than theory B if the former is capable of explaining more rationally, more intelligibly, a certain set of natural phenomena, than the latter. In his own words,

> *"(...) let me choose as goal of the present talk not just kinetic molecular theory but a largely specialized branch of it. Far from wishing to deny that this contains hypothetical elements, I must declare that branch to be a picture that boldly transcends pure facts of observation, and yet I regard it as not unworthy of discussion at this point; a measure of my confidence in the utility of the hypotheses as soon as they throw new light on certain peculiar features of the observed facts, representing their interrelation with a clarity unattainable by other means. Of course we shall always have to remember that we are dealing with hypotheses capable and needful of constant further development and to be abandoned only when all the relations they represent can be understood even more clearly in some other way [24, p. 163]."*

"(...) We must not aspire to derive nature from our concepts, but must adapt the latter to the former. We must not think that everything can be arranged according to our categories or that there is such a thing as a most perfect arrangement: it will only ever be a variable one, merely adapted to current needs [24, p. 166]."

He also noted that since theories are images of Nature, all have some explanatory power, and that a good theory is achieved by being carefully crafted by scientists, in a process similar to Darwin's Natural Selection:

"Mach himself has ingeniously discussed the fact that no theory is absolutely true, and equally hardly any absolutely false either, but each must gradually be perfected, as organisms must according to Darwin's theory. By being strongly attacked, a theory can gradually shed inappropriate elements while the appropriate residue remains [25, p. 153]."

Once more, one should note that these ideas are still being echoed today (for instance, see [19, p. 214]).

3.3 The Search for a Good Theory

It is important to stress that although theories are representations, and, as we saw above, personal theoretical options can enter in their formulation, they are *not* entirely arbitrary. The basic aim of any theory is to represent something that is going on in Nature, and a successful theory does achieve this to a considerable extent. Therefore, such a theory can use some symbols, or a specific mathematical language, just as conventions. However, since Nature itself must be represented in it, conventions will always be limited to only those aspects of the model, of the representation, which are not perceived, in that theory, as being directly dictated by Nature. Thus, under Boltzmann's perspective, one cannot say that theories are just conventions, because after being carefully crafted by the scientists as representations of unique, non-arbitrary, natural phenomena, they become attached to them, and end up saying something unique about what is going on in Nature.

Finally, besides being a good representation, there is still another criterion capable of conducting the preference of scientists towards one particular model: its predictive ability. This is important because once a certain theoretical prediction is confirmed, the scientific knowledge about Nature increases quantitatively. A correct prediction is also important because it is formulated within the context of a specific theoretical picture. So, by being capable of predicting unknown phenomena, a model shows all its explanatory power since it is not only capable of announcing the already known "pieces", but it is also able to go even further, showing the existence of other still missing pieces which are necessary for a deeper and more organized understanding of Nature. One cannot forget that one of the most important aims of science is to increase and organize our knowledge about Nature, and thus, a certain theory is richer than others if it is able to better contribute to such an increase and organization. Such a preference for the richer theories makes them more likely to be used, and developed, than the poorer ones, and after a while the distance between them may be so great that it may no longer be worth for researchers to keep on working with the poorer representations, which are then put aside and, usually, forgotten.

4. Cosmological Models as Images of the Universe

In order to relate the previous discussion to issues in cosmology we need, first of all, to distinguish Nature from its representations, that is, from our theoretical images. Therefore, we shall adopt the following difference between the terms `Universe` and `universe`. The first term, with capital `U`, will refer to the aspects of Nature from which the different theoretical models are built, while the second one, with small `u`, will refer to the models themselves. By means of this distinction, and bearing in mind Boltzmann's theses, we can state that the different theoretical models of the Universe are then "universe models", that is, "cosmological models", or simply "cosmologies" or "universes". Cosmology is then the science that attempts to create working representations of the Universe.* As a consequence, theoretical pluralism tells us that there may be many different cosmologies, where each one adopts different images of the Universe, although its true nature is, and will always be, unknowable.

From the perspective described in the previous sections we can see how damaging dogmatic thinking can be, since in its most basic sense it denies the cosmological community the option of thinking differently than the current accepted view. That inhibits the appearance of different theoretical representations, which, according to the thesis of theoretical pluralism, are fundamental for the development of modern cosmology. In other words, dogmatism goes

* The terms Universe, universe, cosmologies, *etc.*, are found in the literature as usually having meanings close to the ones given in this article. Therefore, what is being done here is only to state precisely their meanings in this context.

against theoretical pluralism. Therefore, bearing in mind what we discussed in §1, it is not only desirable, but *essential* that different theoretical pictures, *i.e.*, theoretical representations other than the FLRW model emerge, be considered and developed in cosmology nowadays. The present day dominance of the standard model, due to its impressive achievements, should not be used as argument against the emergence of models which may challenge the standard FLRW picture. And if in the end those different representations are wrong or produce worse models than the standard, cosmology will gain in the process, especially the FLRW cosmologies.

One must stress that the theoretical pluralism does not necessarily imply competition among the different theories, but it often means complementarity. This is exactly what Matravers, Ellis and Stoeger [2] seek when they argue in favour of a more phenomenological approach to cosmology. Inasmuch as, according to Boltzmann's theses, all theories have some explanatory power, all cosmologies end up saying something about the physical process that are going on in the Universe, because not all cosmologies use the same set of ideas and phenomena which they seek to explain. Therefore, the emergence of different cosmologies, far from being a problem for our better understanding of the Universe, is essential for it. And if those different cosmologies have elements which contradict each other, observations provide the first mechanism, but not the only one, which allows us to discard the inappropriate elements of the emergent cosmologies.

The different cosmologies should either be in competition among themselves or complement each other, but as none can be confused with the Universe, no cosmology produces ultimate knowledge of it. One cosmological model can only be provisionally better than another. It is our observational interaction with the Universe that produces the *empirical* basis upon which cosmologies are created, and inasmuch as this interaction is basically technological, this empirical basis will be changed by technological and theoretical progress which itself creates the conditions for the partial or complete transformation of the cosmological models. By the same token, the diversity of technological means produces different interactions and, therefore, different empirical basis that lead to possible different cosmologies.

5. Conclusion

From what we have seen in the previous sections, we can conclude that dogmatism works against scientific progress and, to avoid it, a change of attitude should be adopted by cosmologists. This change is realized by the adoption of the theoretical pluralism as a better epistemological framework for research in cosmology. By means of this thesis it is possible to avoid the deep rooted and unjustified belief in ideas which are nothing more than just personal beliefs. As seen above, those ideas are necessary, and perhaps indispensable, for the formulation of the different representations of Nature, as such formulation is also a result of the free creation of the scientists. However, those personal beliefs will always be restricted to the theoretical models and, at best, they can only generate better, or worse, representations of Nature. Moreover, they should never be confused with the "true" Universe since its ultimate reality is unknowable.

Inasmuch as no cosmological model can ever be capable of holding ultimate knowledge about the Universe, then it follows that no cosmology can hold eternal truths. Then, the larger the number of cosmologies available to cosmologists, the higher the chances of we obtaining better representations of the Universe.

Scientific knowledge is best characterized by the continuous search for better, but never definitive, representations of natural phenomena. The replacement of a theory by another, the main feature of modern science, can only happen if it is assured that no scientific theory can reach the stage of definitive truth. In other words, a scientific theory can only be better than another one, and nothing more. Therefore, since any cosmological model can only aim to be a temporary explanation of what one chooses or is able to observe and experiment in the Universe, when it is formulated it is already doomed to disappear. The irony always present is that no one can tell with precision when that will happen, unless one takes a dogmatic posture.

In conclusion, we believe that it would be essential that cosmologists in general recognize that their theories and models about the Universe are nothing more than representations. This explicit distinction is very important as it would create the proper environment where different theories and models can live together without the danger of dogmatism.

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In Memory: Chalmers W. Sherwin

Chalmers Williams Sherwin (b. November 27,1916, d. February 20, 1998) was one of America's foremost physicists and science administrators. His professional career included a doctorate in physics at the University of Chicago, a physics professorship and 14 years of teaching at the University of Illinois, posts at the M.I.T. Radiation Laboratory (group leader) and Columbia University, the post of Chief Scientist of the Air Force (1954), positions of leadership in the Office of Deputy for Defense, Research and Engineering (in the Pentagon) and in the Office of Science and Technology, and various administrative positions with industry, including the General Atomic Company (San Diego) and the Aerospace Corporation.

Throughout his varied career Chal never lost his love of physics and his remarkable ability to reduce seemingly complex and sophisticated physical prob-

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lems to their intuitive essence. This is perhaps best illustrated in his book *Basic Concepts of Physics* (Holt, Rinehart and Winston, New York, 1961), which is still one of the most effective antidotes to the prevailing over-mathematization of the subject. He wrote also an Intro*duction to Quantum Mechanics* (same publishers). While at Illinois he conceived and caused to be performed the Sherwin-Rawcliffe experiment ("Electromagnetic Mass & the Inertial Properties of Nuclei," Report 1-92, March 14, 1960, Coordinated Science Laboratory, University of Illinois, Urbana, Illinois), an experiment establishing the *lack of tensor properties of nuclear mass* that I personally consider to rank in significance with Michelson-Morley, as one of the great, all-encompassing null results of our time. It is a commentary on the prevailing state of the scientific literature that this experiment was never reported in the regular journals. In 1960 he published in the *Physical Review* a fundamental paper associating the temperature coefficient of the Moessbauer effect with relativistic time dilatation.

One of his last publications to receive widespread notice was "New experimental test of Lorentz's theory of relativity" [*Phys. Rev. A, 35,* 3650 (1987)]. Aware of the longstanding inability of any experiment to reveal the Lorentz contraction, Chal sought to test the observational consequences of interpreting the contraction as a physical phenomenon affecting atomic bond lengths. This was the original idea behind the Lorentz ether theory. Sherwin showed that a consequence would be a "relaxation" time lag of contraction and de-contraction that could be observed as a periodic phase shift in suitable rotary geometry. In his retirement he set up the required apparatus and carried out the experiment with his own resources. The results were again negative: The Lorentz contraction kept its record clean of never having been seen by any scheme of direct observation. The fact of his putting such a question to nature suggests that as Chal matured he became progressively dissatisfied with conventional views and received opinions in physics. Increasingly, indeed, he struck out on his own and had to think and see for himself. And in direct measure as this independence of mind asserted itself, his papers became unpublishable in the established physics media.

The writer has a number of unpublished "preprints" from Chal's later days, dealing particularly with the subject of optical aberration, which became one of his special interests. These show his penchant for down-to-earth detailed calculations of particular cases. For he knew the dangers of leaping to the general without proceeding through the particular. He felt that there were logical problems with special relativity and again struck out on his own to elaborate "A New Theory of Relative Measurement." Readers of *Apeiron* do not have to be told that this, too, was unpublishable in first-line journals.

Anyone so desiring can obtain from me a sampling of such papers from Sherwin's later years. They mark the final accomplishments of a great and original mind that, like Herbert Dingle's, had in maturity freed itself from fealty to the *status quo*. If physics possessed a social dynamic capable of exploiting the distinction between wisdom and senility, the works of such people would be honored as representing the judgment of experience upon the fashions of the herd. But the herd (for which read "professionalism") has no felt need for such judgments and so "progresses" in its own lemming fashion. Those of us lesser lights who have known nothing but frustration in our attempts to loosen the deathgrip of professional consensus (of idiot savants) upon the vitals of science may take some benefaction from Chalmers Sherwin's failure: If that veritable titan of rationality could not restore a modicum of pluralism to the foundations of physics, who could? It lies already beyond the power of any individual.

In his experience of life Sherwin was fortunate to be helped immeasurably by a wife who complemented his own intelligence and handsome physique, and a large family that supported him loyally to the end. Here is a life to be honored, admired, and emulated. One can only say: May his tribe increase.

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Book Review

Einstein's Theory of Relativity versus Classical Mechanics *by Paul Marmet, Newton Physics Books, 2401 Ogilvie Road, Glocester, Ont. Canada, K1J 7N4 -- ISBN 0-921272-18-9.*

All students of physics are familiar with the story. At the beginning of this century, the physics establishment thought that it had all of the answers. All physical phenomena could be explained by the two existing theories which are now referred to as classical mechanics and classical electrodynamics. Then there came an onslaught of data that could not be interpreted by the existing theories. The establishment tried in vain to explain the new observations with all possible classical models, but to no avail. It was left to courageous new thinkers to realize that reality could only be explained with mathematical models that could not be visualized. The establishment fought these new theories every step of the way and they were only accepted after pitched battles and uncompromising scrutiny.

However, history is written by the victors and few students really appreciate the consequences of this.

It has been said that ninety, perhaps 99%, of physicists that have lived, have spent most of their training learning modern physics. The man-hours spent developing the classical theories are dwarfed by that spent on modern ones. In order to salvage the modern theories from the present onslaught of anomalous data from astronomy and geology, modern models have been developed that are far more complex than any of the classical models that attempted to salvage the classical theories. It should not be surprising, then, if even a modest effort to develop classical models produces a breakthrough.

In recent years it has been shown that much of the motivation for rejecting the classical theories was in error. The orbital model of the atom was rejected because it was thought that a charged particle moving in a circular orbit would continuously loose energy, since it was accelerating. Assim Barut has pointed out that for a fairly simple classical electron model, an orbiting electron will absorb all of the energy that it radiates. Battey-Pratt and Racey have developed a classical electron model that is visualisable and provides a logical basis for special relativity and quantum electrodynamics. Prokhovnik has shown that not only is the Lorentzian interpretation of the Michalson and Morley experiment equivalent to the Einsteinian interpretation, but actually makes more sense in interpreting the "doppler" shift in the cosmic background radiation.

It is in this neo-classical spirit that Paul Marmet's *Einstein's Theory of Relativity versus Classical Mechanics* reexamines the role of general relativity as the only acceptable theory of gravitation. Indeed, general relativity and gravitation have become synonymous. Universities routinely offer courses titled "Gravitation" where other theories are not even mentioned much less taught and where discussion of data is limited to the three classic tests. I recently attended a conference where the discussion turned to nonEinsteinian gravity. A young Ph.D. immediately pointed out that Einstein's theory could be the only valid one because it passed the three tests. This type of acceptance of Einstein's theory is philosophically invalid. A theory cannot establish its own criteria for acceptance. It has to be evaluated in the context of all data (including many well-documented phenomena that fit neither Newton's or Einstein's theory) and cannot benefit by false choice arguments (it is the better fit of the two possible theories—implying that there are no other choices).

Marmet develops a unique theory of gravitation and relativity based on classical mechanics, quantum mechanics, mass-energy conservation, conventional logic and a more traditional view of space and time. And he appears to be at least partially successful.

His approach starts with the conservation of mass and energy, and this is where I believe he is on the weakest ground.

On page 20 he writes, "*We must realize that without mass-energy conservation not much of physics remains. Physics becomes magic.*" This is a statement that is often repeated and must be challenged. First of all, such a broad assertion should not be made without discussing the accuracy with which this law has been proven. The conservation laws are known to be valid in about the same range that the fundamental "constants" are known to be constant. We should be very careful in extrapolating these laws beyond that range as is routinely done in cosmology or even geophysics. While mass-energy conservation can be a very useful tool in calculations, it must be remembered that energy is never directly measured, it is inferred. It is a mathematical expression, it is not a fundamental quantity. And there is no need to imply that theories that allow mass or energy creation from nothing are *magic.* The history of physics is full of laws that appear perfectly valid in one domain but fail in another. When we insist that a law must be valid in a range or domain where it has never been tested, we are not practicing science but religion.

Furthermore on the same page, he writes, "*we must expect that the electron as well as the proton in the atom have individually lost the same relative mass.*" In this journal, Kokus and Barut have shown strong circumstantial evidence that these masses may vary separately.

All of that said, what follows is very interesting. In chapters 1-3, he starts with the conservation of mass and energy and mass-energy equivalence, $E=mc^2$ (in Marmet's derivation the energy and mass are *assumed* to be linearly related and the constant of proportionality turns out to be the speed of light squared). With this he derives the Lorentz transformations. This may not seem surprising because it was these transformations which were used to derive $E=mc^2$. But there are two worthwhile distinctions in the logic. For Marmet, the changes in length are real. The change of energy of a moving particle leads to a change in the mass of the subatomic particles and therefore the Bohr radius and the rate of atomic processes change. We do not have to speculate about space dilating and contracting and time speeding up and slowing down. It is the rulers that dilate and contract and the clocks that speed up and slow down. This is what makes the logic much less ambiguous, especially when it comes to simultaneity. (This is actually very compatible with Prokhovnic's approach.)

I will venture that it is for chapters 4-6 that the book will be remembered. In them, he derives Einstein's general relativistic formula for the advance of the perihelion of Mercury; and he does it with mostly algebra and with a logic that is understandable and unambiguous every step of the way. Because Mercury is close to the Sun and because it is rotating about the Sun, a meter and a second on it will be longer than those that are stationary in outer space. So even though in Mercury's units, it's orbit obeys Newtonian physics and does not advance, when transformed into the shorter seconds and meters of outer space, it advances by the observed amount.

Comparing Marmet's and Einstein's approaches, pure simplicity would weigh in on Marmet's side. I would estimate that it would take less than a tenth of the time to understand his calculations.

Chapters 7-9 rederive and discuss the Lorentz transformations in three dimensions, the doppler effect, simultaneity and the absolute velocity of light with Marmet's assumptions and logic.

In chapter 10, he discusses the principle of equivalence. Einstein believed that if one were in an enclosed elevator, that it would be impossible to distinguish being in a gravitational field and being given the appropriate acceleration in outer space. Marmet suggests a thought experiment which he believes can distinguish between the two situations. He then goes on to discuss the gravitational deflection of light and gets a value much less than Einstein's. It is this section, along with the next two chapters (gravitational effects on the internal structure of atoms, very dense matter, matter creation and destruction) that deserve far more scutiny than I was able to give them.

But for me the real gem is Appendix II. It alone is worth the price of the book. Here Marmet chronicles the history of the 1919 solar eclipse expedition, including many details that were omitted when the victors wrote its history. The expedition was supposed to measure the deflection of light from a distant star as it passed near the sun, which is only possible when and where there is a total eclipse. The deflection predicted by Einstein's theory of general relativity was double that predicted by Einstein with his interpretation of Newtonian mechanics. After three decades in physics and reading countless accounts of this measurement, I had always had the impression, which I am sure is shared by many others, that the results unambiguously favored general relativity. Marmet shows that it was virtually impossible with the apparatus at hand to distinguish between the two predictions and suggests that the quick acceptance of general relativity was due more to politics than science. Unfortunately, Marmet does not follow this with a history and analysis of subsequent experiments on light deflection by massive bodies.

While the book is not an easy read, I hope that it will be read and that it will generate the controversy that it deserves.

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The Bookshelf

New Publications on Physics and Astronomy

Rélativité et Substratum Cosmique, Joseph Lévy, 229 pages (Librairie Lavoisier, 14, rue de Provigny, 93236 Cachan Cedex)

Physics or Metaphysics? Gerhard Kraus, 118 pages (Janus Publishing Company, 19 Nassau St., London W1N 7RE)

International Workshop:
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Universidad Autonoma de Zacatecas, Mexico
June 23-26, 1999

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